Designing Informative Rating Systems for Online Platforms: Evidence from Two Experiments

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Abstract

Platforms critically rely on rating systems to learn the quality of market participants. In practice, however, these ratings are often highly inflated, drastically reducing the signal available to distinguish quality. We consider two questions: First, can rating systems better discriminate quality by altering the meaning and relative importance of the levels in the rating system? And second, if so, how should the platform optimize these choices in the design of the rating system?

We first analyze the results of a randomized controlled trial on an online labor market in which an additional question was added to the feedback form. Between treatment conditions, we vary the question phrasing and answer choices. We further run an experiment on Amazon Mechanical Turk with similar structure, to confirm the labor market findings. Our tests reveal that current inflationary norms can in fact be countered by re-anchoring the meaning of the levels of the rating system. In particular, scales that are positive-skewed and provide specific interpretations for what each label means yield rating distributions that are much more informative about quality.

Second, we develop a theoretical framework to optimize the design of a rating system by choosing answer labels and their numeric interpretations in a manner that maximizes the rate of convergence to the true underlying quality distribution. Finally, we run simulations with an empirically calibrated model and use these to study the implications for optimal rating system design. Our simulations demonstrate that our modeling and optimization approach can substantially improve the quality of information obtained over baseline designs.

Overall, our study illustrates that rating systems that are informative in practice can be designed, and demonstrates how to design them in a principled manner.

1 Introduction

Rating systems are an integral part of modern online markets. Marketplaces for products (Amazon and eBay), ridesharing (Lyft and Uber), housing (Airbnb), and freelancing all employ rating systems to enable platform participants to vet each other. Buyers rely on ratings to choose which products to buy and how much to pay, and platforms use ratings to identify both poor and great performers, and in ranking search results. Ratings are consequential: a high score typically directly translates

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to more visibility and sales. Indeed, without effective mechanisms to collect feedback after matches, online markets would be “flying blind” in reducing search frictions between buyers and sellers.

Despite their central importance, extensive prior work suggests the standard rating systems of many online platforms are not sufficiently informative. Rating inflation is an especially common problem. On eBay, more than 90% of sellers studied between 2011 and 2014 had a rating of at least 98% positive, and more transactions result in a dispute than in negative feedback [Nosko and Tadelis, 2015]. On the online freelancing platform oDesk, average ratings rose by one star over seven years, with the increase not fully accounted for by higher seller performance [Horton and Golden, 2015]. On Uber, an average rating of 4.6 out of 5 stars puts a driver at risk of deactivation [Cook, 2015]. On Airbnb, almost 95% of hosts have an average rating of 4.5 – 5 out of 5 stars [Zervas et al., 2015].

Our emphasis in this paper is to investigate whether platforms could improve the quality of information obtained by optimizing the rating scale that they employ. In other words, by optimizing both the meaning and importance of different ratings in a scale, can platforms elicit higher quality information from their raters? We make several key contributions.

First, we establish evidence that optimizing the rating scale can in fact strongly influence the quality of information obtained by a platform. In particular, we analyze results from two tests: a test in the live rating system of a large online labor market, and a synthetic experiment on Amazon Mechanical Turk. In both tests, we ask buyers to choose from a list of verbal phrases (e.g., Best experience ever) or adjectives (e.g., Fantastic!). Our results show that for certain choices of the adjectives shown, the rating distribution obtained can be substantially more dispersed than under the “standard” star rating scale. Most starkly, on the labor market, 80.6% of freelancers received the best possible numeric (i.e., star) rating, but less than 35.8% were rated with the highest-ranked verbal phrase across non-numeric treatment cells. This finding suggests that in platforms today, the norm is that any acceptable experience is given the top possible rating, with the rest of the scale reserved for various degrees of unacceptable experiences. By breaking this norm, the platform can substantially improve the quality of information obtained from ratings. Our test on the labor market also provides evidence that inflation over time can be countered; ratings on our additional question did not inflate over the test time period, in contrast to an inflation of about 0.3 points (on a five star scale) over a similar time period after the introduction of a new numeric rating system on the same platform, cf. Filippas et al. [2017]. These experimental findings establish that rating behavior responds to the rating system’s design.

Second, motivated by our empirical findings that rating behavior can be influenced by the system design, we develop a theoretical framework to compare various system designs and optimize such a system. Our view is that the goal of a rating system is to learn about sellers as quickly as possible. Accordingly, we develop a framework where the goal of the platform is to optimize the rate of convergence of the seller ranking via observed score to the true underlying seller quality distribution. Formally, we consider the problem of quantifying a particular rating system design’s large deviations rate of convergence to the true quality distribution, and of choosing the design that maximizes this rate.

We develop a stylized model for rating system design, within which we carry out this optimization. In our model, buyers rate sellers using a multi-level rating scale, i.e., buyers are asked to
answer a multiple choice question (e.g. 1-5 stars, or a set of adjectives) when rating the buyer. The platform can choose which levels (e.g., adjectives) to show in the scale, as well as the scores to assign to these adjectives. We define a fictitious “marketplace” in which sellers accumulate ratings over time, with match rates proportional to their quality. We show how to find an approximately optimal rating system design, given behavioral data regarding how people rate sellers using the different possible scales.

Third, we evaluate our design methodology empirically. In particular, using data collected from our experiment on Amazon Mechanical Turk, we calibrate a behavior model that allow us to compute optimal multi-level rating scales. We show through simulated markets that optimization of the rating scale can substantially improve learning rates.

Taken together, our results suggest that platforms have much to gain by optimizing the meaning of the levels in their rating systems. Our behavioral findings provide a positive counter-balance to an empirical ratings literature that has focused on the causes of rating inflation: the platform has a design lever to deflate ratings. Our theoretical work develops a methodology to use this lever effectively, and our empirically calibrated simulations validate the approach. More generally, the managerial insight is that ratings on online platforms are not doomed to be highly inflated; rating behavior is responsive to how the system is designed, and good rating behavior can be both quantified and obtained through a structured design methodology.

The remainder of the paper is organized as follows. Section 2 contains related work. In Section 3, we describe the labor market test and its results, demonstrating that design of the rating scale can substantially alter the information obtained in a rating system. In Section 4 we describe a model and approach to optimizing a multi-level rating scale. Finally, in Section 5, we use an experiment on Amazon Mechanical Turk to both confirm our results in the labor market test, as well as to study the results of optimal system design in an empirically calibrated model.

2 Related literature

Challenges in designing effective online rating systems are well-documented. To help explain the empirical inflation findings discussed above, one branch of the literature focuses on how ratings are given after bad experiences, and in particular conditions under which buyers either don’t leave a review at all or leave a positive review. On Airbnb, for example, Fradkin et al. [2017] find that inducing more reviews resulted in more negative reviews, suggesting that those with negative experiences are less likely to normally submit a review. Though historically this inflation has been thought of as a strategic response to potential retaliation, recent evidence indicates that social pressure also plays a role. For example, Fradkin et al. [2017] find that when sellers incentivize reviews (of any kind) by offering discounts, they create an implicit social obligation for reviewers to reciprocate with a positive review; other works also tackle this question [Li and Xiao, 2014, Cabral and Li, 2014].

2.1 Rating equilibrium and inflation over time

One important aspect of rating behavior on online platforms is that it is not static. Filippas et al. [2017] show that inflation happens over time – on the same online labor market as in our test, average public ratings over a span of nine years went from below 4 stars to about 4.8 stars. This view is consistent with the “disequilibrium” view of rating system design described by Nosko and Tadelis [2015].
2.2 Models of rating behavior

Recent literature has attempted to explain rating behavior, and inflation in particular, through a variety of models [Immorlica et al., 2010, Cabral and Hortasu, 2010, Horton and Golden, 2015, Filippas et al., 2017, Fradkin et al., 2017]. Much of this work seeks to understand how buyer incentives may result in an equilibrium in which they provide with dishonest ratings, or how sellers may be incentivized to accumulate high ratings and then give low effort. For example, Filippas et al. [2017] posit that high ratings are unavoidable when sellers are affected by negative ratings, as buyers are incentivized to incorrectly give positive ratings even after negative experiences. We discuss such models in further detail at the end of Section 3, including how our experimental results suggest they are incomplete.

2.3 Platform measures to counter or encourage inflation

Platforms are aware of the inflation problem and have invested in fixing it. Most existing solutions try to decrease retaliatory pressure from sellers or to encourage more buyers to submit reviews. In 2007, eBay implemented one-sided feedback (i.e., only buyers rating sellers), with anonymous ratings only presented in aggregate; the platform later eliminated negative buyer ratings altogether [Bolton et al., 2013]. Through a test with private feedback, oDesk reports that such feedback predicts both future private and public feedback better than does public feedback, and there is evidence that buyers utilize private ratings more than they do public ratings [Horton and Golden, 2015]. (“Public ratings” are those that are shown publicly, non-anonymized, e.g. “A rated B 5 stars.” “Private ratings” are either shown as a summary statistic, e.g. “B averages 4.6 stars”, or not shown at all, used only internally by the platform.) Other work has attempted to align buyer incentives with providing informative reviews [Gaikwad et al., 2016], but the approach has not yet been widely adopted. Despite such fixes, the problem of inflation largely remains on online platforms: even these systems are vulnerable to inflated ratings, consistent with the hypothesis that norms have shifted so that even average experiences are given the top numeric value.

2.4 Survey design

The idea of using labels for scales in survey responses is well studied, including a recognition that the specific words, number of words, and their positive-negative balance affect responses [Krosnick, 1999, Parasuraman et al., 2006, Klockars and Yamagishi, 1988, Hicks et al., 2000]. A major contribution of our work is to show that the effect of scale design on responses is first order in real rating systems, despite the presence of incentive issues as discussed above. While this point may not seem too surprising, as discussed above our study is preceded by a long line of rating systems work in which substantive changes (making ratings private, trying to prevent retaliation, UI changes) do not in practice lead to informative rating systems. Given the potential costs for giving negative ratings posited by previous work, it is not clear a priori that any change will induce raters to do so.

Furthermore, it is important to note that the survey design literature largely focuses on recovering a distribution of beliefs about a single item (e.g. how many people like a given product) rather than a ranking over multiple items. Beyond this literature, our work studies design and analysis of rating systems in the context of a platform that needs to recover information about many sellers with the same question.
2.5 Learning rates

We conclude by noting that several recent works also study the speed of learning in rating systems and other settings. From a theoretical standpoint, our approach is similar to Glynn and Juneja [2004]. In that work the authors use a large deviations approach to determine how to most efficiently identify the best processor in a set.

