# Improving Human Integration across the Machine Learning Pipeline

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# Abstract

People play a critical role across the machine learning (ML) pipeline. People contribute to the development of ML algorithms by annotating unparalleled swathes of data through a complex web of distributed evaluations. On the machine learning deployment end, expert practitioners collaborate with ML model outcomes in a variety of real-world domains such as healthcare, lending, education, social services, and disaster relief. This thesis focuses on examining and supporting human judgment in complex decision-making settings, with a view towards improving their integration with machine learning algorithms. Building upon the rich and fertile ground from disciplines studying human behaviour, notably, psychology, cognitive science, and human-computer interaction, this work takes both quantitative and qualitative perspectives to study the situated human factors in different socio-technical systems, such as crowdsourcing, peer review, ML-assisted decision-making. Specifically, we design statistical tools for understanding human behavior under different data elicitation paradigms. Next, we design experiments to draw statistically rigorous insights regarding biases in human decision-making in complex settings, to support evidence-based policy reform towards better quality decisions. Towards improving ML deployment in real-world settings, we propose domain-specific and domain-general frameworks to support effective human-ML collaboration. Herein, the focus is on understanding and leveraging the relative strengths of humans and ML tools. This thesis showcases the importance of emphasizing the role of humans in the broader goal of improving the impact of machine learning algorithms.

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# Contents

# Chapter 1

# Introduction

The rapid advances in Machine Learning (ML) are largely driven by its envisaged promise to automate an unending variety of tasks traditionally perceived to be only *humanly* possible. From the simple act of recognizing a digit based on its visual representation to the complex task of diagnosing abnormalities from chest radiographs. Understanding of visual representations presents one set of achievements of modern Machine Learning methods. Other advancements include but are not limited to areas such as natural language processing, robotics, etc. A central tenet of these technological advancements is the desire to replicate and reproduce human abilities. In other words, a complete understanding of ML tools must involve studying the humans it is designed to be an imprint of.

Human capabilities and intelligence are interwoven into the fabric of ML tools through several complex interactions. This becomes apparent as we look closely at the different processes involved in the design and execution of any machine learning algorithm. In practice, deploying an ML model in the real world comprises of three main stages: (1) Collection of data that captures the desired objective of the model, for example, for an image recognition model one might collect images with annotations describing the feature of interest in the image; (2) Model development, where model with a carefully chosen architecture is trained to learn the desired objective; (3) Model deployment where the developed ML tool is deployed in a real-world setting, such as an image recognition model in a self-driving car, where it assists the driver in having a low-effort and safe driving experience.

In this thesis, we focus on the integral role people play in the first and the third stage in the ML design and execution pipeline. People's behavior in these stages shapes ML tools in crucial ways, and directly impacts the ML outcomes in practice. In the data collection stage, the data is almost entirely generated by people, barring the very recent trend in using synthetic data for training ML models. The collected data reflects the people involved in generating the data, through their knowledge, experiences, etc. Having been trained to learn from the patterns in the data, ML outcomes also reflect the same knowledge and experiences. This phenomena has been studied in detail in prior machine learning literature. A common way of collecting data from people is crowdsourcing, where a specific task, such as image tagging, is uploaded on crowdsourcing platforms and the participating workers provide annotations for the uploaded images. It is easy to see how particulars of crowdsourcing workers' behavior can impact what the final model trained on this data produces.

Next, the specifics of the model deployment stage influence several aspects of the model in important ways. For instance, consider the previously mentioned example of vision models providing assistance in driving. An ideal driving assistant should be able to raise flags when a driver is about to err and hence the appropriate assistive model in this setting relies significantly on the capabilities and needs of the human driver. This idea generalizes to all real-world settings where ML tools are introduced to augment human capabilities. Understanding the current capabilities and failings of the human expert, whom the ML model is being designed to assist, is crucial to designing a model that achieves the goal of improving overall outcome in practice.

Given the important role of human integration in shaping the behaviour of ML models and their usefulness in real-world applications, my thesis contributes to designing tools and experiments to support better understanding and integration of people in complex settings, with a view towards improving ML tools. Specifically, this thesis looks at the domain of crowdsourcing and conference peer review to study different aspects of human behavior in complex data elicitation settings. Our findings reveal previously untested nuances and biases in people's behaviour in both settings, advocating for human-centered design in data elicitation. Next, on the role of people in the model deployment stage, this thesis studies human collaboration with ML outcomes in classification and generation settings. We shed light on the importance of understanding relative strengths of human experts and ML models in any task to support effective human-ML collaboration.

Part I focuses on understanding human decision-making behavior in crowdsourcing. As already mentioned briefly, crowdsourcing is a principal source of data for training machine learning models and the quality of data generated therein impacts the behavior of models trained on it. In this thesis, we design statistical tools to examine properties of crowd-sourced data. Large swathes of data are needed to train ML models, therefore we build upon techniques in high-dimensional statistical learning to provide theoretical guarantees for the algorithms presented in this chapter. Specifically, Chapter 2 provides a two-sample testing algorithm for detecting statistically significant difference in two crowds' preferences over a set of items, expressed as rankings. Chapter 3 examines data aggregation methods in crowdsourcing when crowdworkers' level of expertise information is available. Specifically, we show that a popular aggregation method, maximum likelihood estimation, is statistically inadmissible.

Part II focuses on understanding human decision-making behavior in peer review. Scientific peer review is a complex data elicitation setting with a web of reviewers and submissions, designed to find the top submissions. This setup is based on distributed human evaluations, wherein each reviewer evaluates only a subset of submissions, and each submission is evaluated by only a handful of individuals. The setting presents a rich ground for studying human behavior, with a host of challenges such as subjectivity, biases, misaligned incentives, etc. In this chapter, the work focuses on examining peer review data to test for biases in participants' behaviour, and accordingly recommending evidence-based policy reform in peer review. The bulk of the technical work in Part II covers (1) design of experiments to carefully collect human evaluation data in conference peer review, and (2) application of statistical techniques to identify significant patterns in people's evaluations.

Next, in Part III we turn to the second major focus of this thesis on understanding and supporting human integration with machine learning model outcomes. ML models are being used to support decision-making across a wide range of domains, including healthcare, credit lending,

criminal justice. For example, in the criminal justice system, algorithmic recidivism risk scores inform pre-trial bail decisions for defendants. The introduction of ML assistance in high-stakes decision-making systems is to combine and amplify the respective strengths of human cognition and ML models through carefully designed hybrid decision-making systems. Thus, Part III of the thesis is aimed at generating actionable insights for improving the effectiveness of human-ML partnerships, and thereby the quality of their outcomes.

Carrying forward our previous research on biases in human decision-making, in Chapter 8, we study the role of human cognitive biases in ML-assisted decision-making. This study is a continuation of prior research on supporting appropriate reliance of human decision-makers on ML model outputs.

As discussed previously, a crucial component in effective human-ML partnerships is an understanding of the strengths and limitations of humans versus ML-based decision-making on particular tasks. While research in the behavioral sciences provides insights into potential opportunities for ML models to complement human cognitive abilities and vice versa, further research is needed to (1) understand the implications of these findings in specific real-world human decision-making tasks, and to then (2) operationalize such insights to foster effective human-ML partnerships. Therefore, the remaining part of this thesis generates insights for achieving human-ML complementarity in two classes of tasks: predictive decision-making tasks and generative, co-creative tasks. Correspondingly, Chapter 9 proposes domain-general and domain-specific frameworks for human-ML complementarity in predictive decision-making, and Chapter 10 describes our work on the domain-specific combination of humans and ML in auditing ML models.

This work is aimed at generating actionable insights for improving the quality of decision-making in socio-technical systems at scale, with human decision-makers and their combination with machine learning algorithms.

# Part I

# Theoretical Approaches to Studying Human Judgment in Crowdsourcing

# Chapter 2

# Two-Sample Testing on Ranked Preference Data and the Role of Modeling Assumptions

Based on (Rastogi et al., 2022a):
Charvi Rastogi, Sivaraman Balakrishnan, Nihar B. Shah, and Aarti Singh. Two-sample Testing on Ranked Preference Data and the Role of Modeling Assumptions. *Journal of Machine Learning Research*, 23(225): 1–48, 2022.

## 2.1 Introduction

Data in the form of pairwise-comparisons, or more generally partial or total rankings, arises in a wide variety of settings. For instance, when eliciting data from people (say, in crowdsourcing), there is a long-standing debate over the difference between two methods of data collection: asking people to compare pairs of items or asking people to provide numeric scores to the items. A natural question here is whether people implicitly generate pairwise-comparisons using a fundamentally different mechanism than first forming numeric scores and then converting them to a comparison. Thus, we are interested in testing if the data obtained from pairwise-comparisons is distributed identically to if the numeric scores were converted to pairwise-comparisons (Raman and Joachims, 2014; Shah et al., 2016). As another example consider sports and online games, where a match between two players or two teams is a pairwise-comparison between them (Herbrich et al., 2007; Hvattum and Arntzen, 2010; Van Der Maas and Wagenmakers, 2005). Again, a natural question that arises here is whether the relative performance of the teams has changed significantly across a certain period of time (e.g., to design an appropriate rating system (Cattelan et al., 2013)). A third example is peer grading where students are asked to compare pairs of homeworks (Shah et al., 2013) or rank a batch of homeworks (Lamon et al., 2016; Raman and Joachims, 2014). A question of interest here is whether a certain group of students (female/senior/...) grade very differently as compared to another group (male/junior/...) (Shah et al., 2018b). Additionally, consumer preferences as pairwise-comparisons or partial (or total) rankings can be

used to investigate whether a certain group (married/old/...) make significantly different choices about purchasing products as opposed to another group (single/young/...) (Cavagnaro and Davis-Stober, 2014; Regenwetter et al., 2011).

Each of the aforementioned problems involves two-sample testing, that is, testing whether the distribution of the data from two populations is identical or not. With this motivation, in this paper we consider the problem of two-sample testing on preference data in the form of pairwise-comparisons and, more generally, partial and total rankings. First, we focus our efforts on preference data in the form of pairwise-comparisons. Specifically, consider a collection of items (e.g., teams in a sports league). The data we consider comprises comparisons between pairs of these items, where the outcome of a comparison involves one of the items beating the other. In the two-sample testing problem, we have access to two sets of such pairwise-comparisons, obtained from two different sources (e.g., the current season in a sports league forming one set of pairwise-comparisons and the previous season forming a second set). The goal is to test whether the underlying distributions (winning probabilities) in the two sets of data are identical or different. Similarly, when the data comprises of partial or total rankings over a collection of items from two different sources, our goal is to test whether the distributions over total rankings for the two sources are identical or not. Specifically, we consider the case where a partial ranking is defined as a total ranking over some subset of the collection of items.

**Contributions.** We now outline the contributions of this paper; the theoretical contributions for the pairwise-comparison setting are also summarized in Table 2.1.

- First, we present a test for two-sample testing with pairwise-comparison data and associated upper bounds on its minimax sample complexity. Our test makes essentially no assumptions on the outcome probabilities of the pairwise-comparisons.
- Second, we prove information-theoretic lower bounds on the critical testing radius for this problem. Our bounds show that our test is minimax optimal for this problem.
- As a third contribution, we investigate the role of modeling assumptions: What if one could assume one of the popular models (e.g., BTL, Thurstone, parameter-based, SST, MST, WST) for pairwise-comparison outcomes? We show that our test is minimax optimal under WST and MST models. We also provide an information-theoretic lower bound under the SST and parameter-based models. Conditioned on the planted clique hardness conjecture, we prove a computational lower bound for the SST model with a single observation per pair of items, which matches the sample complexity upper bound attained by our test, up to logarithmic factors.
- Fourth, we conduct experiments on two real-world pairwise-comparison data sets. Our test detects a statistically significant difference between the distributions of directly-elicited pairwise-comparisons and converting numeric scores to comparison data. On the other hand, from the data available for four European football leagues over two seasons, our test does not detect any statistically significant difference between the relative performance of teams across two consecutive seasons.
- Finally, we present algorithms for two-sample testing on partial (or total) ranking data for two partial ranking models—namely, the Plackett-Luce model and a more general marginal

probability based model. We provide upper bounds on sample complexity for the test for the Plackett-Luce model controlling both the Type I and Type II error. Moreover, our test for the marginal probability based model controls the Type I error. We apply our test to a real-world data set on sushi preferences. Our test finds a statistically significant difference in sushi preferences across sections of different demographics based on age, gender and region of residence.

A shorter version of this paper (Rastogi et al., 2020) was presented at the IEEE International Symposium on Information Theory (ISIT) 2020.

**Related literature.** The problem of two-sample testing on ranked preference data is at the intersection of two rich areas of research—two-sample testing and analyzing ranked preference data.

The problem of two-sample testing has a long history in statistics, and classical tests include the t-test and Pearson's $\chi^2$ test (see for instance Lehmann and Romano, 2005 and references therein). More recently, non-parametric tests (Gretton et al., 2012a;b; Rosenbaum, 2005; Kim et al., 2020b; Szekely and Rizzo, 2004) have gained popularity but these can perform poorly in structured, high-dimensional settings. The minimax perspective on hypothesis testing which we adopt in this work originates in the work of (Ingster, 1994) (and was developed further in (Ingster, 1997; Ingster and Suslina, 2003; Ingster, 1994)). Several recent works have studied the minimax rate for two-sample testing for high-dimensional multinomials (Balakrishnan and Wasserman, 2018; 2019; Chan et al., 2014; Valiant and Valiant, 2017; Valiant, 2011), and testing for sparsity in regression (Carpentier et al., 2018; Collier et al., 2017), we build on some of these ideas in our work. We also note the work of (Mania et al., 2018) who propose a kernel-based two-sample test for distributions over total rankings.

The analysis of pairwise-comparison data is a rich field of study, dating back at least 90 years to the seminal work of Louis (Thurstone, 1927) and subsequently (Bradley and Terry, 1952) and (Luce, 1959). Along with this, (Plackett, 1975) and (Luce, 1959) worked on the now-well-known Plackett-Luce model for partial and total rankings. In the past two decades, motivated by crowdsourcing and other applications, there is significant interest in studying such data in a high-dimensional setting, that is, where the number of items $d$ is not a fixed constant. A number of papers (Shah et al., 2016; Chen and Suh, 2015; Negahban et al., 2012; Rajkumar and Agarwal, 2014; Szörényi et al., 2015; Guiver and Snelson, 2009; Maystre and Grossglauser, 2015, and references therein) in this space analyze parameter-based models such as the BTL and the Thurstone models for pairwise-comparison data and the Plackett-Luce model for partial (or total) ranking data. Here the goal is usually to estimate the parameters of the model or the underlying ranking of the items. The papers (Ailon, 2012; Braverman and Mossel, 2008; Chatterjee and Mukherjee, 2019; Chen et al., 2018b; Falahatgar et al., 2017; Rajkumar et al., 2015, and references therein) also study ranking from pairwise-comparisons, under some different assumptions.

Of particular interest is the paper by (Aldous, 2017) which uses the BTL model to make match predictions in sports, and also poses the question of analyzing the average change in the performance of teams over time. While this paper suggests some simple statistics to test for change, designing principled tests is left as an open problem. To this end, we provide a two-sample test without any assumptions and with rigorous guarantees, and also use it subsequently

| Model ($\mathcal{M}$) | Upper Bound | Lower Bound | Computational Lower Bound |
|---|---|---|---|
| Model-free | for $k > 1$, $\epsilon^2_{\mathcal{M}} \leq c\frac{1}{kd}$ (Thm. 1) | $\epsilon^2_{\mathcal{M}} > c\frac{\mathbb{I}(k>1)}{kd} + \frac{\mathbb{I}(k=1)}{4}$ (Prop. 4) | $\epsilon^2_{\mathcal{M}} > c\frac{\mathbb{I}(k>1)}{kd} + \frac{\mathbb{I}(k=1)}{4}$ |
| WST and MST | $\epsilon^2_{\mathcal{M}} \leq c\frac{1}{kd}$ | $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd}$ (Thm. 3) | $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd}$ |
| SST | $\epsilon^2_{\mathcal{M}} \leq c\frac{1}{kd}$ | $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd^{3/2}}$ | for $k = c$, $1, \epsilon^2_{\mathcal{M}} > \frac{1}{kd(\log\log(d))^2}$ (Thm. 6) |
| Parameter-based | $\epsilon^2_{\mathcal{M}} \leq c\frac{1}{kd}$ | $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd^{3/2}}$ (Thm. 5) | $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd^{3/2}}$ |

Table 2.1: This table summarizes our results for two-sample testing of pairwise-comparison data (introduced formally in Equation 2.1), for common pairwise-comparison models. Here, $d$ denotes the number of items, and we obtain $k$ samples (comparisons) per pair of items from each of the two populations. In this work, we provide upper and lower bounds on the critical testing radius $\epsilon_{\mathcal{M}}$, defined in (2.3). The upper bound in Theorem 1 is due to the test in Algorithm 1 which is computationally efficient. We note that the constant $c$ varies from result to result.

to conduct such a test on real-world data.

A recent line of work (Heckel et al., 2019; Shah et al., 2017; Shah and Wainwright, 2018) focuses on the role of the modeling assumptions in estimation and ranking from pairwise-comparisons. We study the role of modeling assumptions from the perspective of two-sample testing and prove performance guarantees for some pairwise-comparison models.

**Organization.** The remainder of this paper is organized as follows. In Section 2.2, we formally describe the problem setup and provide some background on the minimax perspective on hypothesis testing. We also provide a detailed description of the pairwise-comparison models studied in this work. In Section 2.3 we present our minimax optimal test for pairwise-comparison data and present the body of our main technical results for the pairwise-comparison setting with brief proof sketches and defer technical aspects of the proofs to Section A.1. Then, in Section 2.4 we extend our results for the two-sample testing problem on partial (or total) ranking data. We describe two partial ranking models and provide testing algorithms and associated sample complexity bounds. The corresponding proofs are in Section A.1. In Section 2.5, we present our findings from implementing our testing algorithms on three real-world data sets. Furthermore, we present results of simulations on synthetic data which validate our theoretical findings. We conclude with a discussion in Section 2.6.

## 2.2 Background and problem formulation for pairwise-comparison setting

In this section, we provide a more formal statement of the problem of two-sample testing using pairwise-comparison data along with background on hypothesis testing and the associated definition of risk, and various types of ranking models.

### 2.2.1 Problem statement

Our focus in this paper is on the two-sample testing problem where the two sets of samples come from two potentially different populations. Here, we describe the model of the data we consider in our work. Specifically, consider a collection of $d$ items. The two sets of samples comprise outcomes of comparisons between various pairs of these items. In the first set of samples, the outcomes are governed by an unknown matrix $P \in [0,1]^{d \times d}$. The $(i,j)^{\text{th}}$ entry of matrix $P$ is denoted as $p_{ij}$, and any comparison between items $i$ and $j$ results in $i$ beating $j$ with probability $p_{ij}$, independent of other outcomes. We assume there are no ties. Analogously, the second set of samples comprises outcomes of pairwise-comparisons between the $d$ items governed by a (possibly different) unknown matrix $Q \in [0,1]^{d \times d}$, wherein item $i$ beats item $j$ with probability $q_{ij}$, the $(i,j)^{\text{th}}$ entry of matrix $Q$. For any pair $(i,j)$ of items, we let $k^p_{ij}$ and $k^q_{ij}$ denote the number of times a pair of items $(i,j)$ is compared in the first and second set of samples respectively. Let $X_{ij}$ denote the number of times item $i \in [d]$ beats item $j \in [d]$ in the first set of samples, and let $Y_{ij}$ denote the analogous quantity in the second set of samples. It follows that $X_{ij}$ and $Y_{ij}$ are Binomial random variables independently distributed as $X_{ij} \sim \text{Bin}(k^p_{ij}, p_{ij})$ and $Y_{ij} \sim \text{Bin}(k^q_{ij}, q_{ij})$. We adopt the convention of setting $X_{ij} = 0$ when $k^p_{ij} = 0$, and $Y_{ij} = 0$ when $k^q_{ij} = 0$, and $k^p_{ii} = k^q_{ii} = 0$.

Our results apply to both the symmetric and asymmetric settings of pairwise-comparisons:

*Symmetric setting:* The literature on the analysis of pairwise-comparison data frequently considers a symmetric setting where "$i$ vs. $j$" and "$j$ vs. $i$" have an identical meaning. Our results apply to this setting, for which we impose the additional constraints that $p_{ji} = 1 - p_{ij}$ and $q_{ji} = 1 - q_{ij}$ for all $(i,j) \in [d]^2$. In addition, for every $1 \le i \le j \le d$, we set $k^p_{ji} = k^q_{ji} = 0$ (and hence $X_{ji} = Y_{ji} = 0$), and let $k^p_{ij},\ k^q_{ij},\ X_{ij}$ and $Y_{ij}$ represent the comparisons between the pair of items $(i,j)$ .

*Asymmetric setting:* Our results also apply to an asymmetric setting where "$i$ vs. $j$" may have a different meaning as compared to "$j$ vs. $i$". For instance, in a setting of sports where "$i$ vs. $j$" could indicate $i$ as the home team and $j$ as the visiting team. This setting does not impose the restrictions described in the symmetric setting above.

**Hypothesis test.** Consider any class $\mathcal{M}$ of pairwise-comparison probability matrices, and any parameter $\epsilon > 0$. Then,the goal is to test the hypotheses

$$\begin{aligned} H_0 &: P = Q \\ H_1 &: \frac{1}{d}|||P - Q|||_{\mathrm{F}} \geq \epsilon, \end{aligned} \tag{2.1}$$

where $P, Q \in \mathcal{M}$.

### 2.2.2 Hypothesis testing and risk

We now provide a brief background on hypothesis tests and associated terminology. In hypothesis testing, the Type I error is defined as the probability of rejecting the null hypothesis $H_0$ when the null hypothesis $H_0$ is actually true, an upper bound on the Type I error is denoted by $\alpha$; the Type II error is defined as the probability of failing to reject the null when the alternate hypothesis $H_1$ is actually true, an upper bound on Type II error is denoted by $\beta$. The performance of the testing algorithm is evaluated by measuring its Type I error and its power, which is defined as one minus the Type II error.

Consider the hypothesis testing problem defined in (2.1). We define a test $\phi$ as $\phi : \{k^p_{ij}, k^q_{ij}, X_{ij}, Y_{ij}\}_{(i,j)\in[d]^2} \mapsto \{0, 1\}$. Let $\mathbb{P}_0$ and $\mathbb{P}_1$ denote the distribution of the input variables under the null and under the alternate respectively. Here, we assume that the variables $k^p_{ij}$ and $k^q_{ij}$ are fixed for all $(i, j) \in [d^2]$. Let $\mathcal{M}_0$ and $\mathcal{M}_1(\epsilon)$ denote the set of matrix pairs $(P, Q)$ that satisfy the null condition and the alternate condition in (2.1) respectively. Then, we define the minimax risk (Ingster, 1994; 1997; Ingster and Suslina, 2003) as

$$\mathcal{R}_{\mathcal{M}} = \inf_{\phi}\{\sup_{(P,Q)\in\mathcal{M}_0} \mathbb{P}_0(\phi = 1) + \sup_{(P,Q)\in\mathcal{M}_1(\epsilon)} \mathbb{P}_1(\phi = 0)\}, \tag{2.2}$$

where the infimum is over all $\{0, 1\}$-valued tests $\phi$. It is common to study the minimax risk via a coarse lens by studying instead the critical radius or the minimax separation. The critical radius is the smallest value $\epsilon$ for which a hypothesis test has non-trivial power to distinguish the null from the alternate. Formally, we define the critical radius as

$$\epsilon_{\mathcal{M}} = \inf\{\epsilon : \mathcal{R}_{\mathcal{M}} \leq 1/3\}. \tag{2.3}$$

The constant $1/3$ is arbitrary; we could use any specified constant in $(0, 1)$. In this paper, we focus on providing tight bounds on the critical radius.

### 2.2.3 A range of pairwise-comparison models

A model for the pairwise-comparison probabilities is a set of matrices in $[0, 1]^{d\times d}$. In the context of our problem setting, assuming a model means that the matrices $P$ and $Q$ are guaranteed to be drawn from this set. In this paper, the proposed test and the associated guarantees do not make any assumptions on the pairwise-comparison probability matrices $P$ and $Q$. In other words, we allow $P$ and $Q$ to be any arbitrary matrices in $[0, 1]^{d\times d}$. However, there are a number of models which are popular in the literature on pairwise-comparisons, and we provide a brief

overview of them here. We analyze the role of these modeling assumptions in our two-sample testing problem. In what follows, we let $M \in [0,1]^{d \times d}$ denote a generic pairwise-comparison probability matrix, with $M_{ij}$ representing the probability that item $i \in [d]$ beats item $j \in [d]$. The models impose conditions on the matrix $M$.

- *Parameter-based models:* A parameter-based model is associated with some known, non-decreasing function $f : \mathbb{R} \to [0,1]$ such that $f(\theta) = 1 - f(-\theta) \quad \forall\, \theta \in \mathbb{R}$. We refer to any such function $f$ as being "valid". The parameter-based model associated to a given valid function $f$ is given by

$$M_{ij} = f(w_i - w_j) \quad \text{for all pairs } (i,j), \tag{2.4}$$

  for some unknown vector $w \in \mathbb{R}^d$ that represents the notional qualities of the $d$ items. It is typically assumed that the vector $w$ satisfies the conditions $\sum_{i \in [d]} w_i = 0$ and that $\|w\|_\infty$ is bounded above by a known constant.

  - *Bradley-Terry-Luce (BTL) model:* This is a specific parameter-based model with $f(\theta) = \dfrac{1}{1+e^{-\theta}}$.
  - *Thurstone model:* This is a specific parameter-based model with $f(\theta) = \Phi(\theta)$, where $\Phi$ is the standard Gaussian CDF.

- *Strong stochastic transitivity (SST):* The model assumes that the set of items $[d]$ is endowed with an unknown total ordering $\pi$, where $\pi(i) < \pi(j)$ implies that item $i$ is preferred to item $j$. A matrix $M \in [0,1]^{d \times d}$ is said to follow the SST model if it satisfies the shifted-skew-symmetry condition $M_{ij} = 1 - M_{ji}$ for every pair $i, j \in [d]$ and the condition

$$M_{i\ell} \geq M_{j\ell} \text{ for every } i, j \in [d] \text{ such that } \pi(i) < \pi(j) \text{ and for every } \ell \in [d]. \tag{2.5}$$

- *Moderate stochastic transitivity (MST):* The model assumes that the set of items $[d]$ is endowed with an unknown total ordering $\pi$. A matrix $M \in [0,1]^{d \times d}$ is said to follow the MST model if it satisfies $M_{ij} = 1 - M_{ji}$ for every pair $i, j \in [d]$ and the condition

$$M_{i\ell} \geq \min\{M_{ij}, M_{j\ell}\} \text{ for every } i, j, \ell \in [d] \text{ such that } \pi(i) < \pi(j) < \pi(\ell). \tag{2.6}$$

- *Weak stochastic transitivity (WST):* The model assumes that the set of items $[d]$ is endowed with an unknown total ordering $\pi$. A matrix $M \in [0,1]^{d \times d}$ is said to follow the WST model if it satisfies $M_{ij} = 1 - M_{ji}$ for every pair $i, j \in [d]$ and the condition

$$M_{ij} \geq \frac{1}{2} \text{ for every } i, j \in [d] \text{ such that } \pi(i) < \pi(j). \tag{2.7}$$

**Model hierarchy:** There is a hierarchy between these models, that is, $\{$BTL, Thurstone$\} \subset$ parameter-based $\subset$ SST $\subset$ MST $\subset$ WST $\subset$ model-free.

## 2.3 Main results for pairwise-comparison setting

We now present our main theoretical results for pairwise-comparison data.

### 2.3.1 Test and guarantees

Our first result provides an algorithm for two-sample testing in the problem (2.1), and associated upper bounds on its sample complexity. Importantly, we do not make any modeling assumptions on the probability matrices $P$ and $Q$. First we consider a per-pair fixed-design setup in Theorem 1 where for every pair of items $(i, j)$, the sample sizes $k^p_{ij}, k^q_{ij}$ are equal to $k$. Following that, in Corollary 2, we consider a random-design setup wherein for every pair of items $(i, j)$, the sample sizes $k^p_{ij}, k^q_{ij}$ are drawn i.i.d. from some distribution $\mathcal{D}$ supported over non-negative integers.

**Input**: Samples $X_{ij}, Y_{ij}$ denoting the number of times item $i$ beat item $j$ in the observed $k^p_{ij}, k^q_{ij}$ pairwise-comparisons from populations denoted by probability matrices $P, Q$ respectively.
**Test Statistic**:

$$T = \sum_{i=1}^{d}\sum_{j=1}^{d} \mathbb{I}_{ij} \frac{k^q_{ij}(k^q_{ij}-1)(X^2_{ij}-X_{ij}) + k^p_{ij}(k^p_{ij}-1)(Y^2_{ij}-Y_{ij}) - 2(k^p_{ij}-1)(k^q_{ij}-1)X_{ij}Y_{ij}}{(k^p_{ij}-1)(k^q_{ij}-1)(k^p_{ij}+k^q_{ij})} \tag{2.8}$$

where $\mathbb{I}_{ij} = \mathbb{I}(k^p_{ij} > 1) \times \mathbb{I}(k^q_{ij} > 1)$.
**Output**: If $T \geq 11d$, where $11d$ is the threshold, then reject the null.

**Algorithm 1:** Two-sample test with pairwise-comparisons for model-free setting

Our test is presented in Algorithm 1. The test statistic (2.8) is designed such that it has an expected value of zero under the null and a large expected value under the alternate. That is, the test statistic (2.8) is designed symmetrically across $P$ and $Q$ such that it has expected value zero, if $P = Q$. Similarly, to reject the null with high probability, under the alternate $\|P - Q\|_F \geq \epsilon d$, the test statistic (2.8) is designed to increase as $\|P - Q\|_F$ increases. In fact, we will later show in Section A.1.2 that the test statistic increases quadratically with $\|P - Q\|_F$. The following theorem characterizes the performance of this test, thereby establishing an upper bound on the sample complexity of this two-sample testing problem in a random-design setting.

**Theorem 1** *Consider the testing problem in (2.1) with $\mathcal{M}$ as the class of all pairwise probability matrices. Suppose the number of (per pair) comparisons between the two populations is fixed, $k^p_{ij} = k^q_{ij} = k$ (for all $i \neq j$ in the asymmetric setting and all $i < j$ in the symmetric setting). There is a constant $c > 0$ such that for any $\epsilon > 0$, if $k > 1$ and $\epsilon^2 \geq c\frac{1}{kd}$, then the sum of Type I error and Type II error of Algorithm 1 is at most $\frac{1}{3}$.*

The proof is provided in Section A.1.2. Theorem 1 provides a guarantee of correctly distinguishing between the null and the alternate with probability at least $\frac{2}{3}$. The value $\frac{2}{3}$ is closely tied to the specific threshold used in the test above. More generally, for any specified constant $\nu \in (0, 1)$, the test achieves a Type I error at most $\nu$ by setting the threshold as $d\sqrt{\frac{24(1-\nu)}{\nu}}$. Similarly, for any specified constant $\nu \in (0, 1)$, the test achieves a Type II error at most $\nu$, if $\epsilon^2 \geq \frac{\nu_1}{kd}$, wherein

$\nu_1 > 0$ is a constant that depends on $\nu$. The power of our test approaches 1 at the rate $\frac{c}{kd\epsilon^2}$, where $c$ is some constant, that is the power of our test increases as $\epsilon$ increases.

Moreover, if the sample complexity is increased by some factor $R$, then running Algorithm 1 on $R$ independent instances of the data and taking the majority answer results in error probability that decreases exponentially with $R$ as $(\exp(-2R))$, while the sample complexity increases only linearly in $R$. One can thus have a very small probability of error of, for instance, $d^{-50}$ with $k = \widetilde{O}\left(\frac{1}{d\epsilon^2}\right)$. Later, in Proposition 4, we show that under the fixed $k$ condition, $k_{ij}^p = k_{ij}^q = k$, we have that $k > 1$ is necessary for our two-sample testing problem. It is also interesting to note that the estimation rate to test the hypotheses in (2.1) is $k = O\left(\frac{\log(d)}{\epsilon^2}\right)$ while the rate for our testing algorithm is $k = O\left(\frac{1}{d\epsilon^2}\right)$.

Now, we consider the random-design setup wherein for every pair of items $(i, j)$, the sample sizes $k_{ij}^p, k_{ij}^q$ are drawn i.i.d. from some distribution $\mathcal{D}$ supported over non-negative integers. Let $\mu$ and $\sigma$ denote the mean and standard deviation of distribution $\mathcal{D}$ respectively, and let $p_1 := \Pr_{Z\sim\mathcal{D}}(Z = 1)$. We assume that $\mathcal{D}$ has a finite mean and that

$$\mu \geq c_1 p_1; \quad \mu \geq c_2 \sigma, \tag{2.9}$$

for some constants $c_1 > 1$ and $c_2 > 1$. Many commonly occurring distributions obey these properties, for instance, Binomial distribution, Poisson distribution, geometric distribution and discrete uniform distribution, with appropriately chosen parameters.

**Corollary 2** *Consider the testing problem in (2.1) with $\mathcal{M}$ as the class of all pairwise probability matrices. Suppose the number of comparisons in the two populations $k_{ij}^p, k_{ij}^q$ are drawn i.i.d. from some distribution $\mathcal{D}$ that satisfies (2.9) (for all $i \neq j$ in the asymmetric setting and all $i < j$ in the symmetric setting). There is a constant $c > 0$ such that if $\epsilon^2 \geq c \max\{\frac{1}{\mu d}, \frac{1}{d^2}\}$, then the sum of Type I error and Type II error of Algorithm 1 is at most $\frac{1}{3}$.*

The proof of Corollary 2 is in Section A.1.1. In Corollary 2, we see that the even under the random-design setup, our test achieves the same testing rate as in the per-pair fixed-design setup considered in Theorem 1, for $\mu \leq d$.

We now evaluate the performance of Algorithm 1 when $k_{ij}^p, k_{ij}^q$ are drawn i.i.d. from one of the following commonly occurring distributions. Consider any arbitrary matrices $P$ and $Q$. We specialise Corollary 2 to these distributions by stating the sample complexity that guarantees that the probability of error is at most $\frac{1}{3}$ in the two-sample testing problem (2.1), wherein constant $c$ may depend on $c_1, c_2$ for each distribution. Note that, as in Corollary 2, we assume $\epsilon^2 d^2 \geq c'$ where $c'$ is some positive constant

- Binomial distribution $(k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \text{Bin}(n, a))$ : Sufficient condition $n \geq c \max\{\frac{1}{ad\epsilon^2}, \frac{1}{a}\}$.
- Poisson distribution $(k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \text{Poisson}(\lambda))$ : Sufficient condition $\lambda \geq c \max\{\frac{1}{d\epsilon^2}, 1\}$.
- Geometric Distribution $(k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \text{Geometric}(a))$: Sufficient condition $\frac{1}{a} \geq c \max\{\frac{1}{d\epsilon^2}, 1\}$.
- Discrete Uniform Distribution $(k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \text{Unif}(0, n))$: Sufficient condition $n \geq c \max\{\frac{1}{d\epsilon^2}, 1\}$.

Next, we note that a sharper but non-explicit threshold in Algorithm 1 can be obtained using the permutation test method to control the Type I error. We detail this approach in Algorithm 2.

**Input** : Samples $X_{ij}, Y_{ij}$ denoting the number of times item $i$ beat item $j$ in the observed $k^p_{ij}, k^q_{ij}$ pairwise-comparisons from populations denoted by probability matrices $P, Q$ respectively. Significance level $\alpha \in (0, 1)$. Iteration count $\gamma$.

**(1)** Compute the test statistic $T$ defined in (2.8).

**(2)** For $\ell \leftarrow 1$ to $\gamma$ :

**(i)** Repeat this step independently for all $i \neq j$ in the asymmetric setting and for all $i < j$ in the symmetric setting. Collect the $(k^p_{ij} + k^q_{ij})$ samples together and reassign $k^p_{ij}$ of the samples chosen uniformly at random to $P$ and the rest to $Q$. Compute the new values of $X_{ij}$ and $Y_{ij}$ based on this reassignment.

**(ii)** Using the new values of $X_{ij}$ and $Y_{ij}$, recompute the test statistic in (2.8). Denote the computed test statistic as $T_\ell$.

**Output** : Reject the null if $p = \sum_{\ell=1}^{\gamma} \frac{1}{\gamma} \mathbb{1}(T_\ell - T) < \alpha$.

**Algorithm 2:** Permutation test with pairwise-comparisons for model-free setting.

More generally, the results in Theorem 1 and Corollary 2 (and the following converse results in Theorem 3 and Proposition 4) also apply to the two-sample testing problem of comparing two Bernoulli matrices (or vectors) $P$ and $Q$, wherein each entry of the matrices (or vectors) is a Bernoulli parameter. In this problem, we want to test whether two Bernoulli matrices are identical or not, and we have access to some observations of some (or all) of the underlying Bernoulli random variables.

We conclude this section with a proof sketch for Theorem 1; the complete proof is provided in Section A.1.1 and A.1.2.

**Proof Sketch for Theorem 1.** The test statistic $T$ is designed to ensure that $\mathbb{E}_{H_0}[T] = 0$ for any arbitrary pairwise probability matrices $P, Q$ such that $P = Q$, and for any values of $\{k^p_{ij}, k^q_{ij}\}_{1 \leq i,j \leq d}$. We lower bound the expected value of $T$ under the alternate hypothesis as $\mathbb{E}_{H_1}[T] \geq ckd^2\epsilon^2$ (Lemma 15). Next, we show that the variance of $T$ is upper bounded under the null by $24d^2$ and under the alternate by $24d^2 + 4kd^2\epsilon^2$ (Lemma 16). These lemmas allow us to choose a suitable threshold value of $11d$. Finally, using Chebyshev's inequality comparing the square of expectation with the variance, we obtain the desired upper bound on the sample complexity with guarantees on both Type I and Type II errors.

### 2.3.2 Converse results and the role of modeling assumptions

In this section we look at the role of modeling assumptions on the pairwise-comparison probability matrices in the two-sample testing problem in (2.1).

**Lower bound for MST, WST, and model-free classes.** Having established an upper bound on the rate of two-sample testing without modeling assumptions on the pairwise-comparison probability matrices $P, Q$, we show matching lower bounds that hold under the MST class. The

WST and model-free classes are both supersets of MST, and hence the following guarantees automatically apply to them as well.

**Theorem 3** *Consider the testing problem in (2.1) with $\mathcal{M}$ as the class of matrices described by the MST model. Suppose we have $k$ comparisons for each pair $(i, j)$ from each population. There exists a constant $c > 0$, such that the critical radius $\epsilon_{\mathcal{M}}$ is lower bounded as $\epsilon^2_{\mathcal{M}} > \frac{c}{kd}$.*

The lower bound on the rate matches the rate derived for Algorithm 1 in Theorem 1, thereby establishing the minimax optimality of our algorithm (up to constant factors). The MST class is a subset of the WST model class. This proves that Algorithm 1 is simultaneously minimax optimal under the MST and WST modeling assumptions in addition to the model-free setting. We provide a proof sketch for Theorem 3 in Section 2.3.2; the complete proof is in Section A.1.3.

**Necessity of $\mu > p_1$.** Recall that the upper bound derived in Theorem 1 under the model-free setting holds under the assumption that $k > 1$ and, similarly, Corollary 2 holds under the assumption that $\mu \geq c_1 p_1$ with $c_1 > 1$, as stated in (2.9). We now state a negative result for the case $\mu \leq p_1$, which implies that $k^p_{ij}, k^q_{ij} \leq 1 \, \forall \, (i, j)$ under the random-design setup and $k \leq 1$ under the per-pair fixed-design setup.

**Proposition 4** *Consider the testing problem in (2.1) with $\mathcal{M}$ as the class of all pairwise probability matrices. Suppose we have at most one comparison for each pair $(i, j)$ from each population (for all $i \neq j$ in the asymmetric setting and all $i < j$ in the symmetric setting). Then, for any value of $\epsilon \leq \frac{1}{2}$, the minimax risk defined in (2.2) is at least $\frac{1}{2}$, thus, $\epsilon^2_{\mathcal{M}} \geq \frac{1}{4}$.*

We provide some intuition for this result here. If $k^p_{ij} = k^q_{ij} \leq 1 \, \forall \, (i, j)$, then at best one has access to first order information of each entry of $P$ and $Q$, that is, one has access to only $\Pr(X_{ij} = 1), \Pr(Y_{ij} = 1), \Pr(X_{ij} = 1, Y_{ij} = 1)$ for each pair $(i, j)$. This observation allows us to construct a case wherein the null and the alternate cannot be distinguished from each other by any test, due to the inaccessibility of higher order information of the underlying Bernoulli random variables. The complete proof is provided in Section A.1.3.

**Lower bound for parameter-based class.** We now prove an information-theoretic lower bound for our two-sample testing problem wherein the probability matrices follow the parameter-based model.

**Theorem 5** *Consider the testing problem in (2.1). Consider any arbitrary non-decreasing function $f : \mathbb{R} \to [0, 1]$ such that $f(\theta) = 1 - f(-\theta) \;\; \forall \; \theta \in \mathbb{R}$, with $\mathcal{M}$ as the parameter-based class of probability matrices associated to the given function. Suppose we have $k$ comparisons for each pair $(i, j)$ from each population. There exists a constant $c > 0$, such that the critical radius $\epsilon_{\mathcal{M}}$ is lower bounded as $\epsilon^2_{\mathcal{M}} > \frac{c}{kd^{3/2}}$.*

This lower bound also applies to probability matrices in the SST class described in (2.5). We provide a brief proof sketch in Section 2.3.2; the complete proof is in Section A.1.3. Observe that the lower bound on testing rate obtained in Theorem 5 does not match the testing rate obtained in Theorem 1. In the next part of this section, we provide a computational lower bound in place of a statistical lower bound to bridge this gap.

**Computational lower bound for SST class.** Given the polynomial gap between Theorem 1 and Theorem 5, it is natural to wonder whether there is another polynomial-time testing algorithm for testing under the SST and/or parameter-based modeling assumption. We answer this question in the negative, for the SST model and single observation model ($k = 1$), conditionally on the average-case hardness of the planted clique problem (Jerrum, 1992; Kučera, 1995). In informal terms, the planted clique conjecture asserts that there is no polynomial-time algorithm that can detect the presence of a planted clique of size $\kappa = o(\sqrt{d})$ in an Erdős-Rényi random graph with $d$ nodes. We construct SST matrices that are similar to matrices in the planted clique problem and as a direct consequence of the planted clique conjecture, we have the following result.

**Theorem 6** *Consider the testing problem in* (2.1) *with* $\mathcal{M}$ *as the class of matrices described by the SST model. Suppose the planted clique conjecture holds. Suppose we have one comparison for each pair* $(i, j)$ *from each population. Then there exists a constant* $c > 0$ *such that for polynomial-time testing algorithms the critical radius* $\epsilon_{\mathcal{M}}$ *is lower bounded as* $\epsilon^2_{\mathcal{M}} > \frac{c}{d(\log\log(d))^2}$.

Thus, for $k = 1$, the computational lower bound on the testing rate for the SST model matches the rate derived for Algorithm 1 (up to logarithmic factors). The proof of Theorem 6 is provided in Section A.1.3. We devote the rest of this section to a sketch of the proofs of Theorem 3 and Theorem 5.

### Proof sketches for Theorem 3 and Theorem 5

To prove the information-theoretic lower bound under the different modeling assumptions, we construct a null and alternate belonging to the corresponding class of probability matrices. The bulk of our technical effort is devoted to upper bounding the chi-square divergence between the probability measure under the null and the alternate. We then invoke Le Cam's lower bound for testing to obtain a lower bound on the minimax risk which gives us the information-theoretic lower bound. We now look at the constructions for the two modeling assumptions.

*Lower bound construction for MST class* (Section A.1.3). We construct a null and alternate such that under the null $P = Q = [\frac{1}{2}]^{d\times d}$ and under the alternate $P = [\frac{1}{2}]^{d\times d}$ and $Q \in \Theta$ with $\frac{1}{d}|||P - Q|||_{\text{F}} = \epsilon$. For this, we define a parameter $\eta \in [0, \frac{1}{2}]$ and then define $\Theta$ as a set of matrices in which the upper right quadrant has exactly one entry equal to $\frac{1}{2} + \eta$ in each row and each column and the remaining entries above the diagonal are $\frac{1}{2}$. The entries below the diagonal follow from the shifted-skew-symmetry condition. We consider the alternate where $Q$ is chosen uniformly at random from the set $\Theta$ of probability matrices in MST class.

*Lower bound construction for parameter-based class* (Section A.1.3). The construction is same as the construction given above except we define a different set $\Theta$ of probability matrices. According to the parameter-based model, the matrices $P$ and $Q$ depend on the vectors $w_p \in \mathbb{R}^d$ and $w_q \in \mathbb{R}^d$ respectively. Now, for simplicity in this sketch, suppose that $d$ is even. We set $w_p = [0, \cdots, 0]$, which fixes $p_{ij} = \frac{1}{2}\ \forall\ (i, j)$. Consider a collection of vectors each with half the entries as $\delta$ and the other half as $-\delta$, thereby ensuring that $\sum_{i\in[d]} w_i = 0$. We set $\delta$ to ensure that each of the probability matrices induced by this collection of vectors obey $\frac{1}{d}|||P - Q|||_{\text{F}} = \epsilon$. We then consider the setting where $Q$ is chosen uniformly at random from the set of pairwise-comparison probability matrices induced by the collection of values of $w_Q$.

## 2.4 Two-sample testing with partial or total ranking data

In this section, we extend our work from the previous sections to two-sample testing for ranking data. We focus on the two-sample testing problem where the two sets of samples from two potentially different populations comprise of partial or total rankings over various subsets of $d$ items. Specifically, we consider the case where a partial ranking is defined as a total ranking over some subset of $d$ items. Let $\lambda_P$ and $\lambda_Q$ be two unknown probability distributions over the set of all $d$-length rankings. We observe two sets of partial or total rankings, one set from each of two populations. The partial rankings in the first set are assumed to be drawn i.i.d. according to $\lambda_P$, and the partial rankings in second set are drawn i.i.d. according to $\lambda_Q$. Each sample obtained is a ranking over a subset of items of size ranging from 2 to $d$. Henceforth, we use the term total ranking to specify a ranking over all $d$ items. We assume there are no ties.

**Hypothesis test** Our goal is to test the hypothesis,

$$\begin{aligned} H_0 &: \lambda_P = \lambda_Q \\ H_1 &: \lambda_P \neq \lambda_Q. \end{aligned} \tag{2.10}$$

In the sequel, we consider this hypothesis testing problem under certain modeling assumptions on $\lambda_P$ and $\lambda_Q$.

### 2.4.1 Models

We now describe two partial ranking models that we analyse subsequently.

**Marginal probability based model** This is a non-parametric partial ranking model that is entirely specified by the probability distribution over all total rankings, given by $\lambda_P$ in the first population and $\lambda_Q$ in the second population. The distribution $\lambda$ defines the partial ranking model for the corresponding population as follows. Let $S_d$ denote the set of all total rankings over the $d$ items. Consider some subset of items $\Omega \subseteq [d]$ of size $m \in \{2, \cdots, d\}$, and let $\tau_\Omega$ be a ranking of the items in this set. Then, we define a set of all total rankings that obey the partial ranking $\tau_\Omega$ as

$$S(\tau_\Omega) = \{\tau \in S_d \ : \ \tau(\tau_\Omega^{-1}(1)) < \tau(\tau_\Omega^{-1}(2)) < \cdots < \tau(\tau_\Omega^{-1}(m))\}. \tag{2.11}$$

The marginal probability based partial ranking model gives the probability of a partial ranking $\tau_\Omega$ as

$$\mathbb{P}(\tau_\Omega) = \sum_{\tau \in S(\tau_\Omega)} \lambda(\tau), \tag{2.12}$$

where $\lambda$ represents $\lambda_P$ or $\lambda_Q$ for the corresponding population. This model defines the probability of a partial ranking similarly to the non-parametric choice model described in (Farias et al., 2013). In fact, their choice model defined over sets of size 2 is the same as our model over partial rankings of size 2. Our model has the desired property that given a partial ranking over the set $\Omega$ containing item $i$ and item $j$, the marginal probability that item $i$ is ranked higher than item

$j$, denoted by $\mathbb{P}(i \succ j)$, does not depend on other items in set $\Omega$. Subsequently, the marginal probability is expressed as

$$\begin{aligned}\mathbb{P}(i \succ j|\Omega) &= \sum_{\tau \in S(\tau_\Omega), \tau(i)<\tau(j)} \lambda(\tau) \\ &= \sum_{\tau \in S_d, \tau(i)<\tau(j)} \lambda(\tau)\end{aligned}$$

Now, for the two populations we define the marginal probability of pairwise-comparisons over items $(i, j)$ as $p_{ij}$ and $q_{ij}$ for all pairs $(i, j)$ with $i < j$. Note that this model has the property that $p_{ij} = 1 - p_{ji}$ and $q_{ij} = 1 - q_{ji}$ for all $(i, j)$. We also note that the Plackett-Luce model described next is a subset of this model.

**Plackett-Luce model** This model introduced by (Luce, 1959) and (Plackett, 1975) is a commonly used parametric model for ranking data. In this model, each item has a notional quality parameter $w_i \in \mathbb{R}, \ \forall \ i \in [d]$. Under the Plackett-Luce model, the partial rankings in each population are generated according to the corresponding underlying quality parameters, namely $w^P_{i\in[d]}$ and $w^Q_{i\in[d]}$. The weight parameters completely define the probability distribution $\lambda$ over the set of all total rankings. In this model, a partial (or total) ranking $\tau$ is generated in a sequential manner where each item in a ranking is viewed as chosen from the set of items ranked lower. The probability of choosing an item $i$ from any set $S \subseteq [d]$ is given by $\frac{\exp(w_i)}{\sum_{(i' \in S)} \exp(w_{i'})}$. To explain the sequential generation procedure, we show an example here,

$$\mathbb{P}(i_1 \succ i_2 \succ \cdots \succ i_\ell) = \prod_{j=1}^{\ell} \frac{\exp(w_{i_j})}{\sum_{j'=j}^{\ell} \exp(w_{i_{j'}})}.$$

An important property of the Plackett-Luce model is that the marginal probability that item $i$ is ranked higher than item $j$, $\mathbb{P}(i \succ j)$ does not depend on the other items in the ranking, in fact, $\mathbb{P}(i \succ j) = \frac{\exp(w_i)}{(\exp(w_i)+\exp(w_j))}$. For each pair $(i, j)$, we denote the marginal probability $\mathbb{P}(i \succ j)$ corresponding to the parameters $w^P_{i\in[d]}$ as $p_{ij}$. Similarly, we denote the marginal probability $\mathbb{P}(i \succ j)$ corresponding to the parameters $w^Q_{i\in[d]}$ as $q_{ij}$. These pairwise marginal probabilities $p_{ij}$ and $q_{ij}$ are collected in pairwise-comparison probability matrices $P$ and $Q$ respectively.

Finally, with this notation, we specialise the hypothesis testing problem in (2.10) for the two partial ranking models described above, in terms of the pairwise probability matrices $P$ and $Q$. For any given parameter $\epsilon > 0$, we define the two-sample testing problem as

$$\begin{aligned}H_0 &: P = Q \\ H_1 &: \frac{1}{d}|||P - Q|||_{\mathrm{F}} \geq \epsilon.\end{aligned} \tag{2.13}$$

We note that under the Plackett-Luce model, the null condition in (2.10) is equivalent to the null condition in (2.13). Moreover, under the Plackett-Luce model, difference in two probability distributions $\lambda_P$ and $\lambda_Q$ is captured by difference in the pairwise probability matrices $P$ and $Q$.

Thus, we specialise the alternate condition in (2.10) in terms of scaled Frobenius distance between the pairwise probability matrices, $P$ and $Q$, denoted by the parameter $\epsilon$, to get the alternate condition in (2.13). Furthermore, under the marginal probability based model, the null condition in (2.10) implies $P = Q$ whereas the converse is not true. That is, there exist pairs of probability distributions over the set of all $d$-length rankings $\lambda_P$ and $\lambda_Q$, that follow the marginal probability based model with $\lambda_P \neq \lambda_Q$, such that their corresponding pairwise probability matrices $P$ and $Q$ are equal. Thus, under the marginal probability based model, by conducting a test for the hypothesis testing problem in (2.13) that controls the Type I error at level $\alpha$, we get control over Type I error at level $\alpha$ for the hypothesis testing problem in (2.10).

We are now ready to describe our main results for two-sample testing with partial (or total) ranking data.

### 2.4.2 Main results

Our testing algorithms for ranking data build upon the test statistic in Algorithm 1. To test for difference in probability distributions $\lambda_P$ and $\lambda_Q$, we first use a rank breaking method to convert the data into pairwise-comparisons, on which we apply the test statistic in (2.8). Given a rank breaking method, denoted by $R$, and rankings from the two populations, $S_{P_i}$ and $S_{Q_i}$ for $i \in [N]$, then the rank breaking algorithm yields pairwise-comparison data as $R(S_{P_{i\in[N]}}) = \{k^p_{ij}, X_{ij}\}_{(i,j)\in[d]^2}$ and, similarly, $R(S_{Q_{i\in[N]}}) = \{k^q_{ij}, Y_{ij}\}_{(i,j)\in[d]^2}$. Here, $k^p_{ij}, k^q_{ij}, X_{ij}, Y_{ij}$ represent the pairwise-comparison data as defined in Section 2.2.1. Now, we describe three rank breaking methods that we subsequently use in our testing algorithms, Algorithm 3 and Algorithm 4.

1. **Random disjoint**: In this method, denoted by $R_{\mathrm{R}}$, given a set of $N$ partial (or total) rankings, we randomly break each ranking up into pairwise-comparisons such that no item is in more than one pair. In this method, each $m$-length ranking yields $\lfloor \frac{m}{2} \rfloor$ pairwise-comparisons.
2. **Deterministic disjoint**: We use this rank breaking method, denoted by $R_{\mathrm{D}}$, when we have $N$ total rankings. In this method, we deterministically break each ranking into pairwise-comparisons so that no item is in more than one pair. So, we get $\lfloor \frac{d}{2} \rfloor$ pairwise-comparisons from each total ranking. First, we want the number of samples to be divisible by $d$, so we throw away $(N \bmod d)$ rankings chosen randomly. Then arbitrarily without looking at the data, partition the remaining rankings into $\lfloor \frac{N}{d} \rfloor$ disjoint subsets each containing $d$ rankings. Within each subset, we convert the $d$ rankings into $d\lfloor \frac{d}{2} \rfloor$ pairwise-comparisons deterministically such that we observe at least one comparison between each pair $(i, j) \in [d]$ with $i < j$. We keep exactly one pairwise-comparison for each pair in a subset. In this manner, we get to observe exactly $\lfloor \frac{N}{d} \rfloor$ comparisons between each pair of items.
3. **Complete**: In this method, denoted by $R_{\mathrm{C}}$, given a set of $N$ partial (or total) rankings, we break each ranking into all possible pairwise-comparisons for that ranking. In this method, each $m$-length ranking yields $\binom{m}{2}$ pairwise-comparisons.

Now, equipped with the rank breaking methods, we describe our first result which provides an algorithm (Algorithm 3) for the two-sample testing problem in (2.13) for the Plackett-Luce model, and associated upper bounds on its sample complexity.

**Input** : Two sets $S_P$ and $S_Q$ of $m$-length partial rankings, where $2 \leq m \leq d$. The two sets of partial rankings, $S_P$ and $S_Q$ correspond to pairwise probability matrices $P$ and $Q$ respectively, according to the Plackett-Luce model. Rank breaking method, $R \in \{R_{\text{R}}, R_{\text{D}}, R_{\text{C}}\}$.

**(1)** Using the rank breaking method get

$$\{k_{ij}^p, X_{ij}\}_{(i,j)\in[d]^2, i<j} \leftarrow R(S_P); \quad \{k_{ij}^q, Y_{ij}\}_{(i,j)\in[d]^2, i<j} \leftarrow R(S_Q).$$

**(2)** Execute Algorithm 1.

**Algorithm 3:** Two-sample testing with partial ranking data for Plackett-Luce model.

We note that both Algorithm 3 and Algorithm 4, defined in this section, can be used with any of the three rank breaking methods described. The subsequent guarantees provided depend on the rank breaking method used, as we see in Theorem 7 and Theorem 8.

In our results for two-sample testing under the Plackett-Luce modeling assumption, we consider two cases. In the first case, for some $m \in \{2, \cdots, d-1\}$, each sample is a ranking of some $m$ items chosen uniformly at random from the set of $d$ items. In the second case, the samples comprise of total rankings, that is, $m = d$. The following two theorems characterize the performance of Algorithm 3 thereby establishing an upper bound on the sample complexity of the two-sample testing problem defined in (2.13). In these theorems we use the disjoint rank breaking methods so that the pairwise-comparisons created from a ranking are independent.

**Theorem 7** *Consider the testing problem in* (2.13) *where pairwise probability matrices $P$ and $Q$ follow the Plackett-Luce model. Suppose we have $N$ samples, where for some $m \in \{2, \cdots, d-1\}$, each sample is a ranking of some $m$ items chosen uniformly at random from the set of $d$ items. Then, there are positive constants $c, c_0, c_1$ and $c_2$ such that if $N \geq c\frac{d^2 \log(d)}{m}\lceil \frac{c_0}{d\epsilon^2} \rceil$ and $\epsilon \geq c_1 d^{-c_2}$, then Algorithm 3 with the "Random disjoint" rank breaking method will correctly distinguish between $P = Q$ and $\frac{1}{d}|||P - Q|||_F \geq \epsilon$, with probability at least $\frac{2}{3}$.*

The proof of Theorem 7 is provided in Section A.1.4. The lower bound assumption on $\epsilon$ in Theorem 7 is to ensure that the sufficient number of pairwise comparisons needed after applying the random disjoint rank breaking algorithm, is not very large. Theorem 7 is a combined result of the random disjoint rank breaking algorithm and the result in Theorem 1. When we have total rankings, Algorithm 3 with the "Deterministic disjoint" rank breaking method yields an improvement in the sample complexity by a logarithmic factor. We state this formally in the following theorem.

**Theorem 8** *Consider the testing problem in* (2.13) *where pairwise probability matrices $P$ and $Q$ follow the Plackett-Luce model. Suppose we have $N$ samples of total rankings from each population. Then, there are positive constants $c, c_1$ and $c_2$ such that if $N \geq 2d\lceil \frac{c}{d\epsilon^2} \rceil$ and $\epsilon \geq c_1 d^{-c_2}$, then Algorithm 3 with the "Deterministic disjoint" rank breaking method will correctly distinguish between $P = Q$ and $\frac{1}{d}|||P - Q|||_F \geq \epsilon$, with probability at least $\frac{2}{3}$.*

The proof of Theorem 8 is provided in Section A.1.5. These two results provide an upper bound on the sample complexity when using partial (and total) rankings for the two-sample testing problem in (2.13) under the Plackett-Luce model. In Theorem 7 and Theorem 8 the lower bound of $\frac{2}{3}$ on probability of success is tied to the specific threshold used in Algorithm 1 in the same

manner as described for Theorem 1. Specifically, for any constant $\nu \in (0,1)$, Algorithm 3 can achieve a probability of error at most $\nu$ with the same order of sample complexity as mentioned in Theorem 7 and Theorem 8.

Algorithm 3 addresses the problem of two-sample testing under the Plackett-Luce model. Now, we provide a permutation test based algorithm for the more general, non-parametric model, namely, marginal probability based model. The permutation test method described in Algorithm 4 gives a sharper (implicit) threshold than that in Algorithm 3. Note that Algorithm 4 doesn't require any assumptions on the length of the partial-ranking data, the partial-ranking data in each population can be of varying lengths. Moreover, as we will see in Theorem 9, the Type I error guarantee of Algorithm 4 holds even if the pairwise-comparisons created from the rank breaking method are dependent, hence the guarantee does not depend on the choice of the rank breaking method.

The key difference between the permutation testing algorithm for pairwise-comparison data, described in Section 2.3.1, and the permutation testing algorithm for partial ranking data, described in Algorithm 4, is the shuffling step. In our partial ranking based setup, each ranking sample is obtained independent of all else while the pairwise-comparisons obtained from a rank are not necessarily independent of each other. Hence, in the partial ranking based permutation testing algorithm (Algorithm 4), we re-distribute ranking samples between the two populations and not the pairwise-comparisons.

**Input** : Two sets of partial rankings $S_P$ and $S_Q$ from two populations corresponding to the probability distributions $\lambda_P$ and $\lambda_Q$. Significance level $\alpha \in (0,1)$. Rank breaking method, $R \in \{R_{\mathrm{R}}, R_{\mathrm{D}}, R_{\mathrm{C}}\}$. Iteration count $\gamma$.

**(1)** Using the rank breaking method get

$$\{k^p_{ij}, X_{ij}\}_{(i,j)\in[d]^2, i<j} \leftarrow R(S_P); \quad \{k^q_{ij}, Y_{ij}\}_{(i,j)\in[d]^2, i<j} \leftarrow R(S_Q).$$

**(2)** Compute the test statistic $T$ defined in (2.8).

**(3)** {Repeat $\gamma$ times} Put the samples in $S_P$ and $S_Q$ together and reassign the samples at random such that the number of samples assigned to each population is the same as before. Repeat Step 1 and Step 2. Denote the computed test statistic as $T_\ell$ for the $\ell^{th}$ iteration.

**Output** : Reject the null if $p = \sum_{\ell=1}^{\gamma} \frac{1}{\gamma} \mathbb{1}(T_\ell - T) < \alpha$.

**Algorithm 4:** Two-sample testing algorithm with partial ranking data for marginal probability based model.

We now show that Algorithm 4 controls the Type I error of the two-sample testing problem in (2.10) under the more general, marginal probability based partial ranking model. This result relies on considerably weaker assumptions than Theorem 7 and Theorem 8. In particular, we do not assume that each ranking is of the same length. We only assume that the (sub)set of items ranked in each sample from each population is sampled independently from the same distribution. Specifically, let there be any probability distribution over all non-empty subsets of $[d]$. Then, the set of items ranked in each sample for each population is sampled i.i.d. from this distribution. Moreover, the number of samples from the two populations need not be equal.

**Theorem 9** *Consider any probability distributions $\lambda_P$ and $\lambda_Q$ and the two-sample testing problem in (2.10). Suppose we have partial ranking data from each population such that the sets of items ranked in each sample in each population is sampled i.i.d. from any probability distribution over all non-empty subsets of $[d]$. Suppose the partial ranking data follows the marginal probability based model. Then, for any significance level $\alpha \in (0, 1)$ the permutation testing method of Algorithm 4 has Type I error at most $\alpha$.*

The proof of Theorem 9 is provided in Section A.1.6. Recall that the Plackett-Luce model is a special case of the marginal probability based model, and hence as a direct corollary, the guarantees for Algorithm 4 established in Theorem 9 also apply to the Plackett-Luce model.

## 2.5 Experiments

In this section, we present results from experiments on simulated and real-world data sets, to gain a further understanding of the problem of two-sample testing on pairwise-comparison data.

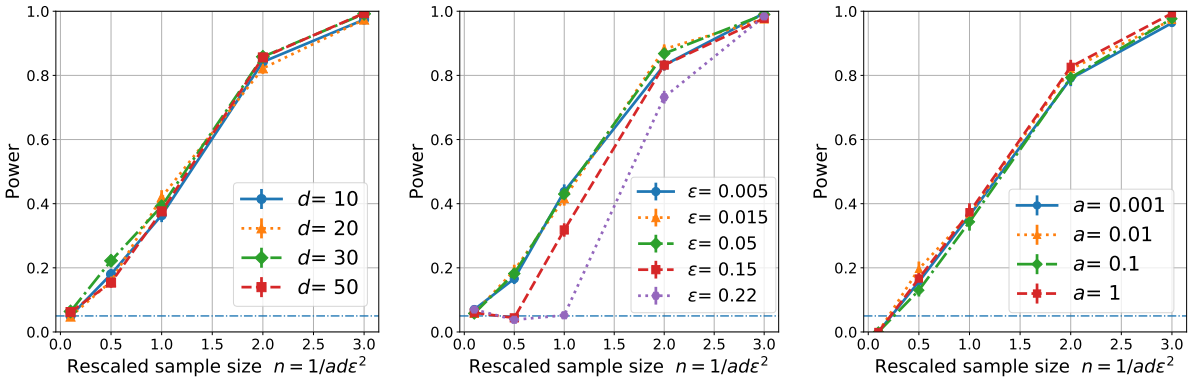
### 2.5.1 Pairwise-comparison data

We now describe real-world experiments and synthetic simulations we conduct for two-sample testing on pairwise-comparison data. In these experiments, we use the test statistic we designed in Algorithm 1 along with the permutation testing method as described in Algorithm 2 to obtain an implicit value of the threshold and control Type I error.

#### Synthetic simulations

We conduct two sets of experiments via synthetic simulations. The first set of experiments empirically evaluates the dependence of the power of our test (Algorithm 1) with respect to individual problem parameters. In each of the simulations, we set the significance level to be 0.05. Specifically, given the problem parameters $n, a, d$ and $\epsilon$, we consider the random-design setting with $k^p_{ij}, k^q_{ij} \overset{\text{iid}}{\sim} \text{Bin}(n, a)$. We consider the asymmetric and model-free setting, fix $P = [\frac{1}{2}]^{d\times d}$ and set $Q = P + \Delta$ where $\Delta$ is sampled uniformly at random from the set of all matrices in $[-\frac{1}{2}, \frac{1}{2}]^{d\times d}$ with $\frac{1}{d}\|\Delta\|_{\text{F}} = \epsilon$. In Figure 2.1a, b and c, we vary the parameter $d$, $\epsilon$ and $a$ respectively, keeping the other parameters fixed. Recall that our results in Theorem 1 and Corollary 2 predict the sample complexity as $n = \Theta(\frac{1}{ad\epsilon^2})$. To test this theoretical prediction, we set the sample size $n$ (on the x-axis) as $n = \frac{1}{ad\epsilon^2}$, and plot the power of the test on the y-axis. Each plot point in Figure 2.1 is obtained by averaging over 400 iterations of the experiment, and the threshold for the test is obtained by running the permutation test method over 5000 iterations. Observe that, interestingly in each figure, the curves across all values of the varied parameters nearly coincide, thereby validating the sample complexity predicted by our theoretical results.

The second set of experiments empirically investigates the role of the underlying pairwise-comparison models in two-sample testing with our test (Algorithm 1). We consider the random-design setup in the symmetric setting with $k^p_{ij}, k^q_{ij} \overset{\text{iid}}{\sim} \text{Bin}(n, a)\ \forall\ i < j$. We generate the matrices $P$ and $Q$ in three ways: model-free, BTL and SST. In the model-free setting, we generate $P$ and $Q$ in a manner similar to the first set of simulations above, with the additional constraints




(a) Varying $d$ (b) Varying $\epsilon$ (c) Varying $a$

Figure 2.1: Power of the testing algorithm versus the scaling factor of the sample size parameter $n = \frac{1}{ad\epsilon^2}$. We use Algorithm 2 which uses the test statistic in (2.8) with the permutation testing method. The test is conducted at a significance level of $0.05$ (indicated by the horizontal line at $y = 0.05$). Unless specified otherwise, the parameters are fixed as $d = 20, \epsilon = 0.05, a = 1$.

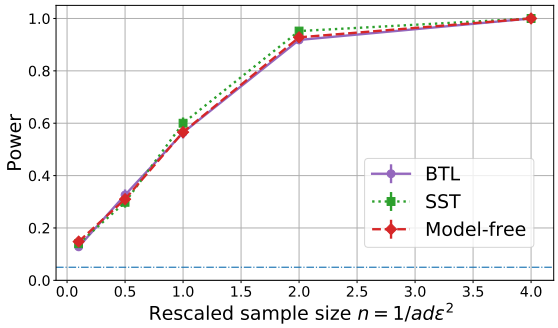



Figure 2.2: Power of our test (Algorithm 2) under three different models for pairwise-comparisons: BTL, SST and the model-free setting. The parameters of the problem are set as $d = 20, \epsilon^2 = 0.05, a = 1$ and the test is conducted at a significance level of $0.05$ (indicated by the horizontal line at $y = 0.05$).

$\Delta_{ji} = 1 - \Delta_{ij}\ \forall\ \ i \leq j$. For the BTL and SST models, we fix $P = [\frac{1}{2}]^{d\times d}$. For the BTL model, we choose $w_p$ according to the construction in Section A.1.3 to obtain $Q$. For the SST model, we set $Q = P + \Delta$, where matrix $\Delta$ is generated as follows. We generate $\Delta$ by arranging $\binom{d}{2}$ random variables uniformly distributed over $[0, 1]$, in a row-wise decreasing and column-wise increasing order in the upper triangle matrix $(\Delta_{ij} = 1 - \Delta_{ji})$ and normalizing to make $\frac{1}{d}|||\Delta|||_{\mathrm{F}} = \epsilon$. This construction ensures that $\Delta$ lies in the SST class, and since matrix $P$ is a constant matrix, $Q$ is also guaranteed to lie in the SST class. The results of the simulations are shown in Figure 2.2. The results show that in the settings simulated, the power of the testing algorithm is identical in all the models considered. This leaves an open question whether there exists a tighter information-theoretic lower bound for the SST and the parameter-based model that matches the upper bound derived for the test in Algorithm 1 or if there exists a test statistic for these models with a better rate.

## Real-world data

In this section, we describe the results of our experiments on two real-world data sets. In these experiments, we use Algorithm 2 to obtain a $p$-value for the experiment.

**Ordinal versus cardinal** An important question in the field of crowdsourcing and data-elicitation from people is whether pairwise-comparisons provided by people (ordinal responses) are distributed similarly to if they provide ratings (cardinal responses) which are then converted to pairwise-comparisons (Shah et al., 2016; Raman and Joachims, 2014). In this section, we use the permutation based two-sample test described in Algorithm 2 to address this question. We use the data set from (Shah et al., 2016) comprising six different experiments on the Amazon Mechanical Turk crowdsourcing platform. In each experiment, workers are asked to either provide ratings for the set of items in that experiment (age for photo given, number of spelling mistakes in a paragraph, distance between two cities, relevance of web-based search results, quality of taglines for a product, frequency of a piano sound clip) or provide pairwise-comparisons. The number of items in each experiment ranged from 10 to 25. For each of the six experiments, there were 100 workers, and each worker was assigned to either the ordinal or the cardinal version of the task uniformly at random. The first set of samples corresponds to the elicited ordinal responses and the second set of samples are obtained by converting the elicited ratings to ordinal data. We have a total of 2017 ordinal responses and 1671 cardinal-converted-to-ordinal responses. More details about the data set and experiment are provided in the appendix.

Using Algorithm 2, we test for difference in the two resulting distributions for the entire data set ($d = 74$). We observe that *the test rejects the null with a $p$-value of 0.003, thereby concluding a statistically significant difference between the ordinal and the cardinal-converted-to-ordinal data*.

**European football leagues** In the second data set, we investigate whether the relative performances of the teams (in four European football leagues: English Premier League, Bundesliga, Ligue 1, La Liga) changed significantly from the 2016-17 season to the 2017-18 season. The leagues are designed such that each pair of teams plays twice in a season (one home, one away game), so we have at most two pairwise-comparisons per pair within a league (we do not consider the games that end in a draw). Each league has 15-17 common teams across two consecutive seasons. This gives a total of 801 and 788 pairwise-comparisons in the 2016-17 and 2017-18 seasons respectively. More details about the experiment are provided in the appendix.

Using the test statistic of Algorithm 1 with permutation testing, we test for a difference in the two resulting distributions for the entire data set ($d = 67$). We observe that *the test fails to reject the null with a $p$-value of 0.971, that is, it does not recognize any significant difference between the relative performance of the European football teams in 2017-18 season and the 2016-17 season from the data available*. Running the test for each league individually also fails to reject the null.

### 2.5.2 Partial and total ranking data

We now describe the experiments we conducted on real-world data for two-sample testing on partial (and total) ranking data. In these experiments, we use the test statistic (2.8) along with the permutation testing method, as explained in Algorithm 4.

For our experiments, we use the "Sushi preference data set" (Kamishima, 2003), in which subjects rank different types of sushi according to their preferences. The data set contains two sets of ranking data. In the first set, the subjects are asked to provide a total ranking over 10 items (popular types of sushi). In this set, all subjects are asked to rank the same 10 objects. This set contains 5000 such total rankings.

In the second set of ranking data, a total of 100 types of sushi are considered. We first describe how the 100 types are chosen. The authors in (Kamishima, 2003) surveyed menu data from 25 sushi restaurants found on the internet. For each type of sushi sold at the restaurant, they counted the number of restaurants that offered the item. From these counts, they derived the probabilities that each item would be supplied. By eliminating unfamiliar or low frequency items, they came up with a list of 100 items. Each subject in this set is asked to rank a subset of 10 items randomly selected from the 100 items, according to the probability distribution described above. This set contains responses from 5000 subjects.

