Designing Incentives for Inexpert Human Raters

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ABSTRACT

The emergence of online labor markets make it far easier to use individual human raters to evaluate materials for data collection and analysis in the social sciences. In this paper, we report the results of an experiment—conducted in an online labor market—that measured the effectiveness of a collection of social and financial incentive schemes for motivating workers to conduct a qualitative, content analysis task. Overall, workers performed better than chance, but results varied considerably depending on task difficulty. We find that treatment conditions which asked workers to prospectively think about the responses of their peers—when combined with financial incentives—produced more accurate task performance. Other treatments generally had weak effects on quality. Non-US workers performed significantly worse than US workers, regardless of treatment group.

ACM Classification Keywords

H.5.2 Information Interfaces and Presentation: Interaction Styles; H.3.3 Information Storage and Retrieval: Information Search and Retrieval; J.4 Social and Behavioral Sciences: Economics, Sociology

General Terms

Economics, Sociology, Experimentation, Human Factors, Measurement

Author Keywords

Search, Amazon Mechanical Turk, Human Computation, Crowdsourcing, Experimentation, Content Analysis

INTRODUCTION

The binding constraint in much observational, empirical social science research is finding data with useful, well-measured indepedent and dependent variables. Often, compelling research questions require the quantification of complex constructs such as trustworthiness, beauty, or aggression. Since these kinds of measures are unlikely to appear in observational data sets, researchers must look at primary source material and then classify it according to some scheme.[1] Often times these qualitative coding tasks require human judgment, but do not require any expertise. This makes them ripe for delegation to inexpert raters, yet the tasks are often tedious and time-consuming, and finding research assistance to perform the tasks is difficult or expensive.

An emerging phenomena—the online labor market—can scale the process of qualitative coding (also known in some social science circles as "content analysis") using large numbers of non-experts. However, it remains difficult to elicit and synthesize high-quality judgments from non-expert raters collaborating remotely through online labor markets. Among the foremost practical challenges of this kind, the design of optimal incentives schemes to facilitate this peculiar form of cooperative work has received scant scholarly attention. Prior economic, sociological, and psychological research offers much theoretical guidance, but little empirical evidence as to the sorts of incentives that elicit the highest quality judgments from non-expert raters.

In this paper, we present the results of a controlled experiment that directly compares the effects of fourteen different incentive schemes within the context of an online labor market. The incentive schemes encompass a wide variety of existing research into human cooperation, labor, motivation, and behavior. We test the incentives using a single non-expert content analysis task, for which we obtained validated answers prior to administering the experiment. We then compare the aggregate performance of workers in the different treatment conditions in order to determine which incentive schemes elicit the most accurate judgments in comparison to the control condition.

Use of Online Labor Markets

In online labor markets, workers from around the world perform data processing tasks for money. While some sites focus on skilled work like computer programming (e.g., oDesk, Elance, Guru), Amazon's Mechanical Turk (MTurk) is intended for small, simple and discrete tasks and thus is probably the most directly useful for researchers. The challenge of tapping this resource is that raters are inexpert and there is sometimes a high degree of inter-rater disagreement, re-

[1]A recent economic example used the photograph-based characterizations trustworthiness of people asking for loans on the social lending site Prosper.com and then used these ratings to predict their outcomes[7]. Another example used inexpert, amateur evaluations of short debate clips from gubernatorial elections and found that these evaluations were predictive[2].

gardless of the measure. The low cost of raters make large numbers of ratings possible, but this volume of data also prohibits a hand-curated approach to selecting high-quality raters.[2]

Several papers in the computer science literature have used online labor markets such as MTurk to conduct experiments [17, 25, 24]. Horton, Rand, and Zeckhauser discuss the social science potential of online experiments in these markets, focusing on how challenges to validity can be overcome [13]. There already exists a small literature on crowdsourcing from a social science perspective [15, 20, 12, 5]. New tools are also being developed that make experimentation easier [19].

Obtaining Quality Work

In online labor markets, the usual rules of labor supply generally apply: pay more money and you can attract more workers on both the intensive and extensive margin. However, attracting more workers does not necessarily lead to better work.

Earlier work [26, 16, 25, 11] has focused on techniques for filtering and processing judgments of inexpert human raters. By contrast, we focus on how to produce better judgments in the first place.