More relevant are a number of works on learning rates in online platforms. Both Ifrach et al. [2017] and Acemoglu et al. [2017] study the Bayesian learning rate of buyers who have access to the history of a seller’s ratings before matching. These papers concern themselves primarily with the way that the granularity of ratings may affect which buyers choose to match, and thus bias the data collected by the platform. Che and Horner [2015] studies how a platform can use recommendations to more quickly learn about new sellers, and Johari et al. [2016] analyzes how to match buyers to sellers in order to minimize regret while simultaneously learning about seller types. What these works have in common is that the platform is influencing which matches occur through its design, and this affects the learning rates. In contrast, we take the matches as given and show how the platform can meaningfully design what it learns from each match. Finally, in a related working paper [Garg and Johari, 2018], we consider the optimal design of binary rating systems, for which far more theoretical structure exists.

3 Rating behavior in an online labor market

Our work focuses on whether we can improve the design of the feedback systems used in online platforms. However, such a direction is irrelevant if buyers’ rating behavior does not change in response to changes in the system. Indeed, the literature suggests that despite substantial effort across a variety of platforms, rating behavior has not changed for the better over time: average ratings on platforms tend to be extremely high or “inflated” (see discussion in Section 2 and references therein). Across numerous platforms, as detailed above, ratings systems and their resulting distribution of ratings do not provide information that can effectively and efficiently differentiate high from low quality participants.

In this section, we begin our study with a simple but under-explored innovation in the design of a rating system: changing the description attached to each level in a rating scale. We study the effect of such a change through the results of a randomized controlled trial on the rating system of a large online labor market. In this test, new ratings questions of various kinds were introduced on a feedback form clients submit upon finishing a job with a freelancer. As discussed below, the results overwhelmingly demonstrate that changing the levels in a rating system can lead to substantially more informative ratings.

3.1 Motivation and hypothesis

We aim to design rating scales for online platforms that lead to more informative ratings. Motivated in part by the emergence of the rating norms discussed in the introduction, where 5 stars is routinely considered “average,” we are interested in evaluating the effectiveness of changes that can counter this norm: in particular, positive-skewed rating scales with specific descriptions attached to each rating.

Our hypothesis is that such scales provide much more informative ratings than do standard, numeric rating scales, and in particular, do not lead to “inflated” ratings (i.e., ratings where a large majority of the distribution is on the highest rating score).
In Section 3.3.1 we discuss the treatment conditions, and motivate specific aspects of the test design. In Section 3.4, we report the associated findings. We did not hypothesize any specific effects (i.e., direction or magnitude) of the tested designs, except for the hypothesis that the non-numeric rating scales would be less inflated than the numeric scale.

3.2 Empirical context

The test ran on a large, online labor market. In this market, clients seek the services of freelancers across a variety of categories (e.g. software development, graphic design, and translation). Clients may choose to contract with a freelancer for a job based on work history, prior ratings, the freelancer’s proposal, and potentially an initial conversation. A client-freelancer pair may work on multiple jobs together during their time on the platform.

At the end of a job, the client is asked to fill out a feedback form in which they rate the freelancer’s work, through a series of multiple choice and free-form questions. This labor market has both private and public ratings, and private ratings are aggregated and made available to potential future clients as part of a freelancer’s public score. Both private and public ratings are high on the platform: even the average private feedback score is over 8.5/10. See Filippas et al. [2017], which analyzes ratings over time on the same labor market, for an in-depth description of the status-quo rating system and its performance.

3.3 Method

We now describe our test method. The authors were involved in test design and analysis of anonymized data, but not implementation or deployment.

In the test, an additional question was added to the feedback form given to clients after they close a job. This question appeared with the current private rating questions and was marked optional. All clients were still asked the existing private rating questions, including rating the freelancer on a numeric 0–10 scale. The answer choices are displayed vertically, after the question.

The test ran over a 90 day period in Summer 2018, with a pilot in January 2018 over 5 days. As the results from the pilot are nearly identical to the longer test, we report the set-up and results of the long test here. Results of the pilot are provided for comparison in the Online Companion.

3.3.1 Treatment conditions

There were six treatment conditions that included an additional question on the feedback form. The question phrasing and answer choices differed between the treatment conditions. See Table 1 for a detailed list of the treatment conditions. There were four different types of answer choices: (1) comparing against a client’s expectations (Expectation); (2) descriptive adjectives (Adjectives); (3) comparing against the average freelancer the client has hired, as well as two variants (Average; Average, not affect score; Average, randomized); and (4) a numeric scale with no descriptions attached to the ratings (Numeric).

The non-numeric treatments are possible ways to design multiple choice rating systems that add more specificity to the rating scale. The choices themselves are skewed toward the positive end: each scale has two “negative” choices, one “neutral” choice, and 3 “positive” choices, in increasing levels of effusiveness. This imbalance was chosen so that (a) clients could give “positive” feedback to most freelancers while still allowing the platform to disambiguate the very best from others, and (b) to emphasize that the best ratings should be reserved for the very best freelancers.

The Numeric treatment, giving freelancers the option of giving 0 – 5 stars, helps disambiguate between novelty effects of introducing new questions and the idiosyncratic effects of the question
Table 1: Treatments groups for labor market test

| Treatment         | Additional Question                                                                 | Answer choices                                                                 |
|-------------------|--------------------------------------------------------------------------------------|--------------------------------------------------------------------------------|
| **Expectation**   | How did this freelancer compare to your expectations?                                | Much worse than I expected, Worse than I expected, About what I expected, Better than I expected, Far better than I expected, Beyond what I could have expected |
| **Adjectives**    | How would you rate this freelancer overall?                                          | Terrible, Mediocre, Good, Great, Phenomenal, Best possible freelancer!          |
| **Average**       | How does this freelancer compare to others you have hired?                           | Worst Freelancer I've Hired, Below Average, Average, Above Average, Well Above Average, Best Freelancer I've Hired |
| **Average, not affect score** | How does this freelancer compare to others you have hired? (This will not impact the freelancer’s score) | Same as Average group |
| **Average, randomized** | How does this freelancer compare to others you have hired? | Same as Average group, but in random order |
| **Numeric**       | How would you rate this freelancer overall?                                          | 0, 1, 2, 3, 4, 5                                                               |

itself. As in the other treatments, this question is asked in addition to the the existing rating questions on the site, which include a 0 – 10 overall rating question. Furthermore, the question phrasing is identical in the Adjectives and Numeric treatments; only the answer choices differ. This design thus teases out the different effects of the type of question itself and the answer choices.

The two additional variants of the question asking freelancers to compare to averages are: (a) with an additional text emphasizing that the answer will not impact the freelancer’s publicly displayed rating (Average, not affect score), and (b) by randomizing the order of the answer choices (Average, randomized). The first variant tests the additional informational gain from clients knowing for certain that a low rating will not affect the freelancer. The second variant helps assess the propensity of clients to not read all the answer choices before responding.

In addition to the six treatments, a Control condition was included, in which no additional question is asked (replicating the status quo feedback form).

3.3.2 Allocation to treatment groups

Allocation was done at the client level when they first closed a job and landed on the feedback form after the start of the test. Clients who had closed less than two jobs in the past were excluded, as several of the treatment conditions ask clients to compare the freelancer to past experiences. Each treatment condition was allocated 15% of the clients, and the remaining 10% of clients were allocated to Control. After being allocated to a treatment group, a given client is assigned the same treatment for the duration of the test and is thus shown the same additional question for any further jobs she may close. (During the pilot in January 2018, 40% of clients were allocated to Control and 10% to each treatment condition.)

Due to a bug in the allocation code during the test, 1,086 out of the 66,755 clients who submitted feedback were assigned to different treatment conditions on different closed jobs. We disregard all such clients in our analysis to eliminate the possibility of contamination between treatment cells. To confirm experimental validity, we show in the Appendix that otherwise the randomization was nevertheless effective: the distribution of clients in different cells are essentially identical on all
observed covariates. This bug does bias the client population in our data in one way, however: clients who close more jobs in the test period were more likely to experience the bug, and thus to be incorrectly assigned to multiple treatment cells. As a consequence, the client population on which we carry out our analysis skews away from the highest volume clients on the platform.

3.3.3 Number of responses and data preprocessing

75,592 unique clients landed on the feedback page, with 66,755 clients submitting feedback for at least one job. We remove the 1,086 clients mistakenly assigned to multiple treatment cells (per the bug described above), as well as seven clients who were correctly assigned but who closed more than 200 jobs during the test period. Table 2 contains, for each treatment cell, the numbers of clients assigned, clients who submitted a job, and clients and jobs in our dataset after the pre-processing.

3.4 Results

We now report several key results from the test. We have provided additional analysis in the Appendix. In particular, our results there demonstrate that the findings reported in this section remain essentially identical even if we use other approaches to the analysis; for example, if we sample only one job per client, if we include all valid clients (i.e. including those with more than 200 jobs submitted), or if we even include the invalid incorrectly allocated clients.

3.4.1 Snapshot analysis

Figure 1a shows the rating distributions for each treatment group, and Table 2 also contains the mean treatment response in each group. There is a large and significant difference between the rating distribution from the numeric scale and each of the other treatment groups. Each treatment cell is different from each of the others at \( p < 10^{-100} \) using the Kolmogorov-Smirnov two-sample test, except for the Average and Average, not affect score cells, where \( p > 0.1 \). While the Numeric treatment ratings follow the J-curve pattern usually seen in ratings, the other treatments are far more evenly distributed as desired. Most starkly, 80.6% of ratings on the Numeric scale are 5/5, while at most 35.8% of responses on any other scale received the highest possible rating.

The substantial effect size of the difference between the Numeric condition and the other treatments confirms our hypothesis that specific and positive skewed scales in particular are an effective way to counter inflation. It is clear from this finding that the answer choices presented to the rater are a first-order determinant of rating behavior.

Rating heterogeneity  One concern regarding these distributions is that there may be significant rating heterogeneity with respect to client, job, or freelancer characteristics. In the following section,
we propose a system optimization that takes as input data on how rating distributions vary with the answer choices offered to raters; if these distributions vary significantly with respect to client, job, or freelancer covariates, such a “global” optimization and rating system design may not be feasible.

