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Department of Economics
Discussion Paper Series

Choosing to be Trained: Evidence from a Field Experiment

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Discussion Paper No: 2012-01
October 2012

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May 2013

Abstract

In this paper, we examine the determinants of self-selection into a training program by combining data from an artefactual field experiment with survey data collected from the targeted community in New Delhi, India. We find that applicants and non-applicants differ in terms of socio-economic characteristics (elicited through survey data), as well as selected behavioral traits (measured using the field experiment). Even after controlling for a range of socio-economic characteristics, we find that individuals with a higher preference for risk and competition are more likely to apply to the program. This suggests that focusing only on the socioeconomic and demographic characteristics might not be sufficient to fully explain selection into the program. Participants' intrinsic traits are important and can influence take up rates in such programs. Our results suggest that as a methodology, there is valuable information to be gained by dissecting the black box of unobservables using artefactual field experiments.

Keywords: Selection, Field Experiment, Household Survey, Risk, Competition
JEL Codes: J24, C93, C81

Acknowledgements: We would like to thank Stefan Dercon, Ben Greiner, Glenn Harrison, John Hoddinott, David Huffman, Tarun Jain, Pramila Krishnan, Aprajit Mahajan, David Reiley, Shailendra Sharma, John Strauss, Robert Thornton, Marie Claire Villeval, seminar participants at Vassar College, Calcutta University, Indian School of Business, Queensland University of Technology, participants at the PACDEV Conference, International Atlantic Economic Society Conference, the DRU workshop at Monash University, the Australian Development Economics Workshop, the ESA International Meetings, the India Development Workshop at UNSW and the Nordic Conference for Development Economics for their comments and suggestions. Shelly Gautam, Inderjeet Saggu, Sarah Scarcelli and Raghav Shrivastav provided excellent research assistance. Funding was provided by Monash University, Australia and Fordham University, USA. We are especially grateful to the staff of Satya and Pratham for their outstanding work in managing the implementation of the vocational training program. The usual caveat applies.

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1. Introduction

Worldwide recession along with increasing unemployment has renewed interest in training programs that help workers accumulate additional skills to obtain new jobs or retain current ones. The economic benefits of participating in such training programs are substantial in developing countries (see, for example Attanasio *et al.* (2011), Card *et al.* (2011), Maitra and Mani (2012)). However, such programs can only help attenuate unemployment if the targeted individuals volunteer to participate in the program. If instead, they shy away from participating in these specialized avenues of skill building, then increasing the supply of training schools and programs as a policy does little towards the final goal of improving labor market outcomes and welfare. For a policy maker then, there is a case for not just promoting labor market training programs, but also, to improve and target them better to reap maximum welfare gains through increased participation. To achieve this, it is crucial to identify not just the impact of a program, but in addition, understand the selection process that identifies the factors influencing participation into the program.

Our goal in this paper is to focus exclusively on the participation decision, that is, to determine whether participants and non-participants of a training program systematically differ along measured behavioral traits and socio-economic characteristics.[1] Identifying these traits can help in designing and promoting programs more effectively.

Self-selection or program participation has been previously studied in different contexts such as a labor market training program in the US (Heckman and Smith (2004)), a school incentive program in India (Barnhardt *et al.* (2009)), a microfinance, soft skills and entrepreneurship program in Uganda (Bandiera *et al.* (2009)) and a migration lottery program in Tonga (McKenzie *et al.* (2010)). Crucially however, all these papers have relied on the use of survey data to estimate the participation/selection equation leaving out possible sources of differences arising out of variation

[1] See Maitra and Mani (2012) for impact estimates of the underlying program discussed in this paper.

in behavioral traits or intrinsic characteristics between participants and non-participants. We aim to fill this gap by examining the differences between applicants and non-applicants in a labor-market training program in terms of both behavioral traits and socio-economic characteristics. We do this by combining data from unique field experiments (experimental data) and from primary surveys (observational data).[2]

The training program we examine was widely advertised to women between ages 18 and 39 residing in selected disadvantaged communities (or slums) in New Delhi, with 5 or more grades of schooling. Our sample population consists of a randomly selected pool of applicants who applied to the training program, and those who chose not to apply in spite of receiving the advertisement – the non-applicants. The artefactual field experiment (see Harrison and List (2004) for a definition) was designed to elicit unobservable intrinsic characteristics such as risk attitudes, attitudes towards competition and confidence levels. In addition, to identify the socio-economic differences, we administered a detailed household survey.

Our results show that the probability of applying to the training program can vary in terms of both socioeconomic and intrinsic characteristics. We find that younger women, with prior experience in stitching and tailoring, members of a Rotating Savings and Credit Association (ROSCA), belonging to households with higher income and dependency ratio (ratio of number of children less than 5 to the number of adult women in the household), have a significantly higher probability of applying to the training program. The results from our behavioral experiment reveal in addition, that women with greater preference for risk and competition are significantly more likely to apply to the training program. This suggests that focusing only on the socioeconomic and demographic characteristics might not be sufficient to fully explain selection into the program as

[2] Banerjee and Duflo (2008) define non-experimental (survey) data as observational data. In our paper, the observational data provides information on the socio-economic characterestics (from the household survey); the experimental data provides empirical evidence on the intrinsic characteristics of individuals.

previously done in the literature. Participants' intrinsic traits are important determinants of self-selection into labor market training programs and can influence take up rates in such programs.