Some work has already been done in this vein. A recent experimental paper by Chandler and Kapelner [4], conducted in MTurk, looked at how knowledge about the purpose of a task affected quality and labor supply. US-based subjects that knew they were labeling cancer cells in an image produced more output than those not similarly informed. Interestingly, they found no evidence of similar effects for non-US workers.

Content Analysis

In some kinds of research, human judgments can be evaluated against objective, correct answers. This is the case for tasks such as image labeling or character recognition, where accurate automated techniques remain costly or unavailable. In others, human judgments are important precisely because they incorporate subjective perceptions, which may be central to the topic of study. This is the case for many types of content analysis tasks, where researchers aim to identify certain qualities or patterns in textual materials that evade automated detection. In both objective and subjective variants, the challenge of developing techniques to aggregate individual judgments as well as to assess their precision and accuracy has given rise to several different methodological techniques, some of which we review as background to the method we used in this study.

Useful methodological approaches to this type of problem have emerged among scholars conducting content analysis of textual materials. Until recently, content analysis techniques have relied on multiple researchers implementing a qualitative labeling or coding scheme of the same text(s), and then using specifically adapted correlation statistics to evaluate inter-rater (or intercoder) reliability [18, 6]. The primary advantage of these approaches lies in the ability to measure empirically the reliability of seemingly subjective observations. The cost of such precision, however, is often quite high in terms of time and labor, making such analysis prohibitively expensive when the scale of data collection and analysis grows large. Recent work by Hopkins and King have demonstrated how machine-learning tools and techniques can be applied to overcome these limitations while retaining high confidence in the precision and accuracy of results [11].

Our Approach

A variety of papers across the social science disciplines have studied human motivation. This literature is far too voluminous to summarize here; much of it is also captured by folk wisdom or even in management cliches. What is certainly not known is the relative merits of different motivations and how they apply in online contexts. For example, does offering workers more money improve effort and hence quality? This lack of knowledge motivated this study, in which we created a large number of treatment groups and recruited a vast number of subjects. While this "kitchen sink" approach creates some problems of analysis, we do get breadth even at the expense of depth. We review the different motivational frameworks in greater depth below.

Our Task

For our task, we asked subjects to complete a set of six closed-ended, qualitative content analysis questions using an online survey interface. All workers in all treatment groups (except one of the two control groups, which only answered demographic questions) were directed to analyze the same website and then presented with the same six questions in the same order and with the same answer choices through the survey interface. The questions asked subjects to conduct content analysis similar to that used in an earlier study by [3] to assess U.S. political blogs. For any questions, workers could choose to leave a blank response.

Overview of Results

How did the Turkers perform on the five content analysis tasks that we asked them to complete? The results varied by question as well as by treatment condition. On the two easiest questions, the Turkers uniformly performed much better than random guessing and only a couple of the treatments seemed to produce any (small) effect at all. By contrast, the results for the three difficult questions varied more widely. In one case, the Turkers' performance was much worse than chance. At the same time, the variance in responses to these questions also revealed stronger treatment effects. Aggregating the results from each condition across all five questions, the Turkers performed better than chance. More importantly, a few treatments emerged as the clear winners of our horse race, producing significant improvements in average answer quality when compared against the control condition. We discuss the experimental design, data collection, and results in greater depth below.

[2] Several innovative start-up companies, such as Crowdflower are offering services as intermediaries. Clients bring them tasks amenable to the crowdsourcing approach and they break the tasks down, recruit workers and ensure quality results.

METHODS AND MATERIALS

Content Analysis Task

In order to establish a reliable standard against which to judge the performance of the workers, we also administered the same questions about the same website through an identical web interface to a group of five research assistants prior to conducting the experiment. On all of the questions included in the study, at least four of the five research assistants gave identical responses, suggesting a high degree of intercoder reliability. Independent of the research assistants, [AUTHOR 1] also collected his own answers to the questions, agreeing with the prevailing answer provided by the research assistants in every case. We used these responses as validated (i.e., gold standard) answers to each question.

The first two questions followed a multiple choice format, in which subjects were asked to identify whether (1) a privacy policy; and (2) "avatars" or other visual representations of user identities were present on the site. For both of these questions an "uncertain" answer choice was also available. The third and fourth questions asked subjects to assess how frequently members of the site engaged in specific behaviors (ranking or rating (3) content and (4) other users) using a five point scale ranging from "Very frequently" to "Very rarely or never." Finally, the last two questions asked subjects to identify whether specific features related to (5) social networking and (6) revenue creation were present or not on the site. In these last two, subjects could check boxes to select any combination of answer choices from a pre-defined list.