However, we find that rating distributions are remarkably consistent across such possible covariates. In the Appendix, through a regression we show that the heterogeneity of ratings given observable covariates (job value, job category, expertise required, previous client spend, and others) is small: the treatment response coefficients for different values of these categorical covariates on the order of 0.1, i.e., average treatment ratings in different subsets of the market (within a treatment cell) are within about 0.3 of each other, much smaller than the difference between the Numeric treatment and other treatments. This non-heterogeneity suggests that the same verbal scale may be interpreted and used on different parts of the platform.

Randomizing answer choices One of the treatment conditions, *Average, Randomized* contained the same question and answer choices as the *Average* condition, but the answer choices were presented in a random order. If the raters read all the answer choices and then pick the most applicable one, then this condition would have returned a rating distribution identical to that of the *Average* condition. However, it does not (recall, with $p < 10^{-100}$). Furthermore, the location of the chosen answer choice would be distributed uniformly ($\frac{1}{6} = 16.6\%$ each), e.g., the rater should be no more likely to pick the answer choice presented first as she is to pick the answer choice presented third. Again, we find this not to be the case: the first answer choice presented to the rater is picked $\frac{6806}{26978} = 25.2\%$ of the time. The second through sixth answer choices are picked $17.3\%, 14.7\%, 14.3\%, 13.9\%,$ and $14.5\%$ of the time each, respectively.

This phenomenon suggests that (a) a small percentage (up to $10 - 13\%$) of raters do not read the answer choices at all and simply select the first answer choice, and (b) many raters start reading from the first presented answer choice, select a choice as soon as they find one that approximately describes their experience, and move on instead of reading the rest of the choices. Our test design cannot disambiguate between these (or other plausible) explanations or weigh their relative importance, and we leave the study of such behavior to future work. Nevertheless,
note that this effect is second-order relative to the overall finding that more descriptive scales are substantially more informative than numeric scales.

3.4.2 Inflation over time

The above analysis provides a snapshot view of what happens when a new question is added to the rating form. Some of the rating dispersion may be a novelty effect that decreases over time. As Filippas et al. [2017] emphasize, a substantial component of rating inflation in online platforms happens over time, on the order of months or even years. Here, we analyze whether ratings on the new questions inflated in the time period of the test.

We find that the rating scales do not inflate substantially. Figure 1b shows the average rating per treatment group over the 90 days after the launch of the test, in a sliding window of 7 days. There is no discernible inflation over time. It is instructive to compare the (lack of) inflationary trend to the inflation after the launch of a new numeric scale on the same platform in 2007, as reported by Filippas et al. [2017]: average ratings inflated from about 3.8 stars to about 4.1 stars in the first three months of the system launch. (Note that introducing a new Numeric question in 2018 yields immediately inflated responses, suggesting that current platform users have been conditioned to the norm of inflated ratings.)

One concern with drawing conclusions from the preceding analysis over time is that there may not be enough clients who actually submit multiple jobs during the test period, and so novelty effects may still predominate when looking at overall averages. To study this concern, we analyze the ratings given by the clients who submitted at least 10 ratings each. We then run a regression for treatment response, as before, with a covariate indicating how many previous jobs the client had submitted during the test period. Appendix Section B.4 has the associated table and discussion. Even for such high-volume clients, inflation is slow: ratings may be inflated by a full point after a client has given 100 ratings.

On the positive side, this finding suggests that as long as new clients continue to enter the platform, ratings should remain informative for a long time horizon. Indeed, given that existing norms are strongly biased towards inflationary ratings (as evidenced by clients’ responses to the Numeric question), it is quite valuable to the platform to see no evidence of inflation in the other treatment groups within a three month period. Of course, in principle it remains possible that over a timescale much longer than that of this test, norms would shift again towards inflated ratings. A longer-term longitudinal analysis of this type of inflationary behavior remains an important direction for future work in this area, though of course data collection over such a long time horizon is a significant obstacle.

3.5 Discussion

These results suggest that a platform can find large improvements over standard rating systems by defining what each rating means in an explicit and designed manner. In particular, though ratings still tend positive in absolute terms (over 80% of freelancers receive Above Average or better), clients seem hesitant to give most freelancers the best possible score when such a score is interpreted as truly exceptional. Thus we receive extra information about sellers from such scales. In the remainder of the paper we develop a framework for optimal design of the rating scale, and confirm the results of the labor market test as part of a study implementing our optimal design methodology on Amazon Mechanical Turk in Section 5.

We briefly conclude by interpreting our results through the lens of a utility model for users; such an approach has been taken in prior work as well. In particular, suppose that users experience two
types of costs: a *dishonesty cost* whenever they choose not to tell the truth about their experience; and a *reciprocity cost* for giving a negative rating. The latter encodes social pressure or even retaliation (implicit or explicit) that a buyer may consider before giving a negative rating. Similar models have been considered in, e.g., Filippas et al. [2017] and Fradkin et al. [2017].

With such a model, for each rater there is a threshold on the reciprocity cost at which she “misreports” or is dishonest in her rating: in particular, as soon as she feels the disutility of a negative rating outweighs the disutility of not being honest. Therefore, the platform could find itself in an equilibrium where all sellers receive a positive rating, regardless of how bad an experience they provide. Under such a model, the rating distribution is invariant to the question asked once this rating cost threshold is crossed.

Our findings suggest that there are countervailing forces that can induce ratings to be more dispersed than in existing systems, by shifting how buyers interpret the rating scale. In particular, our view is that the dishonesty cost to a rater is an increasing function in how dishonest she perceives herself to be. Crucially, this quantity varies both with experience quality and the rating system design. With standard numeric rating systems and today’s norms, a rater arguably does not consider herself dishonest for rating mediocre experiences 5/5, because that is what 5 stars has come to mean. On the other hand, suppose a platform provides explicit guidance on what ratings mean (e.g., “5 stars means best experience you’ve had”); raters would thus face a higher cost of dishonesty for giving low quality sellers a high ratings. These findings are consistent with the self-concept maintenance literature, in which people are understood to be more likely to be dishonest when they can convince themselves that they are acting honestly [Mazar et al., 2008].

### 4 Optimizing the multi-level rating scale

The preceding section shows that a platform can improve the information obtained through the rating system through careful choice of the descriptions for each level of a multi-level rating scale. This finding naturally prompts the question: which rating scale design is “best” for the platform? We now develop a framework to compare designs and to find an optimal design of the rating scale.

We are primarily motivated by the finding that the Adjectives design performed well in our labor market test: but why did we choose the particular adjectives we used in the scale, amongst a whole possible universe of adjectives? For the other scales, why did we skew in the particular way we did? Different adjectives choices are sure to induce different rating distributions, as we reinforce in Section 5. How do we choose among the scales now we have observed the rating behavior induced by them?

In this section we consider the problem of choosing an optimal set of \( K \) adjectives from a larger, totally ordered set of possibilities. This approach can also be used to quantitatively compare the performance of two different designs, e.g. *Expectations* and *Average*.

In particular, we take the perspective that the objective is to ensure that the ranking of sellers based on their aggregate rating score converges to the true ranking at the fastest rate possible in the number of rankings received. We develop a stylized model to formalize this notion, and use it to develop an approach to optimization of the rating system. The stylized model we consider has the following key elements. We assume that buyers enter per time period and match with long-lived sellers, potentially at varying rates according to the seller’s quality. After the match, the buyer rates the seller; the rating behavior depends on the levels (answer choices, e.g. the adjectives or other answer phrasings in Table 1) of the rating scale. The platform’s design levers are the set of rating levels making up the rating scale, and the scores it attaches to those adjectives. We leverage this stylized model to propose an approach to maximize the rate of convergence (in a large
deviations sense) of the estimated ranking based on sellers’ aggregate scores, to the true underlying ranking based on sellers’ qualities. In the next section, we apply this optimization methodology to an empirical setting based on data collected through Amazon Mechanical Turk.

4.1 Model

Our model is constructed to emphasize the platform’s learning rate of participants through its rating system. It is deliberately stylized, so that we can derive a relatively straightforward method for optimization of the system. The key components are as follows.

**Time.** Time is discrete: \( k = 0, 1, 2, \ldots \).

**Sellers.** We assume the system consists of a unit mass of sellers, each associated with a quality type \( \theta \), which is (initially) unknown to the platform. We assume \( \theta \) is drawn independently and uniformly at random from a finite and totally ordered set \( \Theta \), with \( |\Theta| = M \). We will use \( \theta_i \) to denote the \( i \)th element of \( \Theta \) within this order, for \( 0 \leq i < M \).

In addition, each seller has an aggregate score, described further below; we let \( x_k(\theta) \) denote the aggregate reputation score of the seller of type \( \theta \) at time \( k \).

**Rating accumulation.** Sellers accumulate ratings over time by matching with buyers. Each time step, each seller matches with at most a single buyer. We make one key assumption that drives the accumulation of ratings: in particular, that sellers of higher quality are more likely to be matched. We consider an analysis that is asymptotic in the number of ratings received by sellers and so we model this visibility benefit by assuming that sellers of higher quality accumulate ratings at a faster rate. In particular, we assume the existence of a nondecreasing match function \( g(\theta) \), where a seller of type \( \theta \) receives \( n_k(\theta) = \lfloor kg(\theta) \rfloor \) matches, and thus ratings, up to time \( k \).

Our approach to modeling rating accumulation is stylized in at least two important ways. First, the matching function is artificial: in general, sellers will be more likely to match when they have a higher observed aggregate score, and there may be other heterogeneity. Second, we consider all sellers to have the same age: i.e., at time step \( k \), all sellers have had \( k \) opportunities to receive ratings. In reality, of course, sellers will have different ages in the marketplace. We discuss this choice further in our empirical investigation in Section 5. These choices allow us to develop a clean approach to optimizing the learning rate.

**Ratings.** How are sellers rated? After each match, the seller receives a rating in the form of a multiple choice question answered by the buyer. The platform makes two decisions at the beginning when designing this question. First, we assume the platform chooses a set of rating levels \( Y \) to create the rating scale, of size \( |Y| = K \). We assume that \( Y \subset \mathcal{Y} \), where \( \mathcal{Y} \) is a totally ordered “universe” of available levels. (For example, in our labor market test, \( \mathcal{Y} \) is a set of adjectives available to describe rating levels, and we chose a subset to display in the Adjectives treatment.)

