Targeting High Ability Entrepreneurs Using Community Information: Mechanism Design In The Field

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Abstract

The impacts of cash grants and access to credit are known to vary widely, but progress on targeting these services to high-ability, reliable entrepreneurs is so far limited. This paper reports on a field experiment in Maharashtra, India that assesses (1) whether community members have information about one another that can be used to identify high-ability microentrepreneurs, (2) whether organic incentives for community members to misreport their information obscure its value, and (3) whether simple techniques from mechanism design can be used to realign incentives for truthful reporting. We asked 1,380 respondents to rank their entrepreneur peers on various metrics of business profitability and growth and entrepreneur characteristics. We also randomly distributed cash grants of about $100 to measure their marginal return to capital.

We find that the information provided by community members is predictive of many key business and household characteristics including marginal return to capital. While on average the marginal return to capital is modest, preliminary estimates suggest that entrepreneurs given a community rank one standard deviation above the mean enjoy an 8.8% monthly marginal return to capital and those ranked two standard deviations above the mean enjoy a 13.9% monthly return. When respondents are told their reports influence the distribution of grants, we find a considerable degree of misreporting in favor of family members and close friends, which substantially diminishes the value of reports.

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Finally, we find that monetary incentives for accuracy, eliciting reports in public, and cross-reporting techniques motivated by implementation theory all significantly improve the accuracy of reports.

1 Introduction

Identifying high-ability microentrepreneurs is essential to generating sustainable economic growth among the poor. Empirical studies from Sri Lanka, Ghana, and Mexico in which microenterprises are randomly assigned to receive small cash grants find average marginal returns to capital between 4.5% and 25% per month (De Mel, McKenzie, and Woodruff (2008), Fafchamps, McKenzie, Quinn and Woodruff (2014), McKenzie and Woodruff (2008)). But these estimates of average marginal returns mask substantial heterogeneity amongst firms. For instance, while the average marginal return to capital among male business owners in Sri Lanka was 4.5% per month, the quantile treatment effect ranges from 0% - 32.5% per month. However, the authors are unable to predict marginal returns to capital on the basis of the observable information they collected. Thus the problem of identifying high-growth potential microentrepreneurs remains of first order importance for institutions interested in providing capital to microfirms.

In the absence of asymmetric information, finding and empowering potential high-growth entrepreneurs would be easy. The presence of screening costs, however, poses a serious barrier to providing finance (Stiglitz and Weiss, 1981). Though credit registries for small scale borrowers are emerging in many parts of the developing world, they are still very much in their infancy, and asymmetric information continues to plague the informal lending industry in these areas. As such, there is a serious need for tools that can overcome hidden information problems and accurately target capital to firms that can use it well. Low-cost, reliable tools to discern high-growth potential firms may allow financial institutions to extend larger or more flexible loan contracts to these entrepreneurs, fostering economic development while maintaining profitability (Field, Pande, Papp, and Rigol 2013). Additionally, as evidence accumulates about the substantial positive impacts of unconditional transfers, there is rising interest among governments and non-profits in disbursing grants to poor households (e.g. Haushofer and Shapiro 2015; Blattman, Fiala, and Martinez 2013; De Mel, McKenzie, and Woodruff 2008; Fafchamps, McKenzie, Quinn and Woodruff 2014; McKenzie and Woodruff 2008). Enabling targeting on the basis of micro-entrepreneurs' ability might increase the viability and development impact of these initiatives.

What progress has been made thus far in targeting high-growth entrepreneurs? Existing research focuses on developing metrics for assessment of entrepreneurs' individual characteristics. The earliest literature uses family characteristics and personal background; De Mel, McKenzie, and Woodruff (2008) use tests of cognitive ability and personality which evaluate focus, competitiveness, etc.; Khwaja and Klinger (2013) develop a psychometric test that analyses ethics and character, intelligence, attitudes and beliefs, and business skills. Yet, as De Mel, McKenzie, and Woodruff (2008) recognize, most measures of entrepreneurship have been developed using high-income country entrepreneurs who may face vastly different constraints from their developing country counterparts. Such tests might thus be a poor metric of what makes a successful entrepreneur in the developing world.

We report on experimental evidence that suggests community information can be used to target high growth entrepreneurs. We pursue this case along two dimensions. First, we document that community members have valuable information regarding the entrepreneurial characteristics of their peers. Second, we establish that, while the natural inclination of community members is to distort their reports in favor of their family and friends, an array of simple techniques from mechanism design theory are effective in realigning community incentives towards truthfulness, substantially increasing the value of reports.

Specifically, we asked community members to rank their peers on various metrics of business growth potential (such as profits and marginal return to capital), borrower reliability (probability of a late payment or default), and other key firm characteristics. We then randomly allocated grants to some of these firms to induce growth. At the time of eliciting rankings we varied whether respondents were told their reports would influence the likelihood that their peers would receive grants. This induced an organic incentive to shift reports in favor of family and friends. We then cross randomized various mechanisms to encourage truth-telling: paying respondents for the accuracy of reports using peer prediction mechanisms, eliciting reports in public or private, and using cross-reporting techniques to infer which peers each community member was most likely to favor.

We find that respondents have valuable information about one another and that peer reports contain residual information that is not predicted by observable household and business-level characteristics. Using baseline data, we find respondents are able to predict one anothers' household income, value of household assets, business profits, work hours, medical expenses, and score on a digit span memory test. On average, a 1 percentile increase in the average rank is associated with a 0.23 percentile increase in the outcome variable. Other than work hours, respondents continue to predict these outcomes even after controlling for a plethora of easily verifiable and also harder-to-observe demographic, household, and business characteristics. Thus ranks contain valuable residual information about community members beyond that which a loan officer may be able to observe.

More surprisingly, community members were also able to predict the impact of cash grants. Using follow up data on business profits for those who randomly did and did not receive our grants, we find that community members are well able to predict which of their peers enjoy high marginal return to capital. Though on average the impact of our grant on business profits was modest, those who were ranked one standard deviation above the mean enjoyed an 8% monthly marginal return to capital, and those who were ranked two standard deviations above the mean enjoyed a 14% monthly marginal return to capital. The value of community rankings is not diminished by the inclusion of a variety of respondent and business characteristics.

However, we also find that community members distort their reports when they are told that their information will influence the distribution of grants. The correlation between community reports and true outcomes is on average 40% lower, implying a significant amount of manipulation in reports and severely limiting their usefulness. We see manipulations directly when examining how peers rank themselves, their family members, and people who are identified by group members as a respondent's close friends. Although respondents manipulate their reports, we find that our interventions can significantly improve their accuracy. Giving respondents monetary incentives when they report in private increases the accuracy of reports by 75%. Asking respondents to report in public increases the accuracy of reports by 89%. As with manipulations, we also document direct evidence that incentives and public reports make it less likely that respondents favor themselves, their family members, or their close friends in the group.

Our primary motivation is to identify entrepreneurs with the highest growth potential (as measured by marginal returns), since allocating grants to these individuals may be efficient. However institutions targeting the poor may have a variety of welfare priorities. We demonstrate that community information can be used to allocate resources using other types of selection criteria such as wealth, health burden, and average business profitability. By additionally asking community members how they would prefer to allocate the grants, we also estimate how communities themselves trade-off efficiency and redistribution.

This project builds on work done by Rigol and Roth (2016). In a lab in the field experiment, we tested the effectiveness of two different types of monetary incentive schemes to elicit truthful reporting: one that relies on ex-post verification of outcomes and one *peer prediction* incentive scheme that relies on a correlation in community reports to pay respondents based only on their own reports and the contemporaneous reports of their peers. The lab experiment took place in a controlled setting in which farmers were presented with a simple and transparent trade off - to lie or to tell the truth - when asked to report on their farmer peers in the same village. Reassuringly, we find that the two rules are equally effective in eliciting truthful responses. However, relative to a payment rule that relies on ex-post verification of outcomes, peer prediction schemes are much easier to implement for a variety of reasons we discuss below. So, in the present experiment we paid our respondents using the Robust Bayesian Truth Serum of Witkowski and Parkes (2011), a peer prediction scheme identified to be particularly effective in our lab experiment. To our knowledge, this is the first large-scale field experiment to use a peer prediction mechanism to incentivize respondents in a developing country setting and we are among the first to use peer elicitation outside the lab.[1]

Our work adds to the literature that studies the use of community reports for targeting. Alatas et. al. (2012) explore the value of community information in targeting the poor in Indonesia. The authors ask villagers to select the village's poorest members who are then eligible for a limited number of government transfers. They find that communities have information about the consumption of their members over and above the information contained in the standard proxy means tests. Relative to theirs, our project furthers the understanding of community targeting along several directions. First, we quantify the value of community information over a much broader set of characteristics. Alatas et. al. (2012) measured community knowledge of wealth, which may be easily observable to community members and hence an easy test case. So our ability to target high growth potential entrepreneurs along arguably harder to observe metrics is an encouraging step. Second, we identify that incentives to report truthfully are of first order importance in the elicitation of community information. While Alatas et. al. do examine whether elite capture poses a problem for community reporting, we take a substantially closer look by manipulating respondents' incentives to misreport their information and identifying the likely beneficiaries of each respondent's manipulations. We conclude that there is a substantially larger amount of misreporting than Alatas et. al. are able to identify. Finally we identify a variety of mechanisms effective in encouraging truth-telling, substantially improving the informativeness of community reports.

Closely related is Bryan, Karlan, and Zinman (2014), who randomly give existing borrowers incentives for referring a new borrower and for the repayment outcomes of their referral. They find strong peer enforcement effects but no evidence of peer selection effects in excess of the screening that the bank already does. Their interpretation is that the original clients do not have useful information regarding the individuals they refer beyond the information available to the bank. In contrast to our study, theirs is conducted in South Africa, which has a well-functioning credit bureau. In settings without a well-functioning credit bureau, one might expect community information to be of considerably more value.

[1]The notable exception is John, Loewenstein, and Prelec (2012) who use BTS to measure the prevalence of questionable research practices among academic psychologists.

The rest of the paper proceeds as follows. Section 2 describes our experimental setting and design, Section 3 describes the data, Section 4 discusses our key specifications, Section 5 discusses our results, and Section 6 concludes. All tables are relegated to the appendix.