The first of the six questions (about whether or not the site had a privacy policy) was presented prior to treatment. We report the results for this pre-treatment question but do not include it as part of our outcome performance measurement. The actual interface is available on [AUTHOR 2]'s website.[3]

The dependent variable of our study was the number of correct answers to the five post-treatment information seeking questions per worker.[4] We considered blank responses incorrect answers for all questions. After coding responses to identify which ones each worker answered correctly (i.e., in agreement with the gold standard response), we aggregated the number of correct answers per worker. The outcome measure is therefore an integer (count) with a value between zero and five. As we describe in further detail below, the workers on MTurk performed better than chance - estimated as random guessing between all available answer choices for every question - on four of the five post-treatment questions.

The demographic questions asked workers to provide their age; gender; country of residence; education level; language skills; employment status; household size; and internet skills. We included them to increase precision in our treatment estimates as well as to verify that our randomization was valid (we discuss the rationale for this choice in further detail below).

Conduct of the experiment

Recruitment was conducted through the MTurk online labor market, where we advertised a brief information seeking task. Recruitment materials included a description of the study as well as a set of example questions, all of which were included in the actual job, but none of which were among the post-treatment questions included in our outcome variables of interest. Subjects were not informed that they were participating in a study at the time of recruitment so as to preserve the "natural" environment of the field experiment in the online labor market. In the task description, we explained that workers would be paid $0.30 for completing the task. Given the length of the assignment and the fact that workers could only complete our job once (many jobs on MTurk allow workers to return multiple times), this payment rate was comparable with many other jobs posted to the MTurk marketplace.

Upon agreeing to accept the task on the MTurk website, subjects were instructed to click a hyperlink pointing to a private server at an anonymized URL. While we were not able to collect data on how many individuals saw our recruitment materials, once a worker accepted our task, their unique MTurk user id was simultaneously assigned randomly to one of the treatment or control conditions and (together with their IP address and the information about treatment assignment) stored by a database on our server. As a result, we were able to use these different pieces of stored identifying information to block individual subjects from completing the study more than once or from being exposed to more than one of the experimental manipulations. While there is some possibility that individuals could possess more than one account on the MTurk platform and thereby might have circumvented these protections, such behavior is expressly prohibited by the site's terms of service and Amazon actively polices violations (indeed, one of the authors of the study had the somewhat embarassing experience of losing his MTurk account as a result of attempting to create multiple user names in order to test a pilot version of an earlier study). Furthermore, the payoff for circumventing the system protections on our job (which required a little more than 2000 unique judgments) were very low in comparison with some of the large scale jobs on the site that frequently elicit hundreds of thousands or even millions of individual judgments. As a result, we feel confident in the integrity of both the randomization as well as the different treatment conditions.

Once Turkers clicked through to our server, the experimental instrument was then administered through a web-based survey interface. Subjects arriving at the site were presented with the version of the instrument corresponding to their treatment assignment on a single page. Each version of the instrument began with some general instructions about the task, and (in all conditions except for the demographic control) a link to the URL of the site that would serve as the topic

[3]Links removed for peer review.

[4]In the case of the checkbox questions – numbers (5) and (6) – we coded any response including the gold standard answer as correct. Obviously, in the case of a question where we did not know the correct answer ahead of time, a much different process would be needed to identify the best response. As such filtering processes were not the focus of this study, we refer consideration of this topic to the work of others.[25]

of the questions (Kiva.org). These were followed by several pre-treatment questions about the site. Then, we introduced the experimental manipulations (usually consisting of a block of text) followed by the post-treatment questions and any treatment-specific materials. Finally, the instruments concluded with a series of demographic questions.