Second, whenever a seller receives a rating \( y \in Y \), we assume the platform gives this rating a score \( \phi(y) \in [0, 1] \) depending only on the rating received. The score represents the relative positivity assigned to a rating \( y \): higher scores indicate this rating will more help the seller’s aggregate score (as we formally describe below). Platforms often and naively use equi-spaced scores when translating rater’s choices to an aggregate score, e.g. the choice “5 stars” translates to a numeric 5 when averaging, but we allow the possibility that this choice should also be optimized.

At each rating opportunity (i.e., match made), we assume the seller receives a rating from the set \( Y \). We suppose that this rating depends only on the true type of the seller. In particular, we presume that the probability a seller of type \( \theta \) receives a rating \( y \) or higher is \( R(\theta, y) \). (This does not depend on the whole set \( Y \); see discussion below.) We make the natural assumptions that \( R(\theta, y) \) is strictly increasing in \( \theta \) and strictly decreasing with \( y \).
Given a particular choice of the set $Y$, the probability a seller type $\theta$ receives exactly rating $y$ is:

$$\rho(\theta, y|Y) = \max_{y' \in Y : y' \prec y} R(\theta, y') - R(\theta, y)$$  \hspace{1cm} (1)

To ensure that $\rho$ is a probability distribution, we assume a unique lowest rating $0$ is in all $Y$, with $R(\theta, 0) = 1$, and $\rho(\theta, 0|Y) = 1 - \sum_{y \in Y : y \neq 0} \rho(\theta, y|Y)$.

Let $y_0(\theta), y_1(\theta), y_2(\theta), \ldots$ be the sequence of ratings received by the seller of type $\theta$. The aggregate score up to time $k$ of this seller is the average score from ratings received:

$$x_k(\theta) = \frac{1}{n_k(\theta)} \sum_{\ell=0}^{n_k(\theta)} \phi(y_\ell(\theta)).$$  \hspace{1cm} (2)

(We presume $x_0(\theta) = 0$ for all $\theta$.) Since $\phi(y) \in [0, 1]$ for all $y$, then $x_k$ also lies in $[0, 1]$.

This rating behavior is also a strong assumption. First, it does not capture heterogeneity across raters (the types of sellers a buyer matches with may correlate with the buyer’s rating behavior in general). Including such heterogeneity is an interesting direction for future work; indeed, empirical identification of such heterogeneity presents an interesting practical challenge. Second, it presumes that the rating assigned to a seller is not affected by the other levels in $Y$, and we do not model the order in which the levels in $Y$ are shown. We believe our approach is a reasonable first approximation for system design; such effects would substantially complicate the analysis and experimental approach, and we leave their inclusion for future work. As our the labor market test shows, the order in which rating levels are presented has an effect on rating behavior, but it appears to be second-order.

**System state.** We represent the state of the system defined above by a joint distribution $\mu_k(\Theta, X)$, which gives the mass of sellers of type $\theta \in \Theta$ with aggregate score $x_k(\theta) \in X$ at time $k$. Throughout our model presentation, we describe the system model as one emerging from interactions between individual buyers and sellers. However, we assume a unit mass of sellers (and some mass of buyers), and so all such descriptions should be viewed as illuminating the evolution of a joint distribution $\mu_k(\Theta, X)$ of the types of sellers on the platform and their current scores. To formally describe the evolution of $\mu_k$, let $E_k = \{ \theta : n_k(\theta) = n_{k-1}(\theta) + 1 \}$. These are the sellers who receive an additional rating at time $k$; for all $\theta \in E_k$, $n_k(\theta) = n_{k-1}(\theta)$. Next, for each $x, x' \in [0, 1]$, define $\chi_k(x, x', \theta|Y)$ as:

$$\{ y : n_k(\theta)x - n_{k-1}(\theta)x' = \phi(y) \}.$$  

The set $\chi$ describes the rating(s) a seller of type $\theta$ at time $k$ with aggregate score $x'$ can receive to transition to aggregate score $x$. We then have:

$$\mu_{k+1}(\Theta, X) = \int_{E_k} \int_0^1 \int_X \sum_{y \in \chi_k(\theta, x, x'|\theta, \phi)} \rho(\theta, y|Y) dx \mu_k(d\theta, dx') + \int_{E_k} \int_X \mu_k(d\theta, dx').$$

It is straightforward but tedious to check that the preceding dynamics are well defined, given our primitives.

**Platform objective.** We assume that the platform wants the ranking of sellers by observed aggregate score to reflect the underlying true quality ranking as closely as possible.

Formally, given $\theta_1 > \theta_2$, define $P_k(\theta_1, \theta_2)$ as follows:

$$P_k(\theta_1, \theta_2) = \mu_k(x_k(\theta_1) > x_k(\theta_2)|\theta_1, \theta_2) - \mu_k(x_k(\theta_1) < x_k(\theta_2)|\theta_1, \theta_2).$$  \hspace{1cm} (3)

This expression captures the “errors” being made by the ranking according to observed score. In particular, when $\theta_1 > \theta_2$ but $x_k(\theta_1) < x_k(\theta_2)$, the aggregate score ranking swaps the ordering of sellers $\theta_1$ and $\theta_2$. Thus, for a good rating system the goal is to ensure that $P_k(\theta_1, \theta_2)$ is large.
In particular, we consider the problem of maximizing the following objective, a scaled version of Kendall’s \( \tau \) rank correlation between the estimated ranking of sellers and the true ranking:

\[
W_k = \frac{2}{M(M-1)} \sum_{\theta_1 > \theta_2 \in \Theta} P_k(\theta_1, \theta_2) \quad (4)
\]

The coefficient ensures that \( W_k \) remains bounded even as \( M \) increases. This objective depends on the model primitives \( R \) (rater behavior) and \( g \) (matching rates), as well as the platform’s decisions \( Y \) (levels) and \( \phi \) (score).

We note that, in this model, the goal of the rating system is to accurately rank sellers by quality. Another approach may be to directly optimize for total platform revenue or aggregate welfare. This approach would require primarily optimizing which matches occur, a focus of many other works. We optimize information gained per match, for which finding the true ranking of sellers is a reasonable objective. One observation in support of this choice is that the “deliverable” for the ratings team in an online platform company is typically an accurate rating that can be input to models used by other business teams throughout the organization. Further, in a model where matching rates are exogenously determined by quality, we conjecture that optimizing other objectives (accuracy and revenue) would produce qualitatively similar results.

Learning \( R(\theta, y) \). We conclude with a remark on how a platform can learn \( R(\theta, y) \). The idea is to rely on estimates of \( \theta \) for a group of “known” sellers; e.g., these may be long-lived sellers on the platform. The platform can then test new rating scales, and use the resulting data to estimate \( R \). We follow this approach in Section 5 using items for which we have expert ratings.

4.2 Optimizing the system

As noted above, the platform has two design choices it makes: the set of rating levels \( Y \), and the score function \( \phi \). In this section, we consider an approximate approach to maximization of the objective \( W_k \), by appropriate choice of \( Y \) and \( \phi \).

No single choice of \( Y \) and \( \phi \) can simultaneously optimize \( W_k \) for all \( k \): some designs may be effective in separating the best sellers from the worst quickly, but then never separate all sellers. Further, as long as \( \phi(y) \) is strictly increasing, then because \( R(\theta, y) \) is strictly increasing in \( \theta \), we have, for all \( \theta_1 \neq \theta_2 \), and all choices of \( Y \) and \( \phi \): \( \lim_{k \to \infty} P_k(\theta_1, \theta_2) = 1 \). Using the bounded convergence theorem we conclude that \( \lim_{k \to \infty} W_k = 1 \) is constant, independent of the design choice \( Y \) and \( \phi \). Thus any design is asymptotically optimal.

For these reasons, we focus on maximization of the rate at which \( W_k \) converges. We use a large deviations approach to study the rate of convergence [Dembo and Zeitouni, 2010]; we can use this approach to show the following result.

**Theorem 1.**

\[
r \triangleq -\lim_{k \to \infty} \frac{1}{k} \log(1 - W_k) = \min_{0 \leq i < M} \inf_{a \in \mathbb{R}} \{ g(\theta_{i+1})I(a|\theta_{i+1}) + g(\theta_i)I(a|\theta_i) \} \quad (5)
\]

where \( I(a|\theta) = \sup_z \{ za - \Lambda(z|\theta) \} \), and \( \Lambda(z|\theta) = \log \sum_{y \in Y} \rho(\theta, y|Y) \exp(z \phi(y)) \) is the log moment generating function of a single rating given to seller of type \( \theta \).

The proof follows from a standard properties of large deviations and is in the Online Supplement. The expression in (5) is called the large deviations rate for \( W_k \). The theorem shows that \( W_k(\theta_1, \theta_2) \to 1 \) exponentially fast, and provides an explicit relationship between our choice of \( Y \) and \( \phi \), and the corresponding exponent. In other words, \( 1 - W_k = \mathcal{O}(e^{-rk \text{poly}(k)}) \). Two rating
systems can be compared by their respective learning rates: for each design, simply calculate their rates and then compare.

Our optimization problem is thus as follows: choose \( Y \) and \( \phi \) to maximize the large deviations rate \( r \) in (5). We use the following brute force approach to optimization: for each \( Y \subset Y \), choose a random, increasing set of scores \( \phi(y) \in [0, 1], \forall y \in Y \) in each iteration. Given \( \phi \) and \( Y \), the right hand side of (5) can be efficiently calculated as a convex minimization. Then, for each subset \( Y \), run a large (exponential in \( |Y| \)) number of iterations (each with random scores \( \phi \)). Finally, choose the design \( Y, \phi \) with the best rate.

4.3 Discussion and practical advice

The above approach appears computationally expensive: finding the optimal \( Y \) and \( \phi \) has complexity \( O(M(|Y| (\frac{1}{\delta})^{|Y|}) \), where \( \delta \) is the grid width desired for search over the space of \( \phi \). However, we believe in practice this is not a significant issue, largely because the system design is computed on a much slower timescale: we hardly expect platforms to routinely update the design of adjectives in their rating scales.