2 Description of the Experiment

2.1 Sampling and Study Population

Our intervention took place in Amravati, a city of about 550,000 persons in the state of Maharashtra, India. Within Amravati, we selected nine neighborhoods that each had an abundance of microentrepreneurs.[2] In September 2015, we conducted a complete door-to-door census of these neighborhoods, which encompassed 5,573 households. Each person in the household who was engaged in self-employment activities responded to our census. Every respondent reported the total value of their enterprise's durable assets and inventory (excluding the value of land and buildings), their total number of permanent employees, and their business sector. We identified a sample of 1,576 households that had at least one enterprise with (1) USD 1,000 or less in total working and durable capital and (2) no paid, permanent employees. We excluded farmers and self-employed service persons, such as domestic helpers and teachers. Our selection follows the same criteria as recent "cash-drop" experiments (see e.g. De Mel, McKenzie, and Woodruff 2008).[3]

In October 2015, we recruited these households to participate in our study. At the time of recruitment, households were informed that we would be conducting a project to study entrepreneurship and business growth and that some households would be randomly selected to receive a USD 100 grant. Ultimately, 1,380 households agreed to participate in the study. We conducted baseline surveys of these households in December 2015 - April 2016. These 1,380 households were then organized into groups of five based on geographic proximity.[4] We created a total of 277 groups across all neighborhoods. After all respondents in a neighborhood had completed the baseline survey, the groups in that neighborhood were invited to participate in an activity at the community hall, where the lottery would be conducted. At the hall, respondents completed the ranking activity (described in section 2.2) and, upon its completion, a public lottery was held and the winners of the USD 100 grant were announced.

[2] The neighborhoods are: Belpura, Vilash Nagar, Mahajan Pura, Akoli, New Saturna, Old Saturna, Wadali, and Pathan Chawk.

[3] If there were multiple business owners in the household, we selected households that had less than USD 2000 in combined business capital.

[4] We organized respondents into groups that would minimize the geographic distance between study households. The total number of respondents per neighborhood was not always a multiple of 5, so some groups had 4 or 6 clients.

2.2 Experimental Design

The aim of this study is to investigate whether knowledge that community members hold about one another can be useful for making decisions regarding the allocation of business grants. We ask a number of related questions. First, do community members have accurate knowledge regarding their entrepreneur peer's business growth potential? To address this question, we instructed community members rank their peers across several personal and enterprise characteristics that are predictive of business growth (such as profits and marginal returns to capital) and borrower reliability (probability of a late payment or default). We compare community reports to business and household outcomes elicited in a baseline household survey to determine the predictiveness of these measures. To assess whether respondents can predict one another's marginal return to capital, we utilize follow up data on business profits for those who did and did not randomly receive our grants (discussed below). Importantly, the baseline household survey was conducted *before* respondents knew they would be ranking their peers. In addition, we assess whether respondents' reports provide useful information beyond what could be gathered through measurement of observable household and business characteristics or psychometric tests.

Community knowledge–even if accurate– is only useful for allocative decision-making if those collecting the community information can be reasonably confident that they will gather *truthful* reports. But eliciting reliable information from community members is not a straightforward task. If respondents know that information they provide about their peers will be used for targeting resources, they may distort their reports to favor persons with whom they have stronger social ties. Rigol and Roth (2016) find that, in a low stakes lab experiment, farmers in a village setting distort their reports in favor of family and close friends. In this paper, we measure the level of distortion that occurs when stakes are high: half of our study participants, whom we refer to as being in the *revealed* treatment, were told that their reports would influence the probability that their peers would receive cash grants of USD 100 (grants are randomly allocated to a third of our sample).[5] This is a shock approximately equivalent to one and a half months of business profits for the average business in our sample. The control group, whose rankings do not influence grant allocation, provide an estimate of the quality of information embedded in communities in the absence of incentives to distort responses. Comparing the informativeness of the two groups' reports establishes whether community members strategically misreport information to affect allocative decisions (i.e. the possibility of receiving a grant).

If distortions can be corrected, their possibility does not eliminate the utility of community reports.

[5]The treatment name, *revealed,* refers to community reports being revealed to a principle who would make use of them.

We experimentally test whether two simple techniques from mechanism design theory can effectively correct distortions: (1) asking respondents to report in public or in private and (2) offering monetary incentives for the accuracy of reports. Screening mechanisms that are common in practice and that rely on community information (such as joint liability and participatory rural appraisals) require public reporting. If respondents fear negative social repercussions of truth-telling, mechanisms that involve public reporting may discourage reliable reporting. On the other hand, if respondents fear that misreports will cause them to be perceived as liars by their peers, public reports may lead to more truthful elicitation. To test whether reporting in public would increase or decrease the accuracy of reports, our respondents were randomly assigned to make reports in public (ranks were visible to the entire group) and or private (group members did not see ranking reports). The difference in the accuracy of reports between these two groups reveals whether privacy is an important criterion for elicitation of peer information.

In order to test whether monetary incentives impact accuracy of reports, we randomly assigned respondents to either receive no monetary incentive or to receive an incentive delivered via the *Robust Bayesian Truth Serum* of Witkowski and Parkes (2011). We discuss the details of the payment rule and our rationale for choosing it in the following section.

In addition to these treatments, we included two non randomized features of the elicitation exercise. First, we asked all respondents to report about their peers using two different methods: Ranking the five members of the group relative to one another, and separately placing the five members of the group in quintiles relative to the entire neighborhood. The relative ranking may be a coarse measure of the group's knowledge but has the advantage that it induces a zero-sum game in the sense that elevating the status of any member necessarily lowers the status of another, which may deter misreporting. On the other hand, quintiles may contain more information but are also easily manipulable; everyone can be placed into the highest position. The last elicitation mechanism we evaluate is closely related to the cross reporting techniques which play a prominent role in mechanism design and implementation theory (see Maskin, 1979). We asked each respondent to identify who each other respondent was closest to (and hence most likely to lie about) within the group. We also asked each respondent to identify who in the group would give the most accurate ranking.

In summary, our respondents were cross-randomized (at the group level) to give their ranking reports under the following three treatment conditions, for a total of eight treatment cells: Revealed vs Not Revealed (R0 vs R1), Public vs Private (P0 vs P1), and Incentives vs No Incentives (I0 vs I1). Since important predictors of our outcomes of interest such as occupation, religion, caste, and other sociodemographic characteristics are frequently correlated with the area in which respondents live, we stratified our treatments by geographic clusters. Since there are eight treatment groups, we created geographic clusters of eight proximate groups. We then randomized our 8 experimental interventions amongst the eight groups in each cluster.[6] We also randomly selected one-third of our sample to receive USD 100 grants. Selection of grant winners was done via public lottery and is explained in detail in Section 2.4

2.3 Peer Prediction and the Robust Bayesian Truth Serum

Delivering monetary incentives is a somewhat thorny endeavor. A natural inclination might be to pay respondents based on the closeness of their reports with ex-post, objectively measured outcomes. However this approach has several drawbacks. First, it may be costly or even impossible to measure ex-post outcomes; indeed, this is one of the primary motivations of relying on community reports. Second, in cases where it is possible to measure ex-post outcomes via surveys, respondents who know their reports will determine the incentive payments of their peers may lie - a particularly undesirable outcome in a research setting where survey reports are a primary measure of ground truth. Thus we were in need of an alternative method.

As stated above, we delivered monetary incentives via the *Robust Bayesian Truth Serum (RBTS)* of Witkowski and Parkes (2011). RBTS is part of a class of mechanisms known as *peer prediction mechanisms*.[7] The major innovation in these payment rules is that they eschew reliance on ex-post verifiable information to calculate payments. The tradeoff is that these are very complicated payment rules, which rely on comparison between reports from the respondent about the question of interest (which we refer to as *first order beliefs*) and community expectations about the distribution of responses to the question of interest (which we refer to as *second order beliefs*). Roughly speaking, the payment rule rewards respondents who choose responses that are under predicted by the community more than those who choose responses that are over predicted by the community. Details about the implementation of RBTS and the intuition behind its incentive compatibility are relegated to the appendix.

Whether these payment rules are as effective as simpler payment rules for respondents with low numeracy is an empirical question addressed in Rigol and Roth (2016). In a population similar to that of the current study, Rigol and Roth (2016) test the difference between paying incentives via a simple rule based on ex-post accuracy and via a peer prediction payment rule. The first rule was easy for surveyors to explain and for respondents to understand; respondents were paid based on the closeness of

[6] As some neighborhoods had a number of groups indivisible by 8, some clusters have fewer than eight groups. For these clusters, we randomly selected groups to receive one of the eight treatments.

[7] See Prelec, 2004 for a seminal contribution to this literature.

their report and the ex-post realized outcomes. On the other hand, as in our current study, surveyors did not attempt to explain the peer prediction method. They merely elicited first and second order beliefs and asserted to respondents that they would maximize their incentive payments by telling the truth. Rigol and Roth (2016) find that the additional accuracy induced by the simple ex-post incentive is statistically and economically indistinguishable from that induced by the peer prediction method for eliciting information about borrower reliability and entrepreneurial ability.

That upon first exposure, respondents treat the two payments schemes equivalently is reassuring; because in our experiment each respondent was only exposed to our payment scheme once, we feared no loss from relying on the peer prediction scheme. However, if RBTS does not function as theoretically predicted, over a longer period respondents may experiment and discover that truth telling is not their optimal strategy. As such, Rigol and Roth (2016) take the analysis a step further by estimating the higher order beliefs of respondents in the sample. Not only do respondents report as accurately when facing the complicated incentive scheme, but we provided evidence that truth-telling is the strategy that maximizes their subjective expected payments from RBTS. Thus, even over periods of repeated exposure, respondents should continue to tell the truth.[8] Details of this exercise are replicated in the appendix of this paper.

2.4 Ranking Questions and Implementation Method

We asked respondents to rank their peers on a series of dimensions. We collected information about the following criteria: highest level of education attained, marginal returns of the peers' business if she were to receive an Rs.6000 grant, household average monthly income over the past year, projected monthly profits of the peers' business if she were to be given an Rs.6000 grant, total value of household assets, number of hours that their peers work, total household medical expenses in the previous 6 months, loan repayment trouble over the past year, and digit span memory test. For marginal returns, income, profits, and assets, we asked respondents to rank their peers relative to one another as well as to place them in quintiles of the community distribution. For the remainder of the questions, respondents were asked to report only relative ranks. We also asked a subset of groups to report who they thought deserved to receive the grant. We did not provide any criteria for this ranking and asked respondents to choose what they thought were important criteria.