Overview of Treatments: Social, Financial, and Hybrid Incentives

The experimental manipulations we introduced consisted of framing the information seeking task in distinct ways using a series of "social" and "financial" incentives. Together, these different incentive schemes encompass a number of salient theories of human motivation and range across several social science disciplines. Generally speaking, the more social incentives emphasized non-monetary rewards or punishments for performing our information seeking task whereas the financial incentives offered explicit monetary performance rewards or punishments. Some frameworks were hybrids that combined social and financial incentives. In total, we tested fourteen different incentive frameworks and compared worker performance in each condition against a control condition that involved no framing incentives beyond the baseline compensation offered for completing the job. We also included a second control group in which workers responded only to the pre-treatment and demographic questions used in the other conditions. All workers who completed the task were given the baseline compensation. Because of some technical complications, we ended up paying all workers the largest amount they could have received from their experimental treatment, to avoid potentially under-paying deserving workers.

All control and treatment conditions are described in further detail below. For each of the treatment conditions (listed in bold) we have noted in parentheses whether it is social, financial or hybrid in nature and included the full treatment text. Where appropriate, we have also included references to relevant studies in which comparable incentives were found to effect behavioral outcomes.

Control Conditions

Control Workers were presented with all pre-treatment, post-treatment and demographic questions.

Demographic Workers were presented with pre-treatment and demographic questions only.[5]

Treatment Conditions

Tournament scoring (social) "For some of the following five questions, you will be in competition against another worker. After this HIT is completed, we will compare your accuracy on these questions against the accuracy of another worker who we will select at random. We will report the results of the competition to you when we process your payment."

[5]Whenever possible, the demographic questions were taken verbatim from the 2005 codebook of the World Values Survey [1]. As we described later in the paper, we also borrowed two questions about Internet-use skills from Eszter Hargittai [10].

Cheap Talk—Surveillance (social) "After this HIT has been completed, your answers to these questions will be reviewed for accuracy."

Cheap Talk—Normative (social) "It is your job to provide accurate answers to these question. It is important that you do your job well."

Solidarity (hybrid) "For some of the following five questions, you have been assigned to the Red team. You and your teammates have the opportunity to earn bonuses based on your collective performance. After the HIT has been completed, we will verify the answers that you all submitted for these questions (independent of the website you are analyzing) and compare your team's performance with another group of workers completing this HIT. If your team wins, you will all receive a bonus."

Humanization (social) "Before you complete the questions, I just wanted to thank you again for doing this work. My name is Aaron."[6]

Trust (social) "Thank you for completing the first set of questions. Here is your confirmation code, which you may paste into the field on the original HIT page at any time to receive payment. We trust that you will still complete the questions below to the best of your ability. Your confirmation code and payment for this HIT will not change based on the answers you submit."[7]

Normative priming questions (social) "Before answering the next set of questions about the website, we want to ask you a few questions about yourself and your attitudes about work."[8]

Reward Accuracy (financial) "After this HIT has been completed, we will verify the correct answers for at least one of the following five questions. For each 'trap door' question we will increase your total pay by 10% if you answered it correctly. You will not receive this bonus if you do not answer the 'trap door' question(s) correctly."

Reward Agreement (financial) "After this HIT has been completed, we will review the answers for at least one of the following five questions. For each of the questions we review, we will reward you for agreeing with the answers provided by the majority of other workers who complete this HIT. The reward will be a bonus of 10% for every agreement."

[6]This treatment text was accompanied by a photo of one of the authors.

[7]In order to make this treatment condition consistent with the design of all other conditions, all workers were asked to submit a completion code when they finished the job. In every condition except this one, we provided these completion codes once the task had been finished and the answers to all questions submitted to our server. Compensation was not conditional on submitting the completion code in any of the conditions.

[8]This text was followed by a series of questions drawn from the General Social Survey inquiring about subjects' agreement with statements indicating positive attitudes towards responsibilities and hard work. The statements, in order, were "People who don't work become lazy"; "Work is a duty toward society"; "Work should always come first, even if it means less free time"; "Work is a person's most important activity"; "I see myself as someone who does a thorough job."

Punishment Accuracy (financial) "After this HIT has been completed, we will verify the correct answers for at least one of the following five questions. For each on of these 'trap door' questions we will penalize you 10% of the bonus that you would have received if you answered it incorrectly."

Punishment Agreement (financial) "After this HIT has been completed, we will review the answers for at least one of the following five questions. For each of the questions we review, we will penalize you if you disagree with the majority of other workers who complete this HIT. The penalty will be a deduction of 10% from the total bonus you could have earned if your answer had agreed with the majority."