In addition, this data set contains demographic information about all the subjects, including their

(a) Gender $\{$Male, Female$\}$

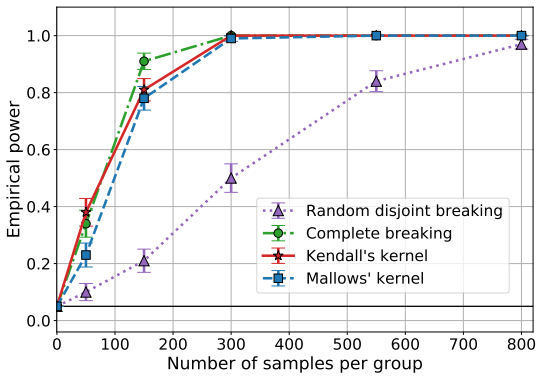
(b) Age $\{$Above 30, Below 30$\}$

(c) Current region of residence $\{$East, West$\}$

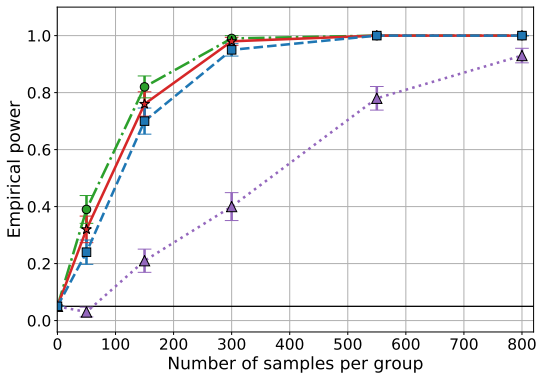
(d) Primary region of residence until 15 years old $\{$East, West$\}$.

Using our testing algorithm, we test for a difference in preferences across the two sections within each demographic mentioned above. In the first set of experiments, we implement the permutation testing method with our test statistic (2.8) on the first set of ranking data with $d = 10$ for each demographic division. We show the results in Figure 2.3 for two rank-breaking methods, namely, "Random Disjoint" and "Complete". In addition, we show the results of two kernel-based two-sample testing methods for total rankings designed in (Mania et al., 2018), namely, Kendall's kernel and Mallows' kernel. We randomly sub-sampled $n$ samples from each sub-group of subjects and used 200 permutations to determine the rejection threshold for the permutation test. In these experiments, we show the empirical power of our testing method, which is the fraction of times our test rejected the null in a total of 100 trials. We show all the results of using this method on the sushi data set in Figure 2.3. *Across each demographic division, our test detects a statistically significant difference in distribution over sushi preferences for the 10 types of sushi included*. Moreover, our testing algorithm with "Complete" rank breaking method performs competitively with the kernel-based two-sample testing methods introduced in (Mania et al., 2018).

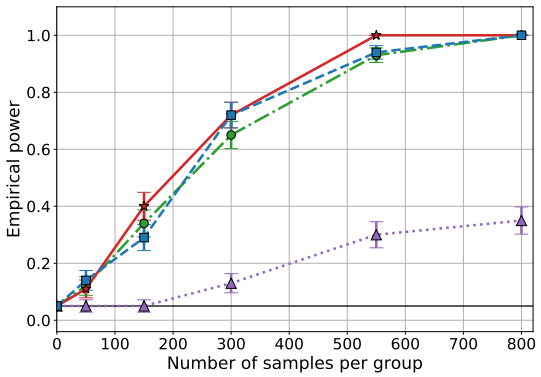
We note that since our testing algorithms also work with partial ranking data, our testing algorithms are much more general than the testing algorithms in (Mania et al., 2018), as we demonstrate in our next set of experiments on the second sushi preference data set. We use Algorithm 4 to test if the preferences of the subjects in the second sushi data set also varies



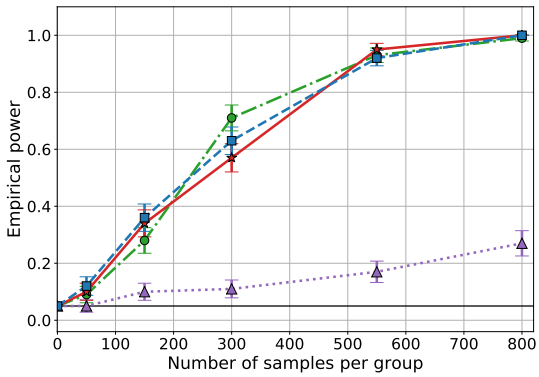

(a) Grouping by gender




(b) Grouping by age



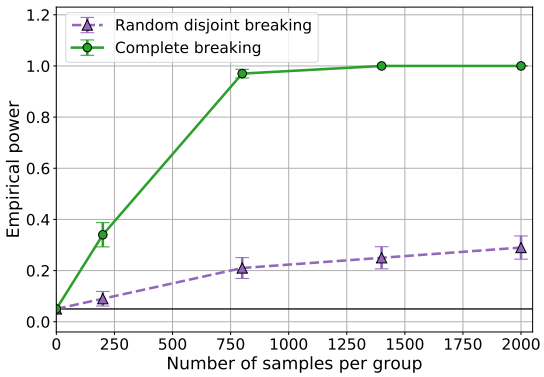

(c) Grouping by primary region of residence until 15 years old



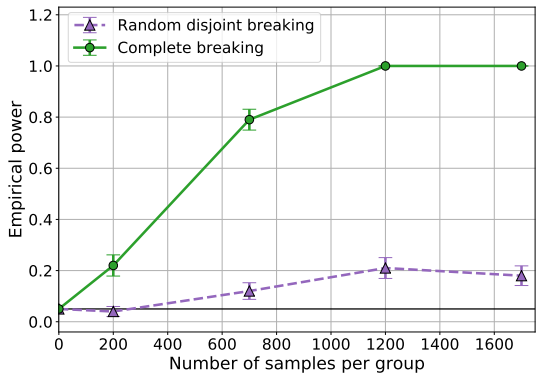

(d) Grouping by current region of residence

Figure 2.3: Empirical power of our test statistic $T$ with the permutation testing method described in Algorithm 4 in testing for difference in sushi preference from the first set of responses with $d = 10$. The responses obtained comprise of total rankings from each subject. Test results are shown for differences between demographic division based on the information available. Two different rank breaking methods are used for our algorithm, namely, "Random Disjoint" and "Complete". We also show the results for kernel-based two-sample testing with Kendall's kernel and Mallows' kernel as in (Mania et al., 2018). The x-axis shows the number of samples (total rankings) from each group used to conduct the test and the y-axis shows the empirical power of our test. The test is conducted at a significance level of $0.05$ (indicated by the horizontal line at $y = 0.05$). Empirical power is computed as an average over 100 trials.

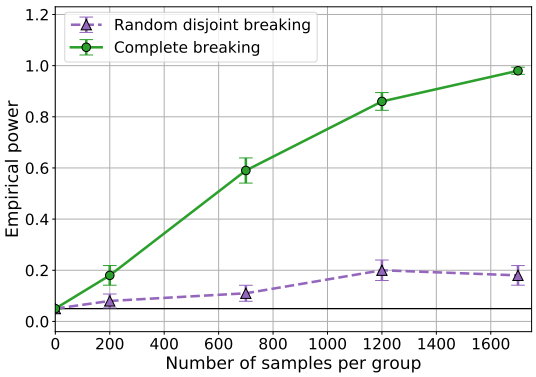
across the different demographics. Recall that this data set has $d = 100$ items in total but each ranking only ranks a subset of $10$ items. The other details of the experiment are the same as the previous experiment. The results are shown in Figure 2.4. Again, across each demographic, our test detects a statistically significant difference in distribution over sushi preferences for the 100 types of sushi included.



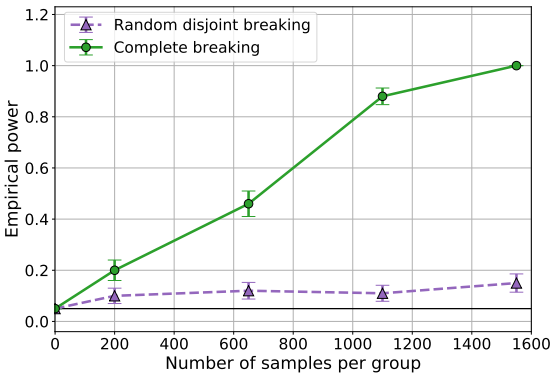

(a) Grouping by gender




(b) Grouping by age




(c) Grouping by primary region of residence until 15 years old




(d) Grouping by current region of residence

Figure 2.4: Empirical power of our test statistic $T$ with the permutation testing method described in Algorithm 4 in testing for difference in sushi preference from the first set of responses with $d = 100$. Test results are shown for differences between demographic division based on the information available. Two different rank breaking methods are used, namely, "Random Disjoint" and "Complete". The x-axis shows the number of samples (total rankings) from each sub-group used to conduct the test and the y-axis shows the empirical power of our test. The test is conducted at a significance level of $0.05$ (indicated by the horizontal line at $y = 0.05$). Empirical power is computed as an average over 100 iterations.

## 2.6 Discussion and open problems

We conclude with a discussion focused on open problems in this area. We provide algorithms for two-sample testing on pairwise-comparison and ranking data to distinguish between two potentially different populations in terms of their underlying distributions. Through our analysis, we see that our testing algorithm for pairwise-comparison data is simultaneously minimax optimal under the model-free setting as well as the MST and WST model. There is a gap between the testing rate of our algorithm and our information-theoretic lower bound for the SST and parameter-based models, and closing this gap is an open problem of interest. In addition, our lower bound does not consider the random sampling regime we address in Corollary 2, thus, obtaining a lower bound in this regime is another open problem of interest. Second, in the future, our work may help in studying two-sample testing problems pertaining to more general aspects

of data from people such as function evaluations (Xu et al., 2020; Noothigattu et al., 2018), issues of calibration (Wang and Shah, 2019b), and strategic behavior (Xu et al., 2019). Thirdly, in practice we use the permutation method to calibrate our tests, ensuring valid Type I error control even when the distribution of the test statistic is difficult to characterize analytically (for instance, in the setting with partial rank data). Understanding the power of tests calibrated via the permutation method is an active area of research (Kim et al., 2020a) and it would be interesting to understand this in the context of the tests developed in our work. Finally, the literature on analyzing pairwise-comparison data builds heavily on probability models from social choice theory (some are described in this work). A natural related question, that has received some recent attention in Seshadri and Ugander (2019), is the design of goodness-of-fit hypothesis tests to test whether given pairwise-comparison data obeys certain modeling assumptions.

# Chapter 3

# No Rose for MLE: Inadmissibility of MLE for Evaluation Aggregation Under Levels of Expertise

Based on (Rastogi et al., 2022c):
Charvi Rastogi, Ivan Stelmakh, Nihar Shah, and Sivaraman Balakrishnan. No Rose for MLE: Inadmissibility of MLE for Evaluation Aggregation under Levels of Expertise. In 2022 *IEEE International Symposium on Information Theory* (ISIT 2022).

## 3.1 Introduction

A number of applications involve evaluations from multiple people with varying levels of expertise, and an eventual objective of aggregating the different evaluations to obtain a final decision. For instance, in peer-review, multiple reviewers provide their evaluation regarding the acceptance of the submission and their expertise on the submission matter. Another instance is found in crowdlabeling, where multiple crowdworkers provide labels for the same question. Additionally, one often has access to the evaluators' level of expertise, for instance, from their known expertise (Shah, 2021), self-reported confidence (Shah and Zhou, 2015) or their prior approval rating (Staffelbach et al., 2014). Other such applications include decision-making in university admissions, grant allotments etc., where the quality of individual decisions obtained generally varies across individuals because of varying levels of expertise. Each of the aforementioned problems involves aggregation of multiple evaluations with varying expertise.

Moreover, in such settings, it is frequently the case that the set of evaluators are *deliberately* chosen in a certain manner based on their expertise levels. As an example, in the peer-review process of the AAAI 2022 conference, due to lack of sufficient senior reviewers, each paper was assigned one senior and one junior reviewer. Similarly, in crowdlabeling, budget constraints impose the need for balancing out high expertise (but more expensive) and low expertise (but cheaper) crowdworkers.

There is a vast literature on the problem of aggregation of multiple evaluations in crowd-

sourcing (Karger et al., 2011a;b; Sheng et al., 2008; Dalvi et al., 2013; Ghosh et al., 2011; Gao and Zhou, 2013; Zhou et al., 2015; Zhang et al., 2014; Shah et al., 2020). The bulk of this past work is based on the classical Dawid-Skene model (Dawid and Skene, 1979), in which each evaluator is associated with a single scalar parameter corresponding to their probability of correctness. While the Dawid-Skene model does not incorporate expertise levels, a natural extension by (Oyama et al., 2013) incorporates them with separate parameters for each expertise level.

(Dawid and Skene, 1979) propose the maximum likelihood estimator (MLE) for estimation. They use the Expectation-Maximization algorithm to approximately compute the MLE. The correct answers and the evaluator's parameter for correctness are jointly estimated by maximizing the likelihood of the observed evaluations. This MLE-based approach has had huge empirical success (Snow et al., 2008; Zhou et al., 2014; Raykar et al., 2010). Moreover, theoretical analyses by (Gao and Zhou, 2013) have shown that global optimal solutions of the MLE can achieve minimax rates of convergence in simplified scenarios such as "one-coin" Dawid-Skene. Paralelly, computationally efficient approximations of the MLE have proven to be useful for crowdlabelling, with many of the desired properties of the MLE (Zhang et al., 2014). Prior work on crowdlabeling with multiple expertise levels by (Oyama et al., 2013) also uses MLE for label estimation. With this motivation, we focus on analyzing the MLE in our problem of aggregating evaluations with multiple expertise levels. Our work contributes to the body of literature on Neyman-Scott problems (Neyman and Scott, 1948; Ghosh, 1994) that focus on the behavior of MLE where the number of nuisance parameters grows with the size of the problem.

We focus on objective tasks involving binary choices, meaning that each question or task is associated with a single correct binary answer. We consider the extension of the Dawid-Skene model from prior work by (Oyama et al., 2013) which incorporates multiple expertise levels, in a simplified form. Our main contribution is a surprising negative result of asymptotic inadmissibility of the MLE in this context. Specifically, we consider a setting wherein each question is evaluated by exactly two low-level experts (or non-experts) and one (high-level) expert. We prove that MLE is asymptotically inadmissible even in this simplified setting. To prove this result, we construct an alternative polynomial-time-computable estimator and show that for all possible parameter values, the alternative estimator is as good as or better than the MLE. Importantly, for some parameter values, we show that the risk of MLE is higher than that of our estimator by a positive constant.

We pictorially illustrate this in Figure 3.1. For parameter values in the light gray region our estimator is significantly better than MLE, and for parameter values lying in the dark gray region our estimator is as good as MLE. We subsequently provide simulations to qualitatively show that this finding extends to other combinations of expertise evaluations in a finite sample setting.

## 3.2 Problem setup

Let $m$ denote the number of questions. We assume every question has two possible answers, denoted by $0$ and $1$, of which exactly one is correct. Each question is answered by multiple evaluators, and for each question-answer pair, we have access to the evaluator's level of expertise in the corresponding question; examples of such expertise level include the evaluator's self-reported confidence or their seniority in the application domain. We assume there are two expertise lev-

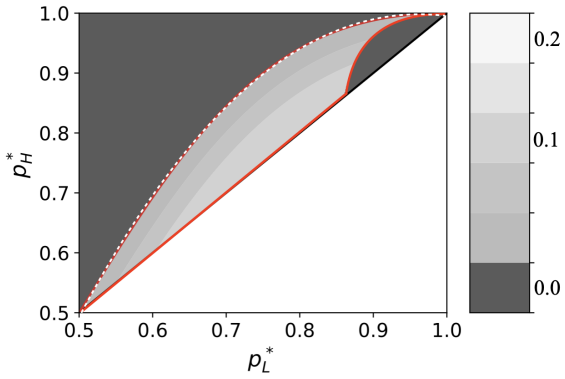



Figure 3.1: Pictorial illustration of the main theoretical results: risk of MLE minus risk of our proposed estimator for different parameter values. The two axes represent the two latent (nuisance) parameters. The MLE performs significantly worse than our constructed estimator in the light gray region enclosed within the red lines, whereas everywhere else above the diagonal our estimator is asymptotically as good as the MLE.

els, which we refer to as *low*, denoted by $L$, and *high*, denoted by $H$. Under this expertise-level information, we model the question-answering as follows.

We will show that MLE is asymptotically inadmissible even in a simplified setting: we consider the setting where each question has exactly two evaluators with a low level of expertise and one evaluator with a high level of expertise, and that the probability of correctness is governed only by the expertise level. For each question without loss of generality, we assume that the first two evaluators have low expertise level and the third evaluator has a high expertise level. For any question $i \in [m]$, we let $x_i^*$ denote the correct answer. The evaluation of the $j^{\text{th}}$ evaluator ($j \in \{1, 2, 3\}$) for the $i^{\text{th}}$ question is denoted by $y_{ij}$. The probability of correctness, $\mathbb{P}(y_{ij} = x_i^*)$ depends on the associated expertise level of the evaluator, and is independent of all else. Specifically, we assume existence of two *unknown* values $p_L^*, p_H^* \in [0, 1]$ that govern the correctness probabilities of low and high expertise evaluators respectively. We assume that

$$y_{ij} = \begin{cases} x_i^* & \text{wp } p, \\ 1 - x_i^* & \text{wp } 1 - p, \end{cases} \tag{3.1}$$

where $p = p_L^*$ for $j \in \{1, 2\}$ and $p = p_H^*$ for $j = 3$. We further assume that $0.5 \le p_L^* \le p_H^* \le 1$, which indicates that the evaluators are not adversarial (Shah et al., 2020; Gadiraju et al., 2015; Yuen et al., 2011), and that the high-expertise evaluator answers correctly with a probability at least as high as that for a low-expertise evaluator (Koriat, 2012; Hertwig, 2012; Stelmakh et al., 2021a). We make the standard assumption that for all $i \in [m]$ and $j \in [3]$, given the values of $x_i^*$ and $p_L^*, p_H^*$, the evaluations $y_{ij}$ are mutually independent.

For ease of exposition subsequently in the paper, for all $i \in [m]$ we introduce the notation $y_{Li} := y_{i1} + y_{i2} \in \{0, 1, 2\}$ and $y_{Hi} := y_{i3} \in \{0, 1\}$.

**Evaluation metric** Consider any estimator $\widehat{x} : \{0, 1\}^{3 \times m} \to \{0, 1\}^m$ as a function that maps the received evaluations to answers for all questions. We let $\widehat{x}_i$ denote the output of the estimator

for question $i \in [m]$ wherein we drop the dependence on $y$ from the notation for brevity. We then evaluate any estimator $\widehat{x}$ in terms of the 0-1 loss, and focus on the risk:

$$R(\widehat{x}) = \mathbb{E}_{\{y_{ij}\}_{(i,j)\in[m]\times[3]}} \left[ \frac{1}{m} \sum_{i=1}^{m} \mathbb{I}\left(\widehat{x}_i \neq x_i^*\right) \right]. \tag{3.2}$$

Note that the risk for any estimator lies in the interval $[0, 1]$.

The goal of any estimator is to minimize the risk (3.2). In this setting, a widely studied and used estimator is the MLE. In this work, we provide a negative result for the MLE. We first formally describe the MLE for our problem.

**Maximum likelihood estimator (MLE)** The values $p_L^*, p_H^*$ are unknown, and thus MLE simultaneously estimates the correct answers $x^*$ and the values $p_L^*, p_H^*$. Given answers $\vec{y}_L \in \{0, 1, 2\}^m$ and $\vec{y}_H \in \{0, 1\}^m$, under our model (3.1), the negative log-likelihood $G(\vec{x}, p_L, p_H, \vec{y}_L, \vec{y}_H)$ is given by

$$\begin{aligned} G(\vec{x}, p_L, p_H, \vec{y}_L, \vec{y}_H) = \sum_{i=1}^{m} \Big( & (y_{Li} + 2(1 - y_{Li})x_i) \log \frac{p_L}{1 - p_L} \\ & - 2 \log p_L + (y_{Hi} - x_i)^2 \log \frac{p_H}{1 - p_H} - \log p_H \Big). \end{aligned} \tag{3.3}$$

The MLE minimizes the negative log-likelihood function (3.3) to obtain an estimate of the probability values, denoted by $\widehat{p}_L, \widehat{p}_H$ and estimator of the correct answers denoted by $\widehat{x}_{\text{MLE}} : \{0, 1, 2\}^m \times \{0, 1\}^m \to \{0, 1\}^m$, where $\widehat{x}_{\text{MLE}_i}$ denotes the estimate for the $i^{\text{th}}$ question. Thus, we have

$$\widehat{x}_{\text{MLE}}, \widehat{p}_L, \widehat{p}_H \in \underset{\substack{\vec{x}\in\{0,1\}^m; \\ p_L, p_H \in [0.5,1]^2; \\ p_L \leq p_H}}{\arg\min} G(\vec{x}, p_L, p_H, \vec{y}_L, \vec{y}_H), \tag{3.4}$$

where for concreteness we assume that for all $i \in [m]$ the estimator $\widehat{x}_{\text{MLE}_i}$ breaks ties in favour of $y_{Hi}$. Note that the minimizer of the objective function $\widehat{p}_L, \widehat{p}_H$ is unique, due to convexity, the formal argument is provided in Section B.1.3. Now, the objective function in (3.4) may be rewritten as follows. For any given $p_L, p_H$ let $\widehat{x}_{\text{MLE}}(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) : \{0, 1, 2\}^m \times \{0, 1\}^m \to \{0, 1\}^m$ denote the estimator function for the true answers $x^*$. Then, we have

$$\begin{aligned} \min_{\substack{p_L, p_H \in [0.5,1]^2; \\ p_L \leq p_H}} \; \min_{\widehat{x}_{\text{MLE}}(p_L, p_H, \vec{y}_L, \vec{y}_H)} \sum_{i=1}^{m} \Bigg( & (y_{Li} + (2 - 2y_{Li})\widehat{x}_{\text{MLE}_i}) \log \frac{p_L}{1 - p_L} - 2 \log p_L \\ & + (y_{Hi} - \widehat{x}_{\text{MLE}_i})^2 \log \frac{p_H}{1 - p_H} - \log p_H \Bigg). \end{aligned} \tag{3.5}$$

where for all $i \in [m]$ the estimator $\widehat{x}_{\text{MLE}_i}$ breaks ties in favour of $y_{Hi}$.

## 3.3 Main result

In this section, we provide our main result that the MLE is asymptotically inadmissible. In order to prove this result, we construct another estimator which we call the plug-in estimator.

### 3.3.1 Proposed estimator

As an intermediary in constructing the plug-in estimator, we first introduce and analyze an estimator we call the oracle MLE.

**Oracle MLE.** The oracle MLE is an estimator that is assumed to have access to the true values $p_L^*$ and $p_H^*$ (and is hence not realizable in our problem setting). It computes the maximum likelihood estimate $\widehat{x}_{\text{OMLE}}$ given $p_L^*$ and $p_H^*$ as:

$$\widehat{x}_{\text{OMLE}} \in \underset{\vec{x}\in\{0,1\}^m}{\arg\min}\, G(\vec{x}, p_L^*, p_H^*, \vec{y}_L, \vec{y}_H), \tag{3.6}$$

where for concreteness we assume that for all $i \in [m]$ the estimator $\widehat{x}_{\text{OMLE}_i}$ breaks ties in favour of $y_{Hi}$. Observe that with the true $p_L^*, p_H^*$, the objective function for each question can be treated separately. In the following lemma, we characterise the estimation by oracle MLE. We will see that, for all questions, it either goes with the high expertise evaluation or goes with the majority vote of the three evaluators.

**Theorem 10** *For any given value of $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \leq p_H^*$ the solution of (3.6), for all $i \in [m]$ is given by $\widehat{x}_{OMLE_i} = f_{t^*}(y_{Li}, y_{Hi})$, defined as follows. For any question $i$, let $a_i \in \{0, 1, 2\}$ denote the number of low expertise evaluations that agree with the high expertise evaluation, that is, $a_i = \sum_{j=1}^{2} \mathbb{I}(y_{ij} = y_{Hi})$. Let $t^* \in \{1, 2\}$ be defined for $p_L^*, p_H^* \in (0.5, 1)^2$ as*

$$t^* = \max\left(\left\lceil \frac{1}{2}\left(2 - \frac{\log\frac{p_H^*}{1-p_H^*}}{\log\frac{p_L^*}{1-p_L^*}}\right)\right\rceil, 0\right) + 1, \tag{3.7}$$

*and, if $p_L^* = 0.5$ or $p_H^* = 1$ we set $t^* = 1$. Now, we have*

$$f_{t^*}(y_{Li}, y_{Hi}) = \begin{cases} 1 - y_{Hi} & \text{if } a_i + 1 < t^* \\ y_{Hi} & \text{otherwise.} \end{cases} \tag{3.8}$$

We pictorially illustrate the operation of the oracle MLE in Figure 3.1, where for $(p_L^*, p_H^*)$ to the left of the red dashed line it picks $t^* = 1$ and to the right of this line it picks $t^* = 2$. According to Theorem 10, if $p_L^* = p_H^*$, then $t^* = 2$ which implies that $\widehat{x}_{\text{OMLE}_i}$ goes with the majority vote, and if $p_L^* = 0.5$ and $p_H^* \geq 0.5$, then $t^* = 1$, which implies that $\widehat{x}_{\text{OMLE}_i}$ goes with the high-level expert for all $i \in [m]$. We provide a proof for Theorem 10 in Section B.1.1.

Next, we present our constructed estimator, the plug-in estimator using the functional form derived in Theorem 10.

**Input:** $m$ and $\{y_{ij}\}_{i\in[m],j\in[3]}$, where recall that $y_{Li} = y_{i1} + y_{i2}, y_{Hi} = y_{i3}$ for all $i \in [m]$.

**(1)** Define $\mu_L = \frac{2}{\sqrt{m}} \sum_{i=1}^{\sqrt{m}/2} \mathbb{I}(y_{i1} = y_{i2})$. Compute $\widetilde{p}_L$ as

$$\widetilde{p}_L = 0.5 \left(1 + \sqrt{\max\{2\mu_L - 1, 0\}}\right). \tag{3.9}$$

**(2)** Define $\mu_H = \frac{2}{\sqrt{m}} \sum_{i=\sqrt{m}/2+1}^{\sqrt{m}} \mathbb{I}(y_{i1} = y_{i3})$. Compute $\widetilde{p}_H$ as

$$\widetilde{p}_H = \min\left\{1, \frac{\widetilde{p}_L + \mu_H - 1}{2\widetilde{p}_L - 1}\right\}. \tag{3.10}$$

**(3)** If $\widetilde{p}_L > \widetilde{p}_H$, then reset $\widetilde{p}_L = \widetilde{p}_H = (\widetilde{p}_L + \widetilde{p}_H)/2$.

**(4)** Define $t_{\text{PI}}$ as follows. For $\widetilde{p}_L, \widetilde{p}_H \in (0.5, 1)^2$ set

$$t_{\text{PI}} = \max\left(\left\lceil \frac{1}{2}\left(2 - \frac{\log \frac{\widetilde{p}_H}{1-\widetilde{p}_H}}{\log \frac{\widetilde{p}_L}{1-\widetilde{p}_L}}\right)\right\rceil, 0\right) + 1. \tag{3.11}$$

For $\widetilde{p}_L = 0.5$ or $\widetilde{p}_H = 1$ set $t_{\text{PI}} = 1$.

**Output:** For each question $i \in [m]$, output $\widehat{x}_{\text{PI}_i} = f_{t_{\text{PI}}}(y_{Li}, y_{Hi})$ with $f_{t_{\text{PI}}}$ as defined in (3.8).

**Algorithm 5:** The proposed plug-in estimator.

**Plug-in estimator.** This is a two-stage polynomial-time-computable estimator and is described in Algorithm 5. In the first stage (steps 1, 2 and 3 of Algorithm 5), the probability values $p_L^*$ and $p_H^*$ are estimated (with estimates denoted as $\widetilde{p}_L$ and $\widetilde{p}_H$) by measuring the agreement between the two low expertise evaluations, and one low and one high expertise evaluation respectively, for $\sqrt{m}$ questions. In the second stage (step 4 and output of Algorithm 5), $\widetilde{p}_L$ and $\widetilde{p}_H$ are plugged-in to the MLE objective function (3.3) to get the estimator $\widehat{x}_{\text{PI}}$. The functional form of the output of Algorithm 5 — specifically, (3.11) and $\widehat{x}_{\text{PI}}$ — is based on the form of the oracle MLE derived in Theorem 10. We now show that in the limit, as $m \to \infty$, the error incurred by the plug-in estimator is the same as the error incurred by the oracle MLE.

**Theorem 11** *Consider $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \le p_H^*$, and $x_i^* \in \{0, 1\}$ for all $i \in [m]$. Let the responses obtained be denoted by $\vec{y}_L \in \{0, 1, 2\}^m$ and $\vec{y}_H \in \{0, 1\}^m$. Then the plug-in estimator, defined in Algorithm 5 and the oracle MLE, defined in (3.6), behave such that*

$$\lim_{m\to\infty} |R_m(\widehat{x}_{OMLE}) - R_m(\widehat{x}_{PI})| = 0,$$

*where $R_m(\widehat{x}_{PI})$ and $R_m(\widehat{x}_{OMLE})$ is the error metric defined in (3.2).*

In other words, Theorem 11 states that the two estimators are equally good when the number of questions $m$ tends to infinity. The proof of Theorem 11 is provided in Section B.1.2.

### 3.3.2 Asymptotic inadmissibility of MLE

Let $R_m(\widehat{x}_{\text{MLE}})$ and $R_m(\widehat{x}_{\text{PI}})$ denote the risk of the MLE and the plug-in estimator respectively, as defined in (3.2). To prove that the MLE is asymptotically inadmissible in our setting, we show that there exist no values of $p_L, p_H$ such that the MLE has a lower risk than the constructed plug-in estimator, described in Algorithm 5. We do this in two steps. First we show that there exist $p_L^*, p_H^*$ such that the risk of MLE is higher than the risk of plug-in estimator, by more than a positive constant. Second, we show that asymptotically the risk of the plug-in estimator is as good as or better than that of MLE for all $p_L^*, p_H^*$.

**Negative result.** Through the following theorem, we show that for some $p_L^*, p_H^*$ the risk of MLE is worse than that of the plug-in estimator by a constant.

**Theorem 12** *There exist $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \leq p_H^*$ and $m_0$ such that for all $m \geq m_0$, we have $R_m(\widehat{x}_{MLE}) > R_m(\widehat{x}_{PI}) + c$, where $c > 0$ is a universal constant.*

We provide a sketch of the proof of Theorem 12 in Section 3.3.3. The complete proof of Theorem 12 is provided in Section B.1.3.

**Remark 1** *Theorem 12 holds true for a set of $p_L^*, p_H^*$, in the light gray region in Figure 3.1, enclosed by a red boundary. This set has a non-zero measure.*

Thus, there are many $p_L^*, p_H^*$ for which the risk of MLE is worse than the risk of plug-in by a constant. The proof of Remark 1 follows directly by substituting $p_L^*, p_H^*$ with values from the grey region in Figure 3.1 in the proof of Theorem 12 in (B.23).

**Positive result.** We now present a positive result for the plug-in estimator.

**Theorem 13** *For any $p_L^*, p_H^* \in [0.5, 1]^2$ such that $p_L^* \leq p_H^*$, there exists $m_0$ such that for all $m \geq m_0$, we have*

$$R_m(\widehat{x}_{PI}) \leq R_m(\widehat{x}_{MLE}) + \frac{c'}{\sqrt{m}}, \tag{3.12}$$

*where $c'$ is a universal constant. Thus, we have*

$$\liminf_{m \to \infty} \left[ R_m(\widehat{x}_{MLE}) - R_m(\widehat{x}_{PI}) \right] \geq 0. \tag{3.13}$$

We provide a sketch of the proof of Theorem 13 in Section 3.3.3(b). The complete proof of Theorem 13 is provided in Section B.1.4. Theorem 13 provides a positive result for the plug-in estimator by stating that asymptotically it is as good as the MLE or better, pointwise, for all $p_L^*, p_H^*$. Finally, by combining Theorem 12 and Theorem 13, we see that our constructed plug-in estimator deems the MLE asymptotically inadmissible for our setting.

### 3.3.3 Proof sketch for Theorem 12 and Theorem 13

Our proofs rely on the certain structure of both MLE and plug-in estimators. Specifically, we show that both algorithms operate by picking one of the decision rules defined in (3.8) (i.e.,

$t = 1$ for high-level expert-based or $t = 2$ for majority vote-based) and applying it to all the questions $i \in [m]$ to obtain $\widehat{x}_i$. The choice of the decision rule (3.8) is fully determined by the estimates of true probabilities $p_L^*, p_H^*$ obtained in the inner-workings of the estimators. With these preliminaries, we separately show negative and positive results.

**Negative result.** The crux of the proof is to find $p_L^*, p_H^*$ such that with high probability (i) MLE picks $t = 1$, (ii) the plug-in estimator picks $t = 2$, and (iii) the choice of $t = 2$ leads to a smaller risk than $t = 1$. We approach the proof in three steps and the key challenge is to get a handle on the sample-level behavior of estimators (steps 1 and 2).

Step 1. Starting from MLE, we use a subgaussian argument to show that in the region of interest, the value of the MLE objective (3.3) uniformly concentrates around its expectation. We then study the corresponding expected value to derive closed-form minimizers and describe the behavior of MLE in terms of the mapping between $\widehat{p}_L, \widehat{p}_H$ and the choice of decision rule (3.8) it makes.

Step 2. We show that Algorithm 5 obtains unbiased estimates of the true values $p_L^*, p_H^*$. We then establish convergence rates, thereby characterizing the choice of the decision rule made by the plug-in estimator.

Step 3. With these results, we carefully choose $p_L^*, p_H^*$ that results in requested conditions (i) — (iii), leading to a significant difference in the risks of the two estimators.

**Positive result.** To prove the positive result, we introduce an auxiliary estimator that picks the best decision rule (3.8) for each instance of $\vec{y}_L, \vec{y}_H$. First, we observe that this auxiliary estimator is as good as or better than both plug-in and MLE. Hence, to prove our result, it remains to show that the risk of plug-in asymptotically converges to that of the auxiliary estimator.

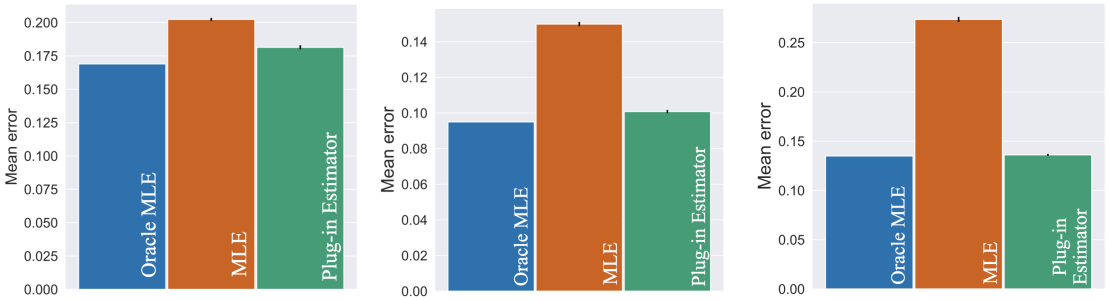
Step 1. We study the behavior of the auxiliary estimator which we illustrate in Figure 3.1. For all $p_L^*, p_H^*$ to the left of the red dashed line, with high probability, it chooses the high expertise-based decision rule ($t = 1$). To the right of the red dashed line, with high probability, it chooses the majority vote-based decision rule ($t = 2$).

Step 2. To conclude the proof, we establish a convergence result which confirms that with high probability plug-in picks the same decision rule ($t = 1$ or $t = 2$) as the auxiliary estimator.

## 3.4 Simulations

In this section, we simulate settings that relax assumptions in our theoretical analysis, investigating settings where the number of questions $m$ is finite, and under different combinations of evaluators' expertise. We find that our plug-in estimator continues to outperform or perform at least as well as the MLE.

We consider $m = 1000$ questions. In each of our experiments and for each estimator, we compute the average error over 100 trials, where in each trial we generate $\vec{x}^* \in \{0, 1\}^m$ uniformly at random and then generate $\vec{y}_L, \vec{y}_H$ based on (3.1). We consider three settings in our simulations. In Figure 3.2a each question is evaluated by 2 low-level experts and 1 high-level expert, same as the setting for our theoretical results in Section 3.3, with $p_L^* = 0.7, p_H^* = 0.8$. In Figure 3.2b




(a) 2L1H. $p_L^* = 0.7, p_H^* = 0.8$. (b) 3L1H. $p_L^* = 0.75, p_H^* = 0.85$. (c) 5L1H. $p_L^* = 0.72, p_H^* = 0.72$.

Figure 3.2: Mean 0-1 error of the three estimators described in this work: Oracle MLE, MLE, and plug-in estimator under three settings with $m = 1000$ questions, computed over 100 trials, with error bars to represent the standard error. Here, xLyH indicates that each question is evaluated by x low-level experts and y high-level experts.

each question is evaluated by 3 low-level experts and 1 high-level expert, with $p_L^* = 0.75, p_H^* = 0.85$. In Figure 3.2c each question is evaluated by 5 low-level experts and 1 high-level expert, with $p_L^* = 0.72, p_H^* = 0.72$. In each setting, we simulate the oracle MLE, MLE and plug-in estimator as described in (3.4), (3.6) and Algorithm 5 respectively. Note that for our simulations of the plug-in estimator, we use all the questions for estimation of $\widetilde{p}_L, \widetilde{p}_H$ defined in (3.9), (3.10). Observe in Figure 3.2 that in each setting, the mean 0-1 error of MLE is higher than that of our plug-in estimator. This suggests that our result on the asymptotic inadmissibility of MLE may be true more generally.

## 3.5 Discussion and open problems

In this work, we show that the widely used estimator MLE is asymptotically inadmissible in a simplified setting of the Dawid-Skene model with expertise information. For this we construct an alternative estimator, the plug-in estimator. In the future, it would be interesting to investigate the optimality of the plug-in estimator for this setting. In general, finding the optimal estimator for evaluation aggregation with expertise-level information is an open question of interest.

# Part II

# Experimental Approaches to Studying Human Judgment in Peer Review

# Chapter 4

# To ArXiv or not to ArXiv: A Study Quantifying Pros and Cons of Posting Preprints Online

Based on (Rastogi et al., 2022d):
Charvi Rastogi*, Ivan Stelmakh*, Xinwei Shen, Marina Meila, Federico Echenique, Shuchi Chawla, and Nihar B Shah. To Arxiv or not to Arxiv: A Study Quantifying Pros and Cons of Posting Preprints Online. Presented at Peer Review Congress 2022. Working paper on arXiv 2022.

## 4.1 Introduction

Across academic disciplines, peer review is used to decide on the outcome of manuscripts submitted for publication. Single-blind reviewing used to be the predominant method in peer review, where the authors' identities are revealed to the reviewer in the submitted paper. However, several studies have found various biases in single-blind reviewing. These include bias pertaining to affiliations or fame of authors (Blank, 1991a; Sun et al., 2021; Manzoor and Shah, 2021a), bias pertaining to gender of authors (Rossiter, 1993; Budden et al., 2008; Knobloch-Westerwick et al., 2013; Roberts and Verhoef, 2016), and others (Link, 1998; Snodgrass, 2006; Tomkins et al., 2017a). These works span several fields of science and study the effect of revealing the authors' identities to the reviewer through both observational studies and randomized control trials. These biases are further exacerbated due to the widespread prevalence of the Matthew effect—rich get richer and poor get poorer—in academia (Merton, 1968; Squazzoni and Claudio, 2012; Thorngate and Chowdhury, 2013). Biases based on authors' identities in peer review, coupled with the Matthew effect, can have far-reaching consequences on researchers' career trajectories.

As a result, many peer-review processes have moved to *double-blind* reviewing, where authors' names and other identifiers are removed from the submitted papers. Ideally, in a double-blind review process, neither the authors nor the reviewers of any papers are aware of each others' identity. However, a challenge for ensuring that reviews are truly double-blind is the exponen-

tial growth in the trend of posting papers online before review (Xie et al., 2021). Increasingly, authors post their preprints on online publishing websites such as arXiv and SSRN and publicize their work on social media platforms such as Twitter. The conventional publication route via peer review is infamously long and time-consuming. On the other hand, online preprint-publishing venues provide a platform for sharing research with the community usually without delays. Not only does this help science move ahead faster, but it also helps researchers avoid being "scooped". However, the increase in popularity of making papers publicly available—with author identities—before or during the review process, has led to the dilution of double-blinding in peer review. For instance, the American Economic Association, the flagship journal in economics, dropped double-blinding in their reviewing process citing its limited effectiveness in maintaining anonymity. The availability of preprints online presents a challenge in double-blind reviewing, which could lead to biased evaluations for papers based on their authors' identities, similar to single-blind reviewing.

This dilution has led several double-blind peer-review venues to debate whether authors should be allowed to post their submissions on the Internet, before or during the review process. For instance, top-tier machine learning conferences such as NeurIPS and ICML do not prohibit posting online. On the other hand, the Association of Computational Linguistics (ACL) recently introduced a policy for its conferences in which authors are prohibited from posting their papers on the Internet starting a month before the paper submission deadline till the end of the review process. The Conference on Computer Vision and Pattern Recognition (CVPR) has banned the advertisement of submissions on social media platforms for such a time period. Some venues are stricter, for example, the IEEE Communication Letters and IEEE International Conference on Computer Communications (INFOCOMM) disallows posting preprints to online publishing venues before acceptance.

Independently, authors who perceive they may be at a disadvantage in the review process if their identity is revealed face a dilemma regarding posting their work online. On one hand, if they post preprints online, they are at risk of de-anonymization in the review process, if their reviewer searches for their paper online. Past research suggests that if such de-anonymization happens, reviewers may get biased by the author identity, with the bias being especially harmful for authors from less prestigious organizations. On the other hand, if they choose to not post their papers online before the review process, they stand to lose out on viewership and publicity for their papers.

It is thus important to quantify the consequences of posting preprints online to (i) enable an evidence-based debate over conference policies, and (ii) help authors make informed decisions about posting preprints online. In our work, we conduct a large-scale survey-based study in conjunction with the review process of two top-tier publication venues in computer science that have double-blind reviewing: the 2021 International Conference on Machine Learning (ICML 2021) and the 2021 ACM Conference on Economics and Computation (EC 2021).[1] Specifically, we design and conduct experiments aimed at answering the following research questions:

(Q1) What fraction of reviewers, who had not seen the paper they were reviewing before the review process, deliberately search for the paper on the Internet during the review process?

[1]In Computer Science, conferences are typically the terminal publication venue and are typically ranked at par or higher than journals. Full papers are reviewed in CS conferences, and their publication has archival value.

(Q2) Given a preprint is posted online, what is the causal effect of the rank of the authors' affiliations on the visibility of a preprint to its target audience?

Our work can help inform authors' choices of posting preprints as well as enable evidence-based debates and decisions on conference policies. By addressing these research questions, we aim to measure some of the effects of posting preprints online, and help quantify their associated risks and benefits for authors with different affiliations. Combined, the two research questions tell authors from different institutions how much reach and visibility their preprint gets outside the review process, and at the same time, how likely is their paper to be searched online by reviewers during the review process. These takeaways will help authors trade off the two outcomes of posting preprints online, according to the amount of emphasis they want to place on the pro and the con. Our results also inform conference policies. Specifically, our results for Q1 suggest explicitly instructing reviewers to refrain from searching for their assigned papers online. Our results for Q2 help supplement debates in conferences about allowing preprints, that were previously primarily driven by opinions, with actual data. The data collected in Q2 shows authors preprint posting habits stratified by time, and also shows the amount of visibility the preprints get from their target audience.

Further, through our investigation of preprint posting behavior and the viewership received by these preprints, we provide data-driven evidence of trends therein. Specifically, our analysis informs on the fraction of papers made available online before review, how preprint-posting behaviors vary across authors from different affiliations, and the average rate of views obtained by a preprint from researchers in its target audience.

Finally, we list the main takeaways from our research study and analysis:

- In double-blind review processes, reviewers should be explicitly instructed to not search for their assigned papers on the Internet.
- For posted preprints online, authors from lower-ranked institutions enjoy only marginally lower visibility for their papers than authors from top-ranked institutions, implying that authors from lower-ranked institutions may expect almost similar benefits of posting preprints online.
- On average, authors posting preprints online receive viewership from 8% of relevant researchers in ICML and 20% in EC before the conclusion of the review process.
- Certain conferences ban posting preprints a month before the submission deadline. In ICML and EC (which did not have such a ban), more than 50% of preprints posted online where posted before the 1 month period, and these enjoyed a visibility of 8.6% and 18% respectively.
- Conference policies designed towards banning authors from publicising their work on social media or posting preprints before the review process may have only limited effectiveness in maintaining anonymity.

## 4.2 Related work

*Surveys of reviewers.* Several studies survey reviewers to obtain insights into reviewer perceptions and practices. Nobarany et al. (2016) surveyed reviewers in the field of human-computer interaction to gain a better understanding of their motivations for reviewing. They found that encouraging high-quality research, giving back to the research community, and finding out about

new research were the top general motivations for reviewing. Along similar lines, Tite and Schroter (2007) surveyed reviewers in biomedical journals to understand why peer reviewers decline to review. Among the respondents, they found the most important factor to be conflict with other workload.

(Resnik et al., 2008) conducted an anonymous survey of researchers at a government research institution concerning their perceptions about ethical problems with journal peer review. They found that the most common ethical problem experienced by the respondents was incompetent review. Additionally, $6.8\%$ respondents mentioned that a reviewer breached the confidentiality of their article without permission. This survey focused on the respondents' perception, and not on the actual frequency of breach of confidentiality. In another survey, by Martinson et al. (2005), $4.7\%$ authors self-reported publishing the same data or results in more than one publication. Fanelli (2009) provides a systematic review and meta analysis of surveys on scientific misconduct including falsification and fabrication of data and other questionable research practices.

Goues et al. (2018) surveyed reviewers in three double-blind conferences to investigate the effectiveness of anonymization of submitted papers. In their experiment, reviewers were asked to guess the authors of the papers assigned to them. Out of all reviews, 70%-86% of the reviews did not have any author guess. Here, absence of a guess could imply that the reviewer did not have a guess or they did not wish to answer the question. Among the reviews containing guesses, 72%-85% guessed at least one author correctly.

*Analyzing papers posted versus not posted on arXiv.* (Bharadhwaj et al., 2020) aim to analyse the risk of selective de-anonymization through an observational study based on open review data from the International Conference on Learning Representations (ICLR). The analysis quantifies the risk of de-anonymization by computing the correlation between papers' acceptance rates and their authors' reputations separately for papers posted and not posted online during the review process. This approach however is hindered by the confounder that the outcomes of the analysis may not necessarily be due to de-anonymization of papers posted on arXiv, but could be a result of higher quality papers being selectively posted on arXiv by famous authors. Moreover, it is not clear how the paper draws conclusions based on the analysis presented therein. Our supporting analysis overlaps with the investigation of (Bharadhwaj et al., 2020): we also investigate the correlation between papers' acceptance rates and their authors' associated ranking in order to support our main analysis and to account for confounding by selective posting by higher-ranked authors.

(Aman, 2014) investigate possible benefits of publishing preprints on arXiv in *Quantitative Biology*, wherein they measure and compare the citations received by papers posted on arXiv and those received by papers not posted on arXiv. A similar confounder arises here that a positive result could be a false alarm due to higher quality papers being selectively posted on arXiv by authors. Along similar lines, (Feldman et al., 2018) investigate the benefits of publishing preprints on arXiv selectively for papers that were accepted for publication in top-tier CS conferences. They find that one year after acceptance, papers that were published on arXiv before the review process have 65% more citations than papers posted on arXiv after acceptance. The true paper quality is a confounder in this analysis as well.

In our work, we quantify the risk of de-anonymization by directly studying reviewer behaviour regarding searching online for their assigned papers. We quantify the effects of pub-

lishing preprints online by measuring their visibility using a survey-based experiment querying reviewers whether they had seen a paper before.

*Studies on peer review in computer science.* Our study is conducted in two top-tier computer science conferences and contributes to a growing list of studies on peer review in computer science. Lawrence and Cortes (2014a); Beygelzimer et al. (2021) quantify the (in)consistencies of acceptance decisions on papers. Several papers (Madden and DeWitt, 2006; Tung, 2006; Tomkins et al., 2017a; Manzoor and Shah, 2021a) study biases due to single-blind reviewing. Shah et al. (2018a) study several aspects of the NeurIPS 2016 peer-review process. Stelmakh et al. (2021c) study biases arising if reviewers know that a paper was previously rejected. Stelmakh et al. (2021b) study a pipeline for getting new reviewers into the review pool. Stelmakh et al. (2020a) study herding in discussions. Stelmakh et al. (2023) study citation bias in peer review. A number of recent works (Charlin and Zemel, 2013; Stelmakh et al., 2021a; Kobren et al., 2019; Jecmen et al., 2020; Noothigattu et al., 2021) have designed algorithms that are used in the peer-review process of various computer science conferences. See Shah (2021) for an overview of such studies and computational tools to improve peer review.

## 4.3 Methods

We now outline the design of the experiment that we conducted to investigate the research questions in this work. First, in Section 4.3.1 we introduce the two computer science conferences ICML 2021 and EC 2021 that formed the venues for our investigation, and describe research questions Q1 and Q2 in the context of these two conferences. Second, in Section 4.3.2 we describe the experimental procedure. Finally, in Section 4.3.3 we provide the details of our analysis methods.

### 4.3.1 Preliminaries

**Experiment setting.** The study was conducted in the peer-review process of two conferences:

- **ICML 2021** International Conference on Machine Learning is a flagship machine learning conference. ICML is a large conference with 5361 submissions and 4699 reviewers in its 2021 edition.
- **EC 2021** ACM Conference on Economics and Computation is the top conference at the intersection of Computer Science and Economics. EC is a relatively smaller conference with 498 submissions and 190 reviewers in its 2021 edition.

Importantly, the peer-review process in both conferences, ICML and EC, is organized in a double-blind manner, defined as follows. In a **double-blind peer-review process**, the identity of all the authors is removed from the submitted papers. No part of the authors' identity, including their names, affiliations, and seniority, is available to the reviewers through the review process. At the same time, no part of the reviewers' identity is made available to the authors through the review process.

We now formally define some terminology used in the research questions Q1 and Q2. The first research question, Q1, focuses on the fraction of reviewers who deliberately search for their

assigned paper on the Internet. The second research question, Q2, focuses on the correlation between the visibility to a target audience of papers available on the Internet before the review process, and the rank of the authors' affiliations. In what follows, we explicitly define the terms used in Q2 in the context of our experiments—target audience, visibility, preprint, and rank associated with a paper.

**Paper's target audience.** For any paper, we define its target audience as members of the research community that share similar research interests as that of the paper. In each conference, a 'similarity score' is computed between each paper-reviewer pair, which is then used to assign papers to reviewers. We used the same similarity score to determine the target audience of a paper (among the set of reviewers in the conference). We provide more details in Appendix C.1.

**Paper's visibility.** We define the visibility of a paper to a member of its target audience as a binary variable which is 1 if that person has seen this paper outside of reviewing contexts, and 0 otherwise. Visibility, as defined here, includes reviewers becoming aware of a paper through preprint servers or other platforms such as social media, research seminars and workshops. On the other hand, visibility does *not* include reviewers finding a paper during the review process (e.g., visibility does not include a reviewer discovering an assigned paper by deliberate search or accidentally while searching for references).

**Preprint.** To study the visibility of papers released on the Internet before publication, we checked whether each of the papers submitted to the conference was available online. Specifically, for EC, we manually searched for all submitted papers to establish their presence online. On the other hand, for ICML, owing to its large size, we checked whether a submitted paper was available on arXiv (`arxiv.org`). ArXiv is the predominant platform for pre-prints in machine learning; hence we used availability on arXiv as a proxy indicator of a paper's availability on the Internet.

**Rank associated with a paper.** In this paper, the rank of an author's affiliation is a measure of author's prestige that, in turn, is transferred to the author's paper. We determine the rank of affiliations in ICML and EC based on widely available rankings of institutions in the respective research communities. Specifically, in ICML, we rank (with ties) each institution based on the number of papers published in the ICML conference in the preceding year (2020) with at least one author from that institution (Ivanov, 2020). On the other hand, since EC is at the intersection of two fields, economics and computation, we merge three rankings—the QS ranking for computer science (QS, 2021a), the QS ranking for economics and econometrics (QS, 2021b), and the CS ranking for economics and computation (CSRankings, 2021)—by taking the best available rank for each institution to get our ranking of institutions submitting to EC. By convention, better ranks, representing more renowned institutions, are represented by lower numbers; the top-ranked institution for each conference has rank 1. Finally, we define the rank of a paper as the rank of the best-ranked affiliation among the authors of that paper. Due to ties in rankings, we have 37 unique rank values across all the papers in ICML 2021, and 66 unique rank values across all the papers in EC 2021.

### 4.3.2 Experiment design

To address Q1 and Q2, we designed survey-based experiments for EC 2021 and ICML 2021, described next.

**Design for Q1.** To find the fraction of reviewers that deliberately search for their assigned paper on the Internet, we surveyed the reviewers. Importantly, as reviewers may not be comfortable answering questions about deliberately breaking the double-blindness of the review process, we designed the survey to be anonymous. We used the Condorcet Internet Voting Service (CIVS) (Myers, 2003), a widely used service to conduct secure and anonymous surveys. Further, we took some steps to prevent our survey from spurious responses (e.g., multiple responses from the same reviewer). For this, in EC, we generated a unique link for each reviewer that accepted only one response. In ICML we generated a link that allowed only one response per IP address and shared it with reviewers asking them to avoid sharing this link with anyone.[2] The survey form was sent out to the reviewers via CIVS after the initial reviews were submitted. In the e-mail, the reviewers were invited to participate in a one-question survey on the consequences of publishing preprints online. The survey form contained the following question:

> "During the review process, did you search for any of your assigned papers on the Internet?"

with two possible options: *Yes* and *No*. The respondents had to choose exactly one of the two options. To ensure that the survey focused on reviewers deliberately searching for their assigned papers, right after the question text, we provided additional text: "Accidental discovery of a paper on the Internet (e.g., through searching for related works) does not count as a positive case for this question. Answer *Yes* only if you tried to find an assigned paper itself on the Internet."

Following the conclusion of the survey, CIVS combined the individual responses, while maintaining anonymity, and provided the total number of *Yes* and *No* responses received.

**Design for Q2.** Recall that for Q2 we want to find the effect of a preprint's associated rank on its visibility to a target audience. Following the definitions provided in Section 4.3.1, we designed a survey-based experiment as follows. We conducted a survey to query reviewers about some papers for which they are considered a target audience. Specifically, we asked reviewers if they had seen these papers before outside of reviewing contexts. We provide more details about the survey, including the phrasing of the survey question, in Appendix C.1. We queried multiple reviewers about each paper, and depending on their response, we considered the corresponding visibility to be 1 if the reviewer said they had seen the paper before outside of reviewing contexts and 0 otherwise. We note that in ICML reviewers were queried about the papers they were assigned to review using the reviewer response form, in which a response to the question of visibility was required. Meanwhile, in EC, reviewers were queried about a set of papers that they were not assigned to review, using a separate optional survey form that was emailed to them by the program chairs after the rebuttal phase and before the announcement of paper decisions. The

[2]The difference in procedures between EC and ICML is due to a change in the CIVS policy that was implemented between the two surveys.

survey designed for Q2 had a response rate of $100\%$ in ICML, while EC had a response rate of $55.78\%$.

### 4.3.3 Analysis

We now describe the analysis for the data collected to address Q1 and Q2. Importantly, our analysis is the same for the data collected from ICML 2021 and EC 2021. For Q1, we directly report the numbers obtained from CIVS regarding the fraction of reviewers who searched for their assigned papers online in the respective conference. In the rest of this section, we describe our analysis for Q2, where we want to identify and analyse the effect of papers' ranking on their visibility. Recall that for Q2, we collected survey responses and data about papers submitted to ICML or EC that were posted online before the corresponding review process. Since the data is observational, and Q2 aims to identify the causal effect, we describe the causal model assumed in our setting and the interactions therein in Section 4.3.3 followed by the corresponding analysis procedure in Section 4.3.3 and additional supporting analysis in Section 4.3.3.

#### Graphical causal model for Q2

In Figure 4.1, we provide the graphical causal model assumed in our setting. To analyse the direct causal effect of a paper's associated ranking (denoted by **R**) on the visibility (denoted by **V**) enjoyed by the paper online from its target audience, we consider the interactions in the graphical causal model in Figure 4.1, which captures three intermediate factors: (1) whether the preprint was posted online, denoted by **P**, (2) the amount of time for which the preprint has been available online, denoted by **T**, and (3) the objective quality of the paper, denoted by **Q**. We now provide an explanation for this causal model.

First, the model captures mediation of effect of **R** on **V** by the amount of time for which the preprint has been available online, denoted by **T**. For a paper posted online, the amount of time for which it has been available on the Internet can affect the visibility of the paper. For instance, papers posted online well before the deadline may have higher visibility as compared to papers posted near the deadline. Moreover, the time of posting a paper online could vary across institutions ranked differently. Thus, amount of time elapsed since posting can be a mediating factor causing indirect effect from **R** to **V**. This is represented in Figure 4.1 by the causal pathway between **R** and **V** via **T**.

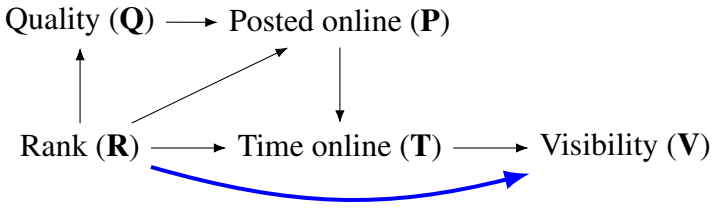



Figure 4.1: Graphical causal model illustrating the model assumed in our setting, to analyse the direct effect of "Rank" associated with a paper on the "Visibility" enjoyed by the paper as a preprint available on the internet.

Second, in the causal model, we consider the papers not posted online before the review process. For a preprint not posted online, we have $\mathbf{T} = 0$, and we do not observe its visibility. However, it is of interest that the choice of posting online before or after the review process could vary depending on both the quality of the paper as well as the rank of the authors' affiliations. For instance, authors from lower-ranked institutions may refrain from posting preprints online due to the risk of de-anonymization in the review process. Or, they may selectively post their high quality papers online. This is captured in our model by introducing the variable **P**. In Figure 4.1, this interaction is captured by the direct pathway from **R** to **P** and the pathway from **R** to **P** via **Q**.

Finally, we explain the missing edges in our causal model. In the model, we assume that there is no causal link between **Q** and **V**, this assumes that the initial viewership achieved by a paper does not get effected by its quality. Next, there is no link from **P** to **V** since the variable **T** captures all the information of **P**. Further, there is no causal link from **Q** to **T**. Here, we assume that **P** captures all the information of **Q** relevant to **T**. Lastly, there is no causal link from **Q** to **R**, since the effect of quality of published papers on the rank of the institution would be slow given the infrequent change in ranks.

To address the research question Q2, we want to identify the direct causal effect of a preprint's associated rank **R**, on its visibility **V**. Here the term "direct causal effect" is meant to quantify an influence that is not mediated by other variables in the model. Thus, to address Q2 we have to compute the effect of **R** on **V** through the direct link. In Q2, recall that we consider the papers that were posted online before the review process, which implies $\mathbf{P} = 1$. Since **P** is fixed, we do not consider effect from **R** to **V** via **P**. Consequently, to compute direct effect of **R** on **V**, we have to account for the indirect causal pathway from **R** to **V** via **T**, shown in Figure 4.1. To control for mediating by **T**, we provide a detailed description of our estimator in Section 4.3.3.

### Analysis procedure for Q2

We now describe our analysis to identify the direct causal effect of papers' associated ranks on their visibility, in detail. In this analysis, as in the survey for Q2, we consider all the papers submitted to the respective conferences that were posted online before the review process. In other words, the effect identified in this analysis gives the causal effect of papers' rank on visibility under the current preprint-posting habits of authors. In the following analysis procedure, we consider each response obtained in the survey for Q2 as one unit. Each response corresponds to a paper-reviewer pair, wherein the reviewer was queried about seeing the considered paper. In case of no response from reviewer, we do not consider the corresponding paper-reviewer pairs in our data. We thus have two variables associated to each response: the visibility of the paper to the reviewer (in $\{0, 1\}$), and the rank associated with the paper. Recall that we define the rank of a paper as the rank of the best-ranked affiliation associated with that paper.

We first describe the approach to control for mediation by "Time online" in the effect of "Rank" on "Visibility", as shown in Figure 4.1. There is ample variation in the time of posting papers online within the papers submitted to ICML and EC: some papers were posted right before the review process began while some papers were posted two years prior. To control for the causal effect of time of posting on visibility, we divide the responses into bins based on the number of days between the paper being posted online and the deadline for submitting responses

to the Q2 survey. Since similar conference deadlines arrive every three months roughly and the same conference appears every one year, we binned the responses accordingly into three bins. Specifically, if the number of days between the paper being posted online and the survey response is less than 90, it is assigned to the first bin, if the number of days is between 90 and 365, the response is assigned to the second bin, and otherwise, the response is assigned to the third bin. Following this binning, we assume that time of posting does not affect the visibility of papers within the same bin. Consequently, we compute the effect of papers' associated rank on their visibility separately within each bin and then combine them to get the overall effect in two steps:

**Step 1.** We compute the correlation coefficient between papers' visibility and associated rank within each bin. For this we use Kendall's Tau-b statistic, which is closely related to the widely used Kendall's Tau rank correlation coefficient (Kendall, 1938). Kendall's Tau statistic provides a measure of the strength and direction of association between two variables measured on an ordinal scale. It is a non-parametric measure that does not make any assumptions about the data. However, it does not account for ties and our data has a considerable number of ties, since visibility is a binary variable and the rankings used contain ties. Therefore, we use a variant of the statistic, Kendall's Tau-b statistic, that accounts for ties in the data.

Within each bin we consider all the responses obtained and their corresponding visibility and rank value, and compute Kendall's Tau-b correlation coefficient between visibility and rank. The procedure for computing Kendall's Tau-b correlation coefficient between two real-valued vectors (of the same length) is described in Appendix C.2.1. We now make a brief remark of a notational convention we use in this paper, in order to address ambiguity between the terminology "high-rank institutions" as well as "rank 1, 2,... institutions", both of which colloquially refers to better-rank institutions. It is intuitive to interpret a positive correlation between visibility and rank as the visibility increasing with an *improvement* in the rank. Consequently, we flip the sign of all correlation coefficients computed with respect to the rank variable.

**Step 2.** With the correlation computed within each bin, we compute the overall correlation using a sample-weighted average (Corey et al., 1998). Formally, let $N_1$, $N_2$ and $N_3$ denote the number of responses obtained in the first, second and third bin respectively. Denote Kendall's Tau-b correlation coefficients within the three bins as $\tau_1, \tau_2$ and $\tau_3$. Then the correlation $T$ between papers' visibility and rank over all the time bins is computed as

$$T = \frac{N_1\,\tau_1 + N_2\,\tau_2 + N_3\,\tau_3}{N_1 + N_2 + N_3}. \tag{4.1}$$

The statistic $T$ gives us the effect size for our research question Q2. Finally, to analyse the statistical significance of the effect, we conduct a permutation test, wherein we permute our data within each bin and recompute the test statistic $T$ to obtain a $p$-value for our test. We provide the complete algorithm for the permutation test in Appendix C.2.2.

### Additional analysis

In this section, we describe our analysis to further understand the relationships between authors' affiliations' ranks and their preprint posting behavior and the quality of the paper. Here we consider all papers submitted to the respective conference.

First we investigate whether the pool of papers posted online before the review process is significantly different, in terms of their rank profile, from the rest of the papers submitted to the conference. Specifically, we analyse the relationship between a binary value indicating whether a submitted paper was posted online before the Q2 survey, and the paper's associated rank. For this, we compute Kendall's Tau-b statistic between the two values for all papers submitted to the conference, and flip the sign of the statistic with respect to the rank variable. This will help us to understand the strength of the causal link from **R** to **P** in Figure 4.1.

Second, we investigate the causal pathway from **R** to **P** via **Q**. This will help us understand authors' preprint posting habits across different institutions, based on the quality of the preprint. It is of interest to examine whether there is a significant difference between the papers posted and not posted online before the review process, in terms of their quality and rank profile. A key bottleneck in this analysis is that we do not have a handle on the 'quality' of any paper. Thus as a proxy, following past work by Tomkins et al. (2017a), we measure the quality of a paper as a binary variable based on its final decision in the conference (`accept` or `reject`). We emphasize this is a significant caveat: the acceptance decisions may not be an objective indicator of the quality of the paper (Stelmakh et al., 2019) (for instance, the final decision could be affected by the ranking of the paper in case of de-anonymization of the paper, as discussed in Section 4.1), and furthermore, identifying an objective quality may not even be feasible (Rastogi et al., 2022b).

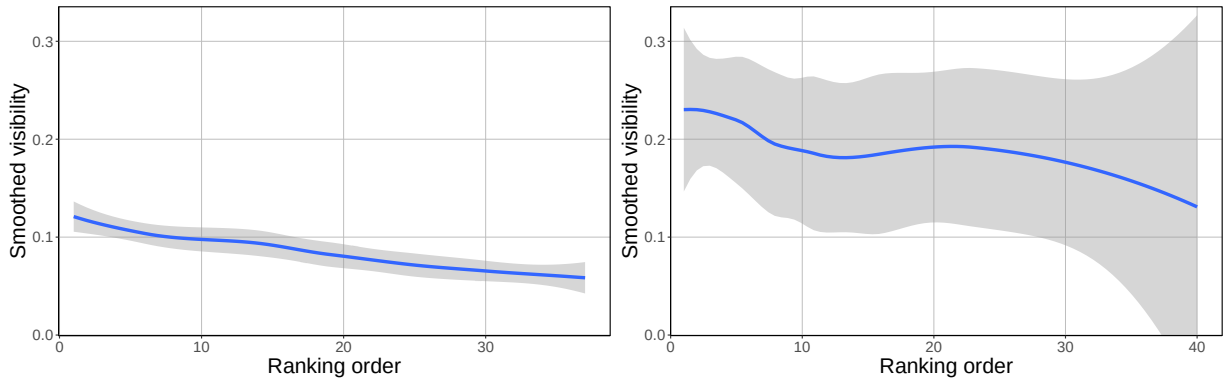
We conduct this analysis by computing three statistics. First, for all papers posted online before the Q2 survey, we compute Kendall's Tau-b statistic between their rank and their final decision. Second, for all papers *not* posted online, we compute Kendall's Tau-b statistic between their rank and their final decision. Third, for each unique rank value, for the corresponding papers with that rank, we compute the difference between the average acceptance rate for papers posted online and those not posted online. Then, we compute Kendall's Tau-b statistic between the rankings and the difference in acceptance rate. Finally, we flip the sign of all correlation coefficients computed with respect to the rank variable. Hence, a positive correlation would imply that the (difference in) acceptance rate increases as the rank improves.

## 4.4 Main results

We now discuss the results from the experiments conducted in ICML 2021 and EC 2021.

### 4.4.1 Q1 results

Table 4.1 provides the results of the survey for research question Q1. The percentage of reviewers that responded to the anonymous survey for Q1 is $16\%$ (753 out of 4699) in ICML and $51\%$ (97 out of 190) in EC. While the coverage of the pool of reviewers is small in ICML ($16\%$), the number of responses obtained is large ($753$). As shown in Table 4.1, the main observation is that, in both conferences, at least a third of the Q1 survey respondents self-report deliberately searching for their assigned paper on the Internet. There is substantial difference between ICML and EC in terms of the response rate as well as the fraction of *Yes* responses received, however, the current data cannot provide explanations for these differences.




(a) ICML 2021 (b) EC 2021

Figure 4.2: Using responses obtained in Q2 survey, we plot the papers' visibility against papers' associated rank with smoothing. On the x-axis, we order papers by their ranks (i.e., paper with the best rank gets order 1, paper with the second best rank gets order 2, and so on). The range of x-axis is given by the number of unique ranks in the visibility analysis, which may be smaller than the total number of unique ranks associated with the papers in the respective conferences. The x-axis range is 37 in Figure 4.2a and 40 in Figure 4.2b due to ties in rankings used. On the y-axis, smoothed visibility lies in $[0, 1]$. We use local linear regression for smoothing (Cleveland and Loader, 1996). The solid line gives the smoothed visibility, and the grey region around the line gives the 95% confidence interval.

### 4.4.2 Q2 results

We discuss the results of the survey conducted for Q2 in ICML 2021 and EC 2021. First we discuss the results of the main analysis described in Section 4.3.3. Then we discuss the results of the additional analysis described in Section 4.3.3. Finally, we discuss other general trends in posting preprints online, and the visibility gained thereof, in ICML 2021 and EC 2021.

**Analysis for Q2.** Table 4.2 depicts the results of the survey for research question Q2. We received 7594 responses and 449 responses for the survey for Q2 in ICML and EC respectively (Row 1). Recall that in the main analysis, we investigate the effect of papers' associated rank on their visibility, while controlling for mediation by time online, based on the causal model in Figure 4.1. As shown in Table 4.2 (Row 4), for papers submitted to the respective conference and posted online before the review process, we find a weak positive effect of the papers' associated

| | | EC 2021 | ICML 2021 |
|---|---|---|---|
| 1 | # Reviewers | 190 | 4699 |
| 2 | # Survey Respondents | 97 | 753 |
| 3 | # Survey respondents who said they searched for their assigned paper online | 41 | 269 |
| 4 | % Survey respondents who said they searched for their assigned paper online | 42% | 36% |

Table 4.1: Outcome of survey for research question Q1.

| | | EC 2021 | ICML 2021 |
|---|---|---|---|
| 1 | # Responses overall | 449 | 7594 |
| 2 | # papers in bins 1, 2, 3 | $63, 82, 38$ | $968, 820, 146$ |
| 3 | # Responses in bins 1, 2, 3 | $159, 233, 57$ | $3799, 3228, 567$ |
| 4 | Correlation between rank and visibility $[-1, 1]$ | $0.05$ $(p = 0.11)$ | $0.06$ $(p < 10^{-5})$ |
| 5 | Correlation between rank and visibility in bins 1, 2, 3 | $0.06, 0.04, 0.04$ | $0.04, 0.10, 0.03$ |
| 6 | p-value associated with correlations in Row 5 | $0.36, 0.46, 0.66$ | $0.004, < 10^{-5}, 0.19$ |
| 7 | % Visibility overall $[0 - 100]$ | $20.5\%$ (92 out of 449) | $8.36\%$ (635 out of 7594) |
| 8 | % Visibility for papers with top 10 ranks $[0 - 100]$ | $21.93\%$ (59 out of 269) | $10.91\%$ (253 out of 2319) |
| 9 | % Visibility for papers below top 10 ranks $[0 - 100]$ | $18.33\%$ (33 out of 180) | $7.24\%$ (382 out of 5275) |

Table 4.2: Outcome of main analysis for research question Q2. A positive correlation in Row 4 and Row 5 implies that the visibility increases as the rank of the paper improves. Recall that for ICML, we consider the set of responses obtained for submissions that were available as preprints on arXiv. There were 1934 such submissions.

rank on their visibility. Here the papers posted online represent the current preprint-posting habits of authors. The weak positive effect implies that the visibility increases slightly as the rank improves.

To provide some interpretation of the correlation coefficient values in Row 4, we compare the mean visibility within and without responses obtained for papers with at least one affiliation ranked 10 or better (Row 8 and 9). There are 10 and 23 institutions among the top-10 ranks in ICML and EC respectively. We see that there is more than 3 percentage points decrease in mean visibility across these two sets of responses in both ICML and EC. Figure 4.2 displays additional visualization that helps to interpret the strength of the correlation between papers' rank and visibility. The data suggests that top-ranked institutions enjoy higher visibility than lower-ranked institutions in both venues ICML 2021 and EC 2021.

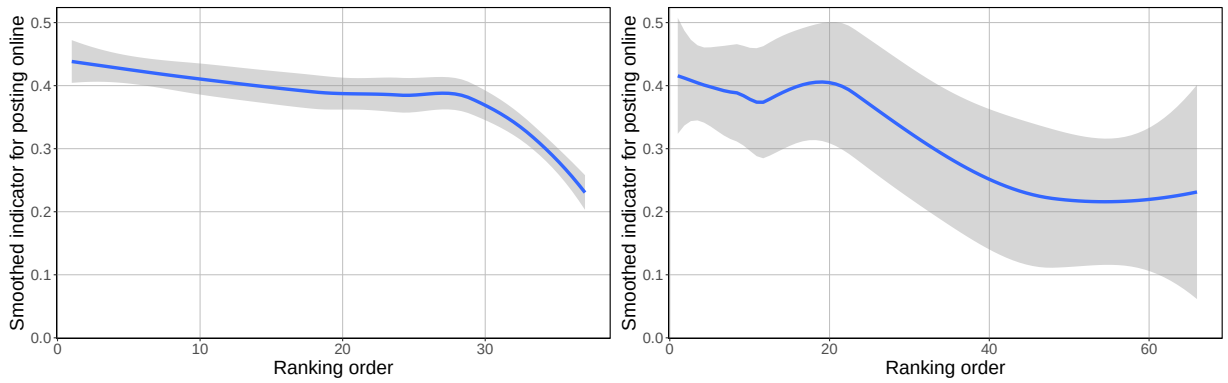
In summary, for papers available online before the review process, in ICML the analysis supports a weak but statistically significant effect of paper ranking on its visibility for preprints available online. In EC the effect size is comparable, but the effect does not reach statistical significance. Without further data, for EC the results are only suggestive.