[8]Note, this is the standard to which we hold commonly used mechanisms even in the developed world. For example, medical students are placed in their first residency via the *deferred acceptance algorithm.* They largely largely do not know how it functions but they are assured that truthfully stating their preferences is their best strategy. And because we have theoretical guarantees that stating their true preferences is indeed their best option, the profession feels confident in giving them this advice.

To minimize respondent fatigue, each respondent answered only a subset of these questions. All members of the same group were asked the same ranking questions. In Figure 1 below, we lay out the question randomization structure. Because incentivized groups also had to report second order beliefs in addition to ranks, they were only be asked to answer a total of 7 questions, while non-incentivized groups answered 10. The order of the first 3 questions was always the same and groups were cross-randomized between P0/P1 and I0/I1. The first question was always about education and we primarily intended it to be a practice round. We chose to elicit education quintiles as it allowed us to explain the quintile rankings early. The next two questions were always about marginal return quintiles and relative rankings. In the relevant groups we elicited marginal return information in public and with incentives but we never used marginal return information to affect the distribution of grants because we did not want reports in this dimension to be adulterated by strategic behavior. For questions 4-7, we randomly picked two of three questions: income, assets, and profits. These were cross-randomized with all 3 of our treatments and we elicited both relative rankings and quintiles. Lastly, we randomized questions 8-10 with the public and private treatments only so as to minimize the amount of time respondents spent doing the rankings exercise. Notice that because income, assets, and profits were also in the rotation for Q8-Q10, we have more data on relative rather than quintile rankings for these questions.

After all baseline surveys were completed in a particular neighborhood, groups were invited to a large community hall to conduct the ranking exercise. One group was invited to conduct the exercise at a time. As soon as a respondent arrived in the hall, he or she was seated behind a privacy screen along with a surveyor. The screen was placed both to reassure the respondents in the privacy treatment that their responses would never be visible to others in the group, but also to avoid potential coordination. Respondents were given name cards with the names of all of the peers they would be ranking. To explain complicated concepts and to minimize variation across surveyors in implementation of the treatments, we created animated videos to guide respondents through the exercise. In the videos we explained the definition of a quintile and how to do a quintile ranking, and the definitions of marginal returns to capital, profits, income, and assets.

For groups in the *Public* treatment, although respondents gave their ranks behind their privacy screens, they were asked to move with their rankings to the center of the hall at the completion of each ranking. While the pretext of the move to the center was that the lead surveyor had to record everyone's answers, the purpose was actually that peers could clearly observe each others' rankings.[9]In the privacy treatment, respondents never interacted with other people in the group until all rankings were completed.

[9]Surveyors report that respondents always looked at their peers' rankings.

For those who received the incentives treatment, the videos explained that incentives would be paid for truthfulness of the responses. Respondents were told that people who reported what they truly believed were more likely to receive higher incentive payments than those who did not report what they truly believed. Since RBTS incentive payments also required respondents to report their second order beliefs, the videos were used to explain what second order beliefs were. For each of her peers, each respondent was given 20 orange coins and was asked to place the coins in proportion to how she thought others would rank her peer. Payments were calculated and distributed in private by the surveyor at the end of each ranking question. Groups that did not receive incentive treatments were not asked to report second order beliefs and were not paid for their reports.

At arrival, respondents were told that at the end of the exercise, a lottery would be conducted to choose the grant winners. Each person was given 20 lottery tickets and was told that at the end, all people present in the room would put their lottery tickets inside a basket and the winner would be selected by picking out lottery tickets. For groups in the revealed treatment, after completion of the marginal returns relative rankings, the video explained that for the next 4 rankings they would be able to help determine the lottery winner. Respondents were told the person that was ranked the highest by the group for each round would receive extra lottery tickets. Since we wanted, as much as possible, to keep the probability of selecting the winner balanced across relative ranks, only 1 extra lottery ticket was awarded for winning a round. Respondents, however, did not know how many extra lottery tickets we were awarding each round until all of the ranking exercises were over. At that point the winners were given their extra tickets and the lottery was conducted in the presence of all respondents.

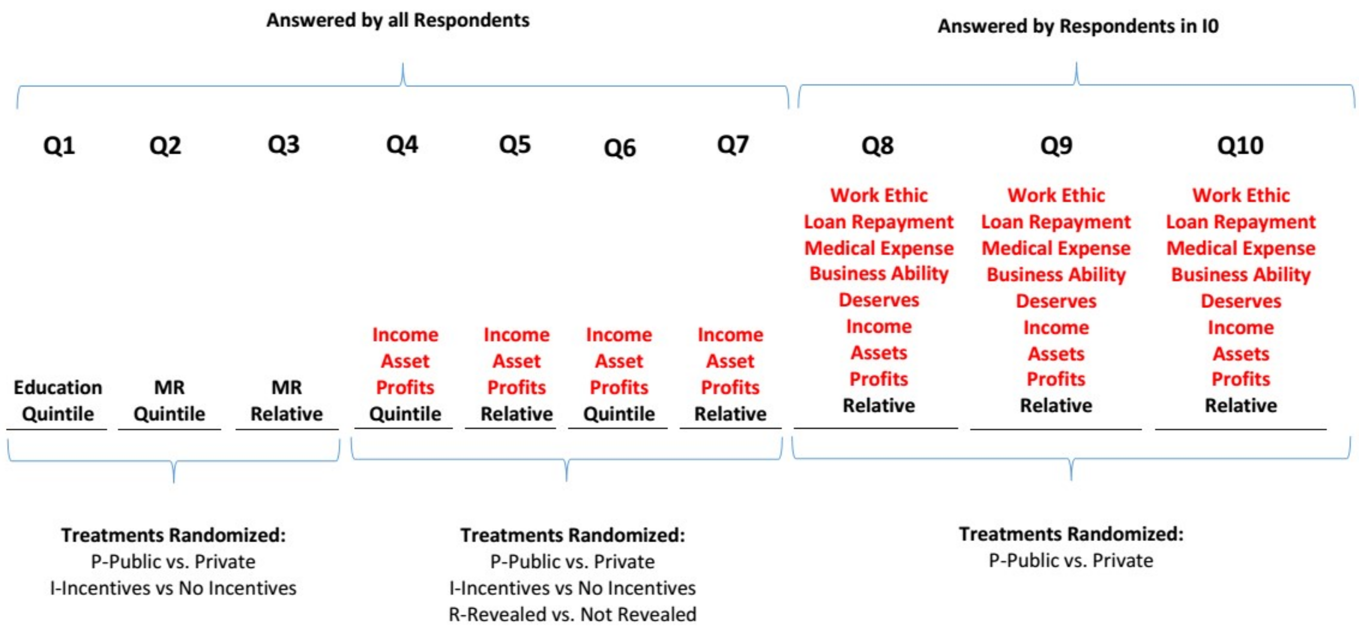

Figure 1: Question Randomization

3 Data and Sample Characteristics

The data for the experiment come from respondent surveys. Baseline surveys were conducted between December 2015 and April 2016 in the privacy of the respondent's home. Respondents were asked detailed information about the economic activities of the household. Specifically, households were asked demographic, occupation, work days and hours, and income information about all household members. They were asked to report about health expenditures and well-being as well as outstanding loans and loan repayment issues. Each business owner was asked to report about the revenues, costs, and profits, inventories, business assets, as well as other aspects of their own businesses. Importantly, all of this was done before respondents knew anything about the ranking exercise. Therefore we have detailed business module of **all** household businesses.

Business owners were asked to complete a psychometric questions section as well as the digit span memory test. A new method that has been utilized to identify credit-worthy entrepreneurs, developed by the Entrepreneurial Finance Lab, largely relies on psychometric questions. Although the precise test used by EFL in commercial institutions is proprietary, it relies on psychometric assessments similar to those used in De Mel, McKenzie, Woodruff (2008), who find that psychometric tests predict whether people will become entrepreneurs versus wage workers in Sri Lanka. We therefore pose the same questions to our respondents, found in the appendix of this paper, as those utilized in their study. Respondents answered each question on a scale of one to five indicating whether they strongly disagreed, disagreed, do not agree or disagree, agreed, or strongly agreed with the statement. The questions are organized according to categories developed by industrial psychologists: polychronicity measures the willingness to juggle multiple tasks at the same time (Bluedorn et al. 1999) ; impulsiveness is a measure of the speed at which a person makes decisions and savings attitudes (Barratt Impulsiveness Scale); tenacity measures a person's ability to overcome difficult circumstances (Baum and Locke 2004); achievement is a measure of satisfaction in accomplishing a task well (McClelland 1985); and locus of control measures a person's willingness to put themselves in situations outside of their control (Rotter 1996).

Households were asked to report on a household assets list in which we asked whether the household owned a particular type of asset, how many pieces, and the current resale value of those assets. Our surveyors were trained to verify that the household actually owned the assets about which they reported.

Baseline data is utilized to validate whether respondents can predict information about their peers. We emphasize that this data was collected before any respondent in a particular neighborhood was aware that they would be asked to report about the household or business activities of their neighborhoods. Once we have collected follow up data, we will test whether respondents' ranks predict future outcomes such as the observed marginal returns to the grant.

Given the selection criteria for the sample described in Section 2.1, it is no surprise that our sample is composed of relatively poor microentrepreneurs. Our sample households report earning approximately Rs.9000 per month in total earnings from all income-generating activities, or USD 5 per day, and have approximately $7000 in total household wealth. As is common in developing countries, poor households diversify across different types of income-earning activities: in 50% of our sample households there is at least one fixed salary or daily wage worker and approximately a fifth of our sample has more than one household business. Medical expenses contribute to a very large portion of household expenses: on average respondents report spending nearly 30% of their monthly earnings on health-related expenditures.

Approximately 60% of the business owners in our sample are male. On average, the firm owners are 40 years old and have about 7 years of formal education. 30% of them work in manufacturing, another 30% in retail, and another 30% work in services with the remainder being spread across construction and livestock rearing. They work 45 hours during an average week and earn about Rs.4500 in profits per month from the businesses. When asked to project the increase in monthly profits that they expected if they were to receive the USD 100 grant, they report expecting to increase their monthly profits by an average of Rs.2000 (USD 35).