While individuals can vary along many intrinsic dimensions, we chose to investigate three sources of behavioral differences. These three sources are arguably important in influencing the choice of selecting/applying into the program. The first characteristic is risk. The role of risk in choice-making (occupational choices, adoption of new technology and investment in higher education) is now well documented.[3] In developing countries, incomplete financial markets fail to smooth economic risks, and in conjunction with institutional hurdles make any start-up venture fraught with uncertainty. As a result, only individuals with a higher tolerance for risk might be willing to engage in any investment activity. Joining a skill accumulation program is also an investment activity, since it involves considerable time costs and sometimes even monetary investment. Consequently, one can expect that risk bearing attitudes might play a role in self-selecting into a training program. The role of risk taking attitude is further exacerbated as many participants in vocational training programs are often interested in becoming micro-entrepreneurs post-training, and hence, should be willing to take a certain degree of risk associated with any new business start-up.

Second, we examine the extent to which variation in competitive preferences influences individual decision-making. For example, Niederle and Vesterlund (2007) use differences in competitiveness to explain wage gaps between men and women. Gneezy *et al.* (2009) and Andersen *et al.* (2010) examine the evolution of gender differences in competitiveness. The literature on behavioral differences in competitiveness leads us to expect that such differences can possibly

[3] Castillo *et al.* (2010), using artefactual field experiments, find differences in risk preferences to have significant implications for occupational choices. Liu (2008) finds that more risk averse farmers in rural China adopt Bt cotton, a relatively new technology, much later. Aversion towards risk has been found to have a negative effect on investment in higher education using survey evidence (see Chen (2003) and Belzil and Leonardi (2009)).

impact the decision to apply for an income enhancing training program.

Third, confidence is claimed to have a significant impact on labor market outcomes (Koszegi (2006), Bénabou and Tirole (2002)), although credible empirical evidence on the effect of confidence on labor market outcomes is rare due to the difficulty in measuring and obtaining data on confidence. It has been pointed out that the level of confidence can affect wage rates (Fang and Moscarini (2005)), performance in financial markets (Biais *et al.* (2005)) entrepreneurial behavior (Cooper *et al.* (1988), Camerer and Lovallo (1999); Bernardo and Welch (2001); Koellinger *et al.* (2007)), and can explain the persistence of intergenerational inequality in income and education (Filippin and Paccagnella (2009)).

The rest of the paper is organized as follows. Section 2 includes a description of the subject pool and experimental design. Descriptive statistics and regression results are discussed in section 3. Concluding remarks follow in section 4.

2. Methodology

2.1 Subject Pool

Our experimental subjects are from the pool of applicants and non-applicants to a subsidized training program in stitching and tailoring, conducted in selected slums/resettlement colonies of New Delhi, India.[4] It was clearly explained during subject recruitment that the experiment was separate from participation in the actual training program and had no bearing on the selection into the training program. A total of 222 women (153 of whom were applicants and 69 were non-applicants) participated in the field experiment. Since only 5.5% of the population of eligible women applied to the training program, we deliberately oversampled applicants. To ensure that this choice based sampling strategy does not bias the parameter estimates reported in section 3.2, we use

[4] See Maitra and Mani (2012) for further details on the program.

the weighted endogenous sampling maximum likelihood (WESML) estimation technique and present the corresponding weighted probit estimates. See Manski and Lerman (1977) and Greene (1992) for more on this methodology. The participation rate in the experiment was similar across the applicants and non-applicants: 76.5% of the applicants and 69% of the non-applicants invited to participate in the field experiment did so.

Subjects who participated in the experiment were requested to complete a household survey that collected detailed information on household demographic characteristics, schooling outcomes, assets, employment and quality of life. The experiment and survey were both conducted before the actual training program began. Due to the length of the household survey, it was not possible to administer the survey during the experiment. The survey was therefore conducted at their homes at a later date. We were unable to collect survey data on 18 of the 222 women who participated in the experiment: either they could not be traced or did not want to participate in the survey. Our final estimating sample consists of 204 women – 146 applicants to the training program and 58 non-applicants – for whom we have both experimental and survey data (see Figure 1). To ensure that non-response in the survey is not systematically related to behavioral characteristics, leading to a potential bias in our results, we compare the intrinsic characteristics of subjects who participated in the experiment and completed the survey, and subjects who only participated in the experiment and did not complete the survey (see Table A1 in Appendix 1). As this Table shows, there are no significant differences between these two groups of participants.[5]

[5] In addition, we also estimate a probit regression where the dependent variable (*non-response*) is a dummy that takes a value of 1 if household survey data is missing and 0 otherwise. The explanatory variables in this regression include the set of intrinsic traits included in specification 3 in Table 3 (see below) and the interaction of these variables with applicant status. The results are presented in Table A2 in Appendix 1. None of the variables included in the set of explanatory variables (interacted or not) are statistically significant and the interaction terms are also not jointly statistically significant. This implies that *non-response* is not systematically related to intrinsic differences between applicants and non-applicants.

2.2 Experimental Design

We conducted 12 sessions with 16 – 20 subjects in each session. Each session lasted approximately 2 hours and subjects participated in only one session. The average payment received from participation was Rs 203.[6]

Each subject participated in two games (the games are similar to those reported in Gneezy *et al.* (2009)). The first game was designed to evaluate subjects' attitudes towards risk (*investment game*). Each subject was endowed with Rs 50 and had the option of allocating any portion of her 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. The subject retained any amount that she chose not to invest. If the *investment game* was chosen for payment purposes, each subject tossed a coin that determined whether her investment succeeded or not.