Promise of Future Work (financial) "After this HIT has been completed, we will review the performance of each worker on the following five questions. If you perform better than average, you will have the opportunity to work on future jobs with us."

Bayesian Truth Serum or BTS (financial) "For the following five questions, we will also ask you to predict the responses of other workers who complete this task. There is no incentive to misreport what you truly believe to be your answers as well as others' answers. You will have a higher probability of winning a lottery (bonus payment) if you submit answers that are more surprisingly common than collectively predicted."[9]

Betting on Results (financial) "For the following five questions, you will have the opportunity to win bonuses. After completing the questions, we will let you bet a portion of your payment on the accuracy of your responses."

Data Collection

The experiment ran from June 2 through September 23, 2009. During that time, we collected a total of 2159 unique subjects, of whom 2055 completed the study and 104 dropped out after treatment assignment. Because we used a random treatment assignment function (instead of stratified random assignment), the distribution of subjects across conditions was unequal, ranging between 113 and 167 subjects per condition. Applying Pearson's χ^2 test to a contingency table with the counts of attriters and compliers across all of the treatment and control groups suggests that attrition was not significantly different from random (p = 0.919).

Following the completion of data collection, we discovered that database storing our records from the study had stored inaccurate values for three of the subjects. As a result, we excluded the results from these three subjects from all subsequent analysis, with the exception of the calculation of the total number of subjects assigned to each treatment group used to generate our estimates of treatment effects (see below).

Statistical Analysis

In all of our estimates of treatment effects, we correct for the increased probability of Type 1 errors when conducting multiple hypothesis tests in an experiment with many treatments by using the single-step Bonferroni correction to adjust our p-values [23, 14]. This correction has the advantage of simplicity as well as strong control of the Familywise Error Rate (FWER) in a context where the comparisons being tested are unordered [22].[10]

We used Intention-To-Treat (ITT) estimators to calculate the average effect of each treatment compared against the control condition. What this means practically is that subjects that quit after assignment to a group were still included in calculations as answering incorrectly. ITT estimators have the advantage of correcting for potentially confounding effects of attrition and avoiding the bias introduced into the analysis of many randomized experimental results by regression estimates [9, 8].

RESULTS

Performance on Individual Questions

Looking at the percentage of correct responses per question across all conditions (except demographic control), worker performance varied significantly from chance (random guessing among the available answer choices) for all five questions (see Table 1).[11] On four of the five, workers performed better than chance, whereas the question about revenue streams elicited performance that was significantly worse than chance.

Table 1: Performance on Individual Questions (All Conditions)

	Actual % Correct	% Correct % w/ "Noise"
Avatars	73.2	25
Content rank/rate	25.6	20
User rank/rate	28.7	20
Revenue streams	47.6	50
Soc. network features	62.8	50

χ^2 test indicates all differences significant ($p \leq 0.05$)

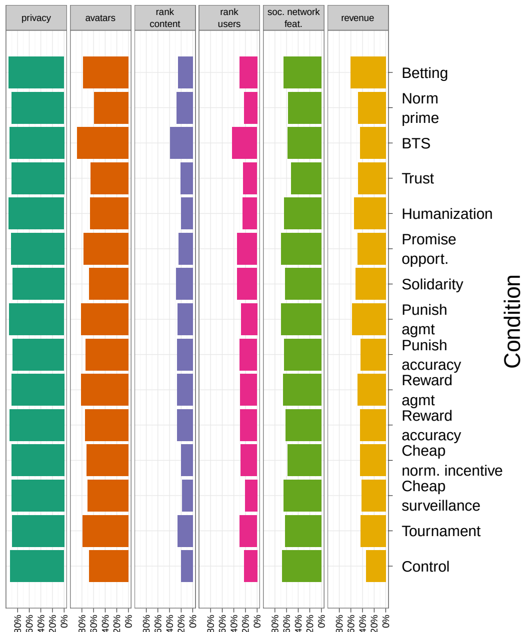
Comparing the percentage of correct answers across questions and across experimental conditions reveals fairly consistent performance from each of the treatment groups despite the substantial variation across questions (see Figure

[9] The design for this treatment comes from [21] who used a near identical method in an effort to elicit honest opinions from their research subjects. After data collection, the responses were subsequently weighted based on the aggregate predicted distributions of the respondents. For our own purposes, we were merely interested in the question of whether presenting our task in a similar way would have a meaningful effect on qualitative information seeking. The results we present do not involve any of the weighting procedures used by Prelec. We refer interested readers to the original paper for more detailed information about this technique.