Peer predictions data comes from the group ranking exercises. Business owners in our sample are well informed about the peers in their group. We find that in less than 1% of the cases a respondent reported not recognizing a peer in her group. On average, they visited one another 22 times over the previous 30 days. 23% report discussing private family or business issues with the peer. To establish a baseline level of knowledge that peers in the group had about one another, we asked them to report some observable and easily verifiable information about one another. We asked respondents whether their peers owned a motorcycle (50% of our sample has at least one motorcycle in the home). 83% of respondents correctly identified when a peer did not own a motorcycle and 70% correctly identified when a peer owned a motorcycle. This number rises to 80% if we include other motorized vehicles in the criteria. We also asked whether there were children living in their peers' homes. As with motorcycle ownership, approximately 80% were able to correctly identify when there were and there were no children in their peers' homes.

4 Empirical Strategy

We present the regression specifications to answer the questions posed in Section 2.2. To address how well peers can predict one another's baseline outcomes we use the following model.

$$Outcome_{ikjc} = \alpha_0 + \alpha_1 Rank_{ikjc} + \gamma_c + \epsilon_{ikjc} \tag{1}$$

where i indexes the person being ranked (rankee), j indexes his group, and k indexes the person doing the ranking (respondent). $Outcome_{ikjc}$ is the percentile at which person i's outcome value lies in the distribution of that outcome for the sample. $Rank_{ikjc}$ is the percentile at which the rank that person k provides for person i in group j lies in the distribution of ranks for the sample. γ_c is a fixed effect for each cluster of groups that form the basis for our stratification. Standard errors are clustered at the group level. In all Tables presented in the paper we additionally include a surveyor and month of baseline survey fixed effect.

We measure outcomes and ranks in percentiles to maintain comparability across outcomes and reporting methods (ranks vs quintiles), and to ease interpretation. A 1 percentile increase in the rank is, therefore, associated with an α_1 percentile increase in the outcome variable distribution.

To estimate the residual information that ranks provide over and above observables, we will estimate regression 1 with the addition of $\mathbf{X}_{ijkc}$, a vector of person i's baseline characteristics that may impact the outcome value. If ranks have residual predictive power, we expect α_1 to remain positive and significant. We will also estimate a related version of this model in which, rather than using each $i-k$ pair as the unit of observation, we will average across all of the reports that peers in group j gave about person i. $Ra\bar{n}k_{ijc} = \sum_{k=1}^{n} \frac{1}{n} * Rank_{ikjc}$ where n is the total number of group members in group j

$$Outcome_{ijc} = \alpha_0 + \alpha_1 Ra\bar{n}k_{ijc} + \gamma_c + \epsilon_{ikjc}$$

To address whether respondents can predict one another's marginal return to capital we estimate the following model.

$$Profit_{ikjct} = \alpha_0 + \alpha_1 Winner_{i,t} + \alpha_2 Rank_{ikjc} + \alpha_3 Winner_{i,t} \times Rank_{ikjc} + \alpha_4 \mathbf{X}_{ijkc} + \alpha_5 Winner_{i,t} \times \mathbf{X}_{ijkc} + \gamma_c + \gamma_t + \epsilon_{ikjct}$$

where $Profit_{ikjct}$ is the profit for person i being ranked by person k in group j, cluster c and time t, $Winner_{i,t}$ is an indicator for whether person i has (randomly) received a grant at time t, γ_t is a wave fixed effect and all other variables are as defined above. α_2 captures the average increase in marginal return to capital associated with being one rank higher in the community prediction. In some specifications we control for borrower and business characteristics $\mathbf{X}_{ijkc}$ as well as its interaction with $Winner_{i,t}$ to measure the additional information conveyed in community reports.

To answer question (2), whether respondents manipulate their rankings when they have a strategic incentive to do so, we test how the accuracy of their responses changes when they are assigned to the revealed treatment (they are told that their ranks would affect the probability that their peers win the lottery) . Specifically, we estimate

$$Outcome_{ikjc} = \beta_0 + \beta_1 Rank_{ikjc} + \beta_2 Rank_{ikjc} * Revealed_{jc} + \beta_3 Revealed_{jc} + \gamma_c + \epsilon_{ikjc} \tag{2}$$

where $Revealed_{jc}$ is a dummy for whether group j was assigned to the revealed treatment. β_1 indicates the accuracy of reports in the group that was told that was told nothing about reports being used to inform the distribution of grants and so respondents had no explicit incentive to misreport. β_2 is then the differential effect of being assigned to the revealed group. If respondents manipulate rankings, we expect β_2 to be negative - the correlation between reports and true outcomes should be lower for this group.

A more direct way to test whether respondents are lying is to check whether they manipulate the reports that they give about peers in the group that they share a close relationship with (measured by self and cross reports). Specifically we estimate,

$$Rank_{ikjc} = \pi_0 + \pi_1 Member_{ijc} + \pi_2 Member_{ijc} * Revealed_{jc} + \pi_3 Revealed_{jc} + \gamma_c + \epsilon_{ikjc}$$

$Member_{ijc}$ is a dummy that will signify one of three types of relationship between the rankee and the respondent: they are family members, they are close peers (as reported by other group members), or the it is the rank that the respondent gives herself. If $\pi_2 > 0$, then respondents up rank these individuals irrespective of their true characteristics.

Lastly, to answer question (3) - whether monetary incentives or reporting in public/private improve the accuracy of reports - we estimate the following model

$$Outcome_{ikjc} = \theta_0 + \theta_1 Rank_{ikjc} + \theta_2 Rank_{ikjc} * Public_{jc} + \theta_3 Rank_{ikjc} * Incentives_{jc} + \tag{3}$$

$$\theta_4 Rank_{ikjc} * Public_j * Incentives_{jc} + \theta_5 Public_{jc} * Incentives_{jc} + \theta_6 Public_{jc} + \theta_7 Incentives_{jc} + \gamma_c + \epsilon_{ikjc}$$

$Public_{jc}$ is a dummy indicating whether the group is assigned to report rankings in public (vs private). $Incentives_{jc}$ is a dummy for whether the group is assigned to receive personal incentives for its reports. θ_1 indicates the accuracy of reports when respondents report in private and receive no incentives. θ_2 is the differential effect of reporting in public without incentives. θ_3 is the differential effect of reporting in private and receiving incentives, so if incentives improve rankings, we expect this to be positive. Lastly θ_4 is the differential effect of reporting in public and receiving incentives relative to either treatment on its own.

4.1 Randomization Check

In Appendix Table 1, we present the randomization check of baseline characteristics by treatment. To check for balance we estimate the model

$$Characteristic_{ijc} = \tau_0 + \tau_1 Treatment_{jc} + \gamma_c + \epsilon_{ijc}$$

where $Treatment_{jc}$ is a dummy for whether the group was assigned to the $Revealed_{jc}$ treatment (columns 1 and 2), the $Incentives_{jc}$ treatment (columns 3 and 4), and the $Public_{jc}$ treatment (columns 5 and 6). The odd columns show the average of each characteristic for the control group in each block. So column 1 shows the means of characteristics for groups that were assigned to $Not\,Revealed$. The even columns show τ_1 for each treatment (the difference between treatment and control characteristics) . The characteristics in rows 1-6 are at the household level. The remainder pertain to the business owner that was selected to participate in the study. As would be expected by chance, there is some imbalance across a few characteristics between the various treatments. Groups assigned to *Revealed* report lower yearly income and slightly fewer years of education, and the incentivized group has somewhat lower baseline assets than the unincentivized group and lower projected monthly profits if they were to receive the grant. There is no significant difference in the means of characteristics in the *Public* treatment. We also present the results of a joint test of statistical significance, and cannot reject that all groups are drawn from the same population.

5 Results

5.1 How Much Do Respondents Know About Baseline Characteristics?

The most basic way to quantify how much respondents know about community members is to estimate the correlation between the ranks given to a respondent by her peers and a respondents' true outcome value for that ranking (regression model 1). In Table 1, we pool across all treatments and report the results at the ranker-rankee pair level of observation. The outcome variables, denoted in the column headings, were elicited from clients (rankees) during the baseline survey, which was conducted before respondents had any knowledge of the rankings portion of the study and before any ranking exercises had been conducted in the community. They correspond exactly to the outcome that peers were asked to rank on during the ranking exercises. Self-reported marginal returns are the outcome values in columns 1 and 2.[10] In columns 3 and 4, the outcome is average monthly household income over the past year. In columns 5 and 6, the outcome value is the clients' predicted monthly profits if they were to receive a $100 grant.[11] In columns 7 and 8, the outcome is the total value of household assets. In column 9, we report

[10]While self reported marginal returns to capital may seem unreliable, in previous work done by the authors, we find that self-reported marginal returns are predictive of true marginal returns in cash drop studies. And as stated above, once our followups are completed we will be able to measure and evaluate community predictiveness of true marginal returns to capital.

[11]We use this outcome as it corresponds to the ranking question posed to respondents. In Appendix Table 1 we show that the results remain nearly identical if we use average yearly profits as the outcome variable.

a households total medical expenses in the past six months. In column 10, the outcome variable is the average number of hours the client works per week and in column 11 we report the total number of digits the client remembered during a digit span memory test. Following De Mel, Mckenzie, and Woodruff (2008), we trim variables in columns 1-10 at the 99.5 % level.[12]

Both the outcome variables and *Rank* have been converted to percentiles of the distribution of observed outcome values. So a 1 percentile increase in the rank is associated with an α_1 percentile increase in the outcome variable distribution. For all outcome measures, the reports are highly predictive. Peers are informed about about relatively observable aspects of their neighbors' lives, such as household assets, but also much less easily observable characteristics including working memory and work ethic and ones that are notoriously hard to predict such as business profits and marginal returns to capital.

The level of accuracy of the prediction varies depending on the question. Respondents are most accurately able to predict peers' household assets: a 1 percentile increase in the relative rank of assets is associated with a 0.19 percentile increase in the distribution of household assets in our sample population. Unsurprisingly, the most difficult outcome to predict accurately is marginal returns. We find that a 1 percentile increase in the marginal returns relative rank is associated with a 0.078 percentile increase in the self-reported marginal return to capital. We asked respondents to rank their peers relative to others in the group and also relative to the community by reporting the quintile of the outcome distribution that they believe the peer to be in. In theory, quintile rankings could be more useful as they could contain more information about the true position of the peer vis-a-vis other similar microentrepreneurs. In both the even and odd columns 1-8 of Table 1 the outcome variable is the same, but what changes is the method of reporting. In the odd columns, the regressor is the percentile in the relative rank distribution and in the even columns the regressor is the percentile of the quintile rank distribution. By comparing the odd and even columns, we find that relative and quintile rankings are equally informative.