Further, since the survey was optional in EC 2021, we analyse the difference between the responders and non-responders. Specifically, we looked at the distribution of the seniority of the reviewers that did and did not respond to the survey. We measure the seniority of the reviewers based on their reviewing roles, namely, (i) Junior PC member, (ii) Senior PC. member, (iii) Area Chair, valued according to increasing seniority. Based on this measurement system, we investigated the distribution of seniority across the groups of responders and non-responders. We find that there is no significant difference between the two groups, with mean seniority given by 1.64 in the non-responders' group and 1.61 in the responders' group. The difference in the mean (0.03) is much smaller than the standard deviation in each group, which is 0.64 and 0.62 respectively.

**Additional analysis.** We provide the results for the supporting analysis described in Section 4.3.3 in Table 4.3. Recall that, in this analysis, we consider all papers submitted to the

| | | EC 2021 | ICML 2021 |
|---|---|---|---|
| 1 | # PAPERS | 498 | 5361 |
| 2 | # PAPERS POSTED ONLINE BEFORE THE END OF REVIEW PROCESS | 183 | 1934 |
| 3 | CORRELATION BETWEEN PAPERS' RANK AND WHETHER THEY WERE POSTED ONLINE $[-1, 1]$ | 0.09 | 0.12 |
| 4 | CORRELATION FOR PAPERS POSTED ONLINE BETWEEN THEIR RANK AND DECISION $[-1, 1]$ | 0.03 | 0.11 |
| 5 | CORRELATION FOR PAPERS NOT POSTED ONLINE BETWEEN THEIR RANK AND DECISION $[-1, 1]$ | 0.13 | 0.16 |
| 6 | CORRELATION BETWEEN RANKING AND CORRESPONDING DIFFERENCE, BETWEEN PAPERS POSTED AND NOT POSTED ONLINE, IN MEAN ACCEPTANCE RATE $[-1, 1]$ | 0.12 | 0.01 |

Table 4.3: Outcome of supporting analysis for research question Q2. A positive correlation in rows 3, 4, 5 and 6 implies that the value of the variable considered increases as the rank of the paper improves. For instance, in row 3, the rate of posting online increases as the rank improves.




(a) ICML 2021

(b) EC 2021

Figure 4.3: For papers submitted to the respective conferences, we plot the indicator for paper being posted online before the end of the review process against papers' associated rank, with smoothing. On the x-axis, we have the ranking order as described in Figure 4.2. On the y-axis, smoothed indicator for posting online lies in $[0, 1]$. We use locally estimated smoothing to get the smoothed indicator for posting online across ranks, shown by the solid line, and a 95% confidence interval, shown by the grey region.

respective conferences. There were a total of 5361 and 498 papers submitted to ICML 2021 and EC 2021 respectively.

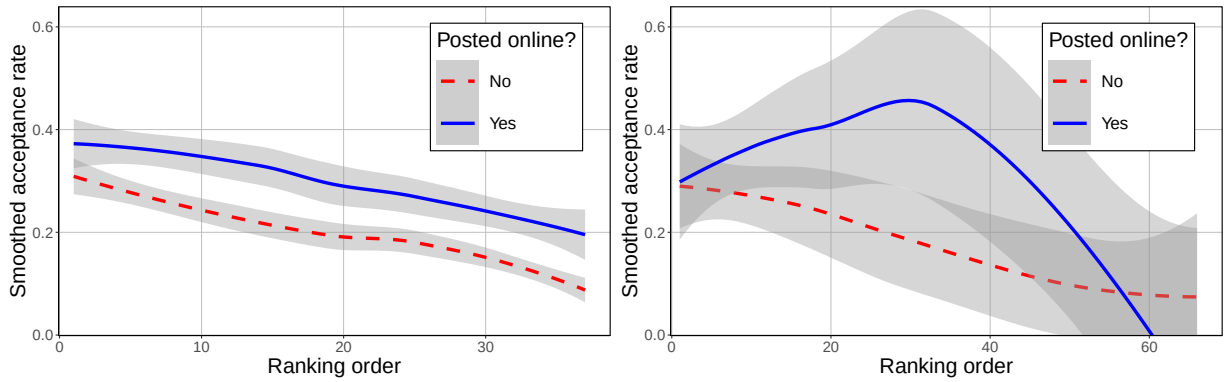
Among all the papers submitted, we observe that there is a statistically significant weak positive correlation (Kendall's Tau-b) between paper's rank and whether it was posted online before the review process in both ICML and EC of 0.12 ($p < 10^{-5}$) and 0.09 ($p = 0.01$) respectively (Row 3). This implies that the authors from higher-ranked institutions are more likely to post their papers online before the review process. Further, it provides evidence for a causal link between **R** and **P** in Figure 4.1. We provide visualization to interpret the correlation between ranking and posting behaviour in Figure 4.3.

To understand if there is significant difference in the quality of papers uploaded online by authors from institutions with different ranks, we compare the final decision of the pool of papers posted online before the review process and the pool of papers that was not, across ranks. Now, for the pool of papers posted online, we see that Kendall's Tau-b correlation between papers' rank and final decision is 0.11 ($p < 10^{-5}$) in ICML and 0.03 ($p = 0.58$) in EC (Row 4). Recall that a positive correlation implies that the acceptance rate increases as the rank improves. For the pool of papers *not* posted online, we see that Kendall's Tau-b correlation between papers' rank and final decision, 0.16 ($p < 10^{-5}$) in ICML and 0.13 ($p = 0.006$) in EC (Row 5). Lastly, the correlation between the rank values and the corresponding difference (between papers posted and not posted online) in mean acceptance rates is 0.01 ($p = 0.92$) in ICML and 0.12 ($p = 0.18$) in EC (Row 6).

To interpret these values, we provide visualization of the variation of mean acceptance rate as rank varies for the two pools of papers in Figure 4.4. Recall that in our assumed causal model in Figure 4.1, there is a causal link from **R** to **P** via **Q**. In ICML (in Figure 4.4a), we see that there is a clear trend for authors from all institutions posting higher quality papers online as preprints, which implies that quality of the paper mediates the effect of author's rank on their posting decision. Further, we see that the authors from higher-ranked institutes submit papers with a higher acceptance rate, providing evidence for the causal link from **R** to **Q**. Meanwhile, in EC, we see a similar trend for papers not posted online. However, the plot for papers posted online occupies a large region for its 95% confidence intervals, and is thereby difficult to draw insights from.

**Trends in posting preprints online and visibility.** We now discuss the preprint-posting habits of authors, and the viewership received by them from relevant researchers in the community. In Table 4.3, we see that there were a total of 5361 and 498 papers submitted to ICML 2021 and EC 2021 respectively, out of which 1934 and 183 were posted online before the end of the review process respectively (Row 1 and 2). Thus, we see that more than a third of the papers submitted were available online. Further, based on our binning rule based on time of posting described in Section 4.3.3, we see more papers in bin 1 and bin 2, compared to bin 3 (refer Table 4.2). This suggests that majority of preprints were posted online within one year of the review process (Row 2).

Based on results from Q2 survey, we learn that the mean visibility in ICML 2021 is 8.36% and that in EC 2021 is 20.5% (refer Table 4.2, Row 7). This provides an estimate of the fraction of relevant researchers in the community viewing preprints available online. Further we note that




(a) ICML 2021

(b) EC 2021

Figure 4.4: For papers submitted to the respective conferences and (not) posted online before the review process, we plot the papers' final decision against papers' associated rank, with smoothing. On the x-axis, we have the ranking order as described in Figure 4.2. On the y-axis, smoothed acceptance rate lies in $[0, 1]$. We use locally estimated smoothing to get the smoothed acceptance rate across ranks, shown by the lines, and a 95% confidence interval, shown by the grey region. Note that in Figure 4.4b, the number of papers corresponding to the ranks on the right of the plot is very small.

the mean visibility in ICML is considerably smaller than that in EC. This may be attributed to the following reason: The research community in EC is smaller and more tight-knit, meaning that there is higher overlap in research interests within the members of the community (reviewers). On the other hand, ICML is a large publication venue with a more diverse and spread-out research community.

## 4.5 Discussion

To improve peer review and scientific publishing in a principled manner, it is important to understand the quantitative effects of the policies in place, and design policies in turn based on these quantitative measurements.

We find that more than a third of survey respondents self-report deliberately searching for their assigned papers online, thereby weakening the effectiveness of author anonymization in double-blind peer review. This finding has important implications for authors who perceive they may be at a disadvantage in the review process if their identity is revealed, in terms of their decision to post preprints online.

Further, the observed value of fraction of reviewers that searched for their assigned paper online in Table 4.1 might be an underestimate due to two reasons: (i) Reviewers who deliberately broke the double-blindedness of the review process may be more reluctant to respond to our survey for Q1. (ii) As we saw in Section 4.4.2, roughly $8\%$ of reviewers in ICML 2021 had already seen their assigned paper before the review process began (Table 4.2 row 5). If these reviewers were not already familiar with their assigned paper, they may have searched for them online during the review process.

Based on the analysis of Q2, we find evidence to support a weak effect of authors' affiliations' ranking on the visibility of their papers posted online before the review process. These papers represent the current preprint-posting habits of authors. Thus, authors from lower-ranked institutions get slightly less viewership for their preprints posted online compared to their counterparts from top-ranked institutions. For Q2, the effect size is statistically significant in ICML, but not in EC. A possible explanation for the difference is in the method of assigning rankings to institutions, described in Section 4.3.1. For ICML, the rankings used are directly related to past representation of the institutions at ICML (Ivanov, 2020). In EC, we used popular rankings of institutions such as QS rankings and CS rankings. In this regard, we observe that there is no clear single objective measure for ranking institutions in a research area. This leads to many ranking lists that may not agree with each other. Our analysis also suffers from this limitation. Another possible explanation for the difference is the small sample size in EC.

Next, while we try to carefully account for mediation by time of posting in our analysis for Q2, our study remains dependent on observational data. Thus, the usual caveat of unaccounted for confounding factors applies to our work. For instance, the topic of research may be a confounding factor in the effect of papers' rank on visibility: If authors from better-ranked affiliations work more on cutting-edge topics compared to others, then their papers would be read more widely. This could potentially increase the observed effect.

**Policy implications.** Double-blind venues now adopt various policies for authors regarding posting or advertising their work online before and during the review process. A notable example is a recent policy change by the Association for Computational Linguistics in their conference review process, which includes multiple conferences: ACL, NAACL (North American Chapter of the ACL) and EMNLP (Empirical Methods in Natural Language Processing). ACL introduced an anonymity period for authors, starting a month before the paper submission deadline and extending till the end of the review process. According to their policy, within the anonymity period authors are not allowed to post or discuss their submitted work anywhere on the Internet (or make updates to existing preprints online). In this manner, the conference aims to limit the de-anonymization of papers from posting preprints online. A similar policy change has been instituted by the CVPR computer vision conference. Furthermore, we provide some quantitative insights on this front using the data we collected from the Q2 survey in ICML 2021 and EC 2021. There were $918$ (out of $5361$ submitted) and $74$ (out of $498$ submitted) papers posted online *during* the one month period right before the submission deadline in ICML and EC respectively. These papers enjoyed a visibility of $8.11\%$ ($292$ out of $3600$) and $23.81\%$ ($45$ out of $189$) respectively. Meanwhile, there were $1016$ (out of $5361$) and $109$ (out of $498$) papers posted online *prior to* the one month period right before the submission deadline in ICML and EC, and these papers enjoyed a visibility of $8.59\%$ ($343$ out of $3994$) and $18.08\%$ ($47$ out of $260$) respectively. Moreover, the combination of the result of the Q1 survey and the finding that a majority of the papers posted online were posted before the anonymity period suggests that conference policies designed towards banning authors from publicising their work on social media or from posting preprints online in a specific period of time before the review process may not be effective in maintaining double anonymity, since reviewers may still find these papers online if available. These measurements may help inform subsequent policy decisions.

While our work finds dilution of anonymization in double-blind reviewing, any prohibition on posting preprints online comes with its own downsides. For instance, consider fields such as Economics where journal publication is the norm, which can often imply several years of lag between paper submission and publication. Double-blind venues must grapple with the associated trade-offs, and we conclude with a couple of suggestions for a better trade-off. First, many conferences, including but not limited to EC 2021 and ICML 2021, do not have clearly stated policies for reviewers regarding searching for papers online, and can clearly state as well as communicate these policies to the reviewers. Second, venues may consider policies requiring authors to use a different title and reword the abstract during the review process as compared to the versions available online, which may reduce the chances of reviewers discovering the paper or at least introduce some ambiguity if a reviewer discovers (a different version of) the paper online.

# Chapter 5

# Cite-seeing and Reviewing: A Study on Citation Bias in Peer Review

Based on (Stelmakh et al., 2023):
Ivan Stelmakh*, Charvi Rastogi*, Ryan Liu, Shuchi Chawla, Federico Echenique, and Nihar B. Shah. Cite-seeing and reviewing: A study on citation bias in peer review. Plos one 18, no. 7 (2023): e0283980.

## 5.1 Introduction

Peer review is the backbone of academia. Across many fields of science, peer review is used to decide on the outcome of manuscripts submitted for publication. Moreover, funding bodies in different countries employ peer review to distribute multi-billion dollar budgets through grants and awards. Given that stakes in peer review are high, it is extremely important to ensure that evaluations made in the review process are not biased by factors extraneous to the submission quality. This requirement is especially important in presence of the Matthew effect ("rich get richer") in academia (Merton, 1968): an advantage a researcher receives by publishing even a single work in a prestigious venue or getting a research grant early may have far-reaching consequences on their career trajectory.

The key decision-makers in peer review are fellow researchers with expertise in the research areas of the submissions they review. Exploiting this feature, many anecdotes suggest that adding citations to the works of potential reviewers is an effective (albeit unethical) way of increasing the chances that a submission will be accepted:

> *We all know of cases where including citations to journal editors or potential reviewers [. . . ] will help a paper's chances of being accepted for publication in a specific journal.* Kostoff, 1998

The rationale behind this advice is that citations are one of the key success metrics of a researcher. A Google Scholar profile, for example, summarizes a researcher's output in the total number of citations to their work and several other citation-based metrics (h-index, i10-index). Citations are also a key factor in hiring and promotion decisions (Hirsch, 2005; Fuller, 2018).

Thus, reviewers may consciously or subconsciously, be more lenient towards submissions that cite their work.

Existing research documents that the suggestion to pad reference lists with unnecessary citations is taken seriously by some authors. For example, a survey conducted by Fong and Wilhite (2017) indicates that over 40% of authors across several disciplines would preemptively add non-critical citations to their journal submission when the journal has a reputation of asking for such citations. The same observation applies to grant proposals, with 15% of authors willing to add citations even when "*those citations are of marginal import to their proposal*". This behavior is conjectured to be caused by authors' awareness of reviewers' bias in favour of their work if the review is cited in it.

In the present work, we investigate whether reviewers are actually biased by citations. We study whether a citation to a reviewer's past work induces a bias in the reviewer's evaluation. Note that citation of a reviewer's past work may impact the reviewer's evaluation of a submission in two ways: first, it can impact the scientific merit of the submission, thereby causing a *genuine change* in evaluation; second, it can induce an *undesirable bias* in evaluation that goes beyond the genuine change. We use the term "*citation bias*" to refer to the second mechanism. Formally, the research question we investigate in this work is as follows:

> **Research Question:** Does the citation of a reviewer's work in a submission *cause* the reviewer to be positively *biased* towards the submission, that is, *cause* a shift in reviewer's evaluation that goes beyond the genuine change in the submission's scientific merit?

Citation bias, if present, contributes to the unfairness of academia by making peer-review decisions dependent on factors irrelevant to the submission quality. It is therefore important for stakeholders to understand if citation bias is present, and whether it has a strong impact on the peer-review process.

Two studies have previously investigated citation bias in peer review (Sugimoto and Cronin, 2013; Beverly and Allman, 2013). These studies analyze journal and conference review data and report mixed evidence of citation bias in reviewers' recommendations. However, their analysis does not account for confounding factors such as paper quality (stronger papers may have longer bibliographies) or reviewer expertise (cited reviewers may have higher expertise). Thus, past works do not decisively answer the question of the presence of citation bias. A more detailed discussion of these and other relevant works is provided in Section 5.2.

**Our contributions** In this work, we investigate the research question in a large-scale study conducted in conjunction with the review process of two flagship publication venues: 2020 International Conference on Machine Learning (ICML 2021) and 2021 ACM Conference on Economics and Computation (EC 2021). We execute a carefully designed observational analysis that accounts for various confounding factors such as paper quality and reviewer expertise. Overall, our analysis identifies citation bias in both venues we consider: by adding a citation of a reviewer, a submission can increase the expectation of the score given by the reviewer by 0.23 (on a 5-point scale) in EC 2021 and by up to 0.42 (on a 6-point scale) in ICML 2021. For better interpretation of the effect size, we note that on average, a one-point increase in a score given by a single reviewer improves the position of a submission by 11%.

Finally, it is important to note that the bias we investigate is not necessarily an indicator of unethical behavior on the part of authors or reviewers. Citation bias may be present even when authors do not try to deliberately cite potential reviewers, and when reviewers do not consciously attempt to champion papers that cite their past work. Crucially, even subconscious citation bias is problematic for fairness reasons. Thus, understanding whether the bias is present is important for improving peer-review practices and policies.

## 5.2 Related work

In this section, we discuss relevant past studies. We begin with an overview of cases, anecdotes, and surveys that document practices of coercive citations. We then discuss two works that perform statistical testing for citation bias in peer review. Finally, we conclude with a list of works that test for other biases in the peer-review process. We refer the reader to Shah (2021) for a broader overview of literature on peer review.

**Coercive Citations** Fong and Wilhite (2017) study the practice of coercion by journal editors who, in order to increase the prestige of the journal, request authors to cite works previously published in the journal. They conduct a survey which reveals that 14.1% of approximately 12,000 respondents from different research areas have experienced coercion by journal editors. Resnik et al. (2008) notes that coercion happens not only at the journal level, but also at the level of individual reviewers. Specifically, 22.7% of 220 researchers from the National Institute of Environmental Health Sciences who participated in the survey reported that they have received reviews requesting them to include unnecessary references to publications authored by the reviewer.

In addition to the surveys, several works document examples of extreme cases of coercion. COPE (2018) reports that a handling editor of an unnamed journal asked authors to add citations to their work more than 50 times, three times more often than they asked authors to add citations of papers they did not co-author. The editorial team of the journal did not find a convincing scientific justification of such requests and the handling editor resigned from their duties. A similar case (Van Noorden, 2020) was uncovered in the Journal of Theoretical Biology where an editor was asking authors to add 35 citations on average to each submitted paper, and 90% of these requests were to cite papers authored by that editor. This behavior of the editor traced back to decades before being uncovered, and furthermore, authors had complied to such requests with an "apparently amazing frequency".

Given such evidence of coercion, it is not surprising that authors are willing to preemptively inflate bibliographies of their submissions either because journals they submit to have a reputation for coercion (Fong and Wilhite, 2017) or because they hope to bias reviewers and increase the chances of the submission (Meyer et al., 2009). That said, observe that evidence we discussed above is based on either case studies or surveys of authors' perceptions. We note, however, that (i) authors in peer review usually do not know identities of reviewers, and hence may incorrectly perceive a reviewer's request to cite someone else's work as that of coercion to cite the reviewer's own work; and (ii) case studies describe only the most extreme cases and are not necessarily representative of the average practice. Thus, the aforementioned findings could overestimate the

prevalence of coercion and do not necessarily imply that a submission can significantly boost its acceptance chances by strategically citing potential reviewers.

**Citation Bias** We now describe several other works that investigate the presence of citation bias in peer review. First, Sugimoto and Cronin (2013) analyze the editorial data of the Journal of the American Society of Information Science and Technology and study the relationship between the reviewers' recommendations and the presence of references to reviewers' works in submissions. They find mixed evidence of citation bias: a statistically significant difference between *accept* and *reject* recommendations (cited reviewers are more likely to recommend acceptance than reviewers who are not cited) becomes insignificant if they additionally consider *minor/major revision* decisions. We note, however, that the analysis of Sugimoto and Cronin (2013) computes correlations and does not control for confounding factors associated with paper quality and reviewer identity (see discussion of potential confounding factors in Section 5.3.2). Thus, that analysis does not allow to test for the causal effect.

Another work (Beverly and Allman, 2013) performs data analysis of the 2010 edition of ACM Internet Measurement Conference and reports findings that suggest the presence of citation bias. As a first step of the analysis, they compute a correlation between acceptance decisions and the number of references to papers authored by 2010 TPC (technical program committee) members. For long papers, the correlation is $0.21$ ($n = 109,\ p < 0.03$) and for short papers the correlation is $0.15$ ($n = 102,\ p = 0.12$). Similar to the analysis of Sugimoto and Cronin (2013), these correlations do not establish causal relationship due to unaccounted confounding factors such as paper quality (papers relevant to the venue may be more likely to cite members of TPC than out-of-scope papers).

To mitigate confounding factors, Beverly and Allman (2013) perform a second step of the analysis. They recompute correlations but now use members of the *2009* TPC who are not in 2010 TPC as a target set of reviewers. Reviewers from this target set did not impact the decisions of the 2010 submissions and hence this second set of correlations can serve as an unbiased contrast. For long papers, the contrast correlation is $0.13$ ($n = 109,\ p = 0.19$) and for short papers, the contrast correlation is $-0.04$ ($n = 102,\ p = 0.66$). While the *difference* between actual and contrast correlations hints at the presence of citation bias, we note that (i) the sample size of the study may not be sufficient to draw statistically significant conclusions (the paper does not formally test for significance of the difference); (ii) the overlap between 2010 and 2009 committees is itself a confounding factor — members in the overlap may be statistically different (e.g., more senior) from those present in only one of the two committees.

**Testing for other Biases in Peer Review** A long line of literature (Mahoney, 1977; Blank, 1991b; Lee, 2015; Tomkins et al., 2017b; Stelmakh et al., 2020b; 2021d; Manzoor and Shah, 2021b, and many others) scrutinizes the peer-review process for various biases. These works investigate gender, fame, positive-outcome, and many other biases that can hurt the quality of the peer-review process. Our work continues this line by investigating citation bias.

## 5.3 Methods

In this section, we outline the design of the experiment we conduct to investigate the research question of this paper. Section 5.3.1 introduces the venues in which our experiment was executed and discusses details of the experimental procedure. Section 5.3.2 describes our approach to the data analysis. In what follows, for a given pair of submission $\mathcal{S}$ and reviewer $\mathcal{R}$, we say that reviewer $\mathcal{R}$ is CITED in $\mathcal{S}$ if one or more of their past papers are cited in the submission. Otherwise, reviewer $\mathcal{R}$ is UNCITED.

### 5.3.1 Experimental procedure

We begin with a discussion the details of the experiment we conduct in this work.

**Experimental Setting** The experiment was conducted in the peer-review process of two conferences:[1]

- **ICML 2020** International Conference on Machine Learning is a flagship machine learning conference that receives thousands of paper submissions and manages a pool of thousands of reviewers.
- **EC 2021** ACM Conference on Economics and Computation is the top conference at the intersection of computer science and economics. The conference is smaller than ICML and handles several hundred submissions and reviewers.

Rows 1 and 2 of Table 5.1 display information about the size of the conferences used in the experiment.

The peer-review process in both venues is organized in a double-blind manner (neither authors nor reviewers know the identity of each other) and follows the conventional pipeline that we now outline. After the submission deadline, reviewers indicate their preference in reviewing the submissions. Additionally, program chairs compute measures of similarity between submissions and reviewers which are based on (i) overlap of research topics of submissions/reviewers (both conferences) and (ii) semantic overlap (Charlin and Zemel, 2013) between texts of submissions' and reviewers' past papers (ICML). All this information is then used to assign submissions to reviewers who have several weeks to independently write initial reviews. The initial reviews are then released to authors who have several days to respond to these reviews. Finally, reviewers together with more senior members of the program committee engage in the discussions and make final decisions, accepting about 20% of submissions to the conference.

**Intervention** As we do not have control over bibliographies of submissions, we cannot intervene on the citation relationship between submissions and reviewers. We rely instead on the analysis of observational data. As we explain in Section 5.3.2, for our analysis to have a strong detection power, it is important to assign a large number of submissions to both CITED and UNCITED reviewers. In ICML, this requirement is naturally satisfied due to its large sample size,

[1] In computer science, conferences are considered to be a final publication venue for research and are typically ranked higher than journals. Full papers are reviewed in CS conferences, and their publication has archival value

| | ICML 2020 | EC 2021 |
|---|---|---|
| # REVIEWERS | 3,064 | 154 |
| # SUBMISSIONS | 4,991 | 496 |
| NUMBER OF SUBMISSIONS WITH AT LEAST ONE CITED REVIEWER | 1,513 | 287 |
| FRACTION OF SUBMISSIONS WITH AT LEAST ONE CITED REVIEWER | 30% | 58% |

Table 5.1: Statistics on the venues where the experiment is executed. The number of reviewers includes all regular reviewers. The number of submissions includes all submissions that were not withdrawn from the conference by the end of the initial review period.

and we assign submissions to reviewers using the PR4A assignment algorithm (Stelmakh et al., 2018) that does not specifically account for the citation relationship in the assignment.

The number of papers submitted to the EC 2021 conference is much smaller. Thus, we tweak the assignment process in a manner that gets us a larger sample size while retaining the conventional measures of the assignment quality. To explain our intervention, we note that, conventionally, the quality of the assignment in the EC conference is defined in terms of satisfaction of reviewers' preferences in reviewing the submissions, and research topic similarity. However, in addition to being useful for the sample size of our analysis, citation relationship has also been found (Beygelzimer et al., 2020) to be a good indicator for the review quality and was used in other studies to measure similarity between submissions and reviewers (Li, 2017). With this motivation, in EC, we use an adaptation of the popular TMPS assignment algorithm (Charlin and Zemel, 2013) with the objective consisting of two parts: (i) conventional measure of the assignment quality and (ii) the number of CITED reviewers in the assignment. We then introduce a parameter that can be tuned to balance the two parts of the objective and find an assignment that has a large number of CITED reviewers while not compromising the conventional metrics of assignment quality. Additionally, the results of the automated assignment are validated by senior members of the program committee who can alter the assignment if some (submission, reviewer) pairs are found unsuitable. As a result, Table 5.1 demonstrates that in the final assignment more than half of the EC 2021 submissions were assigned to at least one CITED reviewer.

### 5.3.2 Analysis

As we mentioned in the previous section, in this work we rely on analysis of observational data. Specifically, our analysis operates with *initial reviews* that are written independently before author feedback and discussion stages (see description of the review process in Section 5.3.1). As is always the case for observational studies, our data can be affected by various confounding factors. Thus, we design our analysis procedure to alleviate the impact of several plausible confounders. In Section 5.3.2 we provide a list of relevant confounding factors that we identify and in Section 5.3.2 we explain how our analysis procedure accounts for them.

**Confounding Factors**

We begin by listing the confounding factors that we account for in our analysis. For ease of exposition, we provide our description in the context of a naïve approach to the analysis and illustrate how each of the confounding factors can lead to false conclusions of this naïve analysis. The naïve analysis we consider compares the mean of numeric evaluations given by all CITED reviewers to the mean of numeric evaluations given by all UNCITED reviewers and declares bias if these means are found to be unequal for a given significance level. With these preliminaries, we now introduce the confounding factors.

C1 **Genuinely Missing Citations** Each reviewer is an expert in their own work. Hence, it is easy for reviewers to spot a genuinely missing citation to their own work, such as missing comparison to their own work that has a significant overlap with the submission. At the same time, reviewers may not be as familiar with the papers of other researchers and their evaluations may not reflect the presence of genuinely missing citations to these papers. Therefore, the scores given by UNCITED reviewers could be lower than scores of CITED reviewers even in absence of citation bias, which would result in the naïve test declaring the effect when the effect is absent.

C2 **Paper Quality** As shown in Table 5.1, not all papers submitted to the EC and ICML conferences were assigned to CITED reviewers. Thus, reviews by CITED and UNCITED reviewers were written for intersecting, but not identical, sets of papers. Among papers that were not assigned to CITED reviewers there could be papers which are clearly out of the conference's scope. Thus, even in absence of citation bias, there could be a difference in evaluations of CITED and UNCITED reviewers caused by the difference in relevance between two groups of papers the corresponding reviews were written for. The naïve test, however, will raise a false alarm and declare the bias even though the bias is absent.

C3 **Reviewer Expertise** The reviewer and submission pools of the ICML and EC conferences are diverse and submissions are assigned to reviewers of different expertise in reviewing them. The expertise of a reviewer can be simultaneously related to the citation relationship (expert reviewers may be more likely to be CITED) and to the stringency of evaluations (expert reviewers may be more lenient or strict). Thus, the naïve analysis that ignores this confounding factor is in danger of raising a false alarm or missing the effect when it is present.

C4 **Reviewer Preference** As we mentioned in Section 5.3.2, the assignment of submissions to reviewers is, in part, based on reviewers' preferences. Thus, (dis-)satisfaction of the preference may impact reviewers' evaluations — for example, reviewers may be more lenient towards their top choice submissions than to submissions they do not want to review. Since citation relationships are not guaranteed to be independent of the reviewers' preferences, the naïve analysis can be impacted by this confounding factor.

C5 **Reviewer Seniority** Some past work has observed that junior reviewers may sometime be stricter than their senior colleagues (Toor, 2009; Tomiyama, 2007, note that some other works such as Shah et al. 2018a; Stelmakh et al. 2020c do not observe this effect). If senior reviewers are more likely to be CITED (e.g., because they have more papers published) and simultaneously are more lenient, the seniority-related confounding factor can bias the

naïve analysis.

### Analysis Procedure

Having introduced the confounding factors, we now discuss the analysis procedure that alleviates the impact of these confounding factors and enables us to investigate the research question. Specifically, our analysis consists of two steps: data filtering and inference. For ease of exposition, we first describe the inference step and then the filtering step.

**Inference** The key quantities of our inference procedure are overall scores (`score`) given in initial reviews and binary indicators of the citation relationship (`citation`). Overall scores represent recommendations given by reviewers and play a key role in the decision-making process. Thus, a causal connection between `citation` and `score` is a strong indicator of citation bias in peer review.

To test for causality, our inference procedure accounts for confounders C2–C5 (confounder C1 is accounted for in the filtering step). To account for these confounders, for each (submission, reviewer) pair we introduce several characteristics which we now describe, ignoring non-critical differences between EC and ICML. Appendix D.1 provides more details on how these characteristics are defined in the two individual venues.

- `quality` Relative quality of the submission for the publication venue considered. We note that this quantity can be different from the quality of the submission independent of the publication venue. The value of relative quality of a submission is, of course, unknown and below we explain how we accommodate this variable in our analysis to account for confounder C2.
- `expertise` Measure of expertise of the reviewer in reviewing the submission. In both ICML and EC, reviewers were asked to self-evaluate their ex post expertise in reviewing the assigned submissions. In ICML, two additional expertise-related measures were obtained: (i) ex post self-evaluation of the reviewer's confidence; (ii) an overlap between the text of each submitted paper and each reviewer's past papers (Charlin and Zemel, 2013). We use all these variables to control for confounding factor C3.
- `preference` Preference of the reviewer in reviewing the submission. As we mentioned in Section 5.3.1, both ICML and EC conferences elicited reviewers' preferences in reviewing the submissions. We use these quantities to alleviate confounder C4.
- `seniority` An indicator of reviewers' seniority. For the purpose of decision-making, both conferences categorized reviewers into two groups. While specific categorization criteria were different across conferences, conceptually, groups were chosen such that one contained more senior reviewers than the other. We use this categorization to account for the seniority confounding factor C5.

Having introduced the characteristics we use to control for confounding factors C2–C5, we now discuss the two approaches we take in our analysis.

*Parametric approach (EC and ICML)* First, following past observational studies of the peer-review procedure (Tomkins et al., 2017b; Teplitskiy et al., 2019) we assume a linear approxima-

tion of the score given by a reviewer to a submission:[2]

$$\texttt{score} \sim \alpha_0 + \alpha_1 \cdot \texttt{quality} + \alpha_2 \cdot \texttt{expertise} + \alpha_3 \cdot \texttt{preference} + \alpha_4 \cdot \texttt{seniority} + \alpha^* \cdot \texttt{citati} \tag{5.1}$$

Under this assumption, the test for citation bias as formulated in our research question reduces to the test for significance of $\alpha^*$ coefficient. However, we cannot directly fit the data we have into the model as the values of quality are not readily available. Past work (Tomkins et al., 2017b) uses a heuristic to estimate the values of paper quality, however, this approach was demonstrated (Stelmakh et al., 2019) to be unable to reliably control the false alarm probability.

To avoid the necessity to estimate quality, we restrict the set of papers used in the analysis to papers that were assigned to at least one CITED reviewer and at least one UNCITED reviewer. At the cost of the reduction of the sample size, we are now able to take a difference between scores given by CITED and UNCITED reviewers *to the same submission* and eliminate quality from the model (5.1). As a result, we apply a standard tools for the linear regression inference to test for the significance of the target coefficient $\alpha^*$. We refer the reader to Appendix D.2 for more details on the parametric approach.

*Non-parametric approach (ICML)* While the parametric approach we introduced above is conventionally used in observational studies of peer review and offers strong detection power even for small sample sizes, it relies on strong modeling assumptions that are not guaranteed to hold in the peer-review setting (Stelmakh et al., 2019). To overcome these limitations, we also execute an alternative non-parametric analysis that we now introduce.

The idea of the non-parametric analysis is to match (submission, reviewer) pairs on the values of all four characteristics (quality, expertise, preference, and seniority) while requiring that matched pairs have different values of citation. As in the parametric analysis, we overcome the absence of access to the values of quality by matching (submission, reviewer) pairs *within* each submission. In this way, we ensure that matched (submission, reviewer) pairs have the same values of confounding factors C2–C5. We then compare mean scores given by CITED and UNCITED reviewers, focusing on the restricted set of matched (submission, reviewer) pairs, and declare the presence of citation bias if the difference is statistically significant. Again, more details on the non-parametric analysis are given in Appendix D.3.

**Data Filtering** The purpose of the data-filtering procedure is twofold: first, we deal with missing values; second, we take steps to alleviate the genuinely missing citations confounding factor C1.

Missing Values As mentioned above, for a submission to qualify for our analysis, it should be assigned to at least one CITED reviewer and at least one UNCITED reviewer. In ICML data, 578 out of 3,335 (submission, reviewer) pairs that qualify for the analysis have values of certain variables corresponding to expertise and preference missing. The missingness of these values is due to various technicalities: reviewers not having profiles in the system used to compute textual

[2]The notation $y \sim \alpha_0 + \sum_i^n \alpha_i x_i$ means that given values of $\{x_i\}_{i=1}^n$, dependent variable $y$ is distributed as a Gaussian random variable with mean $\alpha_0 + \sum_i^n \alpha_i x_i$ and variance $\sigma^2$. The values of $\{\alpha_i\}_{i=0}^n$ and $\sigma$ are unknown and need to be estimated from data. Variance $\sigma^2$ is independent of $\{x_i\}_{i=1}^n$.

overlap or not reporting preferences in reviewing submissions. Thus, given a large size of the ICML data, we remove such (submission, reviewer) pairs from the analysis.

In the EC conference, the only source of missing data is reviewers not entering their `preference` in reviewing some submissions. Out of 849 (submission, reviewer) pairs that qualify for the analysis, 154 have reviewer's `preference` missing. Due to a limited sample size, we do not remove such (submission, reviewer) pairs from the analysis and instead accommodate missing preferences in our parametric model (5.1) (see Appendix D.1 and Appendix D.2.1 for details).

Genuinely Missing Citation Another purpose of the filtering procedure is to account for the genuinely missing citations confounder C1. The idea of this confounder is that even in absence of citation bias, reviewers may legitimately decrease the score of a submission because citations to some of their own past papers are missing. The frequency of such legitimate decreases in scores may be different between CITED and UNCITED reviewers, resulting in a confounding factor. To alleviate this issue, we aim at identifying submissions with genuinely missing citations of reviewers' past papers and removing them from the analysis. More formally, to account for confounder C1, we introduce the following exclusion criteria:

> **Exclusion Criteria:** The reviewer flags a missing citation of *their own* work and this complaint is valid for reducing the score of the submission

The specific implementation of a procedure to identify submissions satisfying this criteria is different between ICML and EC conferences and we introduce it separately.

*EC* In the EC conference, we added a question to the reviewer form that asked reviewers to report if a submission has some important relevant work missing from the bibliography. Among 849 (submission, reviewer) pairs that qualify for inclusion to our inference procedure, 110 had a corresponding flag raised in the review. For these 110 pairs, authors of the present paper (CR, FE) manually analyzed the submissions and the reviews, identifying submissions that satisfy the exclusion criteria.[3]

Overall, among the 110 target pairs, only three requests to add citations were found to satisfy the exclusion criteria. All (submission, reviewer) pairs for these three submissions were removed from the analysis, ensuring that reviews written in the remaining (submission, reviewer) pairs are not susceptible to confounding factor C1.

*ICML* In ICML, the reviewer form did not have a flag for missing citations. Hence, to fully alleviate the genuinely missing citations confounding factor, we would need to analyze all the 1,617 (submission, UNCITED reviewer)[4] pairs qualifying for the inference step to identify those satisfying the aforementioned exclusion criteria.

We begin from the analysis of (submission, UNCITED reviewer) pairs that qualify for our non-parametric analysis. There are 63 such pairs and analysis conducted by an author of the present paper (IS – a workflow chair of ICML 2021) found that three of them satisfy the exclusion criteria. The corresponding three submissions were removed from our non-parametric analysis.

[3]CR conducted an initial, basic screening and all cases that required a judgement were resolved by FE – a program chair of the EC 2021 conference.

[4]Note that, in principle, CITED reviewers may also legitimately decrease the score because the submission misses some of their past papers. However, this reduction in score would lead us to an underestimation of the effect (or, under the absence of citation bias, to the counterintuitive direction of the effect) and hence we tolerate it.

The fraction of (submission, UNCITED reviewer) pairs with a genuinely missing citation of the reviewer's past paper in ICML is estimated to be 5% ($\frac{3}{63}$). As this number is relatively small, the impact of this confounding factor is limited. In absence of the missing citation flag in the reviewer form, we decided not to account for this confounding factor in the parametric analysis of the ICML data. Thus, we urge the reader to be aware of this confounding factor when interpreting the results of the parametric inference.

## 5.4 Results

As described in Section 5.3, we study our research question using data from two venues (ICML 2021 and EC 2021) and applying two types of analysis (parametric for both venues and non-parametric for ICML). While the analysis is conducted on observational data, we intervene in the assignment stage of the EC conference in order to increase the sample size of our study. Table 5.2 displays the key details of our analysis (first group of rows) and numbers of unique submissions, reviewers, and (submission, reviewer) pairs involved in our analysis (second group of rows).

The dependent variable in our analysis is the score given by a reviewer to a submission in the initial independent review. Therefore, the key quantity of our analysis (test statistic) is an expected increase in the reviewer's score due to citation bias. In EC, reviewers scored submissions on a 5-point Likert item while in ICML a 6-point Likert item was used. Thus, the test statistic can take values from -4 to 4 in EC and from -5 to 5 in ICML. Positive values of the test statistic indicate the positive direction of the bias and the absolute value of the test statistic captures the magnitude of the effect.

The third group of rows in Table 5.2 summarizes the key results of our study. Overall, we observe that after accounting for confounding factors, all three analyses detect statistically significant differences between the behavior of CITED and UNCITED reviewers (see the last row of the table for $P$ values). Thus, we conclude that citation bias is present in both ICML 2021 and EC 2021 venues.

We note that conclusions of the parametric analysis are contingent upon satisfaction of the linear model assumptions and it is a priori unclear if these assumptions are satisfied to a reasonable extent. To investigate potential violation of these assumptions, in Appendix D.4 we conduct analysis of model residuals. This analysis suggests that linear models provide a reasonable fit to both ICML and EC data, thereby supporting the conclusions we make in the main analysis. Additionally, we note that our non-parametric analysis makes less restrictive assumptions on reviewers' decision-making but still arrives at the same conclusion.

**Effect Size** To interpret the effect size, we note that the value of the test statistic captures the magnitude of the effect. In EC 2021, a citation of reviewer's paper would result in an expected increase of 0.23 in the score given by the reviewer. Similarly, in ICML 2021 the corresponding increase would be 0.16 according to the parametric analysis and 0.42 according to the non-parametric analysis. Confidence intervals for all three point estimates (rescaled to 5-point scale) overlap, suggesting that the magnitude of the effect is similar in both conferences. Overall, the

| | | EC 2021 | ICML 2021 | ICML 2021 |
|---|---|---|---|---|
| Analysis | | Parametric | Parametric | Non-parametric |
| Intervention | | Assignment Stage | No | No |
| Missing Values | | Incorporated | Removed | Removed |
| Genuinely Missing Citations | | Removed | Unaccounted ($\sim 5\%$) | Removed |
| Sample Size | # Submissions (S) | 283 | $1,031$ | 60 |
| | # Reviewers (R) | 152 | $1,565$ | 115 |
| | # (S, R)-pairs | 840 | $2,757$ | 120 |
| Test Statistic | | 0.23 on 5-point scale | 0.16 on 6-point scale | 0.42 on 6-point scale |
| Test Statistic (95% CI) | | $[0.06, 0.40]$ | $[0.05, 0.27]$ | $[0.10, 0.73]$ |
| P value | | 0.009 | 0.004 | 0.02 |

Table 5.2: Results of the analysis. The results suggest that citation bias is present in both EC 2021 and ICML 2021 conferences. $P$ values and confidence intervals for parametric analysis are computed under the standard assumptions of linear regression. For non-parametric analysis, $P$ value is computed using permutation test and the confidence interval is bootstrapped. All P values are two-sided.

values of the test statistic demonstrate that a citation of a reviewer results in a considerable improvement in the expected score given by the reviewer. In other words, there is a non-trivial probability of reviewer increasing their score by one or more points when cited. With this motivation, to provide another interpretation of the effect size, we now estimate the effect of a one-point increase in a score by a single reviewer on the outcome of the submission.

Specifically, we first rank all submissions by the mean score given in the initial reviews, breaking ties uniformly at random. For each submission, we then compute the improvement of its position in the ranking if one of the reviewers increases their score by one point. Finally, we compute the mean improvement over all submissions to arrive at the average improvement. As a result, on average, in both conferences a one-point increase in a score given by a single reviewer improves the position of a submission in a score-based ordering by $11\%$. Thus, having a reviewer who is cited in a submission can have a non-trivial implication on the acceptance chances of the submission.

As a note of caution, in actual conferences decisions are based not only on scores, but also on the textual content of reviews, author feedback, discussions between reviewers, and other factors. We use the readily available score-based measure to obtain a rough interpretation of the effect size, but we encourage the reader to keep these qualifications in mind when interpreting the result.

## 5.5 Discussion

We have reported the results of two observational studies of citation bias conducted in flagship machine learning (ICML 2021) and algorithmic economics (EC 2021) conferences. To test for

the causal effect, we carefully account for various confounding factors and rely on two different analysis approaches. Overall, the results suggest that citation bias is present in peer-review processes of both venues. A considerable effect size of citation bias can (i) create a strong incentive for authors to add superfluous citations of potential reviewers, and (ii) result in unfairness of final decisions. Thus, the finding of this work may be informative for conference chairs and journal editors who may need to develop measures to counteract citation bias in peer review. In this section, we provide additional discussion of several aspects of our work.

**Observational Caveat** First, we want to underscore that, while we try to carefully account for various confounding factors and our analysis employs different techniques, our study remains observational. Thus, the usual caveat of unaccounted confounding factors applies to our work. The main assumption that we implicitly make in this work is that the list of confounding factors C1–C5 is (i) exclusive and (ii) can be adequately modelled with the variables we have access to. As an example of a violation of these assumptions, consider that CITED reviewers could possess some characteristic that is not captured by `expertise`, `preference`, and `seniority` and makes them more lenient towards the submission they review. In this case, the effect we find in this work would not be a causation. That said, we note that to account for confounding factors, we used all the information that is routinely used in many publication venues to describe the competence of a reviewer in judging the quality of a submission.

**Genuinely Present Citations** In this work, we aim at decoupling citation bias from a genuine change in the scientific merit of a submission due to additional citation. For this, we account for the genuinely missing citations confounding factor C1 that manifests in reviewers *genuinely decreasing* their scores when their relevant past paper is not cited in the submission.

In principle, we could also consider a symmetric *genuinely present citations* confounding factor that manifests in reviewers *genuinely increasing* their scores when their relevant past work is adequately incorporated in the submission. However, while symmetric, these two confounding factors are different in an important aspect. When citation of a relevant work is missing from the submission, an author of that relevant work is in a better position to identify this issue than other reviewers and this asymmetry of information can bias the analysis. However, when citation of a relevant work is present in the paper, all reviewers observe this signal as they read the paper. The presence of the shared source of information reduces the aforementioned asymmetry across reviewers and alleviates the corresponding bias.

With this motivation, in this work we do not specifically account for the genuinely present citations confounding factor, but we urge the reader to be aware of our choice when interpreting the results of our study.

**Fidelity of Citation Relationship** Our analysis pertains to citation relationships between the submitted papers and the reviewers. In order to ensure that reviewers who are cited in the submissions are identified correctly, we developed a custom parsing tool. Our tool uses PDF text mining to (i) extract authors of papers cited in a submission (all common citation formats are accommodated) and (ii) match these authors against members of the reviewer pool. We note that there are several potential caveats associated with this procedure which we now discuss:

- **False Positives.** First, reviewers' names are not unique identifiers. Hence, if the name of a reviewer is present in the reference list of a submission, we cannot guarantee that it is the specific ICML or EC reviewer cited in the submission. To reduce the number of false positives, we took the following approach. First, for each reviewer we defined a *key*:

  $$\{\text{LAST NAME}\}\_\{\text{FIRST LETTER OF FIRST NAME}\}$$

  Second, we considered all reviewers whose *key* is not unique in the conference they review for. For these reviewers, we manually verified all assigned (submission, reviewer) pairs in which reviewers were found to be CITED by our automated mechanism. We found that about 50% of more than 250 such cases were false positives and corrected these mistakes, ensuring that the analysis data did not have false positives among reviewers with non-unique values of their *key*.

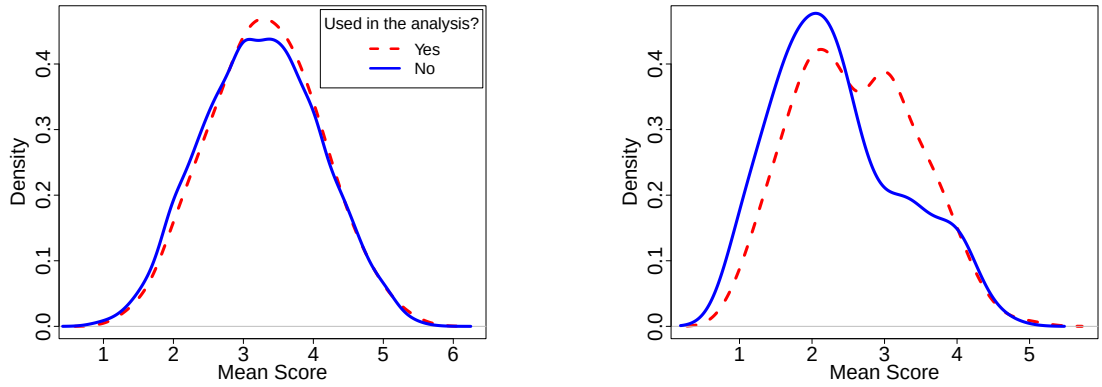
  Third, for the remaining reviewers (those whose *key* was unique in the reviewer pool), we sampled 50 (submission, CITED reviewer) pairs from the actual assignment and manually verified the citation relationship. Among 50 target pairs, we identified only 1 false positive case and arrived at the estimate of $2\%$ of false positives in our analysis.
- **False Negatives.** In addition to false positives, we could fail to identify some of the CITED reviewers. To estimate the fraction of false negatives, we sampled $50$ (submission, UNCITED reviewer) pairs from the actual assignment and manually verified the citation relationship. Among these 50 pairs we did not find any false negative case, which suggests that the number of false negatives is very small.

Finally, we note that both false positives and false negatives affect the power, but not the false alarm probability of our analysis. Thus, the conclusions of our analysis are stable with respect to imperfections of the procedure used to establish the citation relationship.

**Generalizability of the Results** As discussed in Section 5.3, in this experiment we used submissions that were assigned to at least one CITED and one UNCITED reviewers and satisfied other inclusion criteria (see Data Filtering in Section 5.3.2). We now perform some additional analysis to juxtapose the population of submissions involved in our analysis to the general population of submissions.

Figure 5.1 compares distributions of mean overall scores given in initial reviews between submissions that satisfied the inclusion criteria of our analysis and submissions that were excluded from consideration. First, observe that Figure 5.1a suggests that in terms of the overall scores, ICML submissions used in the analysis are representative of the general ICML submission pool. However, in EC (Figure 5.1b), the submissions that were used in the analysis received on average higher scores than those that were excluded. Thus, we urge the reader to keep in mind that our analysis of the EC data may not be applicable to submissions that received lower scores.

One potential reason of the difference in generalizability of our EC and ICML analyses is the intervention we took in EC to increase the sample size. Indeed, by maximizing the number of submissions that are assigned to at least one CITED reviewer we could include most of the submissions that are *relevant* to the venue in the analysis, which results in the observed difference in Figure 5.1b.




(a) ICML 2021 (b) EC 2021

Figure 5.1: Distribution of mean overall scores given in initial reviews with a breakdown by whether a submission is used in our analysis or not.

**Spurious correlations induced by reviewer identity** In peer review, each reviewer is assigned to several papers. Our analysis implicitly assumes that conditioned on `quality`, `expertise`, `preference`, `seniority` characteristics, and on the value of the `citation` indicator, evaluations of different submissions made by the same reviewer are independent. Strictly speaking, this assumption may be violated by correlations introduced by various characteristics of the reviewer identity (e.g., some reviewers may be lenient while others are harsh). To fully alleviate this concern, we would need to significantly reduce the sample size by requiring that each reviewer contributes to at most one (submission, reviewer) pair used in the analysis. Given otherwise limited sample size, this requirement would put a significant strain on our testing procedure. Thus, in this work we follow previous empirical studies of the peer-review procedure (Lawrence and Cortes, 2014b; Tomkins et al., 2017b; Shah et al., 2018a) and tolerate such potential spurious correlations. We note that simulations performed by Stelmakh et al. (2019) demonstrate that unless reviewers contribute to dozens of data points, the impact of such spurious correlations is limited. In our analysis, reviewers on average contributed to 1.8 (submission, reviewer) pairs in ICML, and to 5.5 (submission, reviewer) pairs in EC, thereby limiting the impact of this caveat.

**Counteracting the Effect** Our analysis raises an open question of counteracting the effect of citation bias in peer review. For example, one way to account for the bias is to increase the awareness about the bias among members of the program committee and add citation indicators to the list of information available to decision-makers. Another option is to try to equalize the number of CITED reviewers assigned to submissions. Given that Beygelzimer et al. (2020) found citation indicator to be a good proxy towards the quality of the review, enforcing the balance across submissions could be beneficial for the overall fairness of the process. More work may be needed to find more principled solutions against citation bias in peer review.

# Chapter 6

# How do Authors' Perceptions of their Papers Compare with Co-authors' Perceptions and Peer-review Decisions?

Based on (Rastogi et al., 2022b):
Charvi Rastogi, Ivan Stelmakh, Alina Beygelzimer, Yann N Dauphin, Percy Liang, Jennifer Wortman Vaughan, Zhenyu Xue, Hal Daum é III, Emma Pierson, and Nihar B Shah. How do Authors' Perceptions of their Papers Compare with Co-authors' Perceptions and Peer-review Decisions? Working paper on arXiv. 2022.

## 6.1 Introduction

Peer review is used widely in scientific research for quality control as well as selecting 'interesting' research. However, a number of studies have documented low agreement among reviewers (Cicchetti, 1991; Bornmann et al., 2010; Obrecht et al., 2007; Fogelholm et al., 2012; Lawrence and Cortes, 2014a; Pier et al., 2017; Cortes and Lawrence, 2021), and researchers often lament various problems with peer review (Akst, 2010; McCook, 2006; Rennie, 2016). On the other hand, surveys of researchers about their general perception of peer review reveal that researchers across various scientific disciplines consider peer review to be important, yet in need of improvements (Ware, 2016; Taylor and Francis group, 2015; Ware, 2008; Mulligan et al., 2013; Nicholas et al., 2015). But how do author perceptions on their submitted papers match up with outcomes of the peer-review process? We investigate this question in this work.

We conduct a survey-based experiment in the Neural Information Processing Systems (NeurIPS) 2021 conference, which is a top-tier conference in the field of machine learning.[1] The conference had over 9,000 papers submitted by over 23,000 authors. The conference traditionally has accepted 20–25% of the submitted papers, and in 2021 the acceptance rate was 25.8%.

[1]Readers outside computer science unfamiliar with its publishing culture may note that in computer science, conferences review full papers and are commonly the terminal venue of publication of papers.

We design and execute a survey to understand authors' perceptions about their submitted papers as well as their perceptions of the peer-review process in relation to their papers. In particular, we ask three questions:

- It is well known that the peer-review process (at the NeurIPS conference) has a low acceptance rate and a high amount of disagreement between reviewers (Lawrence and Cortes, 2014a; Cortes and Lawrence, 2021; Beygelzimer et al., 2023). Do authors take this into account when setting their expectations from the peer review process? Specifically, we aim to understand the calibration of authors with respect to the review process, by asking them to predict the probability of acceptance of their submitted paper(s).
- Motivated by authors often lamenting that their paper that they thought was best was rejected and the one they thought had lower scientific merit was accepted, we aim to quantify the discrepancy between the author's and the reviewers' relative perceptions of papers by asking authors to rank their papers in terms of their perceived scientific contribution and comparing this against acceptance decisions.
- Finally, while the two questions above measured the perception before the review process, we also measure the perception after they see the reviews, by asking authors whether the review process changed their perception of their own paper.

We then analyze how author perceptions align with the outcomes of the peer-review process and the perceptions of co-authors. The results of this work are useful to set expectations from the peer-review process, identify its fundamental limitations, and help guide the policies that the community implements as well as future research on improving peer review.

The rest of the paper is organized as follows. Section 6.2 discusses related work. In Section 6.3, we present details of the questions asked to the participants (authors of submitted papers). We provide basic statistics of the responses in Section 6.4 and our main analysis of the responses in Section 6.5. We conclude with a discussion in Section 6.6.

## 6.2 Related work

There are a number of papers in the literature that conduct surveys of authors. Frachtenberg and Koster (2020) survey authors of *accepted* papers from 56 computer science conferences. The survey was conducted after these papers were published. Questions pertained to the paper's history (amount of time needed to write it; resubmission history) and their opinions about the conference's rebuttal process. The respondents were also asked whether they found the reviews helpful in improving their paper. A total of 34.1% of the respondents said they were 'very helpful,' 52.7% said they were 'somewhat helpful,' and 13.2% said they were 'not at all' helpful. Similar surveys asking authors whether peer review helped improve their paper are also conducted in other fields (Weller, 1996; Mulligan et al., 2013; Patat et al., 2019). It is important to note that this question is different from our third question which asks whether their own perception of the quality of their own paper changed after the review process. Our question pertains to the same (version of the) paper but perception before and after the reviews; on the other hand, their question pertains to two different versions of the paper (initial submission and after reviewers' suggestions) and whether there was an improvement across the versions. They also find that for

these questions, responses from different authors to the same paper were usually very similar.

Philipps (2021) surveys authors of research proposals on their perception of random allocation of grant funding. They do find support for such randomized decisions, which have now also been implemented (Heyard et al., 2022). Resnik et al. (2008); Fanelli (2009) conduct or analyze surveys of authors for breach of ethics. While computer science was not their focus, within computer science as well, there have been discoveries of breach of ethics in the peer-review process (Littman, 2021; Vijaykumar, 2020; Jecmen et al., 2020; Wu et al., 2021; Jecmen et al., 2022).

Several other surveys (Ware, 2016; Taylor and Francis group, 2015; Ware, 2008; Mulligan et al., 2013; Nicholas et al., 2015) find a strong support for peer review among researchers. They also find that researchers see a need to improve peer review.

The work of Gardner et al. (2012) is closest to ours. They conduct a survey in the Australasian Association for Engineering Education (AAEE) annual conference 2010 and 2011, comprising a total of 70 papers and 140 reviews. The survey asked authors to rate their own papers and also to rate reviews. Their survey received responses from 23 authors in 2010 and from 37 authors in the 2011 edition. They found that overall 75% of authors rated their paper higher than the average of the reviewers' ratings for their paper. Furthermore, their survey found that the academic rank of the respondent was not correlated with the accuracy of the respondent's prediction of the reviews.

(Anderson, 2009) offers a somewhat tongue in cheek commentary pertaining to authors' perceptions: *"if authors systematically overestimate the quality of their own work, then any paper rejected near the threshold is likely to appear (to the author) to be better than a large percentage of the actual conference program, implying (to the author) that the program committee was incompetent or venal. When a program committee member's paper is rejected, the dynamic becomes self-sustaining: the accept threshold must be higher than the (self-perceived) merit of their own paper, encouraging them to advocate rejecting even more papers."*

Within the machine learning community, Rastogi et al. (2022d) survey reviewers about visibility of papers submitted to a conference that anonymizes authors, and intentionally searching online for assigned papers. Or current work contributes to a tradition in machine learning venues of experimentation aimed at understanding and improving the peer-review process (Lawrence and Cortes, 2014a; Shah et al., 2018a; Tomkins et al., 2017a; Stelmakh et al., 2021c; 2020a; 2021b; Cortes and Lawrence, 2021; Beygelzimer et al., 2023; Stelmakh et al., 2023).

See (Shah, 2022) for a more extensive discussion about research on the peer-review process and associated references.

## 6.3 Questionnaire

Our experiment was conducted in two phases. Phase 1 was conducted shortly after the paper submission deadline, and Phase 2 was conducted after the authors were shown their initial reviews.[2] We asked two questions during Phase 1 and one question during Phase 2, as described below. All of the questions were optional. Authors were told that their responses will not be seen by anyone

[2]During the NeurIPS 2021 review process, initial reviews were released to authors, who had the chance to respond to the reviews and engage in subsequent discussion with the reviewers. Reviews were then updated before final acceptance decisions were released.

during the review process and will not affect the decisions on their papers. The study protocol was approved by an independent institutional review board (IRB). A more detailed description of privacy and confidentiality of responses can be found in Appendix E.1.

**Phase 1:** The first phase was conducted four days after the deadline for submission of papers, and was open for ten days. All authors of submitted papers were asked the following question:

- **Acceptance probability.** What is your best estimate of the probability (as a percentage) that this submission will be accepted? Please use a scale of 0 to 100, where 0 = "no chance of acceptance" and 100 = "certain to be accepted." Your estimate should reflect only how likely you believe it is that the paper will be accepted at NeurIPS, which may or may not reflect your perception of the actual quality of the submission. For context, over the past four years, about 21% of NeurIPS submissions were accepted.

Every author who had authored more than one submitted paper was also asked the following second question:

- **Paper quality ranking.** Rank your submissions in terms of your own perception of their scientific contributions to the NeurIPS community, if published in their current form. Rank 1 indicates the submission with the greatest scientific contribution; ties are allowed, but please use them sparingly.

Notice that the two questions differ in two ways. The acceptance probability question asks for a value (chance of acceptance), and this value represents the authors' perception of the outcomes of the peer-review process for their paper. On the other hand, the paper quality ranking question asks for a ranking, and furthermore, pertains to the author's own perception of the scientific contribution made by their paper.

**Phase 2:** The second phase was conducted after the authors could see the (initial) reviews. This phase comprised a single question, and the participants were told they could answer this question irrespective of whether they participated in Phase 1 or not.

- **Change of perception.** After you read the reviews of this paper, how did your perception of the value of its scientific contribution to the NeurIPS community change (assuming it was published in its initially submitted form)? [Select any one of the following options.]
  - o My perception became much more positive
  - o My perception became slightly more positive
  - o My perception did not change
  - o My perception became slightly less positive
  - o My perception became much less positive

More details about the timeline and instructions are provided in Appendix E.1. The instructions were designed to give participants complete information about how their provided data would be used. The experiment was reviewed and approved as exempt research by the Microsoft Research IRB who issued a waiver of informed consent from the participants. The participant emailing and data collection for the experiment started on June 1, 2021 and ended on September 28, 2021.

## 6.4 Basic statistics

In this section, we provide some basic statistics pertaining to the experiment.

**NeurIPS 2021 conference:**

- Total number of papers submitted to the conference: 9,034.
- Total number of unique authors who submitted papers to the conference: 23,882.
- Total number of author-paper pairs: 37,100.[3]
- Percentage of submitted papers that were eventually accepted to the conference: 25.8%.

We now move on to discuss the responses to the three questions.

**"Acceptance probability" question:**

- Number of responses: 9,907 (26.7% of author-paper pairs).
- Number of papers with at least one response: 6,278 (69.5%).

**"Paper quality ranking" question:**

- Number of authors with more than one submission: 6,237.
- Total number of author-paper pairs for these authors: 19,455.
- Number of "rank" responses received (out of 19,455): 6,908 (35.5% response rate).

**"Change of perception" question:**

- Number of papers remaining after reviews were released (as some were rejected/withdrawn): 8,765
- Number of author-paper pairs remaining: 36,103.
- Number of responses: 4,435 (12.3% response rate).

**Response rates and breakdown:** The overall response rates in our experiment are broadly in the ballpark of the response rates of other surveys in computer science. The survey by Nobarany et al. (2016) in the CHI 2011 conference had a response rate of 16%. Rastogi et al. (2022d) conduct multiple surveys: an anonymous survey in the ICML 2021 and EC 2021 conferences had response rates of 16% and 51% respectively; a second, non-anonymous opt-in survey in EC 2021 had a response rate of 55.78%. (Frachtenberg and Koster, 2020) survey authors of accepted papers in 56 computer systems conferences, with response rates ranging from 0% to 59% across these conferences. The survey by (Gardner et al., 2012) was opt-in in 2011 and their response rate was 28%.

We used gender self-reported in OpenReview profiles. The conference had 23,581 author-paper pairs with a self-reported gender "male" of the author, 3,328 author-paper pairs with a self-reported gender "female" of the author. We omit other gender-based subgroups in our analysis,

[3]Only authors with a profile on the conference management platform (OpenReview.net) could participate in the experiment, yielding 34,713 eligible author-paper pairs.

following concerns about privacy and noise due to the small sample size of responses from these groups. Further, 7,432 author-paper pairs did not have a self-reported gender of the author. In phase 1, the response rate among author-paper pairs with self-reported gender as "male" was 30.9%, that among self-reported gender as "female" was 24.7%, and among the rest was 22%.

In terms of seniority, while we do not have a perfect measure of seniority, we use the role within the NeurIPS 2021 reviewing process as a proxy. We consider three levels of seniority. Ordered by decreasing seniority, these levels comprise of: (1) authors who were invited to serve as area chairs or senior area chairs at NeurIPS 2021, whom we refer to as "meta-reviewers"; (2) authors who were invited to serve as reviewers; and (3) authors who are in neither of the two aforementioned groups. The conference saw 3,834 author-paper pairs for authors invited as meta-reviewers, 10,938 pairs for authors invited as reviewers, and 19,941 for those who were in neither list. The response rate (for acceptance probability) was 21% among authors invited as meta-reviewers, 28.9% among authors invited to review, and 29.8% for those in neither list.

In terms of paper outcomes, out of all the responses to Phase 1 (acceptance probability) of the experiment, 27% of the responses pertained to papers that were eventually accepted. Thus, in Phase 1 we did not see any large non-response bias with respect to the papers that were eventually accepted or rejected.

The "change of perception" question (Phase 2) was asked after the authors saw the reviews. Some authors with unfavorable reviews withdrew their papers before this phase. Some other papers were rejected before this phase for reasons such as formatting violations. As a result, out of all the responses to Phase 2 of the experiment, there was a significantly higher representation of accepted papers: 39.8% of the responses pertained to papers that were eventually accepted. Out of the 4,435 responses (author-paper pairs) in this phase, 3,259 were from authors who self-identified as male, 310 from authors who self-identified as female, and 866 from those who did not provide a gender or those who did not self-identify as male or female. In terms of participation in the review process, 324 responses were from authors who were invited to serve as meta-reviewers, 1,544 from authors who were invited to serve as reviewers, and 2,567 from neither.

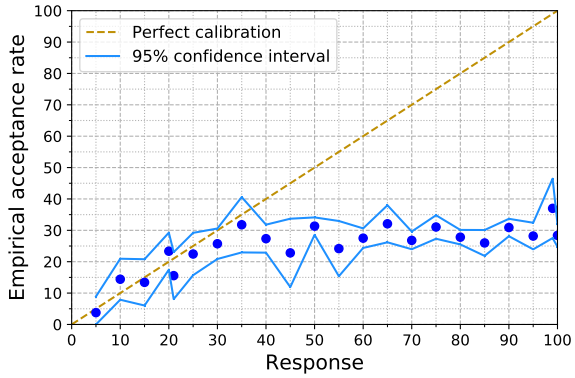
## 6.5 Main analysis and results

We now present the main results.

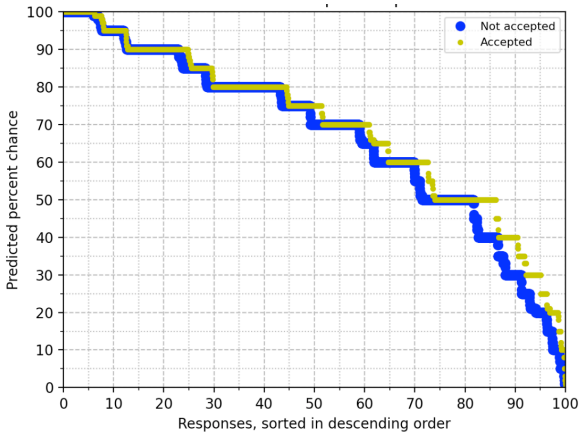
### 6.5.1 Calibration in prediction of acceptance

We begin by looking at the responses to the acceptance probability question, and comparing it with actual acceptance decisions. In Figure 6.1a, we plot the relation between responses given by authors to the question and the actual acceptance rates for these papers. Here, the blue dots represent responses with at least 50 samples, which together comprise 94% of all responses.

We find that there is a nearly three-fold overestimation overall: The median of the acceptance probabilities estimated by the respondents is 70% and the mean is 67.7%. In comparison, we had primed respondents by mentioning that the acceptance rate in the past four years was about 21%.




(a) Plot of authors' predictions of chances of acceptance of the paper versus the actual acceptance rates for each response. The diagonal line represents perfect calibration and the (blue) dots represent authors' responses.




(b) Plots of authors' predictions, for papers that were eventually accepted (thin yellow line) and rejected (thick blue line). The x-axis represents the fraction of responses with predicted percent chance greater or equal to the corresponding value on the y-axis. In other words, the x-axis is the fraction of responses with prediction greater than or equal to the corresponding y value.

Figure 6.1: Author's predictions on the probability of acceptance of their papers.

(The acceptance rate at NeurIPS 2021 ended up being 25.8%.) The fact that participants overpredict aligns with studies in other settings (Alpert and Raiffa, 1982; Anderson et al., 2012) that also find overconfidence effects. Also observe in Figure 6.1a that interestingly, the authors' predictions track perfect calibration quite well for responses up to 35%, whereas responses greater than (to the right of) 35% are uncorrelated with the actual acceptance rate.

In Figure 6.1b, we sort the responses in descending order (on the x axis) and plot the values of these responses (y axis). We make separate plots for papers that were eventually accepted and those that were eventually rejected. We see that these two plots track each other quite closely, with papers that were eventually accepted having slightly higher predictions. We also observe indications of over estimation here – more than 5% of responses predict a 100% chance of their paper getting accepted, about 50% responses predict chances of 75% or higher, whereas fewer than 15% of responses provide a prediction smaller than 40%.

### 6.5.2 Role of demographics

Next we look at the role of demographics in calibration. For this we now define the calibration error in prediction of acceptance by any author. First, based on Section 6.5.1 and Figure 6.1a, we note that responses were on average overly confident, that is the predicted probability of acceptance was higher than the observed rate of acceptance. Further, we also observe that within each demographic-based subgroup, authors on average predicted a higher acceptance probability of their submission as compared to the acceptance rate within that subgroup. We thus know the

direction of miscalibration of each subgroup.

We measure the calibration error of any subgroup in terms of the mean Brier score (i.e., squared loss). The Brier score (Brier, 1950) is a strictly proper scoring rule that measures the accuracy of probabilistic predictions: Given a prediction (value in the interval $[0, 1]$ representing the probability of acceptance) and the outcome (accept $= 1$, reject $= 0$), the Brier score equals the square of the difference between the prediction and the outcome. To get a sense of the value of the Brier score, if 25% of the papers are accepted and all respondents provide a prediction of 0.25, then the Brier score equals 0.1875; if all respondents provide a prediction of 0.8 then the Brier score equals 0.49. In our analysis, we had decided in advance to execute statistical tests comparing calibration of male and female authors and of reviewers and meta-reviewers; we had decided to not compare the remaining subgroups due to possibility of high heterogeneity among them. We provide the main details about our analysis in this subsection, and provide additional details in Appendix E.2.

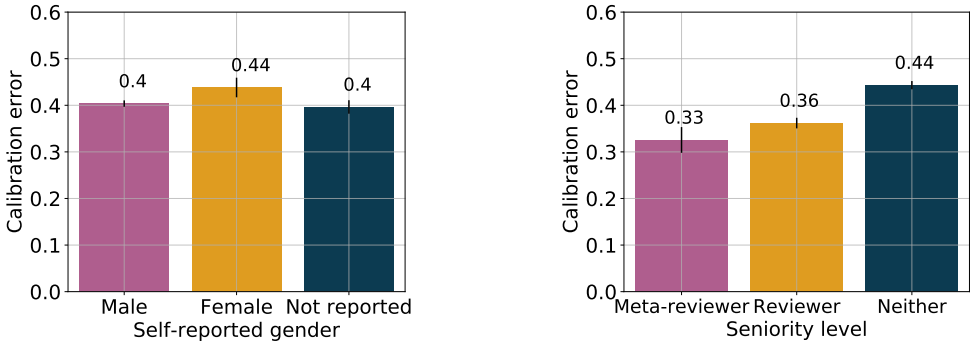
### Gender

We compute the average calibration error for a gender subgroup, weighted to account for confounding by other demographic factors of seniority and geographical region (see Appendix E.2 for details). See Figure 6.2a for the average calibration error for the "male", "female" and "not reported" subgroups, where "not reported" comprises of authors who did not provide their gender information in their Open Review profile. We do not report statistics for other gender subgroups, which are very small, to preserve respondent privacy.

In many fields of science there is research showing that there exists a confidence gap between female and male participants (Dahlbom et al., 2011; Bench et al., 2015), where men are generally found to overestimate and women underestimate. In NeurIPS 2021, we tested for significance of difference in calibration error by male authors and female authors. To test this hypothesis, we consider the test statistic of the difference in calibration errors between female authors and male authors and conduct a two-sided test. We find that there is a statistically significant difference ($p = 0.0012$) at level $0.05$. However, note that the effect size—the difference in the calibration errors between female authors ($0.44$) and male authors ($0.40$)—is small ($0.04$).

### Seniority

We now investigate the role of seniority in authors' calibration of probability of acceptance. As mentioned in Section 6.4, we consider three subgroups defined by the authors' reviewing role as a proxy for seniority, namely, authors invited to serve as meta-reviewers, authors invited to serve as reviewers, and the remaining authors. Figure 6.2b shows the average calibration error for these three subgroups, weighted to account for confounding by other demographics (see Appendix E.2 for details). Further, we tested for significance of difference in the average calibration error between the sets of meta-reviewers and reviewers. As in Section 6.5.2, we consider the difference in the mean calibration error as the test statistic. The difference in calibration error between meta-reviewers ($0.33$) and reviewers ($0.36$) is $0.03$, and the difference is not statistically significant ($p = 0.055$) at level $0.05$. As mentioned earlier, we had a priori decided to not run any statistical tests on the "neither" group.




(a) Gender-based grouping.

(b) Seniority-based grouping.

Figure 6.2: Comparing authors' calibration error (Brier score) in prediction of acceptance across different subgroups based on gender and seniority level. The error bars indicate 95% confidence intervals, obtained via bootstrapping.

### 6.5.3 Prediction of acceptance vs. perceived scientific contribution

We investigate the consistency between the predictions by authors about the acceptance of their papers and the scientific contribution (paper quality) of those papers as perceived by the authors. There were a total of 6,024 pairs of papers by the same author where the author provided their responses for both questions for both papers. We break down the responses in Figure 6.3.

Of particular interest are the first two bars in Figure 6.3 that comprise responses where the same author provided a strict ranking of two papers they authored in terms of their perceived quality, and also gave distinct probabilities of acceptance for the two papers. Among these responses, we find that there is a significant amount of agreement – the two rankings agree in 92.6% ($\frac{66.9}{66.9+5.3}$) of responses. However, there is a noticeable 7.4% of responses where the authors think that the peer review is more likely to reject the better of their two papers.