[10] We calculate these corrections using the "multtest" package in R.

[11] We used χ^2 tests for goodness of fit to calculate these comparisons between the distribution of correct responses and predicted probabilities of producing correct answers through random guessing for each question.

Figure 1: Worker Performance Distribution - All Conditions



1).[12] [13]

Aggregate Performance (All Five Questions)

Figure 2 illustrates aggregated worker performance across all five questions and all experimental conditions. On average, workers did significantly better than chance, which would have yielded a mean of approximately 1.58 questions correct. The actual distribution of responses is strikingly close to normal, with a slight concentration at 2 and a mean of 2.38.[14]

The results of our ITT estimation of average treatment effects (ATE) are reported in Table 2.[15] To facilitate the readability of the table, we only report $p \leq 0.05$.

As described above, we used the "simple" Bonferroni correction for the difference of means comparisons between each treatment group and the control condition. The results suggest that only two of our treatments produced a significant improvement in worker performance over the control: punishment-agreement and Bayesian Truth Serum. In each case, the effect was approximately .5 above the mean outcome in control (2.08). Both were significant at $p \leq 0.05$.

Figure 2: Worker Performance Distribution - All Conditions

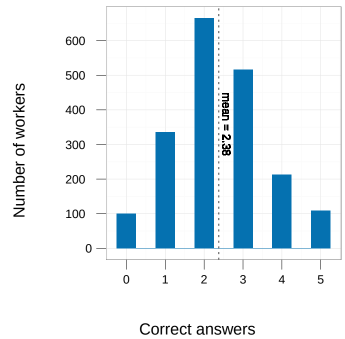


Table 2: Treatment Effects on Aggregate Performance

	Mean	ATE[†]	Std. Err.	p-value[‡]
Control	2.079	NA	NA	NA
Tournament scoring	2.310	0.232	0.142	
Cheap talk-surveillance	2.027	-0.052	0.131	
Cheap talk-normative	2.075	-0.003	0.141	
Reward-accuracy	2.214	0.136	0.139	
Reward-agreement	2.421	0.342	0.135	
Punishment-accuracy	2.275	0.197	0.131	
Punishment-agreement	**2.538**	**0.459**	**0.131**	**0.015**
Solidarity	2.296	0.217	0.149	
Promise-opportunity	2.404	0.326	0.138	
Humanization	2.171	0.092	0.142	
Trust	2.029	-0.050	0.137	
Bayesian Truth Serum	**2.549**	**0.471**	**0.132**	**0.017**
Normative Priming	2.057	-0.021	0.142	
Betting	2.438	0.359	0.137	

[†] ATE calculated using Intention-to-Treat (ITT) estimators.
[‡] p-values reported ≤ 0.05.

Post-hoc Demographic Analysis

To evaluate whether any significant demographic factors may have affected our estimates, we ran an ordinary least squares (OLS) model on the outcome variable (aggregate performance score), incorporating the full set of demographic control variables together with the treatment assignments. The results of this "full" model (not reported here) suggested that three covariates may have had a significant association with worker

[12] We did not conduct hypothesis tests comparing average treatment effects for each question. Such question-level effects were not our primary outcome variables in part because of the specificity of the content of each question and the fact that we looked at responses only from a single website. See the Discussion section below for additional consideration of this topic.

[13] We created both Figures 1 and 2 using ggplot2 in R [27].

[14] This mean reflects only the performance of compliers - not the full set of subjects exposed to treatment. This corrected (ITT) sample mean was 2.26.

[15] The ITT estimate of the ATE captures the mean difference in aggregated performance between the subjects in each treatment condition and the subjects in the control group. The estimates themselves are identical with the results of a linear regression on the same data. The standard errors are different as are the underlying p-values[9, 8]. As discussed above, all p-values have been corrected using the simple Bonferroni correction procedure [23, 14].

performance despite the randomization: web-use skills[16], household size, and country of residence. To zero-in on any potentially confounding effects of these variables, we ran a second model that included only the outcome, the treatment conditions, and these three covariates.[17]

The second model (reported in Table 3) suggests a significant, negative association between performance on our outcome measure, poor web skills and residence in India (both covariates were significant at the $p \leq 0.001$ level after correcting for multiple comparisons). Remarkably, the point estimate of the association between residence in India and the outcome variable dwarfed any of our estimated treatment effects. While household size also remained significant at the $p \leq 0.1$ level, the point estimate dropped below 0.05, implying a weaker association with worker performance.