The second game was designed to investigate the inherent competitiveness of subjects (*competition game*). Each subject participated in a real-effort task, which consisted of filling up 1.5 fl oz. zip lock bags in a minute with kidney beans (locally known as *rajma*). Prior to the task a subject had to privately choose one of two methods of compensation. She could choose a piece-rate compensation method, which depended solely on her own performance, and she would receive Rs 4 for each correctly filled bag. Alternatively, she could choose a competition-rate compensation method where her earnings would depend on how she performed relative to another randomly chosen subject in the same session. A subject received Rs 16 per bag if she filled equal number of bags or more bags than her matched opponent. If she filled fewer bags than her opponent, she

[6] The official minimum wages for unskilled workers in Delhi was Rs 203 per day at the time of running these experiments (in 2010). Typically though most women in our sample would be receiving less than this stipulated amount, since the minimum wage legislations are rarely imposed in India. 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.

received nothing. If the *competition game* was chosen for payment purposes and if the participant had chosen the competition rate payment method, she was matched with one other person in the session for payment. The matching was done as follows. The subject drew a chit from a box containing the IDs of the other participants in the session. Her performance was matched to that of the person whose ID was drawn. The matched participant's payoff remained unaffected. The participants were informed of this process beforehand and assured that all parts of the decision-making will be in private.

When choosing their compensation method, the subjects were also asked to guess their own performance in the game. More specifically, each subject was asked to provide an estimate of the number of bags she expected to fill in the real-effort task, and also her own performance-based relative rank. We use participant's guesses about her performance in the real effort task to construct three different measures of confidence: (a) an absolute measure of confidence (the subject's estimate about the number of bags she would be able to fill in one minute); (b) a relative measure of confidence (the subject's estimate about her relative standing (rank) vis-à-vis other participants in the session); and (c) confidence ratio, (the ratio of the number of bags the subject expects to fill to the number of bags she actually fills).[7]

In each session, only one of the games was chosen for payment purposes. We chose the payoffs such that the returns from choosing the riskier alternative were comparable in the two games. In both the games, choosing the riskier outcome gave four times higher payoffs compared to the riskless option. For the real-effort task in the experiment we wanted to avoid a task that was very familiar to a particular sub-section of our subjects as that could possibly bias their expectations about their performance in the game (See Gneezy *et al.* (2009) for a discussion). At the same time, we needed to choose a task that was feasible for our subject population, which ruled out many of

[7] We define this ratio to be 1, if the participant has realistic expectations about her performance, greater than 1 if she is overconfident, and less than 1 if she is under-confident.

the familiar experimental tasks such as computing sums, or word tasks since our participants (and indeed the population they are drawn from) are weak in these skills. Kidney beans comprise a staple diet in the region; women are used to handling the beans regularly – they take them out in bowls, clean and cook them, and all our participants are likely to be equally familiar with this particular task.

No communication was allowed during the session. The instructions were read out in Hindi.[8] We also displayed visual descriptions of the tasks while reading out the instructions, (see Figures A1 and A2 in Appendix 2). To enhance comprehension and minimize anchoring-bias, the instructions contained examples different from the ones displayed in the charts. In addition, to ensure comprehension of the game, each subject was asked a few questions prior to making choices in each game. While the same female experimenter read the instructions out aloud in every session, the questions were administered by two or three experimenters, depending on availability.[9]

Several of our subjects, despite having completed 5 or more grades of schooling, had poor reading and writing skills.[10] The experimenters were therefore required to be actively involved in administering the questions and noting down the responses. Such a protocol could reduce the social distance between the subject and the experimenter, and potentially create scrutiny effects. Our main interest lies in the differences in the responses of applicants and non-applicants, and as long as any one of the groups is not systematically more affected by the scrutiny effect, any potential bias arising from the scrutiny effect will be differenced out. The fact that the decisions taken in the games were not hypothetical and influenced by non-trivial monetary amounts, reinforces the

[8] The instructions were first prepared in English, and then translated into Hindi by a native Hindi speaker. The English and Hindi versions were compared and verified for consistency by a person fluent in both Hindi and English. The English version of the instructions are presented in Appendix 2.

[9] An analysis of responses indicates that there are no differences depending on the gender of the experimenter administering the questions.

[10] Even with recent advances in overall educational attainment in India, as of 2005 half the children enrolled in grade five could not read (and write) grade two level text (see Pratham (2006)). Levels of educational attainment were only worse when our participant pool attended school.

contention that subject choices can be viewed as real investment decisions, and are minimally affected by any lack of social distance. We think that our method is particularly relevant for field experiments run in developing countries where participating subjects might not have sufficient reading and writing skills.

The experimental protocol remained the same in every session: the experimenter read the general instructions aloud first; she then read out the instructions for the *investment game*; subjects made allocation decisions privately for the *investment game*; the experimenter read out the instructions for the *competition game*, and then administered questions about the choice of the compensation method and the confidence level of subjects in private. The real effort task was conducted last, and finally a coin was tossed to decide the game that would be used for payment. At the conclusion of the experiment each subject was called and paid their earnings in cash privately.

The games were always run in the same order (i.e., the *investment game*, followed by the *competition game*), no feedback was provided to the subjects in between the two games and subjects were paid on the basis of the outcomes in one of the two tasks, randomly determined after all participants had finished participating in both games. The only task that a subject received any feedback for was the one for which she was paid. Due to our chosen experimental design we cannot explicitly test for order effects; however, paying for one game with no feedback between games, minimizes such a concern. Paying for one game also helps reduce wealth effects.

3. Results

3.1 Descriptive Statistics

We start our analysis by discussing sample descriptives. Table 1 presents average socioeconomic characteristics for our sample. The average participant in our experiment is 24 years old and about 50% of them are married. The likelihood of secondary school completion is low with only 43% of women completing ten grades of schooling. Our sample is primarily Hindu (97%) and more than

one-third (37%) of the women in our sample have some prior experience in tailoring and stitching. Our subjects reside in households where average household monthly income is approximately Rs 7000 and when compared to average income reported in the 2005 Indian Human Development Survey, these households would lie between the 1^{st} and the 5^{th} percentile of the income distribution in urban India and would be identified as poor.