For example, a platform with a small number of adjectives being considered may be able to afford the brute force solution to this problem; for a platform designing a rating system, this computational cost is potentially small compared to the time and resources needed for design and experimentation. Furthermore, as we elaborate further in the next section, choosing the right subset is in practice much more important than optimizing the score function; e.g., a naive score function may be sufficient, such as an equispaced \( \phi(y_i) = \frac{i}{|Y|} \), where \( y_i \) is the \( i \)th level in the totally ordered set \( Y \). An optimal subset with naive scores can be found with \( M(|Y|) \) calculations of the convex minimization inside (5).

We conclude with some comments on operationalizing our insights. The platform first requires knowledge of two primitives: (1) \( g(\theta) \), i.e., how the number of matches a seller has varies with quality; and (2) \( R(\theta, y), \forall y \in Y \), i.e., how rater behavior changes with the words chosen in the scale. It can learn \( R \) by running an experiment with different rating scales, and estimate \( g \) from existing data. Second, for a given \( g, R \), the platform must calculate \( Y, \phi \) to maximize Equation (5).

We carry out the above procedure on Amazon Mechanical Turk as described in Section 5. For the setting described in Section 5, it takes several days using 50 cores on a modern computing cluster to perform the full optimization including with scores, but an optimal subset with equispaced scores can be found instantaneously. We use these approaches to compute adjectives scale and then measure their performance in a simulated market.

Finally, we note that the approach can be used more generally to compare different designs, such as the different treatment conditions in Section 3. Each treatment condition is a different design—i.e., a different set of answer choices \( Y \), drawn from different totally ordered sets of available labels (e.g., it’s not clear how Phenomenal and Well Above Average relate to each other). To compare these designs, we can estimate \( R(\theta, y) \) separately for each design, and then compare the resulting large deviations rates (using either a naive score function \( \phi \) or an optimal function for each).

5 Amazon Mechanical Turk experiment and calibrated simulations

In this section, we deploy an experiment on Amazon Mechanical Turk ("MTurk") to apply the design insights of the preceding section in practice. Our analysis has three parts. First, we confirm the findings of the study presented in Section 3, that changing the rating scale indeed alters
the information content of feedback. Second, we use the data collected to calibrate a model of $R(\theta, y)$, as a proof-of-concept with which we can apply our optimization approach. Finally, we use this calibrated model to demonstrate some key features of optimal designs as computed via our methodology, and show through simulation that they perform well relative to natural benchmarks.

5.1 Experimental method

Subjects in our experiment were asked to complete a task consisting of two parts. We ran two smaller pilots (with a total of 60 workers), followed by an experiment that had 200 respondents. We did not perform any response quality control or exclude data. All workers were paid $1.00, with seven workers in the pilots receiving a bonus of $0.20 for providing constructive comments on the interface we created. In the experiment, about 80% of workers spent 8 minutes or less on our site.

In the first part of the task, we asked subjects to rate the English proficiency of ten paragraphs which are modified TOEFL (Test of English as a Foreign Language) essays with known scores as determined by experts and reported in a TOEFL study guide [Educational Testing Service, 2005]; these are our true quality types for each essay. Expert scores range from 1 through 5, with two paragraphs with each score. Subjects were given a six point scale, with words drawn from the following list: $Y = \{\text{Abysmal, Awful, Bad, Poor, Mediocre, Fair, Good, Great, Excellent, Phenomenal}\}$, following the recommendation of Hicks et al. [2000]. This is our universe of available
adjectives. Poor and Good were always chosen, and the other four were sampled independently and uniformly at random for each worker. They were asked, “How does the following rate on English proficiency and argument coherence?” One paragraph was shown per page; returning to modify a previous answer was not allowed; and paragraphs were presented in a random order.

In the second part of the task, subjects were asked to evaluate the quality of their experience on the first task; in particular, we asked, “How does this MTurk experience rate?” One of three rating scales was shown to subjects, chosen at random: Star ratings (1 to 5 stars), Normal adjectives, or Inflated adjectives. The Normal adjectives scale is: Worst MTurk Experience, Far Below Average, Below Average, Average, Above Average. The Inflated adjectives scale is: Below Average, Average, Above Average, Far Above Average, Best MTurk Experience. These scales partially mimic the feedback scales from the labor market test, but note that the Inflated adjectives are substantially more positive-skewed than the Normal adjectives.

5.2 Results and analysis

We now go through the results of the experiment, with supporting plots in Figure 2. In addition we use the experimental results to build a calibrated simulation of different rating system designs. Appendix Section A has further detail of the experiment and simulations.

5.2.1 Confirmation of results from labor market experiment

We compare our results from the second part of the task with our test results in Section 3. In particular, we compare the rating distributions obtained via each of the three scales shown to the subjects. Figure 2a shows the feedback distributions for the three different scales respectively. The results largely replicate those in Section 3. Both the Star rating and Normal adjectives scales exhibit nearly identical behavior (and the null hypothesis that they are the same is not rejected with $p > .9$). On the other hand, the Inflated adjectives scale yields a distribution that is significantly different ($p < 10^{-8}$ using the Kolmogorov-Smirnov two-sample test) and notably more dispersed than either of the other two scales. Our results suggest that: (1) current norms are to interpret star ratings as in our Normal adjectives scale, absent further guidance; and (2) it is possible to substantially increase the dispersion of the rating distribution by positive-skewing the adjective scale.

Furthermore, although we simplified our technical analysis by assuming there were no order effects, this experiment also suggests that there is some consequence to the position of a label. For example, although over 85% of workers rated us as Above Average or better when it was the third of 5 options, only about 70% did so when it was the top option. However, this effect remains second-order relative to the effect of the choice of adjectives used to describe the scale.

We note that the comparison to Section 3 is inexact. First, in this experiment, all workers rate a single platform participant (us, the people running the experiment), whereas on the labor market there are many freelancers being rated. Second, the ratings in this experiment can be interpreted as equivalent to public feedback; workers expect that we – the people they are rating – may see their answers. Third, of course, Mechanical Turk is a vastly different setting than the online labor market, both in terms of the social dynamics of posting and completing tasks, and in their respective user populations. Nevertheless, in both settings we find that meaningful phrases attached to rating levels can substantially shift the distribution of ratings.
5.2.2 Calibration: Learning $R(\theta, y)$

We now turn to analyzing the results of the first task of the experiment. We describe how we can use this task to estimate $\hat{R}(\theta, y)$. Let $\hat{r}(i, y)$ denote the fraction of observations in which paragraph $i$ received a rating of $y$ or higher, when $y$ is presented as an option and $y$ is not the worst option in the scale. Let $\theta_i$ be the true quality (expert score) of paragraph $i$, and let $n(\theta)$ be the number of paragraphs of true quality (expert score) $\theta$; in our experiment, $n(\theta) = 2$ for each $\theta$. We then define:

$$\hat{R}(\theta, y) = \frac{1}{n(\theta)} \sum_{i: \theta_i = \theta} \hat{r}(i, y).$$

The preceding expression shows more generally how a platform can estimate $\hat{R}(\theta, y)$, given data of similar structure to the our experimental data.

Figure 2b shows $\hat{r}(i, y)$ over paragraphs. The colors code the external, expert rating of the TOEFL essays, with dark blue the highest rated essays and light blue the lowest rated. Note that only 9 words appear on the plot because the probability of picking the worst adjective or higher is 1 by definition. Observe that, in the absence of these external expert ratings, it would not be difficult to first estimate $\theta$ using the experiment ratings themselves and then generate $\hat{R}(\theta, y)$.

5.2.3 Simulations

Using $\hat{R}(\theta, y)$, we carry out the optimization described in Section 4 and simulate markets using the resulting designs. In this section we describe this process in detail. We simulate markets with a rating scale system, with the simulation setup differing slightly from the model described in Section 4.1; in particular, in our simulations we actually match market participants.

Simulation description Sellers and buyers. There are 5000 “sellers” with i.i.d. quality in $\{1 \ldots 5\}$, where the number corresponds to the expert rating of a paragraph. We treat $\hat{R}(\theta, y)$ as the true value of $R(\theta, y)$. There are 1000 “buyers”, each of which matches to a unique seller per time period. In other words, matching is not independent across sellers, and each seller can only match once per time period. Each seller is equally likely to match with each buyer.

Entry and exit. In some simulations, all sellers enter the market at time $k = 0$ and do not leave. In the others, with entry and exit, each seller independently leaves the market with probability .02 at the end of each time period, and a new seller with quality drawn i.i.d. from $\{1 \ldots 5\}$ takes her place. This new seller starts with no reputation score.

Various system designs. We design different subsets $Y$ and score functions $\phi$ through various methods. Note that $Y$ is the set of 10 adjectives from our MTurk experiments; we design rating systems where the number of adjectives has size $|Y| = 5$. Since each seller is equally likely to match with each buyer, and there are 5 sellers per buyer, we set $g(\theta) = 0.2$ for all $\theta$ in our optimization approach.

Best Model refers to the best subset and score function found after thousands of compute hours (50 cores over several days) of the brute force optimization discussed in Section 4.2. Best model with naive scores is the approximation achieved (close to instantaneously) by checking the rates of each subset of words $Y$ with naive scores, $\{0.02, 0.25, 0.5, 0.75, 1\}$. Most Deflated and Most Inflated subsets are the bottom 5 and top 5 adjectives, respectively, with optimal scores calculated. Finally, Worst subset is the subset with the worst large deviations rate identified using our brute force approach, but an optimal $\phi$ for that subset.

Simulation results. Figure 2c shows the performance of Best Model compared to the performance of other models when there is no entry or exit. These results show that the optimal design for the multi-level rating scale in Section 4 can be substantially faster at differentiating sellers than
naive designs, given the available label set $\mathcal{Y}$. However, finding the right subset (i.e., which answer choices appear) is more important than optimizing scores; Best model with naive scores performs nearly as well as Best Model.

Figures 3a and 3b in the Appendix further pinpoint the cost of choosing a wrong subset. They include, for each possible subset, the performance of a system with the optimal $\phi$ using that subset, compared to Best Model. In 3a, without entry and exit, Best Model outperforms all other models. In 3b, with 2% chance of entry and exit, Best Model (optimized for a system without entry and exit) performs almost as well as the empirically best model we found.