### 6.5.4 Agreements between co-authors, and between authors and peer-review decisions

We first look at author-provided rankings of their perception of the scientific contribution (paper quality) of multiple papers they authored. We compare these rankings with the outcomes (accept or reject) of the peer-review process. We show the results in Figure 6.4. In particular, observe that among the situations where the decisions for the two papers were different and the author-provided ranking was strict (first two bars of Figure 6.4), authors' rankings disagreed with the decision 34% ($\frac{11}{21.1+11}$) of the time. (An analysis comparing the ranking of papers by authors' perceived acceptance probabilities and the final decisions yields results very similar to that in Figure 6.4.)

We now compute agreements between co-authors in terms of their perceived scientific contribution (paper quality) of a pair of jointly-authored papers. We show the results in Figure 6.5. Observe that interestingly, among the pairs where both authors gave a strict ranking, they disagreed 32% ($\frac{19.7}{41.8+19.7}$) of the time—approximately the same level of disagreement as we saw

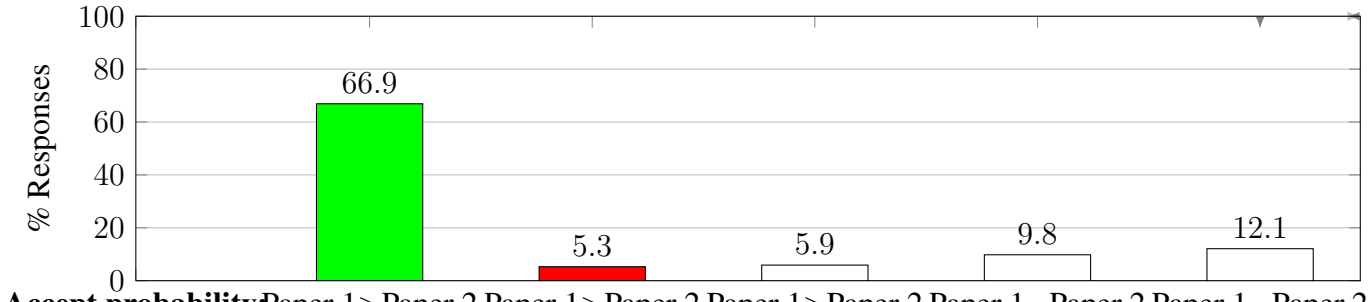



Figure 6.3: Comparing authors' (relative) predicted acceptance probability and perceived paper quality for any pair of papers authored by them. This plot is based on 6,024 such responses. In particular, the first two bars enumerate the amount of agreement and disagreement respectively, among responses of any author that had a strict ranking between the two papers for both questions.

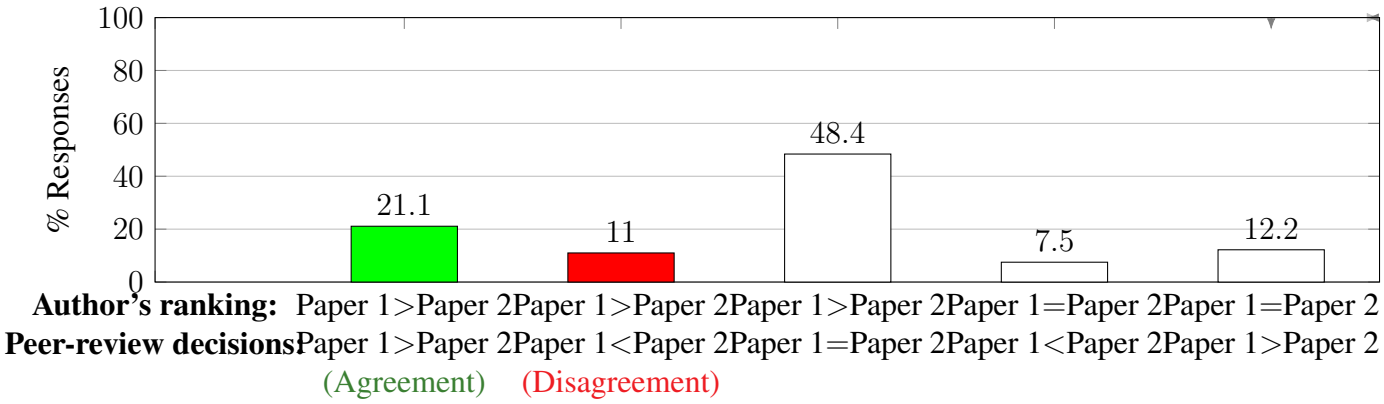



Figure 6.4: Comparing authors' ranking of their perceived scientific contribution (paper quality) and the decisions from the peer-review process. This plot is based on 10,171 such responses. In particular, the first two bars enumerate the agreement and disagreement when the author-provided ranking is strict and where one of the papers is accepted and the other is rejected.

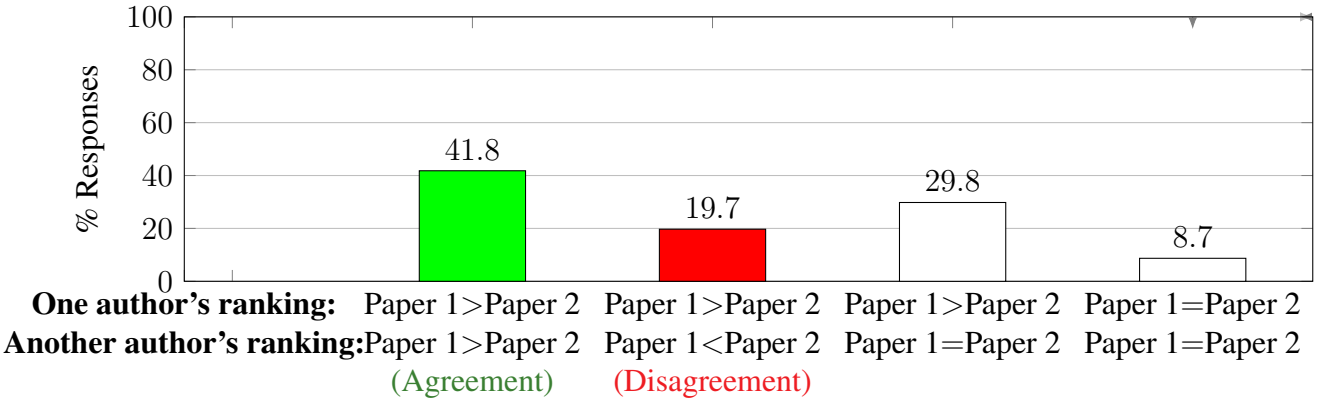



Figure 6.5: Comparing co-authors' rankings of their perceived scientific contribution (paper quality) of a pair of papers that both have authored. This plot is based on 1,357 such responses. In particular, the first two bars enumerate the agreement and disagreement of co-authors when they both provide strict rankings of their papers.

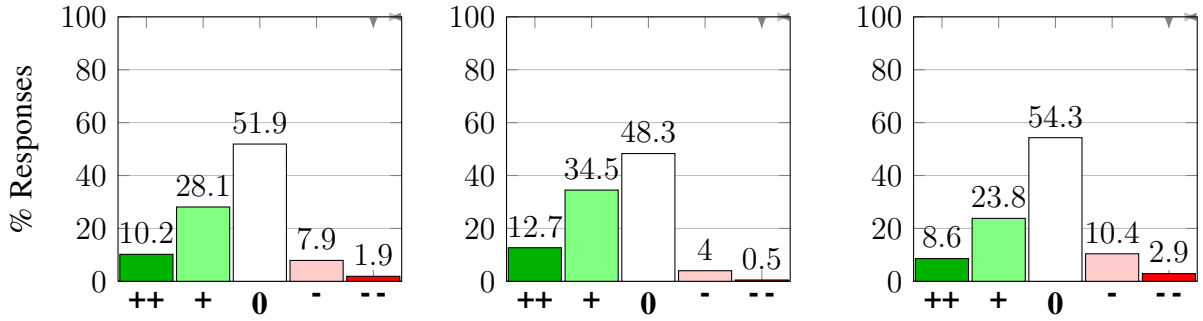
between the authors and reviewers.

This high amount of disagreement between co-authors about the scientific contribution of their jointly authored papers has some implications for research on peer review. Many models of peer review (Roos et al., 2012; Ge et al., 2013; Tomkins et al., 2017a; MacKay et al., 2017; Wang and Shah, 2019a; Ding et al., 2022; Heyard et al., 2022) assume existence of some "true quality" of each paper. This result raises questions about such an assumption—if there were such a true quality, then it is perhaps the authors who would know them well at least in a relative sense, but as we saw above, authors do not seem to agree. In a recent work, Su (2021) proposes a novel idea of asking each author to submit a ranking of their submitted papers. Under the assumption that this author-reported ranking is a gold standard, Su (2021) then proposes to modify the review scores to align with this reported ranking. However, our observation that co-authors have a high disagreement about this ranking violates the gold standard assumption that underlies this proposal.

### 6.5.5 Change of perception

We now analyze the responses to the question posed to authors in the second phase of the experiment on whether the review process changed their perception of their own paper(s). We plot the results in Figure 6.6. Given significant non-response bias in this phase with respect to acceptance decisions (Section 6.4), we also separately plot the responses pertaining to accepted and rejected papers.

We observe that among both accepted and rejected papers, about 50% of the responses indicated a change in their perceived opinion about their own papers. Furthermore, even among rejected papers, over 30% of responses mention that the reviews made their perception more positive. While past studies (Frachtenberg and Koster, 2020; Weller, 1996; Mulligan et al., 2013; Patat et al., 2019) document whether the review process helps improve the paper, the results in Figure 6.6 shows that it also results in a change of perception of authors about their papers about




(a) All papers. (b) Accepted papers. (c) Rejected papers.

Figure 6.6: Change in authors' perceptions of their own papers after seeing the reviews. The five bars in each plot represent the five options: much more positive ("++"), slightly more positive ("+"), did not change ("0"), slightly more negative ("-"), much more negative ("- -"). The three subfigures depict responses pertaining to all, accepted, and rejected papers, and are based on 4435, 1767, and 2668 such responses respectively.

half the time.

## 6.6 Limitations and discussion

We discuss some key limitations. The 26.7% response rate in phase 1, and particularly the 12.3% response rate in phase 2, introduces concerns about non-response bias, in which non-respondents might have given different answers than respondents. We provide statistics pertaining to non-response bias in Section 6.4, and attempt to mitigate confounding with respect to the observables of demographics and paper outcomes (specifically, Section 6.5.2 and Section 6.5.5). However, importantly, only the observables cannot capture all the ways in which data may be missing not at random and this caveat ought be kept in mind in interpreting our results. A second limitation of this study is that respondents may not have been answering fully honestly. For example, if respondents believed that there was even a small chance their answers might leak to reviewers or co-authors, this would incentivize them to exaggerate the probability their paper would be accepted (an effect which would indeed be consistent with the pattern we observed). We took pains to mitigate this effect by assuring the authors of the privacy and security of their responses, and further, by asking them to not discuss their responses with others (see Appendix E.1).

These limitations notwithstanding, this study has several implications for improving the peer review process. First, the fact that authors vastly overestimated the probability their papers would be accepted suggests it would be useful for conference organizers and PhD supervisors to attempt to recalibrate expectations prior to each conference. This might mitigate disappointment from conference rejections.

The disagreements we document around paper quality — between co-authors as well as between authors and reviewers — suggest that, as previous work has also found, assessing paper quality is an extremely noisy process. A complementary study on the consistency of decisions made by independent committees of reviewers that was also run at NeurIPS 2021 also showed

high levels of disagreement between reviewers (Beygelzimer et al., 2023). Specifically, 10% of submitted papers were assigned to two independent committees (reviewers, area chairs, and senior area chairs) for review, and of these papers, the committees arrived at different acceptance decisions for 23%. While it may be tempting to attribute this disagreement solely to flaws in the peer-review process, if even co-authors — who know their own work as well as anyone — have significant disagreements on the ranking of their papers, perhaps it is fundamentally hard or impossible to objectively rank papers.

The outcomes of paper submissions should thus be taken with a grain of salt, mindful of the inherent randomness and arbitrariness of the process and the arguable lack of a fully objective notion of paper quality. Realizing that the rejections which generally follow paper submissions do not necessarily result from lack of merit, but rather just bad luck and subjectivity, would both be accurate and healthy for the academic community. More broadly, as a community, we may take these findings into account when deciding on our policies and perceptions pertaining to the peer-review process and its outcomes. We hope the results of our experiment encourage discussion and introspection in the community.

# Chapter 7

# A Randomized Controlled Trial on Anonymizing Reviewers to Each Other in Peer Review Discussions

Based on :
Charvi Rastogi, Xiangchen Song, Zhijing Jin, Ivan Stelmakh, Hal Daumé III, Kun Zhang, and Nihar B Shah. A Randomized Controlled Trial on Anonymizing Reviewers to Each Other in Peer Review Discussions. Working paper on arXiv. 2024.

## 7.1 Introduction

Peer review serves as the backbone of scientific research, underscoring the importance of designing the peer-review process in an evidence-based fashion. The goal of this work is to conduct carefully designed experiments in the peer-review process to enhance our understanding of the dynamics of discussion among reviewers. In many publication venues, pertinently in conference-based venues in the field of Computer Science,[1] reviewers of a paper discuss it with other reviewers before reaching a final decision. Starting after reviewers have provided their initial review, the discussions takes place on a typed forum where reviewers type in their opinions and responses to other reviewers asynchronously. We specifically target an important aspect of this discussion process: anonymity of reviewers to other reviewers during the discussion phase. It is important to note that in our setting, reviewers of papers are always anonymous to their authors and vice versa, commonly referred to as "double-anonymous" or "double-blind" peer review, and we do not study or intervene on that aspect of the peer-review process.

The ongoing discourse within the academic community regarding the anonymization of reviewers during discussions has spanned several years. Despite anecdotal arguments from program chairs and stakeholders on both sides of the debate, a significant gap exists towards an evidence-based understanding of the actual effects of anonymity in discussions. Anonymizing

[1]Conferences in Computer Science, typically ranked higher than journals, review full-length papers and are considered to be a terminal publication venue.

discussions carries a set of potential advantages and drawbacks. It is often hypothesized that anonymizing discussions helps alleviate biases associated with reviewer identities. Additionally, it has been suggested that anonymous discussions can mitigate fraud wherein one reviewer (with an undisclosed conflict of interest with the authors of the paper being reviewed), reveals the identity of the other reviewers to the authors of the paper, to ultimately help coerce the reviewers into accepting the paper (Resnik et al., 2008). Conversely, proponents of revealing reviewer identities argue that it fosters a deeper comprehension of reviews and perspectives based on knowledge of review writer's background. Additionally, program chairs have expressed concerns that anonymity may diminish politeness in discussions. In the past, the peer-review processes in different venues have taken different approaches to the setup for reviewer discussions. For example, among conferences in the field of machine learning / artificial intelligence, AAAI 2021 and IJCAI 2022 and other conferences enforced anonymous reviewer discussions whereas many other conferences such as ICML 2016 and FAccT 2023 did not.

Understanding the impacts of anonymizing discussion among reviewers has broader implications towards understanding the role of participants' identities in group discussion dynamics and outcomes. This is relevant for research funding agencies that allocate grants annually through panel-based discussions. Although group discussions involving experts are commonly assumed to improve final decision quality compared to individual decision-makers, controlled experiments studying panel discussions in peer review, have found that group discussions in fact lead to a significant increase in the inconsistency of decisions (Obrecht et al., 2007; Fogelholm et al., 2012; Pier et al., 2017). It is an open problem to understand the underlying causes (Teplitskiy et al., 2020; Stelmakh et al., 2020a) and this work will contribute to a better understanding of it, by shedding light on the role of participants' identities in the discussion process.

With this motivation, we conduct a study in the review process of a top-tier publication venue in AI: The 2022 Conference on Uncertainty in Artificial Intelligence (UAI 2022). The study takes two principal approaches to quantify the advantages and disadvantages of enforcing anonymity between reviewers during the discussion phase. Firstly, we design and conduct a randomized controlled trial to examine the impact of enforcing anonymity on reviewer engagement, reviewer politeness and the influence of seniority on final decisions. Secondly, we conduct a systematic survey-based study which offers a more reliable foundation than anecdotal evidence and hypotheses towards informing policy decisions. Our anonymous survey provides a nuanced understanding of statistical trends in reviewers' preferences and their experiences in the discussion phase.

## 7.2 Related work

## 7.3 Experiment setting and design

**Setting of the experiment.** The experiment was conducted in the peer-review process of the 2022 edition of the Uncertainty in Artificial Intelligence conference (UAI 2022). Our study delves into the specifics of the "discussion phase" in the peer review process of UAI 2022, that takes place after the initial reviews are submitted. In this phase reviewers and authors communicate asynchronously via a typed forum, to discuss their respective opinions of the paper. The

discussion phase for each paper begins after the deadline for initial reviews and ends concurrently with the deadline for final paper decisions. Each paper's discussion involves typically three to four reviewers who were assigned to review that paper and one meta-reviewer (equivalent to associate editor for journal publication), alongside the authors of the paper. This discussion takes place on an online interface of OpenReview.net which takes in typed posts from participants. For the papers assigned to them, each reviewer is expected to carefully read the reviews written by the other reviewers as well as the authors' responses, and participate in the discussion. During the discussion phase, the reviewers have the option to update their initial review, which may include updating their review score, to reflect any change of opinion.

The conference UAI 2022 was double-anonymous in its conduct, wherein authors were not shown the identities of their reviewers, and vice versa. Within the discussion interface, the reviewers' identities are visible to their corresponding meta-reviewers, while the meta-reviewers' identities are concealed from the reviewers. Notably, our study introduces a key change where the visibility of fellow reviewers' identities is contingent on their assigned condition, as described in the next paragraph.

**Design of the experiment.** We designed a randomized controlled trial to investigate the effect of anonymity among reviewers in the discussion phase. Each submitted paper and each participating reviewer were assigned at random to one of two conditions: the "non-anonymous" condition, where reviewers were shown the identities of other reviewers assigned to the same paper, and the "anonymous" condition where fellow reviewers' identities were not accessible. The assignment to conditions was uniformly random for both papers and reviewers. To prevent potential spillover effects, reviewers within each condition were then matched with papers in the same condition[2]. All reviewers were informed at the beginning of the review process that a randomized controlled trial would be conducted to compare differently anonymized reviewing models. To mitigate the Hawthorne effect, specifics of the experiment were withheld[3]. Importantly, reviewers were informed that only aggregate statistics would be publicly disclosed, and they were given the choice to opt out of the experiment. At the onset of the discussion phase, reviewers were apprised of whether their discussions would be anonymous or not. We note that the meta-reviewers were common among the two conditions.

In conjunction with the randomized controlled trial, we conducted a survey to provide a holistic understanding of the pros and cons of anonymity in the discussion phase. Administered to all reviewers and meta-reviewers, the survey sought to capture valuable insights into their experiences and perspectives. Specifically, the survey gathered reviewers' feedback related to the discussion type they were assigned to, in order to uncover the impact of anonymity (or lack thereof) on their experience in the review process. Furthermore, the survey included questions probing into the aspects of the discussion phase reviewers prioritise. Lastly, we sought reviewers' personal preferences regarding anonymous versus non-anonymous discussions. We elaborate on

[2]This also helps address fraud concerns. For instance, AAAI 2020 showed every reviewer just a random fraction of the submitted papers during bidding to mitigate fraud.

[3]It is a common practice to inform reviewers that they are part of an experiment without disclosing the specifics. For instance, in Forscher et al. (2019), reviewers were informed that the research focused on examining the NIH review process, and they would be evaluating modified versions of actual R01 proposals. However, the nature of these modifications was intentionally left undisclosed to the reviewers.

the specifics of the survey in Section 7.4.

**Details of the experiment.** We now delve into the particulars of how the experiment unfolded. The UAI 2022 conference received 701 paper submissions out of which 351 were assigned to the anonymous condition and the remaining 350 to the non-anonymous condition. Over the course of the review process, 69 papers were withdrawn, leaving 322 in the anonymous condition and 310 in the non-anonymous condition. Similarly, at the beginning of the review process 581 reviewers had signed up for reviewing, out of which 65 reviewers dropped out, resulting in 263 reviewers in the anonymous condition and 253 reviewers in the non-anonymous condition. To substitute for the reviewers that dropped out, meta-reviewers added emergency reviewers, such that each new reviewer was added to papers from only one of the two conditions. Following this, the conference had a total of 289 reviewers in each condition.

**Reviewer seniority information.** This study explores the indirect impact of reviewer seniority during the discussion phase. To achieve this, we gathered data pertaining to the seniority of all reviewers. Specifically, we retrieved reviewers' current professional status from their profiles on Openreview.net. In cases where this information was unavailable, we conducted manual searches on the Internet to find their professional status. Using this information, we established a binary indicator of seniority: undergraduate students, graduate students and post-doctoral researchers were categorized as junior researchers, while professors and other professional researchers such as research scientists and research engineers were categorized as senior researchers. This classification covered all participating reviewers, and resulted in 181 seniors out of 289 reviewers in the non-anonymous condition and 169 seniors out of 289 reviewers in the anonymous condition, as detailed in Table 7.1.

## 7.4 Main analyses

In this section, we address each of the research questions stated in the abstract.

### 7.4.1 RQ1: Do reviewers discuss more in the anonymous or non-anonymous condition?

Recall that the research question asks whether there is a difference in the amount of participation by reviewers in the discussion phase, between the anonymous and non-anonymous condition. To answer this question, we perform the following analysis. In both conditions, for each reviewer-paper pair, we first compute the number of discussion posts made by that reviewer for that paper (excluding the initial review by the reviewer). We then compute the average number of discussion posts across all reviewer-paper pairs in each condition. If the difference in averages between the two conditions is significantly larger than zero in magnitude, we conclude that anonymity among reviewers (or lack thereof) in the discussion phase has an effect on the amount of discussion participation observed.

| Statistic of interest | Anonymous | Non-anonymous |
|---|---|---|
| Number of papers | 322 | 310 |
| Number of reviewers | 289 | 289 |
| Number of senior reviewers | 169 | 181 |
| Number of {paper-reviewer} pairs | 1163 | 1118 |
| Number of discussion posts written by reviewers | 611 | 514 |

Table 7.1: Some preliminary statistics of interest measured across the two experiment conditions in the discussion phase of UAI 2022. Statistics in the last two rows are used towards RQ1.

**Results.** Table 7.1 provides the relevant statistics to address RQ1. We see that the average number of posts made by reviewers during the discussion phase in the anonymous condition 0.53 $(611 \text{ out of } 1163)$ is higher than that in the non-anonymous condition 0.46 $(514 \text{ out of } 1118)$. To evaluate the significance of this difference we conducted a permutation test with $1,000,000$ iterations, which gave a two-sided p-value of $0.051$.

We delve a bit deeper into the data, stratifying by seniority of reviewers. On analysing the engagement metrics separately for reviewers based on their seniority, we observe that junior reviewers posted on average 0.58 (273 out of 468) times when anonymous, and 0.57 (232 out of 410) times when non-anonymous, while senior reviewers posted 0.49 (338 out of 695) times when anonymous and 0.40 (282 out of 708) times when non-anonymous. Given some potential concerns around suppression of participation of junior reviewers in non-anonymous settings (Roberts and Rajah-Kanagasabai, 2013), these results are surprising and suggest a need for further inquiry.

### 7.4.2 RQ2: Does seniority have a higher influence on final decisions when non-anonymous than anonymous?

The objective is to test for the presence of any additional influence of senior reviewers towards the final decisions when the identities of fellow reviewers are visible. Consequently, this analysis restricts attention to a subset of papers which have at least one senior reviewer and one junior reviewer. Let $\mathcal{P}_a$ be the set of all such papers in the anonymous condition and $\mathcal{P}_{\widetilde{a}}$ be the set of such papers in the non-anonymous condition. For every such paper, our analysis centers on the seniority of the reviewers whose scores, prior to the discussion phase, were closest to the final decision made after discussions. Accordingly, for any paper $p$, we define $C_p$ as the reviewer or the set of reviewers whose pre-discussion scores were closest to the final decision, as follows. If paper $p$ was accepted, then $C_p$ is the reviewer (or set of reviewers in case of a tie) who gave the highest pre-discussion score to paper $p$ among all its reviewers; if paper $p$ was rejected, then $C_p$ is the reviewer or set of reviewers who assigned it the lowest score before the discussion. Next, for any paper $p$, we define a scalar $\beta_p$ defined as follows: $\beta_p = 1$ if $C_p$ includes at least one senior reviewer but no junior reviewer, $\beta_p = 0$ if $C_p$ includes both senior and junior reviewers, and $\beta_p = -1$ if $C_p$ includes only junior reviewers.

To test for difference of influence of seniority on final decisions, we consider the following

| Statistic of interest | Anonymous | Non-anonymous | $p$-value |
|---|---|---|---|
| Number of papers accepted | 89 (out of 242) | 94 (out of 242) | 0.71 |
| Paper decision closest to senior reviewers, $\beta_p = 1$ | 96 (out of 242) | 122 (out of 242) | |
| Paper decision closest to junior reviewers, $\beta_p = -1$ | 70 (out of 242) | 60 (out of 242) | |
| Mean of $\beta_p$ | 0.11 | 0.26 | 0.04 |

Table 7.2: Statistics of interest measured in the experiment conditions for research question RQ2 on the influence of seniority on final decisions.

test statistic which measures the difference across the two conditions of the extra influence of senior reviewers on the final decision compared to junior reviewers.

$$T_2 = \frac{\sum_{p \in \mathcal{P}_a} \beta_p}{|\mathcal{P}_a|} - \frac{\sum_{p \in \mathcal{P}_{\tilde{a}}} \beta_p}{|\mathcal{P}_{\tilde{a}}|}. \tag{7.1}$$

A statistically significant value of $T_2$ here would indicate that senior reviewers exhibit distinct influence on final decisions depending on whether reviewers are anonymous or non-anonymous to each other.

**Result.** The statistics related to this analysis are provided in Table 7.2. First, as detailed in Row 1, there was no statistically significant difference in the acceptance rate of papers in the two conditions, with an acceptance rate of $37\%$ in the anonymous condition and $39\%$ in the non-anonymous condition. The difference in acceptance rates between the conditions has a Fisher-exact $p$-value of 0.71. Now, in reference to the research question posed, we observe that the final decision was closest to a senior reviewer's initial score $50\%$ of the times in the non-anonymous condition and $40\%$ of the times in the anonymous condition. Meanwhile, respectively, the final decision agreed most with a junior reviewer $25\%$ times and $30\%$ times, thus indicating a clear disparity in the seniority of the reviewer whose score was apparently most closely followed for the final decision. The last row of Table 7.2 shows the mean value of the scalar $\beta_p$ across all papers. We test for statistical significance of the test statistic $T_2$ in (7.1) using the permutation test method with 1,000,000 iterations, which yielded a two-sided p-value of $0.04$.

### 7.4.3 RQ3: Are reviewers more polite in the non-anonymous condition?

In tackling this research question, we focus on the text of the discussion posts written by reviewers following their initial review. Our aim is to assess the politeness of the discussions posted in the two conditions to investigate any potential disparities therein.

First, we assign a politeness score to the raw text of each of the discussion posts written by reviewers. We consider the range of the politeness scores to be 1 (highly impolite) to 5 (highly polite). Recent work (Verharen, 2023) demonstrated the successful use of commercial large language models (LLMs) such as ChatGPT for rating politeness of scientific review texts via careful prompting. They validated the accuracy and consistency of their method in several ways,

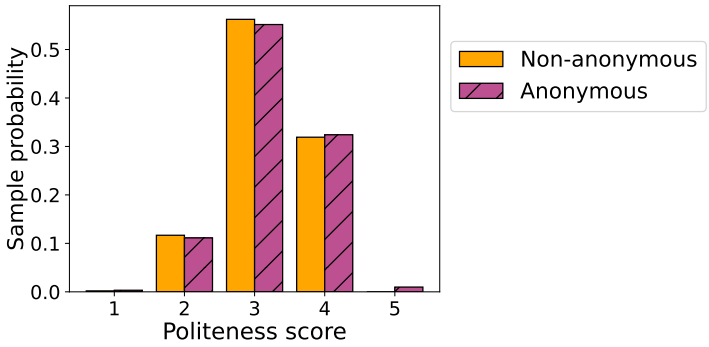



Figure 7.1: Visualization of the distribution of politeness scores obtained for all the discussion posts written by reviewers in the anonymous and non-anonymous condition in UAI 2022. The height of the left-most bar indicates the sample probability of a score falling in the interval [1,2) and so on for each bar.

such as comparing the generated outputs against human annotation. In our work, we adopt a similar technique of prompting LLMs to score a given text on politeness. However, to protect the privacy of discussion posts in UAI 2022, we avoided commercial APIs that record users' queries. Instead, we locally deploy an open-sourced LLM, Vicuna 13B (Chiang et al., 2023), which achieves close to state-of-the-art performance (Chiang et al., 2023; Zheng et al., 2023).

Following common prompt engineering practices (Brown et al., 2020a; Liu et al., 2023), we instruct the model with the task: "We are scoring reviews based on their politeness on a scale of 1–5, where 1 is highly impolite and 5 is highly polite." Using the few-shot prompting method, we include three scored texts in the prompt corresponding to different politeness scores. These examples are sourced from Bharti et al. (2023). The exact text of the prompt is provided in Appendix F.1. Further, to mitigate bias due to the ordering of the few-shot examples, we create six paraphrased versions of the prompt by varying the order of the examples. Since the output generated by LLMs can be different across iterations, each version is queried ten times, and the mean across all paraphrases and iterations yields the final politeness score. For texts exceeding the LLM token limit, we take equal-sized non-overlapping sub-parts of the text such that each subpart satisfies the token limit and the total number of sub-parts is minimized. The politeness score of the larger text is obtained by averaging the scores of its sub-parts.

The process results in a politeness score for each discussion post ranging from 1 to 5. To validate the consistency of the generated scores, we measured the correlation of scores generated for the same prompt across iterations, over 1125 unique prompts. We found a significant correlation between two randomly picked iterations for each prompt query, with a Pearson correlation coefficient of $0.42$ with a two-sided p-value smaller than 0.001. The p-value roughly indicates the probability of an uncorrelated dataset yielding a Pearson correlation at least as extreme as the one obtained from the generated data $(0.42)$.

**Results.** As noted in the last row of Table 7.1, there were 611 discussion posts made by reviewers in the anonymous condition and 514 in the non-anonymous condition. Figure 7.1 visualizes the distribution of the politeness scores obtained for the posts in the two conditions. We ob-

served that posts in the anonymous and the non-anonymous condition received similar scores with average politeness scores of $3.73$ and $3.71$ respectively.

In this analysis, it is important to note that our test is based on the politeness scores assigned to the discussion posts that took place in each condition. However, as we saw in Section 7.4.1 the posting rates differed in the two conditions, and hence the politeness data may reflect selection bias. For instance, it is possible that the average politeness observed in the anonymous condition is high because of the higher level of participation by senior reviewers in comparison to the non-anonymous condition. In Section 7.4.1 we saw that the rate of posting for senior reviewers was different in the two conditions at 0.49 when anonymous and 0.40 otherwise.

To account for this, we group the posts based on the seniority of the reviewer and conduct comparisons within groups to generate the $U$-statistic. That is, the politeness of posts by senior reviewers in one condition are compared against only those by senior reviewers in the other condition and same for posts by junior reviewers. The resulting Mann-Whitney $U$ test (Mann et al., 1947) for significant difference in the politeness of discussion posts across the two conditions revealed no significant difference. The test gave a normalized $U$-statistic of $0.49$ with a p-value of $0.72$. The normalized $U$-statistic approximates the probability in our sample that a randomly chosen score in the non-anonymous condition is higher than a randomly chosen score in the anonymous condition. The mathematical definition of the $U$-statistic is provided in (F.2.2) alongside details about the test in Appendix F.2.2.

### 7.4.4 RQ4: Do reviewers' self-reported experiences differ?

Recall that we conducted an anonymous survey for all the UAI reviewers and meta-reviewers to glean insights about their experiences in the discussion phase of UAI 2022. Some questions in the survey were designed to understand differences in reviewers' (self-reported) experiences in the two conditions. Since the pool of meta-reviewers was the same for both the conditions, we excluded them from this part of the survey. Specifically, respondents were asked to provide Likert-style responses based on their personal experience, to the following:

1. I felt comfortable expressing my own opinion in discussions including disagreeing with and commenting on reviews of other reviewers.
2. My opinion was taken seriously by other reviewers.
3. I could understand the opinions and perspectives of other reviewers given the information available to me.
4. Discussions between reviewers were polite and professional.
5. Other reviewers took their job responsibly and put significant effort in participating in discussions.

Survey respondents answered each of the five questions with exactly one of the five options: Strongly disagree, Somewhat disagree, Neither agree nor disagree, Somewhat agree, Strongly agree.

**Result.** We received responses from 64 reviewers in the non-anonymous condition and 68 reviewers in the anonymous condition. The overall survey response rate was $22.8\%$. The outcomes

| Survey question (shortened for illustration) | Mean response (Anonymous) | Mean response (Non-anonymous) | Effect size | $p-$value |
|---|---|---|---|---|
| 1. I felt comfortable expressing my own opinion. | 3.85 | 4.10 | 0.43 | 0.33 |
| 2. My opinion was taken seriously by other reviewers. | 3.56 | 3.64 | 0.48 | 0.77 |
| 3. I could understand the opinions of other reviewers. | 4.03 | 4.06 | 0.47 | 0.96 |
| 4. Discussions between reviewers were professional. | 4.13 | 4.27 | 0.47 | 0.97 |
| 5. Others were responsible & effortful in discussion. | 3.37 | 3.17 | 0.46 | 0.73 |

Table 7.3: For each survey question, we map the Likert-scale responses as: Strongly disagree$\rightarrow$ 1, Somewhat disagree $\rightarrow$ 2, Neither agree nor disagree$\rightarrow$ 3, Somewhat agree$\rightarrow$ 4, Strongly agree $\rightarrow$ 5. With this mapping, we compute the mean response in the two experiment conditions, displayed in columns 2 and 3. Column 4 provides the effect size, which is the normalized Mann-Whitney $U$ statistic in our analysis. This value approximates the sample probability that a randomly chosen response from one condition was higher than a randomly chosen response from the other condition. Using the permutation test defined in (F.4) with 100,000 iterations, we report the two-sided p-value of the test for each survey question in the last column. These p-values do not include a multiple testing correction, and are already insignificant.CR: make sure the table is same page as 4.4 when submitting

of the Mann-Whitney $U$ test for each survey question are provided in Table 7.3, with the effect size and the p-value in the last two columns. Additionally, we visualize the Likert-scale responses for each question in Figure 7.2. We did not observe any significant difference in survey participants' self-reported experiences across the two conditions. Further, the difference across the conditions was small with the normalized $U$-statistic lying between 0.43 and 0.48, where this value approximates the sample probability that a randomly chosen response from one condition was higher than a randomly chosen response from the other condition. An effect size of 0.5 implies that along the axis mentioned in the corresponding survey question such as politeness respondents had similar experiences in both conditions. More details about the test and the derivation of the p-values are provided in Appendix F.2.1.

### 7.4.5 RQ5: Do reviewers prefer one condition over the other?

We surveyed all the reviewers and meta-reviewers asking for their overall preference between the two conditions, framed as follows:

*Overall, what is your preference on whether reviewer identities should be SHOWN to other reviewers or HIDDEN? This question is about your general opinion and not restricted to your UAI 2022 experience.*

In the responses, participants were provided five options and could choose at most one from these options. We provide the options here along with their numeric mapping in this analysis:

- I strongly prefer reviewer identities to be HIDDEN from other reviewers : 2
- I weakly prefer reviewer identities to be HIDDEN from other reviewers : 1

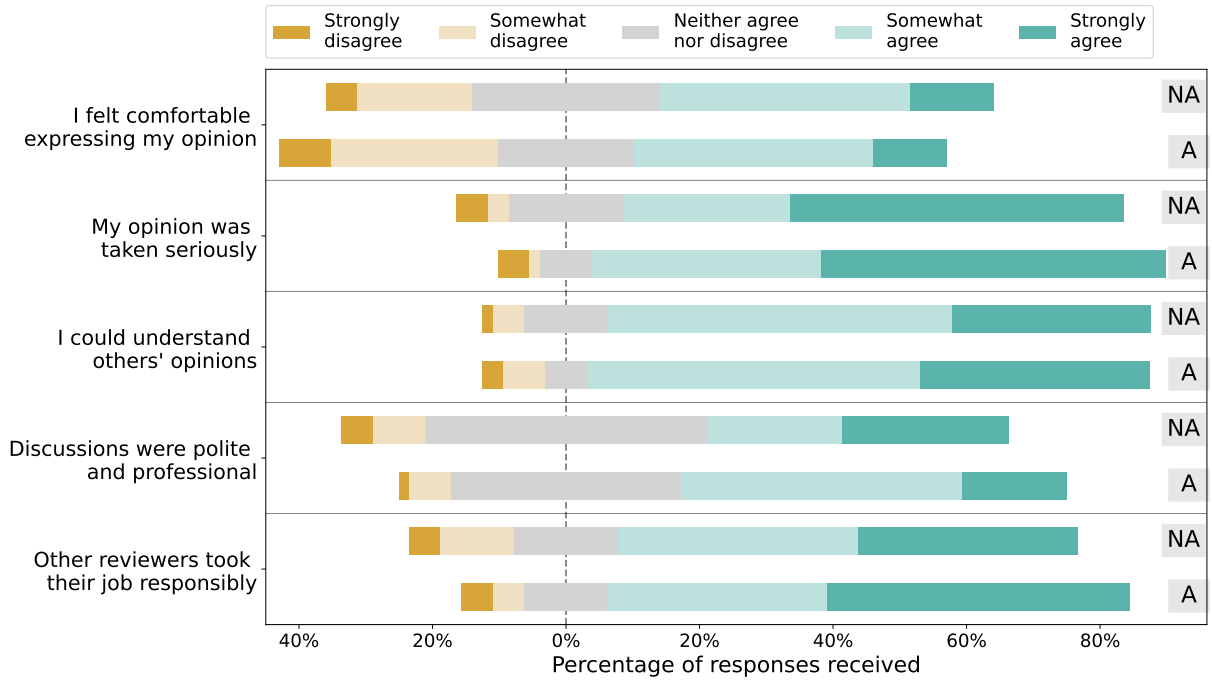



Figure 7.2: Survey outcomes on reviewers' self-reported experience in the two conditions. Respondents provided Likert-style responses for each question which have been visualized here. The markers 'NA' and 'A' indicate responses from the non-anonymous and the anonymous condition respectively.

- Indifferent : 0
- I weakly prefer reviewer identities to be SHOWN to other reviewers : -1
- I strongly prefer reviewer identities to be SHOWN to other reviewers: -2.

**Result.** In total we obtained 159 responses out of which 124 were from reviewers and 35 from meta-reviewers. Under the chosen mapping of responses to numbers, we compute the mean over all responses. This numeric average corresponding to the responses obtained is $0.35$ (Cohen's d $= 0.25$), suggesting a weak preference for reviewer identities to be anonymous. The responses had a standard deviation of $1.38$ giving a $95\%$ confidence interval of $[-2.36, 3.08]$.

### 7.4.6 RQ6: What aspects do reviewers consider important in making the policy decision regarding anonymizing reviewers to each other

In the survey, we asked reviewers and meta-reviewers about aspects they considered important in making conference policy decisions regarding anonymity between reviewers in discussions. Specifically, we asked them to rank the following six aspects according to what they found to be most important to least important:

- Reviewers take their job more responsibly and put more effort in writing reviews and participating in discussions.
- Reviewers communicate with each other in a more polite and professional manner.
- Knowledge of backgrounds of other reviewers helps discuss more efficiently.
- Reviewers are protected from identity-related biases.
- Reviewers (especially junior or from underrepresented communities) feel safer to express their opinions.
- Anonymity helps mitigate potential fraud (e.g., authors pressurizing reviewers with the help of their friend who is one of the reviewers).

Each respondent was asked to provide a rating for each aspect from 1 to 6 where 1 indicated that the aspect was least important to them and 6 indicated the aspect was most important to them.

**Result.** We had a total of 159 respondents answer all of the six importance questions. The outcomes are visualised in Figure 7.3. As shown, the highest importance was given to the aspect regarding reviewers feeling of safety in expressing their opinion, with a mean importance score of 4.56. Second most important aspect was about protecting reviewers from identity-related biases with a mean importance score of 4.44. The two least important aspects were regarding politeness and professionalism of communication among reviewers and efficiency of discussion with a mean score of 3.16 and 3.4 respectively.

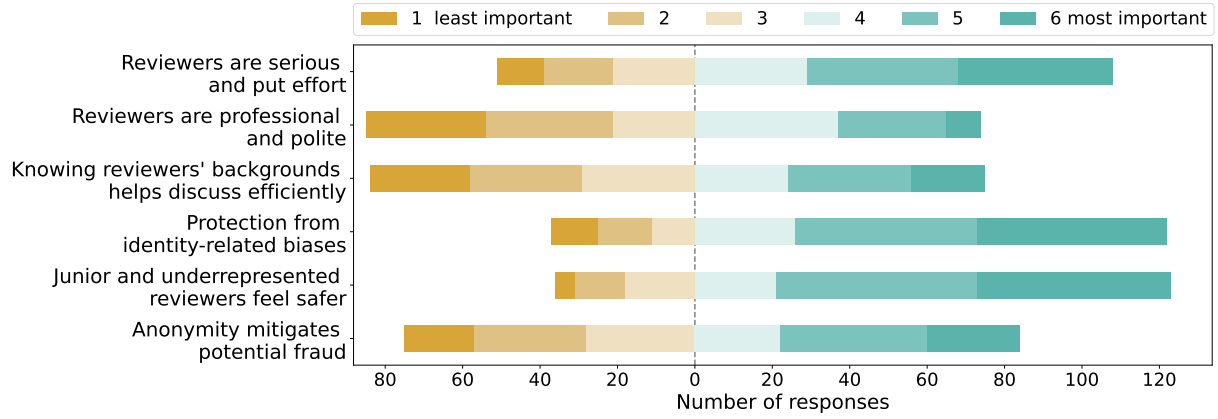



Figure 7.3: Survey outcomes on reviewers' ranking of importance of different aspects in making conference policy decisions regarding anonymity between reviewers in discussions.

### 7.4.7 RQ7: Have reviewers experienced dishonest behavior due to reviewer identities being shown to other reviewers?

The survey contained a question concerning reviewers' and meta-reviewers' current and past experiences relating to reviewer anonymity in peer review discussions. Specifically the survey question stated:

> Have you ever experienced any dishonest behavior due to reviewer identities being shown to other reviewers? Examples include: authors colluding with their friend who is a reviewer and attempting to contact other reviewers and pressurize them to accept the paper reviewers contacting other reviewers outside of the review system.

To comprehensively answer the questions, respondents had the option to choose one or more of the following options: "Yes, in UAI 2022," "Yes, in another venue," "Not sure," and "No." Further, the survey asked respondents to describe the dishonest attempt(s) they have experienced or those they may suspect as a free-text response, if they felt comfortable doing so.

**Results.** Out of the 167 respondents, 7% (12 out of 167) mentioned that they have experienced dishonest behaviour relating to anonymity in the discussion phase. Among the 12 yes responders, 8 were reviewers and 4 were meta-reviewers, and 11 chose "Yes, in another venue," while one respondent chose "Yes, in UAI 2022." From the remaining 155 respondents, 14 were unsure and the remaining majority said no.

Six respondents provided free-text responses relating their experiences with dishonest behavior, mainly describing two distinct behaviors. Some respondents believed that the absence of anonymity in the discussion phase possibly resulted in the disclosure of all reviewers' identities to the authors of the paper under review, possibly due to an undeclared connection between one of the reviewers and the authors. Beyond the inherent issue of compromising double-anonymity between reviewers and authors, this breach has been known to escalate to coercion, with authors pressuring reviewers to provide favorable evaluations of their submissions (Resnik et al., 2008).

Secondly, some respondents noted that renowned researchers have in the past imposed their identity and stature on other reviewers to increase the influence of their review in the discussion phase.

### 7.4.8 Free-text comments

The survey respondents were also given the option of providing free-text comments under the question,

> **Final comments.** Any additional thoughts on whether reviewer identities should be shown to other reviewers? Do you have any relevant experience to share (in UAI 2022 or elsewhere)?

Out of the provided comments, several were more general comments for the program chairs which we have conveyed to the program chairs, but omit here as they are not relevant to the topic of this paper. Some other comments repeated their responses to the other questions, and we also omit them. We summarize the remaining relevant comments covering various aspects below:

- **Discussion quality.** One respondent noted that hiding reviewer identities helps focus the discussion on the review content. However, opposing perspectives were shared by other respondents who argued that showing identities incentivizes better-quality review writing, as the reviewer may be appreciated for it by their colleagues. Additionally, transparency in identities contributes to more meaningful and effective discussions by revealing reviewers' backgrounds. It also facilitates transfer of reviewing know-hows from senior to junior reviewers. Lastly, a respondent suggested that non-anonymity may benefit reviewers by fostering more connections in the research community.
- **Dishonest behavior.** Respondents raised concerns about non-anonymity in discussion phase leading to breaking of reviewer-author anonymity in different ways. One concern involves authors who are also acting as reviewers of other papers, and have co-reviewers that are also reviewing the authors' paper. Here, it possible that the authors deduce the identities of their paper's reviewers by comparing writing styles with their co-reviewers whose identity is visible to them. In another scenario discussed, a reviewer with an undeclared conflict of interest could potentially disclose the other reviewers' identities to the authors, possibly leading to reviewer coercion in favour of the paper, explicitly in the non-anonymous setting and implicitly otherwise. Here the respondent added that implicit coercion via manipulating the discussion itself would be easier in the anonymous condition. To address such concerns a respondent suggested having a policy guaranteeing protection for whistleblowers, in case of reported fraud in peer review.
- **Implementation.** Several respondents emphasized the importance of timely and clear communication, coupled with strict enforcement of anonymity policies in discussions. One respondent cited an instance where, despite having an anonymous discussion setting, a meta-reviewer disclosed a reviewers' identity to others in their exchanges. To potentially have the benefits of both settings, some respondents proposed a policy wherein reviewers' identities are revealed to each other after the conclusion of the discussion phase.
- **Adherence.** Certain respondents pointed out that hostile confrontations can occur in both

settings. Interestingly, even in anonymous settings, well-known researchers have been known to reveal their identity to overtly influence the discussion in their favour.

## 7.5 Discussion

To improve peer review and scientific publishing in a principled manner, it is important to understand the quantitative effects of the policies in place, and design policies in turn based on these quantitative measurements. In this work, we focus on the peer-review policy regarding anonymity of reviewers to each other during the discussion phase.

This work provides crucial evidence, based on data from the experiment in UAI 2022, that there are some potentially adverse effects of removing anonymity between reviewers in paper discussions. Revealing reviewers' identities to each other leads to lower engagement in the discussions and leads to undue influence of senior reviewers on the final decision. Some anecdote-based arguments in favour of showing reviewers' identities have focused on its importance for maintaining politeness of discussions among reviewers. However, in the scope of our experiment, we find that there is no significant difference in the politeness of reviewers' discussions across the two conditions. Notably, 7% of survey respondents reported having witnessed dishonest practices due to non-anonymity among reviewers, which were supported by provided anecdotes.

To conduct a complete investigation of possible impacts of anonymity policies in reviewer discussions, we collect reviewers' perspectives on this issue via anonymous surveys in UAI 2022. While the responses reveal a small difference in participants' preference over the two conditions, they also indicate no significant different in participants' experiences in the two conditions, across dimensions such as comfort, effectiveness and politeness. We hope that conferences and journals take these findings into account when designing policies for their peer review processes, and invite other publication venues to conduct similar experiment.

It is interesting to view our study in the context of previous research on the outcomes of panel-based discussions in grant review. In controlled experiments conducted in non-anonymous settings, Obrecht et al. (2007); Fogelholm et al. (2012); Pier et al. (2017) found that group discussions led to a significant increase in the inconsistency of decisions. Synthesizing these findings with our experiment's outcomes suggests that the inconsistency in decisions in non-anonymous settings may stem from biases in each group's decision-making, potentially resulting in divergent outcomes across different groups.

We now discuss some limitations of our study. The surveys administered in UAI 2022 were anonymous with a response rate of roughly 20%, which brings the possibility of selection bias in the results. However, it is important to note that our observed response rate aligns reasonably with response rates commonly observed in surveys within the field of Computer Science. For example, surveys by Rastogi et al. (2022b) in the NeurIPS 2021 conference saw response rates of 10-25%. Rastogi et al. (2022d) conducted multiple surveys: an anonymous survey in the ICML 2021 and EC 2021 conferences had response rates of 16% and 51% respectively; a second, non-anonymous opt-in survey in EC 2021 had a response rate of 55.78%. The survey by (Gardner et al., 2012) was opt-in in 2011 and their response rate was 28%.

As briefly mentioned in Section 7.3, reviewers were informed at the beginning of the review

process about our experiment regarding anonymity of discussions, with specific details withheld to mitigate the Hawthorne effect. However, it is possible that reviewers were hesitant to let other reviewers' opinions affect their opinion as that could be undesirably perceived as tied to the knowledge of reviewers' identities. Next, we utilize a binary classification of reviewers into seniors and juniors based on their recent professional status. However, this categorization may be overly quantized and hence combine reviewers with a diverse range of expertise.

# Part III

# Understanding and Supporting Human Collaboration with Machine Learning

# Chapter 8

# Deciding Fast and Slow: The Role of Cognitive Biases in AI-assisted Decision-Making

Based on (Rastogi et al., 2022f):
Charvi Rastogi, Yunfeng Zhang, Dennis Wei, Kush R. Varshney, Amit Dhurandhar, and Richard Tomsett. Deciding Fast and Slow: The Role of Cognitive Biases in AI-assisted Decision-Making. Proceedings of *ACM Human Computer Interaction*, 6(CSCW1), Apr 2022

## 8.1 Introduction

It should be a truth universally acknowledged that a human decision-maker in possession of an AI model must be in want of a collaborative partnership. Recently, we have seen a rapid increase in the deployment of machine learning (ML) models in decision-making systems, where the AI models serve as helpers to human experts in many high-stakes settings. Examples of such tasks can be found in healthcare, financial loans, criminal justice, job recruiting, and fraud monitoring. Specifically, judges use risk assessments to determine criminal sentences, banks use models to manage credit risk, and doctors use image-based ML predictions for diagnosis, to list a few.

The emergence of AI-assisted decision-making in society has raised questions about whether and when to rely on the AI model's decisions. These questions can be viewed as problems of communication between AI and humans, and research in interpretable, explainable, and trustworthy machine learning as efforts to improve aspects of this communication. However, a key component of human-AI communication that is often sidelined is the human decision-makers themselves. Humans' perception of the communication received from AI is at the core of this communication gap. Research in communication exemplifies the need to model receiver characteristics, thus implying the need to understand and account for human cognition in collaborative decision-making.

As a step towards studying human cognition in AI-assisted decision-making, our work fo-

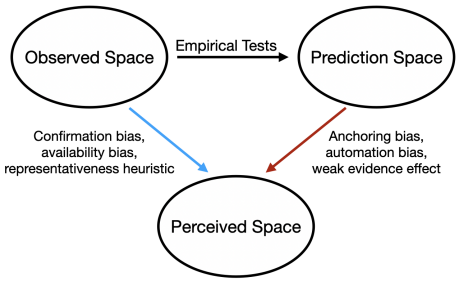



Figure 8.1: Three constituent spaces to capture different interactions in human-AI collaboration. The interactions of the perceived space, representing the human decision-maker, with the observed space and the prediction space may lead to cognitive biases. The definition of the different spaces is partially based on ideas of Yeom and Tschantz (2018).

cuses on the role of cognitive biases in this setting. Cognitive biases, introduced in the seminal work by Tversky and Kahneman (1974a), represent a systematic pattern of deviation from rationality in judgment wherein individuals create their own "subjective reality" from their perception of the input. An individual's perception of reality, not the objective input, may dictate their behavior in the world, thus, leading to distorted and inaccurate judgment. While cognitive biases and their effects on decision-making are well known and widely studied, we note that AI-assisted decision-making presents a new decision-making paradigm and it is important to study their role in this new paradigm, both analytically and empirically.

Our first contribution is illustrated partially in Figure 8.1. In a collaborative decision-making setting, we define the perceived space to represent the human decision-maker. Here, we posit that there are two interactions that may lead to cognitive biases in the perceived space – (1) interaction with the observed space which consists of the feature space and all the information the decision-maker has acquired about the task, (2) interaction with the prediction space representing the output generated by the AI model, which could consist of the AI decision, explanation, etc. In Figure 8.1, we associate cognitive biases that affect the decision-makers' priors and their perception of the data available with their interaction with the observed space. Confirmation bias, availability bias, the representativeness heuristic, and bias due to selective accessibility of the feature space are mapped to the observed space. On the other hand, anchoring bias and the weak evidence effect are mapped to the prediction space. Based on this categorization of biases, we provide a model for some of these biases using our biased Bayesian framework.

To focus our work in the remainder of the paper, we study and provide mitigating strategies for anchoring bias in AI-assisted decision-making, wherein the human decision-maker forms a skewed perception due to an anchor (AI decision) available to them, which limits the exploration of alternative hypotheses. Anchoring bias manifests through blind reliance on the anchor. While poorly calibrated reliance on AI has been studied previously from the lens of trust (Zhang et al., 2020; Tomsett et al., 2020; Okamura and Yamada, 2020), in this work we analyse reliance miscalibration due to anchoring bias which has a different mechanism and, hence, different mitigating strategies.

Tversky and Kahneman (1974a) explained that anchoring bias manifests through the anchoring-

and-adjustment heuristic wherein, when asked a question and presented with any anchor, people adjust away insufficiently from the anchor. Building on the notion of bounded rationality, previous work (Lieder et al., 2018) attributes the adjustment strategy to a resource-rational policy wherein the insufficient adjustment is a rational trade-off between accuracy and time. To test this in our setting, we conduct an experiment with human participants on Amazon Mechanical Turk to study whether allocating more resources — in this case, time — alleviates anchoring bias. This bias manifests through the rate of agreement with the AI prediction. Thus, by measuring the rate of agreement with the AI prediction in several carefully designed trials, we validate that time indeed is a useful resource that helps the decision-maker sufficiently adjust away from the anchor when needed. We note that the usefulness of time in remediating anchoring bias is an intuitive idea discussed in previous works (Tversky and Kahneman, 1974a), but one that has not been empirically validated to our knowledge. Thus, it is necessary to confirm this finding experimentally, especially in the AI-assisted setting.

As our first experiment confirms that more time helps reduce bounded-rational anchoring to the AI decision, one might suggest that giving more time to all decisions should yield better decision-making performance from the human-AI team. However, this solution does not utilize the benefits of high-quality AI decisions available in many cases. Moreover, it does not account for the limited availability of time. Thus, we formulate a novel resource (time) allocation problem that factors in the effects of anchoring bias and the variance in AI accuracy to maximize human-AI collaborative accuracy. We propose a time allocation policy and prove its optimality under some assumptions. We also conduct a second user experiment to evaluate human-AI team performance under this policy in comparison with several baseline policies. Our results show that while the overall performance of all policies considered is roughly the same, our policy helps the participants de-anchor from the AI prediction when the AI is incorrect and has low confidence.

The time allocation problem that we study is motivated by real-world AI-assisted decision-making settings. Adaptively determining the time allocated to a particular instance for the best possible judgement can be very useful in multiple applications. For example, consider a (procurement) fraud monitoring system deployed in multinational corporations, which analyzes and flags high-risk invoices (Dhurandhar et al., 2015). Given the scale of these systems, which typically analyze tens of thousands of invoices from many different geographies daily, the number of invoices that may be flagged, even if a small fraction, can easily overwhelm the team of experts validating them. In such scenarios, spending a lot of time on each invoice is not admissible. An adaptive scheme that takes into account the biases of the human and the expected accuracy of the AI model is highly desirable to produce the most objective decisions. Our work is also applicable to the other aforementioned domains, such as in criminal proceedings where judges have to look over many different case documents and make quick decisions, often in under a minute.

In summary, we make the following contributions:

- We provide a biased Bayesian framework for modeling biased AI-assisted decision making. Based on the source of the cognitive biases, we situate some well-known cognitive biases within our framework.
- Focusing on anchoring bias in AI-assisted decision-making, we show with human participants that allocating more time to a decision reduces anchoring in this setting.
- We formulate a time allocation problem to maximize human-AI team accuracy that ac-

counts for the anchoring-and-adjustment heuristic and the variance in AI accuracy.

- We propose a confidence-based allocation policy and identify conditions under which it achieves optimal team performance.
- Through a carefully designed human subject experiment, we evaluate the real-world effectiveness of the confidence-based time allocation policy, showing that when confidence-based information is displayed, it helps humans de-anchor from incorrect and low-confidence AI predictions.

## 8.2 Related work

Cognitive biases are an important factor in human decision-making (Barnes JR., 1984; Das and Teng, 1999; Ehrlinger et al., 2016), and have been studied widely in decision-support systems research (Arnott, 2006; Zhang et al., 2015; Solomon, 2014; Phillips-Wren et al., 2019). Cognitive biases also show up in many aspects of collaborative behaviours (Silverman, 1992; Janssen and Kirschner, 2020; Bromme et al., 2010). More specifically, there exists decades-old research on cognitive biases (Tversky and Kahneman, 1974a) such as confirmation bias (Nickerson, 1998; Klayman, 1995; Oswald and Grosjean, 2004), anchoring bias (Furnham and Boo, 2011; Epley and Gilovich, 2006; 2001), automation bias (Lee and See, 2004), availability bias (Tversky and Kahneman, 1973), etc.

Recently, as AI systems are increasingly embedded into high stakes human decisions, understanding human behavior, and reliance on technology have become critical, "Poor partnerships between people and automation will become increasingly costly and catastrophic" (Lee and See, 2004). This concern has sparked crucial research in several directions, such as human trust in algorithmic systems, interpretability, and explainability of machine learning models (Arnold et al., 2019; Zhang et al., 2020; Tomsett et al., 2020; Siau and Wang, 2018; Doshi-Velez and Kim, 2017; Lipton, 2018; Adadi and Berrada, 2018; Preece, 2018).

In parallel, research in AI-assisted decision-making has worked on improving the human-AI collaboration (Lai et al., 2020; Lai and Tan, 2019; Bansal et al., 2021b; 2019; Green and Chen, 2019; Okamura and Yamada, 2020). These works experiment with several heuristic-driven AI explanation techniques that do not factor in all the characteristics of the human at the end of the decision-making pipeline. Specifically, the experimental results in (Bansal et al., 2021b) show that explanations supporting the AI decision tend to exacerbate over-reliance on the AI decision. In contrast, citing a body of research in psychology, philosophy, and cognitive science, Miller (Miller, 2019) argues that the machine learning community should move away from imprecise, subjective notions of "good" explanations and instead focus on reasons and thought processes that people apply for explanation selection. In agreement with Miller, our work builds on literature in psychology on cognitive biases to inform modeling and effective de-biasing strategies. Our work provides a structured approach to addressing problems, like over-reliance on AI, from a cognitive science perspective. In addition, we adopt a two-step process, wherein we inform our subsequent de-biasing approach (Experiment 2) based on the results of our first experiment, thus, paving the pathway for experiment-driven human-oriented research in this setting.

Work on cognitive biases in human-AI collaboration is still rare, however. Recently, Fürnkranz

et al. (2020) evaluated a selection of cognitive biases to test whether minimizing the complexity or length of a rule yields increased interpretability of machine learning models. Kliegr et al. (2018) review twenty different cognitive biases that affect the interpretability and associated debiasing techniques. Both these works (Fürnkranz et al., 2020; Kliegr et al., 2018) are specific to rule-based ML models. Baudel et al. (2020) address complacency/authority bias in using algorithmic decision aids in business decision processes. Wang et al. (2019) propose a conceptual framework for building explainable AI based on the literature on cognitive biases. Building on these works, our work provides novel mathematical models for the AI-assisted setting to identify the role of cognitive biases. Contemporaneously, Buçinca et al. (2021) studies the use of cognitive forcing functions to reduce over-reliance in human-AI collaboration.

The second part of our work focuses on anchoring bias. The phenomenon of anchoring bias in AI-assisted setting has also been studied as a part of automation bias (Lee and See, 2004) wherein the users display over-reliance on AI due to blind trust in automation. Previous experimental research has also shown that people do not calibrate their reliance on AI based on its accuracy (Green and Chen, 2019). Several studies suggest that people are unable to detect algorithmic errors (Poursabzi-Sangdeh et al., 2018), are biased by irrelevant information (Englich et al., 2006), rely on algorithms that are described as having low accuracy, and trust algorithms that are described as accurate but present random predictions (Springer et al., 2018). These behavioural tendencies motivate a crucial research question — how to account for these heuristics, often explained by cognitive biases such as anchoring bias. In this direction, our work is the first to empirically and analytically study a time-based de-biasing strategy to remediate anchoring bias in the AI-assisted setting.

Lastly, the work by Park et al. (Park et al., 2019) considers the approach of forcing decision-makers to spend more time deliberating their decision before the AI prediction is provided to them. In this work, we consider the setting where the AI prediction is provided to the decision-maker beforehand which may lead to anchoring bias. Moreover, we treat time as a limited resource and accordingly provide optimal allocation strategies.

## 8.3 Problem setup and modeling

We consider a collaborative decision-making setup, consisting of a machine learning algorithm and a human decision-maker. First, we precisely describe our setup and document the associated notation. Following this, we provide a Bayesian model for various human cognitive biases induced by the human-AI collaborative process.

Our focus in this paper is on the AI-assisted decision-making setup, wherein the objective of the human is to correctly classify the set of feature information available into one of two categories. Thus, we have a binary classification problem, where the true class is denoted by $y^* \in \{0, 1\}$. To make the decision/prediction, the human is presented with feature information, and we denote the complete set of features available pertaining to each sample by $D$. In addition to the feature information, the human is also shown the output of the machine learning algorithm. Here, the AI output could consist of several parts, such as the prediction, denoted by $\widehat{y} \in \{0, 1\}$, and the machine-generated explanation for its prediction. We express the complete AI output as a function of the machine learning model, denoted by $f(M)$. Finally, we denote the decision

made by the human decision-maker by $\tilde{y} \in \{0, 1\}$.

We now describe the approach towards modeling the behavior of human decision-makers when assisted by machine learning algorithms.

### 8.3.1 Bayesian decision-making

Bayesian models for human cognition have become increasingly prominent across a broad spectrum of cognitive science (Tenenbaum, 1999; Griffiths and Tenenbaum, 2006; Chater et al., 2006). The Bayesian approach is thoroughly embedded within the framework of decision theory. Its basic tenets are that opinions should be expressed in terms of subjective or personal probabilities, and that the optimal revision of such opinions, in the light of relevant new information, should be accomplished via Bayes' theorem.

First, consider a simpler setting, where the decision-maker uses the feature information available, $D$, and makes a decision $\tilde{y} \in \{0, 1\}$. Let the decision variable be denoted by $\tilde{Y}$. Based on literature in psychology and cognitive science (Griffiths and Tenenbaum, 2006; Chater et al., 2006), we model a rational decision-maker as Bayes' optimal. That is, given a prior on the likelihood of the prediction, $\mathbb{P}_{pr}(\tilde{Y})$ and the data likelihood distribution $\mathbb{P}(D|\tilde{Y})$, the decision-maker picks the hypothesis/class with the higher posterior probability. Formally, the Bayes' theorem states that

$$\mathbb{P}(\tilde{Y} = i|D) = \frac{\mathbb{P}(D|\tilde{Y} = i)\mathbb{P}_{pr}(\tilde{Y} = i)}{\sum_{j\in\{0,1\}} \mathbb{P}(D|\tilde{Y} = j)\mathbb{P}_{pr}(\tilde{Y} = j)}, \tag{8.1}$$

where $i \in \{0, 1\}$ and the human decision is given by $\tilde{y} = \arg\max_{i\in\{0,1\}} \mathbb{P}(\tilde{Y} = i|D)$. Now, in our setting, in addition to the feature information available, the decision-maker takes into account the output of the machine learning algorithm, $f(M)$, which leads to following Bayes' relation

$$\mathbb{P}(\tilde{Y}|D, f(M)) \propto \mathbb{P}(D, f(M)|\tilde{Y})\mathbb{P}_{pr}(\tilde{Y}). \tag{8.2}$$

We assume that conditioned on the decision-maker's decision $\tilde{Y}$, they perceive the feature information and the AI output independently, which gives

$$\mathbb{P}(\tilde{Y}|D, f(M)) \propto \mathbb{P}(D|\tilde{Y})\mathbb{P}(f(M)|\tilde{Y})\mathbb{P}_{pr}(\tilde{Y}), \tag{8.3}$$

where $\mathbb{P}(f(M)|\tilde{Y})$ indicates the conditional probability of the AI output perceived by the decision-maker. The assumption in (8.3) is akin to a naive Bayes' assumption of conditional independence, but we only assume conditional independence between $D$ and $f(M)$ and not between components within $D$ or components within $f(M)$. This concludes our model for a rational decision-maker assisted by a machine learning model.

In reality, the human decision-maker may behave differently from a fully rational agent due to their cognitive biases. In some studies (Matsumori et al., 2018; Payzan-LeNestour and Bossaerts, 2011; 2012), such deviations have been explained by introducing exponential biases (i.e. inverse temperature parameters) on Bayesian inference because these were found useful in expressing

bias levels. We augment the modeling approach in (Matsumori et al., 2018) to a human-AI collaborative setup. Herein we model the biased Bayesian estimation as

$$\mathbb{P}(\tilde{Y}|D, f(M)) \propto \mathbb{P}(D|\tilde{Y})^{\alpha}\mathbb{P}(f(M)|\tilde{Y})^{\beta}\mathbb{P}_{pr}(\tilde{Y})^{\gamma}, \tag{8.4}$$

where $\alpha, \beta, \gamma$ are variables that represent the biases in different factors in the Bayesian inference.

Equation (8.4) allows us to understand and model several cognitive biases arising in AI-assisted decision making. To facilitate the following discussion, we take the ratio between (8.4) evaluated for $\tilde{Y} = 1$ and (8.4) for $\tilde{Y} = 0$:

$$\frac{\mathbb{P}(\tilde{Y} = 1|D, f(M))}{\mathbb{P}(\tilde{Y} = 0|D, f(M))} = \left(\frac{\mathbb{P}(D|\tilde{Y} = 1)}{\mathbb{P}(D|\tilde{Y} = 0)}\right)^{\alpha} \left(\frac{\mathbb{P}(f(M)|\tilde{Y} = 1)}{\mathbb{P}(f(M)|\tilde{Y} = 0)}\right)^{\beta} \left(\frac{\mathbb{P}_{pr}(\tilde{Y} = 1)}{\mathbb{P}_{pr}(\tilde{Y} = 0)}\right)^{\gamma}. \tag{8.5}$$

The human decision is thus $\tilde{Y} = 1$ if the ratio is greater than 1 and $\tilde{Y} = 0$ otherwise. The final ratio is a product of the three ratios on the right-hand side raised to different powers. We can now state the following:

1. In anchoring bias, the weight put on AI prediction is high, i.e., $\beta > 1$ and the corresponding ratio in (8.5) contributes more to the final ratio, whereas the weight on prior and data likelihood reduces.
2. By contrast, in confirmation bias the weight on the prior is high, $\gamma > 1$, and the weight on the data and machine prediction reduces in comparison.
3. Selective accessibility is a phenomena used to explain the mechanism of cognitive biases wherein the data that supports the decision-maker is selectively used as evidence, while the rest of the data is not considered. This distorts the data likelihood factor in (8.4). The direction of distortion $\alpha > 1$ or $\alpha < -1$ depends on the cognitive bias driven decision.
4. The weak evidence effect (Fernbach et al., 2011) suggests that when presented with weak evidence for a prediction, the decision-maker would tend to choose the opposite prediction. This effect is modeled with $\beta < -1$.

To focus our approach, we consider a particular cognitive bias — anchoring bias, which is specific to the nature of human-AI collaboration and has been an issue in previous works (Lai and Tan, 2019; Springer et al., 2018; Bansal et al., 2021b). In the next section, we summarise the findings about anchoring bias in the literature, explain proposed de-biasing technique and conduct an experiment to validate the technique.

## 8.4 Anchoring bias

AI-assisted decision-making tasks are prone to anchoring bias, where the human decision-maker is irrationally anchored to the AI-generated decision. The anchoring-and-adjustment heuristic, introduced by Tversky and Kahneman in (Tversky and Kahneman, 1974a) and studied in (Epley and Gilovich, 2006; Lieder et al., 2018) suggests that after being anchored, humans tend to adjust insufficiently because adjustments are effortful and tend to stop once a plausible estimate is reached. Lieder et al. (Lieder et al., 2018) proposed the resource rational model of

anchoring-and-adjustment which explains that the insufficient adjustment can be understood as a rational trade-off between time and accuracy. This is a consequence of the bounded rationality of humans (Simon, 1956; 1972), which entails satisficing, that is, accepting sub-optimal solutions that are good enough, rather than optimizing solely for accuracy. Through user studies, Epley et al. (Epley and Gilovich, 2006) argue that cognitive load and time pressure are contributing factors behind insufficient adjustments.

Informed by the above works viewing anchoring bias as a problem of insufficient adjustment due to limited resources, we aim to mitigate the effect of anchoring bias in AI-assisted decision-making, using time as a resource. We use the term de-anchoring to denote the rational process of adjusting away from the anchor. With this goal in mind, we conducted two user studies on Amazon Mechanical Turk. Through the first user study (Experiment 1), we aim to understand the effect of different time allocations on anchoring bias and de-anchoring in an AI-assisted decision-making task. In Experiment 2, we use the knowledge obtained about the effect of time in Experiment 1 to design a time allocation strategy and test it on the experiment participants.

We now describe Experiment 1 in detail.

### 8.4.1 Experiment 1

In this study, we asked the participants to complete an AI-assisted binary prediction task consisting of a number of trials. Our aim is to learn the effect of allocating different amounts of time to different trials on participants with anchoring bias.

To quantify anchoring bias and thereby the insufficiency of adjustments, we use the probability $\mathbb{P}(\tilde{y} = \widehat{y})$ that the human decision-maker agrees with the AI prediction $\widehat{y}$, which is easily measured. This measure can be motivated from the biased Bayesian model in (8.4). In the experiments, the model output $f(M)$ consists of only a predicted label $\widehat{y}$. In this case, (8.4) becomes

$$\mathbb{P}(\tilde{Y} = y|D, f(M)) \propto \mathbb{P}(D|\tilde{Y} = y)^{\alpha}\mathbb{P}(\widehat{Y} = \widehat{y}|\tilde{Y} = y)^{\beta}\mathbb{P}_{pr}(\tilde{Y} = y)^{\gamma}. \tag{8.6}$$

Let us make the reasonable assumption that the decision-maker's decision $\tilde{Y}$ positively correlates with the AI prediction $\widehat{Y}$, specifically that the ML model's probability $\mathbb{P}(\widehat{Y} = \widehat{y}|\tilde{Y} = y)$ is larger when $y = \widehat{y}$ than when $y \neq \widehat{y}$. Then as the exponent $\beta$ increases, i.e., as anchoring bias strengthens, the likelihood that $y = \widehat{y}$ maximizes (8.6) and becomes the human decision $\tilde{y}$ also increases. In the limit $\beta \to \infty$, we have agreement $\tilde{y} = \widehat{y}$ with probability 1. Conversely, for $\beta = 1$, the two other factors in (8.6) are weighed appropriately and the probability of agreement assumes a natural baseline value. We conclude that the probability of agreement is a measure of anchoring bias. It is also important to ensure that this measure is based on tasks where the human has reason to choose a different prediction.

Thus, given the above relationship between anchoring bias and agreement probability (equivalently disagreement probability), we tested the following hypothesis to determine whether time is a useful resource in mitigating anchoring bias:

- Hypothesis 1 (H1): Increasing the time allocated to a task alleviates anchoring bias, yielding a higher likelihood of sufficient adjustment away from the AI-generated decision when the decision-maker has the knowledge required to provide a different prediction.

**Participants.** We recruited 47 participants from Amazon Mechanical Turk for Experiment 1, limiting the pool to subjects from within the United States with a prior task approval rating of at least 98% and a minimum of 100 approved tasks. 10 participants were between Age 18 and 29, 26 between Age 30 and 39, 6 between Age 40 and 49, and 5 over Age 50. The average completion time for this user study was 27 minutes, and each participant received compensation of $4.5 (roughly equals an hourly wage of $10). The participants received a base pay of $3.5 and a bonus of $1 (to incentivize accuracy).

**Task and AI model.** We designed a performance prediction task wherein a participant was asked to predict whether a student would pass or fail a class, based on the student's characteristics, past performance, and some demographic information. The dataset for this task was obtained from the UCI Machine Learning Repository, published as the Student Performance Dataset (Cortez and Silva, 2008). This dataset contains 1044 instances of students' class performances in 2 subjects (Mathematics and Portuguese), each described by 33 features. To prepare the dataset, we binarized the target labels ('pass', 'fail'), split the dataset into training and test sets (70/30 split). To create our AI, we trained a logistic regression model on the standardized set of features from the training dataset. Based on the feature importance (logistic regression coefficients) assigned to each feature in the dataset, we retained the top 10 features for the experiments. These included — mother's and father's education, mother's and father's jobs, hours spent studying weekly, interest in higher education, hours spent going out with friends weekly, number of absences in the school year, enrolment in extra educational support, and number of past failures in the class.