Table 2 presents the descriptive statistics for the intrinsic traits. Participants on an average allocate Rs 25 (50% of their endowment) to the risky option in the *investment game* (indicator of risk tolerance of participants). On an average 36% of the participants choose to be paid according to the competition rate (indicator of competitive behavior). As in Niederle and Vesterlund (2007), we find participants in our sample to be overwhelmingly overconfident (as measured by the confidence ratio). Their ex-ante assessment of number of bags filled is much more than the actual number of bags filled in the real effort task.

Several other points are worth noting about our sample. First, women who choose the competition-rate compensation method are significantly more likely to place themselves at a higher rank within the group (correlation coefficient is 0.18 with a p-value = 0.007). This is not surprising, since in the competition-rate compensation method they will earn a positive amount only if they fill more bags than their competitor, and it seems logical to expect that a woman is likely to choose this method of compensation only if she believes herself to be better than others in the group. The choice of the compensation method is however not affected by their expectation of the number of bags they are likely to fill in the allotted one minute (the measure of absolute confidence).

Second, while the average number of bags filled in one minute is significantly higher for women choosing the competition-rate compensation method (2.06 compared to 1.81, p-value = 0.015), there is no difference in the between-subject variance in the number of bags filled in the allotted one minute depending on the compensation method chosen. Therefore there is no evidence that sorting based on choice of the payment mechanism is efficiency increasing unlike in Eriksson

et al. (2009), where the mean of effort is higher and the variance lower with a competitive wage scheme.

Finally, choosing the competition rate as opposed to the piece-rate payment scheme can potentially be a risky alternative since the payoff in this case depends on relative performance and not absolute performance. One could view this as a reflection of participants' attitude towards strategic risk in the *competition game*. Competitive women would have invested more in the risky asset if strategic risk were to be positively correlated with exogenous risk, that is, the kind of risk the subject faces in the *investment game*. To examine this, we test for differences in the amount allocated to the risky asset in the *investment game* by type of wage scheme chosen in the *competition game* and find that on an average women chose to invest Rs 25 in the *investment game* and this does not differ by their decision (piece rate or competitive rate) in the *competition game* (difference in risk amount = 0.69 and p-value = 0.65). While there is positive correlation between individual's attitude towards exogenous and strategic risk, i.e., decisions in the *investment game* and the *competition game*, this is not statistically significant (p-value = 0.65). Additionally the choice in the *investment game* is in response to an endowment, while the choice in the *competition game* is in response to earnings from a real effort task. Recent research suggests that individuals behave differently depending on whether the money is allocated to them or whether they earn it (for example, see Cherry et al (2002), Dasgupta (2011), Erkal *et al.* (2011) and the references cited therein). Therefore, even though the relative returns from choosing the riskier alternative were identical in the two games, the lack of significant correlation between the two games suggests that behavior is game specific in the experiment.

3.2 Regression Results

The sample is deliberately skewed in favor of the applicants and does not represent the true population. To address this bias, we follow the weighed endogenous sampling maximum likelihood

(WESML) estimation technique proposed by Manski and Lerman (1977). This estimation strategy requires that the true population proportions be known for both the applicants and the non-applicants. Fortunately, we have data on both the sample and the population proportion of applicants and non-applicants. Using these proportions, the WESML estimator applies weight = 0.077 for applicants and weight = 3.31 for non-applicants.

The weighted probit estimates reported in Table 3 capture the causal effect of the behavioral variables on the decision to apply to the program, controlling for demographic characteristics. The marginal effects and robust standard errors are reported in Table 3. Results corresponding to different specifications are presented in columns (1) – (3) in Table 3. In column (1), we include only socio-economic characteristics obtained from the survey. In column (2) we include the proportion of endowment allocated to the risky asset in the *investment game*, choice of the competitive wage scheme in the *competition game* and actual performance in the real effort task (number of bags filled in the allotted one minute) as additional controls. In column (3) we also control for the confidence ratio. Hence this specification includes the full set of socioeconomic characteristics and intrinsic traits, and is our preferred specification. Additional specifications to examine the robustness of our results are discussed in Section 3.3.

The results from the full specification in column (3), Table 3 show that applicants and non-applicants differ in terms of a number of socioeconomic and demographic characteristics. Younger women are more likely to apply to the program. An additional year in age is associated with a 0.3 percentage point reduction in the probability of applying to the program. Women with some prior experience in tailoring and stitching are almost 30-percentage points more likely to apply to the program.

Applicants are likely to belong to less poorer families and to families with a higher dependency ratio (defined as the ratio of the number of children under 5 in a household and the number of adult females in the household). In our sample, a Rs 1000 increase in household income

increases the probability of applying to the program by 3-percentage points. Majority of our applicants hope to set up small businesses. This would typically require initial capital investment. It is therefore no surprise that most of the applicants were from a relatively richer household (the targeted sample are all disadvantaged, so *richer* is only defined in a relative sense). Dependency ratio can influence choice in two different ways. First, since women are typically the primary care-givers for children, a woman belonging to a household that has relatively more children compared to the available adult women faces a substantially higher time-cost of participating in the training program. In this case an increase in the dependency ratio might result in a participant substituting away from the training program, and hence reduce the probability of applying to the program. On the other hand, it is often the case that in our subject-pool, it is the woman's responsibility to find the resources required to send children to school or take them to a doctor/hospital when they are sick. Most applicants report that the primary reason for applying to the program is to increase future income. An increase in dependency ratio would put more pressure on the adult woman to seek out additional ways to enhance household income. We would then expect a positive relation between the increase in the dependency ratio and the probability of applying to the program due to the underlying income-earning motive. Which of the two effects is stronger is an empirical question. In our sample, we find that the income earning effect dominates the substitution effect. Finally, being a member of a Rotating Savings and Credit Association (ROSCA) is associated with a 3-percentage point increase in the probability of applying to the program.[11]

Turning to the effects of the behavioral traits, we find that women who have a greater tolerance for risk, i.e., those who choose to invest more in the risky asset in the *investment game*, and prefer a competitive wage scheme are more likely to apply to the vocational training program.