Respondents may have idiosyncratic preferences for misreporting about certain peers in their group and may otherwise make idiosyncratic errors. One way to reduce the influence of the errors is by averaging across all reports given about a particular group member. We do so and present these results in Table 2, where the unit of observation is the rankee. We observe that the average reports are significantly more predictive of all outcome variables. While a 1 percentile increase in the profits rank leads to a 0.14 percentile increase in the profits distribution in Table 1, it is associated with a 0.25 percentile increase in Table 2.

[12] Digit span is a commonly used test for working memory. Respondents are shown flashcards with an increasing number of digits and asked to recall the numbers from memory. The surveyor records the total number of digits that the respondent correctly repeated back.

While community reports contain valuable information about community members, are they informative beyond what would be captured by observables? To test whether there is residual information, we add household and business-level controls to the regressions in Table 2 and observe whether the *Rank* variable continues to be statistically and economically significant. We present the most conservative version of this exercise in Table 3. In addition to adding easily observable household and demographic characteristics that would be verifiable by a principal - such as gender of the main business owner, education, age, household size, household composition - we also add self-reported information about the household's primary business. Specifically, we control for the number of household businesses, the value of business assets, business revenues in the previous 30 days, and average yearly profits. The control variables tend to predict the outcome variables in the directions we would expect. Average yearly profits are positively predictive of nearly all outcome measures (except digit span). Due to correlation with average yearly profits, revenues is nearly always insignificant except in predicting profits in columns 5 and 6 (average yearly profits is omitted as a control in this regression). Larger households, as well as households with more business owners, have higher household income as well as more assets. Despite controlling for a plethora of household and business characteristics, ranks continue to significantly predict outcomes. The one exception is in column 10: covariates appear to contain fully overlapping information with the work hours ranks. In Appendix Table 3, we have replicated Table 3, but removed hard to observe business characteristics: total business capital, revenues, and average yearly profits.

In Table 3 and Appendix Table 3, we see that marginal returns ranks continue to be informative even after controlling for a set of household and business characteristics. Another set of characteristics that have been identified as predictive of credit worthiness and entrepreneurial aptitude are psychological questions that identify characteristics such as tenacity, polychronicity, and optimism (see Klinger, Khwaja, and Carpio 2013). We therefore test how well psychometric questions perform at predicting self-reported marginal returns and how they compare to community rankings in Table 4. The regressors are labeled according to the psychological trait for which they are meant to proxy (the specific wording of the statement is found in the Appendix). In column 1, we regress self-reported marginal returns percentile on all of the psychometric questions that we collected. The traits that are strongly predictive of marginal returns fall into two categories: optimism and achievement. Optimism negatively predicts marginal returns: business owners who are more likely to agree with the statements "In times of uncertainty I expect the best" and "I'm always optimistic about the future" and those who are more like to disagree with "If something can go wrong with me, it will" have lower self-reported marginal returns. People who agree with the statement "Part of my enjoyment in doing things is improving my past performance" tend to have higher marginal returns. Both the direction of prediction of growth and the adjusted R^2 on this regression (0.05) is in line with the results found by De Mel, McKenzie, and Woodruff (2008).

In column 2 of the table we replicate the result of column 1 of Table 2. We see that the adjusted R^2 on this regression is equivalent to the psychometric regression, but the predictive power of community ranks is twice as large as any of the psychometric questions. In addition to asking respondents to rank one another on marginal returns, we also asked respondents to rank their peers on their ability to grow a business. The specific wording of the question asked was

> "Consider a world in which everyone in your group – no matter how rich they are right now, or how poor they are, or how small or big their business or their current loan is – is starting at exactly zero, at exactly the same place as everyone else. Now we give each of you Rs.20,000, and we ask you to each, individually, start any type of business you want. Who do you think will grow their business the most and make the most profits from that business? What we are trying to understand is: who is most talented in growing a business?"

While this question has no natural analogue in the data, much like the psychometric questions, it was meant to proxy for entrepreneurial ability using concepts and wording that were more familiar to the respondent. The question was asked to only a subset of respondents as explained in Section 2.4, but in column 3 we see that despite the small sample size, the question is strongly predictive of marginal returns. A 1 percentile increase this ranking increases marginal returns by 0.2 percentile. The adjusted R^2 also increases to 0.15. Do psychometrics and community ranks provide overlapping information? To test this, in columns 4 and 5, we include both in a regression to predict self reported marginal returns. We find that ranks continue to be predictive even after controlling for psychometrics and given that the coefficient on marginal returns remains nearly identical to columns 2 and 3, it appears that the information is not overlapping.

5.2 Can Respondents Predict One Another's True Marginal Return To Capital?

We next ask whether respondents can predict one another's true marginal return to capital, measured by comparing profits of those who did and did not receive grants, collected during followup surveys. We are in the midst of collecting follow up data, so the results in this section should be viewed as indicative. In general we find that, while some of our estimates are noisy, community information appears to be quite valuable in identifying the entrepreneurs with high marginal return to capital. Tables 10 - 13 make the case.

In Table 10 we examine the relationship of a variety of measures of grant expenditure (of those who received our grant) with the average community ranking of marginal return to capital. We find that community members ranked highly on average spend more of their own money augmenting the grant to buy larger business assets, spend a higher fraction of the grant on business assets, and allocate less of the grant to household expenditures and savings.

Table 11 presents our main result: community rankings are highly predictive of marginal return to capital. As outlined in Section 4, our primary specification is

$$Profit_{ikjct} = \alpha_0 + \alpha_1 Winner_{i,t} + \alpha_2 Rank_{ijc} + \alpha_3 Winner_{i,t} \times Rank_{ikjc} + \alpha_4 \mathbf{X}_{ijkc} + \alpha_5 Winner_{i,t} \times \mathbf{X}_{ijkc} + \gamma_c + \gamma_t + \epsilon_{ikjct}$$

where $Profit_{ikkjct}$ measures either business profits or household income of person i in period t, depending on the specification, $Winner_{i,t}$ is an indicator for whether person i received a grant at or before period t, and $Rank_{ijc}$ is the average rank assigned to person i by the members of group j in cluster c. α_3 measures the average additional marginal return to capital an entrepreneur enjoys for every additional rank he is assigned by his group. Across the various specifications in Table 11, α_3 is large and positive, with varying levels of statistical significance. The standard deviation on the average ranking of marginal return to capital is .83. Therefore, while the estimates in column 2 imply that the average marginal return to capital in the whole population was about 3.7% per month, the average entrepreneur ranked one standard deviation above the mean enjoyed an 8.8% monthly marginal return to capital, and an entrepreneur ranked two standard deviations above the mean enjoyed a 13.9% marginal return to capital. Thus community information appears to be extremely valuable in targeting grants.

Tables 12 and 13 examine how much the value of community reports diminishes when controlling for other business and household characteristics, as well as their interaction with $Winner_{i,t}$. The regressions in Table 12 control for the entrepreneur's gender and the type of business she is in, as these are easily observable characteristics that tend to be predictive of marginal return to capital. Nevertheless we find that community information is almost orthogonal to these characteristics; the estimates in Table 12 are strikingly similar to those obtained without controls. In addition to gender and business category, Table 13 presents estimates that also control for baseline profits. If anything, controlling for baseline profits seems to enhance the value of community information, as the estimates in Table 13 are substantially larger than the preceding ones. Thus it seems that community information is valuable in identifying entrepreneurs with high marginal return to capital even if the implementing organization has access to a large variety of observable demographic and business information.

5.3 Do Peers Distort their Responses?

Aside from quantifying how much information neighbors have about one another, the second major goal of this project is to quantify whether and how much peers misreport in high stakes settings. Half of our sample was informed that they could affect the probability that their peers (or themselves) would win the $100 grant (the other half was not given any information about how their rankings would be used). One test of whether respondents behave strategically is to compare the accuracy of reports in groups in which ranks affected the grant allocation (*Revealed*) and groups in which ranks had no effect on grant distribution.

As explained in Section 2.4, we implemented this treatment only for the income, profits, and assets ranking questions. Therefore in Table 5, we only show these three outcome variables in columns 1 to 3. As in previous tables, the treatment and outcome variables are standardized in percentile terms. To increase power, in columns 1-3 we pool across quintile and relative ranks and in column 4 we pool across the previous 3 columns. The *Rank* variable captures the accuracy of the report in the control group (*Not Revealed*). The $Rank * Revealed$ coefficient tells us whether the rankings are differentially informative when respondents are told their ranks will be used to help determine grant allocation. First, we note that the in the control group, predictions are all significant at the 1% level and range between 0.15 percentile increase in income and 0.23 percentile increase in assets. We cannot reject that respondents do not distort their ranks when reporting about income, although the standard error on the coefficient is large. On the other hand, for both profits and assets, the coefficient on $Rank * Revealed$ is large, negative, and significant. This implies that responses are significantly less accurate when respondents have an incentive to behave strategically. We should note that this was not ex-ante obvious: the *Revealed* treatment may have had a positive effect since revealing ranks may have caused respondents to focus and take the exercise more seriously. Looking at the pooled estimate in column 4, we see that, on average, responses become 40% less accurate in the *Revealed* group.

We asked respondents to rank their peers in the group relative to one another and also relative to the community via quintiles. Quintile rankings have the advantage of potentially being more informative for a principal that is interested in understanding who are, for example, the best entrepreneurs in the community, not just in the group. However, quintile rankings may also be easier to manipulate. Relative ranks have a zero-sum feature that ranking someone higher necessarily harms another person in the group. For quintiles, however, group members could simply say that all of their peers are the best in the community. We test both of these hypotheses - that quintile ranks are more informative in the absence of distortionary incentives and that they are more manipulable in their presence - in Table 6. We show the results separately for relative ranks and for quintile ranks. Column 7 pools columns 1, 3, and 5 (relative rankings) while column 8 pools across 2, 4, and 6 (quintile rankings). In the control group, quintile ranks are never more (or less) informative than relative ranks: the coefficient on *Rank* in the odd and even columns are nearly numerically identical for all questions. The *Revealed* treatment does not have an impact on the accuracy of quintile or relative ranks for household income. We do, however, observe a differential impact of the treatment in the quintile and relative ranks of profits and assets. As predicted, the coefficient on $Rank * Revealed$ is more negative in the quintile as compared to the relative regressions. The difference is significant in the pooled versions of the regressions: while the *Revealed* treatment reduces the accuracy of relative ranks by 20%, it reduces the accuracy of the quintile ranks by 40%. Thus relative ranks may be a more robust way to elicit community information.