6 Conclusion and discussion

In this work, we study the the design of informative rating systems. First, we demonstrate through two different field tests that there can be substantial benefit to changing the meaning attached to the levels in a rating scale. In particular, in both tests, we saw that it was possible to choose a design of the adjectives (or other phrases) attached to answer choices present in the rating system that led to a much more informative rating distribution than is seen in standard numeric rating systems. Second, motivated by this finding, we develop a technical framework to optimize the rating distribution. Third, we show that application of this framework can lead to designs that appear to substantially outperform ad hoc choice of the rating scale. We believe this work provides a foundation for a much more systematic approach to the design of rating systems, and that it has direct practical guidance for platforms to build more informative systems.

6.1 Challenges, opportunities, and limitations

International markets One potential difficulty in implementing verbal rating scales is that they must be designed in each language, and people in different cultures may interpret the same scale differently. This difficulty is especially acute as modern online platforms often operate globally. We note that verbal scales provide an opportunity as well as a challenge. There is variation across cultures in numeric rating systems, both for response scales in general and for online platforms in particular [Chen et al., 1995, Hamamura et al., 2008, Koh et al., 2010, Wang et al., 2015]. In the status quo, the platform is left without a mechanism through which it can equalize the rating distributions. On the other hand, with verbal rating scales, if comparable ratings across regions are important, the platform can experiment with various scales and optimize the rating distribution.

Using ratings for search and matching Another potential concern is that at the moment the answers to these questions are not used on the platform for other functions, such as search or matching. As illustrated by Filippas et al. [2017], some inflation for private questions is to be expected once the answers start affecting freelancers – even if freelancers cannot directly identify the client who provided any specific rating. We cannot completely eliminate this concern, and leave the question for future work after a treatment condition is chosen to be implemented permanently on the platform. However, note that the rating distributions for Average, and Average, not affect score are extremely similar; in fact, they are the only two treatments for which we cannot reject the null hypothesis that the rating distributions are the same, with $p > .1$. Either the clients already are aware that the question they are answering is a test question that will not affect freelancers, or this additional information does not substantially influence how clients rate beyond the deflating effects of the answer choices in question.
Switching to a new rating system  One final practical concern with introducing a new rating system with drastically different behavior is that it may be challenging from a data integrity perspective: how can old, inflated ratings be compared to the new ratings, and how can models throughout the platform be adjusted to handle both types of ratings? In some settings, such as our large online labor market, the new system can simply co-exist with the status quo one; multiple questions can be asked in the rating form until enough time has passed with the new system such that older, inflated data is no longer useful. This approach adds friction in the form of additional work for clients, but it may be a price worth temporarily paying for finer resolution information. On other platforms, such as in ride sharing where typically only one question is asked to users, the transition may be more challenging. However, we note that such platforms have begun experimenting with their rating systems, such as by asking follow-up questions, in an effort to overcome the inflation discussed in this work.

6.2 Future work

Ground truth verification  We do not yet have “ground-truth” verification of the ratings: are the resulting ratings freelancers receive more predictive in practice of future performance on the platform? Such an analysis requires a far longer term test of the scales, and remains an important direction for continuation of this work.

Platform goals  Rating systems should reflect the specific goals of a platform. Some platforms may not care about fully recovering the ranking of sellers. For example, platforms – such as ride-sharing or delivery services – that provide a commodified experience may only care about identifying bad actors on the platform. This may mean pushing raters to give good ratings unless something truly bad happened. Platforms in practice already do this; for example, on Lyft, when a passenger rates a driver 4 stars out of 5, the platform describes this as “OK, could have been better.” Future work should closely examine the practical and theoretical relationships between a platform’s informational goals and its rating system design. We take a theoretical step in this direction in our work on designing binary rating systems [Garg and Johari, 2018].

Dynamic design and combating inflation over time  Even with our non-inflated rating scales, it may be possible that over time norms shift so that again ratings become inflated. In this event, optimization of comparison points and rating scales may need to be a dynamic process for a platform. An important direction for future research is to consider a dynamic equilibrium view of rating system design. In particular, designing systems that are naturally robust to inflation yet provide an good user experience will be an important aspect of online marketplaces and platforms. A complete picture should consider how search, buyer rating behavior, and seller behavior may change in response to changes in the rating system. Capturing these short- and long-run equilibrium effects remain important challenges. We believe our work provides an important empirical and theoretical building block in this direction, by suggesting that the meaning raters attach to levels of a scale can substantially influence the quality of information obtained by the platform.

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A Mechanical Turk experiment further description

Figure 3: Simulated performance of all possible \( Y \), with optimal \( \phi \). In light blue is Best Model. The thinner and more transparent the plot line, the worse the predicted performance (worse rate function).

Different experiment trials are described below. Pilots were primarily used to garner feedback regarding the experiment from workers (fair pay, time needed to complete, website comments, etc). All trials yielded qualitatively similar results in terms of both paragraph ratings and feedback rating distributions for various scales.

**Pilot 1** 30 workers. Similar conditions as final experiment (6 words sampled for paragraph ratings, all uniformly at random, 5 point scale feedback rating), with identical question phrasing, “How does the following rate on English proficiency and argument coherence?,” and feedback question phrasing, “How does this MTurk experience rate?”

**Pilot 2** 30 workers. 7 words sampled for paragraph ratings, 6 point scale feedback rating, with the following question phrasing: “How does the following person rate on English proficiency and argument coherence?,” and feedback question phrasing, “How was your experience in this task?”

**Experiment** 200 workers. 6 words sampled for paragraph ratings, with 2 fixed as described above, 5 point scale feedback rating. Question phrasing, “How does the following rate on English proficiency and argument coherence?,” and feedback question phrasing, “How does this MTurk experience rate?”

We use paragraphs modified from a set published by the Educational Testing Service as sample material for the Test of English as a Foreign Language (TOEFL) [Educational Testing Service, 2005]. There are 10 paragraphs, 5 each on 2 different topics. For each topic, the paragraphs have 5 distinct expert scores. Essays are shortened to just a paragraph of just a few sentences, and the top rated paragraphs are improved and the worst ones are made worse; this is largely to ensure the quality could be sufficiently distinguished between paragraphs despite having shortened them. In other words, for each topic, we improved the language of the best rated paragraph and degraded the language of the worst one further. In principle, our editing of these paragraphs may remove the validity of the expert ratings. However, the estimated \( \hat{R}(\theta, y) \) indicates that this does not
Figure 4: Additional information for MTurk experiment

substantially occur, suggesting our editing of the paragraphs preserved the quality ordering of the paragraphs per the expert ratings.

Figure 4a shows time spent on each page of the experiment, Figure 4b shows the time spent per paragraph, and Figure 4c shows the cumulative density function for time spent by workers. The paragraphs are presented to workers in a random order. No workers are excluded in our data and all workers were paid $1.00, including the ones that spent 2-3 seconds per page. 7/60 workers in the pilots received a bonus of $0.20 for providing feedback. The instructions advised workers to spend no more than a minute per question, though this was not enforced. The instructions for the main experiment were as follows: “Please rate on English proficiency (grammar, spelling, sentence structure) and coherence of the argument, but not on whether you agree with the substance of the text.” Then, the question as stated above, “How does the following rate on English proficiency and argument coherence?,” was asked. No additional context was provided.

B Labor market test further description

In this section, we report more detail from the test on the online labor market. We analyze a subset of jobs in this section: some job covariate information is missing in what was given to us by the labor market. We have full covariate data for 100438 jobs (out of 184172).

B.1 Verifying randomization in allocation of clients

As noted in Section 3.3.2 of the main paper, there was a bug in the allocation code such that 1,086 clients were assigned to different treatment cells upon submissions of different jobs. Since this could potentially create contamination between our cells, we disregard these clients in our analysis. Here we make sure that neither this bug nor any other affected experimental validity by checking the distribution of client covariates across the treatment cells. We do so as follows.

We have a set of job level covariates for a subset of the jobs: hourly rate of job (if applicable), total cost of project if not hourly (if applicable), previous number of closed jobs by client at time of job, previous spend by client at time of job, value of the job (4 options), Tier 1 category (12 options), Tier 2 category (88 options), and expertise level (3 options). The first four are continuous, the last 4 are categorical covariates.

For each client, we sample one of that client’s jobs and associate the client with that job’s covariates. Then we run tests of independence for the samples of each covariate across the treatment cells. Across a variety of tests and all covariates, the results are consistent with the randomization being valid.
- For each continuous covariate, using the Kruskal-Wallis H-test for independent samples on all the treatment groups together, the null hypothesis that the population median of all of the groups are equal is not rejected, with \( p > .9 \).

- Similarly, for each continuous covariate, using the one way ANOVA F test, the null hypothesis that all the treatment groups have the same population mean is not rejected, with \( p > .2 \).

- For each categorical covariate, we run the Chi-square test of independence of variables in a contingency table, which tests whether the observed frequencies of values is independent of the treatment group. The null hypothesis is not rejected with \( p > .1 \), for each covariate.

These tests are consistent with fact that the allocation of valid clients we used for analysis across treatment cells was truly random.

Note that these tests do not check whether the invalid clients (which we threw out) are similar to the valid clients. As discussed in the main text: we already know they are not. Invalid clients are more likely to be higher volume clients, as those who submitted many jobs during the test period provided more chances for the bug to manifest.

B.2 Robustness against high volume clients and allocation bug

Recall that in the main text we further threw out the 7 clients who submitted more than 200 jobs during the test period. However, the following may still be the case: idiosyncratic rating behavior of medium-volume clients (over 50 or 100 jobs submitted) may be driving the difference in behavior between treatment cells. Here we show that this is not the case, as well as the fact that throwing out the seven clients was not consequential. We further show that including the clients who were thrown out due to the allocation bug does not materially affect results.

We plot the rating distributions when only sampling 1 job per client, with including the 7 clients excluded for submitting at least 200 jobs during the test period, and using all jobs and clients (even those who submitted in multiple treatment cells), respectively. The mean treatment responses in each case are also included.

![Histograms of responses by treatment](image)

(a) Only sampling 1 job per client  (b) Using all valid clients and jobs  (c) Using all clients and jobs

Figure 5: Rating distributions for different client sampling techniques. As in main text, the confidence intervals are too small to show: the largest across all bars and all ways to sample is .0209.

The sample means with only 1 job per client is lower: clients with more jobs during the treatment period gave higher ratings. However, the results are otherwise qualitatively identical.