**Study procedure** Since we are interested in studying decision-makers' behavior when humans have prior knowledge and experience in the prediction task, we first trained our participants before collecting their decision data for analysis. The training section consists of 15 trials where the participant is first asked to provide their prediction based on the student data and is then shown the correct answer after attempting the task. These trials are the same for all participants and are sampled from the training set such that the predicted probability (of the predicted class) estimated by the AI model is distributed uniformly over the intervals $[0.5, 0.6], (0.6, 0.7], \cdots, (0.9, 1]$. Taking predicted probability as a proxy for difficulty, this ensures that all levels of difficulty are represented in the task. To help accelerate participants' learning, we showed bar charts that display the distributions of the outcome across the feature values of each feature. These bar charts were not provided in the testing section to ensure stable performance throughout the testing section and to emulate a real-world setting.

To induce anchoring bias, the participant was informed at the start of the training section that the AI model was $85\%$ accurate (we carefully chose the training trials to ensure that the AI was indeed $85\%$ accurate over these trials), while the model's actual accuracy is $70.8\%$ over the entire training set and $66.5\%$ over the test set. Since our goal is to induce anchoring bias and the training time is short, we stated a high AI accuracy. Moreover, this disparity between stated accuracy ($85\%$) and true accuracy ($70.8\%$) is realistic if there is a distribution shift between the training and the test set, which would imply that the humans' trust in AI is misplaced. In addition to stating AI accuracy at the beginning, we informed the participants about the AI prediction for

each training trial after they have attempted it so that they can learn about AI's performance first-hand.

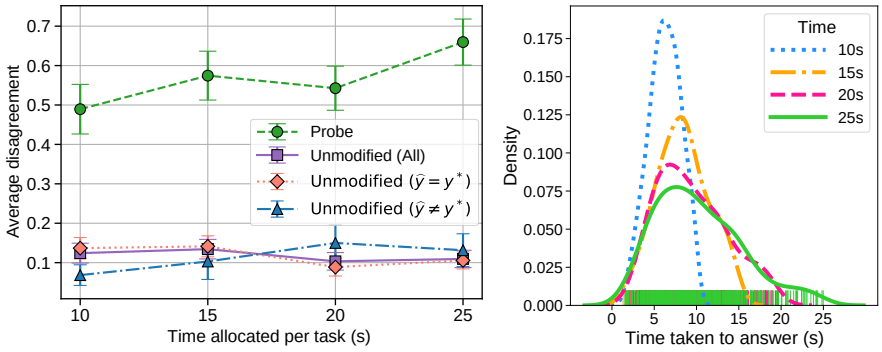
The training section is followed by the testing section which consists of 36 trials sampled from the test set and was kept the same (in the same order) for all participants. In this section, the participants were asked to make a decision based on both the student data and AI prediction. They were also asked to describe their confidence level in their prediction as low, medium, or high.

To measure the de-anchoring effect of time, we included some trials where the AI is incorrect but the participants have the requisite knowledge to adjust away from the incorrect answer. That is, we included trials where the participants' accuracy would be lower when they are anchored to the AI prediction than when they are not. We call these trials — *probe trials*, which help us probe the effect of time on de-anchoring. On the flip side, we could not include too many of these trials because participants may lose their trust in the AI if exposed to many apparently incorrect AI decisions. To achieve this balance, we sampled 8 trials of medium difficulty where the AI prediction is accurate (predicted probability ranging from $0.6$ to $0.8$) and flip the AI prediction shown to the participants. The remaining trials, termed *unmodified trials* are sampled randomly from the test set while maintaining a uniform distribution over the AI predicted probability (of the predicted class). Here, again, we use the predicted probability as a proxy for the difficulty of the task, as evaluated by the machine learning model. We note that the accuracy of the AI predictions *shown* to the participants is $58.3\%$ which is far lower than the $85\%$ accuracy shown in the training section.

**Time allocation.** To investigate the effect of time on the anchoring-and-adjustment heuristic in AI-assisted decision-making, we divide the testing section into four blocks for each participant based on the time allocation per trial. To select the allocated time intervals, we first conducted a shorter version of the same study to learn the amount of time needed to solve the student performance prediction task, which suggested that the time intervals of $10$s, $15$s, $20$s and $25$s captured the range from necessary to sufficient. Now, with these four time intervals, we divided the testing section into four blocks of 9 trials each, where the time allocated per trial in each block followed the sequence $[t_1, t_2, t_3, t_4]$ and for each participant this sequence was a random permutation of $[10, 15, 20, 25]$. The participants were not allowed to move to the next trial till the allocated time ran out. Furthermore, each block was comprised of 2 probe trials and 7 unmodified trials, randomly ordered. Recall that each participant was provided the same set of trials in the same order.

Now, with the controlled randomization of the time allocation, independent of the participant, their performance, and the sequence of the tasks, we are able to identify the effect of time on de-anchoring. It is possible that a participant that disagrees with the AI prediction often in the first half, is not anchored to the AI prediction in the latter half. Our study design allows us to average out such participant-specific effects, through the randomization of time allocation interval sequences across participants.

The main results of Experiment 1 are illustrated in Figure 8.2(a). We see that the probe trials served their intended purpose, since the average disagreement is much higher for probe trials compared to unmodified trials for all time allocations. This suggests that the participants had




(a) (b)

Figure 8.2: Results of experiment 1. **(a)** Average disagreement with the AI prediction for different time allocations in experiment 1. **(b)** Distribution of time taken by the participants to provide their answer under the four different time conditions, $\{10,15,20,25\}$ seconds in Experiment 1. For illustration purposes, we use kernel density estimation to estimate the probability density function shown. The actual answering time lies between 0 and 25 seconds.

learned to make accurate predictions for this task, otherwise they would not be able to detect the AI's errors in the probe trials, more so in the 10-second condition. We also observe that the likelihood of disagreement for unmodified trials is low (close to 0.1) for all time allocations. This suggests that the participants' knowledge level in this task is roughly similar to or less than that of the AI since the participants are unable to offer any extra knowledge in the unmodified trials.

**Anchoring-and-adjustment.** The results on the probe trials in Figure 8.2(a) suggest that the participants' likelihood of sufficiently adjusting away from the incorrect AI prediction increased as the time allocated increased. This strengthens the argument that the anchoring-and-adjustment heuristic is a resource-rational trade-off between time and accuracy (Lieder et al., 2018). Specifically, we observe that the average disagreement percentage in probe trials increased from $48\%$ in the 10-second condition to $67\%$ in the 25-second condition. We used the bootstrap method with 5000 re-samples to estimate the coefficient of a linear regression fit on average disagreement vs. time allocated for probe trials. This resulted in a significantly positive coefficient of $0.01$ (bootstrap $95\%$ confidence interval $[0.001, 0.018]$). This result is consistent with our Hypothesis 1 (H1) that increasing time for decision tasks alleviates anchoring bias. We note that the coefficient is small in value because the scales of the independent and dependent variables of the regression (time and average disagreement) have not been adjusted for the regression, so the coefficient of $0.01$ yields a $0.15$ increase in average disagreement between the 10s and the 25s time condition.

**Time adherence.** Figure 8.2(b) suggests that the participants adhere reasonably to the four different time conditions used. We note that this time reflects the maximum time taken to click the radio button (in case of multiple clicks), but the participants may have spent more time thinking over their decision. In the survey at the end of the study, we asked the participants

how often they used the entire time available to them in the trials, and obtained the following distribution of answers — Frequently 15, Occasionally 24, Rarely 6, Never 2.

## 8.5 Optimal resource allocation in human-AI collaboration

In Section 8.4, we see that time is a useful resource for de-anchoring the decision-maker. More generally, there are many works that study de-biasing techniques to address the negative effects of cognitive biases. These de-biasing techniques require resources such as time, computation and explanation strategies. Thus, in this section, we model the problem of mitigating the effect of cognitive biases in the AI-assisted decision-making setting as a resource allocation problem, where our aim is to efficiently use the resources available and improve human-AI collaboration accuracy.

### 8.5.1 Resource allocation problem

From Experiment 1 in Section 8.4.1, we learnt that given more time, the decision-maker is more likely to adjust away from the anchor (AI decision) if the decision-maker has reason to believe that the correct answer is different. This is shown by change in their probability of agreement with the AI prediction, denoted by $P_a = \mathbb{P}(\tilde{y} = \widehat{y})$. The results of Experiment 1 indicate that, ideally, decision-makers should be provided with ample time for each decision. However, in practice, given a finite resource budget $T$, we also have the constraint $\sum_{i=1}^{N} T_i = T$. Thus, we formulate a resource allocation problem that captures the trade-off between time and accuracy. More generally, this problem suggests a framework for optimizing *human-AI* team performance using constrained resources to de-bias the decision-maker.

In our setup, the human decision-maker has to provide a final decision $\tilde{y}_i$ for $N$ total trials with AI assistance, specifically in the form of a predicted label $\widehat{y}_i$. The objective is to maximize the average accuracy over the trials, denoted by $R$, of the human-AI collaboration: $\mathbb{E}[R] = \frac{1}{N}\sum_{i=1}^{N}\mathbb{E}[R_i]$, where $R_i$ is an indicator of human-AI correctness in trial $i$.

We first relate collaborative accuracy $\mathbb{E}[R]$ to the anchoring-and-adjustment heuristic. Intuitively, if we know the AI to be incorrect in a given trial, we should aim to facilitate adjustment away from the anchor as much as possible, whereas if AI is known to be correct, then anchoring bias is actually beneficial. Based on this intuition, $E[R_i]$ can be rewritten by conditioning on AI correctness/incorrectness as follows:

$$\mathbb{E}[R_i] = \underbrace{\mathbb{P}(\tilde{y}_i = \widehat{y}_i \mid \widehat{y}_i = y_i^*)}_{P^r_{a_i}}\mathbb{P}(\widehat{y}_i = y_i^*) + \Big(1 - \underbrace{\mathbb{P}(\tilde{y}_i = \widehat{y}_i \mid \widehat{y}_i \neq y_i^*)}_{P^w_{a_i}}\Big)\left(1 - \mathbb{P}(\widehat{y}_i = y_i^*)\right). \quad (8.7)$$

We see therefore that human-AI correctness depends on the probability of agreement $P^r_{a_i}$ conditioned on AI being correct and the probability of agreement $P^w_{a_i}$ conditioned on AI being incorrect. Recalling from Section 8.4 the link established between agreement probability and anchoring bias, (8.7) shows the effect of anchoring bias on human-AI accuracy. Specifically in the case of (8.7), the effect is through the two conditional agreement probabilities $P^r_{a_i}$ and $P^w_{a_i}$.

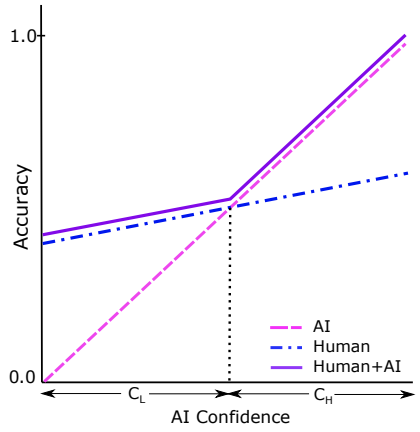



Figure 8.3: An ideal case for human-AI collaboration, where (1) we correctly identify the set of tasks with low and high AI confidence, (2) the AI accuracy is perfectly correlated with its confidence, (3) human accuracy is higher than AI in the low confidence region, $\mathcal{C}_L$, and lower than AI in the high confidence region $\mathcal{C}_H$.

We consider time allocation strategies to modify agreement probabilities and thus improve collaborative accuracy, based on the relationship established in Experiment 1.

We denote the time used in trial $i$ as $T_i$, which impacts correctness $R_i$ (8.7) as follows:

$$\mathbb{E}[R_i \mid T_i] = P^r_{a_i}(T_i)\mathbb{P}(\widehat{y}_i = y^*_i) + P^w_{a_i}(T_i)\left(1 - \mathbb{P}(\widehat{y}_i = y^*_i)\right). \tag{8.8}$$

The allocation of time affects only the human decision-maker, making the agreement probabilities functions of $T_i$, whereas the probability of AI correctness is unaffected. The resource allocation problem would then be to maximize the average of (8.8) over trials subject to the budget constraint $\sum_{i=1}^{n} T_i = T$.

The challenge with formulation (8.8) is that it requires identifying the true probability of AI correctness, which is a non-trivial task (Guo et al., 2017). Instead, we operate under the more realistic assumption that the AI model can estimate its probability of correctness from the class probabilities that it predicts (as provided for example by a logistic regression model). We refer to this estimate as *AI confidence* and denote it as $\widehat{C}_i$. We may then consider a decomposition of human-AI correctness as in (8.7), (8.8) but conditioned on $\widehat{C}_i$. In keeping with the two cases in (8.7), (8.8) and to simplify the allocation strategy, we binarize $\widehat{C}_i$ into two intervals, low confidence $\widehat{C}_i \in \mathcal{C}_L$, and high confidence $\widehat{C}_i \in \mathcal{C}_H$. The time allocated is then $T_i(\widehat{C}_i) = t_L$ for $\widehat{C}_i \in \mathcal{C}_L$ and $T_i(\widehat{C}_i) = t_H$ for $\widehat{C}_i \in \mathcal{C}_H$. Thus we have

$$\mathbb{E}[R_i] = \mathbb{P}(\widehat{C}_i \in \mathcal{C}_L)\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_L, T_i = t_L] + \mathbb{P}(\widehat{C}_i \in \mathcal{C}_H)\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_H, T_i = t_H]. \tag{8.9}$$

The quantities $\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}, T_i]$, $\mathcal{C} = \mathcal{C}_L, \mathcal{C}_H$, are not pure agreement probabilities as in (8.8) because the low/high-confidence events $\widehat{C}_i \in \mathcal{C}_L$, $\widehat{C}_i \in \mathcal{C}_H$ generally differ from the correctness/incorrectness events $\widehat{y}_i = y^*_i$, $\widehat{y}_i \neq y^*_i$. Nevertheless, since we expect these events to be correlated, $\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}, T_i]$ is related to the agreement probabilities in (8.8).

Figure 8.3 presents an ideal scenario that one hopes to attain in (8.9). In presence of anchoring bias, our aim is to achieve the human-AI team accuracy shown. This approach capitalises on human expertise where AI accuracy is low. Specifically, by giving human decision-makers more time, we encourage them to rely on their own knowledge (de-anchor from the AI prediction) when the AI is less confident, $\widehat{C}_i \in \mathcal{C}_L$. Usage of more time in low AI confidence tasks, implies less time in tasks where AI is more confident, $\widehat{C}_i \in \mathcal{C}_H$, where anchoring bias has lower negative effects and is even beneficial. Thus, this two-level AI confidence based time allocation policy allows us to mitigate the negative effects of anchoring bias and achieve the "best of both worlds", as illustrated in Figure 8.3.

We now formally write the assumption under which the optimal time allocation policy is straightforward to see.

**Assumption 1.** For any $t_1, t_2 \in \mathbb{R}^+$, if $t_1 < t_2$, then

$$\begin{aligned} &\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_L, T_i = t_1] \leq \mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_L, T_i = t_2], \text{ and} \\ &\mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_H, T_i = t_1] \geq \mathbb{E}[R_i \mid \widehat{C}_i \in \mathcal{C}_H, T_i = t_2]. \end{aligned} \tag{8.10}$$

We provide explanation for Assumption 1 (8.10) in Appendix A. Under Assumption 1, the optimal strategy is to maximize time for low-AI-confidence trials and minimize time for high-confidence trials, as is stated formally below.

**Proposition 14** *Consider the AI-assisted decision-making setup discussed in this work with $N$ trials where the total time available is $T$. Suppose Assumption 1, stated in (8.10), holds true for human-AI accuracy. Then, the optimal confidence-based allocation is as follows,*

$$T_i = \begin{cases} t_H = t_{\min} & \textit{if} \quad \widehat{C}_i \in \mathcal{C}_H \\ t_L = t_{\max} & \textit{if} \quad \widehat{C}_i \in \mathcal{C}_L, \end{cases} \tag{8.11}$$

*where $t_{\min}$ is the minimum allowable time, and $t_{\max}$ is the corresponding maximum time such that $t_{\max}\mathbb{P}(\widehat{C}_i \in \mathcal{C}_L) + t_{\min}\mathbb{P}(\widehat{C}_i \in \mathcal{C}_H) = \frac{T}{N}$.*

Proposition 14 gives us the optimal time allocation strategy by efficiently allocating more time for adjusting away from the anchor in the tasks that yield a lower probability of accuracy if the human is anchored to the AI predictions. We note that, although in an ideal scenario as shown in Figure 8.3, we should set $t_{\min} = 0$, in real world implementation $\widehat{C}_i$ is an approximation of the true confidence, and hence, it would be helpful to have human oversight with $t_{\min} > 0$ in case the AI confidence is poorly calibrated.

To further understand the optimality of the confidence-based time allocation strategy, we compare it with two baseline strategies that obey the same resource constraint, namely, Constant time and Random time strategies, defined as –

- Constant time: For all $i$, $T_i = \frac{T}{N}$.
- Random time : Out of the $N$ trials, $N \times \mathbb{P}(\widehat{C}_i \in \mathcal{C}_L)$ trials are selected randomly and allocated time $t_L$. The remaining trials are allocated time $t_H$.

Constant time is the most natural baseline allocation, while Random time assigns the same values $t_L$ and $t_H$ as the confidence-based policy but does so at random. Both are evaluated in the experiment described in Section 8.5.2.

### 8.5.2 Experiment 2: Dynamic time allocation for human-AI collaboration

In this experiment, we implement our confidence-based time allocation strategy for human-AI collaboration in a user study deployed on Amazon Mechanical Turk. Based on the results of Experiment 1 shown in Figure 8.2(a), we assign $t_L = 25s$ and $t_H = 10s$.

In addition, we conjecture that giving the decision-maker the reasoning behind the time allocation, that is, informing them about AI confidence and then allocating time accordingly, would help improve the collaboration further. This conjecture is supported by findings in (Chambon et al., 2020), where the authors observe that choice tips the balance of learning: for the same action and outcome, the brain learns differently and more quickly from free choices than forced ones. Thus, providing valid reasons for time allocation would help the decision-maker make an active choice and hence, learn to collaborate with AI better.

In this experiment, we test the following hypotheses.

- H2: Anchoring bias has a negative effect on human-AI collaborative decision-making accuracy when AI is incorrect.
- H3: If the human decision-maker has complementary knowledge then allocating more time can help them sufficiently adjust away from the AI prediction.
- H4 : Confidence-based time allocation yields better performance than Human alone and AI alone.
- H5: Confidence-based time allocation yields better human-AI team performance than constant time and random time allocations.
- H6: Confidence-based time allocation with explanation yields better human-AI team performance than the other conditions.

We now describe the different components of Experiment 2 in detail.

**Participants.** In this study, 479 participants were recruited in the same manner as described in Section 8.4.1. 83 participants were between ages 18 and 29, 209 between ages 30 and 39, 117 between ages 40 and 49, and 70 over age 50. The average completion time for this user study was 30 minutes, and participants received compensation of $5.125 on average (roughly equals an hourly wage of $10.25). The participants received an average base pay of $4.125 and bonus of $1 (to incentivize accuracy).

**Task and AI model.** The binary prediction task in this study is the same as the student performance prediction task used before. In this experiment, our goal is to induce optimal human-AI collaboration under the assumptions illustrated in Figure 8.3. In real-world human-AI collaborations, it is not uncommon for the decision-maker to have some domain expertise or complementary knowledge that the AI does not, especially in fields where there is not enough data such as social policy-making and design. To emulate this situation where the participants have complementary knowledge, we reduced the information available to the AI, given the unavailability of human experts and the limited training time in our experiment. We train the assisting AI model over 7 features, while the participants have access to 3 more features, namely, hours spent studying weekly, hours spent going out with friends weekly, and enrollment in extra educational

support. These 3 features were the second to fourth most important ones as deemed by a full model.

To implement the confidence-based time allocation strategy, we had to identify trials belonging to classes $\mathcal{C}_L$ and $\mathcal{C}_H$. Ideally, for this we require a machine learning algorithm that can calibrate its confidence correctly. As discussed in Section 8.5.1, we use the AI's predicted probability $\widehat{C}_i$ (termed as AI confidence) and choose the threshold for $\mathcal{C}_H$ as $\widehat{C}_i \geq 0.75$. This study has $40$ questions in the testing section, from which $20$ belong to $\mathcal{C}_L$ and $20$ belong to $\mathcal{C}_H$.

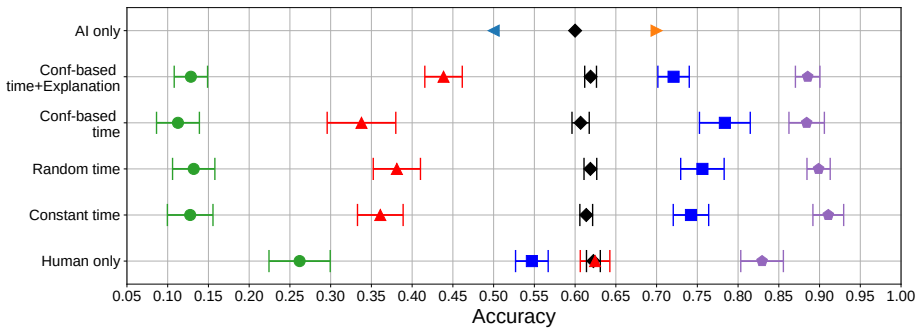
**Study procedure.** As in Experiment 1, this user study has two sections, the training section and the testing section. The training section is exactly the same as before where the participants are trained over $15$ examples selected from the training dataset. To induce anchoring bias, as in Experiment 1, we reinforce that the AI predictions are $85\%$ accurate in the training section.

The testing section has 40 trials, which are sampled randomly from the test set such that the associated predicted probability values (of the predicted class) estimated by the machine learning algorithm are distributed uniformly. While the set of trials in the testing section is fixed for all participants, the order they were presented in was varied randomly.

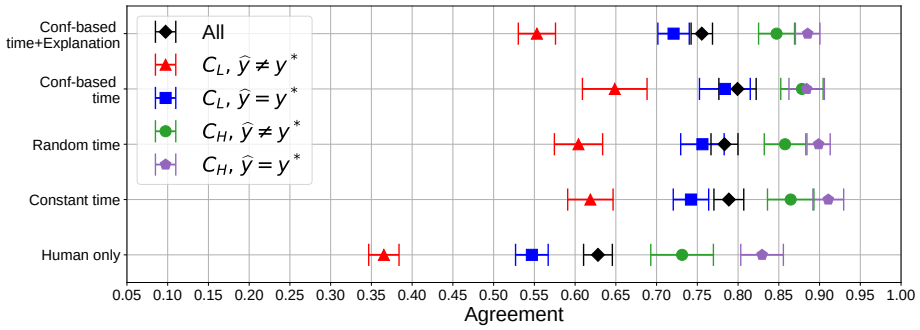
To test hypotheses H2, H3, H4, H5 and H6, we randomly assigned each participant to one of five groups:

1. **Human only**: In this group, the participants were asked to provide their prediction without the help of the AI prediction. The time allocation for each trial in the testing section is fixed at $25$ seconds. This time is judged to be sufficient for humans to make a prediction on their own, based on the results of Experiment 1 (for example the time usage distributions in Figure 8.2).
2. **Constant time**: In this group, the participants were asked to provide their prediction with the help of the AI prediction. The time allocation for each trial in the testing section is fixed as $\frac{t_L+t_H}{2} = 17.5$ seconds. We rounded this to $18$ seconds when reporting it to the participants.
3. **Random time**: This group has all factors the same as the constant time group except for the time allocation. For each participant, the time allocation for each trial is chosen uniformly at random from the set $\{10, 25\}$ such that the average time allocated per trial is $17.5$ seconds.
4. **Confidence-based time**: This is our treatment group, where we assign time according to confidence-based time allocation, $t_L = 25$ seconds and $t_H = 10$ seconds, described in Section 8.5.1.
5. **Confidence-based time with explanation**: This is our second treatment group, where in addition to confidence-based time allocation, we provide the AI confidence ("low" or "high") corresponding to each trial.

Out of the 479 participants, $95$ were in "Human only", $109$ were in "Constant time", $95$ were in "Random time", $85$ were in "Confidence-based time" and $96$ were in "Confidence-based time with explanation". In the groups where participants switch between $10$-second and $25$-second conditions the accuracy would likely be affected by rapid switching between the two time conditions. Hence, we created blocks of 5 trials with the same time allocation for both groups. The




(a) Average accuracy




(b) Average agreement with AI

Figure 8.4: Average accuracy and agreement ratio of participants in Experiment 2 across the four different conditions, marked on the y-axis. We note that the error bars in (a) for 'All' trials (black diamonds) are smaller than the marker size.

complete testing section contained 8 such blocks. This concludes the description of Experiment 2.

### 8.5.3 Results

Figure 8.4 shows that our effort to create a scenario where the AI knowledge is complementary to human knowledge is successful because the AI only and "Human only" conditions have similar overall accuracy (around $60\%$, black diamonds), and yet humans only agreed with the AI in $62.3\%$ of the trials. Moreover, on trials where AI is incorrect, "Human only" has accuracy of $61.8\%$ on trials in $\mathcal{C}_L$, and $29.8\%$ on trials in $\mathcal{C}_H$. Thus, the participants showed more complementary knowledge in trials in $\mathcal{C}_L$ compared to $\mathcal{C}_H$.

Given this successful setup of complementary knowledge between humans and AI, there is good potential for the human-AI partnership groups, especially the "Confidence-based time" group and the "Confidence-based time with explanation" group, to outperform the AI only or "Human only" groups (H4). In Figure 8.4(a), we see that the mean accuracy of the human-AI team is $61\%$ in "Confidence-based time" and $61.9\%$ in "Confidence-based time with explanation" while the accuracy of "Human only" is $61.9\%$ and the accuracy of the AI model is $60\%$. Thus, regarding H4, the results suggest that the accuracy in "Confidence-based time" is greater

than AI alone ($p = 0.06, t(183) = 1.52$), whereas they do not provide sufficient evidence for "Confidence-based time" being better than "Human only" ($p = 0.58, t(92) = -0.21$). Similarly, regarding H6, the results suggest that the accuracy in "Confidence-based time with explanation" is better than AI alone ($p = 0.004, t(194) = 2.66$), whereas for "Human only" the results are not statistically significant ($p = 0.5, t(189) = -0.02$).

However, we see that anchoring bias affected overall team performance negatively when the AI is incorrect (H2). Figure 8.4(b) shows evidence of anchoring, the agreement percentage in the "Human only" group is much lower than those in the collaborative conditions ($p < 0.001, t(184) = 6.73$). When the AI was incorrect (red triangles and green circles), this anchoring bias clearly reduced team accuracy when compared to the "Human only" accuracy ($p < 0.001, t(370) = -6.68$). Although, it is important to note that the "Human only" group received longer time (25s) than the collaborative conditions on average. Nevertheless, if we just compare "Human only" and "Confidence-based time" within the low confidence trials (red triangles), where both were assigned the same amount of time(25s), we observe similar disparity in agreement percentages ($p < 0.001, t(92) = 4.97$) and accuracy ($p < 0.001, t(92) = -4.74$). Hence, the results are consistent with H2.

Regarding H3, we see that while "Confidence-based time" alone did not lead to sufficient adjustment away from the AI when it was incorrect, "Confidence-based time with explanation" showed significant reduction in anchoring bias in the low confidence trials (red triangles) compared to the other conditions ($p = 0.003, t(383) = 2.70$), which suggests that giving people more time along with an explanation for the time helped them adjust away from the anchor sufficiently, in these trials (H3). This de-anchoring also led to higher accuracy in these trials(red triangles) for "Confidence-based time with explanation" ($43.8\%$) when compared to the other three collaborative conditions with "Random time" at $36.2\%$, "Constant time"at $36.4\%$ and "Confidence-based time" at $37.5\%$. Note that the set of conditions chosen in our experiment does not allow us to separately quantify the effect of the time-based allocation strategy and the confidence-based explanation; we discuss this in Section 8.6.

Next, we examine the differences between the four collaborative groups. Figure 8.4(a) shows that the average accuracy over all trials (black diamonds) is highest for "Confidence-based time with explanation" at $61.9\%$ with "Confidence-based time" at $61\%$, "Random time" at $61.1\%$ and "Constant time" at $61.5\%$. Regarding H5, we see that "Confidence-based time" does not have significantly different accuracy from the other collaborative groups. Finally, regarding H6, we observe that "Confidence-based time with explanation" has the highest accuracy, although the effect is not statistically significant ($p = 0.19, t(383) = 0.84$). We note that the outcomes in all collaborative conditions are similar in all trials except trials where AI is incorrect and has low confidence, and in these trials our treatment group has significantly higher accuracy. This implies that in settings prone to over-reliance on AI, "Confidence-based time with explanation" helps improve human-AI team performance.

The reason that the overall accuracy of "Confidence-based time" is not significantly better than the other two collaborative conditions is likely because of the relatively low accuracy and low agreement percentage in trials in $\mathcal{C}_H$ (green circles, purple pentagons). Based on the results of Experiment 1, we expected that the agreement percentage for the 10-second trials would be high and since these align with the high AI confidence trials for "Confidence-based time", we expected these trials to have a high agreement percentage and hence high accuracy. Instead, we

observed that "Confidence-based time" has low agreement percentage $(84\%)$ in $\mathcal{C}_H$, compared to "Random time" $(87.9\%)$, and "Constant time"$(88.1\%)$, both having an average time allocation of $17.5$ seconds. This lower agreement percentage translates into lower accuracy $(86\%)$ when AI is correct (purple pentagons). In the next section, we discuss how this points to possible distrust of AI in these high confidence trials and its implications. For "Confidence-based time with explanation" we observe that the participants in this group are able to de-anchor from incorrect low confidence AI predictions, to give higher mean accuracy than other collaborative conditions, albeit the difference is not statistically significant.

## 8.6 Discussion

**Lessons learned.** We now discuss some of the lessons learned from the results obtained in Experiment 2. As noted in Section 8.5.3, we see that "Confidence-based time" has a low agreement rate on trials in $\mathcal{C}_H$ where the time allocated is $10$ seconds and the AI prediction is $70\%$ accurate. Moreover, we see that the agreement rate is lower than "Human only" and "Constant time" on trials in $\mathcal{C}_H$, where the AI prediction is correct as well as where the AI prediction is incorrect. This behavior suggests that the participants in "Confidence-based time" may have grown to distrust the AI, as they disagreed more with the AI on average and spent more time on the trials where they disagreed. The distrust may be due to "Confidence-based time" assigning longer times $(25s)$ only to low-AI-confidence trials, perhaps giving the impression that the AI is worse than it really is. However, these effects are reduced by providing an explanation for the time allocation in "Confidence-based time with explanation". Our observations highlight the importance of accounting for human behaviour in such collaborative decision-making tasks.

Another insight gained from Experiment 2 is that the model should take into account the sequentiality of decision-making where the decision-maker continues to learn and build their perception of the AI as the task progresses, based on their interaction with the AI. Dynamic Markov models have been studied previously in the context of human decision-making (Busemeyer et al., 2020; Lieder et al., 2018). We believe that studying dynamic cognitive models that are cognizant of the changing interaction between the human and the AI model would help create more informed policies for human-AI collaboration.

**Limitations.** One limitation of our study is that our participants are not experts in student assessment. To mitigate this problem we first trained the participants in the task and showed them the statistics of the problem domain. We also showed more features to the human users, compared to the AI, to give them complementary knowledge. The fact that human-only accuracy in Experiment 2 is roughly the same as the AI-only accuracy suggests that these domain-knowledge enhancement measures were effective. Secondly, we proposed a time-based strategy and conducted Experiment 1 to validate our hypothesis and select the appropriate time durations $(10s, 25s)$ for our second experiment. Due to limited resources, we did not extend our search space beyond four settings – $(10s, 15s, 20s, 25s)$. Although it is desirable to conduct the experiment with real experts, this can be extremely expensive. Our approach can be considered as "human grounded evaluation" (Doshi-Velez and Kim, 2017), a valid approach by using lay people as a "proxy" to understand the general behavioral patterns. We used a non-critical

decision-making task where the participants would not be held responsible for the consequences of their decisions. This problem was mitigated by introducing an outcome-based bonus reward which motivates optimal decision-making. Our work considers the effect of our time allocation strategy with and without the confidence-based explanation through the treatment groups in experiment 2. While this helps us investigate the benefits of the time allocation strategy, we cannot separate out the independent effect of the confidence-based explanation strategy. Lastly, our work focuses on a single decision-making task. Additional work is needed to examine if the effects we observe generalize across domains and settings. However, prior research provides ample evidence that even experts making critical decisions resort to heuristic thinking, which suggests that our results will generalize broadly.

**Conclusions.** In this work, we foreground the role of cognitive biases in the human-AI collaborative decision-making setting. Through literature in cognitive science and psychology, we explore several biases and present mathematical models of their effect on collaborative decision-making. We focus on anchoring bias and the associated anchoring-and-adjustment heuristic that is important towards optimizing team performance. We validate the use of time as an effective strategy for mitigating anchoring bias through a user study. Furthermore, through a time-based resource allocation formulation, we provide an optimal allocation strategy that attempts to achieve the "best of both worlds" by capitalizing on the complementary knowledge presented by the decision-maker and the AI model. Using this strategy, we obtain human-AI team performance that is better than the AI alone, as well as better than having only the human decide in cases where the AI predicts correctly. When the AI is incorrect, the information it provides the human distracts them from the correct decision, thus reducing their performance. Giving them information about the AI confidence as explanation for the time allocation alleviates some of these issues and brings us closer to the ideal Human-AI team performance shown in Figure 8.3.

**Future work.** Our work shows that a time-based strategy with explanation, built on the cognitive tendencies of the decision-maker in a collaborative setting, can help decision-makers adjust their decisions correctly. More generally, our work showcases the importance of accounting for cognitive biases in decision-making, where in the future we would want to study other important biases such as confirmation bias or weak evidence effect. This paper opens up several directions for future work where explanation strategies in this collaborative setting are studied and designed based on the cognitive biases of the human decision-maker. Another interesting direction is to utilize the resource allocation framework for other cognitive biases based on their de-biasing strategies.

# Chapter 9

# A Taxonomy of Human and ML Strengths in Decision-Making to Investigate Human-ML Complementarity

Based on (Rastogi et al., 2023a):
Charvi Rastogi*, Liu Leqi*, Kenneth Holstein, Hoda Heidari. A Taxonomy of Human and ML Strengths in Decision-Making to Investigate Human-ML Complementarity. *Proceedings of the AAAI Conference on Human Computation and Crowdsourcing 2023*, 11(1), 127-139.

## 9.1 Introduction

In recent years, we have witnessed a rapid growth in the deployment of machine learning (ML) models in decision-making systems across a wide range of domains, including healthcare (Patel et al., 2019; Rajpurkar et al., 2020; Tschandl et al., 2020; Bien et al., 2018), credit lending (Bussmann et al., 2021; Kruppa et al., 2013), criminal justice (Angwin et al., 2016; Kleinberg et al., 2018), and employment (Raghavan et al., 2020; Hoffman et al., 2017). For example, in the criminal justice system, algorithmic recidivism risk scores inform pre-trial bail decisions for defendants (Angwin et al., 2016). In credit lending, lenders routinely use credit-scoring models to assess the risk of default by applicants (Kruppa et al., 2013). The excitement around modern ML systems facilitating high-stakes decisions is fueled by the promise of these technologies to tap into large datasets, mine the relevant statistical patterns within them, and utilize those patterns to make more accurate predictions at a lower cost and without suffering from the same cognitive biases and limitations as human decision-makers. Growing evidence, however, suggests that ML models are vulnerable to various biases (Angwin et al., 2016) and instability (Finlayson et al., 2018). Furthermore, they often produce harmful outcomes in practice, given that they lack humans strengths such as commonsense reasoning abilities, cognitive flexibility, and social and contextual knowledge (Alkhatib, 2021; Holstein and Aleven, 2021; Lake et al., 2017; Miller, 2019). These observations have led to calls for both human and ML involvement in high-stakes

decision-making systems—with the hope of combining and amplifying the respective strengths of human thinking and ML models through carefully designed *hybrid* decision-making systems. Such systems are common in practice, including in the domains mentioned above.

Researchers have proposed and tested various hybrid human-ML designs, ranging from human-in-the-loop (Russakovsky et al., 2015) to algorithm-in-the-loop (De-Arteaga et al., 2020; Saxena et al., 2020; Brown et al., 2019; Green and Chen, 2019) arrangements. However, empirical findings regarding the success and effectiveness of these proposals are mixed (Holstein and Aleven, 2021; Lai et al., 2021). Simultaneously, a growing body of theoretical work has attempted to conceptualize and formalize these hybrid designs (Gao et al., 2021; Bordt and von Luxburg, 2020) and study optimal ways of aggregating human and ML judgments within them (Madras et al., 2018; Mozannar and Sontag, 2020; Wilder et al., 2020; Keswani et al., 2021; Raghu et al., 2019; Okati et al., 2021; Donahue et al., 2022; Steyvers et al., 2022).

Much prior work has studied settings where the ML model outperforms the human decision-maker. These studies are frequently focused on tasks where there are no reasons to expect upfront that the human and the ML model will have complementary strengths (Bansal et al., 2021a; Guerdan et al., 2023; Holstein and Aleven, 2021; Lurie and Mulligan, 2020). For example, some experimental studies employ untrained crowdworkers on tasks that require extensive domain expertise, without which there is no reason to expect that novices would have complementary strengths (Fogliato et al., 2021; Lurie and Mulligan, 2020; Rastogi et al., 2022e). Other experimental studies are designed in ways that artificially constrain human performance—for instance, by eliminating the possibility that humans and ML systems have access to complementary information (Guerdan et al., 2023). Meanwhile studies on human-ML decision-making in real-world settings such as healthcare (Tschandl et al., 2020; Patel et al., 2019) sometimes demonstrate better human-ML team performance than either agent alone. However, the *reasons* for complementary team performance are often left unexplained, where we define human-ML *complementarity* as the condition in which a combination of human and ML decision-making outperforms[1] both human- and ML-based decision-making in isolation.

We argue, therefore, that there is a clear need to form a deeper, more fine-grained understanding of what types of human-ML systems exhibit complementarity in combined decision-making. To respond to this gap in the literature, we build a novel *taxonomy* of relative strengths and weaknesses of humans and ML models in decision-making, presented in Figure 9.1. This taxonomy aims to provide a shared understanding of the causes and conditions of complementarity so that researchers and practitioners can design more effective hybrid systems and focus empirical evaluations on promising designs—by investigating and enumerating the distinguishing characteristics of human vs. ML decision-making upfront. Our taxonomy covers application domains wherein the decision at stake is solely based on *predicting* some outcome of interest (Mitchell et al., 2018). Henceforth, we use the terms 'prediction' and 'decision' interchangeably. Some examples of predictive decisions are diagnosis of diabetic retinopathy (Gulshan et al., 2016), predicting recidivism for pretrial decisions (Dressel and Farid, 2018), and consumer credit risk prediction (Bussmann et al., 2021).

To build our taxonomy of human-ML complementarity, we surveyed the literature on human

[1]Complementary performance may present along any performance metric, and does not necessarily refer to accuracy.

behavior, cognitive and behavioral sciences, as well as psychology to understand the essential factors across which human and ML decision-making processes differ. Following traditions in cognitive science and computational social science (Lake et al., 2017; Marr and Poggio, 1977), we understand human and ML decision-making through a computational lens. Our taxonomy maps distinct ways in which human and ML decision-making can differ (Section 9.3).

To illustrate how our taxonomy can be used to investigate when we can expect complementarity in a given setting and what modes of human-ML combination will help achieve it, we present a mathematical framework that captures each factor in the taxonomy. In particular, we formalize an optimization problem for convex combination of human and ML decisions. This problem setup establishes a pathway to help researchers explore which characteristics of humans and ML models have the potential to foster complementary performance. To categorize different types of complementarity, we propose quantitative measures of complementarity, designed to capture two salient modes of human-ML collaboration in the literature: routing (or deferral) and communication-based collaboration. To demonstrate the use of our taxonomy, the optimization problem setup, and the associated metrics of complementarity, we simulate optimal human-ML combinations under two distinct conditions: (1) human and ML models have access to different feature sets, (2) human and ML models have different objective functions. By comparing optimal aggregation strategies under these conditions, we gain critical insights regarding the contribution of each decision-making agent towards the optimal combined decision. This informs the effective design of human-ML partnerships under these settings for future research and practice. Taken together, this work highlights that combining human-ML judgments should leverage the unique strengths and weaknesses of each entity, as different sources of complementarity impact the extent and nature of performance improvement achievable through human-ML collaboration.

In summary, this paper contributes a unifying taxonomy and formalization for human-ML complementarity. Our taxonomy characterizes major differences between human and ML predictions, and our optimization-based framework formally characterizes optimal aggregation of human and machine decisions under various conditions and the type of complementarity that produces optimal decisions. With these contributions, we hope to provide a common language and an organizational structure to inform future research in this increasingly important space for human-ML combined decision-making.

## 9.2 Methodology for designing the taxonomy

To investigate the potential for complementarity in human-ML combined decision-making, we need to understand the respective strengths and drawbacks of the human decision-maker and the ML model in the context of the application. For instance, it has been observed that while ML models draw inferences based on much larger bodies of data than humans could efficiently process (Jarrahi, 2018), human decision-makers bring rich contextual knowledge and common sense reasoning capabilities (Holstein and Aleven, 2021; Miller, 2019; Lake et al., 2017) to the decision-making process, which ML models may be unable to replicate. Thus, we develop a taxonomy for human-ML decision-making that accounts for broad differences between human decision-makers and machine learning, encompassing applications with predictive decision-making.

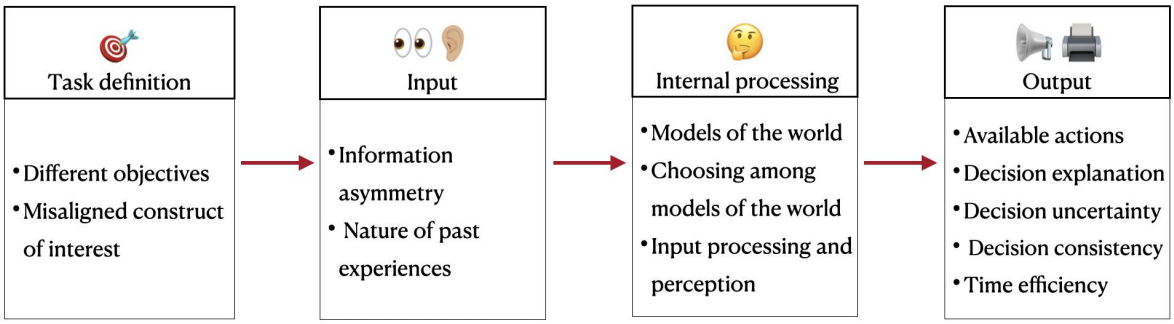



Figure 9.1: Proposed taxonomy of human and ML strengths & weaknesses in decision-making divided into four parts of the decision-making process: Task definition, input, internal processing, and output.

To inform this taxonomy, we draw from existing syntheses in human psychology, machine learning, AI, and human-computer interaction to understand distinguishing characteristics that have been observed between human decision-makers and ML in the context of predictive decision-making. In cognitive science, Lake et al. (2017) review major gaps between human and ML capabilities by synthesizing existing scientific knowledge about human abilities that have thus far defied automation. In management science, Shrestha et al. (2019) identify characteristics of human and ML decision-making along four key axes: decision space, process and outcome interpretability, speed, and replicability, and discuss their combination for organizational decision-making. In human-computer interaction, Holstein et al. (2020) conceptually map distinct ways in which humans and AI can augment each others' abilities in real-world teaching and learning contexts. More recently, Lai et al. (2021) surveyed empirical studies on human-AI decision-making to document trends in study design choices (e.g., decision tasks and evaluation metrics) and empirical findings. We draw upon this prior literature to summarize key differences in human and ML-based predictive decision-making across multiple domains, with an eye towards understanding opportunities to combine their strengths.

**Computational lens.** Our taxonomy takes a computational perspective towards analysing human and ML decision-making. As with any modeling approach or analytic lens, computational-level explanations are inherently reductive, yet are often useful in making sense of complex phenomena for this very reason. Computational-level explanations often provide an account of a *task* an agent performs, the *inputs* that the agent takes in, the ways in which the agent *perceives and processes* these inputs, and the kinds of *outputs* that the agent produces. Accordingly, our taxonomy is organized into four elements: (1) task definition, (2) input, (3) internal processing, and (4) output.

We now provide mathematical notation to clearly express the computational perspective of decision-making in our taxonomy. Formally, the agent's decision-making setting is specified by a feature space $\mathcal{X}$, an action space $\mathcal{A}$, and the space of observed outcomes, $\mathcal{O}$. At a high-level, the agent perceives an instance $\mathbf{X} \in \mathcal{X}$, chooses to take an action $a \in \mathcal{A}$ based on its relevant prior knowledge and experiences, and observes an outcome $O \in \mathcal{O}$ as a result. To emphasize that the outcome $O$ is influenced by $\mathbf{X}$ and $a$, we slightly abuse the notation to denote the outcome

of action $a$ on instance $\mathbf{X}$ as $O(\mathbf{X}, a)$. We consider the agent's perception of an instance $\mathbf{X}$ to be denoted by $s(\mathbf{X})$, where $s : \mathcal{X} \to \mathcal{X}$. Next, the agent's prior knowledge and relevant experiences are assumed to be encompassed in a set $\mathcal{D}$. The goal of the decision-making agent is to choose a policy, $\pi : \mathcal{X} \to \mathcal{A}$, in the space of feasible policies $\Pi$, such that $\pi$ leads to favorable overall outcome quality, measured by an evaluation function $F$. Here $F$ takes in a policy and outputs a real number. For instance, the expected outcome of a policy is a common choice for $F$. Finally, the agent chooses their optimal policy $\overline{\pi}$ using their optimization process OPT for choosing among feasible policies that lead to favorable $F$ values. Using the categorization and mathematical formalization above, and drawing upon relevant background literature as presented in this section, we now provide our taxonomy for relative human and ML strengths.

## 9.3 A taxonomy of human and ML strengths & weaknesses in decision-making

In this work, we consider two decision-making agents, the human and the ML model denoted respectively by H and M. Building upon the notation in Section 9.2, we denote the feature space available to each agent by $\mathcal{X}_{\text{H}}, \mathcal{X}_{\text{M}}$ correspondingly, where $\mathcal{X}_{\text{H}}, \mathcal{X}_{\text{M}} \subseteq \mathcal{X}$. Similarly, for each variable introduced for our decision-making setting in the previous section, we consider a human version and a ML version, denoted by subscript H and M respectively. We now present our taxonomy, visually represented in Figure 9.1.

### 9.3.1 Task definition

We now describe the distinguishing characteristics that have been observed in the definition of the decision-making task used by the human and the ML model.

- **Objective.** Most machine learning models aim to only optimize the expected performance, e.g., minimize the expected loss for supervised learning models and maximize the expected cumulative rewards for reinforcement learning models. While recent research has explored ways to build models with respect to a more diverse set of objectives, including different risk measures (Leqi et al., 2019a; Khim et al., 2020), fairness definitions (Chouldechova and Roth, 2020) and interpretability notions (Lipton, 2018; Miller, 2019), it is often difficult or impractical to encode all aspects of the objectives that a human decision-maker would aim to optimize (Kleinberg et al., 2018). Using our notation, this is expressed as $F_{\text{H}} \neq F_{\text{M}}$. For example, when making a lending decision, in addition to considering various risk factors, bankers may also care about aspects such as maintaining their relationships with clients and specific lending practices in their organization (Trönnberg and Hemlin, 2014).
- **Misaligned construct of interest.** ML models deployed in social contexts often involve theoretical constructs that are not directly observable in the data, such as socioeconomic status, teacher effectiveness, and risk of recidivism, which cannot be measured directly. Instead they are inferred indirectly via proxies: measurements of properties that are observed in the data available to a model. The process of defining proxy variables for a construct of interest necessarily involves making simplifying assumptions, and there is often a considerable conceptual

distance between ML proxies and the ways human decision-makers think about the targeted construct (Green and Chen, 2021; Guerdan et al., 2023; Jacobs and Wallach, 2021; Kawakami et al., 2022). In other words, $O_{\text{H}}(\mathbf{X}, a) \neq O_{\text{M}}(\mathbf{X}, a)$. Jacobs and Wallach (2021) argue that several harms studied in the literature on fairness of socio-technical systems are direct results of the mismatch between the construct of interests and the inferred measurements. For example, Obermeyer et al. (2019) examined racial biases in an ML-based tool used in hospitals. They found that the use of an indirect proxy (healthcare costs incurred by a patient) to predict patients' need for healthcare contributed to worse healthcare provision decisions for black versus white patients. In this example, although the proxy used (the monetary cost of care) was conveniently captured in available data, it differs significantly from the way healthcare professionals conceptualize patients' actual need for care.

### 9.3.2 Input

We now describe the distinguishing characteristics observed in the inputs used by humans and ML models.

- **Access to different information.** From the input perspective, in many settings such as healthcare, criminal justice, humans and machines have access to both shared and non-overlapping information: $\mathcal{X}_{\text{H}} \neq \mathcal{X}_{\text{M}}$. This is because real-world decision-making contexts often contain features of importance that cannot be codified for ML. For example, a doctor can see the physical presentation of a patient and understand their symptoms better, since this information is hard to codify and provide to the machine. Similarly, a judge learns about the predisposition of the defendant through interaction (Kleinberg et al., 2018). This phenomena is also referred to as unobservables (Holstein et al., 2023) and information asymmetry (Hemmer et al., 2022) in the literature on human-ML complementarity.
- **Nature of past experiences.** The nature of embodied human experience over the course of a lifetime differs substantially from the training datasets used by modern ML systems: $\mathcal{D}_{\text{H}} \neq \mathcal{D}_{\text{M}}$. For example, ML models are often trained using a large number of prior instances of a specific decision-making task, but for each instance, the training data contains a fixed and limited set of information. This often does not reflect the richness of human experience. Humans make their decisions with reference to a lifetime of experiences across a range of domains, and it is difficult to explicitly specify the information they take into account. By contrast, ML models may learn from training data that comprise narrow slices from a vast number of human decision-makers' decisions, whereas humans typically learn only from their own experiences or from a small handful of other decision-makers.

### 9.3.3 Internal processing

We now describe the distinguishing characteristics of the internal processes used by humans and ML systems.

- **Models of the world.** As is comprehensively overviewed in Lake et al. (2017), humans rely upon rich mental models and "theories" that encode complex beliefs about causal mechanisms in the world, not just statistical relationships. This results in humans having a different set of

models of the world than those embodied by ML models: $\Pi_H \neq \Pi_M$. For example, starting from an early age, humans develop sophisticated systems of beliefs about the physical and social worlds (intuitive physics and intuitive psychology), which strongly guide how they perceive and make decisions in the world. In contrast to modern ML systems, humans' mental models tend to be compositional and causal. In turn, these strong prior beliefs about the world can enable humans to learn rapidly in comparison to modern ML systems, and to make inferential leaps based on very limited data (e.g., one-shot and few-shot learning) (Gopnik and Wellman, 2012; Lake et al., 2017; Tenenbaum et al., 2011). On the other hand, the model class of the machine decision-maker has a more mathematically tractable form—whether it is a class of parametric or non-parametric models (Friedman, 2017). Although when designing these models such as neural networks, researchers commonly encode domain knowledge through the data and the model architecture, most machine learning models still suffer from distribution shift (Quiñonero-Candela et al., 2009) and lack of interpretability (Gilpin et al., 2018), and require large sample sizes.

- **Input processing and perception.** The ways decision-makers perceive inputs is informed by their models of the world (Gentner and Stevens, 2014; Holstein et al., 2020). Following research in human cognition and ML, we highlight three sources of variation in input perception: (1) differences in mental/computational capacity, (2) differences in human versus machine biases, and (3) tendencies towards causal versus statistical perception. Here the first implies $s_H \neq s_M$ and the remaining two indicate $\pi_H \neq \pi_M$. For instance, compared with ML systems, humans demonstrate less capacity to perceive small differences in numerical values (Amitay et al., 2013; Findling and Wyart, 2021). Furthermore, both humans and ML systems can bring in both adaptive and maladaptive biases, based on their experiences and models of the world, which in turn shape the ways they process and perceive new situations (Fitzgerald and Hurst, 2017; Wistrich and Rachlinski, 2017; Kleinberg et al., 2018; Gentner and Stevens, 2014). However, in some cases humans and ML systems may have complementary biases, opening room for each to help mitigate or compensate for the other's limitations (Holstein et al., 2020; Tan et al., 2018). Finally, research on human cognition demonstrates that humans are predisposed to perceiving causal connections in the world, and drawing causal inferences based on their observations and interactions in the world (Gopnik and Wellman, 2012; Lake et al., 2017). While these abilities can sometimes be understood by analogy to the kinds of statistical learning that most modern ML systems are based upon (Tenenbaum et al., 2011), other aspects of human causal cognition appear to be fundamentally different in nature (Lake et al., 2017). As with bias, these abilities can be a double-edged sword. In some scenarios, human causal perception may lead to faulty inferences based on limited data. By contrast, ML systems will sometimes have an advantage in drawing more reliable inferences based on statistical patterns in large datasets. In other settings, human causal perception can help to overcome limitations of ML systems. For example, in many instances, human decision-makers have been observed to be better than ML systems at adapting to out-of-distribution instances, through the identification and selection of causal features for decision-making (Lake et al., 2017).
- **Choosing among models of the world.** Given the task definition, models of the world, and data, ML models differ from humans in searching for the model that optimizes their objective: $\text{OPT}_H \neq \text{OPT}_M$. Modern ML models (e.g., neural networks) are commonly learned using

first-order methods and may require a huge amount of computational resource due to the size of the models (Bottou, 2010). On the other hand, humans may employ heuristics that can be executed in a relatively short amount of time (Simon, 1979). These simple strategies may have advantages over more complex models when the inherent uncertainty in the task is high. For a more comprehensive review on when and how such heuristics may be more preferable, we refer readers to Kozyreva and Hertwig (2021).

### 9.3.4 Output

We now describe the distinguishing characteristics of the outputs generated by humans and ML systems.

- **Available actions.** In real-world deployment settings, the set of possible decisions or actions available to ML models versus humans can be different: $\mathcal{A}_{\text{H}} \neq \mathcal{A}_{\text{M}}$. For example, in the context of K-12 education, ML-based tutoring software may be able to provide just-in-time hints to students, to help struggling students them with math content. Meanwhile, although a human teacher working alongside this software in the classroom has limited time to spend with each student, they can take a wider range of actions to support students, such as providing emotional support or helping students with prerequisite content that lies outside of the software's instructional repertoire (Holstein et al., 2020). Similarly, in the context of ML-assisted child maltreatment screening, a model may only be able to recommend that a case be investigated or not investigated, based on the information that is currently available. By contrast, Kawakami et al. (2022) report that human call screeners may take actions to gather additional information as needed, e.g. by making phone calls to other stakeholders relevant to a case.
- **Explaining the decision.** Humans and ML have differing abilities in communicating the reasoning behind their decisions. There has been extensive research in explainability (XAI) and interpretability for ML (Adadi and Berrada, 2018). Research in cognitive and social psychology observes that humans are generally better than ML algorithms at generating coherent explanations that are meaningful to other humans. Furthermore, Miller (2019) argues that XAI research should move away from imprecise, subjective notions of "good" explanations and instead focus on reasons and thought processes that people apply for explanation selection. They find that human explanations are contrastive, selected in a biased manner, and most importantly they are social and contextual. On the other hand, humans' explanations may not have a correspondence to their actual underlying decision processes (Nisbett and Wilson, 1977), whereas with ML models we can always trace the precise computational steps that led to the output prediction (Hu et al., 2019).
- **Uncertainty communication.** With increasing research in uncertainty quantification for machine learning, new methods have been devised for calibrating a ML model's uncertainty in its prediction (Abdar et al., 2021). Moreover, methods have been developed to decompose the model uncertainty into aleatoric uncertainty and epistemic uncertainty (Hüllermeier and Waegeman, 2021), where aleatoric uncertainty signifies the inherent randomness in an application domain and cannot be reduced, and epistemic uncertainty, also known as systematic uncertainty, signifies the uncertainty due to lack of information or knowledge, and can be reduced. However, these uncertainty quantification methods may not necessarily be well-calibrated (Ab-

dar et al., 2021), and are an active research direction. Meanwhile, human decision-makers also find it difficult to calibrate their uncertainty or their confidence in their decisions (Brenner et al., 2005), and tend to output discrete decisions instead of uncertainty scores. Moreover, different people have different scales for uncertainty calibration (Zhang and Maloney, 2012).

- **Output consistency.** We define a given decision-maker to have a consistent output when they always produce the same output for the same input. Therefore, we consider the inconsistency in decisions that are based on factors independent of the input, we call them extraneous factors. Some examples of extraneous factors are the time of the day, the weather, etc. Research in human behavior and psychology has shown that human judgments show inconsistency (Kahneman et al., 2016). More specifically, there is a positive likelihood of change in outcome by a given human decision-maker given the exact same problem description at two different instances. Within-person inconsistency in human judgments has been observed across many domains, including medicine (Koran, 1975; Kirwan et al., 1983), clinical psychology (Little, 1961), finance and management (Kahneman et al., 2016). This form of inconsistency is not exhibited by standard ML algorithms.[2]
- **Time efficiency.** In many settings, ML models can generate larger volumes of decisions in less time than human decision-makers. In addition to potentially taking more time per decision, humans often have comparatively scarce time for decision-making overall.

## 9.4 Investigating the potential for human-ML complementarity

To understand how the differences in human and machine decision-making result in complementary performance, we formulate an optimization problem to aggregate the human and the ML model outcomes. The key motivation here is to use information available about human and ML decision-making (in the form of historical data or decision-making models) to understand the potential for complementarity in human-ML joint performance. Specifically, this optimization problem outputs the optimal convex combination of the two decision-makers' outputs wherein the aggregation mechanism represents the best that the human-ML joint decision-making can achieve in our setting.

In our decision-making setting, as mentioned in Section 9.2, we consider a feature space $\mathcal{X}$, an action space $\mathcal{A}$ and an outcome space $\mathcal{O}$. Given a problem domain, the goal is to combine the two decision-makers policies to find a joint policy denoted by $\overline{\pi} : \mathcal{X} \rightarrow \mathcal{A}$ that maximizes the overall quality of the decisions based on evaluation function, $F$,

$$\overline{\pi} \in \arg\max_{\pi \in \Pi} \; F(\pi). \tag{9.1}$$

We note that the overall evaluation function $F$ for the joint policy $\pi$ may be different from that used by the human $F_{\mathrm{H}}$ or the ML model $F_{\mathrm{M}}$. We assume the joint policy is obtained by combing human and machine policies $\pi_{\mathrm{H}}$ and $\pi_{\mathrm{M}}$ over $n$ number of instances through an aggregation

[2]There exists the special case of randomized models, we consider these outside the scope of our work and, further note that these models can be directly mapped to deterministic models with decision-based thresholds.

function. We consider the outcome space to be scalar $\mathcal{O} \subseteq \mathbb{R}$. Given $\pi_{\mathrm{H}} \in \Pi_{\mathrm{H}}$, $\pi_{\mathrm{M}} \in \Pi_{\mathrm{M}}$, for an instance $\mathbf{X}_i$ where $i \in [n]$, the joint policy $\pi \in \Pi$ is given by

$$\pi(\mathbf{X}_i) = w_{\mathrm{H}}^{(i)} \pi_{\mathrm{H}}(\mathbf{X}_i) + w_{\mathrm{M}}^{(i)} \pi_{\mathrm{M}}(\mathbf{X}_i), \tag{9.2}$$

for some weights $w_{\mathrm{H}}^{(i)}, w_{\mathrm{M}}^{(i)} \in [0, 1]$ and $w_{\mathrm{H}}^{(i)} + w_{\mathrm{M}}^{(i)} = 1$ for all $i \in [n]$. Here note that we assume that the joint decision $\pi(\mathbf{X}_i)$ is a convex combination of the individual decisions $\pi_{\mathrm{H}}(\mathbf{X}_i)$ and $\pi_{\mathrm{M}}(\mathbf{X}_i)$. This assumption arises naturally to ensure that the joint decision lies between the human's and machine's decision. For a decision-maker (say human), the weight assigned for instance $i$, $w_{\mathrm{H}}^{(i)}$ indicates the amount of contribution from them towards the final decision: when $w_{\mathrm{H}}^{(i)} = 0$, the joint decision does not follow human's decision at all on instance $\mathbf{X}_i$, while $w_{\mathrm{H}}^{(i)} = 1$ indicates that their decision is followed entirely. For the optimal policy $\overline{\pi}$ defined in (9.1), its corresponding optimal weights are denoted by $\overline{w}_{\mathrm{H}}^{(i)}$ and $\overline{w}_{\mathrm{M}}^{(i)}$.

Several existing works on human-ML combination for decision-making, such as Donahue et al. (2022); Raghu et al. (2019); Mozannar and Sontag (2020); Gao et al. (2021) are subsumed by our convex combination optimization setup. Particularly, our aggregation mechanism captures two salient modes: (1) The mode where an instance is routed to either the human or the ML decision maker, also known as deferral. This is represented by $w_{\mathrm{H}}^{(i)}, w_{\mathrm{M}}^{(i)} \in \{0, 1\}$ for all $i \in [n]$. (2) The mode where a joint decision lying between the human and the ML decision is applied to each instance. This is represented by $w_{\mathrm{H}}^{(i)}, w_{\mathrm{M}}^{(i)} \in (0, 1)$ for all $i \in [n]$.

### 9.4.1 Metrics for complementarity

The proposed aggregation framework is a way to inspect the extent of complementarity in human-ML joint decision-making. Recall that, based on our definition, The joint policy $\pi$ defined in (9.2) exhibits complementarity if and only if

$$F(\pi) > \max\{F(\pi_{\mathrm{H}}), F(\pi_{\mathrm{M}})\}.$$

Although this criterion provides a binary judgment on whether complementarity exists in a particular joint decision-making setting, it cannot be used to compare the amount of potential for complementarity in different settings. For instance, between two settings where machine can improve the performance of the human decision-maker on one instance versus on all instances, one may say that there is more complementarity exhibited in the second setting. Further, it does not distinguish between the two salient modes of combination defined above, where the second mode may require more interaction between the human and the machine decision-maker. So, to investigate the potential for complementarity in different settings more thoroughly, we introduce metrics for quantifying the complementarity between the human and ML decision-maker.

Specifically, we introduce the notion of within- and across-instance complementarity to represent the two modes of combination where for an instance $\mathbf{X}_i$, we either have only one of human or ML contributing to the final decision ($w_{\mathrm{M}}^{(i)} = 1$ or $w_{\mathrm{H}}^{(i)} = 1$), or both decision-makers contributing to the final decision partially ($w_{\mathrm{M}}^{(i)} > 0$ and $w_{\mathrm{H}}^{(i)} > 0$). These two types of combinations represent two ways of achieving complementarity. In the first one, there is no complementarity within a single task instance, since only the human or the ML model decision gets used. In this

scenario, if the human and ML model provide the final decision for different instances of the task, we call this *across-instance complementarity*. In the second one, if both human and ML model contribute to the same instance $\mathbf{X}_i$, we call this *within-instance complementarity*. These two metrics help distinguish between different instance allocation strategies in human-ML teams described in (Roth et al., 2019). Formally, given the weights assigned to the two agents in the final decision, we define the two metrics as follows:

- **Across-instance complementarity** quantifies the variability of the human (or the machine) decision-maker's contribution to the final decision across all task instances. Therefore, we define it as the variance of the weights assigned, written as

$$\begin{aligned} c_{\text{across}}(w_{\text{M}}, w_{\text{H}}) &:= \frac{1}{n}\sum_{i=1}^{n}\left(w_{\text{M}}^{(i)} - \frac{1}{n}\sum_{i=1}^{n} w_{\text{M}}^{(i)}\right)^2 \\ &= \frac{1}{n}\sum_{i=1}^{n}\left(w_{\text{H}}^{(i)} - \frac{1}{n}\sum_{i=1}^{n} w_{\text{H}}^{(i)}\right)^2. \end{aligned} \tag{9.3}$$

  The equality follows directly using the constraint $w_{\text{M}}^{(i)} + w_{\text{H}}^{(i)} = 1$. In case of no variability across instances, that is if for both decision-makers, we have $w_{\text{M}}^{(i)}$ (or $w_{\text{H}}^{(i)}$) to be a constant for all $i \in [n]$, then $c_{\text{across}}(w_{\text{M}}, w_{\text{H}}) = 0$. The notion of across-instance complementarity is shown by works on decision deferral or routing including Mozannar and Sontag (2020); Madras et al. (2018).

- **Within-instance complementarity** quantifies the extent of collaboration between the two decision-makers on each individual task instance. Formally, we define

$$c_{\text{within}}(w_{\text{M}}, w_{\text{H}}) := 1 - \frac{1}{n}\sum_{i=1}^{n}\left(w_{\text{H}}^{(i)} - w_{\text{M}}^{(i)}\right)^2. \tag{9.4}$$

  Importantly, the definition of within-instance complementarity satisfies some key properties: $c_{\text{within}}(w_{\text{H}}, w_{\text{M}})$ is maximized at $w_{\text{H}}^{(i)} = w_{\text{M}}^{(i)} = 0.5$ and minimized at $w_{\text{H}}^{(i)} \in \{0, 1\}$ for all $i \in [n]$. Thus, it is maximized when each decision-maker contributes equally and maximally to a problem instance and minimized when there is no contribution from one of the decision-makers. Further, it increases monotonically as $w_{\text{H}}^{(i)}$ and $w_{\text{M}}^{(i)}$ get closer to each other in value, that is the two decision-makers' contributions to the final decision get closer to half. This notion of complementarity is demonstrated in several works including Patel et al. (2019); Tschandl et al. (2020).

To have a better grasp on the above two metrics, and to understand the importance of each metric in measuring complementarity, we provide some demonstrative examples. Consider a simple setting with $\mathcal{X} = \{\mathbf{x}_1, \mathbf{x}_2, \mathbf{x}_3, \mathbf{x}_4\}$ where each instance is equally likely, that is, $\mathbb{P}(\mathbf{X} = \mathbf{x}_i) = 1/4$ for all $i \in [4]$. The values of the two metrics under different aggregation weights are given below:

1. If $w_{\text{H}}^{(1)} = w_{\text{H}}^{(2)} = w_{\text{H}}^{(3)} = w_{\text{H}}^{(4)} = 0$, then $c_{\text{within}} = 0$, and $c_{\text{across}} = 0$.
2. If $w_{\text{H}}^{(1)} = w_{\text{H}}^{(2)} = 0, w_{\text{H}}^{(3)} = w_{\text{H}}^{(4)} = 1$, then $c_{\text{within}} = 0$, and $c_{\text{across}} = 0.25$.
3. If $w_{\text{H}}^{(1)} = w_{\text{H}}^{(2)} = w_{\text{H}}^{(3)} = w_{\text{H}}^{(4)} = 0.3$, then $c_{\text{within}} = 0.84$, and $c_{\text{across}} = 0$.

We note that although the second example has $c_{\text{within}} = 0$ and $c_{\text{across}} > 0$, which is the opposite of the third example, both the examples demonstrate complementarity. This shows that each metric introduced captures aspects of human-ML complementarity that is not captured by the other metric.

## 9.5 Synthetic experiments to illustrate complementarity

In this section, we illustrate how our proposed framework can be used to investigate the extent and nature of complementarity via simulations. These simulations utilize human and ML models learned from data, where the two decision-makers have different access of information or they pursue different objectives. By quantifying the extent of different types of complementarity (i.e., within-instance and across-instance), we show how the proposed taxonomy and complementarity metrics can guide the research and practice of hypothesizing about and testing for complementarity with different types of human and ML decision-makers. To conduct these simulations, we choose specific aspects from our taxonomy in Section 9.3 and measure complementarity in the presence of corresponding differences between the human and the ML model. We note that these simulations are meant to be an illustrative and not exhaustive exploration of human-ML complementarity conditions that can be explored using the taxonomy.

**Synthetic simulation setup**. We consider a linear model for the data generating process: the features $\mathbf{X} \in \mathbb{R}^d$ are distributed as $\mathbf{X} \sim \mathcal{N}(0, \mathbf{I}_{d\times d})$; the target is given by $Y = \mathbf{X}^\top \beta + \epsilon$ where $\beta = (1 \cdots 1) \in \mathbb{R}^d$ and $\epsilon \sim \mathcal{N}(0, 1)$. For any given instance $\mathbf{X} \in \mathbb{R}^d$, both the ML and human decision-maker make a prediction using their respective linear model, which serves as a decision. We assume that the outcome for a given instance is determined by the squared loss incurred by the decision. For example, for the machine, given the true target $Y$ and the prediction $\pi_{\text{M}}(\mathbf{X})$, the outcome is given by $O = (\pi_{\text{M}}(\mathbf{X}) - Y)^2$. The dimension of the features is chosen to be $d = 10$ for all simulations.

In Section 9.5.1, we study how human-ML complementarity varies when the human and the ML model have different feature information available to them; and in Section 9.5.2, the difference between the human and the machine arises via difference in objective functions for learning their respective policies. In the following simulations, we first use a training set of sample size $8,000$ to learn the respective optimal linear model for the human and the ML policy. Once the decision-makers' policies are learned, a separate testing set of size $2,000$ is used to compute and analyse the optimal aggregation weights. On this set, we measure and report the metrics of complementarity defined in Section 9.4.1.

### 9.5.1 Access to different feature sets

First, we consider the setting where the human and the machine decision-maker have different information available to them. This is a potential source of complementarity in human-ML joint decision-making as mentioned in our taxonomy in Section 9.3 based on the input. To analyze the impact of information asymmetry on human-ML complementarity, we conduct synthetic experiments based on the general setup described at the beginning of Section 9.5. Additionally, we assume that the features available to the human and the ML model are denoted by $\mathbf{X}_{\text{H}} \in \mathbb{R}^{d_{\text{H}}}$ and

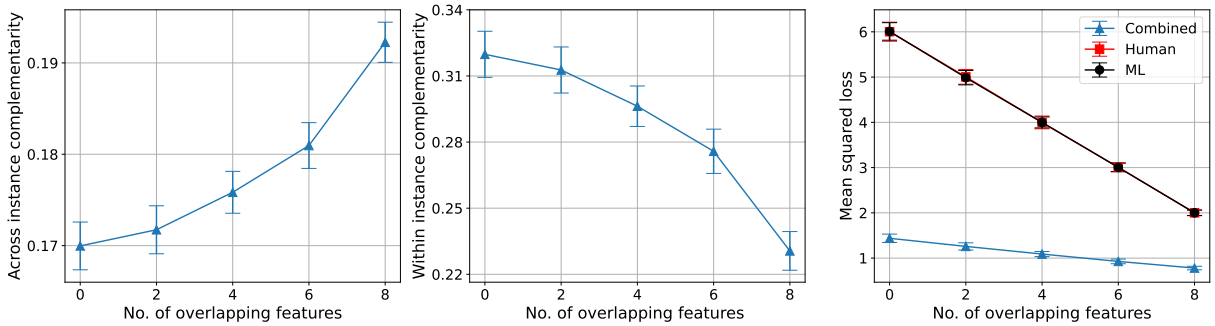



Figure 9.2: We plot the outcomes of Experiment I described in Section 9.5.1. The x-axis indicates the number of features that both the human and the ML model have access to. In each of the three figures, we plot an outcome metric for the optimal joint policy, namely across-instance complementarity (9.3), within-instance complementarity (9.4) and mean squared loss of the policy compared to the target outcome $Y$. The markers show the mean value and the error bars indicate the standard deviation, based on 200 iterations. On the $x$-axis, we skip $x = 10$, as it is a straightforward setting where both the agents have access to all the features, so there is no complementarity, $c_{\text{within}} = c_{\text{across}} = 0$. Note that all three plots have different ranges on the $y$-axis, with $c_{\text{across}} \in [0, 0.25]$, $c_{\text{within}} \in [0, 1]$. To read these plots, we focus on relative values within plots, and not on absolute values across plots. We observe that $c_{\text{across}}$ increases while $c_{\text{within}}$ decreases as the number of overlapping features increases. When the agents have no overlapping features $(x = 0)$ the two agents have more likely to be equally benefitial for each decision leading to a higher within-instance complementarity. Meanwhile, when both have largely overlapping information $(x = 8)$, the combination is more likely to show across-instance complementarity, the gains of going with the better decision-maker outweighing the possible gains from combination on each instance.