Respondents manipulate their reports when they have an incentive to do so. We now turn to predicting the beneficiaries of each respondent's manipulations. In Table 7 we directly check how respondents ranked different types of peers in the group. The outcome variable in this set of regressions is the rank that respondent i gives to her peer j. Our regressors include dummies for whether respondents are ranking themselves or their family members. We also asked group members to cross-report who they thought was each group members' closest peer in the group. In other words, we asked respondent i to report who she thought was person j's closest friend in the group. We created a binary variable *Closest Peer* which takes a value 1 if two or more people in the group reported that person i is group member j's closest peer. The coefficients $Family$, $Self$, *Closest Peer* indicate the average percentile difference in the rank given to each of these groups over other peers in the control group (*Not Revealed*). Even in the absence of incentives to misreport, respondents rank themselves 6 percentage points higher (significant at the 1% level). They rank their family members 2 percentage points higher, although this is not significant, and they rank their peers 4 percentage points higher (significant at the 1% level). The peers result remains significant even if we exclude family members from the *Closest Peer* group, which implies that neighbors are well informed about the interpersonal relationships of their group members beyond what may be easily observable by an outsider (familial connections). The fact that respondents rank these groups of people higher in the control group, however, does not necessarily imply that they are behaving strategically: it may be that people rank those that they know better more highly simply because they are better informed about these people and feel more confident about these reports.To test whether they lie to favor themselves or those who they are close to we compare the control group to the *Revealed* group. We see that they are much more likely to rank themselves highly. The average percentile rank increases by 200% in the *Revealed* group. Although noisy, they also appear to be ranking their family members more highly. The only group that does not seem to benefit are *Closest Peer*. In the next section, we will give further evidence of reports manipulations.

5.4 Can Mechanism Design Tools Correct Incentives to Misreport?

In the previous section we provided evidence that respondents distort their reports to favor themselves and their family members when they have a strategic incentive to do so. These distortions have a substantial impact on the accuracy of reports, particularly when respondents have an incentive to misreport such as when their information is used to allocate a desired good. Can we use mechanism design tools to generate incentives for truthful reporting? We test two tools: incentive payments for the accuracy of reports and reporting in public versus private. In Table 8, we provide evidence that reports become more accurate when respondents in either the *Public*, *Incentives*, or *Public and Incentives* treatments. The coefficient on *Rank* indicates the accuracy of reports in groups in which respondents do not receive incentives and report in private. Reports are informative and, in the pooled outcomes regression in column 4, a 1 percentile increase in the rank is associated with a 0.1 percentile increase in the outcome variable.

Providing groups with incentives, when group members report in private, increases the accuracy of predictions for both income and profits over the private groups that receive no incentives by between 86% and 179%. We cannot reject that the incentives have no effect on assets ranks, but the incentive boost remains large and significant in the pooled specification. It is ex-ante ambiguous whether the public treatment should lead to more or less accurate predictions: on the one hand group members, the fact that group members can observe a respondents' ranks may lead to policing and therefore coordination on the truth. On the other hand, respondents may find themselves implicitly pressured to up-rank their friends in the group. While noisy in columns 1 and 2, we see that the public treatment increases the accuracy of reports. In the case of assets, the public treatment has a large and significant effect. When we pool across all variables in column 4, the public treatment without incentives and the incentives treatment in private have what appears like an equivalently-sized effect.

We break down the ranks of self, family, and close peer ranks by public and incentive treatments in Table 9. The patterns are consistent with the results observed in Table 8. In the private treatment without incentives, respondents up rank themselves, their family members, and their close peers by 16.5 percentage points, 9.7 percentage points, and 5.6 percentage points respectively. Consistent with Table 8, offering monetary incentives or eliciting reports in public both mitigate this tendency substantially.

6 Conclusion

We find that community members have valuable residual information about their peers that can be useful in targeting. Not only can community members identify one another's business characteristics, but they can also predict which of their peers have high return to capital. By distributing cash grants and measuring profits over time we find that we can reliably identify entrepreneuers with profitable investment opportunities. In particular, while the average impact of our grant on business profits was modest, community information can reliably identify entrepreneurs with upwards of 14% *monthly* marginal return to capital.

While community information appears to be quite valuable for targeting, we also find that its accuracy is sensitity to the conditions under which it is elicited. In particular, we identify a natural tendency for respondents to favor their friends and family members. Moreover this tendency is amplified when respondents are told that their reports influence the distribution of grants. However we also find that a variety of techniques motivated by mechanism design theory are effective in realigning incentives for truthfulness. In particular, small monetary payments for accuracy, eliciting reports in public, rather than in private, and cross reporting techniques to identify which community members are likely to favor one another all substantially improved the accuracy of reports. We therefore hope that the techniques identified in this paper may prove useful in improving the targeting of financial services.

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APPENDIX

Details for Robust Bayesian Truth Serum

This discussion is based on Rigol and Roth (2016)

Theory and Intuition

In this appendix section we discuss the details of the Robust Bayesian Truth Serum, an intuition for the underlying incentive properties, and our implementation of the payment rule in the field. The following discussion of the model is based on Witkowski and Parkes (2012).

Suppose there is a binary state of the world $t \in (h, l)$ (high, low) representing the entrepreneurial quality of a community member. Agents get a binary signal which is informative of the state of the world. That is each agent receives a signal $s \in \{h, l\}$ which may represent what they observe about their peer (e.g. they appear responsible, smart etc). Suppose further that all agents share a common prior about the state of the world such that they all agree on the prior probability of a high state, and they all agree on the distribution of signals conditional on the state. Let $p_h = P(s_j = h|s_i = h)$ be the probability an agent assigns to one of his peers receiving a high signal conditional on himself receiving a high signal, and analogously let $p_l = P(s_j = h|s_i = l)$. We say the common prior is *admissable* if $p_h > p_l$, which in English implies that the probability that one's peer receives a high signal is higher if the agent himself receives a high signal. Many natural distributions satisfy this weak requirement.

In order to define the RBTS we must first define the quadratic scoring rule. Let

$$R_q(y, \omega) = \begin{cases} 2y - y^2 & \text{if } \omega = 1 \\ 1 - y^2 & \text{if } \omega = 0 \end{cases}$$

Imagine an agent trying to predict whether some true state ω is 1 or 0. The quadratic scoring rule has the property that his expected score is maximized by reporting his true belief about the probability the state ω is 1 (see e.g. Selten, 1998).

The RBTS is implemented as follows. Every agent states their first order belief (their signal), in a report $x_i \in \{0, 1\}$ (imagine $x_i = 1$ corresponding to $s_i = h$). Further they report their second order belief $y_i \in [0, 1]$ (this is the fraction of the population they believe will report a high signal, $x_k = 1$). For each agent i, assign them a peer agent j, and a reference agent k, and calculate

$$y_i' = \begin{cases} y_j + \delta & \text{if } x_i = 1 \\ y_j - \delta & \text{if } x_i = 0 \end{cases}$$

for arbitrary δ. The RBTS payment for agent i is

$$u_i = R_q\left(y_i', x_k\right) + R_q\left(y_i, x_k\right)$$

The main theorem of Witkowski and Parkes is that under the assumption of an admissable prior and risk neutral agents, there is a Bayes' Nash Equilibrium in which all agents report their first and second order beliefs truthfully.

The intuition behind the payment rule is fairly straightforward. The payment rule has two components. The second component incentivizes the agent to be truthful about his second order beliefs. That is, the agent is paid via the quadratic scoring rule to predict what some reference agent k will announce as his signal. And by the discussion above, agent i maximizes his expected payment from this component of the scoring rule by truthfully announcing his belief y_i about the likelihood agent k will announce a high signal. In simpler terms, the payment rule rewards agent i for choosing a second order belief as close as possible to the truth (the realized distribution of first order beliefs).

The first component of the payment rule incentivizes the agent to be truthful about his first order beliefs. The term y_i' takes an arbitrary person j's second order belief y_j and either raises or lowers it depending on i's report x_i. RBTS pays agent i $R_q\left(y_i', x_k\right)$, and so i wants y_i' to be as near as possible to the true distribution of responses in the population. The admissibility assumption guarantees that if person j were to know that person i's signal were high, then person j would increase his assessment as to the number of people in the group who received high signals. Likewise, if j were to learn that i's signal were low, j would lower his assessment about the number of people in the group who received high signals. In effect the mechanism raises or lowers j's assessment based on i's report, and then pays i based on the closeness of this modified report to the truth. Thus i can do no better than to tell the truth.

Practical Implementation

We used this payment rule in the field to incentivize rank order responses about members of each group. The model and payment rule, however, were designed for binary responses. Thus while responses contain a rank ordering of 5 people, we treat each ranking as a composite response to 25 yes/no questions of the form "Is person i the highest ranking individual in the group?", "Is he the second highest?" and so on. We elicited second order beliefs of the form "How many people will say person i is the highest ranking individual in the group?" "How many will say he is the second highest?" and so on. From there we directly applied the payment rule, calibrated so that the expected difference between payments arising from truthful and deceptive answers was large. Note that the accuracy of responses across various questions in a single ranking were correlated, but under the assumption of risk neutrality (which is maintained throughout the peer prediction literature and may be empirically reasonable with respect to moderate sums of money), these correlations are irrelevant.

Incentive Compatibility Exercise for RBTS

Throughout the experiment we told respondents that they would maximize their personal payoffs if they reported truthfully. While RBTS is truthful under certain reasonable assumptions about how beliefs are formed, its incentive compatibility under the empirical distribution of beliefs in practice remains an open question. We therefore evaluate whether respondents are maximizing their subjective expected utility by telling the truth.

Due to the coarseness of our elicited measures of belief, we cannot verify directly whether or not the respondents' priors are admissable. However we can determine the distribution of payoffs respondents can expect to receive under alternative responses to see whether they succeeded in maximizing their subjective expected utility. Respondents' payments depend on the distribution of first order beliefs (i.e. the empirical distribution of responses about the question of interest) and on the distribution of second order beliefs. Therefore, to determine whether truth telling is incentive compatible, we must understand what the respondent believes are the distributions of first and second order beliefs in the population. We obtain the former for free; respondents' beliefs about the distribution of first order beliefs are their second order beliefs, and we elicited these in our survey. We did not, however, elicit their beliefs about the distribution of second order beliefs: their third order beliefs. We must therefore construct those. The intuition behind the construction is presented in the following three steps:

1. We assume that respondents hold a common prior. If so, we can back out their third order beliefs from (a) the distribution of second order beliefs conditional on each first order belief and (b) their belief about how probable each first order belief is. The latter corresponds to her second order beliefs. [13]

2. We approximate the distribution of second order beliefs in the population conditional on any given first order belief with the (sparse) empirical distributions we observe.