B.3 Regressing treatment response with treatment cell and other covariates

We regress the treatment response with treatment cell and all of our job covariates (except tier 2 category, which had 88 unique values and is a more granular version of tier 1 category). (Note:
Data sampling policy:

| Data sampling policy:     | From main text | One job per client | With outlier clients | All clients, even incorrectly allocated |
|--------------------------|----------------|--------------------|----------------------|----------------------------------------|
| Expectations             | 3.339          | 3.244              | 3.354                | 3.359                                   |
| Adjectives               | 3.650          | 3.597              | 3.650                | 3.651                                   |
| Average, not affect score| 3.763          | 3.687              | 3.788                | 3.774                                   |
| Average, Randomized      | 3.777          | 3.693              | 3.777                | 3.771                                   |
| Average                   | 3.465          | 3.438              | 3.463                | 3.458                                   |
| Numeric                   | 3.594          | 4.534              | 4.635                | 4.639                                   |

Table 3: Average treatment responses under different data policies

to maintain full rank, each categorical covariate is encoded such that one of the levels is missing, except for treatment-cell, and there is no intercept. As a result, the treatment cell coefficients cannot be interpreted as treatment means – they are the treatment means conditional on a specific value of each of the categorical covariates and of 0 for the continuous variables). Further note that for simplicity, we only include one set of interaction terms: treatment cell vs the number of previous treatment responses.

We learn several things from this regression:

- There is some heterogeneity in ratings across the job covariates, but on the order of .1 points on the average rating. This heterogeneity is dwarfed by the differences between the treatment cells, especially the numeric vs non-numeric treatments. This relative lack of heterogeneity further supports that the differences between the mean treatment responses are not due to randomness caused by some types of jobs being more present in some treatment groups than others.

- We can directly measure the effect of the number of previous jobs during that testing period a given client has submitted, i.e. estimate the inflation that will result over time as clients submit additional jobs.

From the table below, each additional job a client has submitted raises the treatment response for the Expectations and the Averages treatments, on the order of .008 to .014 points per previous response. At this rate, these coefficients suggest that only after giving 100 ratings would a client inflate ratings by an average of between .8 and 1.4 points. The Numeric treatment cell does not further inflate substantially. Surprisingly, clients in the Adjectives cell give lower ratings the more previous responses they have submitted, on the order of −.004 points per previous response.

Note that the interpretations above are not exact: the set of clients who submit 10 jobs in the test period are a different cohort than those who submit fewer. This effect is partially captured by the term containing the previous number of client assignments. We further analyze this claim in the next section.
B.4 More on Inflation over time

Here we dive deeper into studying whether the ratings in the non-numeric treatment groups inflated over time. In particular, in the regression in the previous section one concern is that the coefficients for the previous number of treatment responses are partially capturing the fact that the population who accumulates more ratings during the test period is different than the population that accumulates fewer ratings. Here, we produce the same regression but limit the analysis to those clients who have more than ten treatment responses during the test period (all of which have all the job covariates). Note that the coefficients for inflation over time are largely the same.
### Model Summary

| Dep. Variable: treatment-response | R-squared: 0.138 |
|----------------------------------|------------------|
| Model: OLS                       | Adj. R-squared: 0.137 |
| Method: Least Squares            | F-statistic: 104.6 |
| Date: Tue, 02 Oct 2018           | Prob (F-statistic): 0.00 |
| Time: 10:12:01                    | Log-Likelihood: -32085.0 |
| No. Observations: 20201          | AIC: 6.423e+04 |
| Df Residuals: 20169              | BIC: 6.449e+04 |
| Df Model: 31                      |                      |

### Coefficients

|                      | coef  | std err | t      | P > | [0.025 | 0.975] |
|----------------------|-------|---------|--------|------|--------|--------|
| treatment_cell[1]    | 3.6170| 0.119   | 30.41  | 0.00 | 3.384  | 3.850  |
| treatment_cell[2]    | 3.8838| 0.120   | 32.41  | 0.00 | 3.649  | 4.119  |
| treatment_cell[3]    | 3.8569| 0.119   | 32.51  | 0.00 | 3.624  | 4.089  |
| treatment_cell[4]    | 3.7780| 0.120   | 31.61  | 0.00 | 3.544  | 4.012  |
| treatment_cell[5]    | 3.5729| 0.119   | 29.98  | 0.00 | 3.339  | 3.807  |
| treatment_cell[6]    | 4.8595| 0.119   | 40.81  | 0.00 | 4.626  | 5.093  |
| value_group[T.lv]    | 0.1761| 0.050   | 3.51   | 0.00 | 0.078  | 0.274  |
| value_group[T.mv]    | -0.0905| 0.051  | -1.77  | 0.08 | -0.191 | 0.010  |
| value_group[T.vlv]   | 0.2719| 0.044   | 6.25   | 0.00 | 0.187  | 0.357  |
| category_group[T.Admin Support] | -0.3269| 0.103  | -3.17  | 0.00 | -0.528 | -0.124 |
| category_group[T.Customer Service] | -0.1992| 0.148  | -1.35  | 0.19 | -0.490 | 0.091  |
| category_group[T.Data Science & Analytics] | -0.0867| 0.116  | -0.74  | 0.45 | -0.314 | 0.141  |
| category_group[T.Design & Creative] | -0.0853| 0.102  | -0.83  | 0.40 | -0.286 | 0.115  |
| category_group[T.Engineering & Architecture] | -0.0197| 0.112  | -0.17  | 0.86 | -0.239 | 0.199  |
| category_group[T.IT & Networking] | -0.4083| 0.134  | -3.05  | 0.00 | -0.670 | -0.147 |
| category_group[T.Legal] | -0.1079| 0.160  | -0.65  | 0.52 | -0.248 | 0.016  |
| category_group[T.Sales & Marketing] | -0.1709| 0.105  | -1.63  | 0.10 | -0.376 | 0.034  |
| category_group[T.Translation] | -0.1612| 0.103  | -1.57  | 0.11 | -0.363 | 0.040  |
| category_group[T.Web, Mobile & Software Dev] | -0.0968| 0.102  | -0.94  | 0.34 | -0.297 | 0.091  |
| category_group[T.Writing] | -0.3528| 0.102  | -3.45  | 0.00 | -0.553 | -0.153 |
| expertise_tier[T.Expert/Expensive] | 0.2207| 0.024  | 9.06   | 0.00 | 0.173  | 0.268  |
| expertise_tier[T.Intermediate] | 0.1134| 0.020  | 5.59   | 0.00 | 0.074  | 0.153  |
| hr_charge            | 1.326e-05| 4.54e-06| 2.92  | 0.003 | 4.37e-06 | 2.22e-05 |
| fp_charge            | -4.42e-05| 3.54e-05| -0.61 | 0.54 | -0.2085 | 0.1565 |
| log(1 +client_prev_spend) | -0.0276| 0.007  | -4.11  | 0.00 | -0.041 | -0.014 |
| log(1 +num_prev_asg) | -0.0306| 0.011  | -2.76  | 0.006 | -0.052 | -0.009 |
| treatment_cell[1]    | 0.0555| 0.001  | 7.14   | 0.00 | 0.004  | 0.007  |
| treatment_cell[2]    | -0.0068| 0.001  | -4.79  | 0.00 | -0.010 | -0.004 |
| treatment_cell[3]    | 0.0086| 0.001  | 9.34   | 0.00 | 0.007  | 0.010  |
| treatment_cell[4]    | 0.0161| 0.001  | 12.47  | 0.00 | 0.014  | 0.019  |
| treatment_cell[5]    | 0.0028| 0.001  | 3.81   | 0.00 | 0.001  | 0.004  |
| treatment_cell[6]    | -0.0004| 0.001  | -0.29  | 0.77 | -0.003 | 0.002  |

Omnibus: 2269.950 Durbin-Watson: 1.690
Probs(translation): 0.000 Jarque-Bera (JB): 3205.739
Skew: 0.878 Prob(JB): 0.00
Kurtosis: 3.852 Cond. No. 9.79e+04

To further help visualize (the relative lack of) inflation over the number of submitted ratings, Figure 6 shows the mean ratings for each treatment cell by the number of previous treatment responses given during the test period. As the plot has no covariate data, we use the first ten responses for all 2145 clients who submitted at least 10 ratings during the test period. The figure indicates that the clients are not substantially more likely to give more positive ratings on their 10th rating during the test than they give on their first rating.
B.5 Results from Pilot

Here, we analyze some results from the short pilot run in January 2018. We were only given 1 unique response per client-freelancer pair. We have 5,893 submitted jobs from 4,523 clients if we include the jobs without full covariate data. For the regression with covariate data, we have 4,051 such submitted jobs with full covariate data over the test period, with 3,179 unique clients.

The Snapshot results are nearly identical to those of the longer test, and of course the pilot was too short to analyze inflation over time.

• Figure 7 shows the rating distributions per treatment cell. The treatment response means, in order, are: 3.414, 3.515, 3.601, 3.653, 3.397, and 4.609.

• As before, the distributional differences can be quantified with a 2 sample Kolmogorov-Smirnov test. As before, the Numeric is different than each of the others at $p < 10^{-80}$, and the null hypothesis that Average and Average, not affect score are the same cannot be rejected, with $p > .9$. In contrast to the long test, the null hypothesis that Expectations and Adjectives are the same cannot be rejected with $p > .1$. All other distributions are different with $p < .001$. 

Figure 7: Rating distributions during pilot period. Error bars are 95% boot-strapped confidence intervals.
Table 4 shows the regression table for the pilot. Heterogeneity across observed covariates is comparatively small as before or statistically insignificant.