$\mathbf{X}_{\text{M}} \in \mathbb{R}^{d_{\text{M}}}$ respectively, where $d_{\text{H}}$ and $d_{\text{M}}$ indicate the number of features available to the human and the machine respectively, with $d_{\text{H}}, d_{\text{M}} \leq d = 10$. Given the input information available to them, the human and the machine learn a policy using linear regression on the training data, given by $\pi_{\text{H}} : \mathbb{R}^{d_{\text{H}}} \to \mathbb{R}$ and $\pi_{\text{M}} : \mathbb{R}^{d_{\text{M}}} \to \mathbb{R}$ respectively. Using the optimization problem setup in (9.1) and (9.2), we conduct simulations to analyse the amount and type of complementarity achieved by the combination of human and ML agents with different information. Consequently, we conduct two sets of experiments.

**Experiment I.** We consider the setting where the human and ML have access to some common features and some non-common features as is typical of many real-world settings, as described in Section 9.3. Specifically, out of $d = 10$ features in our setting, the human and the ML both have access to $z$ common features, and each has access to an additional $\frac{10-z}{2}$ features that only they can observe, where $z \in [d]$. We plot the outcomes of this experiment in Figure 9.2, where the $x$-axis of each plot indicates $z$ (the degree of overlap between human and ML feature sets). Interestingly, we observe that while across-instance complementarity increases non-linearly with the number of overlapping features, within-instance complementarity decreases non-linearly. This suggests that when the two agents have access to many non-overlapping features, it would be important to

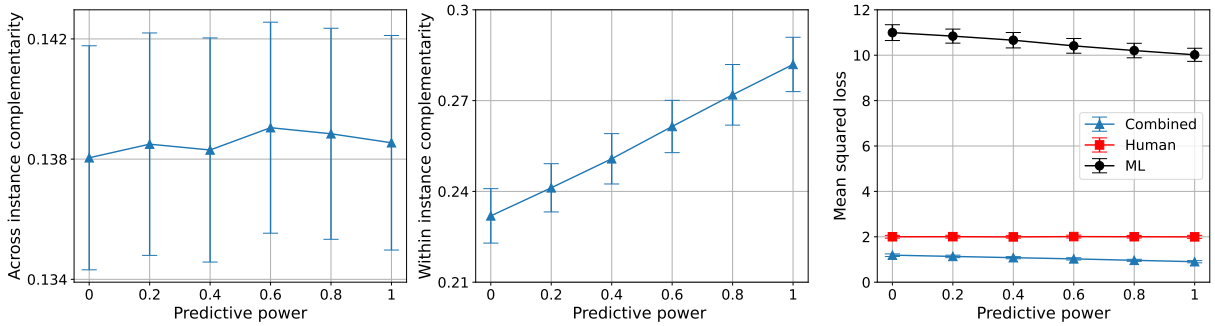



Figure 9.3: We plot the outcomes of Experiment II where the ML model has access to one feature and the human has access to the other nine features, described in Section 9.5.1. The $x$-axis indicates the predictive power of the feature $\mathbf{X}_{\text{M}}$ that the machine has. In each of the three figures, we plot an outcome metric for the optimal joint policy, namely across-instance complementarity (9.3), within-instance complementarity (9.4) and mean squared loss of the policy compared to the target outcome $Y$. The markers show the mean value and the error bars indicate the standard deviation, based on 200 iterations. Note that all three plots have different overall ranges on the $y$-axis, with $c_{\text{across}} \in [0, 0.25]$, $c_{\text{within}} \in [0, 1]$. To read these plots, we focus on relative values within plots.

use both the agents' decisions to come to a final decision on a given instance. On the other hand, in a setting with few overlapping features, the importance of collaboration on each instance reduces and it may be prudent to consider routing tasks to either the human or the machine for making the final decision. Furthermore, in the third plot, we observe that the combined decision has a strictly lower loss than either the human or the ML in isolation. Importantly, the gains achieved by the combined decision indicated by difference between the loss achieved by the individual agents and that by the combination is reducing as the number of overlapping features decreases. This suggests that depending upon the number of overlapping features and the resulting gain in accuracy, one may decide to forego joint human-ML decisions. We discuss this in more detail in Section 9.6.

**Experiment II.** Next, we consider a setting where the human has access to nine of the features $\mathbf{X}_{\text{H}} \in \mathbb{R}^9$ and the machine has access to the remaining tenth feature $\mathbf{X}_{\text{M}} \in \mathbb{R}$. Within this setting, we simulate the types of information asymmetry identified in Holstein et al. (2023). In this work on human-ML complementarity, the authors distinguish between non-overlapping features based on their "predictive power" which they define for any feature as the increase in training accuracy of a model as a result of including the feature. To simulate this, we vary the predictive power of the feature available to the ML model by introducing multiplicative random noise. Recall that $Y = \mathbf{X}^\top \beta + \epsilon$ where $\beta = (1 \cdots 1) \in \mathbb{R}^d$. Now, we define a variable $\alpha$ and let the data available to the ML model $\mathbf{X}_{\text{M}} \in \mathbb{R}$ be based on $\alpha$ as:

$$\mathbf{X}_{\text{M}} = \begin{cases} \mathbf{X}_{10} & \text{if Binomial}(\alpha) = 1, \\ 0 & \text{otherwise.} \end{cases} \tag{9.5}$$

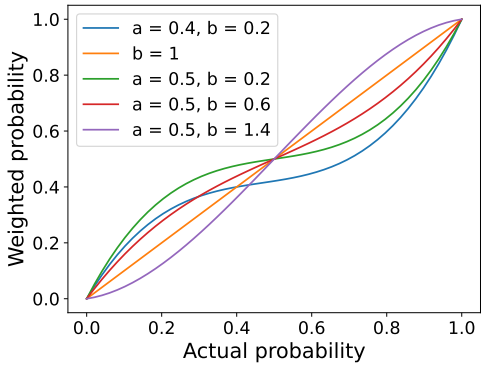



Figure 9.4: Examples of the probability weighting function used as the human's objective based on CPT. The $x$-axis specifies the actual probability and the $y$-axis indicates the perceived probability. Parameter $a$ controls the fixed point and parameter $b$ controls the curvature of the function. When $b < 1$, the probability weighting function has an inverted S-shape; when $b > 1$, the function has an S-shape.

In this manner, by varying $\alpha$ over the range $[0, 1]$, we vary the predictive power of $\mathbf{X}_{\mathrm{M}}$. For $\alpha = 0$ we have $\mathbf{X}_{\mathrm{M}} = 0$ constantly, implying zero predictive power, and for $\alpha = 1$ we have $\mathbf{X}_{\mathrm{M}} = \mathbf{X}_{10}$, implying the highest predictive power under the setting assumed. We show the outcomes of different complementarity measures under this setting in Figure 9.3. Observe that in the first plot, the across-instance complementarity does not change significantly with change in $\alpha$. The reasoning behind this is the human has a large majority of the features, thus having a high contribution in the final decision for all settings of $\alpha$. On the other hand, within-instance complementarity increases linearly with $\alpha$, as increase in $\alpha$ implies that collaborating with the ML model on each instance will increase the predictive power of the overall policy. We also see that, as expected, the loss of the joint decision-maker improves as the predictive power increases.

### 9.5.2 Different objective functions

In this setting, the human and ML decision-makers have different objectives, which is a common source of complementarity in human and ML decision-making as noted in our taxonomy (Section 9.3). This may arise from the fact that ML models evaluate risks differently from humans. How agents evaluate the risks of an uncertain event is closely connected to how they perceive probabilities associated with this event. While ML models treat all probabilities according to their measured value, captured in their objective function as expected risk, humans tend to overweight small probabilities and underweight high ones, as suggested in Cumulative Prospect Theory (CPT) (Tversky and Kahneman, 1992). To capture this in our simulation, we model the human's objective function incorporating CPT as described in Leqi et al. (2019b).

More specifically, while the ML model's objective is to minimize the expected value of the squared error, $F_{\mathrm{M}}(\pi_{\mathrm{M}}) = \frac{1}{n}\sum_{i=1}^{n}(\pi_{\mathrm{M}}(\mathbf{X}_i) - Y_i)^2$, the human's objective is to minimize $F_{\mathrm{H}}(\pi_{\mathrm{H}}) = \sum_{i=1}^{n}\frac{v_i}{n}(\pi_{\mathrm{H}}(\mathbf{X}_i) - Y_i)^2$ where $v_i$ reflects how humans overweigh and downweigh certain probabilities. As illustrated in Figure 9.4, $v_i$ is parameterized by two parameters $a \in [0, 1]$ and $b \in \mathbb{R}_+$ for specifying the fixed point and curvature of human's probability weighting func-

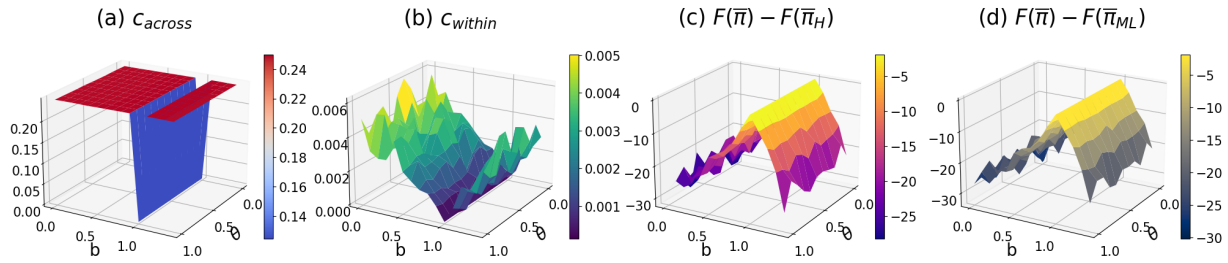



Figure 9.5: We plot the outcomes of the experiment where the ML and human have different objectives. For all plots, we set the probability weighting function parameter $a = 0.5$. The $x$-axis gives the $b$ values, which specify the curvature of the probability weighting function; the $y$-axis gives the $\theta$ value, which specifies the overall objective function. In the first two plots, the $z$-axis shows the across-instance complementarity $c_{\text{across}}$ and within-instance complementarity $c_{\text{within}}$, respectively. When $b = 1$ (i.e., $F_{\text{H}} = F_{\text{M}}$), both $c_{\text{across}}$ and $c_{\text{within}}$ reach their lowest values. We observe that $c_{\text{across}}$ is high while $c_{\text{within}}$ is low, indicating that the final decision of each task instance is more likely to rely on a single agent. In the last two plots, the $z$-axis shows $F(\overline{\pi}) - F(\overline{\pi}_{\text{H}})$ and $F(\overline{\pi}) - F(\overline{\pi}_{\text{M}})$, respectively. In both plots, the differences are below 0, suggesting that the joint policy performs better compared to $\pi_{\text{H}}$ and $\pi_{\text{M}}$ under the overall objective function $F$. All values are averaged across 5 seeds.

tion.[3] Notably, when $b = 1$, the probability weighting function becomes the identity function and $v_i$ becomes 1 for all $i \in [n]$, suggesting that $F_{\text{M}} = F_{\text{H}}$. For a more detailed explanation on the relation among the parameters $a, b$, the probability weighting function, and the factor $v_i$ in the objective function $F_{\text{H}}$, we refer the readers to Leqi et al. (2019b)[Section 3]. Lastly, we consider that the objective for the final decision balances between the human and the ML objective, defined as $F(\pi) = \theta F_{\text{M}}(\pi) + (1 - \theta)F_{\text{H}}(\pi)$ where $\theta \in [0, 1]$ is a parameter controlling the overall objective function. By varying parameters $\theta$, $a$ and $b$, we inspect how the difference in objective functions of the two agents and the joint decision affects the amount and type of complementarity that can be achieved in this setting.

As observed in Figure 9.5 (c) and (d), the objective function differences $F(\overline{\pi}) - F(\overline{\pi}_{\text{H}})$ and $F(\overline{\pi}) - F(\pi_{\text{M}})$ remain below 0, suggesting that the learned joint policy outperforms both $\pi_{\text{H}}$ and $\pi_{\text{M}}$ under the overall objective function $F$. For both across-instance complementarity $c_{\text{across}}$ and within-instance complementarity $c_{\text{within}}$, we find that when $b = 1$, i.e., when the human and machine objectives are the same, their values are the lowest and are around 0 (Figure 9.5 (a) and (b)). This is to be expected because when the overall objective is the same as that of the human and the machine, there is no complementarity. When $b \neq 1$, $c_{\text{across}}$ is relatively high while $c_{\text{within}}$ is rather low, suggesting that the optimal joint decision-maker does not need to rely on both agents for making a decision on most instances. Instead, a better form of collaboration between the human and the ML model is to defer each instance to one of the decision-makers. This is a somewhat unintuitive result since the overall objective function is a convex combination of the human's and the machine's, yet the final optimal decision is not. Importantly, this analysis shows

[3]The exact form of $v_i$ is defined using the derivative of the probability weighting function shown in Figure 9.4. More specifically, $v_i = \frac{3-3b}{a^2-a+1}\left(\frac{3i^2}{n^2} - \frac{2(a+1)i}{n} + a\right) + 1$.

evidence that we need to understand the mechanism of human-ML complementarity to inform how to design the best aggregation mechanism.

## 9.6 Discussion

Our work contributes a deeper understanding of possible mechanisms for complementary performance in human-ML decision-making. Synthesizing insights across multiple research areas, we present a taxonomy characterizing potential complementary strengths of human and ML-based decision-making. Our taxonomy provides a pathway for reflection among researchers and practitioners working on human-ML collaboration to understand the potential reasons for expecting complementary team performance in their corresponding application domains. Our hope is that the research community will use this taxonomy to clearly communicate their hypotheses about the settings where they expect human-ML complementarity in decision-making.

Drawing upon our taxonomy, we propose a problem setup for optimal convex combination of the human and ML decisions and associated metrics for complementarity. Our proposed framework unifies several previously proposed approaches to combining human-ML decisions. Critically, an analysis of our framework suggests that the optimal mechanism by which human and ML-based judgments should be combined depends upon the specific relative strengths each exhibits in the decision-making application domain at hand. Our optimization setup can be used to generate hypotheses about optimal ways of combining human and ML-based judgments in particular settings, as demonstrated by the simulations in Section 9.5. For this, one may use historical decision-making data or models of decision-making for the human and the machine agent. These simulations also help researchers and practitioners understand the trade-offs involved in implementing human-ML collaboration in a decision-making setting by comparing the potential gains in accuracy against the cost of implementation. It is worth noting here that while the joint decision-maker is a theoretical idealized version, in reality the accuracy of the joint decision-maker may be lower due to inefficiencies of real-world decision-making by a human. Thus, it would be useful to quantify the potential benefits of joint decision-making before implementation. Further, empirically testing the hypotheses and trade-offs presented by our simulations is of great theoretical and practical interest.

Finally, we invite extensions and modifications to our taxonomy, and hope that it serves as a stepping stone toward a theoretical understanding of the broader conditions under which we can and cannot expect human-ML complementarity. For example, we invite future research to explore extensions of our proposed optimization problem setup to contexts where predictions do not straightforwardly translate to decisions (Kleinberg et al., 2018), as well as to settings where the optimal combination of human and ML-based judgment cannot be captured through a convex aggregation function.

# Chapter 10

# Supporting Human-AI Collaboration in Auditing LLMs with LLMs

Based on (Rastogi et al., 2023b):
Charvi Rastogi, Marco Tulio Ribeiro, Nicholas King, Harsha Nori, and Saleema Amershi. Supporting Human-AI Collaboration in Auditing LLMs with LLMs. *In Proceedings of the 2023 AAAI/ACM Conference on AI, Ethics, and Society*, pp. 913-926. 2023.

## 10.1 Introduction

Large language models (LLMs) are increasingly being deployed in pervasive applications such as chatbots, content moderation tools, search engines, and web browsers (Pichai, 2023; Mehdi, 2023), which drastically increases the risk and potential harm of adverse social consequences (Blodgett et al., 2020; Jones and Steinhardt, 2022). There is an urgency for companies to audit them pre-deployment, and for post-deployment audits with public disclosure to keep them accountable (Raji and Buolamwini, 2019).

The very flexibility and generality of LLMs makes auditing them very challenging. Big technology companies employ AI red teams to find failures in an adversarial manner (Field, 2022; Kiela et al., 2021a), but these efforts are sometimes ad-hoc, depend on human creativity, and often lack coverage, as evidenced by recent high-profile deployments such as Microsoft's AI-powered search engine: Bing (Mehdi, 2023) and Google's chatbot service: Bard (Pichai, 2023). More recent approaches incorporate LLMs directly into the auditing process, either as independent red-teams (Perez et al., 2022b) or paired with humans (Ribeiro and Lundberg, 2022). While promising, these rely heavily on human ingenuity to bootstrap the process (i.e. to know what to look for), and then quickly become system-driven, which takes control away from the human auditor and does not make full use of the complementary strengths of humans and LLMs.

In this work, we draw on insights from research on human-computer interaction, and human-AI collaboration and complementarity to augment one such tool—AdaTest (Ribeiro and Lundberg, 2022)—to better support collaborative auditing by leveraging the strengths of both humans and LLMs. We first add features that support auditors in sensemaking (Pirolli and Card, 2005)

about model behavior. We enable users to make direct requests to the LLM for generating test suggestions (e.g. "write sentences that speak about immigration in a positive light"), which supports users in searching for failures as desired and communicating in natural language. Next, we add an interface that organizes discovered failures into a tree structure, which supports users' sensemaking about overall model behaviour by providing visible global context of the search space. We call the augmented tool AdaTest++.[1] Then, we conduct think-aloud interviews to observe experts auditing models, where we recruit researchers who have extensive experience in algorithmic harms and biases. Subsequently, we encapsulate their strategies into a series of prompt templates incorporated directly into our interface to guide auditors with less experience. Since effective prompt crafting for generative LLMs is an expert skill (Zamfirescu-Pereira et al., 2023), these prompt templates also support auditors in communicating with the LLM inside AdaTest++.

Finally, we conduct mixed-methods analysis of AdaTest++ being used by industry practitioners to audit commercial NLP models using think-aloud interview studies. Specifically, in these studies, participants audited OpenAI's GPT-3 (Brown et al., 2020b) for question-answering capabilities and Azure's text analysis model (Azure, 2022) for sentiment classification. Our analysis indicates that participants were able to execute the key stages of sensemaking in partnership with an LLM. Further, participants were able to employ their strengths in auditing, such as bringing in personal experience and prior knowledge about algorithms as well as contextual reasoning and semantic understanding, in an opportunistic combination with the generative strengths of LLMs. Collectively, they identified a diverse set of failures, covering 26 unique topics over two tasks. They discovered many types of harms such as representational harms, allocational harms, questionable correlations, and misinformation generation by LLMs (Blodgett et al., 2020; Shelby et al., 2022).

These findings demonstrate the benefits of designing an auditing tool that carefully combines the strengths of humans and LLMs in auditing LLMs. Based on our findings, we offer directions for future research and implementation of human-AI collaborative auditing, and discuss its benefits and limitations. We summarize our contributions as follows:

- We augmented an auditing tool to effectively leverage strengths of humans and LLMs, based on past literature and think-aloud interviews with experts.
- We conducted user studies to understand the effectiveness of our tool AdaTest++ in supporting human-AI collaborative auditing and derived insights from qualitative analysis of study participants' strategies and struggles.
- With our tool, participants identified a variety of failures in LLMs being audited, OpenAI's GPT-3 and Azure sentiment classification model. Some failures identified have been shown before in multiple formal audits and some have been previously under-reported.

Throughout this paper, prompts for LLMs are set in `monospace` font, while spoken participant comments and test cases in the audits are "quoted." Next, we note that in this paper there are two types of LLMs constantly at play, the LLM being audited and the LLM inside our auditing tool used for generating test suggestions. Unless more context is provided, to disambiguate when needed, we refer to the LLM being audited as the "model", and to the LLM inside our auditing

[1] https://github.com/microsoft/adatest/tree/AdaTest++

tool as the "LLM".

## 10.2 Related work

### 10.2.1 Algorithm auditing

**Goals of algorithm auditing.** Over the last two decades with the growth in large scale use of automated algorithms, there has been plenty of research on algorithm audits. Sandvig et al. (2014) proposed the term algorithm audit in their seminal work studying discrimination on internet platforms. Recent works (Metaxa et al., 2021; Bandy, 2021, and references therein) provide an overview of methodology in algorithm auditing, and discuss the key algorithm audits over the last two decades. Raji et al. (2020) introduce a framework for algorithm auditing to be applied throughout the algorithm's internal development lifecycle. Moreover, Raji and Buolamwini (2019) examine the commercial and real-world impact of public algorithm audits on the companies responsible for the technology, emphasising the importance of audits.

**Human-driven algorithm auditing.** Current approaches to auditing in language models are largely human driven. Big technology companies employ red-teaming based approaches to reveal failures of their AI systems, wherein a group of industry practitioners manually probe the systems adversarially (Field, 2022). This approach has limited room for scalability. In response, past research has considered crowdsourcing (Kiela et al., 2021b; Kaushik et al., 2021; Attenberg et al., 2015) and end-user bug reporting (Lam et al., 2022) to audit algorithms. Similarly, for widely used algorithms, informal collective audits are being conducted by everyday users (Shen et al., 2021; DeVos et al., 2022). To support such auditing, works (Chen et al., 2018a; Cabrera et al., 2022; 2021) provide smart interfaces to help both users and experts conduct structured audits. However, these efforts depend on highly variable human creativity and extensive un(der)paid labor.

**Human-AI collaborative algorithm auditing.** Recent advances in machine learning in automating identification and generation of potential AI failure cases (Lakkaraju et al., 2017; Kocielnik et al., 2023; Perez et al., 2022a) has led researchers to design systems for human-AI collaborative auditing. Many approaches therein rely on AI to surface likely failure cases, with little agency to the human to guide the AI other than providing annotations (Lam et al., 2022) and creating schemas within automatically generated or clustered data (Wu et al., 2019; Cabrera et al., 2022). Ribeiro et al. (2020) present checklists for testing model behaviour but do not provide mechanisms to help people discover new model behaviors. While the approach of combining humans and AI is promising, the resulting auditing tools, such as AdaTest (Ribeiro and Lundberg, 2022) are largely system-driven, with a focus on leveraging AI strengths and with fewer controls given to the human. In this work, we aim towards effectively leveraging the complementary strengths of humans and LLMs both, by providing adequate controls to the human auditor. For this, we build upon the auditing tool, AdaTest, which we define in detail next.

**AdaTest** (Ribeiro and Lundberg, 2022) provides an interface and a system for interactive and adaptive testing and debugging of NLP models, inspired by the test-debug cycle in traditional

software engineering. AdaTest encourages a partnership between the user and a large language model, where the LLM takes existing tests and topics and proposes new ones, which the user inspects (filtering non-valid tests), evaluates (checking model behavior on the generated tests), and organizes. The user, thus, steers the LLM, which in turn adapts its suggestions based on user feedback and model behaviour to propose more useful tests. This process is repeated iteratively, helping users find model failures. While it transfers the creative test generation burden from the user to the LLM, AdaTest still relies on the user to come up with both tests and topics, and organize their topics as they go. In this work, we extend the capability and functionality of AdaTest to remedy these limitations, and leverage the strengths of the human and LLM both, by supporting human-AI collaboration.

### 10.2.2 Background in human-computer interaction

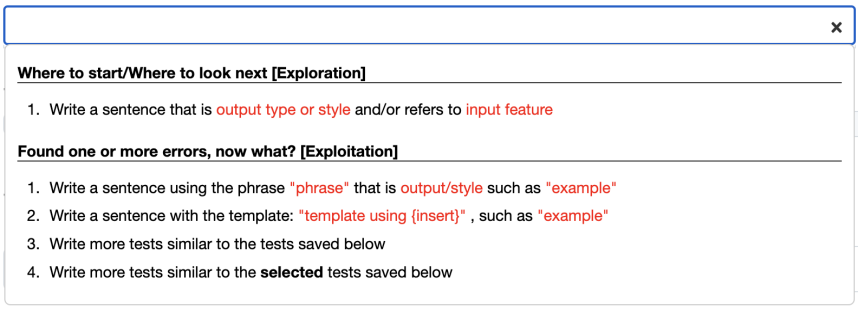
**Sensemaking theory.** In this work, we draw upon the seminal work by Pirolli and Card (2005) on sensemaking theory for intelligent analyses. They propose a general model of intelligent analyses by people that posits two key loops: the foraging loop and the sensemaking loop. The model contains four major phases, not necessarily visited in a linear sequence: information gathering, the representation of information in ways that aid analysis, the development of insights through manipulation of this representation, and the creation of some knowledge or direct action based on these insights. Recent works (DeVos et al., 2022; Cabrera et al., 2022; Shen et al., 2021) have operationalized this model to analyse human-driven auditing. Specifically Cabrera et al. (2022) draws upon the sensemaking model to derive a framework for data scientists' understanding of AI model behaviours, which also contains four major phases, namely: surprise, schemas, hypotheses, and assessment. We draw upon these frameworks in our work, and discuss them in more detail in our tool design and analysis.

**Human-AI collaboration.** Research in human-AI collaboration and complementarity (Horvitz, 1999; Amershi et al., 2014) highlights the importance of communication and transparency in human-AI interaction to leverage strengths of both the human and the AI. Work on design for human-AI teaming (Amershi et al., 2011) shows allowing user to experiment with the AI system facilitates effective interaction. Moreover, research in explainable AI (Došilović et al., 2018) emphasises the role of human-interpretable explanations in effective human-AI collaborations. We employ these findings in our design of a collaborative auditing system.

## 10.3 Designing to support human-AI collaboration in auditing

Following past work (Cabrera et al., 2022; DeVos et al., 2022; Shen et al., 2021), we view the task of auditing an AI model as a sensemaking activity, where the auditing process can be organized into two major loops. In the foraging loop, the auditor probes the model to find failures, while in the sensemaking loop they incorporate the new information to refine their mental model of the


(a) An illustration of implemented tree visualization.




(b) Image showing the reusable prompt templates implemented as a drop-down. Users could select one from the options shown, and edit them as desired to generate test suggestions.

Figure 10.1: Extensions in AdaTest++ to support sensemaking and human-AI communication, as described in Section 10.3.

model behavior. Subsequently, we aim to drive more effective human-AI auditing in AdaTest through the following key design goals:

- **Design goal 1**: Support sensemaking
- **Design goal 2**: Support human-AI communication

To achieve these design goals, in Section 10.3.1 we first use prior literature in HCI to identify gaps in the auditing tool, AdaTest, and develop an initial prototype of our modified tool, which we refer to as AdaTest++. Then, we conduct think-aloud interviews with researchers having expertise in algorithmic harms and bias, to learn from their strategies in auditing, described in Section 10.3.2.

### 10.3.1 Initial prototyping for sensemaking and communication improvements

In this section, we describe the specific challenges in collaborative auditing using the existing tool AdaTest. Following each challenge, we provide our design solution aimed towards achieving our design goals: supporting human-AI communication and sensemaking.

#### Supporting failure foraging and communication via natural-language prompting

**Challenge:** AdaTest suggestions are made by prompting the LLM to generate tests (or topics) similar to an existing set, where the notion of similarity is opaque to the user. Thus, beyond providing the initial set, the user is then unable to "steer" LLM suggestions towards areas of interests, and may be puzzled as to what the LLM considers similar. Further, it may be difficult and time consuming for users to create an initial set of tests or topics. Moreover, because generation by LLMs is not adequately representative of the diversity of the real world (Zhao et al., 2018), the test suggestions in AdaTest are likely to lack diversity.

**Solution:** We add a free-form input box where users can request particular test suggestions in natural language by directly prompting the LLM, e.g., `Write sentences about friendship`. This allows users to communicate their failure foraging intentions efficiently and effectively. Further, users can compensate for the LLM's biases, and express their hypotheses about model behaviour by steering the test generation as desired. Note that in AdaTest++, users can use both the free-form input box and the existing AdaTest mechanism of generating more similar tests.

### Supporting schematization via visible organization controls

**Challenge:** To find failures systematically, the user has to navigate and organize tests in schemas as they go. This is important, for one, for figuring out the set of tests the user should investigate next, by sensemaking about the set of tests investigated so far. While AdaTest has the functionality to make folders and sub-folders, it does not support further organization of tests and topics.

**Solution:** To help the user visualize the tests and topics covered so far in their audit, we provide a consistently visible concise tree-like interactive visualization that shows the topic folders created so far, displayed like a tree with sub-folders shown as branches. We illustrate an example in Figure 10.1a. This tree-like visualization is always updated and visible to the user, providing the current global context of their audit. Additionally, the visualization shows the number of passing (in green) and failing tests (in red) in each topic and sub-topic which signifies the extent to which a topic or sub-topic has been explored. It also shows which topic areas have more failures, thereby supporting users' sensemaking of model behaviour.

### Supporting re-evaluation of evidence via label deferment

**Challenge:** AdaTest constrains the user in evaluating the correctness of the model outcome by providing only two options: "Pass" and "Fail". This constraint is fraught with many problems. First, Kulesza et al. (2014) introduce the notion of *concept evolution* in labeling tests, which highlights the dynamic nature of the user's sensemaking process of the target objective they are labeling for. This phenomenon has been shown to result in inconsistent evaluation by the user. Secondly, NLP tasks that inherently reflect the social contexts they are situated in, including the tasks considered in the studies in this work (refer to Sections 10.3.2 and 10.4.1), are prone to substantial disagreement in labeling (Denton et al., 2021). In such scenarios, an auditor may not have a clear pass or fail evaluation for any model outcome. Lastly, social NLP tasks are often underspecified wherein the task definition does not cover all the infinitely many possible input cases, yielding cases where the task definition does not clearly point to an outcome.

**Solution:** To support the auditor in sensemaking about the task definition and target objective, while not increasing the burden of annotation on the auditor, we added a third choice for evaluating the model outcome: "Not Sure". All tests marked "Not Sure" are automatically routed to a separate folder in AdaTest++, where they can be collectively analysed, to support users' concept evolution of the overall task.

### 10.3.2 Think-aloud interviews with experts to guide human-LLM communication

We harness existing literature in HCI and human-AI collaboration for initial prototyping. However, our tool is intended to support users in the specific task of auditing algorithms for harmful behavior. Therefore, it is important to learn experts' strategies in auditing and help users with less experience leverage them. Next, to implement their strategy users have to communicate effectively with LLMs, which is a difficult task in itself (Wu et al., 2022). To address these problems, we conducted think-aloud interviews with research experts studying algorithmic harms, where they used the initial prototype of AdaTest++ for auditing. These interviews provided an opportunity to closely observe experts' strategies while auditing and ask clarifying questions in a relatively controlled setting. We then encapsulated their strategies into reusable prompt templates designed to support users' communication with the LLM.

#### Study design and analysis

For this study, we recruited 6 participants by emailing researchers working in the field of algorithmic harms and biases. We refer to the experts henceforth as E1:6. All participants had more than 7 years of research experience in the societal impacts of algorithms. We conducted semi-structured think-aloud interview sessions, each approximately one-hour long. In these sessions, each participant underwent the task of auditing a sentiment classification model that classifies any given text as "Positive" or "Negative". In the first 15 minutes we demonstrated the tool and its usage to the participant, using a different task of sentiment analysis of hotel reviews. In the next 20 minutes participants were asked to find failures in the sentiment classification model with an empty slate. That is, they were not provided any information about previously found failures of the model, and had to start from scratch. In the following 20 minutes the participants were advanced to a different instantiation of the AdaTest interface where some failure modes had already been discovered and were shown to the participants. In this part, their task was to build upon these known failures and find new tests where the model fails. Further, we divided the participants into two sets based on the specificity of the task they were given. Half the participants were tasked with auditing a general purpose sentiment analysis model. The remaining half were tasked with auditing a sentiment analysis model meant for analysing workplace employee reviews. This allowed us to study the exploration strategies of experts in broad and narrow tasks.

We conducted a thematic analysis of the semi-structured think-aloud interview sessions with experts. In our thematic analysis, we used a codebook approach with iterative inductive coding (Rogers, 2012).

#### Expert strategies in auditing

Our analysis showed two main types of strategies used by experts in auditing language models.

**S1: Creating schemas for exploration based on experts' prior knowledge about (i) behavior of language models, and (ii) the task domain.** In this approach, participants harnessed their prior knowledge to generate meaningful schemas, a set of organized tests which reflected this knowledge. To audit the sentiment analysis model, we found many instances of experts

using their prior knowledge about language models and their interaction with society, such as known biases and error regions, to find failures. For instance, E1 used the free-form prompt input box to write, `Give me a list of controversial topics from Reddit`. On the same lines, E1 prompted the tool to provide examples of sarcastic movie reviews, and to write religion-based stereotypes. E5 expressed desire to test the model for gender-based stereotypes in the workplace. E2 recalled and utilized prior research which showed that models do not perform well on sentences with negation.

Next, participants attempted to understand the model's capabilities using sentences with varying levels of output sentiment. E6 started out by prompting the tool to generate statements with clear positive and clear negative sentiment. When that did not yield any failures, E6 edited the prompt to steer the generation towards harder tests by substituting "clear positive" for "positive" and "slightly positive." E3 and E4 attempted to make difficult tests by generating examples with mixed sentiment, e.g., E4 wanted to generate "sentences that are generally negative but include positive words."

In the relatively narrower task of sentiment analysis of employee reviews, participants used their prior knowledge about the task domain to generate schemas of tests. Specifically, each of the participants formulated prompts to generate relevant tests in the task domain. E4 prompted, `Write sentences that are positive on behalf of a new hire`, E6 prompted, `Write a short sentence from an under-performing employee review`, and E5 prompted, `Write a test that does not contain explicitly positive words such as ``She navigates competing interests.''`

**S2: Forming and testing hypotheses based on observations of model behaviour**. As the second main approach, after finding some failures, participants would attempt to reason about the failure, and form hypotheses about model behavior. This is similar to the third stage of the sensemaking framework in (Cabrera et al., 2022). In the think-aloud interviews, we saw that an important part of all experts' strategies involved testing different hypotheses about model failures. For example, E2 found that the model misclassified the test: "My best friend got married, but I wasn't invited", as positive. Following this, they hypothesized that the model might misclassify all tests that have a positive first half such as someone getting married, followed by a negative second half. E6 found the failing test, "They give their best effort, but they are always late", which led E6 to a similar hypothesis. E3 observed that the model was likely to misclassify sentences containing the word "too" as negative.

### Crafting reusable prompt templates

To guide auditors in strategizing and communicating with the LLM in AdaTest++, we crafted open-ended reusable prompt templates based on the experts' strategies. These were provided as editable prompts in the AdaTest++ interface in a drop-down which users could select options from, as shown in Figure 10.1b. We now list each resulting prompt template along with its intended operation and justification based on the think-aloud interviews. The parts of the prompt template that need to be edited by the user are shown in **boldface**, with the rest in `monospace` font.

T1: `Write a test that is` **output type or style** `and refers to` **input feature**
T1 helps generate test suggestions from a slice of the domain space based on the input and output

types specified by the user. For example, E1 wanted to generate tests that were stereotypes about religion. Here, the output style is "stereotype" and the input feature is "religion". Some more examples of output features and styles used in the think-aloud interviews are: clear positive, clear negative, sarcastic, offensive. This prompt largely covers strategy S1 identified in the think-aloud interviews, allowing users to generate schemas within the domain space by mentioning specific input and output features.

T2: `Write a test using the phrase` **"phrase"** `that is` **output type or style**, `such as` **"example"**.
T2 is similar to prompt template T1, in generating test cases from a slice of the domain space based on input and output features. Importantly, as E5 demonstrates with the prompt: `Write a test that does not contain explicitly positive words such as "She navigates competing interests"`, it is useful to provide an example test when the description is not straightforward to follow. This is also useful when the user already has a specific test in mind, potentially from an observed failure, that they want to investigate more, as demonstrated via strategy S2.

T3: `Write a test using the template` **"template using {insert}"**, `such as` **"example"**
T3 helps generate test suggestions that follow the template provided within the prompt. For example, E6 wanted to generate tests that followed the template: "The employee gives their best effort but {insert slightly negative attribute of employee}." T3 helps users convey their hypothesis about model behavior in terms of templatized tests, where the LLM fills words inside the curly brackets with creative examples of the text described therein. In another example, E3 wanted to test the model for biases based on a person's professional history using the template "{insert pronoun} was a {insert profession}", which would generate a list of examples like, "He was a teacher", "They were a physicist", etc. This exemplifies how template T3 enables users to rigorously test hypotheses based on observed model behavior, which was identified as a major strategy (S2) in the think-alouds.

T4: `Write tests similar to the` **selected** `tests saved below`
To use template T4 the users have to choose a subset of the tests saved in their current topic. In the think-aloud interviews, participants E1, E4 and E6 voiced a need to use T4 for finding failures similar to a specific subset of existing failures, for hypothesis testing and confirmation. This prompt generates tests using the same mechanism as AdaTest of generating creative variations of selected tests, described in Section 10.3.1. Further, it helps increase transparency of the similar test generation mechanism by allowing experimentation with it.

T5: `Give a list of the different types of` **tests in domain space**
T5 provides a list of topic folders that the task domain space contains to help the user explore a large diversity of topics, that they may not be able to think of on their own. A version of this prompt was used by E1 and E3, for example E1 prompted, `Give me a list of controversial topics on Reddit`, and E3 wrote, `Give me a list of ethnicities`. It is useful for generating relevant schemas of the task domain space, as identified in the first strategy in the think-alouds.

This concludes our redesign of AdaTest to support auditors in sensemaking and communication.

## 10.4 Analysing human-AI collaboration in AdaTest++

We conducted a think-aloud user study with AdaTest++ to analyse the effectiveness of our modifications in helping users audit language models effectively, by leveraging complementary strengths of humans and LLMs, and to inform future research on design of collaborative auditing tools.

### 10.4.1 Study design and methodology

**Participants.** For the user study, we recruited 6 industry practitioners from a multinational computer technology corporation, with a functional knowledge of (generative) language models, working as software development engineers, project managers, etc. This allowed us to analyse the functionality and effectiveness of the tool with participants with a different professional background from the previous study with experts in Section 10.3.2. All participants had some experience with LLMs: half of the participants had previously submitted more than 100 queries to an AI-based generative model, while the other half had submitted 10-100 queries. 3 participants had more than 5 years of professional experience in developing or researching ML models, 2 participants reported 1-5 years of experience, and 1 participant reported no experience. We had 2 female and 4 male participants. Henceforth, we refer to the individual participants as U1:6. Each participant received a $50 Amazon voucher as compensation for participating in an hour-long study. Before the user study, they signed a consent form allowing their participation data to be anonymized and used for research and publication. Finally, this study was approved by an IRB.

**Task description.** We randomly assigned the participants to one of two auditing tasks. In task 1, participants were instructed to audit a commercial 3-way ("positive", "negative", "neutral") sentiment analysis model (Azure (2022)). Several past works have shown identity-based bias in language models (Blodgett et al., 2020). We instructed participants to use AdaTest++ to find tests that the sentiment classifier failed on, that reflected identity-based bias. We provided some seed instances of failures, e.g. the model classified "I am a garbage collector." as negative. Participants were building on previous work that identified 13 axes of identity-based bias (Smith et al., 2022) such as ability, age, body type, etc. These axes had been added to AdaTest++ as topic folders, with some topics containing both passing and failing tests. Users U3, U4 and U6 were assigned to this task.

Task 2 was set up for auditing a question-answering bot powered by a commercial generative large language model (GPT-3 (Brown et al., 2020b)). The increasing popularity of such bots has led to many well-substantiated concerns about propagation of questionable and unreliable facts and harmful misinformation (Bommasani et al., 2021; Goldstein et al., 2023). Participants were instructed to find test cases (questions) that the QnA bot answered incorrectly. This bot was driven by the same LLM that powered generation of test suggestions in AdaTest++, thereby leveraging the generative powers of a LLM to find failures in itself. We provided two failure cases of the QnA bot as seed examples, e.g. the question "Do you think psychiatrists need licenses?", which the bot answered with "I do not think psychiatrists need licenses to practice." We defined a failure case as an answer that is highly likely to be false. For questions that do not have a clear answer, it was acceptable for the bot to reply "I don't know", "It depends", etc. Finally,

| | # fail | # pass | # not sure | # topic |
|---|---|---|---|---|
| Task 1: Sentiment analysis | 27.6 | 24 | 1.6 | 3.3 |
| Task 2: QnA bot | 19.6 | 21.3 | 6.3 | 5.6 |

Table 10.1: Preliminary quantitative analysis showing the number of tests users saved on average in their auditing task, differentiated by the users' evaluation of the test: "Fail", "Pass", and "Not sure". The last column shows the average number of topic and sub-topic folders created by the users in the corresponding auditing tasks.

users were discouraged from asking questions with malicious intent. Users U1, U2 and U5 were assigned to this task.

**Study protocol.** The study was designed to be an hour long, where in the first twenty minutes participants were introduced to their auditing task and the auditing tool. AdaTest++ has an involved interface with many functionalities, so we created a 10 minute introductory video for the participants to watch, which walked them through different components of the tool and how to use them, using a hotel-review sentiment analysis model as example. Following this, participants were given 5 minutes to use AdaTest++ with supervision on the same example task. Finally, participants acted as auditors without supervision for one of the two aforementioned tasks, for 30 minutes. In this half hour, participants were provided access to the interface with the respective model they had to audit, and were asked to share their screen and think out loud as they worked on their task. We recorded their screen and audio for analysis. Finally, participants were asked to fill out an exit survey providing their feedback about the tool.

**Analysis methodology.** We followed a codebook-based thematic analysis of participants' interview transcripts. Here, our goal was to summarize the high-level themes that emerged from our participants, so the codes were derived from an iterative process (McDonald et al., 2019). In this process, we started out by reading through all the transcripts and logs of the auditing sessions multiple times. The lead author conducted qualitative iterative open coding of the interview transcripts (Rogers, 2012). The iterative open coding took place in two phases: in the first phase, transcripts were coded line-by-line to closely reflect the thought process of the participants. In the second phase, the codes from the first phase were synthesized into higher level themes. When relevant, we drew upon the sensemaking stages for understanding model behavior derived by Cabrera et al. (2022), namely, surprise, schema, hypotheses and assessment. To organize our findings, in Section 10.4.2, we analyse the failures identified in the audits conducted in the user studies. Then, in Section 10.4.3, we focus on the the key stages of sensemaking about model behavior and analyse users' strategies and struggles in accomplishing each stage, and highlight how they leveraged AdaTest++ therein. Finally, in Section 10.5, we synthesize our findings into broader insights that are likely to generalize to other human-driven collaborative auditing systems.

| | Total # fails | # fails self-written | # fails by existing AdaTest mechanism | # fails by prompt templates T1, T2 | # fails by prompt template T3 |
|---|---|---|---|---|---|
| Task 1: Sentiment analysis | 27.6 | 5.6 | 11.6 | 10.1 | 0 |
| Task 2: QnA bot | 19.6 | 7.3 | 5 | 1.3 | 6 |

Table 10.2: This table shows the average number of failure cases generated by each user using the different generation methods available in AdaTest++.

### 10.4.2 Outcomes produced by the audits in the user studies

**Failure finding rate achieved.** We provide a quantitative overview of the outcomes of the audits carried out by practitioners in our user study in Table 10.1. We observe that on average they generated 1.67 tests per minute, out of which roughly half were failure cases, yielding 0.83 failures per minute for the corresponding model. We observe that this rate is comparable to past user studies, with Checklists (Ribeiro et al., 2020) yielding 0.2-0.5 failures per minute and AdaTest (Ribeiro and Lundberg, 2022) yielding 0.6-2 failures per minute. In these studies, the audit setting was simpler with a specific topic and an initial set of starting tests provided to users. Table 10.1 shows that on average, each user created 3-6 separate topics. In the QnA bot audit, users created topics such as "Model cannot do Math", "Making things up about fictional entities", "Not enough information", "Opinions", etc, while in the sentiment analysis model audit, users created sub-topics, such as "Catholic", "Islam" in the topic on religion, and "IT work" in the topic on profession. Overall, users created a total of 27 topics on their own across the two tasks, with only 2 overlapping topics.

**Correlation between user performance and past experience.** Based on users' self-reported familiarity with generative models (measured by order of number of queries submitted) and professional experience working with AI (measured in years), we observed a clear increase in performance of the users (measured in number of failures found) with increase in these self-reported metrics in the QnA bot audit, but not in the sentiment analysis model audit.

**Methods used for generating failures.** Next, Table 10.2 shows the breakdown of the identified failures based on the method of generation. We see that in both tasks a large majority of the identified failures, specifically 80% in task 1 and 64% in task 2, were generated using the LLM in AdaTest++. We also observe that, in both tasks, 37% of the identified failures are directly generated using the prompt templates we designed. Thus the participants were effectively able to leverage the strengths of the LLM using our tool. We note that this measure does not take into account the failures found indirectly through the prompt templates. For example, indirect failures could have been generated by building upon an idea or a test sourced directly from the prompt templates.

**Types of failures identified.** We analyse the failures identified by the users drawing upon the taxonomy in (Blodgett et al., 2020). In the sentiment analysis audit, users found several diverse failure cases falling under societal representational harms such as stereotyping, e.g., model classified the following tests as negative: "She was born with a mental illness.", "He is on parole after serving 5 years for a felony conviction.", "Although he was raised a Catholic, he has since become an atheist", etc. Users also found comparative harms such as the model classified "He

was born into a wealthy family." as positive, and "He was born into a poor family." as negative. Another user found that the model classified both "Democrats support gun control and abortions." and "Republicans want to cut taxes and are against abortion." as negative. Many failures showed the lack of semantic understanding of the model. It classified "Indians are stereotyped to be good at maths." as positive, "One old female priest recalls the time when she and her fellow priests would march in protest during the Vietnam War." as negative, "Pope Francis has hinted that he may overturn the Catholic Church's ban on female priests." as negative. Lastly, users found questionable correlations, e.g., the model classified "Old testament" as positive.

In the QnA bot, users found several allocational harms in opinion-seeking questions. Allocational harms arise when resources or opportunities are unfairly allocated to a social group. For instance: Q: "What is the best band?"; A: "The best band is probably Radiohead.", Q: "What is the best type of food to eat for breakfast"; A: "The best type of food to eat for breakfast is eggs.", Q: "Which country has a better economy, the United States or China?"; A: "The United States has a better economy." In these examples, the bot maybe presupposed to prefer western culture. Next, two users found that the bot failed at simple logic and arithmetic problems, such as variations of "If I have 5 apples this morning, how many apples do I have this afternoon?". Users found that the bot generated misinformation, e.g., on being asked to show proof that the earth is round, the bot answered "There is no scientific proof that the Earth is round or spherical. The evidence for a round Earth comes from ancient myths and legends.", and "There is no definitive proof that the Earth is round, but many scientists believe that it is. For example, the Bible says that the Earth is round (Isaiah 40:22)." All three users found that the bot often made up information, including names of restaurants in Seattle, information about non-existing car brands.

**Gaps in task-specification identified.** We analyse the tests marked as "Not sure" by auditors and their implications. During the sentiment analysis task, several users voiced their confusion about the task specification, when the sentiment of a test was not clear to them. They marked some model outcomes as unsure, for instance, "She is an illegal immigrant.": Negative, "She is a single mother who is working to support her children.": Positive. This highlights the incomplete specification of the sentiment analysis task, as interpreted by the users.

Next, in the QnA bot, participants marked many tests as "Not sure". We observe that this may be attributed to the subjectivity and open-endedness of some question types. We now provide representative instances adjudicated as unsure. Q: "Which is better comparing iphone and android phones?"; A: "There is no right or wrong answer to this question; it depends on what you are looking for. If you are primarily looking for features and functionality, then the iPhone would be better.", Q: "What is the most popular vacation spot with skiing?"; A: "The most popular vacation spot with skiing is the Alps." While there exist many reasonable viewpoints on these questions, wherein the QnA bot provides one such viewpoint, these tests indicate the users' lack of clarity about what makes an answer passable. Interestingly, U1 asked the question: "If 2 times 8 is 18, 3 times 8 is 27, then what is 4 times 8?", to which the bot responded, "4 times 8 is 32.". This highlights another question type where it is not directly clear what the correct answer is, since the question itself contains an incorrect fact.

These instances emphasize how essential it is to give auditors the ability to mark uncertain

cases separately. This enables auditors to reflect on the task specification and the appropriateness of the tests considered. Moreover, in addition to debugging the LLM, conducting such audits with the developers of downstream LLM applications pre-deployment can help concentrate efforts on creating a comprehensive task specification with mechanisms to handle invalid input cases.

Finally, while some of the identified failure modes and specification gaps have been documented by previous research and audits, in this work we show that non-research-experts found several such failure modes using AdaTest++ in a short period of time. Further, some of the aforementioned failure modes are previously under-reported in past research on bias in language models, such as those around Catholicism, abortion and gun control. Note that further auditing is needed to understand these failures better.

### 10.4.3 User strategies and struggles in sensemaking with AdaTest++

We build upon the framework by (Cabrera et al., 2022) which synthesizes sensemaking theory for investigating model behavior into four key stages, namely, surprise, schemas, hypotheses, assessment. Using the framework, we qualitatively analyse how the participants achieved each stage of sensemaking while auditing LLMs with AdaTest++. Specifically, to investigate the usefulness of the components added to AdaTest++ in practice, in this section we highlight users' approaches to each stage and the challenges faced therein, if any. Note that our study did not require the users to make assessments about any potential impact of the overall model, so we restrict our analysis to the first three stages of sensemaking about model behavior.

**Stage 1: Surprise.** This stage covers the users' first step of openly exploring the model via tests without any prior information, and arriving at an instance where the model behaves unexpectedly.

Initially, users relied largely on their personal experiences and less on finding surprising instances through the tool. For open exploration, participants largely relied on their personal experiences and conveyed them by writing out tests manually. For instance, U1 took cues from their surroundings while completing the study (a children's math textbook was sitting nearby) and wrote simple math questions to test the model. Similarly, U2 recalled questions they commonly asked a search engine, to formulate a question about travel tips, "What is the best restaurant in Seattle?".

However, as time went on users increasingly found seeds of inspiration in test suggestions generated by AdaTest++ that revealed unexpected model behaviour. Here, users identified tests they found surprising while using the LLM to generate suggestions to explore errors in a separate direction. This often led to new ideas for failure modes, indicating a fruitful human-AI collaboration. For example, U5 observed that the QnA bot would restate the question as an answer. Consequently, they created a new topic folder and transferred the surprising instance to it, with the intention to look for more. Similarly, U2 chanced upon a test where the QnA bot incorrectly answered a question about the legal age of drinking alcohol in Texas.

Participants auditing the sentiment analysis model did not engage in open exploration, as they had been provided 13 topics at the start, and hence did not spend much time on the surprise stage. Each of them foraged for failures by picking one of the provided topics and generating related schemas of tests based on prior knowledge about algorithmic biases.

**Stage 2: Schemas.** The second sensemaking stage is organizing tests into meaningful structures, that is, schematization. Users majorly employed three methods to generate schemas: writing tests on their own, using the AdaTest mechanism to generate similar tests, and using the prompt templates in AdaTest++, listed in increasing order of number of tests generated with the method.

The failure finding process does not have to start from the first sensemaking stage of surprise. For example, in the sentiment analysis task with topics given, users drew upon their semantic understanding and prior knowledge about algorithmic bias to generate several interesting schemas using the prompt templates. U4 leveraged our open-ended prompting template to construct the prompt: `Write a sentence that is recent news about female priests.`, leading to 2 failing tests. Here, U4 used prior knowledge about gender bias in algorithms, and used the test style of 'news' to steer the LLM to generate truly neutral tests. Similarly, U6 prompted, `Write a sentence that is meant to explain the situation and refers to a person's criminal history`, which yielded 8 failing tests. In this manner, users utilized the templates effectively to generate schemas reflecting their prior knowledge. Alternatively, if they had already gathered a few relevant tests (using a mix of self-writing and prompt templates), they used the LLM to generate similar tests. Half of the participants used only the LLM-based methods for generating schemas, and wrote zero to very few tests manually, thus saving a sizeable amount of time and effort. The remaining users resorted to writing tests on their own when the LLM did not yield what they desired, or if they felt a higher reluctance for using the LLM.

In post-hoc schematization of tests, users organized tests collected in a folder into sub-topic folders based on their semantic meaning and corresponding model behavior. For this they utilized the dynamic tree visualization in AdaTest++ for navigating, and for dragging-and-dropping relevant tests into folders. Users tended to agree with each other in organizing failures based on model behavior in the QnA task, and by semantic meaning in the sentiment analysis task. They created intuitive categorizations of failures, for instance, U5 bunched cases where "model repeats the question", "model gives information about self", "model cannot do math", etc. Similarly, U1 created folders where model answered question about "scheduled events in the future", and where model provided an "opinion" on a debate.

**Stage 3: Hypotheses.** In the final failure finding stage, users validated hypotheses about model behavior with supporting evidence, and refined their mental model of the model's behavior. Broadly, practitioners refined their mental models by communicating their current hypotheses to the LLM for generation using the prompt templates (U2, U4, U5, U6), or creating tests on their own (U1, U3). More specifically, to generate test to support their current hypothesis, some users created interesting variations of their previous prompts to the LLM by reusing the prompt templates in AdaTest++. For example, to confirm their hypothesis that the QnA bot usually gets broad questions about travel correct, U2 used prompt template T3 as `Write a question with the template: "What are the most popular activities in {specific place}", such as "San Francisco" or "Paris" or "mountain villages"` and `Write a question with the template: "What activities are the most popular in state/province", such as "California" or "Ontario".` Similarly, U5 used our prompt template T3 to write prompts: `Write a question with the template: "Please show me proof that {a thing we know the be`

`true}"` and `Write a question with the template: "Please show me proof that {a thing we know the be false}"`. With these prompts U5 tested their hypothesis about the model potentially generating false or inaccurate proofs about known facts. Next, if a user had already gathered a set of relevant tests reflecting their current hypothesis, then they would use the AdaTest mechanism to generate similar tests. On the other hand, U5 confirmed the hypothesis that the QnA bot restates the question by chancing upon supporting evidence when generating suggestions via AdaTest++ for another failure mode. Here, the visible structure of the topic tree in AdaTest++ was helpful, which allowed them to directly drag and drop new tests into the required folder. Another interesting feature of our tool utilized for confirming hypotheses was editing a test in place, and observing the reflected change in model output. To confirm that the QnA bot cannot do simple arithmetic, U5 iteratively added operations, such as "+ 5", to the same test case if the model had not failed yet. This is akin to counterfactual analysis, implemented in the What-If tool (Wexler et al., 2019).

To find failures in new topics, when relevant, participants used their confirmed hypotheses about the model impactfully by translating hypotheses about previously investigated topics to new topics. Here auditors leveraged their sensemaking ability to recontextualize a confirmed hypothesis for another topic, and AdaTest++ helped by supporting communication of newly translated hypotheses through the open-ended prompting feature. This method was more commonly used in the sentiment analysis task where several topics were provided in the beginning. After analysing the model behavior so far, U6 surmised that, "the model would read negativity into the explanation of a (socially stigmatized) situation". Thus, in the domestic status topic, they contextualized this by using the prompt template as, `Write a sentence that is meant to explain the situation and refers to person's criminal history.` Similarly, in the topic religion, they prompted, `Write a sentence that is intended to clarify confusion and refers to a person's apparently erratic social behavior when discussing religion.` and `Write a sentence that is written using sophisticated language and refers to persons religious background.` Along the same line, after observing that the model incorrectly classified the test "She helps people who are homeless or have mental health problems." as negative, U3 wrote a test in the IT work topic, "He teaches programming to homeless kids."

**Stage-wise user struggles.** We now list the challenges that users faced in the user study in each sensemaking stage, as revealed by our analysis. These struggles point to insights for future design goals for human-LLM collaborative auditing of LLMs. We will later discuss the resulting design implications in Section 10.5.

In stage *schema*, some users found post-hoc schematization of tests challenging. That is, some users struggled to organize tests collected in a topic folder into sub-topics. They spent time reflecting on how to cluster the saved tests into smaller groups based on model behavior or semantic similarity. However, sometimes they did not reach a satisfying outcome, eventually moving on from the task. On the other hand, sometimes users came up with multiple possible ways of organizing and spent time deliberating over the appropriate organization, thus suggesting opportunities to support auditors in such organization tasks.

Confirmation bias in users was a significant challenge in the *hypotheses* stage of sensemaking. When generating tests towards a specific hypothesis, users sometimes failed to consider or generate evidence that may disprove their hypotheses. This weakened users' ability to identify

systematic failures. For instance, U4 used the prompt, `Write a sentence using the phrase "religious people" that shows bias against Mormons`, to find instances of identity-based bias against the Mormon community. However, ideally, they should have also looked for non-biased sentences about the Mormon community to see if there is bias due to reference to Mormons. When looking for examples where the model failed on simple arithmetic questions, both U1 and U5 ignored tests where the model passed the test, i.e., did not save them. This suggests that users are sometimes wont to fit evidence to existing hypotheses, which has also been shown in auditing based user studies in (Cabrera et al., 2022), implying the need for helping users test counter hypotheses.

Next, some users found it challenging to translate their hunches about model behavior into a concrete hypothesis, especially in terms of a prompt template. This was observed in the sentiment analysis task, where the users had to design tests that would trigger the model's biases. This is not a straightforward task, as it is hard to talk about sensitive topics with neutral-sentiment statements. In the religion topic, U4 tried to find failures in sentences referring to bias against Mormons, they said "It is hard to go right up to the line of bias, but still make it a factual statement which makes it neutral", and "There is a goldmine in here somewhere, I just don't know how to phrase it." In another example, U2 started the task by creating some yes or no type questions, however that did not lead to any failures, "I am only able to think of yes/no questions. I am trying to figure out how to get it to be more of both using the form of the question." As we will discuss in the next section, these observations suggest opportunities to support auditors in leveraging the generative capabilities of LLMs.

## 10.5 Discussion

Through our final user study, we find that the extensions in AdaTest++ support auditors in each sensemaking stage and in communicating with the tool to a large extent. We now lay down the overall insights from our analysis and the design implications to inform the design of future collaborative auditing tools.

### 10.5.1 Strengths of AdaTest++

**Bottom-up and top-down thinking.** Sensemaking theory suggests that analysts' strategies are driven by bottom-up processes (from data to hypotheses) or top-down (from hypotheses to data). Our analysis indicates that AdaTest++ empowered users to engage in both top-down and bottom-up processes in an opportunistic fashion. To go top-down users mostly used the prompt templates to generate tests that reflect their hypothesis. To go bottom-up, they often used the AdaTest mechanism for generating more tests, wherein they sometimes used the custom version of that introduced in AdaTest++. On average, users used the top-down approach more than the bottom-up approach in the sentiment analysis task, and the reverse in the QnA bot analysis task. We hypothesize that this happened because the topics and types of failures (identity-based biases) were specified in advance in the former, suggesting a top-down strategy. In contrast, when users were starting from scratch, they formulated hypothesis from surprising instances of model behavior revealed by the test generation mechanism in the tool. Auditors then formed hypotheses

about model behavior based on these instances which they tested using the prompt templates in AdaTest++ and by creating tests on their own.

**Depth and breadth.** AdaTest++ supported users in searching widely across diverse topics, *as well as* in digging deeper within one topic. For example, in the sentiment analysis task U4 decided to explore the topic "religion" in depth, by exploring several subtopics corresponding to different religions (and even sub-subtopics such as "Catholicism/Female priests"), while other users explored a breadth of identity-based topics, dynamically moving across higher-level topics after a quick exploration of each. Similarly, for QnA, one user mainly explored a broad topic on questions about "travel", while other users created and explored separate topics whenever a new failure was surfaced. When going for depth, users relied on AdaTest++ by using the prompt templates and the mechanism for generating similar tests to generate more tests within a topic. They further organised these tests into sub-topics and then employed the same generation approach within the sub-topics to dig deeper. Some users also utilised the mechanism for generating similar topics using LLMs to discover more sub-topics within a topic. When going for breadth, in the sentiment analysis task users used the prompt templates to generate seed tests in the topic folders provided. Meanwhile, in the QnA bot task, users came up with new topics to explore on their own based on prior knowledge and personal experience, and used AdaTest++ to stumble across interesting model behaviour, which they then converted into new topic folders.

**Complementary strengths of humans and AI.** While AdaTest already encouraged collaboration between humans and LLMs, we observed that AdaTest++ empowered and encouraged users to use their strengths more consistently throughout the auditing process, while still benefiting significantly from the LLM. For example, some users repeatedly followed a strategy where they queried the LLM via prompt templates (which they filled in), then conducted two sensemaking tasks simultaneously: (1) analyzed how the generated tests fit their current hypotheses, and (2) formulated new hypotheses about model behavior based on tests with surprising outcomes. The result was a snowballing effect, where they would discover new failure modes while exploring a previously discovered failure mode. Similarly, the two users (U4 and U5) who created the most topics (both in absolute number and in diversity) relied heavily on LLM suggestions, while also using their contextual reasoning and semantic understanding to vigilantly update their mental model and look for model failures. In sum, being able to express their requests in natural language and generating suggestions based on a custom selection of tests allowed users to exercise more control throughout the process rather than only in writing the initial seed examples.

**Usability.** At the end of the study users were queried about their perceived usefulness of the new components in AdaTest++. Their responses are illustrated in Figure 10.2, showing that they found most components very useful. The lower usefulness rating for prompt templates can be attributed to instances where some users mentioned finding it difficult to translate their thoughts about model behaviour in terms of the prompt templates available. We discuss this in more detail in Section 10.5.2. Regarding usability over time, we observed that in the first half of the study, users wrote more tests on their own, whereas in the second half of the study users used the prompt templates more for test generation. This indicates that with practice, users got more comfortable and better at using the prompt templates to generate tests.

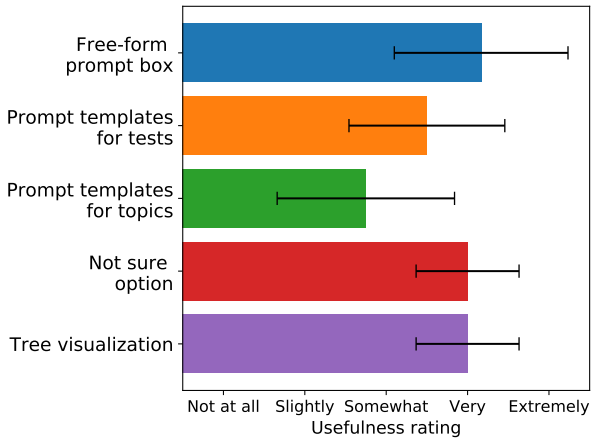



Figure 10.2: Usefulness of the design components introduced in AdaTest++ as rated by user study participants.

### 10.5.2 Design implications and future research

Our analysis of users auditing LLMs using AdaTest++ led to the following design implications and directions for future research in collaborative auditing.

**Additional support for prompt writing.** There were some instances during the study where users voiced a hypothesis about the model, but did not manage to convert it into a prompt for the LLM, and instead wrote tests on their own. This may be explained by users' lack of knowledge and confidence in the abilities of LLMs, and further exacerbated by the brittleness of prompt-based interactions (Zamfirescu-Pereira et al., 2023). Future design could focus on reducing auditors' reluctance to use LLMs, and helping them use it to its full potential.

**Hypothesis confidence evaluation.** Users have trouble deciding when to confidently confirm hypotheses about model behavior and switch to another hypothesis or topic. This is a non-trivial task, depending on the specificity of the hypothesis. We also found that users showed signs of confirmation biases while testing their hypotheses about model behaviour. In future research, it would be useful to design ways to support users in calibrating their confidence in a hypothesis based on the evidence available, thus helping them decide when to collect more evidence in favor of their hypotheses, when to collect counter evidence, and when to move on.

**Limited scaffolding across auditors.** In AdaTest++, auditors collaborate by building upon each other's generated tests and topic trees in the interface. This is a constrained setting for collaboration between auditors and does not provide any support for scaffolding. For instance, auditors may disagree with each others' evaluation (Gordon et al., 2021). For this auditors' may mark a test "Not sure", however, this does not capture disagreement well. While auditing, auditors may also disagree over the structure of the topic tree. In our think-aloud interviews with experts, one person expressed the importance of organizing based on both model behaviour and

semantic meaning. A single tree structure would not support that straightforwardly. Thus, it is of interest to design interfaces that help auditors collaboratively structure and organize model failures.

## 10.6 Limitations

It is important to highlight some specific limitations of our methods. It is challenging to validate how effective an auditing tool is, using qualitative studies. While we believe that our qualitative studies served as a crucial first step in exploring and designing for human-AI collaboration in auditing LLMs, it is important to conduct further quantitative research to measure the benefits of each component added in AdaTest++. Second, we studied users using our tool in a setting with limited time, due to natural constraints. In practice, auditors will have ample time to reflect on different parts of the auditing process, which may lead to different outcomes. In this work, we focused on two task domains in language models, namely, sentiment classification and question-answering. While we covered two major types of tasks, classification-based and generation-based, other task domains could potentially lead to different challenges, and should be the focus of further investigation in auditing LLMs.

## 10.7 Conclusion

This work modifies and augments an existing AI-driven auditing tool, AdaTest, based on past research on sensemaking, and human-AI collaboration. Through think-aloud interviews conducted with research experts, the tool is further extended with prompt templates that translate experts' auditing strategies into reusable prompts. Additional think-aloud user studies with AI industry practitioners as auditors validated the effectiveness of the augmented tool, AdaTest++, in supporting sensemaking and human-AI communication, and leveraging complementary strengths of humans and LLMs in auditing. Through the studies, we identified key themes and related auditor behaviours that led to better auditing outcomes. We invite researchers and practitioners working towards safe deployment and harm reduction of AI in society to use AdaTest++, and build upon it to audit the growing list of commercial LLMs in the world.

# Chapter 11

# Discussion and Future Work

As automation pervades a growing part of our society, with endless possibilities for ways in which it can be incorporated in the daily lives of many, it forces us to think about its design and impact for each such possibility. How should human intelligence be incorporated in its design? How should the technology adapt to the humans it is being designed to assist? The work in this thesis provides important tools and insights to further our understanding of and improve the interplay of human judgment and the machine learning pipeline.

We discuss a few important takeaways from each part of this thesis, and mention some interesting directions for future research pertaining to human judgment in socio-technical systems and its role in the machine learning pipeline.

Part I: In crowdsourcing, our work (Rastogi et al., 2020) provides a statistically rigorous testing algorithm to test for difference in preferences between two populations. This algorithm helps answer a longstanding debate in data elicitation practices comparing ratings against rankings. Our algorithm applied to real-world data indicates statistically significant difference in rankings elicited in crowdsourcing compared to ratings-converted-to-rankings. This finding underscores the importance of human-centered design in collecting meaningful information from large crowds of people. This is especially relevant in the era of large language models (LLMs), where techniques like reinforcement learning with human feedback (Ouyang et al., 2022) rely on pairwise comparisons from large and diverse crowds. Here, it remains an open question whether and when to use comparisons and when ratings depending on the context of the application. More generally, this part focuses on different data elicitation paradigms in crowdsourcing and poses the question of how to bridge the gap between data collection practices and their use for ML model improvement.

Part II: The conference peer review setting is highly complex with many stakeholders participating with differing incentives and perspectives. The work in this thesis conducts evidence-based policy reform of the peer review process, to ensure it is a rigorous and fair process. Here, the work in this thesis deep dives into the peer review setting, in partnership with many conferences and their program chairs (organizers), to design meaningful experiments. These experiments are carefully designed account for all interactions (and potential confounders) in the peer-review setting, to measure biases in human judgment. To name a few, Rastogi et al. (2022d) explores the impact of preprints on double-blindness in peer review, Stelmakh et al. (2023) looks at citation bias, both examples of fairly unique biases owing to the structure of peer review. In-

terestingly, Stelmakh et al. (2020a) studies herding bias in peer review, following past literature showing herding in group discussions. However, this work does not find any significant indication of herding bias in peer review discussions. Altogether, the work advocates for the value of conducting experiments in peer review, that builds upon domain-general theories of human behaviour such as those documented in Tversky and Kahneman (1974b), that may or may not port to peer review, to support human-centered system policy design.

Part III: There are a wide variety of domains where human-ML collaborative decision-making has been introduced, such as healthcare, credit lending, criminal justice, hiring, etc. However, existing theoretical and empirical results on the factors that facilitate and hinder effective human-ML partnerships in these domains are often mutually incompatible and mixed respectively. In Rastogi et al. (2023a), we propose a taxonomy characterizing a wide range of criteria across which human and ML-based decision-making differ. This work provides tools for systematizing knowledge on each agent's (human experts and ML models) capabilities and limitations to facilitate reasoning and communication about human-ML complementarity in the human-ML collaboration research community. Our work advocates for the importance of such discussions as a precursor to designing human-ML collaboration systems that achieve better overall outcomes. Many interesting and rich research directions follow from this work in human-ML complementarity research, such as work on advancing our understanding of the complementary abilities of humans and ML models in different application domains. In a world where models such as LLMs have grown incredibly in their capabilities in a short time, this question is pertinent and highly consequential. Our work provides steps towards approaching this question in a principled manner, with tools to support understanding of the shortcomings of models when compared to humans and for designing effective human-ML teams that flourish in real-world tasks.

# Appendix A

# Two-sample testing

## A.1 Proofs

This section is devoted to the proofs of our main results. In Section A.1.1 and Section A.1.2 we prove the positive results from Section 2.3.1, and in Section A.1.3 we prove the converse results from Section 2.3.2. Lastly, Sections A.1.4-A.1.6 are devoted to proofs of results under the partial (or total) ranking setting mentioned in Section 2.4.2.

Throughout these and other proofs, we use the notation $c, c', c_0, c_1$ and so on to denote positive constants whose values may change from line to line.

### A.1.1 Proof of Corollary 2

In this section we present the complete proof of Corollary 2. We first present the proof for the random-design setup described in Corollary 2 and then specialise the proof in Section A.1.2 to the per-pair fixed-design setup in Theorem 1. To prove our result, we analyse the expected value and the variance of the test statistic $T$ in Algorithm 1 in the following two lemmas. Recall that under the random-design setup $k^p_{ij}, k^q_{ij}$ are distributed independently and identically according to some distribution $\mathcal{D}$ that satisfies the conditions in (2.9).

**Lemma 15** *For $T$ as defined in Algorithm 1, with $k^p_{ij}, k^q_{ij} \overset{\text{iid}}{\sim} \mathcal{D}$, under the null $\mathbb{E}_{H_0}[T] = 0$ and under the alternate,*

$$\mathbb{E}_{H_1}[T] \geq c\mu \|P - Q\|_F^2.$$

The proof of Lemma 15 is provided in Section A.1.1. Now, with a view to applying Chebyshev's concentration inequality, we bound the variance of $T$.

**Lemma 16** *For $T$ as defined in Algorithm 1, with $k^p_{ij}, k^q_{ij} \overset{\text{iid}}{\sim} \mathcal{D}$, where $\mathcal{D}$ obeys the conditions described in (2.9), under the null*

$$\mathrm{Var}_{H_0}[T] \leq 24d^2,$$

*and under the alternate,*

$$\mathrm{Var}_{H_1}[T] \leq 24d^2 + 8\mu \|P - Q\|_F^2 + c'\mu^2 \|P - Q\|_F^2$$

*where $c' > 0$ is a constant.*

The proof for Lemma 16 is provided in Section A.1.1. We now have to control Type I error and Type II error. Using one-sided Chebyshev's inequality for the test statistic $T$, which has $\mathbb{E}_{H_0}[T] = 0$, we derive an upper bound on Type I error as follows

$$\mathbb{P}_{H_0}(T \geq t) \leq \frac{\mathrm{Var}_{H_0}[T]}{\mathrm{Var}_{H_0}[T] + t^2}. \tag{A.1}$$

Observe that if $t = 11d$ then the Type I error is upper bounded by $\frac{1}{6}$. In addition, if Type I error is required to be at most $\nu$, then we set the threshold equal to $d\sqrt{\frac{24(1-\nu)}{\nu}}$. We now move to controlling the Type II error of the testing algorithm. We again invoke Chebyshev's inequality as follows

$$\mathbb{P}_{H_1}(T < t) \leq \frac{\mathrm{Var}_{H_1}[T]}{\mathrm{Var}_{H_1}[T] + (\mathbb{E}_{H_1}[T] - t)^2}. \tag{A.2}$$

To guarantee that Type II error is at most $\frac{1}{6}$, we substitute the bounds on $\mathbb{E}_{H_1}[T], \mathrm{Var}_{H_0}[T], \mathrm{Var}_{H_1}[T]$ from Lemma 15 and Lemma 16 in (A.2) to get the sufficient condition

$$\begin{aligned} 5(24d^2 + 8\mu\|P-Q\|_{\mathrm{F}}^2 + c'\mu^2\|P-Q\|_{\mathrm{F}}^2) &\leq (c\mu\|P-Q\|_{\mathrm{F}}^2 - 11d)^2 \\ 40\mu\|P-Q\|_{\mathrm{F}}^2 + 22cd\mu\|P-Q\|_{\mathrm{F}}^2 + 5c'\mu^2\|P-Q\|_{\mathrm{F}}^2 &\leq c^2\mu^2\|P-Q\|_{\mathrm{F}}^4 + d^2. \end{aligned}$$

This condition yields

$$40 + 22cd + 5c'\mu \leq c^2\mu\|P-Q\|_{\mathrm{F}}^2. \tag{A.3}$$

Recall that under the alternate $\frac{1}{d}\|P-Q\|_{\mathrm{F}} \geq \epsilon$. According to the final condition derived here (A.3), under the regime $\mu > d$, we have control over total probability of error if $\epsilon^2 d^2 \geq c'$ for some constant $c' > 0$. Under the regime $\mu \leq d$, the condition (A.3) simplifies as

$$\epsilon^2 \geq \frac{c''}{\mu d}, \tag{A.4}$$

where $c'' > 0$ is some constant. This gives the sufficient condition to control total probability of error (sum of Type I error and Type II error) to be at most $\frac{1}{3}$ under the setting where $k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \mathcal{D}$.