3. Given second and third order beliefs, we can calculate a respondent's subjective expected utility from reporting the truth (her stated first order belief) and from any other report.[14] Specifically, we assume that the report the respondent has given is her true belief and calculate her payment. Holding constant her own second order belief and the first and second order beliefs of her peers, we then calculate her payments for every other possible report she could have given.

The results from this exercise are presented in Figure 2 below. Column 1 of the figure depicts the percentage of instances in which telling the truth gives the largest payment, column 2 depicts the percentage of instances that telling the truth results in the second largest payment, etc. Taking the graph at face value, telling the truth maximizes the respondent's subjective expected utility in about 35% of instances and it minimizes her subjective expected utility in only about 10% of instances. An ideal graph would place all of its weight in the first column.

[13] If agents have common priors then conditional on the signal they receive, they would update to have the same posterior belief. We stress here that we elicited ranks and not signals. Therefore two agents who report the same rank do not necessarily have the same posterior as the rank is a coarse measure of a signal.

[14] Notice that we only utilize incentivized data since it is only for these data that we collected second order beliefs.

Figure 2: Incentive Compatibility of RBTS Using Collected Data

The observed departure from this ideal may be due to the failure of our assumptions required by RBTS holding in practice, or by our noisy approximation of third order beliefs. To evaluate this, we perform a simulation in which we generate data that perfectly abides by all of the assumptions required for the incentive compatibility of RBTS. We generate groups of artificial agents, each of whom holds a common prior and receives a signal about the skill level of their peers. Agents update their priors based on these signals and these form the basis of their second and third order beliefs, each of which we can compute.

Because the data is generated to be perfectly consistent with the assumptions of RBTS, the agent always maximizes his expected utility by telling the truth. Next we compress our simulation data to correspond exactly to the data we collected from our respondents: just first and second order beliefs about group rank. This allows us to have two data sets (collected and simulated) that contain identical level of detail. We then generate the same graph as we did for our collected data and present it in Figure 3.

Figure 3: Incentive Compatibility of RBTS Using Collected and Simulated Data

The graph produced with the collected and with the simulated data are strikingly similar. We therefore conclude that our noisy approximation of third order beliefs could be to blame for the observed weights in columns 2 through 5, and argue that our test yields the strongest evidence in favor of the incentive compatibility that could be achieved via this method. Therefore, telling respondents that they will maximize their expected payments by reporting truthfully may indeed be good advice.

Entrepreneurial Psychology

Impulsiveness:

- I plan tasks carefully.
- I make up my mind quickly
- I save regularly.

Optimism:

- In uncertain times I usually expect the best.
- If something can go wrong for me, it will.
- I'm always optimistic about my future.

- Generally speaking, most people in this community are honest and can be trusted

Locus of Control

- A person can get rich by taking risks.

- I only try things that I am sure of.

Tenacity

- I can think of many times when I persisted with work when others quit

- I continue to work on hard projects even when others oppose me.

Polychronicity:

- I like to juggle several activities at the same time

- I would rather complete an entire project every day than complete parts of several projects.

- I believe it is best to complete one task before beginning another.

Achievement

- Part of my enjoyment in doing things is improving my past performance

- If given the chance, I would make a good leader of people.

Organized person:

- My family and friends would say I am a very organized person

Table 1: What Respondents Know: Individual Regressions

	(1) Marginal Return	(2) Marginal Return	(3) Income	(4) Income	(5) Profits	(6) Profits	(7) Assets	(8) Assets	(9) Medical Expenses	(10) Work Hours	(11) Digitspan
	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Relative	Relative
Rank	0.0769***	0.0612***	0.165***	0.143***	0.141***	0.0903***	0.185***	0.139***	0.0955**	0.123***	0.158***
	(0.0207)	(0.0172)	(0.0250)	(0.0217)	(0.0208)	(0.0191)	(0.0276)	(0.0246)	(0.0394)	(0.0405)	(0.0281)
Mean of the Dependent Variable	2047.61	2047.61	9283.00	9373.71	7060.92	6982.32	475223.51	468453.00	3004.46	47.65	5.18
	[1666.38]	[1666.38]	[7363.78]	[7518.20]	[5268.07]	[5304.03]	[671935.96]	[684897.80]	[4962.67]	[21.12]	[1.61]
N	3928	3924	3080	2709	3135	2905	2804	2324	799	772	827
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the rank given to a respondent by each rankee, computed by question.

Table 2: What Respondents Know: Average Regressions

	(1) Marginal Return	(2) Marginal Return	(3) Income	(4) Income	(5) Profits	(6) Profits	(7) Assets	(8) Assets	(9) Medical Expenses	(10) Work Hours	(11) Digitspan
	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Relative	Relative
Rank	0.126***	0.136***	0.240***	0.243***	0.251***	0.188***	0.266***	0.219***	0.232***	0.165**	0.254***
	(0.0328)	(0.0320)	(0.0348)	(0.0372)	(0.0329)	(0.0365)	(0.0349)	(0.0365)	(0.0825)	(0.0762)	(0.0415)
Mean of the Dependent Variable	2004.61	2007.01	9202.27	9244.57	6949.58	6857.15	466511.65	458235.08	2989.85	47.33	5.10
	[1609.82]	[1609.74]	[7420.04]	[7496.99]	[5170.26]	[5196.07]	[642918.27]	[649158.24]	[4893.97]	[21.30]	[1.73]
Mean of Rank	3.03	3.47	3.04	3.27	3.03	3.43	3.02	3.14	3.02	3.04	3.04
	[0.88]	[0.70]	[0.95]	[0.77]	[0.87]	[0.71]	[0.96]	[0.79]	[0.84]	[0.79]	[0.91]
N	1039	1039	772	672	834	774	774	659	203	210	214
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the average rank given to a respondent, computed by question.

Table 3: The Informativeness of Ranks With Household and Business Controls

	(1) Marginal Return	(2) Marginal Return	(3) Income	(4) Income	(5) Profits	(6) Profits	(7) Assets	(8) Assets	(9) Medical Expenses	(10) Work Hours	(11) Digitspan
	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Quintile	Relative	Relative	Relative
Rank	0.059*	0.083***	0.070**	0.069*	0.161***	0.140***	0.210***	0.171***	0.156**	0.037	0.118***
	(0.033)	(0.031)	(0.034)	(0.036)	(0.033)	(0.035)	(0.035)	(0.036)	(0.075)	(0.075)	(0.043)
Gender	0.029	0.028	-0.135***	-0.147***	0.219***	0.220***	-0.065**	-0.053	0.067	0.104	0.064
	(0.024)	(0.024)	(0.026)	(0.027)	(0.026)	(0.027)	(0.030)	(0.035)	(0.081)	(0.068)	(0.042)
Education	0.000	0.000	0.009***	0.008**	0.000**	0.000**	-0.000*	-0.000**	-0.013*	0.000***	0.029***
	(0.000)	(0.000)	(0.003)	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)	(0.007)	(0.000)	(0.005)
Age	-0.002**	-0.002**	-0.001	-0.001	-0.002**	-0.002*	0.002**	0.002*	0.002	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Martial Status	0.007	0.005	-0.006	-0.003	0.005	0.009	0.016	0.020	0.054	-0.007	-0.026
	(0.013)	(0.013)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.017)	(0.037)	(0.032)	(0.025)
Household Size	0.001	0.000	0.049***	0.052***	0.001	0.003	0.011*	0.016**	0.026	0.008	-0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	(0.008)	(0.019)	(0.016)	(0.010)
Children 0-5 yrs	0.004	0.005	0.011	0.010	-0.010	-0.008	-0.003	0.000	0.057*	-0.002	0.010
	(0.007)	(0.007)	(0.009)	(0.010)	(0.012)	(0.012)	(0.008)	(0.008)	(0.031)	(0.023)	(0.023)
Children 6-12 yrs	-0.004	-0.005	-0.011	-0.011	-0.028***	-0.029***	0.004	0.001	-0.042	0.003	-0.007
	(0.007)	(0.007)	(0.009)	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)	(0.032)	(0.023)	(0.013)
Nu HH Businesses	0.017	0.015	0.091***	0.083***	0.013	0.004	0.089***	0.110***	0.059	0.042	0.010
	(0.023)	(0.023)	(0.022)	(0.025)	(0.027)	(0.027)	(0.026)	(0.031)	(0.077)	(0.047)	(0.048)
Business Assets	0.000	0.000	-0.000	0.000	0.000***	0.000***	0.000***	0.000**	-0.000***	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Revenues	-0.000	-0.000	0.000	0.000*	0.000***	0.000***	0.000*	0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Avg Yearly Profits	0.000***	0.000***	0.000***	0.000***			0.000***	0.000***	0.000*	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean of the Dependent Variable	2004.61 [1609.82]	2007.01 [1609.74]	9202.27 [7420.04]	9244.57 [7496.99]	6949.58 [5170.26]	6857.15 [5196.07]	466511.65 [642918.27]	458235.08 [649158.24]	2989.85 [4893.97]	47.33 [21.30]	5.10 [1.73]
Mean of Rank	3.03 [0.88]	3.47 [0.70]	3.04 [0.95]	3.27 [0.77]	3.03 [0.87]	3.43 [0.71]	3.02 [0.96]	3.14 [0.79]	3.02 [0.84]	3.04 [0.79]	3.04 [0.91]
N	1038	1038	771	671	833	774	773	658	202	210	213
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the average rank given to a respondent, computed by question. Due to space constraints, the coefficients on sector controls are not shown.