Table 3: OLS Regression on Worker Performance

	Estimate	Std. Err.	p-value†
(Intercept)	1.851	0.153	0.000
Tournament scoring	0.358	0.143	
Cheap talk-surveillance	0.035	0.147	
Cheap talk-normative	0.131	0.140	
Reward-accuracy	0.232	0.139	
Reward-agreement	0.310	0.139	
Punishment-accuracy	0.230	0.133	
Punishment-agreement	0.482	0.137	0.008
Solidarity	0.291	0.144	
Promise-opportunity	0.398	0.139	0.074
Humanization	0.136	0.141	
Trust	0.047	0.138	
BTS	0.596	0.138	0.000
Normative Priming	0.039	0.138	
Betting	0.437	0.139	0.031
Web skill	0.147	0.024	0.000
Household size	-0.048	0.018	
India resident	-0.739	0.068	0.000

Adjusted $R^2 = 0.127$
† p-values reported ≤ 0.05.

DISCUSSION

Our results suggest a significant, positive effect of two treatment conditions - punishment for disagreement with other workers, and “Bayesian Truth Serum” - on worker performance in a qualitative information seeking task on MTurk. None of the “social” incentive schemes altered performance significantly. This suggests that workers in the MTurk environment may not respond to these sorts of motivational levers.

Even though we consider examples of financial incentive schemes, the fact that they alone succeeded where so many other financial incentive schemes failed hardly implies a ringing endorsement of monetary incentives by the workers.

Rather, we believe the most compelling explanation of these treatments’ relative success derives from the fact that both asked workers to prospectively consider how their peers would answer the information seeking questions. In this regard, it is noteworthy that the third treatment designed along similar lines (rewards for agreement with other workers) did not produce significant effects according to our ITT estimation of difference of means. However, the rewards-agreement condition did produce one of the larger point-estimates, suggesting that this sort of prospective reasoning by workers about their peers may have played an attenuated role in that group as well.

We also find evidence of a strong association between residence in India, web skills, and our outcome variable (information seeking task performance). This implies that culturally-relevant knowledge and experience online may play an important role mediating workers’ ability to perform the sort of qualitative information seeking task we asked them to do here.

At the same time, we do not believe that these demographic factors undermine our findings with regards to the effects of punishment for disagreement and Bayesian Truth Serum. While the association between web skills, residence in India, and our outcome variable were quite strong, their presence in the model hardly altered the point estimates for the effects of the treatment conditions. This suggests that the effects we observed for the treatment conditions (at least the significant ones) were robust and that the randomization worked as a means for distributing these sub-populations evenly across the different treatment groups.

CONCLUSIONS AND FUTURE WORK

The connection we observe between qualitative information seeking performance and treatment conditions asking workers to engage in prospective reasoning about their peers merits further analysis in online and offline settings. In addition, future studies conducted online and with international subject populations should consider the effects of potentially confounding covariates such as country of origin and web-use skills when designing comparable studies. In our case, the randomization proved effective, but this might not be possible in other settings.

Finally, we believe that similar studies should be conducted among other populations online where the existing institutional structure favors other motivational criteria. The rules and norms of the MTurk marketplace strongly favor financial incentives and arms-length relational contracting over more personalistic or socially-oriented modes of exchange. There-

[16] To measure this variable, we borrowed a survey item from an instrument designed, validated, and implemented by Eszter Hargittai in several of her studies.[10] The item asks subjects about their understanding of two web-browsing tools: “tabs” in an internet browser and rss feeds. Hargittai found that both items correlate highly with independent measures of web-browsing and Internet skill.

[17] The fact that country of origin was significant suggested a result consistent with previous findings about the differences between workers from India and the U.S.[16]. As a result, we re-coded country of residence as a binary variable, indicating whether workers self-reported as residing in India or not.

fore, it would be interesting to know whether the same incentive schemes would work among a population of Wikipedia contributors who are accustomed to performing similar tasks without any financial payoffs at all.

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