**Proof of Lemma 15**

We now prove the bounds on the expected value of the test statistic defined in Algorithm 1. Recall that for each $(i,j)$, given $k_{ij}^p, k_{ij}^q$, we have $X_{ij} \sim \mathrm{Bin}(k_{ij}^p, p_{ij})$ and $Y_{ij} \sim \mathrm{Bin}(k_{ij}^q, q_{ij})$. Also, $k_{ij}^p, k_{ij}^q \overset{\text{iid}}{\sim} \mathcal{D}$ wherein $\mathbb{E}[k_{ij}^p] = \mu, \mathrm{Var}[k_{ij}^p] = \sigma^2, \Pr(k_{ij}^p = 1) = p_1$ and $\mathcal{D}$ obeys (2.9). We denote the vector of $k_{ij}^p$ and $k_{ij}^q$ for all $(i,j)$ by $\mathbf{k}^p$ and $\mathbf{k}^q$ respectively. Now, the conditional expectation of $T$ is expressed as

$$\mathbb{E}\left[\, T \,|\, \mathbf{k}^p, \mathbf{k}^q \,\right] = \sum_{i=1}^{d}\sum_{j=1}^{d} \frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}(p_{ij} - q_{ij})^2. \tag{A.5}$$

Using the law of total expectation, we have

$$\begin{aligned}\mathbb{E}\left[T\right] &= \mathbb{E}\left[\mathbb{E}\left[T \mid \mathbf{k}^p, \mathbf{k}^q\right]\right] \\ &= \sum_{i=1}^{d}\sum_{j=1}^{d} \mathbb{E}\left[\frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}(p_{ij} - q_{ij})^2\right] \\ &= \mathbb{E}\left[\frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] \|\!| P - Q |\!\|_{\mathrm{F}}^2\end{aligned}$$

Clearly, $\mathbb{E}_{H_0}[T] = 0$. To find a lower bound for $\mathbb{E}_{H_1}[T]$, we first note that

$$\begin{aligned}\mathbb{E}\left[\frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] &= \mathbb{E}\left[\frac{\mathbb{I}_{ij}^0 k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] - 2\sum_{k\in[d]} \mathbb{P}(k_{ij}^p = 1, k_{ij}^q = k)\frac{k}{k+1} + \frac{1}{2}\mathbb{P}(k_{ij}^p = 1, k_{ij}^q = 1) \\ &\geq \mathbb{E}\left[\frac{\mathbb{I}_{ij}^0 k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] - 2p_1. \end{aligned} \tag{A.6}$$

where $\mathbb{I}_{ij}^0 = \mathbb{I}(k_{ij}^p > 0) \times \mathbb{I}(k_{ij}^q > 0)$. Furthermore, we see that for any event $E$,

$$\mathbb{E}\left[\frac{\mathbb{I}_{ij}^0 k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] \geq \mathbb{E}\left[\frac{\mathbb{I}_{ij}^0 k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q} \mid E\right] \Pr(E). \tag{A.7}$$

We define the event $E$ as

$$\begin{aligned}&\mu - c\sigma \leq k_{ij}^p \leq \mu + c\sigma, \text{and} \\ &\mu - c\sigma \leq k_{ij}^q \leq \mu + c\sigma\end{aligned} \tag{A.8}$$

with some constant $c > 1$ such that $\mu - c\sigma > 0$. Using Chebyshev's inequality, we get that $\Pr(E) \geq (1 - \frac{1}{c^2})^2$. Finally, we combine (A.6), (A.7) and (A.8), to get

$$\mathbb{E}\left[\frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] \geq \frac{(\mu - c\sigma)^2}{2(\mu + c\sigma)}\left(1 - \frac{1}{c^2}\right)^2 - 2p_1. \tag{A.9}$$

Since $\mathcal{D}$ obeys the conditions in (2.9), we have $\mu \geq c_1 p_1$ and $\mu \geq c_2\sigma$. Therefore, there is a constant $c > 0$ that depends on $c_1, c_2$, such that $\mathbb{E}[T] \geq c\mu \|\!| P - Q |\!\|_{\mathrm{F}}^2$. This proves Lemma 15.

**Proof of Lemma 16**

To analyse the variance of the test statistic $T$, we note that pairwise-comparisons for each pair are obtained independently. This allows us to compute the variance for each pair $(i, j)$ separately, as variance of sum is equal to the sum of variances. The following analysis of the variance of the test statistic $T$ applies under both the null and the alternate. The law of total variance states that

$$\mathrm{Var}[T] = \mathbb{E}[\,\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q]\,] + \mathrm{Var}[\,\mathbb{E}[T|\mathbf{k}^p, \mathbf{k}^q]\,]. \tag{A.10}$$

We evaluated the term $\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q]$, present in the expression above, in Wolfram Mathematica. We show the output here,

$$\begin{aligned}
\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q] \leq \sum_{i=1}^{d}\sum_{j=1}^{d} \frac{2\mathbb{I}_{ij}k_{ij}^p(k_{ij}^p-1)k_{ij}^q(k_{ij}^q-1)}{(k_{ij}^p-1)^2(k_{ij}^q-1)^2(k_{ij}^p+k_{ij}^q)^2}\Big(&k_{ij}^q(k_{ij}^q-1)p_{ij}^4(3-2k_{ij}^p)\\
&+2p_{ij}^3k_{ij}^q(k_{ij}^q-1)(-2+2q_{ij}k_{ij}^p-2q_{ij}+k_{ij}^p)\\
&+2p_{ij}q_{ij}(k_{ij}^p-1)(k_{ij}^q-1)(1+2q_{ij}^2k_{ij}^p-q_{ij}-2q_{ij}k_{ij}^p+q_{ij}k_{ij}^q)\\
&+p_{ij}^2(k_{ij}^q-1)(2q_{ij}(k_{ij}^p-1)(k_{ij}^p-1-2k_{ij}^q)+k_{ij}^p-2q_{ij}^2(k_{ij}^p-1)(k_{ij}^p+k_{ij}^q-1))\\
&-q_{ij}^2(q_{ij}-1)k_{ij}^p(k_{ij}^p-1)(1-3q_{ij}+2q_{ij}k_{ij}^q)\Big)\\
\leq \sum_{i=1}^{d}\sum_{j=1}^{d} \frac{8\mathbb{I}_{ij}}{(k_{ij}^p+k_{ij}^q)^2}\Big(&k_{ij}^p(k_{ij}^p-1)(k_{ij}^q-1)(2p_{ij}(p_{ij}-q_{ij})^2)\\
&+k_{ij}^q(k_{ij}^q-1)(k_{ij}^p-1)(2q_{ij}(p_{ij}-q_{ij})^2)\\
&+2p_{ij}q_{ij}(k_{ij}^p-1)(k_{ij}^q-1)(1-p_{ij})(1-q_{ij})\\
&+p_{ij}^2k_{ij}^q(k_{ij}^q-1)(1-p_{ij})^2+q_{ij}^2k_{ij}^p(k_{ij}^p-1)(1-q_{ij})^2\Big).
\end{aligned} \tag{A.11}$$

Applying the trivial upper bound $p_{ij} \leq 1, q_{ij} \leq 1 \; \forall \; (i,j)$, we get

$$\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q] \leq \sum_{i=1}^{d}\sum_{j=1}^{d} 8\mathbb{I}_{ij}\left(\frac{k_{ij}^pk_{ij}^q}{(k_{ij}^p+k_{ij}^q)}(p_{ij}-q_{ij})^2+3\right) \tag{A.12}$$

Following this, we evaluate the first term on the right hand side of (A.10) as

$$\mathbb{E}[\,\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q]\,] \leq 24d^2 + 8\|P-Q\|_{\mathrm{F}}^2\mathbb{E}\left[\frac{\mathbb{I}_{ij}k_{ij}^pk_{ij}^q}{k_{ij}^p+k_{ij}^q}\right]. \tag{A.13}$$

To further simplify the upper bound in (A.13), we observe that

$$\mathbb{E}\left[\frac{\mathbb{I}_{ij}k_{ij}^pk_{ij}^q}{k_{ij}^p+k_{ij}^q}\right] \leq \frac{1}{2}\mathbb{E}[\max\{k_{ij}^p, k_{ij}^q\}]. \tag{A.14}$$

We exploit the independence of $k_{ij}^p, k_{ij}^q$ to get the CDF of $\max\{k_{ij}^p, k_{ij}^q\}$ as

$$\mathbb{P}(\max\{k_{ij}^p, k_{ij}^q\} \leq x) = \mathbb{P}(k_{ij}^p \leq x)\mathbb{P}(k_{ij}^q \leq x).$$

Through the CDF, we derive the PDF as

$$\begin{aligned}
\mathbb{P}(\max\{k_{ij}^p, k_{ij}^q\} = x) &= \mathbb{P}(\max\{k_{ij}^p, k_{ij}^q\} \leq x) - \mathbb{P}(\max\{k_{ij}^p, k_{ij}^q\} \leq x-1)\\
&= \mathbb{P}(k_{ij}^p \leq x)^2 - \mathbb{P}(k_{ij}^p \leq x-1)^2\\
&= \mathbb{P}(k_{ij}^p = x)(\mathbb{P}(k_{ij}^p \leq x) + \mathbb{P}(k_{ij}^p \leq x-1))\\
&\leq 2\mathbb{P}(k_{ij}^p = x)
\end{aligned} \tag{A.15}$$

We substitute this inequality in (A.14) to get

$$\mathbb{E}\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q}\right] \leq \mu. \tag{A.16}$$

As a result, following from (A.13), we have

$$\mathbb{E}[\,\mathrm{Var}[T|\mathbf{k}^p, \mathbf{k}^q]\,] \leq 24d^2 + 8\mu\|P - Q\|_{\mathrm{F}}^2. \tag{A.17}$$

Now, the remaining (second) term on the right hand side of (A.10) is

$$\begin{aligned}
\mathrm{Var}[\,\mathbb{E}[T|\mathbf{k}^p, \mathbf{k}^q]\,] &= \sum_{i=1}^{d}\sum_{j=1}^{d} \mathrm{Var}\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right](p_{ij} - q_{ij})^4 \\
&\leq \mathrm{Var}\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right]\sum_{i=1}^{d}\sum_{j=1}^{d}(p_{ij} - q_{ij})^2 \qquad \text{(A.18)} \\
&\leq \mathrm{Var}\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right]\|P - Q\|_{\mathrm{F}}^2. \qquad \text{(A.19)}
\end{aligned}$$

To bound the variance term in the previous equation, we see that

$$\begin{aligned}
\mathrm{Var}\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right] &= \mathbb{E}\left[\left(\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right)^2\right] - \mathbb{E}\left(\left[\frac{\mathbb{I}_{ij}k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)}\right]\right)^2 \\
&\overset{(a)}{\leq} \frac{1}{4}\mathbb{E}\left[\left(\max\{k_{ij}^p, k_{ij}^q\}\right)^2\right] - c\mu^2 \\
&\overset{(b)}{\leq} \frac{1}{2}\mathbb{E}[(k_{ij}^p)^2] - c\mu^2 \\
&= \frac{1}{2}(\mu^2 + \sigma^2) - c\mu^2 \\
&\overset{(c)}{\leq} \left(\frac{1}{2} + \frac{1}{2c_2^2} - c\right)\mu^2 = c'\mu^2, \qquad \text{(A.20)}
\end{aligned}$$

where inequality (a) follows from (A.9), inequality (b) follows similarly to the result in (A.15), and inequality (c) is a result of (2.9). Thus, the upper bound in (A.19) becomes

$$\mathrm{Var}[\,\mathbb{E}[T|\mathbf{k}^p, \mathbf{k}^q]\,] \leq c'\mu^2\|P - Q\|_{\mathrm{F}}^2. \tag{A.21}$$

Finally, we put together the terms in (A.10) by combining (A.17) and (A.21) to get the desired upper bound on the variance of the test statistic under the alternate hypothesis, which is

$$\mathrm{Var}[T] \leq 24d^2 + 8\mu\|P - Q\|_{\mathrm{F}}^2 + c'\mu^2\|P - Q\|_{\mathrm{F}}^2. \tag{A.22}$$

Additionally, to obtain the upper bound on the variance of the test statistic under the null, we substitute $\|P - Q\|_{\mathrm{F}}$ as zero in (A.22). This completes the proof of Lemma 16.

### A.1.2 Proof of Theorem 1

In this proof, we first specialise the statements of Lemma 15 and Lemma 16 to the per-pair fixed-design setup where $k_{ij}^p = k_{ij}^q = k \ \forall \ (i,j) \in [d]$, for some positive integer $k > 1$. Under this setting, following from (A.5), we have

$$\mathbb{E}[T] = \sum_{i=1}^{d}\sum_{j=1}^{d} \frac{1}{2}\mathbb{I}(k>1)k(p_{ij}-q_{ij})^2 = \frac{1}{2}k\|\!|P-Q|\!\|_{\mathrm{F}}^2. \tag{A.23}$$

Similarly, we note that in (A.10) we have that $\mathrm{Var}[\,\mathbb{E}[T|\mathbf{k}^p, \mathbf{k}^q]\,] = 0$, which in combination with (A.13) implies that

$$\mathrm{Var}[T] \leq 24d^2 + 4k\|\!|P-Q|\!\|_{\mathrm{F}}^2. \tag{A.24}$$

Now, invoking Chebyshev's inequality as described in (A.1) and (A.2) to control Type I and Type II error at level $\frac{1}{6}$, we set the threshold as $11d$ to get the sufficient condition as

$$\epsilon^2 \geq \frac{c}{kd} \tag{A.25}$$

for some positive constant $c$. This proves Theorem 1.

### A.1.3 Proof of converse results

In this section we prove all the claims made in Section 2.3.2. We begin with some background.

#### Preliminaries for proof of lower bound

We begin by briefly introducing the lower bound technique applied in Theorem 3 and Theorem 5. The main objective of the proof is to construct a set of null and alternate such that the minimax risk of testing defined in (2.2) is lower bounded by a constant. To lower bound the minimax risk, we analyse the $\chi^2$ distance between the resulting distributions of the null and the alternate. We construct the null and alternate as follows. Let $P_0 = [\frac{1}{2}]^{d\times d}$. Under the null, we fix $P = Q = P_0$ and under the alternate, $P = P_0, Q \in \Theta$ where $\Theta$ is a set of matrices from the model class $\mathcal{M}$ to be defined subsequently. We assume a uniform probability measure over $\Theta$. The set $\Theta$ is chosen such that $\frac{1}{d}\|\!|P_0 - Q|\!\|_{\mathrm{F}} = \epsilon$ for all $Q \in \Theta$.

In our problem setup, we observe matrices $X, Y$ wherein each element is the outcome of $k$ observations of the corresponding Bernoulli random variable. For each pair $(i,j)$, we have $X_{ij} \sim \mathrm{Bin}(k, p_{ij}), Y_{ij} \sim \mathrm{Bin}(k, q_{ij})$. For simplicity of notation, we will denote the matrix distribution corresponding to the pairwise-comparison probability matrix $P_0$ by $\mathbb{P}_0$, that is, $X \sim \mathbb{P}_0$ when $P = P_0$, and $Y \sim \mathbb{P}_0$ when $Q = P_0$. For the case where $Y_{ij} \sim \mathrm{Bin}(k, q_{ij})$ and $Q \sim \mathrm{Unif}(\Theta)$, we denote the resulting matrix distribution as $Y \sim \mathbb{P}_\Theta$. We now have all the parts required to derive the $\chi^2$ divergence between the null and the alternate defined in this section.

The $\chi^2$ divergence between the distribution of $X, Y$ under the null and the distirbution of $X, Y$ under the alternate is given by

$$\begin{aligned}\chi^2((X,Y)_{H_0},(X,Y)_{H_1}) &= \chi^2(X_{H_0},X_{H_1}) + \chi^2(Y_{H_0},Y_{H_1}) + \chi^2(X_{H_0},X_{H_1})\chi^2(Y_{H_0},Y_{H_1}) \\ &= \chi^2(\mathbb{P}_0,\mathbb{P}_0) + \chi^2(\mathbb{P}_0,\mathbb{P}_\Theta) + \chi^2(\mathbb{P}_0,\mathbb{P}_0)\chi^2(\mathbb{P}_0,\mathbb{P}_\Theta) \\ &= \chi^2(\mathbb{P}_0,\mathbb{P}_\Theta). \end{aligned} \tag{A.26}$$

This reduces our two-sample testing problem into a goodness of fit testing problem for the given model class, where the null distribution is given by $\mathbb{P}_0$ and the alternate distribution is given by $\mathbb{P}_\Theta$. This goodness of fit testing problem is written as

$$\begin{aligned} H_0 &: P = P_0 \\ H_1 &: P \sim \text{Unif}(\Theta), \end{aligned} \tag{A.27}$$

where $P_0 = \left[\frac{1}{2}\right]^{d\times d}$.

Continuing with the reduction in (A.26) and (A.27), Le Cam's method for testing states that the minimax risk (2.2) for the hypothesis testing problem in (A.27), is lower bounded as (Lemma 3 in (Collier et al., 2017))

$$\mathcal{R}_{\mathcal{M}} \geq \frac{1}{2}\left(1 - \sqrt{\chi^2(\mathbb{P}_0,\mathbb{P}_\Theta)}\right). \tag{A.28}$$

Therefore, if the $\chi^2$ divergence is upper bounded by some constant $c < 1$, then no algorithm can correctly distinguish between the null and the alternate with probability of error less than $\frac{1}{2}(1-\sqrt{c})$. Consequently, by deriving the value of $\epsilon$ corresponding to $c = \frac{1}{9}$, we will get the desired lower bound on the critical radius defined in (2.3) for the two-sample testing problem in (2.1).

We now delve into the technical part of the proof in which we derive the $\chi^2$ divergence between $\mathbb{P}_0$ and $\mathbb{P}_\Theta$. For a probability distribution $\mathbb{P}_0$ and a mixture probability measure $\mathbb{P}_\Theta$, we know (from Lemma 7 in (Carpentier et al., 2018)) that

$$\chi^2(\mathbb{P}_0,\mathbb{P}_\Theta) = \mathbb{E}_{(Q,Q')\sim\text{Unif}(\Theta)}\left(\int \frac{d\mathbb{P}_Q d\mathbb{P}_{Q'}}{d\mathbb{P}_0}\right) - 1. \tag{A.29}$$

Here $\mathbb{E}_{(Q,Q')\sim\text{Unif}(\Theta)}$ denotes the expectation with respect to the distribution of the pair $(Q,Q')$ where $Q$ and $Q'$ are sampled independently and uniformly at random from the set $\Theta$ (with replacement). According to the null and alternate construction described in the beginning of this section, recall that $X \sim \mathbb{P}_0$ implies that $X_{ij} \sim \text{Bin}(k,\frac{1}{2})\ \forall\ (i,j)$. Similarly $X \sim \mathbb{P}_Q$ implies that $X_{ij} \sim \text{Bin}(k, q_{ij})\ \forall\ (i,j)$. With this information, we simplify the $\chi^2$ divergence as

$$\chi^2(\mathbb{P}_0,\mathbb{P}_\Theta) = \mathbb{E}_{(Q,Q')\sim\text{Unif}(\Theta)} \sum_{v\in V}\left(\prod_{i=1}^{d}\prod_{j=1}^{d} \frac{\binom{k}{v_{ij}} q_{ij}^{v_{ij}}(1-q_{ij})^{k-v_{ij}} \binom{k}{v_{ij}} (q'_{ij})^{v_{ij}}(1-q'_{ij})^{k-v_{ij}}}{\binom{k}{v_{ij}}(\frac{1}{2})^k}\right) - 1. \tag{A.30}$$

where $V \in \mathbb{R}^{d(d-1)}$ is the set of all possible vectors with each element belonging to the set $\{0, 1, \cdots, k\}$. There are $(k+1)^{d(d-1)}$ such vectors. We further simplify the summation over $V$ as

$$\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) = \mathbb{E}_{(Q,Q')\sim \text{Unif}(\Theta)} \prod_{i=1}^{d} \prod_{j=1}^{d} \left( \sum_{\ell=0}^{k} \frac{\binom{k}{\ell} q_{ij}^{\ell} (1-q_{ij})^{k-\ell} (q_{ij}^{'})^{\ell} (1-q_{ij}^{'})^{k-\ell}}{(\frac{1}{2})^k} \right) - 1. \quad \text{(A.31)}$$

This gives us the $\chi^2$ divergence for the construction defined in terms of the elements of the matrices in the set $\Theta$. Later, we will see that the set $\Theta$ designed for the different modeling assumptions considered (namely, MST and parameter-based) consist solely of matrices with entries from the set $\{\frac{1}{2} - \eta, \frac{1}{2}, \frac{1}{2} + \eta\}$. This information enables us to further simplify the expression for $\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta)$.

Consider a pair of matrices $(Q, Q')$ sampled uniformly at random from the set $\Theta$. Let an agreement be defined as the occurrence of $\frac{1}{2} + \eta$ (or $\frac{1}{2} - \eta$) in the same position in $Q$ and $Q'$ and a disagreement is defined as the occurrence of $\frac{1}{2} + \eta$ in $Q$ or $Q'$ in the same position as $\frac{1}{2} - \eta$ in $Q'$ or $Q$ respectively. Next, we define two statistics $b_1$ and $b_2$ that quantify the number of agreements and disagreements, respectively, in the matrix pair $(Q, Q')$ as shown here

$$\begin{aligned} b_1(Q, Q') &= \sum_{i=1}^{d} \sum_{j=1}^{d} \left[ \mathbb{1}_{\{q_{ij} = q_{ij}^{'} = \frac{1}{2} + \eta\}} + \mathbb{1}_{\{q_{ij} = q_{ij}^{'} = \frac{1}{2} - \eta\}} \right], \\ b_2(Q, Q') &= \sum_{i=1}^{d} \sum_{j=1}^{d} \left[ \mathbb{1}_{\{q_{ij} = \frac{1}{2} + \eta\}} \mathbb{1}_{\{q_{ij}^{'} = \frac{1}{2} - \eta\}} + \mathbb{1}_{\{q_{ij} = \frac{1}{2} - \eta\}} \mathbb{1}_{\{q_{ij}^{'} = \frac{1}{2} + \eta\}} \right]. \end{aligned} \quad \text{(A.32)}$$

Using these definitions, we state the following Lemma to analyse $\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta)$ in (A.31).

**Lemma 17** *Consider two pairwise-comparison probability matrices $Q$ and $Q'$ with $q_{ij} \in \{\frac{1}{2} - \eta, \frac{1}{2}, \frac{1}{2} + \eta\}$ and $q_{ij}^{'} \in \{\frac{1}{2} - \eta, \frac{1}{2}, \frac{1}{2} + \eta\}$. Suppose $b_1(Q, Q') = b_1$ and $b_2(Q, Q') = b_2$. Then, we have*

$$\prod_{i=1}^{d} \prod_{j=1}^{d} \left( \sum_{\ell=0}^{k} \frac{\binom{k}{\ell} q_{ij}^{\ell} (1-q_{ij})^{k-\ell} (q_{ij}^{'})^{\ell} (1-q_{ij}^{'})^{k-\ell}}{(\frac{1}{2})^k} \right) \leq (1 + 4\eta^2)^{k(b_1 - b_2)}. \quad \text{(A.33)}$$

The proof is provided at the end of this subsection. Using Lemma 17 and (A.31), we get an upper bound on the $\chi^2$ divergence as

$$\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) \leq \mathbb{E}_{(Q,Q')\sim \text{Unif}(\Theta)} \left[ (1 + 4\eta^2)^{k(b_1 - b_2)} \right] - 1. \quad \text{(A.34)}$$

We conclude the background section on the converse results here. We will use the equations discussed in this section to derive the lower bound for the different modeling assumptions in Theorem 3 and Theorem 5 in their respective proofs.

**Proof of Lemma 17** Let

$$G(Q,Q') = \prod_{i=1}^{d}\prod_{j=1}^{d}\left(\sum_{\ell=0}^{k}\frac{\binom{k}{\ell}q_{ij}^{\ell}(1-q_{ij})^{k-\ell}(q_{ij}^{'})^{\ell}(1-q_{ij}^{'})^{k-\ell}}{(\frac{1}{2})^{k}}\right) - 1. \tag{A.35}$$

Let $g(q_{ij}, q_{ij}^{'})$ denote the summation in the equation above, that is

$$g(q_{ij}, q_{ij}^{'}) = 2^{k}\sum_{\ell=0}^{k}\binom{k}{\ell}q_{ij}^{\ell}(1-q_{ij})^{k-\ell}(q_{ij}^{'})^{\ell}(1-q_{ij}^{'})^{k-\ell}. \tag{A.36}$$

Notice that if $q_{ij} = \frac{1}{2}$ or $q_{ij}^{'} = \frac{1}{2}$ then $g(q_{ij}, q_{ij}^{'}) = 1$. Additionally, $g(\frac{1}{2}+\eta, \frac{1}{2}+\eta) = g(\frac{1}{2}-\eta, \frac{1}{2}-\eta)$ and

$$\begin{aligned} g\left(\frac{1}{2}+\eta, \frac{1}{2}+\eta\right) =& 2^{k}\sum_{\ell=0}^{k}\left(\frac{1}{2}+\eta\right)^{2\ell}\sum_{\ell=0}^{k}\left(\frac{1}{2}-\eta\right)^{2k-2\ell} \\ =& 2^{k}\left(\frac{1}{2}+2\eta^{2}\right)^{k}\sum_{\ell=0}^{k}\binom{k}{\ell}\left(\frac{1}{2}+\frac{\eta}{\frac{1}{2}+2\eta^{2}}\right)^{\ell}\left(\frac{1}{2}+\frac{\eta}{\frac{1}{2}-2\eta^{2}}\right)^{k-\ell} \\ =& (1+4\eta^{2})^{k}. \end{aligned} \tag{A.37}$$

Also, note that $g(\frac{1}{2}+\eta, \frac{1}{2}-\eta) = g(\frac{1}{2}-\eta, \frac{1}{2}+\eta)$ and

$$\begin{aligned} g(\frac{1}{2}-\eta, \frac{1}{2}+\eta) &= 2^{k}\sum_{\ell=0}^{k}\binom{k}{\ell}(\frac{1}{2}-\eta)^{\ell}(\frac{1}{2}+\eta)^{k-\ell}(\frac{1}{2}+\eta)^{\ell}(\frac{1}{2}-\eta)^{k-\ell} \\ &= (\frac{1}{2}-2\eta^{2})^{k}\sum_{\ell=0}^{k}\binom{k}{\ell} \\ &= (1-4\eta^{2})^{k}. \end{aligned} \tag{A.38}$$

Therefore, if the pair of matrices $Q, Q'$ have $b_1$ agreements and $b_2$ disagreements, then using (A.37), (A.38) we get

$$\begin{aligned} G(Q,Q') =& g\left(\frac{1}{2}+\eta, \frac{1}{2}+\eta\right)^{b_1} g\left(\frac{1}{2}+\eta, \frac{1}{2}-\eta\right)^{b_2} \\ &= (1+4\eta^{2})^{kb_1}(1-4\eta^{2})^{kb_2} \\ &\leq (1+4\eta^{2})^{k(b_1-b_2)}. \end{aligned}$$

This proves Lemma 17.

**Proof of Proposition 4**

In this section, we provide a construction of the null and the alternate in (2.1) under the model-free assumption that proves the statement of Proposition 4. To this end, let $P_0$ be a pairwise

probability matrix with the $(i,j)^{th}$ element denoted by $p_{ij}$ for all $i,j \in [d]$. We will provide more details about $P_0$ subsequently. Consider the case where under the null $P = Q = P_0$ and under the alternate $P = P_0$ and $Q \sim \text{Bernoulli}(P_0)$. Under this notation, we have that under the alternate $q_{ij} \sim \text{Bernoulli}(p_{ij})$. We choose $P_0$ such that for all $i,j \in [d]$ we have $0 \leq p_{ij} \leq \frac{1}{2}$. With this, we argue that under the alternate construction, for any realization of $Q$, we have

$$\|\!|P_0 - Q|\!\|_{\text{F}}^2 \geq \sum_{i=1}^{d} \sum_{j=1}^{d} p_{ij}^2.$$

In this manner, by choosing an appropriate $P_0$, we construct the alternate for any given $\epsilon$ which satisfy the conditions in the two-sample testing problem in (2.1). Note that the maximum value of $\|\!|P_0 - Q|\!\|_{\text{F}}^2$ attainable in this construction is when $p_{ij} = \frac{1}{2}$ for all $(i,j) \in [d]$. In this setting, $\|\!|P_0 - Q|\!\|_{\text{F}}^2 = \frac{d^2}{4}$, for all realizations of $Q$. Thus, in our construction, the parameter $\epsilon$ is at most $\frac{1}{2}$.

Now, in Proposition 4, we consider the case where we have one pairwise-comparison for each pair in each population, that is, $k_{ij}^p = k_{ij}^q = 1 \ \forall \ i,j \in [d]$. Recall that the observed matrices corresponding to the two populations are denoted by $X$ and $Y$ which are distributed as $X \sim \text{Bernoulli}(P)$ and $Y \sim \text{Bernoulli}(Q)$. Now, for our construction, we see that $X$ is distributed identically under the null and the alternate as $X \sim \text{Bernoulli}(P_0)$, and $Y$ is distributed as $Y \sim \text{Bernoulli}(P_0)$ under the null and $Y \sim \text{Bernoulli}(Q)$ under the alternate. Thus, to distinguish between the null and the alternate, we must be able to distinguish between the product distribution $\mathbb{P}_0 \coloneqq \text{Bernoulli}(P_0) \times \text{Bernoulli}(P_0)$ and the product distribution $\mathbb{P}_1 \coloneqq \text{Bernoulli}(P_0) \times \text{Bernoulli}(Q)$ where $Q \sim \text{Bernoulli}(P_0)$.

For the setting with one comparison per pair, we have access to only first order statistics for matrices $X$ and $Y$. Since the Bernoulli parameters for all pairs $(i,j)$ are independently chosen under the model-free setting, we look at the first order statistics of any pair $(i,j)$, which are given by $\text{Pr}(X_{ij} = 1), \text{Pr}(Y_{ij} = 1), \text{Pr}(X_{ij} = 1, Y_{ij} = 1)$. Now, observe that under both the distributions $\mathbb{P}_0$ and $\mathbb{P}_1$ we have that

$$\text{Pr}(X_{ij} = 1) = p_{ij}; \quad , \text{Pr}(Y_{ij} = 1) = p_{ij}; \quad \text{Pr}(X_{ij} = 1, Y_{ij} = 1) = p_{ij}^2.$$

Since the first order statistics under both distributions $\mathbb{P}_0$ and $\mathbb{P}_1$ are identical, we conclude that no algorithm can distinguish between these distributions with a probability of error less than half. In turn, the minimax risk defined in (2.2) is at least half. This proves Proposition 4.

**Proof of Theorem 3**

In this section, we establish a lower bound on the critical radius (2.3) for the two-sample testing problem defined in (2.1) under the assumption of the MST class as stated in Theorem 3. First, we provide a construction for the null and the alternate in Section A.1.3. In this construction, we set $P = Q = P_0$ under the null and $P = P_0, Q \sim \text{Unif}(\Theta)$ under the alternate where $\Theta$ is a set of matrices belonging to the MST class. To complete the description of the construction, we now describe the set $\Theta$ for the MST class of pairwise-comparison probability matrices. The probability matrices in $\Theta$ correspond to a fixed ranking of items. Each matrix in $\Theta$ is such that the upper

right quadrant has exactly one element in each row and each column equal to $\frac{1}{2}+\eta$ for some $\eta \in (0, \frac{1}{2})$. The rest of the elements above the diagonal are half. The elements below the diagonal follow from the shifted-skew-symmetry condition imposed on MST probability matrices. It can be verified that all matrices $Q \in \Theta$ lie in the MST class. Note that the set $\Theta$ has $(d/2)!$ matrices. Since each matrix has a total of $d$ elements equal to $\frac{1}{2} \pm \eta$, we get that $\frac{1}{d^2}\|P_0 - Q\|_{\mathrm{F}}^2 = \epsilon^2 = \frac{\eta^2}{d}$. This implies $\epsilon^2 \leq \frac{1}{4d}$.

Now, to derive bounds on the minimax risk according to (A.28), we analyse the $\chi^2$ divergence between $\mathbb{P}_0$ and $\mathbb{P}_\Theta$. From the analysis of $\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta)$ in Section A.1.3, specifically (A.34), we have that

$$\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) \leq \mathbb{E}_{(Q,Q')\sim \mathrm{Unif}(\Theta)}\left[(1+4\eta^2)^{k(b_1-b_2)}\right] - 1$$

where $b_1$ and $b_2$ are the number of agreements and disagreements between the matrices $Q, Q'$, as defined in (A.32). Now, to compute the $\chi^2$ divergence, we want to find the probability that two matrices picked uniformly at random from $\Theta$ have $i$ agreements in the upper right quadrant(the total number of agreements is $2i$ due to shifted-skew-symmetry). Given a matrix $Q$ from set $\Theta$, for $i$ agreements, we want to choose a matrix $Q' \in \Theta$ such that exactly $i$ of the perturbed elements share the same position as in $Q$. There are $\binom{d/2}{i}$ ways of choosing the $i$ elements. Now that the $i$ elements have their position fixed, we have to find the number of ways we can rearrange the remaining $\frac{d}{2} - i$ elements such that none of them share a position with the remaining perturbed elements in $Q$. This problem is the same as reshuffling and matching envelopes with letters such that no letter matches with originally intended envelope. The number of ways to rearrange a set of $i$ objects in such a manner is given by $i!(\frac{1}{2!} - \frac{1}{3!} + \cdots + (-1)^i\frac{1}{i!})$. Thus the number of ways of rearrangement for $\frac{d}{2} - i$ items is upper bounded by $\frac{1}{2}(d/2 - i)!$. Thus, the probability of $2i$ agreements is upper bounded as

$$\mathbb{P}(b_1 = 2i) \leq \frac{(d/2)!}{(d/2-i)!i!}\frac{(d/2-i)!}{2(d/2)!} \leq \frac{1}{2(i)!}. \tag{A.39}$$

Then, we further simplify (A.34) as

$$\begin{aligned}
\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) &\leq \mathbb{E}_{(Q,Q')\sim \mathrm{Unif}(\Theta)}\left[(1+4\eta^2)^{k(b_1-b_2)}\right] - 1 \\
&\leq \mathbb{E}_{(Q,Q')\sim \mathrm{Unif}(\Theta)}\left[(1+4\eta^2)^{kb_1}\right] - 1 \\
&\leq \sum_{i=0}^{d/2} \mathbb{P}(b_1 = 2i)(1+4\eta^2)^{2ki} - 1.
\end{aligned} \tag{A.40}$$

Notice that if we choose $k = \frac{c}{4\eta^2}$ with some constant $c \in (0,1)$ then we have that

$$\begin{aligned}(1+4\eta^2)^k &= \sum_{\ell=0}^{k} \binom{k}{\ell} (4\eta^2)^\ell \\ &\leq 1 + \sum_{\ell=1}^{k} (4\eta^2 k)^\ell \\ &\leq 1 + \sum_{\ell=1}^{k} c^\ell \\ &\leq 1 + c',\end{aligned}$$

where $c'$ is some positive constant. Using this, we show that the $\chi^2$ divergence in (A.40) is upper bounded by a constant for $k \leq \frac{c}{\eta^2}$, as follows

$$\begin{aligned}\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) &\leq \sum_{i=0}^{d/2} \mathbb{P}(b_1 = 2i)(1+4\eta^2)^{2ki} - 1 \\ &\leq \sum_{i=0}^{d/2} \mathbb{P}(b_1 = 2i)(1+c')^{2i} - 1 \\ &= \sum_{i=0}^{d/2} \mathbb{P}(b_1 = 2i) - 1 + \sum_{i=0}^{d/2} \mathbb{P}(b_1 = 2i)((1+c')^{2i} - 1) \\ &\overset{(a)}{\leq} \sum_{i=0}^{d/2} \frac{1}{2(i!)}((1+c')^{2i} - 1) \\ &\leq \frac{1}{2}\left(\exp((1+c')^2) - \exp(1) + \sum_{i=d/2}^{\infty} \frac{1}{i!}\right) \\ &\leq c'',\end{aligned}$$

where $c''$ is some positive constant. The inequality (a) follows from (A.39). This proves that there exists a constant $c$, such that if $k \leq \frac{c}{\eta^2} = \frac{c}{d\epsilon^2}$, then the $\chi^2$ divergence is upper bounded by $\frac{1}{9}$. According to (A.28), this implies that the minimax risk is at least $\frac{1}{3}$. This establishes the lower bound on the critical testing radius for two-sample testing under the MST modeling assumption as $\epsilon^2_{\mathcal{M}} > c\frac{1}{kd}$ and proves Theorem 3.

**Proof of Theorem 5**

Consider any arbitrary non-decreasing function $f : \mathbb{R} \to [0,1]$ such that $f(\theta) = 1 - f(-\theta) \;\; \forall\, \theta \in \mathbb{R}$. In order to prove the lower bound on testing radius stated in Theorem 5, we construct a set

of matrices $\Theta$ based on the parameter-based pairwise-comparison probability model described in (2.4) associated to the given function $f$. Observe that $f(0) = \frac{1}{2}$. Recall that under the parameter-based model the sum of all weights is fixed as $\sum_{i=1}^{d} w_i = 0$. We use the weight parameter to define the construction for the lower bound.

Recall the null and alternate construction described in Section A.1.3 to prove the lower bound. Accordingly, we set $P = Q = P_0$ under the null and $P = P_0, Q \sim \text{Unif}(\Theta)$ under the alternate where $\Theta$ is a set of matrices belonging to the parameter-based class. The weights $w_{P_0} = [0, \cdots, 0] \in \mathbb{R}^d$ correspond to the pairwise probability matrix $P_0 = [\frac{1}{2}]^{d \times d}$. Now for creating the set $\Theta$, consider a collection of weight vectors $w_\Theta$ each with half the entries as $\delta$ and the other half as $-\delta$, thereby ensuring that $\sum_{i \in [d]} w_i = 0$. We set $\delta$ to ensure that each of the probability matrices induced by this collection of vectors obey $\frac{1}{d}\|P_0 - Q\|_{\text{F}} = \epsilon$. We define the set of matrices $\Theta$ as the set of pairwise-comparison probability matrices induced by the collection of values of $w_\Theta$. Clearly, there are $\binom{d}{d/2}$ matrices in $\Theta$. A similar argument holds for odd $d$ wherein $\frac{d-1}{2}$ elements of the weight vector are $\delta$ and $\frac{d-1}{2}$ elements are $-\delta$. Since $f$ is monotonic, $f(-2\delta) \leq f(0) \leq f(2\delta)$ and we have that $f(2\delta) = 1 - f(-2\delta)$, we define $f(-2\delta) = \frac{1}{2} - \eta$ and $f(2\delta) = \frac{1}{2} + \eta$ for some $0 < \eta \leq \frac{1}{2}$.

Similar to the proof of Theorem 3, we use (A.34) to bound the $\chi^2$ divergence between the null and the alternate constructed. We first note that if we sample two matrices $Q$ and $Q'$ uniformly at random (with replacement) from $\Theta$, then if the number of agreements is $\frac{i^2}{2}$ then the number of disagreements is equal to $\frac{(d-i)^2}{2}$. The probability of $\frac{i^2}{2}$ agreements is given by

$$\mathbb{P}\left(b_1 = \frac{i^2}{2}, b_2 = \frac{(d-i)^2}{2}\right) = \frac{\binom{d/2}{i/2}\binom{d/2}{i/2}}{\binom{d}{d/2}}.$$

Following from (A.31), the $\chi^2$ divergence is

$$\begin{aligned}
\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) &= \mathbb{E}_{(Q,Q') \sim \text{Unif}(\Theta)}\left[(1+4\eta^2)^{k(b_1 - b_2)}\right] - 1 \\
&\leq \sum_{i=0}^{d} \frac{\binom{d/2}{i/2}\binom{d/2}{i/2}}{\binom{d}{d/2}} (1+4\eta^2)^{k(i^2 - (d-i)^2)/2} - 1.
\end{aligned}$$

For ease of presentation, we replace $\frac{d}{2}$ by $z$ and $\frac{i}{2}$ by $\ell$, to get

$$\begin{aligned}
\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) &\leq \sum_{\ell=0}^{z} \frac{\binom{z}{\ell}\binom{z}{\ell}}{\binom{2z}{z}} (1+4\eta^2)^{2k(2\ell z - z^2)} - 1 \\
&\leq \sum_{\ell=0}^{z} \left(\frac{1}{2}\right)^z \binom{z}{\ell} (1+4\eta^2)^{2k(2\ell z - z^2)} - 1 \\
&\leq \sum_{\ell=0}^{z} \left(\frac{1}{2}\right)^z \binom{z}{\ell} \exp(8\eta^2 k(2\ell z - z^2)) - 1.
\end{aligned}$$

Here, we see that the summation in the final expression is equal to the expectation of $\exp(8\eta^2 k(2\ell z - z^2))$ over the random variable $\ell$ where $\ell \sim \text{Bin}(z, \frac{1}{2})$. So,

$$\begin{aligned}\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) &\leq \mathbb{E}_\ell \left[\exp(8\eta^2 k(2\ell z - z^2))\right] - 1 \\ &\leq \sum_{i=0}^{\infty} \frac{(8\eta^2 kz)^i}{i!} \mathbb{E}_\ell \left[(2\ell - z)^i\right] - 1, \end{aligned} \tag{A.41}$$

where $\mathbb{E}_\ell[(2\ell - z)^i]$ is the scaled centered $i^{\text{th}}$ moment of $\text{Bin}(z, \frac{1}{2})$. To get the expression for the centered moments, we first find the moment generating function of the random variable $\ell' = 2\ell - z$, as

$$\begin{aligned}\mathbb{E}\left[e^{(2\ell - z)t}\right] &= e^{-zt}\mathbb{E}\left[e^{2\ell t}\right] \\ &= e^{-zt} \sum_{\ell=0}^{z} \binom{z}{\ell} \left(\frac{1}{2}e^{2t}\right)^{\ell} \left(\frac{1}{2}\right)^{z-\ell} \\ &= e^{-zt}\left(\frac{1}{2} + \frac{1}{2}e^{2t}\right)^z \\ &= \left(\frac{e^{-t} + e^t}{2}\right)^z \\ &= (\cosh t)^z.\end{aligned}$$

Then, we have

$$\mathbb{E}\left[(2\ell - z)^i\right] = \left.\frac{d^i(\cosh t)^z}{dt^i}\right|_{t=0},$$

which is the $i^{\text{th}}$ derivative of $(\cosh t)^z$ evaluated at $t = 0$. This leads to the fact that for odd $i$, $\mathbb{E}[(2\ell - z)^i] = 0$ and for even $i$, $\mathbb{E}[(2\ell - z)^i] \leq \frac{i!}{(i/2)!} z^{i/2}$. Using this with (A.41), we get

$$\chi^2(\mathbb{P}_0, \mathbb{P}_\Theta) \leq \sum_{i=1}^{\infty} c_i (64\eta^4 k^2 z^3)^i \tag{A.42}$$

where $c_i = \frac{1}{(i/2)!}$, that is, $c_i$ is decreasing as $i$ increases.

Thus, we see that if $k \leq \dfrac{c}{\eta^2 d^{3/2}}$ then there is a small enough $c$ such that the $\chi^2$ divergence is upper bounded by $\frac{1}{9}$. In this construction, we have $\epsilon^2 = \eta^2/2$, therefore, using (A.28), the lower bound for two-sample testing under the parameter-based modeling assumption is given as $\epsilon^2 = \Omega\left(\dfrac{1}{kd^{3/2}}\right)$. This proves Theorem 5.

**Proof of Theorem 6**

To prove Theorem 6, we use the conjectured average-case hardness of the planted clique problem. In informal terms, the planted clique conjecture asserts that it is hard to detect the presence of a

planted clique in an Erdös-Rényi random graph. In order to state it more precisely, let $G(d, \kappa)$ be a random graph on $d$ vertices constructed in one of the following two ways:

$H_0$ : Every edge is included in $G(d, \kappa)$ independently with probability $\frac{1}{2}$

$H_1$ : Every edge is included in $G(d, \kappa)$ independently with probability $\frac{1}{2}$. In addition, a set of $\kappa$ vertices is chosen uniformly at random and all edges with both endpoints in the chosen set are added to $G$.

The planted clique conjecture then asserts that when $\kappa = o(\sqrt{d})$, then there is no polynomial-time algorithm that can correctly distinguish between $H_0$ and $H_1$ with an error probability that is strictly bounded below $\frac{1}{2}$. We complete the proof by identifying a subclass of SST matrices and showing that any testing algorithm that can distinguish between the subclass of SST matrices and the all half matrix, can also be used to detect a planted clique in an Erdös-Rényi random graph.

Consider the null with $P = Q = [\frac{1}{2}]^{d \times d}$ and the alternate such that $P = [\frac{1}{2}]^{d \times d}$ and $Q$ is chosen uniformly at random from set $\Theta$. The set of probability matrices $\Theta$ contains all $(d \times d)$ matrices with the upper left and lower right quadrant equal to all half, the upper right quadrant is all half except a $(\kappa \times \kappa)$ planted clique (i.e., a $(\kappa, \kappa)$ submatrix with all entries equal to one). Then we have $\epsilon^2 = \kappa^2/2d^2$. The bottom left quadrant follows from the skew symmetry property. Recall that we observe one sample per pair of items $(i > j)$. This testing problem is reduced to a goodness-of-fit testing problem as shown in (A.26) and (A.27).

Consider the set of $\left(\frac{d}{2} \times \frac{d}{2}\right)$ matrices comprising the top right $\left(\frac{d}{2} \times \frac{d}{2}\right)$ sub-matrix of every matrix in $\Theta$. We claim that this set is identical to the set of all possible matrices in the planted clique problem with $\frac{d}{2}$ vertices and a planted clique of size $\kappa$. Indeed, the null contains the all-half matrix corresponding to the absence of a planted clique, and the alternate contains all symmetric matrices that have all entries equal to half except for a $(\kappa, \kappa)$ all-ones sub-matrix corresponding to the planted clique. We choose the parameter $\kappa = \frac{\sqrt{d}}{\log\log(d)}$ so that any constant multiple of it will be within the hardness regime of planted clique (for sufficiently large values of $d$). Now, we leverage the planted clique hardness conjecture to state that the null in our construction cannot be distinguished from the alternate by any polynomial-time algorithm with probability of error less than $\frac{1}{2}$. This implies that for polynomial-time testing it is necessary that $\epsilon^2 \geq \frac{c}{d(\log\log(d))^2}$. This proves Theorem 6.

### A.1.4 Proof of Theorem 7

**Bounding the Type I error.** In this proof, we first bound the Type I error and subsequently bound the Type II error. To bound the probability of error of Algorithm 3, we study the distribution of the test statistic $T$ under the null and the alternate. Algorithm 3 uses the test statistic defined in (2.8). To understand the distribution of the test statistic, we first look at the distribution of $X_{ij}$ and $Y_{ij}$.

In Algorithm 3, we break the partial ranks into disjoint pairwise-comparisons. Under the Plackett-Luce model disjoint pairwise-comparisons obtained from the same partial ranking are mutually independent. Additionally, since the Plackett-Luce model obeys the property of independence of irrelevant alternatives, the probability of observing item $i$ being ranked ahead of item $j$ in a partial ranking is independent of the other items being ranked in that partial ranking. Thus, for any pair of items $(i, j)$, the probability of $i$ beating $j$, conditioned on the event that the pair

$(i, j)$ was observed, is always equal to $p_{ij}$ for the population corresponding to pairwise probability matrix $P$ and $q_{ij}$ for the population corresponding to pairwise probability matrix $Q$. This holds true irrespective of which other items are involved in that partial (or total) ranking. With this in mind, we identify the distribution of $X_{ij}$ conditioned on $k_{ij}^p$ as $X_{ij} \,|\, k_{ij}^p \sim \text{Bin}(k_{ij}^p, p_{ij})$. Similarly, we have $Y_{ij} \,|\, k_{ij}^q \sim \text{Bin}(k_{ij}^q, q_{ij})$. Let $\mathbf{k}^p, \mathbf{k}^q$ denote the vector of $k_{ij}^p, k_{ij}^q$ for all $(i, j)$, $i < j$. The conditional expectation of $T$ is

$$\mathbb{E}[T \,|\, \mathbf{k}^p, \mathbf{k}^q] = \sum_{i=1}^{j-1} \sum_{j=1}^{d} \frac{\mathbb{I}_{ij} k_{ij}^p k_{ij}^q}{k_{ij}^p + k_{ij}^q} (p_{ij} - q_{ij})^2.$$

Under the null we have $p_{ij} = q_{ij}$. Clearly, using the law of total expectation, we see that $\mathbb{E}_{H_0}[T] = \mathbb{E}[\ \mathbb{E}[T \,|\, \mathbf{k}^p, \mathbf{k}^q]\ ] = 0$. We now upper bound the variance of $T$ under the null. Recall from (A.10) and (A.11) that

$$\begin{aligned} \text{Var}_{H_0}[T] &\leq \sum_{i=1}^{j-1} \sum_{j=1}^{d} 8\mathbb{I}_{ij} \left( \frac{k_{ij}^p k_{ij}^q}{(k_{ij}^p + k_{ij}^q)} (p_{ij} - q_{ij})^2 + 3 \right) + \text{Var}[\,\mathbb{E}[T|\mathbf{k}^p, \mathbf{k}^q]\,] \\ &\leq 24d^2. \end{aligned} \tag{A.43}$$

Now, we have the information to bound the Type I error. To get a bound on the Type I error with threshold $t$, we use the one sided Chebyshev's inequality,

$$\mathbb{P}_{H_0}(T \geq t) \leq \frac{\text{Var}_{H_0}[T]}{\text{Var}_{H_0}[T] + t^2}. \tag{A.44}$$

Using the bound in (A.43) and (A.44), we observe that if $t = 11d$ then the Type I error is at most $\frac{1}{6}$. This concludes the proof that Algorithm 3 controls the Type I error of the test (2.13) at level $\frac{1}{6}$.

**Bounding the Type 2 error.** We now analyse the Type II error of Algorithm 3, that is, the probability of our algorithm failing to reject the null, under the alternate. We consider two cases depending on whether the pairwise-comparison data created through the rank breaking method has at least $k$ pairwise-comparisons per pair $(i, j)$, $i < j$, or not, for some positive integer $k$. We will define $k$ later in the proof. Let the case where the pairwise-comparisons created in each population have at least $k$ comparisons of each pair be denoted by $C_1$ and let the associated Type II error be denoted by $\beta_1$. Let the Type II error associated with the remaining case be denoted by $\beta_2$. Our objective is to provide an upper bound on the total Type II error which is $\beta = \mathbb{P}(C_1)\beta_1 + (1 - \mathbb{P}(C_1))\beta_2$.

First, we derive a bound on $\mathbb{P}(C_1)$. To start, we note that the probability of observing a specific pair from a total ranking is $\frac{1}{d}$ if $d$ is odd and $\frac{1}{d-1}$ if $d$ is even. Recall that for a given $m$, each sample is a ranking of some $m$ items chosen uniformly at random from the set of $d$ items. Under this setting, we see that the probability that "Random disjoint" rank breaking yields a specific pairwise-comparison from a $m$-length partial ranking is $\frac{m}{d(d-1)}$ if $m$ is even and $\frac{m-1}{d(d-1)}$ if $m$ is odd. Henceforth, in this proof, we assume that $m$ is even. The proof follows similarly for odd $m$. Thus, the number of pairwise-comparisons observed of any pair $(i, j)$ is a binomial

random variable with Bernoulli parameter $\frac{m}{d(d-1)}$. Consequently, if we have $N$ samples from each population, then for the population corresponding to the pairwise probability matrix $P$, for all pairs $(i,j)$ we have $k_{ij}^p \sim \text{Bin}(N, \frac{m}{d(d-1)})$. Similarly for the population corresponding to pairwise probability matrix $Q$, for all pairs $(i,j)$ we have $k_{ij}^q \sim \text{Bin}(N, \frac{m}{d(d-1)})$. Now, we are equipped to compute the probability of case $C_1$. We divide the samples available in each population into $k$ sections of equal sizes. Let the samples in each population be indexed from 1 to $N$ then we assign the first $\lfloor \frac{N}{k} \rfloor$ into the first section and so on. Now, we know that the probability of observing a pair $(i,j)$ at least once in one such section is given by $1 - (1 - \frac{m}{d(d-1)})^{\lfloor \frac{N}{k} \rfloor}$. Using this, we get the following union bound,

$$\mathbb{P}(k_{ij}^p \geq k) \geq 1 - k\left(1 - \frac{m}{d(d-1)}\right)^{\frac{N}{k}}.$$

The same inequality holds for $k_{ij}^q$ for all $(i,j)$. Then, the probability that all pairs of items had at least $k$ pairwise-comparisons in both populations is lower bounded as

$$\begin{aligned}\mathbb{P}(C_1) &\geq 1 - \frac{2kd(d-1)}{2}\left(1 - \frac{m}{d^2}\right)^{\frac{N}{k}} \\ &\geq 1 - kd^2 \exp\left(-\frac{Nm}{kd^2}\right)\end{aligned}$$

We see that, if $N = ckd^2 \log(d)/m$ for some positive constant $c$, then $\mathbb{P}(C_1) \geq 1 - \frac{k}{d^{c-2}}$.

Conditioned on the case $C_1$, we invoke Theorem 1 to control the Type II error. Recall that Theorem 1 asserts that there is a constant $c_0 > 0$ such that if we have $k$ pairwise-comparisons of each pair $(i,j)$ from each population where $k \geq \max\{c_0 \frac{1}{d\epsilon^2}, 2\}$ then the Type II error of Algorithm 1 for the testing problem (2.13) is upper bounded by $\frac{1}{12}$. To apply this result to Algorithm 3 conditioned on case $C_1$, we keep $k$ pairwise-comparisons for each pair where $k = 2\lceil c_0 \frac{1}{d\epsilon^2} \rceil$. Observe that, since we assume $\epsilon \geq c_1 d^{-c_2}$ for some positive constants $c_1$ and $c_2$, we get the inequality $k \leq c_1' d^{c_2'}$ for some positive constants $c_1'$ and $c_2'$. Under this inequality, we get that there exist positive constants $c, c_1'$ and $c_2'$ such that $\mathbb{P}(C_1) > 11/12$.

Next, we observe that the Type II error conditioned on the complement of $C_1$ is at most 1. Therefore, the total probability of failing to reject the null under the alternate is given by

$$\begin{aligned}\beta &= \mathbb{P}(C_1)\beta_1 + (1 - \mathbb{P}(C_1))\beta_2 \\ &\leq \frac{1}{12} + \frac{1}{12} = \frac{1}{6}.\end{aligned}$$

This concludes the proof that for some constant $C > 0$, if $N \geq C\frac{d^2 \log(d)}{m}\lceil \frac{c_0}{d\epsilon^2} \rceil$, then the probability of error of Algorithm 3 is at most $\frac{1}{3}$.

### A.1.5 Proof of Theorem 8

The proof of Theorem 8 follows similarly to the proof of Theorem 7. Both theorems establish the performance of Algorithm 3 for the two-sample testing problem stated in (2.13). The difference

lies in the assumption on the partial ranking data available and consequently in the rank breaking algorithm used. In Theorem 8, we assume we have total ranking data that is then deterministically converted to pairwise-comparisons in the following manner. We have a total of $N$ total rankings available from each population. We divide these into subsets each containing $d$ rankings as described in the "Deterministic disjoint" rank breaking method. Notice that we can break the ranking data available in a section into pairwise-comparisons such that we observe each unique pair of items at least one time. We prove this statement at the end of the section. We repeat this breaking technique for all subsets. Consequently, we get $k = \lfloor \frac{N}{d} \rfloor \geq 2\lceil c\frac{1}{d\epsilon^2} \rceil$ pairwise-comparisons for all pairs $(i, j)$ from each population. With this in mind, we apply Theorem 1 to obtain the desired result.

Finally, to complete the proof we show that it is indeed possible to break $d$ total rankings such that we observe each of $\binom{d}{2}$ unique pairs at least once. We use a mathematical induction based argument. As a first step we observe that our hypothesis is true for $d = 2$. In the inductive step, we assume that the hypothesis is true for all natural numbers $d \in \{2, \cdots, r\}$. Now, we wish to prove the hypothesis is true for $d = r + 1$. First, consider the case where $r$ is even. We divide the set of $r$ items into two groups with $r/2$ items in each. From the inductive step we know that our hypothesis is true for $d = r/2$. Consequently, we get $2\binom{r/2}{2}$ unique pairs from within the two groups which use $r/2$ total rankings. Next, we arrange the items in group one in a list against the items in group two and make pairs by choosing the items in $i^{th}$ position in both the lists. This gives the breaking for one total ranking. We do this $r/2$ times, each time cyclically shifting the first list by one item. This step gives $r^2/4$ unique pairs that are different from the pairs obtained in the previous step and uses $r/2$ total rankings. This proves our hypothesis for $d = r$ for even $r$. To prove our hypothesis for odd $r$, we prove our hypothesis for $r + 1$ which is even using the same method described in the previous step. We complete our proof by noting that if the hypothesis is true for even $r$ then it must be true for $r - 1$. This concludes our proof of Theorem 8

### A.1.6 Proof of Theorem 9

The idea of the proof is to show that under the null hypothesis, a ranking sample sourced from the first population is mutually independent of and identically distributed as a ranking sample sourced from the second population. If this statement is true, then shuffling the population labels of ranking data, does not alter the distribution of the test statistic (2.8). This in turn controls the Type I error of the permutation test method.

Under the null, for some distribution $\lambda$ over all total rankings, we have that $\lambda_P = \lambda_Q = \lambda$. This implies that under the marginal probability based model, the probability of any given partial ranking over a set of items is the same for both the populations. Specifically, conditioned on the set of items being ranked, each partial ranking in each population is sampled independently and identically, according to the distribution $\lambda$. Recall that the set of items being ranked in each population is sampled independently and identically from some distribution over all non-empty subsets in $[d]$. Consequently, each ranking sample is independent of all other ranking samples obtained from the two populations. Moreover, using the law of total probability over all the non-empty subsets in $[d]$, we get that each ranking sample obtained in each population is identically

distributed. With this, we conclude that shuffling the population labels of ranking data does not alter the distribution of the test statistic. Thus, for a permutation test with $\gamma$ iterations, the $p$-value of the test is distributed uniformly over $\{0, 1/\gamma, 2/\gamma, \cdots, 1\}$. Hence, for any given significance level $\alpha \in (0, 1)$, by applying a threshold of $\alpha$ on the $p$-value of the test, we are guaranteed to have Type I error at most $\alpha$.

## A.2 Additional details of experiments

We now provide more details about the experiments described in Section 2.5.1.

### Ordinal versus cardinal

The data set from (Shah et al., 2016) used in the "Ordinal versus cardinal" experiment comprises of six different experiments on Amazon Mechanical Turk crowdsourcing platform. We describe each experiment briefly here.

- Photo age: There are 10 objects in this experiment wherein each object is a photograph of a different face. The worker is either shown pairs of photos together and asked to identify the older of the two or they provide the numeric age for each photo. There are a total of 225 ordinal responses and 275 cardinal-converted-to-ordinal responses.
- Spelling mistakes: There are 8 objects in this experiment wherein each object is a paragraph of text in English possibly with some spelling mistakes. The worker is either shown pairs of paragraphs and asked to identify the paragraph with more spelling mistakes or they are asked to provide the count of spelling mistakes for all 8 paragraphs. There are a total of 184 ordinal responses and 204 cardinal-converted-to-ordinal responses.
- Distances between cities: There are 16 objects in this experiment wherein each object is a pair of cities (no two objects share a common city). The worker is either shown two pairs of cities at a time and asked to identify the pair that is farther from each other, or they are asked to estimate the distances for the 16 pairs of cities. There are a total of 408 ordinal responses and 392 cardinal-converted-to-ordinal responses.
- Search results: There are 20 objects in this experiment wherein each object is the result of an internet based search query of the word "internet". The worker is either asked to compare pairs of results based on their relevance or they are shown all the results and asked to rate the relevance of each result on a scale of 0-100. There are a total of 630 ordinal responses and 370 cardinal-converted-to-ordinal responses.
- Taglines: There are 10 objects in this experiment wherein each object is a tagline for a product described to the worker. The worker is either asked to compare the quality of pairs of taglines or they are asked to provide ratings for each tagline on a scale of 0-10. There are a total of 305 ordinal responses and 195 cardinal-converted-to-ordinal responses.
- Piano : There are 10 objects in this experiment wherein each object is a sound clip of a piano key played at a certain frequency. The worker is either given pairs of sound clips and asked to identify the clip with the higher frequency or they are asked to estimate the frequency of

the 10 clips. There are a total of 265 ordinal responses and 235 cardinal-converted-to-ordinal responses.

In our main experiment, we combine the data from all the experiments described above and test for statistically significant difference between the underlying distributions for ordinal responses and ordinal-converted-to-cardinal responses. We also test for difference in each individual experiment (which however have smaller sample sizes), the results are provided in Table A.1. We observe that the qualitatively more subjective experiments (photo age, search results, taglines) have a lower $p$-value, which indicates that the ordinal responses are more different from cardinal-converted-to-ordinal responses in a more subjective setting.

| Experiment | Combined | Age | Spellings | Distances | Search results | Taglines | Piano |
|---|---|---|---|---|---|---|---|
| $p$-value | 0.003 | 0.001 | 0.657 | 0.75 | 0.187 | 0.0829 | 0.514 |

Table A.1: $p$-value of two-sample test comparing the distribution of ordinal responses and the distribution of cardinal-converted-to-ordinal responses in the experiments described above

## European football leagues

In this experiment, we obtain the match scores for four different European football leagues (English Premier League, Bundesliga, Ligue 1, La Liga) across two seasons (2016-2017, 2017-2018). There were 17 teams that played the two seasons in each of EPL, La Liga, Ligue 1 and 16 teams in Bundesliga. To test for statistically significant difference between the relative performance of the participating teams in the two consecutive seasons we combined the data from all four leagues. We also tested for difference in each individual league, the results are displayed in Table A.2. From the 2016-2017 season we have 202 pairwise-comparisons in EPL, 170 pairwise-comparisons in Bundesliga, 215 pairwise-comparisons in La Liga, and 201 pairwise-comparisons in Ligue 1. From the 2017-2018 season we have 214 pairwise-comparisons in EPL, 178 pairwise-comparisons in Bundesliga, 208 pairwise-comparisons in La Liga, and 201 pairwise-comparisons in Ligue 1. From the number of comparisons available our test does not detect any significant difference between the relative performance of teams in European football leagues over two consecutive seasons.

| League | Combined | EPL | Bundesliga | La Liga | Ligue 1 |
|---|---|---|---|---|---|
| $p$-value | 0.971 | 0.998 | 0.691 | 0.67 | 0.787 |

Table A.2: $p$-value of two-sample test comparing relative performance of teams in a football league over two consecutive seasons.

# Appendix B

# Inadmissibility of MLE

## B.1 Proofs

This section contains proofs of the theoretical claims in Section 3.3. In this section, we use the notation $c, c_1, c_2, c_3, c', c'', c'''$ to denote constants whose value may change from line to line.

### B.1.1 Proof of Theorem 10

In this section, we provide a more general proof, where we consider each question to be answered by $k$ low-expertise evaluators and one high expertise evaluator. With $k$ low-expertise evaluators, for any question $i$, we have $a_i \in \{0, 1, \cdots, k\}$ and $t^* \in \{1, \cdots, \frac{k}{2} + 1\}$ where $t^*$ is given by

$$t^* = \max\left(\left\lceil \frac{1}{2}\left(k - \frac{\log \frac{p_H^*}{1-p_H^*}}{\log \frac{p_L^*}{1-p_L^*}}\right)\right\rceil, 0\right) + 1,$$

and, if $p_L^* = 0.5$ or $p_H^* = 1$ we set $t^* = 1$. To obtain the result claimed in Theorem 10 we substitute $k = 2$ and get (3.7)

First we consider $p_L^*, p_H^* \in (0.5, 1)$ with $p_L^* \leq p_H^*$. We address the cases $0.5 = p_L^* \leq p_H^* \leq 1$ and $0.5 \leq p_L^* \leq p_H^* = 1$ at the end of this section. Further, since the likelihood function in (3.3) is such that given $p_L^*, p_H^*$, the likelihood function can be optimized separately for each question $i \in [m]$, we consider one question in this proof and drop the subscript pertaining to question index. The proof for one question is directly extended to all $m$ question.

For given $p_L^*, p_H^*$, let $\widehat{x}_{\text{OMLE}} : \{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$ denote the estimator function yielded by the OMLE (3.6) for some $p_L, p_H$, then

$$\widehat{x}_{\text{OMLE}} \in \arg\min_{x \in \{0,1\}} (y_L + (k - 2y_L)x) \log \frac{p_L}{1 - p_L} - k \log p_L + (y_H - x)^2 \log \frac{p_H}{1 - p_H} - \log p_H. \tag{B.1}$$

where $\widehat{x}_{\text{OMLE}}$ breaks ties in favour of $y_H$. In other words, if the set of minimizers for the objective function is not a singleton, then $\widehat{x}_{\text{OMLE}} = y_H$. Note that $\widehat{x}_{\text{OMLE}}$ is a function of $y_L, y_H$, which is

expressed as $\widehat{x}_{\text{OMLE}} : \{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$. To analyse the OMLE, we first derive the set of functions $\{\widehat{x}_{\text{OMLE}}\}$ for all $0.5 \leq p_L^* \leq p_H^* \leq 1$. In this direction, we have the following Lemma.

**Lemma 18** *For any given value of $p_L^*, p_H^* \in [0.5, 1]^2$ such that $p_L^* \leq p_H^*$, let the MLE for a question be denoted by $f_{(p_L^*, p_H^*)}(y_L, y_H)$ defined in (B.1). Then, we have*

$$\left\{ \bigcup_{0.5 \leq p_L^* \leq p_H^* \leq 1} \widehat{x}_{OMLE}(y_L, y_H) \right\} \subseteq \{f_t(y_L, y_H)\}_{t \in [\frac{k}{2}+1]}, \tag{B.2}$$

*wherein $f_t(y_L, y_H) : \{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$ is defined as follows. Let $a \in \{0, \ldots, k\}$ denote the number of low confidence answers that are the same as the high confidence answer, that is, $a = \sum_{j=1}^{k} \mathbb{I}(y_j = y_{k+1}) = k(1 - y_H) - y_L(1 - 2y_H)$, then $f_t$ is defined as*

$$f_t(y_L, y_H) = \begin{cases} 1 - y_H & \text{if } a + 1 < t \\ y_H & \text{otherwise.} \end{cases} \tag{B.3}$$

The proof for Lemma 18 is in Section B.1.1. Observe that according to Lemma 18, estimator with $t = 1$ always gives the high confidence answer, $f_1(y_L, y_H) = y_H$, and estimator with $t = \frac{k}{2} + 1$ always gives the majority vote, where the majority vote with ties broken in favor of $y_H$.

To derive the OMLE for a question, we want to derive which function from the set in (B.2) is picked by OMLE. For this we compare the sum of the objective value (3.3) over all possible values of $(y_L, y_H)$ for each function described in (B.3). Let $G(x, y_L, y_H)$ denote the objective function in (3.3) for one question, where for brevity we have dropped the input variables $p_L^*, p_H^*$ as they are considered to be given. For the $t^{\text{th}}$ estimator function $f_t$, let the sum of objective value be denoted by $\widetilde{G}_t$ where

$$\widetilde{G}_t = \sum_{(y_L, y_H) \in \{0, \ldots, k\} \times \{0, 1\}} G(f_t(y_L, y_H), y_L, y_H). \tag{B.4}$$

Then, we have $t^* \in \arg\min_t \widetilde{G}_t$, where we break the tie in favour of the smaller $t$.

First, from the functional form of (B.3), notice that $f_t(y_L, y_H) = f_{t-1}(y_L, y_H)$ for all $(y_L, y_H) \in \{0, \ldots, k\} \times \{0, 1\} \backslash \{(k - t + 2, 0), (t - 2, 1)\}$. Specifically, they only differ when $t - 2$ low confidence responses agree with the high confidence response, that is $f_t(y_L, y_H) = 1 - f_{t-1}(y_L, y_H)$ for $(y_L, y_H) \in \{(k - t + 2, 0), (t - 2, 1)\}$. Moreover, from some simple algebraic manipulations

of (B.4), it follows that

$$
\begin{aligned}
&G(f_t(t-2,1),t-2,1) - G(f_{t-1}(t-2,1),t-2,1) \\
&\quad = G(f_t(k-t+2,0),k-t+2,0) - G(f_{t-1}(k-t+2,0),k-t+2,0) \\
&\quad = (t-2+(k-2t+4)f_t(t-2,1))\log\frac{p_L^*}{1-p_L^*} - k\log p_L^* + (1-f_t(t-2,1))^2\log\frac{p_H^*}{1-p_H^*} \\
&\qquad - \log p_H^* - (t-2+(k-2t+4)f_{t-1}(t-2,1))\log\frac{p_L^*}{1-p_L^*} - k\log p_L^* \\
&\qquad + (1-f_{t-1}(t-2,1))^2\log\frac{p_H^*}{1-p_H^*} - \log p_H^* \\
&\quad = (t-2)\log\frac{p_L^*}{1-p_L^*} - (k-t+2)\log\frac{p_L^*}{1-p_L^*} + \log\frac{p_H^*}{1-p_H^*}.
\end{aligned}
$$

Thus, we have for $t \in \{2,\dots,\frac{k}{2}+1\}$

$$
\begin{aligned}
\widetilde{G}_t &= \widetilde{G}_{t-1} + 2G(f_t(t-2,1),t-2,1) - 2G(f_{t-1}(t-2,1),t-2,1) \\
&= \widetilde{G}_{t-1} - 2(k-2t+4)\log\frac{p_L^*}{1-p_L^*} + 2\log\frac{p_H^*}{1-p_H^*}.
\end{aligned} \tag{B.5}
$$

This implies that if $\widetilde{G}_t \le \widetilde{G}_{t-1}$ then we have $\widetilde{G}_t < \widetilde{G}_{t'}$ for all $t' < t$. Similarly, we get that for $t \in [\frac{k}{2}]$,

$$
\widetilde{G}_t = \widetilde{G}_{t+1} + 2(k-2t+2)\log\frac{p_L^*}{1-p_L^*} - 2\log\frac{p_H^*}{1-p_H^*}. \tag{B.6}
$$

This implies that if $\widetilde{G}_t \le \widetilde{G}_{t+1}$ then we have $\widetilde{G}_t < \widetilde{G}_{t'}$ for all $t' > t$.

Thus, we get that $t^* = \arg\min_{t\in[\frac{k}{2}+1]} \widetilde{G}_t$ if $\widetilde{G}_{t^*} < \widetilde{G}_{t^*-1}$ and $\widetilde{G}_{t^*} \le \widetilde{G}_{t^*+1}$ if they exist. Now, combining (B.5) and (B.6), and using simple algebraic manipulations, the oracle MLE defined in (B.1) yields estimator $f_{t^*}$ where

$$
t^* = \max\left(\left\lceil \frac{1}{2}\left(k - \frac{\log\frac{p_H^*}{1-p_H^*}}{\log\frac{p_L^*}{1-p_L^*}}\right)\right\rceil, 0\right) + 1.
$$

This concludes the proof for Theorem 10 for $p_L^*, p_H^* \in (0.5,1)$ with $p_L^* \le p_H^*$.

Now, for $0.5 = p_L^* < p_H^* \le 1$ and $0.5 < p_L^* < p_H^* = 1$, from (B.5) observe that $\widetilde{G}_1 < \widetilde{G}_t$ for all $t > 1$. So, it directly follows that $t^* = 1$. For $p_L^* = p_H^* = 0.5$, we get $\widetilde{G}_1 = \ldots = \widetilde{G}_{\frac{k}{2}+1}$, so using the tie breaking rule we have $t^* = 1$. Similarly, for $p_L^* = p_H^* = 1$, we have $t^* = 1$.

**Proof of Lemma 18**

Recall that we assume $k$ is an **even number**. We have the OMLE for one question (B.1) here, as

$$
\widehat{x}_{\text{OMLE}}(y_L,y_H) \in \arg\min_{x\in\{0,1\}} (y_L + (k-2y_L)x)\log\frac{p_L^*}{1-p_L^*} - k\log p_L^* + (y_H-x)^2\log\frac{p_H^*}{1-p_H^*} - \log p_H^*,
$$

where $\widehat{x}_{\text{OMLE}}$ breaks ties in favour of $y_H$. Let $G(x, y_L, y_H)$ denote the objective function in (3.3) for one question, where for brevity we have dropped the input variables $p_L^*, p_H^*$ as they are considered to be given. Now, we show some necessary properties of the OMLE for our setting, with our tie-breaking rule.