Table 4: The Predictiveness of Psychometrics and Ranks

	(1) Marginal Return	(2) Marginal Return	(3) Marginal Return	(4) Marginal Return	(5) Marginal Return
Rank		0.126***	0.196***	0.117***	0.218***
		(0.033)	(0.069)	(0.033)	(0.074)
Impulsiveness 1	0.008			0.007	0.022
	(0.026)			(0.026)	(0.062)
Impulsiveness 2	0.013			0.011	-0.115*
	(0.025)			(0.025)	(0.058)
Impulsiveness 3	0.009			0.011	-0.012
	(0.027)			(0.027)	(0.061)
Optimism 1	-0.069**			-0.067**	-0.008
	(0.028)			(0.028)	(0.083)
Optimism 2	0.067**			0.065**	0.089
	(0.030)			(0.030)	(0.066)
Optimism 3	-0.037			-0.032	-0.099
	(0.041)			(0.040)	(0.089)
Optimism 4	-0.077**			-0.072**	-0.009
	(0.034)			(0.034)	(0.080)
Tenacity 1	0.013			0.018	0.005
	(0.032)			(0.032)	(0.062)
Tenacity 2	0.034			0.033	0.041
	(0.029)			(0.030)	(0.072)
Polychronicity 1	0.036			0.040	0.125*
	(0.039)			(0.038)	(0.064)
Polychronicity 2	-0.054			-0.055	0.010
	(0.035)			(0.034)	(0.086)
Polychronicity 3	-0.032			-0.037	-0.269***
	(0.030)			(0.030)	(0.062)
Locus of Contro 1l	-0.019			-0.012	0.011
	(0.028)			(0.028)	(0.068)
Locus of Contro l 1	0.051			0.043	0.104
	(0.033)			(0.033)	(0.087)
Achievement 1	0.035			0.033	0.083
	(0.032)			(0.032)	(0.104)
Achievement 2	0.070**			0.064**	0.050
	(0.032)			(0.031)	(0.098)
Organization 1	-0.011			-0.012	0.022
	(0.027)			(0.027)	(0.080)
Mean of the Dependent Variable	2004.61	2004.61	1967.12	2004.61	1967.12
	[1609.82]	[1609.82]	[1515.00]	[1609.82]	[1515.00]
N	1040	1039	184	1039	184

Table 5: Do Respondents Distort Their Responses? Individual Regressions

	(1) Income	(2) Profits	(3) Assets	(4) Total
Rank*Revealed	0.010	-0.077**	-0.141***	-0.068**
	(0.043)	(0.035)	(0.046)	(0.028)
Rank	0.149***	0.153***	0.228***	0.178***
	(0.032)	(0.025)	(0.034)	(0.021)
Revealed	-0.018	0.045	0.119***	0.042*
	(0.037)	(0.033)	(0.039)	(0.024)
Mean of the Dependent Variable	9325.46	7022.62	472153.54	.
	[7435.95]	[5285.32]	[677786.22]	[.]
N	5789	6040	5128	16957
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the rank given to a respondent by each rankee, computed by question.

Table 6: Do Respondents Lie in Relative and Quintile Responses?

	(1) Income Relative	(2) Income Quintile	(3) Profits Relative	(4) Profits Quintile	(5) Assets Relative	(6) Assets Quintile	(7) Relative Questions Relative	(8) Quintile Questions Quintile
Rank*Revealed	-0.011	-0.016	-0.059	-0.130*	-0.111	-0.225***	-0.050	-0.112**
	(0.070)	(0.074)	(0.066)	(0.073)	(0.068)	(0.072)	(0.039)	(0.047)
Rank	0.246***	0.252***	0.277***	0.252***	0.310***	0.322***	0.261***	0.274***
	(0.047)	(0.056)	(0.043)	(0.049)	(0.046)	(0.050)	(0.024)	(0.033)
Revealed	-0.008	-0.003	0.024	0.064	0.080*	0.135***	0.005	0.057*
	(0.045)	(0.047)	(0.041)	(0.045)	(0.044)	(0.045)	(0.024)	(0.030)
Mean of the Dependent Variable	9202.27	9244.57	6949.58	6857.15	466511.65	458235.08		
	[7420.04]	[7496.99]	[5170.26]	[5196.07]	[642918.27]	[649158.24]		
N	772	672	834	774	774	659	3007	2105
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the average rank given to a respondent, computed by question.

Table 7: Do Respodents Manipulate Rankings?

	(1) Rank	(2) Rank	(3) Rank
Family Member*Revealed	0.0307		
	(0.0228)		
Family Member	0.0193		
	(0.0172)		
Closest Peer*Revealed		-0.0207	
		(0.0185)	
Closest Peer		0.0444***	
		(0.0128)	
Self*Revealed			0.0909***
			(0.0234)
Self			0.0565***
			(0.0139)
Revealed	-0.0212***	0.00601	-0.0162**
	(0.00780)	(0.00588)	(0.00627)
Mean of the Dependent Variable	0.60	0.60	0.60
	[0.29]	[0.29]	[0.29]
N	12005	17015	17015
Controls	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster fixed effects. The outcome variable are all of the ranks provided for all ranking questions asked. The regression includes a control for each question and for how confident the respondent felt about the ranking. Column (1) contains fewer observations due to missing family relationship information.

Table 8: The Impact of Incentive Payments and Public Reporting on Accuracy of Individual Reports

	(1) Income	(2) Profits	(3) Assets	(4) Total
Rank	0.113***	0.061*	0.112***	0.100***
	(0.040)	(0.036)	(0.037)	(0.026)
Rank*Incentives	0.098*	0.109**	0.009	0.075**
	(0.055)	(0.052)	(0.063)	(0.038)
Rank*Public	0.063	0.072	0.142**	0.089**
	(0.059)	(0.050)	(0.061)	(0.041)
Rank_Public_Incentives	-0.158*	-0.148**	-0.101	-0.146**
	(0.084)	(0.073)	(0.095)	(0.058)
Public_Incentives	0.155**	0.146**	0.034	0.118**
	(0.072)	(0.068)	(0.082)	(0.050)
Public	-0.052	-0.055	-0.083	-0.056
	(0.052)	(0.048)	(0.052)	(0.035)
Incentives	-0.125**	-0.113**	-0.042	-0.096***
	(0.051)	(0.047)	(0.057)	(0.034)
Mean of the	9325.46	7022.62	472153.54	.
Dependent Variable	[7435.95]	[5285.32]	[677786.22]	[.]
N	5789	6040	5128	16957
Controls	Yes	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the average rank given to a respondent, computed by question.

Table 9: The Impact of Incentive Payments and Public Reporting on Accuracy of Individual Reports

	(1) Rank	(2) Rank	(3) Rank
Family Member	0.097***		
	(0.019)		
Family*Incentives	-0.057*		
	(0.031)		
Family*Public	-0.066**		
	(0.027)		
Family*Incentives*Public	0.056		
	(0.042)		
Closest Peer		0.056***	
		(0.015)	
Closest Peer*Incentives		-0.054**	
		(0.025)	
Closest Peer*Public		-0.033	
		(0.022)	
Closest Peer*Incentives*Public		0.090**	
		(0.035)	
Self			0.165***
			(0.014)
Self*Incentives			-0.052**
			(0.023)
Self*Public			-0.025
			(0.021)
Self*Incentives*Public			0.049
			(0.034)
Incentives*Public	-0.016	-0.022**	-0.015
	(0.013)	(0.010)	(0.011)
Incentives	0.011	0.010	0.011
	(0.009)	(0.008)	(0.007)
Public	0.007	0.002	0.001
	(0.009)	(0.006)	(0.007)
Mean of the Dependent Variable	0.60 [0.29]	0.60 [0.29]	0.60 [0.29]
N	21099	29844	29844
Controls	Yes	Yes	Yes

Robust standard errors clustered at group level in parentheses. The model includes cluster, surveyor, and date of survey fixed effects. The outcome variable is the percentile of the outcome in the column header. The regressor is the percentile of the

Table 10: Grant Expenditures

	(1) Rs. Added to Grant Amount	(2) Business Expenditures	(3) Household Expenditures	(4) Amt of Grant Saved
Average Rank	351.586	683.875***	-391.044***	-292.831***
	(265.463)	(127.386)	(97.376)	(101.430)
Outcome Variable Mean	847	4554	753	693
N	444	444	444	444

$^{+}$Sample limited to grant recipient.

Notes: Robust standard errors clustered at the Group level in parentheses. All regressions include survey month and surveyor fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table 11: Returns No Controls

	(1) Profits	(2) Trim Profits	(3) Log Profits	(4) Income	(5) Trim Income	(6) Log Income
Winner*Average Rank	351.410	366.373	0.319**	702.195*	676.852*	0.118
	(297.358)	(295.644)	(0.148)	(389.402)	(388.813)	(0.097)
Winner	-1050.338	-882.108	-0.848*	-1794.383	-1719.862	-0.326
	(830.810)	(817.938)	(0.483)	(1142.007)	(1141.339)	(0.295)
Outcome Variable Mean	4695	4599	7	8323	8316	9
N	5340	5314	5340	5339	5313	5340

Notes: Robust standard errors clustered at the Group level in parentheses. All regressions include survey month and surveyor fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table 12: Returns with Observable Controls

	(1) Profits	(2) Trim Profits	(3) Log Profits	(4) Income	(5) Trim Income	(6) Log Income
Winner*Average Rank	395.168	402.798	0.335**	721.168*	690.924*	0.111
	(302.056)	(301.194)	(0.151)	(403.256)	(402.447)	(0.099)
Winner	-2826.914*	-2697.521	-1.900	-1556.390	-1536.941	-0.330
	(1684.708)	(1687.015)	(1.337)	(2545.332)	(2548.325)	(0.520)
Outcome Variable Mean	4695	4599	7	8323	8316	9
N	5336	5310	5336	5335	5309	5336

Notes: Robust standard errors clustered at the Group level in parentheses. All regressions include survey month and surveyor fixed effects. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10.

Table 13: Returns with Observable and Business Controls

	(1) Profits	(2) Trim Profits	(3) Log Profits	(4) Income	(5) Trim Income	(6) Log Income
Winner*Average Rank	862.316***	867.135***	0.467***	1047.927**	1018.101**	0.181*
	(262.483)	(260.014)	(0.150)	(405.568)	(405.491)	(0.102)
Winner	-1384.817	-1242.767	-1.577	-506.894	-468.060	-0.146
	(1343.045)	(1335.504)	(1.309)	(2413.272)	(2417.933)	(0.488)
Outcome Variable Mean	4695	4599	7	8323	8316	9
N	5336	5310	5336	5335	5309	5336

Notes: Robust standard errors clustered at the Group level in parentheses. All regressions include survey month and surveyor fixed effects. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10.