[11] Anderson and Baland (2002) propose an explanation of membership of ROSCAs in Kenya (similar to chit funds in India) based on conflictual interactions within the household. In their paper, participation in a ROSCA is a strategy a wife employs to protect her savings against claims by her husband for immediate consumption. So membership in a ROSCA could be viewed as a measure of bargaining power of the woman.

A one-percent increase in the proportion of the endowment allocated to the risky asset in the *investment game* is associated with a 6-percentage point increase in the probability of applying to the program (see column (3), Table 3). Women who choose the competitive wage scheme in the *competition game* are 3-percentage points more likely to apply to the program. A unit increase in the confidence ratio is associated with a 0.3-percentage point increase in the probability of applying to the program, though this effect is not statistically significant. The effects of risk tolerance and competitiveness persist even when we control for participants' confidence levels. These are large conditional effects, controlling for a rich set of socioeconomic characteristics. The behavioral variables are also always jointly significant in explaining applicant status.

3.3. Robustness

We estimate several alternative specifications to ensure that the findings presented in Table 3 are robust. We discuss these robustness tests in this section and Table 4 reports the corresponding results, as before, using the WESML estimation technique. First, in columns (1) and (2) in Table 4 we include alternative measures of confidence: self-assessment of the number of bags they could fill in the real effort task (column (1)) and perceived rank within the group (column (2)). In these two specifications we do not include confidence ratio in the set of explanatory variables. A unit increase in the number of bags the woman expects to be able to fill is associated with a 0.2 percentage points increase in the probability of applying for the program. Similarly a unit increase in the perceived rank within the group is associated with a 0.7 percentage point increase in the probability of applying for the program. Though in neither case is the effect statistically significant. The rest of the results remain qualitatively similar.

Second, in column (3) we include time preference as an additional control (the rest of the explanatory variables are as in column (3) in Table 3). The rate at which an individual discounts future pay-offs can influence the decision to be an applicant to the program. Returns from a training

program (and indeed from all educational programs) require a gestation lag to bear fruit (see for example Mullainathan (2005) for a discussion on how time preference can shape schooling decisions). It is possible that women who have a higher discount rate for future utility might tend to discount the future returns from the program more heavily and choose not to apply. We capture time preference using a question in our household survey: the respondent is asked to choose between a sure prize of Rs 100 today versus Rs 150 one month from today. The variable *time preference* takes the value of 1 if the respondent chooses Rs 100 today.[12] The results from specification 3 in Table 4 show that while the coefficient of the *time preference* dummy is in the expected direction, it is not statistically significant and the inclusion of this variable does not have any effect on the other explanatory variables (compare column (3) in Tables 3 and 4).

In addition to the specifications reported in Table 4, we conducted a number of other sensitivity checks, which are not reported here given space constraints. In particular, we investigated whether the effects of the intrinsic characteristics are different in economically better-off households? To examine this we constructed a dummy variable (*rich households*), which takes the value 1 if the household income is greater than the mean household income for the sample and 0 otherwise. We interacted the three intrinsic characteristics with this *rich household* dummy and included these interaction terms as additional controls. The difference estimate (given by the coefficient estimate of the interaction term) is never statistically significant, indicating that the intrinsic characteristics do not have a differential impact on the likelihood of applying to the program across different income levels. Another specification explored cluster effects. Women in the sample reside in a number of different clusters (within the South Shahdara area of New Delhi).

[12] There are different ways of capturing this *present bias*. Our measure of *present bias* is based on hypothetical choices. In recent years, a number of experiments have been conducted to capture *present bias* using monetary incentives (see for example Harrison *et al.* (2002)). Alternatively membership in ROSCA could be viewed as a commitment device; women who are members of a ROSCA are likely to be less present biased.

To account for common cluster level unobservables we include cluster fixed effects with robust standard errors. We find that the magnitude and signs of all the coefficient estimates are very similar to those obtained from the estimates reported in column (3) in Table 3. These results are available on request.

4. Discussion

This paper uses a novel design that combines household survey data with unique experimental data to shed light on the determinants of self-selection into vocational training programs. Identifying the mechanisms underlying self-selection into training programs can be important for multiple reasons: It can enable us to determine which observable characteristics, individual or at the household-level, matter in encouraging the targeted population to apply for training programs. Identifying such determinants, can help policy makers decide on the possible roles of subsidies/transfers in the application process (see Heckman (1992)) to promote participation, since low participation to such programs can potentially underestimate the overall benefits of the program. Second, very little is known about the individual-level intrinsic traits such as differences in preferences, inherent competitiveness, and abilities, that can potentially influence self-selection into programs. For example, individuals who choose to apply to training programs might be more competitive and confident than the average non-applicant, and ignoring such intrinsic characteristics can result in biased program effects.

Using our unique approach to identify behavioral traits along with the more standard demographic and socioeconomic characteristics, we find that younger women, with prior experience in stitching and tailoring, belonging to households with higher income and dependency ratio, and who are members of a rosca, have a significantly higher probability of applying to the stitching and tailoring training program. The results from our behavioral experiment reveal in

addition, that women who have a greater tolerance for risk and are more competitive have a higher propensity to apply for the specific training program.