In the Average, Randomized treatment, the locations were chosen 27.36%, 16.18%, 14.93%, 14.07%, 13.97%, and 13.49% times each, respectively. As in the main test, this is far from uniform as it would be if clients equally considered all answer choices before responding. Note that these percentages are from the 1038 responses in the Average, Randomized treatment (1 per client-freelancer pair; including jobs without full covariate data.)

| Dep. Variable: treatment-response | R-squared: 0.169 |
|----------------------------------|------------------|
| Model: OLS                       | Adj. R-squared: 0.164 |
| Method: Least Squares            | F-statistic: 32.77 |
| Date: Tue, 02 Oct 2018           | Prob (F-statistic): 1.19e-141 |
| Time: 10:22:20                    | Log-Likelihood: -6280.9 |
| No. Observations: 4051           | AIC: 1.261e+04     |
| DF Residuals: 4025                | BIC: 1.278e+04     |
| DF Model: 25                      |                   |

| coef | std err | t | P>|t| | [0.025 0.975] |
|------|---------|---|------|----------------|
| treatment_cell[1] | 2.5608 | 0.219 | 11.704 | 0.000 | [2.132 2.990] |
| treatment_cell[2] | 2.6536 | 0.219 | 12.111 | 0.000 | [2.224 3.083] |
| treatment_cell[3] | 2.7535 | 0.218 | 12.610 | 0.000 | [2.325 3.182] |
| treatment_cell[4] | 2.8184 | 0.220 | 12.795 | 0.000 | [2.387 3.250] |
| treatment_cell[5] | 2.5602 | 0.219 | 11.667 | 0.000 | [2.130 2.990] |
| treatment_cell[6] | 3.8333 | 0.220 | 17.414 | 0.000 | [3.402 4.265] |
| value_group[T.lv] | 0.3781 | 0.101 | 3.754 | 0.000 | [0.181 0.576] |
| value_group[T.mv] | 0.3220 | 0.098 | 3.272 | 0.001 | [0.129 0.515] |
| value_group[T.vlv] | 0.6745 | 0.087 | 7.709 | 0.000 | [0.503 0.846] |
| category_group[T.Admin Support] | 0.0870 | 0.192 | 0.452 | 0.651 | [-0.290 0.464] |
| category_group[T.Customer Service] | -0.3351 | 0.287 | -1.168 | 0.243 | [-0.897 0.227] |
| category_group[T.Data Science & Analytics] | 0.1408 | 0.210 | 0.672 | 0.502 | [-0.270 0.552] |
| category_group[T.Design & Creative] | 0.2513 | 0.188 | 1.346 | 0.182 | [0.115 0.622] |
| category_group[T.Engineering & Architecture] | 0.2909 | 0.217 | 1.338 | 0.181 | [0.115 0.571] |
| category_group[T.TT & Networking] | 0.1596 | 0.223 | 2.061 | 0.039 | [0.022 0.897] |
| category_group[T.Legal] | -0.0969 | 0.272 | -0.357 | 0.721 | [-0.629 0.435] |
| category_group[T.Sales & Marketing] | 0.0029 | 0.193 | 0.015 | 0.988 | [-0.376 0.382] |
| category_group[T.Translation] | 0.1523 | 0.217 | 0.772 | 0.440 | [-0.235 0.539] |
| category_group[T.Web, Mobile & Software Dev] | 0.2664 | 0.187 | 1.421 | 0.155 | [-0.101 0.634] |
| category_group[T.Writing] | 0.0346 | 0.189 | 0.184 | 0.854 | [-0.335 0.405] |
| expertise_tier[T.Expert/Expensive] | 0.1769 | 0.054 | 3.251 | 0.001 | [0.070 0.284] |
| expertise_tier[T.Intermediate] | 0.0755 | 0.045 | 1.690 | 0.091 | [-0.012 0.163] |
| hr_charge | 4.939e-05 | 1.45e-05 | 3.401 | 0.001 | [2.09e-05 7.79e-05] |
| fp_charge | 1.037e-05 | 5.81e-06 | 1.784 | 0.074 | [-1.02e-06 2.18e-05] |
| log(1+client_prev_spend) | 0.0143 | 0.014 | 1.003 | 0.316 | [-0.014 0.042] |
| log(1+num_prev_asg) | -0.0299 | 0.021 | -1.414 | 0.157 | [-0.071 0.012] |

Table 4: OLS Regression Results for 5 day pilot

C Proofs

Lemma 1.

$$\lim_{k \to \infty} -\frac{1}{k} \log \left[ \mu\left( (x_k(\theta_1) - x_k(\theta_2)) \leq 0 | \theta_1, \theta_2 \right) \right] = \inf_{\alpha \in \mathbb{R}} \left\{ g(\theta_1)I(\alpha | \theta_1) + g(\theta_2)I(\alpha | \theta_2) \right\}$$

where \( I(\alpha | \ell) = \sup_z \{ za - \Lambda(z | \theta) \} \), \( \Lambda(z | \theta) \) is the log moment generating function of a single sample from \( x(\theta_1) \), and \( g(\theta) \) is the sampling rate.
\[ \text{Proof. } \lim_{k \to \infty} -\frac{1}{k} \log \left[ \mu((x_k(\theta_1) - x_k(\theta_2)) \leq 0|\theta_1, \theta_2) \right] \]

\[ = \lim_{k \to \infty} -\frac{1}{k} \log \left[ \int_{a \in \mathbb{R}} \mu((x_k(\theta_1) = a|\theta_1) \mu(x_k(\theta_2) \geq a|\theta_2) da \right] \quad (6) \]

\[ = \lim_{k \to \infty} -\frac{1}{k} \log \left[ \int_{a \in \mathbb{R}} e^{-k g(\theta_1)I(a|\theta_1)} e^{-k g(\theta_2)I(a|\theta_2)} da \right] \quad (7) \]

\[ = \inf_{a \in \mathbb{R}} \{ g(\theta_1)I(a|\theta_1) + g(\theta_2)I(a|\theta_2) \} \text{ Laplace principle} \quad (8) \]

Where (7) is a basic result from large deviations, and \( k g(\theta_i) \) is the number of samples item of quality \( \theta_i \) has received.

Note that this lemma also appears in Glynn and Juneja [2004], which uses the Gartner-Ellis Theorem in the proof. Our proof is conceptually similar but instead uses Laplace’s principle.

We can now establish the rate function for \( P_k(\theta_1, \theta_2) \).

Recall \( P_k(\theta_1, \theta_2) = \mu_k(x_k(\theta_1) > x_k(\theta_2)|\theta_1, \theta_2) - \mu_k(x_k(\theta_1) < x_k(\theta_2)|\theta_1, \theta_2) \). Then, we have

**Lemma 2.** Given \( \theta_1, \theta_2 \), let \( P_k(\theta_1, \theta_2) = 1 - P_k(\theta_1, \theta_2) \). Then:

\[ -\lim_{k \to \infty} \frac{1}{k} \log P_k(\theta_1, \theta_2) = \inf_{a \in \mathbb{R}} \{ g(\theta_1)I(a|\theta_1) + g(\theta_2)I(a|\theta_2) \}, \quad (9) \]

where \( I(a|\theta) = \sup_z \{ za - \Lambda(z|\theta) \} \), and \( \Lambda(z|\theta) \) is the log moment generating function of a single rating given to seller of type \( \theta \):

\[ \Lambda(z|\theta) = \log \sum_{y \in \mathcal{Y}} \rho(\theta, y|Y) \exp(z \phi(y)). \]

**Proof.** Follows directly from Lemma 1.

\[ -\lim_{k \to \infty} \frac{1}{k} \log P_k(\theta_1, \theta_2|\beta) \]

\[ = \lim_{k \to \infty} -\frac{1}{k} \log \left[ \mu_k(x_k(\theta_1) - x_k(\theta_2) < 0|\theta_1, \theta_2) - \mu_k(x_k(\theta_1) - x_k(\theta_2) > 0|\theta_1, \theta_2) \right] \]

\[ = \lim_{k \to \infty} -\frac{1}{k} \log \left[ 2\mu_k(x_k(\theta_1) - x_k(\theta_2) < 0|\theta_1, \theta_2) + \mu_k(x_k(\theta_1) - x_k(\theta_2) = 0|\theta_1, \theta_2) \right] \]

\[ = \lim_{k \to \infty} -\frac{1}{k} \log \left[ \mu_k(x_k(\theta_1) - x_k(\theta_2) \leq 0|\theta_1, \theta_2) \right] \]

\[ = \inf_{a \in \mathbb{R}} \{ g(\theta_1)I(a|\theta_1) + g(\theta_2)I(a|\theta_2) \} \quad \text{Lemma 1} \]

Now we show that this rate function transfers to a rate function for \( W_k \).

**Proof of Theorem 1**

\[ r \triangleq -\lim_{k \to \infty} \frac{1}{k} \log(1 - W_k) = \min_{0 \leq i < M} \inf_{a \in \mathbb{R}} \{ g(\theta_{i+1})I(a|\theta_{i+1}) + g(\theta_i)I(a|\theta_i) \} \quad (10) \]

where \( I(a|\theta) = \sup_z \{ za - \Lambda(z|\theta) \} \), and \( \Lambda(z|\theta) \) is the log moment generating function of a single rating given to seller of type \( \theta \).
Proof.

\[- \lim_{k \to \infty} \frac{1}{k} \log(1 - W_k) = - \lim_{k \to \infty} \frac{1}{k} \log \left(1 - \frac{2}{M(M - 1)} \sum_{\theta_1 > \theta_2 \in \Theta} P_k(\theta_1, \theta_2) \right) \tag{11}\]

\[= - \lim_{k \to \infty} \frac{1}{k} \log \frac{2}{M(M - 1)} \sum_{0 \leq i < j \leq M} \bar{P}_k(\theta_j, \theta_i) \tag{12}\]

\[= - \max_{0 \leq i < j \leq M} \left( \lim_{k \to \infty} \frac{1}{k} \log \left( \bar{P}_k(\theta_j, \theta_i) \right) \right) \tag{13}\]

\[= \min_{0 \leq i < j \leq M} \left( - \lim_{k \to \infty} \frac{1}{k} \left[ \log \left( \bar{P}_k(\theta_j, \theta_i) \right) \right] \right) \tag{14}\]

\[= \min_{0 \leq i < j \leq M} \inf_{a \in \mathbb{R}} \{ g(\theta_j) I(a|\theta_j) + g(\theta_i) I(a|\theta_i) \} \tag{15}\]

\[= \min_{0 \leq i < M} \inf_{a \in \mathbb{R}} \{ g(\theta_{i+1}) I(a|\theta_{i+1}) + g(\theta_i) I(a|\theta_i) \} \tag{16}\]

Where the last line follows from adjacent $\theta_i, \theta_{i+1}$ dominating the rate due to properties of $R$. Line (13) follows from: $\forall a_i^\epsilon \geq 0, \limsup_{\epsilon \to 0} \epsilon \log \left( \sum_{i} N a_i^\epsilon \right) = \max_i N \limsup_{\epsilon \to 0} \epsilon \log (a_i^\epsilon)$. See, e.g., Lemma 1.2.15 in Dembo and Zeitouni [2010] for a proof of this property.

\[\square\]