There are several implications of our results. First, the underlying differences in traits can potentially explain the heterogeneous policy outcomes often observed in the field: for example, why a program succeeds in certain neighborhoods and not in others, even after controlling for a range of observable characteristics. For instance recent work by Maitra and Mani (2012) find that women who have a greater tolerance for risk, are more competitive and are more confident of their relative ability as assessed prior to the training indeed exhibit significantly better labor market outcomes post-training. The magnitudes of the differential effects are also large.

Second, identifying the specific sources of behavioral traits can help researchers address the selection issue better by specifically controlling for these characteristics instead of including them in the black box called unobservables.

Finally, the inclusion and better measurement of these behavioral traits can inform policy makers how to devise and advertise new policies aimed at improving participation rates. For example, for an observed level of risk attitudes, a policy can be promoted such that the risk associated with its returns are better articulated, thereby influencing the probability weights used by individuals to calculate their expected payoffs. It is important to note here that there might be other behavioral traits as well, that can differ between applicants and non-applicants. Examining those are beyond the scope of this paper and is left for future research. However, using our approach one can possibly envision, policy-makers designing perfectly targeted individual-specific programs where a large set of identified behavioral determinants and their effects are incorporated in the implemenation stages of the programs.

Table 1: Summary Statistics on Socioeconomic Characteristics

Variables	Full sample (1)
Age in years	24.57
	(6.69)
Married	0.49
	(0.50)
Completed secondary school (class 10 in India)	0.43
	(0.49)
OBC	0.10
	(0.30)
Hindu	0.97
	(0.16)
Experience in stitching/tailoring	0.37
	(0.48)
Happy at home (=1 if very happy and 4 if very unhappy)	1.66
	(0.91)
Household Income excluding Respondent Income	6970.29
	(6624.57)
Dependency Ratio (number of children under 5 years divided by the number of adult females)	0.31
	(0.51)
Member of a ROSCA	0.10
	(0.31)
Sample size	204

Notes: Standard deviations are reported in parenthesis.

Table 2: Summary Statistics on Behavioral Outcomes

Variables	Full sample (1)
Amount allocated to the risky option in *Investment Game*	24.86
	(10.69)
Proportion allocated to the risky option in the *Investment Game*	49.73
	(21.39)
Self-assessment of number of bags they could fill in Competition Game	4.35
	(2.36)
Perceived rank within group (1 = Lowest, 5 = Highest)	4.05
	(1.01)
Competitive wage scheme in *Competition Game*	0.36
	(0.48)
Number of bags actually filled	1.89
	(0.71)
Confidence ratio	2.65
	(1.96)
Sample Size	204

Notes: Standard deviations are reported in parenthesis.

Table 3: Determinants of Applicant Status: Marginal Effects from a Weighted Probit Regression

	(1)	(2)	(3)
Age	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
Completed secondary school	-0.015	-0.013	-0.015
	(0.012)	(0.010)	(0.010)
OBC	-0.022**	-0.020***	-0.021***
	(0.010)	(0.008)	(0.007)
Hindu	0.018*	-0.001	-0.005
	(0.011)	(0.017)	(0.020)
Experienced in tailoring and stitching	0.186**	0.283***	0.287***
	(0.075)	(0.079)	(0.079)
Family income excluding own income	0.030***	0.030***	0.029***
	(0.010)	(0.009)	(0.009)
Married	-0.006	-0.002	-0.001
	(0.019)	(0.014)	(0.013)
Dependency Ratio	0.035**	0.027**	0.025*
	(0.015)	(0.013)	(0.013)
Happy at family	-0.010	-0.005	-0.006
	(0.007)	(0.005)	(0.005)
Member of a ROSCA	0.016	0.033	0.036
	(0.042)	(0.047)	(0.050)
Number of bags actually filled		-0.023***	-0.020**
		(0.008)	(0.008)
***Intrinsic Traits (Behavioral Outcomes)*:**			
Proportion Allocated to Risky Asset in		0.058**	0.062**
Investment Game × 10^{-2}		(0.024)	(0.131)
Competitive wage scheme in		0.030*	0.030**
Competition Game		(0.016)	(0.015)
Confidence Ratio			0.003
			(0.002)
Joint Significance (Behavioral variables)		18***	19.57***
Sample Size	204	204	204
Predicted Probability	0.02	0.018	0.018
Pseudo R-squared	0.22	0.28	0.29
Log Likelihood	-34.01	-31.06	-30.85

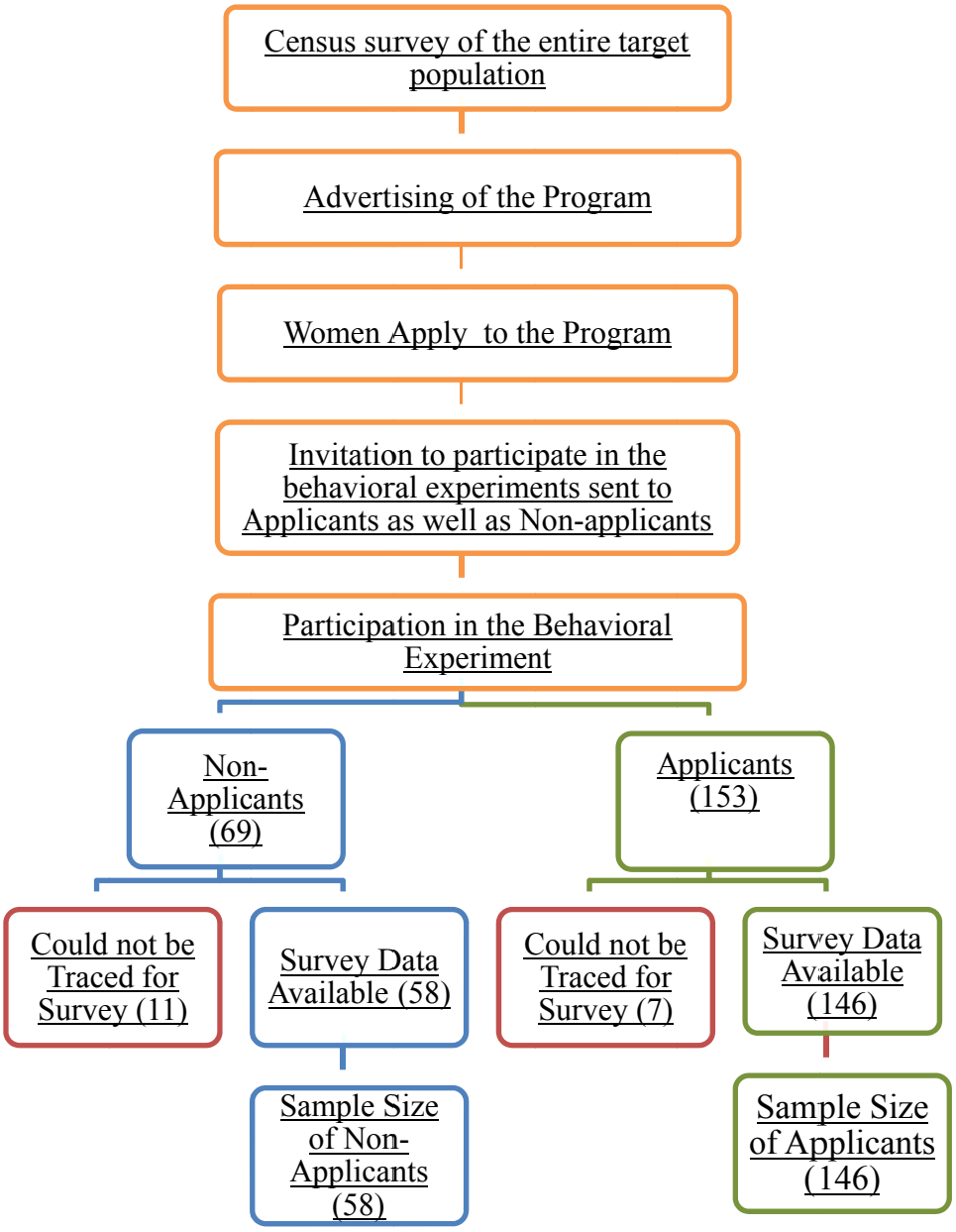
Notes:
Marginal Effects from a weighted Probit regression are presented. The dependent variable is a binary variable – whether the woman applied for the training program.
Robust standard errors in paretheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Determinants of Applicant Status: Robustness (Marginal Effects from a Weighted Probit Regression)

	(1)	(2)	(3)
***Observables* (*Socioeconomic Characteristics)*:**			
Age	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
Completed secondary school	-0.014	-0.015	-0.015
	(0.010)	(0.010)	(0.010)
OBC	-0.020***	-0.019***	-0.021***
	(0.008)	(0.007)	(0.007)
Hindu	-0.004	0.004	-0.012
	(0.020)	(0.014)	(0.024)
Experienced in tailoring and stitching	0.296***	0.286***	0.268***
	(0.078)	(0.081)	(0.079)
Family income excluding own income × 10^{-4}	0.030***	0.033***	0.029***
	(0.009)	(0.009)	(0.009)
Married	-0.001	-0.004	0.006
	(0.014)	(0.014)	(0.013)
Dependency Ratio	0.026**	0.029**	0.022*
	(0.013)	(0.013)	(0.012)
Happy at family	-0.005	-0.004	-0.005
	(0.005)	(0.005)	(0.005)
Member of a ROSCA	0.033	0.043	0.048
	(0.048)	(0.054)	(0.055)
Number of bags actually filled	-0.024***	-0.024***	-0.020**
	(0.008)	(0.008)	(0.008)
***Intrinsic Traits (Behavioral Outcomes)*:**			
Proportion Allocated to Risky Asset in *Investment Game* × 10^{-2}	0.061**	0.052**	0.062**
	(0.025)	(0.024)	(0.024)
Competitive wage scheme in *Competition Game*	0.030*	0.028*	0.030**
	(0.015)	(0.015)	(0.015)
Self-assessment of number of bags they could fill in *Competition Game*	0.002		
	(0.001)		
Perceived rank within group (1 = Lowest, 5 = Highest)		0.007	
		(0.005)	
Time Preference			-0.020
			(0.015)
Confidence ratio			0.003
			(0.002)
Joint Significance (Behavioral variables)	18.89***	20.49***	21.68***
Sample Size	204	204	204
Predicted Probability	0.018	0.017	0.018
Pseudo R-squared	0.29	0.29	0.30
Log Likelihood	-30.92	-30.64	-30.45

Marginal Effects from a Weighted Probit regression are presented. The dependent variable is a binary variable – whether the woman
applied for the training program.
Robust Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Subject Pool



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Appendix 1:

Table A1: Summary Statistics of Behavioral Outcomes by Missing and Non-missing Survey Data

	Sample without missing survey data (1)	**Sample with missing survey data (2)**	**Difference [1-2] (3)**
Amount allocated to the risky option in *Investment Game*	24.86	24.44	0.42
	(10.69)	(10.56)	(2.63)
Proportion allocated to the risky option in the *Investment Game*	49.73	48.89	0.84
	(21.39)	(21.11)	(5.25)
Self-assessment of number of bags they could fill in *Competition Game*	4.35	4.33	0.02
	(2.36)	(2.58)	(0.59)
Perceived rank within group (1 = Lowest, 5 = Highest)	4.05	3.89	0.16
	(1.01)	(1.18)	(0.25)
Competitive wage scheme in *Competition Game*	0.36	0.28	0.08
	(0.48)	(0.46)	(0.12)
Number of bags actually filled	1.89	2.00	-0.11
	(0.71)	(1.03)	(0.18)
Confidence Ratio	2.64	2.44	0.20
	(1.96)	(1.39)	(0.47)
Sample Size	204	18	

In columns 1 and 2, standard deviation reported in parenthesis and in column 3.
Standard error in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.
None of the differences reported in column 3 are statistically significant.

Table A2: Non-Response in Surveys: Marginal Effects from Probit Regressions

Covariates	Non-response
Proportion allocated to the risky option in the *Investment Game* ($\times 10^{-2}$)	-0.033
	(0.099)
Competitive wage scheme in *Competition Game*	-0.024
	(0.049)
Number of bags actually filled	-0.011
	(0.036)
Confidence ratio	-0.008
	(0.012)
Applicant status (=1 if applicant)	-0.425
	(0.349)
Proportion allocated to the risky option in the *Investment Game* ×	0.081
Applicant status	(0.147)
Competitive wage scheme in *Competition Game* × Applicant status	-0.006
	(0.066)
Number of bags actually filled × Applicant status	0.046
	(0.058)
Confidence ratio × Applicant status	0.015
	(0.018)
Observations	222

Notes: Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1. None of the coefficients are statistically significant.
Non-response is defined as a dummy variable which takes a value 1 if missing information on household questionnaire data and zero otherwise. Robust standard errors in parenthesis.

Supplementary Material
Appendix 2: English Version of the Subject Instructions

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.

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.

Decision Sheet for the Investment Game

Player ID #: _____

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.

Decision Sheet for the Competition Game

For experimenter use only

Paired Player ID #:

end of 1

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ler Option
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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

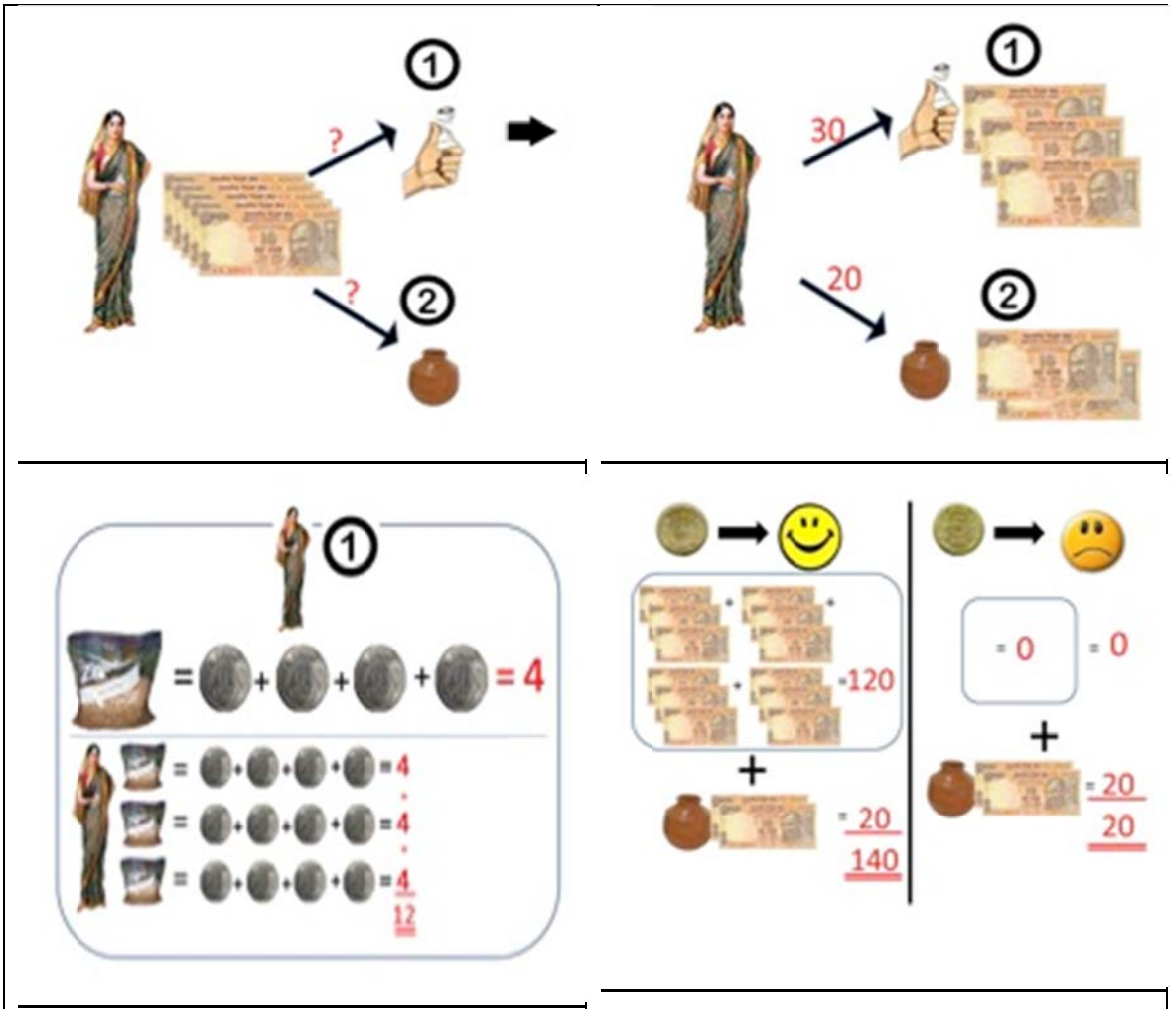

Figure A2: Visual slides used in the *Competition Game* in conjunction with the oral instructions