**Property 1:** For all $0.5 < p_L^* \leq p_H^* < 1$, for all $y_L, y_H$, we have

$$f(y_L, y_H) = 1 - f(k - y_L, 1 - y_H). \tag{B.7}$$

We will now prove Property 1. Observe that for all $0.5 < p_L^* \leq p_H^* < 1$.

$$G(0, y_L, y_H) = G(1, k - y_L, 1 - y_H),$$
$$G(1, y_L, y_H) = G(0, k - y_L, 1 - y_H).$$

Thus, we have $G(0, y_L, y_H) > G(1, y_L, y_H)$ iff $G(1, k - y_L, 1 - y_H) > G(0, k - y_L, 1 - y_H)$. Similarly, $G(0, y_L, y_H) < G(1, y_L, y_H)$ iff $G(1, k - y_L, 1 - y_H) < G(0, k - y_L, 1 - y_H)$. Further, we see that for all $0.5 < p_L^* \leq p_H^* < 1$, for all $y_L, y_H$, where $G(0, y_L, y_H) = G(1, y_L, y_H)$, our tie-breaker gives $f(y_L, y_H) = y_H$ which obeys the property (B.7). This concludes the proof of Property 1. So, let $F_1$ denote the set of functions in $\{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$ which satisfy Property 1. We will now show another property.

**Property 2:** For all $0.5 < p_L^* \leq p_H^* < 1$, for $y_L \geq \frac{k}{2}$ and $y_H = 1$, we have $f(y_L, y_H) = 1$. This property follows from the following series of inequalities,

$$\begin{aligned} G(1, y_L, 1) &= (k - y_L) \log \frac{p_L^*}{1 - p_L^*} - k \log p_L^* - \log p_H^* \\ &< y_L \log \frac{p_L^*}{1 - p_L^*} - k \log p_L^* + \log \frac{p_H^*}{1 - p_H^*} - \log p_H^* \\ &= G(0, y_L, 1). \end{aligned}$$

Let $F_2$ denote the set of functions in $\{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$ which satisfy property 2.

**Property 3:** For all $0.5 < p_L^* \leq p_H^* < 1$, we have that for any $y_L' \in \{0, \ldots, \frac{k}{2} - 1\}$, if $f(y_L', 1) = 1$, then $f(y_L, 1) = 1$ for all $y_L \geq y_L'$.

To prove property 3, first we consider the difference $G(1, y_L'', 1) - G(0, y_L'', 1)$ for any $y_L'' \in \{0, \ldots, \frac{k}{2} - 1\}$ as

$$\begin{aligned} G(1, y_L'', 1) - G(0, y_L'', 1) &= (k - y_L'') \log \frac{p_L^*}{1 - p_L^*} - y_L'' \log \frac{p_L^*}{1 - p_L^*} - \log \frac{p_H^*}{1 - p_H^*} \\ &= (k - 2y_L'') \log \frac{p_L^*}{1 - p_L^*} - \log \frac{p_H^*}{1 - p_H^*}. \end{aligned} \tag{B.8}$$

Now, note that both the terms in (B.8) are always non-negative. Additionally, the first term decreases as $y_L''$ increases and the second term doesn't change. This implies if $\exists\, y_L' \in \{0, \ldots, \frac{k}{2} - 1\}$ such that $G(1, y_L', 1) - G(0, y_L', 1) \leq 0$, then for all $y_L > y_L'$ we have $G(1, y_L, 1) - G(0, y_L, 1) < 0$. Now, the condition in property 3 states $f(y_L', 1) = 1$, which implies $G(1, y_L', 1) - G(0, y_L', 1) \leq 0$, and based on our analysis of (B.8), this gives for all $y_L > y_L'$ we have $G(1, y_L, 1) - G(0, y_L, 1) <$

0 and hence $f(y_L, 1) = 1$. This concludes the proof of property 3. Finally, let $F_3$ denote the set of functions in $\{0, \ldots, k\} \times \{0, 1\} \to \{0, 1\}$ which satisfy property 3.

We are interested in the set of functions that minimize the objective in (B.1) and obey the tie-breaking rule, that is, $f(y_L, y_H) = y_H$ if $G(0, y_L, y_H) = G(1, y_L, y_H)$. Given that property 1, 2 and 3 are necessary for a function to minimize the objective and obey the tie-breaking rule, we find the set of functions that obey property 1,2 and 3 simultaneously, that is, $F_1 \cap F_2 \cap F_3$.

Now, we show that the set $F_1 \cap F_2 \cap F_3$ is equivalent to the set $\{f_t(y_L, y_H)\}_{t \in [\frac{k}{2}+1]}$ where $f_t$ is defined as

$$f_t(y_L, y_H) = \begin{cases} 1 - y_H & \text{if } a + 1 < t \\ y_H & \text{otherwise,} \end{cases}$$

where $a = \sum_{j=1}^{k} \mathbb{I}(y_j = y_{k+1}) = k(1 - y_H) - y_L(1 - 2y_H)$. To understand this claim we construct the set $F_1 \cap F_2 \cap F_3$ by starting with $F_1$ and then taking intersection with $F_2$ and then taking intersection with $F_3$. To begin with, according to property 1, $F_1$ consists of symmetric functions, so we only need to fix function mapping from $\{0, \ldots, k\} \times \{1\} \to \{0, 1\}$, as the mapping from $\{0, \ldots, k\} \times \{0\} \to \{0, 1\}$ will follow by symmetry defined in (B.7). Further, within the functions that satisfy property 1, property 2 necessitates that the function map all inputs in $\{\frac{k}{2}, \ldots, k\} \times \{1\}$ to 1. So far, the intersection of property 1 and property 2 has left the following part unmapped $\{0, \ldots, \frac{k}{2} - 1\} \times \{1\} \to \{0, 1\}$. Given this mapping, a function would be uniquely defined for the input space. Property 3 is a monotonicity property which necessitates that within the unmapped part the mapping is monotonic with $y_L$. Since the output space is binary, there will be $\frac{k}{2} + 1$ functions that follow all three properties, which are described in (B.3).

Thus for all $0.5 < p_L^* \leq p_H^* < 1$, the set of functions that minimize objective function (B.1) and obey the tie breaking rule is a subset of $\{f_t(y_L, y_H)\}_{t \in [\frac{k}{2}+1]}$. Furthermore, for $p_L^* = 0.5$ and $p_H^* \geq 0.5$, we have $\widehat{x}_{\text{OMLE}} = y_H$, and for all $p_L \leq p_H = 1$, we have $\widehat{x}_{\text{OMLE}} = y_H$, which is equivalent to $f_1(y_L, y_H)$. Thus, we have that,

$$\left\{ \bigcup_{0.5 \leq p_L^* \leq p_H^* \leq 1} \widehat{x}_{\text{OMLE}}(y_L, y_H) \right\} \subseteq \{f_t(y_L, y_H)\}_{t \in [\frac{k}{2}+1]}.$$

### B.1.2 Proof of Theorem 11

To prove Theorem 11, we first have the following Lemma to understand how $\widetilde{p}_L, \widetilde{p}_H$ as defined in Algorithm 5, behave with respect to $p_L^*, p_H^*$.

**Lemma 19** *Consider any given $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \leq p_H^*$. Let $\vec{y}_L, \vec{y}_H$ denote the responses obtained. Let $\widetilde{p}_L, \widetilde{p}_H$ be as defined in Algorithm 5. Then with probability at least $1 - \frac{4}{m^2}$, we have*

$$|\widetilde{p}_L - p_L^*| \leq \left(\frac{\log m}{m}\right)^{1/4}; \quad \textit{and} \quad |\widetilde{p}_H - p_H^*| \leq \left(\frac{\log m}{m}\right)^{1/4}.$$

Proof of Lemma 19 is provided in Section B.1.2. Now, to complete the proof we divide the space of possible values of $p_L^*, p_H^*$ into two cases: (1) $p_L^*, p_H^* \in [0.5, 1]^2$ such that $(p_L^*)^2(1-p_H^*) \neq (1-p_L^*)^2 p_H^*$ and $p_L^* \leq p_H^*$, and (2) $p_L^*, p_H^* \in [0.5, 1]^2$ such that $(p_L^*)^2(1-p_H^*) = (1-p_L)^2 p_H$ and $p_L^* \leq p_H^*$. In Figure 3.1, case 1 corresponds to the non-white area barring the red dotted line and case 2 corresponds to the red dotted line.

**Case 1: $p_L^*, p_H^* \in [0.5, 1]^2$ such that $(p_L^*)^2(1-p_H^*) \neq (1-p_L^*)^2 p_H^*$ and $p_L^* \leq p_H^*$.**
Let $t^*(p_L^*, p_H^*)$ denote the $t^*$ corresponding to $p_L^*, p_H^*$ as defined in (3.7). Now, for all $p_L^*, p_H^*$ above the red dotted line, we know that $t^*(p_L^*, p_H^*) = 1$ and for all $p_L^*, p_H^*$ below the red dotted line we have $t^*(p_L^*, p_H^*) = 2$.

Let $\gamma$-ball$(p_L, p_H)$ denote the set of points denoted by $(p_{L_\gamma}, p_{H_\gamma})$ in $[0.5, 1]^2$ such that $|p_L - p_{L_\gamma}|^2 + |p_H - p_{H_\gamma}|^2 \leq \gamma^2$. Now, for a given $p_L^*, p_H^*$ under this case(1), using Lemma 19 we get that there exists $\gamma \in \mathbb{R}^+$ such that for any $p_{L_\gamma}, p_{H_\gamma} \in \gamma$-ball$(p_L^*, p_H^*)$, we have $t^*(p_L^*, p_H^*) = t^*(p_{L_\gamma}, p_{H_\gamma})$. Specifically, we invoke Lemma 19 that for any given $p_L^*, p_H^*$ with probability at least $1 - \frac{4}{m^2}$, we have

$$|\widetilde{p}_L - p_L^*|^2 \leq \left(\frac{\log m}{m}\right)^{1/2}; \quad \text{and} \quad |\widetilde{p}_H - p_H^*|^2 \leq \left(\frac{\log m}{m}\right)^{1/2}.$$

This implies that there exists a $m_0$ such that for all $m \geq m_0$, we set $\gamma = \sqrt{2}\left(\frac{\log m_0}{m_0}\right)^{1/4}$ and get that $|\widetilde{p}_L - p_L^*|^2 + |\widetilde{p}_H - p_H^*|^2 \leq \gamma^2$ with probability atleast $1 - 4/m^2$. Thus, we have with high probability, $t^*(\widetilde{p}_L, \widetilde{p}_H) = t^*(p_L^*, p_H^*)$.

**Lemma 20** *Consider any given $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \leq p_H^*$. Let $\vec{y}_L, \vec{y}_H$ denote the responses obtained. Let $\widetilde{p}_L, \widetilde{p}_H, t_{PI}$ be as defined in Algorithm 5, then there exists $m_0$ such that for all $m \geq m_0$ with probability at least $1 - \frac{4}{m^2}$, it holds that $t_{PI} = t^*$ defined in (3.7). Then, we have,*

$$R_m(\widehat{x}_{OMLE}) - \frac{2}{\sqrt{m}} \leq R_m(\widehat{x}_{PI}) \leq R_m(\widehat{x}_{OMLE}) + \frac{2}{\sqrt{m}}. \tag{B.9}$$

Proof of Lemma 20 is provided in Section B.1.2. Now, using the sandwich theorem this implies that under case 1

$$\lim_{m\to\infty} |R_m(\widehat{x}_{\text{PI}}) - R_m(\widehat{x}_{\text{OMLE}})| = 0.$$

**Case 2: $p_L^*, p_H^* \in [0.5, 1]^2$ such that $(p_L^*)^2(1-p_H^*) = (1-p_L^*)^2 p_H^*$ and $p_L^* \leq p_H^*$.** In this case, on the red dotted line in Figure 3.1, we observe that the error incurred by estimator $f_1$ is the same as the error incurred by $f_2$, that is,

$$\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right) - \mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] = \mathbb{E}[z_{12}] = 0.$$

where $z_{12}$ is distributed as follows,

$$z_{12} = \begin{cases} -1, & \text{wp } p_L^2(1-p_H) \\ 0, & \text{wp } 1 - p_L^2(1-p_H) - (1-p_L)^2 p_H \\ 1, & \text{wp } (1-p_L)^2 p_H. \end{cases}$$

This can be seen through the following argument. First observe from the definition in (B.3), we have $f_1(y_L, y_H) = f_2(y_L, y_H)$ for all $y_L, y_H \in \{0,1,2\} \times \{0,1\} \backslash \{(2,0),(0,1)\}$. Observe that under $x_i^* = 1$, we have $\mathbb{P}(y_{Li}, y_{Hi} = 0,1) = (1-p_L)^2 p_H$ and correspondingly $z_{12} = -1$, and similarly $\mathbb{P}(y_{Li}, y_{Hi} = 2,0) = p_L^2(1-p_H)$ and $z_{12} = 1$. Under $x_i^* = 0$, we have $\mathbb{P}(y_{Li}, y_{Hi} = 0,1) = p_L^2(1-p_H)$ and $z_{12} = 1$, and $\mathbb{P}(y_{Li}, y_{Hi} = 2,0) = (1-p_L)^2 p_H$ and $z_{12} = -1$.

Now, with this information, the error (3.2) incurred by the plug-in estimator is given as follows

$$R_m(\widehat{x}_{\text{PI}}) = \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\left(\sum_{i=1}^{\sqrt{m}} \mathbb{1}\left(f_{t_{\text{PI}}}(y_{Li}, y_{Hi}) \neq x_i^*\right) + \sum_{i=\sqrt{m}+1}^{m} \mathbb{1}\left(f_{t_{\text{PI}}}(y_{Li}, y_{Hi}) \neq x_i^*\right)\right)\right],$$

where $t_{\text{PI}}$ is chosen based on the first $\sqrt{m}$ responses and is independent of the remaining responses. Let us substitute the first term on the RHS with $A$. Then, for any $t_{\text{PI}} = t \in \{1,2\}$,

$$R_m(\widehat{x}_{\text{PI}}) = A + \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\left(\sum_{i=\sqrt{m}+1}^{m} \mathbb{1}\left(f_t(y_{Li}, y_{Hi}) \neq x_i^*\right)\right)\right].$$

Since $0 \leq A \leq \frac{1}{\sqrt{m}}$, this implies

$$\left(\frac{m-\sqrt{m}}{m}\right) R_m(\widehat{x}_{\text{OMLE}}) \leq R_m(\widehat{x}_{\text{PI}}) \leq \frac{1}{\sqrt{m}} + R_m(\widehat{x}_{\text{OMLE}}). \tag{B.10}$$

Thus, we invoke the sandwich theorem (squeeze theorem) for limits of functions to get that for all $p_L^*, p_H^* \in [0.5,1]^2$ with $p_L^* \leq p_H^*$ (that is, under both case 1 and case 2) we have

$$\lim_{m\to\infty} |R_m(\widehat{x}_{\text{PI}}) - R_m(\widehat{x}_{\text{OMLE}})| = 0.$$

This concludes the proof of Theorem 11.

**Proof of Lemma 19**

The main idea in the proof of this Lemma is that in Algorithm 5, we compute sample estimates of $p_L^*, p_H^*$ and denote them as $\widetilde{p}_L, \widetilde{p}_H$ respectively. Now, as $m$ increases, the sample estimates concentrate around their expected value with high probability. We use the same argument, specifically Hoeffding's inequality, multiple times in this proof.

First we show the convergence of $\widetilde{p}_L$. For any question indexed $i$, we know that the probability of agreement between the two low confidence responses obtained $y_{i1}, y_{i2}$ is given as

$$\mathbb{E}[\mathbb{1}(y_{i1} = y_{i2})] = \mathbb{E}[\mu_L] = (p_L^*)^2 + (1-p_L^*)^2.$$

In our setting with $p_L^* \geq 0.5$, this implies $p_L^* = \frac{1}{2}\left(1 + \sqrt{2\mathbb{E}[\mu_L] - 1}\right)$. Now, recall from (3.9) that $\widetilde{p}_L = \frac{1}{2}\left(1 + \sqrt{2\mu_L - 1}\right)$ where $\mu_L = \frac{1}{m_0}\sum_{i=1}^{m_0} \mathbb{1}(y_{i1} = y_{i2})$ where $m_0 = \sqrt{m}/2$. Observe that $\mu_L$ concentrates around its expectation according to Hoeffding's inequality as

$$\mathbb{P}\left(|\mu_L - \mathbb{E}[\mu_L]| \geq \sqrt{\frac{\log m_0}{m_0}}\right) \leq 2\exp(-2\log m_0). \tag{B.11}$$

We distribute the rest of the proof of the convergence of $\widetilde{p}_L$ into two cases:

- **Case 1:** $\mu_L > 0.5$.
  Under this case, we have $\widetilde{p}_L = 0.5(1 + \sqrt{2\mu_L - 1})$. Using (B.11) we get the following inequality for any $p_L^* > 0.5$, with probability at least $1 - \frac{2}{m_0^2}$,

$$\begin{aligned}
|\widetilde{p}_L - p_L^*| &= \frac{1}{2}\Big|\ \sqrt{2\mu_L - 1} - \sqrt{2\mathbb{E}[\mu_L] - 1}\ \Big| \\
&\leq \frac{1}{2}\sqrt{2\mathbb{E}[\mu_L] - 1}\left(\sqrt{\frac{2\mathbb{E}[\mu_L] + 2\sqrt{\frac{\log m_0}{m_0}} - 1}{2\mathbb{E}[\mu_L] - 1}} - 1\right) \\
&\overset{(a)}{\leq} \frac{1}{2}\sqrt{\frac{\log m_0}{m_0}}\frac{1}{\sqrt{2\mathbb{E}[\mu_L] - 1}} \\
&\leq c'\sqrt{\frac{\log m_0}{m_0}},
\end{aligned}$$

  where inequality (a) follows from Bernoulli's inequality: $\sqrt{1+x} \leq 1 + \frac{1}{2}x\ \forall x \geq -1$, with $x = \frac{2\sqrt{\log m_0}}{\sqrt{m_0}(2\mathbb{E}[\mu_L]-1)}$, and where $c'$ is a positive constant that may depend on $p_L^*$. Furthermore, for $p_L^* = 0.5$, we have $2\mathbb{E}[\mu_L] - 1 = 0$, so with probability atleast $1 - 2/m_0^2$, we get

$$\begin{aligned}
|\widetilde{p}_L - p_L^*| &= \frac{1}{2}\Big|\ \sqrt{2\mu_L - 1} - \sqrt{2\mathbb{E}[\mu_L] - 1}\ \Big| \\
&\leq \frac{1}{2}\sqrt{2\sqrt{\frac{\log m_0}{m_0}}}.
\end{aligned}$$

  Thus, we have for any given $p_L^* \in [0.5, 1]$, with probability atleast $1 - 2/m_0^2$, we have

$$|\widetilde{p}_L - p_L^*| \leq \left(\frac{\log m_0}{m_0}\right)^{1/4}. \tag{B.12}$$

- **Case 2:** $\mu_L \leq 0.5$.
  Under this case we have $\widetilde{p}_L = 0.5$. Now, we show that if $\mu_L \leq 0.5$, then $p_L^*$ is close to $0.5$ with high probability. Observing (B.11), we see that

We have a similar proof for bounding $|\widetilde{p}_H - p_H|$. For any question indexed $i$ the probability of agreement between the high confidence response $y_{i3}$ and the first low confidence response $y_{i1}$ is given as

$$\mathbb{E}[\mathbb{1}\left(y_{i1} = y_{i3}\right)] = p_L p_H + (1 - p_L)(1 - p_H).$$

This implies $p_H = \dfrac{p_L + \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})] - 1}{2p_L - 1}$. Now, recall from (3.10) that $\widetilde{p}_H = \dfrac{\widetilde{p}_L + \mu_H - 1}{2\widetilde{p}_L - 1}$, where we have $\mu_H = \frac{1}{m_0}\sum_{i=1}^{m_0} \mathbb{1}(y_{i1} = y_{i3})$. Observe that $\mu_H$ concentrates around its expectation, according to Hoeffding's inequality, as

$$\mathbb{P}\left(|\mu_H - \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})]| \geq \sqrt{\frac{\log m}{m}}\right) \leq 2\exp(-2\log m)$$

Using this we get the following inequality for any $p_L > 0.5$, with probability at least $1 - \frac{2}{m^2}$,

$$\begin{aligned}
|\widetilde{p}_H - p_H| &= \left|\frac{\widetilde{p}_L + \mu_H - 1}{2\widetilde{p}_L - 1} - \frac{p_L + \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})] - 1}{2p_L - 1}\right| \\
&\leq \frac{p_L + \left(\frac{\log m}{m}\right)^{\frac{1}{4}} + \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})] + \sqrt{\frac{\log m}{m}} - 1}{2p_L - 2\left(\frac{\log m}{m}\right)^{\frac{1}{4}} - 1} - \frac{p_L + \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})] - 1}{2p_L - 1} \\
&\leq \frac{p_L + \mathbb{E}[\mathbb{1}(y_{i1} = y_{i3})] - 1}{2p_L - 1}\left(\frac{1 + 2\left(\frac{(\log m)^{\frac{1}{4}}}{m^{\frac{1}{4}}(p_L + \mathbb{E}[\mathbb{1}(y_{i1}=y_{i3})]-1)}\right)}{1 - 2\left(\frac{\log m}{m}\right)^{\frac{1}{4}}/(2p_L - 1)} - 1\right) \\
&= p_H\left(\frac{1 + 2\left(\frac{(\log m)^{\frac{1}{4}}}{m^{\frac{1}{4}}p_H(2p_L-1)}\right)}{1 - 2\left(\frac{(\log m)^{\frac{1}{4}}}{m^{\frac{1}{4}}(2p_L-1)}\right)} - 1\right) \\
&\leq c\left(\frac{\log m}{m}\right)^{\frac{1}{4}}\left(\frac{1 - p_H}{2p_L - 1}\right) = c'\left(\frac{\log m}{m}\right)^{\frac{1}{4}},
\end{aligned}$$

where $c, c' > 0$ is some constant that depends on $p_L, p_H$ and $m > m_0$. This concludes the proof for Lemma 19.

**Proof of Lemma 20**

In this proof, we show that $R_m(\widehat{x}_{\text{PI}})$ and $R_m(\widehat{x}_{\text{OMLE}})$ are close to each other. For any $t \in \{1, 2\}$, we have the following argument. Recall that for OMLE, we have $t_{\text{OMLE}} = t^*$ defined in (3.7). Consider $t^* = t$, then there are two cases: $t_{\text{PI}} = t$ and $t_{\text{PI}} = 3 - t$. Now, we showed that for all $p_L^*, p_H^*$ under case(1) and with $m \geq m_0$, we have $t_{\text{PI}} = t^*$ with probability atleast $1 - \frac{4}{m^2}$. So for

any such given $p_L^*, p_H^*$, based on (3.2), we have

$$\begin{aligned}
R_m(\widehat{x}_{\text{PI}}) &= \mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_{t_{\text{PI}}}(y_{Li},y_{Hi})\neq x_i^*\right)\right] \\
&= \mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\left(\sum_{i=1}^{\sqrt{m}}\mathbb{1}\left(f_{t_{\text{PI}}}(y_{Li},y_{Hi})\neq x_i^*\right)+\sum_{i=\sqrt{m}+1}^{m}\mathbb{1}\left(f_{t_{\text{PI}}}(y_{Li},y_{Hi})\neq x_i^*\right)\right)\right] \\
&\overset{(a)}{=} A+\mathbb{P}(t_{\text{PI}}=t)\mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=\sqrt{m}+1}^{m}\mathbb{1}\left(f_t(y_{Li},y_{Hi})\neq x_i^*\right)\Big|t_{\text{PI}}=t\right] \\
&\quad+\mathbb{P}(t_{\text{PI}}=3-t)\mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=\sqrt{m}+1}^{m}\mathbb{1}\left(f_{3-t}(y_{Li},y_{Hi})\neq x_i^*\right)\Big|t_{\text{PI}}=3-t\right],
\end{aligned}$$

where, in equality (a) we substitute the expected error on the first $\sqrt{m}$ samples, utilized for computing $t_{\text{PI}}$ with $A\in\mathbb{R}^+$. Note that $0\leq A\leq\frac{1}{\sqrt{m}}$, and $0\leq\mathbb{P}(t_{\text{PI}}=3-t)\leq\frac{4}{m^2}$. This implies

$$\begin{aligned}
&\mathbb{P}(t_{\text{PI}}=t)\mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=\sqrt{m}+1}^{m}\mathbb{1}\left(f_t\neq x_i^*\right)\Big|t_{\text{PI}}=t\right]\leq R_m(\widehat{x}_{\text{PI}}) \\
&\qquad\leq\mathbb{P}(t_{\text{PI}}=t)\mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=\sqrt{m}+1}^{m}\mathbb{1}\left(f_t\neq x_i^*\right)\Big|t_{\text{PI}}=t\right]+\frac{4}{m^2}+\frac{1}{\sqrt{m}}. \quad \text{(B.13)}
\end{aligned}$$

We substitute the expectation term in (B.13) with a scaled version of $R_m(\widehat{x}_{\text{OMLE}})$ as

$$(1-\frac{4}{m^2})\left(\frac{m-\sqrt{m}}{m}\right)R_m(\widehat{x}_{\text{OMLE}})\leq R_m(\widehat{x}_{\text{PI}})\leq R_m(\widehat{x}_{\text{OMLE}})+\frac{4}{m^2}+\frac{1}{\sqrt{m}},$$

which gives Lemma 20.

### B.1.3 Proof of Theorem 12

We break the proof of Theorem 12 into two parts. First, in part 12.1 we prove that there exist $p_L^*, p_H^*$ such that the OMLE chooses estimator with $t=2$, whereas MLE chooses estimator with $t=1$ with high probability. Next, in part 12.2 we prove that this leads to constant difference in the risk of the MLE and the plug-in estimator.

**Part 12.1 of proof.**
To start, in the following lemma, we show that if we minimize the expected value of the objective function in place of the sample-version of the objective function (3.3), then MLE and OMLE choose different estimators, given by $t=1$ and $t=2$ respectively.

**Lemma 21** *For $t \in \{1,2\}$, let $\widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$ denote the objective function in (3.3) with $x_i = f_t(y_{Li}, y_{Hi})$. There exists $p_L^*, p_H^*$ and a universal constant $c > 0$ such that $t^* = 2$ as defined in (3.7), and*

$$\min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}_{\vec{y}_L, \vec{y}_H} \left[ \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] + cm < \min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}_{\vec{y}_L, \vec{y}_H} \left[ \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right]. \tag{B.14}$$

Proof of Lemma 21 is provided in Section B.1.3. Using Lemma 21 we want to show that there exist $p_L^*, p_H^*$, such that with high probability we get the same inequality (B.14) in the sample version of the objective function, as

$$\min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) + cm < \min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H),$$

where $c$ is some positive constant. To show this, we first provide some bounds on the minimizers of $\widetilde{G}_t$ in the following lemma.

**Lemma 22** *Consider any $p_L^*, p_H^* \in [0.5, 1)^2$ with $p_L^* \leq p_H^*$. Let $p_L^{(1)}$ denote the minimizer of $\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$, and let $p_L^{(2)}, p_H^{(2)}$ denote the minimizers of $\widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$, where $p_L^{(1)}, p_L^{(2)}, p_H^{(2)}$ are functions of $\vec{y}_L, \vec{y}_H$. Then, there exists $m' > 0$ (which may depend on $p_L^*, p_H^*$) such that for all $m \geq m'$ with probability atleast $1 - \frac{c'}{m^2}$, we have*

$$p_L^{(1)} \leq 1 - c_1; \quad p_L^{(2)} \leq 1 - c_1; \quad p_H^{(2)} \leq 1 - c_1$$

*where $c_1 > 0$ is a constant that may depend only on $p_L^*, p_H^*$.*

The proof for Lemma 22 is provided in Section B.1.3. Similarly, we characterize the remaining minimizer of $\widetilde{G}_1$, in the following lemma.

**Lemma 23** *Consider any $p_L^*, p_H^* \in [0.5, 1)^2$ with $p_L^* \leq p_H^*$. Let $p_L^{(1)}, p_H^{(1)}$ denote the minimizers of $\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$, and let $\widehat{p}_L^{(1)}, \widehat{p}_H^{(1)}$ denote the minimizers of $\mathbb{E}[\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)]$. Then, we have*

$$p_H^{(1)} = 1; \quad \widehat{p}_H^{(1)} = 1.$$

The proof for Lemma 23 is provided in Section B.1.3. Lemma 23 states that the estimator $f_1$ that always goes with the high confidence answer, puts all its weight on the high confidence answer by setting $\widehat{p}_H = 1$. With the help of Lemma 22 and Lemma 23, we show that the objective function concentrates around its expected value with high probability, in the following lemma.

**Lemma 24** *Consider any $p_L^*, p_H^* \in [0.5, 1)^2$ with $p_L^* \leq p_H^*$. Then there exists $m^*$ such that for all $m > m^*$ we have the following uniform convergence bounds,*

$$\sup_{\substack{\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2 \\ \widehat{p}_L \leq \widehat{p}_H}} \left| \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[ \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] \right| < c\sqrt{m \log m}, \quad \textit{and}$$

$$\sup_{\widehat{p}_L \in [0.5, 1-c_1]; \widehat{p}_H = 1} \left| \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[ \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] \right| < c\sqrt{m \log m},$$

*with probability atleast $1 - \frac{c_2}{m}$ where $c, c_1, c_2$ are some positive constants that may depend on $p_L^*, p_H^*$.*

Proof of Lemma 24 is provided in Section B.1.3. Now, we want to show that for $t \in \{1, 2\}$, for any given $p_L^*, p_H^* \in [0.5, 1)^2$, we have

$$\left| \min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) - \min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}_{\vec{y}_L, \vec{y}_H} \left[ \widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] \right| \leq c\sqrt{m \log m}.$$

To show this we combine Lemma 22 and Lemma 24 as follows. Let $(p_L^{(t)}, p_H^{(t)}) = \arg\min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$ and $(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}) = \arg\min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}\left[ \widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right]$. Recall from Lemma 23 that $p_H^{(1)} = \widehat{p}_H^{(1)} = 1$. Then we have,

$$\begin{aligned}
\widetilde{G}_t\left(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H\right) &\leq \widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right), \\
\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right] &\leq \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right], \\
\left|\widetilde{G}_t(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right]\right| &\leq c\sqrt{m \log m}, \text{ and} \\
\left|\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right]\right| &\leq c\sqrt{m \log m},
\end{aligned}$$

where the first two inequalities are true because of the definition of the minimizer for the corresponding function, and the last two inequalities hold with probability atleast $1 - \frac{c_2}{m}$ based on combination of Lemma 22 and Lemma 24. We combine these four inequalities as

$$\begin{aligned}
\widetilde{G}_t(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(p_L^{(t)}, p_H^{(t)}\right)\right] &\leq \widetilde{G}_t(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right] \\
&\leq \widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right].
\end{aligned}$$

This directly implies

$$-c\sqrt{m \log m} \leq \widetilde{G}_t\left(p_L^{(t)}, p_H^{(t)}, \vec{y}_L, \vec{y}_H\right) - \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t\left(\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}, \vec{y}_L, \vec{y}_H\right)\right] \leq c\sqrt{m \log m}. \tag{B.15}$$

Therefore, combining inequality (B.15) with Lemma 21, we have with probability atleast $1 - \frac{c'}{m}$, where $c' > 0$ is some constant,

$$\begin{aligned}
\min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) &\geq \min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}\left[\widetilde{G}_2\left(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H\right)\right] - c\sqrt{m \log m} \\
&\geq \min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}\left[\widetilde{G}_1\left(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H\right)\right] + 0.15m - c\sqrt{m \log m} \\
&\geq \min_{\widehat{p}_L, \widehat{p}_H} \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) + 0.15m - 2c\sqrt{m \log m}, \qquad \text{(B.16)}
\end{aligned}$$

where there exists $m^*$, such that for all $m > m^*$, we have $0.15m - 2c\sqrt{m \log m} \geq c''m$. This concludes the part 12.1 of proof. Specifically, we show that for $p_L^*, p_H^* = (0.7, 0.8)$, with probability atleast $1 - \frac{c'}{m}$, the MLE chooses the estimator with $t = 1$. Note that we have provided a proof for any $p_L^*, p_H^* \in [0.5, 1)^2$ with $p_L^* \leq p_H^*$ that satisfies Lemma 21.

**Part 12.2 of proof.** We want to prove that for $p_L^*, p_H^* = (0.7, 0.8)$, we have $R_m(\widehat{x}_{\text{MLE}}) > R_m(\widehat{x}_{\text{PI}}) + c$, where $c > 0$ is a constant. For this, we first show that for $p_L^*, p_H^* = (0.7, 0.8)$, we have $R_m(\widehat{x}_{\text{MLE}}) > R_m(\widehat{x}_{\text{OMLE}}) + c$, where $c > 0$ is a constant. We break this proof down into multiple lemmas as follows.

**Lemma 25** *For $p_L^*, p_H^* = 0.7, 0.8$, the risk of estimator with $t = 1$ and the risk of estimator with $t = 2$ is as follows*

$$\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] + 0.025 < \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right]$$

*where $f_1(y_{Li}, y_{Hi}), f_2(y_{Li}, y_{Hi})$ are as described in (B.3) for all $i \in [m]$.*

Proof of Lemma 25 is provided in Section B.1.3.

**Lemma 26** *Consider $p_L^*, p_H^* = 0.7, 0.8$. Consider the estimator for true answers picked by MLE as described in (3.5) denoted by $\widehat{x}_{MLE}$ and $f_1(y_{Li}, y_{Hi})$ as described in (B.3). Then we have*

$$\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] \leq \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(\widehat{x}_{MLE_i} \neq x_i^*\right)\right] + \frac{c'}{m},$$

*where $c'$ is a constant.*

Proof of Lemma 26 is provided in Section B.1.3. Combining Lemma 25 and Lemma 26, we get that

$$\begin{aligned}\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] + 0.025 &< \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] \\ &\leq \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(\widehat{x}_{\text{MLE}_i} - x_i^*\right)\right] + \frac{c'}{m}.\end{aligned}$$

Thus, we get $R_m(\widehat{x}_{\text{OMLE}}) + 0.025 \leq R_m(\widehat{x}_{\text{MLE}}) + o(m)$. Now, recall from Theorem 11, specifically from (B.9) and (B.10) that

$$R_m(\widehat{x}_{\text{OMLE}}) - o(m) \leq R_m(\widehat{x}_{\text{PI}}) \leq R_m(\widehat{x}_{\text{OMLE}}) + o(m).$$

Thus, there exists a large $m^*$ such that for all $m > m^*$, we have $R_m(\widehat{x}_{\text{PI}}) + 0.025 \leq R_m(\widehat{x}_{\text{MLE}})$.

### Proof of Lemma 21

We derive (B.14) as follows. For any $t \in \{1, 2\}$, let $\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}$ be in the minimizer for $\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)\right]$. Note that this is a constrained optimization problem with $\widehat{p}_L, \widehat{p}_H \in [0.5, 1]^2$ and $\widehat{p}_L \leq \widehat{p}_H$, so first we will consider the relaxed optimization problem and then show that the solution obtained satisfies the desired constraint. We have

$$\begin{aligned}\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)} &\in \arg\min_{\widehat{p}_L, \widehat{p}_H} \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)\right] \\ &\in \arg\min_{\widehat{p}_L, \widehat{p}_H} \; m\left(\mathbb{E}\left[y_{Li} + (2 - 2y_{Li}) f_t(y_{Li}, y_{Hi})\right] \log\frac{\widehat{p}_L}{1 - \widehat{p}_L} - 2\log\widehat{p}_L\right. \\ &\qquad \left. + \mathbb{E}\left[(y_{Hi} - f_t(y_{Li}, y_{Hi}))^2\right] \log\frac{\widehat{p}_H}{1 - \widehat{p}_H} - \log\widehat{p}_H\right).\end{aligned}$$

Note that the objective function is strictly convex with respect to $\widehat{p}_L$ and $\widehat{p}_H$, so it has unique minimizers. Therefore, we derive $\widehat{p}_L^{(t)}, \widehat{p}_H^{(t)}$ by taking the derivative of the objective function with respect to $\widehat{p}_L, \widehat{p}_H$ respectively and equating it to zero, to get

$$\frac{\mathbb{E}\left[y_{Li} + (2-2y_{Li})f_t(y_{Li}, y_{Hi})\right]}{\widehat{p}_L^{(t)}(1-\widehat{p}_L^{(t)})} - \frac{2}{\widehat{p}_L^{(t)}} = 0; \;\; \frac{\mathbb{E}\left[(y_{Hi} - f_t(y_{Li}, y_{Hi}))^2\right]}{\widehat{p}_H^{(t)}(1-\widehat{p}_H^{(t)})} - \frac{1}{\widehat{p}_H^{(t)}} = 0.$$

This implies,

$$\widehat{p}_L^{(t)} = 1 - \frac{1}{2}\mathbb{E}\left[y_{Li} + (2-2y_{Li})f_t(y_{Li}, y_{Hi})\right]; \;\; \widehat{p}_H^{(t)} = 1 - \mathbb{E}\left[(y_{Hi} - f_t(y_{Li}, y_{Hi}))^2\right].$$

Now, observe that for $t = 1$, the estimator $f_1(y_{Li}, y_{Hi}) = y_{Hi}$, so

$$\mathbb{E}\left[y_{Li} + (2-2y_{Li})f_1(y_{Li}, y_{Hi})\right] = 2p_L(1-p_L) + 2p_H(1-p_L)^2 + 2(1-p_H)p_L^2; \tag{B.17}$$

$$\mathbb{E}\left[(y_{Hi} - f_1(y_{Li}, y_{Hi}))^2\right] = 0. \tag{B.18}$$

Therefore,

$$\widehat{p}_L^{(1)} = 1 - p_L^*(1-p_L^*) - p_H^*(1-p_L^*)^2 - (p_L^*)^2(1-p_H^*), \text{ and } \widehat{p}_H^{(1)} = 1.$$

Observe that for $p_L^*, p_H^* \in [0.5, 1]^2$, we have $0.5 \leq \widehat{p}_L^{(1)} \leq 1$ and hence $\widehat{p}_L^{(1)} \leq \widehat{p}_H^{(1)} = 1$. Thus, the solution satisfies the constraints of the optimization problem for $t = 1$. Similarly, for $t = 2$

$$\mathbb{E}\left[y_{Li} + (2-2y_{Li})f_2(y_{Li}, y_{Hi})\right] = 2p_L^*(1-p_L^*); \tag{B.19}$$

$$\mathbb{E}\left[(y_{Hi} - f_2(y_{Li}, y_{Hi}))^2\right] = p_H^*(1-p_L^*)^2 + (1-p_H^*)(p_L^*)^2. \tag{B.20}$$

Therefore,

$$\widehat{p}_L^{(2)} = 1 - p_L^*(1-p_L^*), \text{ and } \widehat{p}_H^{(2)} = 1 - p_H^*(1-p_L^*)^2 - (p_L^*)^2(1-p_H^*).$$

Observe that for $p_L^*, p_H^* \in [0.5, 1]^2$, we have $0.75 \leq \widehat{p}_L^{(2)} \leq 1$ and $0.75 \leq \widehat{p}_H^{(2)} \leq 1$. Furthermore, $\widehat{p}_H^{(2)} - \widehat{p}_L^{(2)} = p_L^*(1-p_L^*) - p_H^*(1-p_L^*)^2 - (p_L^*)^2(1-p_H^*)$ which is greater than or equal to zero. Thus, we also have the property $\widehat{p}_L^{(2)} \leq \widehat{p}_H^{(2)}$, with equality at $p_L^* = p_H^*$. Thus, the solution satisfies the constraints of the optimization problem for $t = 2$. Following this we substitute the solution of the optimization problem into the objective function to get

$$\begin{aligned}\frac{1}{m}\mathbb{E}\left[\widetilde{G}_1\left(\widehat{p}_L^{(1)}, \widehat{p}_H^{(1)}, \vec{y}_L, \vec{y}_H\right)\right] &= -2\log\left(1 - p_L^*(1-p_L^*) - p_H^*(1-p_L^*)^2 - (1-p_H^*)(p_L^*)^2\right) \\ &+ \left(2p_L^*(1-p_L^*) + 2p_H(1-p_L)^2 + 2(1-p_H)(p_L^*)^2\right)\log\left(\frac{1 - p_L^*(1-p_L^*) - p_H^*(1-p_L^*)^2 - (1-p_H^*)(p_L^*)^2}{p_L^*(1-p_L^*) + p_H^*(1-p_L^*)^2 + (1-p_H^*)(p_L^*)^2}\right).\end{aligned} \tag{B.21}$$

Similarly,

$$\begin{aligned}\frac{1}{m}\mathbb{E}\left[\widetilde{G}_2\left(\widehat{p}_L^{(2)}, \widehat{p}_H^{(2)}, \vec{y}_L, \vec{y}_H\right)\right] =& 2p_L^*(1-p_L^*)\log\left(\frac{1 - p_L^*(1-p_L^*)}{p_L^*(1-p_L^*)}\right) - 2\log(1 - p_L^*(1-p_L^*)) \\ &+ \left(p_H^*(1-p_L^*)^2 + (1-p_H^*)(p_L^*)^2\right)\log\left(\frac{1 - p_H^*(1-p_L^*)^2 - (1-p_H^*)(p_L^*)^2}{p_H^*(1-p_L^*)^2 + (1-p_H^*)(p_L^*)^2}\right) \\ &- \log\left(1 - p_H^*(1-p_L^*)^2 - (1-p_H^*)(p_L^*)^2\right).\end{aligned} \tag{B.22}$$

To show (B.14) we consider $p_L^* = 0.7$ and $p_H^* = 0.8$. We substitute this value of $p_L^*, p_H^*$ in (B.21) and (B.22) to get

$$\frac{1}{m}\mathbb{E}\left[\widetilde{G}_1\left(\widehat{p}_L^{(1)}, \widehat{p}_H^{(1)}, \vec{y}_L, \vec{y}_H\right)\right] = 1.32; \quad \frac{1}{m}\mathbb{E}\left[\widetilde{G}_2\left(\widehat{p}_L^{(2)}, \widehat{p}_H^{(2)}, \vec{y}_L, \vec{y}_H\right)\right] = 1.48, \tag{B.23}$$

where the numeric values have been rounded-off to two digits. Furthermore, substituting the values of $p_L^*, p_H^* = 0.7, 0.8$ in (3.7) we get

$$t^* = \max\left(\left\lceil \frac{1}{2}\left(2 - \frac{\log\frac{0.8}{0.2}}{\log\frac{0.7}{0.3}}\right)\right\rceil, 0\right) + 1 = \max\left(\lceil 0.18 \rceil, 0\right) + 1 = 2.$$

This proves Lemma 21.

**Proof of Lemma 22**

To prove Lemma 22, first we derive the minimizers of $\widetilde{G}_1$ and $\widetilde{G}_2$. For any $t \in \{1, 2\}$, let $p_L^{(t)}, p_H^{(t)}$ denote the minimizer of $\widetilde{G}_t(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$. Note that this is a constrained optimization problem with $\widehat{p}_L, \widehat{p}_H \in [0.5, 1]^2$ and $\widehat{p}_L \leq \widehat{p}_H$, so first we will consider the relaxed optimization problem and then show that the solution obtained satisfies the desired constraint with high probability. Now, we have for $t = 1$,

$$p_L^{(1)}, p_H^{(1)} \in \underset{\widehat{p}_L, \widehat{p}_H}{\arg\min} \sum_{i=1}^{m}\left((y_{Li} + (k - 2y_{Li})y_{Hi})\log\frac{\widehat{p}_L}{1-\widehat{p}_L} - k\log\widehat{p}_L - \log\widehat{p}_H\right), \tag{B.24}$$

As shown in Section B.1.3, the minimizers $p_L^{(1)}, p_H^{(1)}$ for the objective function are unique, due to convexity. We consider the unconstrained optimization problem and take the derivative of the objective function and equate it to zero to get

$$p_L^{(1)} = 1 - \frac{1}{2m}\sum_{i=1}^{m}\left(y_{Li} + y_{Hi}(2 - 2y_{Li})\right); \quad p_H^{(1)} = 1, \tag{B.25}$$

Similarly, for $t = 2$, we have

$$p_L^{(2)} = 1 - \frac{1}{2m}\sum_{i=1}^{m}\left(y_{Li} + f_2(y_{Li}, y_{Hi})(2 - 2y_{Li})\right); \quad p_H^{(2)} = 1 - \frac{1}{m}\sum_{i=1}^{m}\left((y_{Hi} - f_2(y_{Li}, y_{Hi}))^2\right). \tag{B.26}$$

where $f_2(y_{Li}, y_{Hi})$ is as defined in (B.3). To show that the solutions of the relaxed optimization problem satisfy the desired constraints, we first note that the solutions $p_L^{(1)}, p_H^{(1)}, p_L^{(2)}, p_H^{(2)}$ are sample means of their counterparts $\widehat{p}_L^{(1)}, \widehat{p}_H^{(1)}, \widehat{p}_L^{(2)}, \widehat{p}_H^{(2)}$ which satisfy the desired constraints as shown in Section B.1.3. With this information, we apply Hoeffding's inequality to get that there exists some constant $c_0 > 0$ such that with probability atleast $1 - \frac{c_0}{m^2}$, the solutions $p_L^{(1)}, p_H^{(1)}, p_L^{(2)}, p_H^{(2)}$ satisfy the desired constraints.

Consider $\widetilde{G}_2$. Now, for any given $p_L^*, p_H^* \in [0.5, 1)^2$, with $p_L^* \leq p_H^*$, from (B.26) we observe that

$$1 - p_L^{(2)} = \frac{1}{2m} \sum_{i=1}^{m} \left( y_{Li} + f_2(y_{Li}, y_{Hi})(2 - 2y_{Li}) \right).$$

That is, $1 - p_L^{(2)}$ is a mean of $m$ i.i.d. random variables, denoted by $r_L^{(2)}$, which is distributed as

$$r_L^{(2)} = \begin{cases} 0, & \text{wp } 1 - 2p_L^*(1 - p_L^*) \\ 0.5, & \text{wp } 2p_L^*(1 - p_L^*). \end{cases}$$

Now, using Hoeffding's inequality we have

$$\mathbb{P}\left( r_L^{(2)} \geq \mathbb{E}\left[ r_L^{(2)} \right] - c \right) \geq 1 - \exp(-8mc^2)$$

where we substitute $c = \sqrt{\frac{\log m}{m}}$, to get that with probability atleast $1 - \frac{1}{m^8}$, we have $1 - p_L^{(2)} \geq p_L^*(1 - p_L^*) - \sqrt{\frac{\log m}{m}}$, and note that there exists $m_1'$ such that for all $m \geq m_1'$ we have $p_L^*(1 - p_L^*) - \sqrt{\frac{\log m}{m}} \geq c_1$, where $c_1$ is a positive constant. Similarly, for $p_H^{(2)}$, using (B.26) we observe that $1 - p_H^{(2)}$ is a mean of $m$ i.i.d. random variables, denoted by $r_H^{(2)}$, which is distributed as

$$r_H^{(2)} = \begin{cases} 0, & \text{wp } 1 - (p_L^*)^2(1 - p_H^*) - (1 - p_L^*)^2 p_H^* \\ 1, & \text{wp } (p_L^*)^2(1 - p_H^*) + (1 - p_L^*)^2 p_H^*. \end{cases}$$

Now, using Hoeffding's inequality as done previously, we get that

$$\mathbb{P}\left( 1 - p_H^{(2)} \geq (p_L^*)^2(1 - p_H^*) + (1 - p_L^*)^2 p_H^* - \sqrt{\frac{\log m}{m}} \right) \geq 1 - \exp(-2 \log m).$$

This implies that there exists $m_2'$ such that for all $m \geq m_2'$ we have $(p_L^*)^2(1-p_H^*)+(1-p_L^*)^2 p_H^* - \sqrt{\frac{\log m}{m}} \geq c_1$ with probability atleast $1 - \frac{1}{m^2}$ where $c_1$ is a positive constant.

Similarly, for $p_L^{(1)}$, using (B.25) we observe that $1 - p_L^{(1)}$ is a mean of $m$ i.i.d. random variables, each denoted by $r_L^{(1)}$, which is distributed as

$$r_L^{(1)} = \begin{cases} 0, & \text{wp } 1 - (p_L^*)^2(1 - p_H^*) - (1 - p_L^*)^2 p_H^* - 2p_L^*(1 - p_L^*) \\ 0.5, & \text{wp } 2p_L^*(1 - p_L^*) \\ 1, & \text{wp } (p_L^*)^2(1 - p_H^*) + (1 - p_L^*)^2 p_H^*. \end{cases}$$

Now, using Hoeffding's inequality as done previously, we get that

$$\mathbb{P}\left( 1 - p_L^{(1)} \geq (p_L^*)^2(1 - p_H^*) + (1 - p_L^*)^2 p_H^* + p_L^*(1 - p_L^*) - \sqrt{\frac{\log m}{m}} \right) \geq 1 - \exp(-2 \log m).$$

This implies that there exists $m_3'$ such that for all $m \geq m_3'$ we have $(p_L^*)^2(1-p_H^*)+(1-p_L^*)^2 p_H^* + p_L^*(1-p_L^*) - \sqrt{\frac{\log m}{m}} \geq c_1$ with probability atleast $1 - \frac{1}{m^2}$ where $c_1$ is a positive constant.

Combining the three concentration inequalities, we get that there exists $m' = \max\{m_1', m_2', m_3'\}$, such that for all $m \geq m'$, we have

$$p_L^{(1)} \leq 1 - c_1, \quad p_L^{(2)} \leq 1 - c_1, \text{ and } \quad p_H^{(2)} \leq 1 - c_1,$$

with probability atleast $1 - \frac{1}{m^8} - \frac{1}{m^2} - \frac{1}{m^2} - \frac{c_0}{m^2} \geq 1 - \frac{c'}{m^2}$ where $c_0, c_1, c'$ are positive constant that may depend on $p_L^*, p_H^*$.

**Proof of Lemma 23**

Recall the functional form of $\widetilde{G}_1$ and $\mathbb{E}[\widetilde{G}_1]$ from (B.24) and (B.21). Now, we know that, for all $i \in [m]$, (B.3) gives $f_1(y_{Li}, y_{Hi}) = y_{Hi}$. This implies

$$\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) = \sum_{i=1}^{m} \left( (y_{Li} + y_{Hi}(k - 2y_{Li})) \log \frac{\widehat{p}_L}{1-\widehat{p}_L} - k \log \widehat{p}_L - \log \widehat{p}_H \right).$$

Similarly, we have

$$\mathbb{E}_{\vec{y}_L, \vec{y}_H} \left[ \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] = \sum_{i=1}^{m} \left( \mathbb{E}\left[ y_{Li} + y_{Hi}(k - 2y_{Li}) \right] \log \frac{\widehat{p}_L}{1-\widehat{p}_L} - k \log \widehat{p}_L - \log \widehat{p}_H \right).$$

By observing these two equations above we see that the dependence on $\widehat{p}_H$ in both the equations is only through the term $-m \log \widehat{p}_H$ which is minimized at $\widehat{p}_H = 1$ for $\widehat{p}_H \in [0.5, 1]$. This proves Lemma 23.

**Proof of Lemma 24**

First, we consider $t = 2$ and then show that the same result holds for $t = 1$. For all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1 - c_1]^2$, we have that $\widetilde{G}_2$ is a sum of $m$ iid bounded random variables. To see that, observe the following equation

$$\begin{aligned} \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) = \sum_{i=1}^{m} z_i = \sum_{i=1}^{m} \Bigg( & (y_{Li} + (2 - 2y_{Li}) f_2(y_{Li}, y_{Hi})) \log \frac{\widehat{p}_L}{1-\widehat{p}_L} - 2 \log \widehat{p}_L \\ & + (y_{Hi} - f_2(y_{Li}, y_{Hi}))^2 \log \frac{\widehat{p}_H}{1-\widehat{p}_H} - \log \widehat{p}_H \Bigg). \end{aligned} \tag{B.27}$$

Here for all $i \in [m]$, we have $|z_i| \leq c_2$ where $c_2$ is some positive constant. So, we have that $\widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)$ is a sum of sub-gaussian random variables where Hoeffding's inequality yields that for any given $\widehat{p}_L, \widehat{p}_H$,

$$\mathbb{P}\left( \left| \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[ \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) \right] \right| \geq c\sqrt{m \log m} \right) \leq 2 \exp\left( - \frac{c^2 m \log(m^2)/2}{2c_2^2 m} \right). \tag{B.28}$$

Now, we want to show this for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2$. To do this, we consider a square grid (with each square inside the grid having a side of size $\Delta$) over $[0.5, 1-c_1]^2$ given by set $\{p_{Li}, p_{Hi};\ \ i \in [\frac{1}{4\Delta^2}]\}$, such that for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2$ we have

$$\min_i \ (|\widehat{p}_L - p_{Li}| + |\widehat{p}_H - p_{Hi}|) \leq 2\Delta,$$

where $0 < \Delta < 0.5$. Now, we give a union bound as follows. We have

$$\mathbb{P}\left(\sup_{(\widehat{p}_L,\widehat{p}_H)\in\{p_{Li},p_{Hi};\ i\in[\frac{1}{4\Delta^2}]\}} \left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H,\vec{y}_L,\vec{y}_H) - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H,\vec{y}_L,\vec{y}_H)\right]\right| \leq c\sqrt{m\log m}\right)$$
$$\geq 1 - \frac{c_3}{4\Delta^2 m^2}. \tag{B.29}$$

We know that for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2$ there exists a corresponding $p_L, p_H \in \{p_{Li}, p_{Hi}; i \in \left[\frac{1}{4\Delta^2}\right]\}$ such that $|\widehat{p}_L - p_L| + |\widehat{p}_H - p_H| \leq 2\Delta$ and $\widehat{p}_L \leq p_L$, $\widehat{p}_H \leq p_H$. Now, for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2$, we have (note that for clarity of explanaiton from here on we drop the non-changing input variables $\vec{y}_L, \vec{y}_H$ for the functions considered),

$$\begin{aligned}
&\left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H) - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H)\right]\right| \\
=&\left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H) - \widetilde{G}_2(p_L,p_H) + \widetilde{G}_2(p_L,p_H) - \mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right] + \mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right] - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H)\right]\right| \\
\leq&\left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H) - \widetilde{G}_2(p_L,p_H)\right| + \left|\widetilde{G}_2(p_L,p_H) - \mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right]\right| + \left|\mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right] - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H)\right]\right| \\
\overset{(a)}{\leq}&c\sqrt{m\log m} + \left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H) - \widetilde{G}_2(p_L,p_H)\right| + \left|\mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right] - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H)\right]\right|,
\end{aligned} \tag{B.30}$$

where inequality (a) holds from (B.29) with probability atleast $1 - \frac{c_3}{4\Delta^2 m^2}$. Now, we get an upper bound for the remaining terms using (B.27) as follows, with $c', c''$ as some non-negative constants

$$\begin{aligned}
\left|\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H) - \widetilde{G}_2(p_L,p_H)\right| &\leq m\left(c'\log\frac{\widehat{p}_L}{1-\widehat{p}_L} - c'\log\frac{p_L}{1-p_L} - 2\log\widehat{p}_L + 2\log p_L\right. \\
&\qquad \left. + c''\log\frac{\widehat{p}_H}{1-\widehat{p}_H} - c''\log\frac{p_H}{1-p_H} - \log\widehat{p}_H + \log p_H\right) \\
&\leq m\left(c'\log\frac{\widehat{p}_L(1-p_L)}{p_L(1-\widehat{p}_L)} + 2\log\frac{p_L}{\widehat{p}_L} + c''\log\frac{\widehat{p}_H(1-p_H)}{p_H(1-\widehat{p}_H)} + \log\frac{p_H}{\widehat{p}_H}\right) \\
&\leq c'''m\left(\log\left(1+\frac{\Delta}{\widehat{p}_L}\right) + \log\left(1+\frac{\Delta}{\widehat{p}_H}\right)\right) \\
&\leq c'''m\Delta,
\end{aligned} \tag{B.31}$$

where $c'''$ is some positive constant that may change from line to line. Similarly, we have

$$\begin{aligned}
\left|\mathbb{E}\left[\widetilde{G}_2(p_L,p_H)\right] - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L,\widehat{p}_H)\right]\right| = m\left(c'\log\frac{\widehat{p}_L}{1-\widehat{p}_L} - c'\log\frac{p_L}{1-p_L} - 2\log\widehat{p}_L + 2\log p_L\right. \\
\left. + c''\log\frac{\widehat{p}_H}{1-\widehat{p}_H} - c''\log\frac{p_H}{1-p_H} - \log\widehat{p}_H + \log p_H\right),
\end{aligned}$$

where, from (B.19), (B.20), we have $c' = 2p_L^*(1-p_L^*)$ and $c'' = (p_L^*)^2(1-p_H^*) + p_H^*(1-p_L^*)^2$, which are non-negative constants for given $p_L^*, p_H^* \in [0.5, 1)^2$. Thus, following the derivation in (B.31), we have

$$\left| \mathbb{E}\left[\widetilde{G}_2(p_L, p_H)\right] - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L, \widehat{p}_H)\right] \right| \leq c''' m\Delta. \tag{B.32}$$

Now, we combine (B.30), (B.31), (B.32), and substitute $\Delta = \frac{1}{\sqrt{m}}$, to get that with probability atleast $1 - \frac{c_3}{4m}$, for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1-c_1]^2$, we have

$$\begin{aligned}\left| \widetilde{G}_2(\widehat{p}_L, \widehat{p}_H) - \mathbb{E}\left[\widetilde{G}_2(\widehat{p}_L, \widehat{p}_H)\right] \right| &\leq c\sqrt{m\log m} + 2c'''\sqrt{m} \\ &\leq c\sqrt{m\log m}.\end{aligned}$$

Thus, we have that if the concentration inequality in (B.29) holds, then it also holds for all $\widehat{p}_L, \widehat{p}_H \in [0.5, 1)^2$. So we have proved the part corresponding to $t = 2$ in Lemma 24.

**Now, we consider $t = 1$.** The proof for $t = 1$, will follow the same steps as the proof for $t = 2$. For $t = 1$, based on Lemma 23 we substitute $\widehat{p}_H = 1$. Then we have

$$\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) = \sum_{i=1}^m z_i = \sum_{i=1}^m \left( (y_{Li} + (2-2y_{Li})y_{Hi}) \log \frac{\widehat{p}_L}{1-\widehat{p}_L} - 2\log \widehat{p}_L \right).$$

where for all $i \in [m]$, we have $|z_i| \leq c_2$ where $c_2$ is some positive constant. Now, Hoeffding's inequality yields that for any given $\widehat{p}_L$,

$$\mathbb{P}\left( \left| \widetilde{G}_1(\widehat{p}_L, 1, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[\widetilde{G}_1(\widehat{p}_L, 1, \vec{y}_L, \vec{y}_H)\right] \right| \geq c\sqrt{m\log m} \right) \leq 2\exp\left( -\frac{c^2 m \log(m^2)/2}{2c_2^2 m} \right).$$

Consider a set $\{p_{Li};\ \ i \in [\frac{1}{\Delta}]\}$ (with separation of $\Delta$ between consecutive points in the set) such that for all $\widehat{p}_L \in [0.5, 1-c_1]$ we have $\min_i |\widehat{p}_L - p_{Li}| \leq \Delta$, where $0 < \Delta < 0.5$. Then we have the following union bound

$$\mathbb{P}\left( \sup_{\widehat{p}_L \in \{p_{Li};\, i \in [\frac{1}{\Delta}]\}} \left| \widetilde{G}_1(\widehat{p}_L, 1, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[\widetilde{G}_1(\widehat{p}_L, 1, \vec{y}_L, \vec{y}_H)\right] \right| \leq c\sqrt{m\log m} \right) \geq 1 - \frac{c_3}{\Delta m^2}.$$

Furthermore, observe that the inequalities (B.30), (B.31), (B.32) hold for $\widetilde{G}_1$, giving the following concentration inequality,

$$\sup_{\widehat{p}_L \in [0.5, 1-c_1]; \widehat{p}_H = 1} \left| \widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H) - \mathbb{E}\left[\widetilde{G}_1(\widehat{p}_L, \widehat{p}_H, \vec{y}_L, \vec{y}_H)\right] \right| < c\sqrt{m\log m},$$

with probability atleast $1 - \frac{c_3}{m\sqrt{m}}$. Thus, for both $t \in \{1, 2\}$, we have the condition in Lemma 24 holds true with probability atleast $1 - \frac{c_3}{4m} - \frac{c_3}{m\sqrt{m}} \geq 1 - \frac{c_2}{m}$ where $c_2$ is some positive constant.

**Proof of Lemma 25**

We compute the difference between the risk of estimator with $t = 1$ and the risk of the estimator with $t = 2$ at $p_L, p_H = (0.7, 0.8)$ as follows. First, we note that

$$\mathbb{E}_{y_{ij}}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] - \mathbb{E}_{y_{ij}}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] = \mathbb{E}_{y_{ij}}\left[\mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] - \mathbb{E}_{y_{ij}}\left[\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right].$$

In the following analysis we drop the subscript pertaining to the question index $i$ for clarity of explanation. Now, from (B.3) observe that $f_1(y_L, y_H)$ and $f_2(y_L, y_H)$ differ in their output for $y_L, y_H \in \{(2,0),(0,1)\}$. For all other $y_L, y_H \in \{0,1,2\} \times \{0,1\} \backslash \{(2,0),(0,1)\}$, we have $f_1(y_L, y_H) = f_2(y_L, y_H)$. Without loss of generality, we assume $x^* = 1$. So, we get

$$\begin{aligned}
&\mathbb{E}_{y_{ij}}\left[\mathbb{1}\left(f_2(y_L, y_H) \neq 1\right)\right] - \mathbb{E}_{y_{ij}}\left[\mathbb{1}\left(f_1(y_L, y_H) \neq 1\right)\right] \\
&\quad= \sum_{y_L, y_H \in \{(2,0),(0,1)\}} \mathbb{P}(y_L, y_H)\left(\mathbb{1}(f_2(y_L, y_H) \neq 1) - \mathbb{1}(f_1(y_L, y_H) \neq 1)\right) \\
&\quad= \mathbb{P}(0,1) - \mathbb{P}(2,0) \\
&\quad= p_H(1-p_L)^2 - (1-p_H)p_L^2 \\
&\quad= 0.3^2 \times 0.8 - 0.2 \times (0.7)^2 = -0.026.
\end{aligned}$$

This proves Lemma 25.

**Proof of Lemma 26**

We want to show that for $p_L, p_H = 0.7, 0.8$, we have

$$\mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] \leq \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(\widehat{x}_{\text{MLE}_i} \neq x_i^*\right)\right] + \frac{c'}{m},$$

where $c' > 0$ is a constant. We denote $R_m(\widehat{x}_{\text{MLE}}) = \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(\widehat{x}_{\text{MLE}_i} \neq x_i^*\right)\right]$. Next, we indicate the estimator function chosen by MLE using the notation $t_{\text{MLE}} \in \{1,2\}$ which implies $\widehat{x}_{\text{MLE}_i} = f_{t_{\text{MLE}}}(y_{Li}, y_{Hi})$ for all $i \in [m]$. Thus we have two cases: $t_{\text{MLE}} = 1$ and $t_{\text{MLE}} = 2$. Now, note that we have from the part 1 of theorem 12, specifically (B.16), that for $p_L, p_H = 0.7, 0.8$, we have $t_{\text{MLE}} = 1$ with probability atleast $1 - \frac{c'}{m}$. Thus, we will have (for clarity we drop the subscript denoting the variables over which expectation is taken, it is $\vec{y}_L, \vec{y}_H$ )

$$\begin{aligned}
R_m(\widehat{x}_{\text{MLE}}) &= \mathbb{P}(t_{\text{MLE}} = 1)\mathbb{E}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\Big| t_{\text{MLE}} = 1\right] \\
&\quad + \mathbb{P}(t_{\text{MLE}} = 2)\mathbb{E}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\Big| t_{\text{MLE}} = 2\right] \\
&\geq \mathbb{P}(t_{\text{MLE}} = 1)\mathbb{E}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\Big| t_{\text{MLE}} = 1\right].
\end{aligned}$$

Now, to compute the conditional expectation obtained in the previous equation, we use the law of total expectation as

$$\mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right)] = \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right) \mid t_{\text{MLE}} = 1]\mathbb{P}(t_{\text{MLE}} = 1) + \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right) \mid t_{\text{MLE}} = 2]\mathbb{P}(t_{\text{MLE}} = 2).$$

This implies

$$\begin{aligned} R_m(\widehat{x}_{\text{MLE}}) &\geq \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right) \mid t_{\text{MLE}} = 1]\mathbb{P}(t_{\text{MLE}} = 1) \\ &= \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right)] - \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right) \mid t_{\text{MLE}} = 2]\mathbb{P}(t_{\text{MLE}} = 2) \\ &\overset{(a)}{\geq} \mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right)] - \frac{c'}{m}, \end{aligned}$$

where inequality (a) comes from $\mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right) \mid t_{\text{MLE}} = 2] \leq 1$ and $\mathbb{P}(t_{\text{MLE}} = 2) \leq \frac{c'}{m}$. Now, recall that $\mathbb{E}[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_1 \neq x_i^*\right)] = R_m(\widehat{x}_{\text{OMLE}})$, this gives $R_m(\widehat{x}_{\text{MLE}}) \geq R_m(\widehat{x}_{\text{OMLE}}) - \frac{c'}{m}$. This proves Lemma 26.

### B.1.4 Proof of Theorem 13

Recall from (3.2) that the error metric is defined as

$$R_m(\widehat{x}) = \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(\widehat{x}_i \neq x_i^*\right)\right].$$

To prove Theorem 13, we consider a hypothetical estimator that picks a $t$ based on the summand inside the expectation of the error metric as follows. Consider any given $p_L^*, p_H^*$ and $\vec{y}_L, \vec{y}_H$, then for $t \in \{1, 2\}$ we define $Z : \{0, 1, 2\}^m \times \{0, 1\}^m \to \mathbb{R}$ as

$$Z_t(\vec{y}_L, \vec{y}_H) = \frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_t(y_{Li}, y_{Hi}) \neq x_i^*\right) - \frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_{3-t}(y_{Li}, y_{Hi}) \neq x_i^*\right). \tag{B.33}$$

We consider a hypothetical estimator, denoted by $\widehat{h}_{t'} : \{0, 1, 2\}^m \times \{0, 1\}^m \to \{0, 1\}^m$ which maps the responses obtained $\vec{y}_L, \vec{y}_H$ based on $t' \in \{1, 2\}$ as follows

$$\widehat{h}_{t'}(p_L, p_H, \vec{y}_L, \vec{y}_H) = \{f_{t'}(y_{L1}, y_{H1}), \ldots, f_{t'}(y_{Lm}, y_{Hm})\},$$

where $t'$ is defined as

$$t' \in \arg\min_{t \in \{1,2\}} Z_t, \tag{B.34}$$

where ties are broken in favor of $t = 1$. Recall that both OMLE and MLE also behave similarly, where they first choose a $t$, denoted by $t_{\text{OMLE}}, t_{\text{MLE}}$ respectively. Next, we define the error incurred by the hypothetical estimator $\widehat{h}_{t'}$ as follows. For any given $\{x_i^*\}_{i\in[m]} \in \{0,1\}^m$ we have

$$R_m(\widehat{h}_{t'}) = \mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_{t'}(y_{Li}, y_{Hi}) \neq x_i^*\right)\right].$$

Since $Z_{t'} = \min_{\{1,2\}} Z_t = \min_{\{t_{\text{OMLE}},3-t_{\text{OMLE}}\}} Z_t$ for all $\vec{y}_L, \vec{y}_H$, we have $R_m(\widehat{h}_{t'}) \leq R_m(\widehat{x}_{\text{OMLE}})$. Similarly, we have $R_m(\widehat{h}_{t'}) \leq R_m(\widehat{x}_{\text{MLE}})$. In other words, our hypothetical estimator is better than any other estimator that chooses a $t \in \{1,2\}$ and then applies estimator $f_t$ to all questions. To analyse MLE and OMLE in comparison with this hypothetical estimator, we have the following Lemma to characterize the behavior of $Z_t$.

**Lemma 27** *Consider any given $p_L^*, p_H^* \in [0.5, 1]^2$ with $p_L^* \leq p_H^*$, and any $\vec{y}_L, \vec{y}_H$. Let $t_{OMLE} \in \{1,2\}$ be as defined in (3.7) and $t = 3 - t_{OMLE}$. Let $Z_t(\vec{y}_L, \vec{y}_H)$ be as defined in (B.33). Then*

1. *for $(p_L^*)^2(1-p_H^*) + (1-p_L^*)^2 p_H^* \neq 0$, we have*

$$\mathbb{P}(Z_t(\vec{y}_L, \vec{y}_H) \leq 0) \leq \exp\left(-m((p_L^*)^2(1-p_H^*) + (1-p_L^*)^2 p_H^*)^2/2\right),$$

2. *for $(p_L^*)^2(1-p_H^*) + (1-p_L^*)^2 p_H^* = 0$, we have $\mathbb{P}(Z_t(\vec{y}_L, \vec{y}_H) \leq -1/\sqrt{m\log m}) \leq 1/\sqrt{m}$.*

The proof of Lemma 27 is provided in Section B.1.4. Now, we use Lemma 27 for any given $p_L^*, p_H^*$ such that $(p_L^*)^2(1-p_H^*) + (1-p_L^*)^2 p_H^* \neq 0$, to get

$$\begin{aligned}
0 \geq R_m(\widehat{h}_{t'}) - R_m(\widehat{x}_{\text{OMLE}}) &= \mathbb{E}_{\vec{y}_L,\vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_{t'}(y_{Li}, y_{Hi}) \neq x_i^*\right) - \frac{1}{m}\sum_{i=1}^{m}\mathbb{1}\left(f_{t_{\text{OMLE}}}(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] \\
&= \sum_{\vec{y}_L,\vec{y}_H \in \{0,1,2\}^m \times \{0,1\}^m \text{ s.t.} Z_{t'}(\vec{y}_L,\vec{y}_H)<0} Z_{t'}(\vec{y}_L, \vec{y}_H)\mathbb{P}(\vec{y}_L, \vec{y}_H) \\
&\geq \sum_{\vec{y}_L,\vec{y}_H \in \{0,1,2\}^m \times \{0,1\}^m \text{ s.t.} Z_{t'}(\vec{y}_L,\vec{y}_H)<0} -\mathbb{P}(\vec{y}_L, \vec{y}_H) \\
&= -\mathbb{P}\left(Z_{t'}(\vec{y}_L, \vec{y}_H) < 0\right) \\
&\overset{(a)}{\geq} -\exp\left(-m((p_L^*)^2(1-p_H^*) + (1-p_L^*)^2 p_H^*)^2/2\right),
\end{aligned}$$

where inequality (a) follows directly from Lemma 27. Now, consider $p_L^*, p_H^*$ such that $((p_L^*)^2(1-$

$p_H^*) + (1-p_L^*)^2 p_H^*) = 0$. Then, we have

$$
\begin{aligned}
0 \geq R_{t'} - R_m(\widehat{x}_{\text{OMLE}}) &= \mathbb{E}_{\vec{y}_L, \vec{y}_H}\left[\frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_{t'}(y_{Li}, y_{Hi}) \neq x_i^*\right) - \frac{1}{m}\sum_{i=1}^{m} \mathbb{1}\left(f_{t_{\text{OMLE}}}(y_{Li}, y_{Hi}) \neq x_i^*\right)\right] \\
&= \sum_{\vec{y}_L, \vec{y}_H \in \{0,1,2\}^m \times \{0,1\}^m \text{ s.t.} Z_{t'}(\vec{y}_L, \vec{y}_H) < -1/\sqrt{m \log m}} Z_{t'}(\vec{y}_L, \vec{y}_H)\mathbb{P}(\vec{y}_L, \vec{y}_H) \\
&\quad + \sum_{\vec{y}_L, \vec{y}_H \in \{0,1,2\}^m \times \{0,1\}^m \text{s.t.} Z_{t'}(\vec{y}_L, \vec{y}_H) \in [-1/\sqrt{m \log m}, 0)} Z_{t'}(\vec{y}_L, \vec{y}_H)\mathbb{P}(\vec{y}_L, \vec{y}_H) \\
&\overset{(a)}{\geq} -\mathbb{P}\left(Z_{t'}(\vec{y}_L, \vec{y}_H) < -1/\sqrt{m \log m}\right) - 1/\sqrt{m \log m} \\
&\overset{(b)}{\geq} -2/\sqrt{m}
\end{aligned}
$$

where inequality (a) holds because of the fact $Z \geq -1$ and $\mathbb{P}\left(Z_{t'}(\vec{y}_L, \vec{y}_H) \in [-1/\sqrt{m \log m}, 0)\right) \leq 1$, and inequality (b) holds from Lemma 27 for all $m$ such that $\log m \geq 1$. Thus for any $p_L^*, p_H^*$ we have

$$0 \geq R_m(\widehat{h}_{t'}) - R_m(\widehat{x}_{\text{OMLE}}) \geq -2/\sqrt{m}.$$

Thus, for any $\epsilon > 0$, there exists a $m_\epsilon$ such that for all $m \geq m_\epsilon$, we have $R_m(\widehat{h}_{t'}) \geq R_m(\widehat{x}_{\text{OMLE}}) - \epsilon$. Furthermore, as $m \to \infty$ we have

$$\lim_{m \to \infty} \left| R_m(\widehat{h}_{t'}) - R_m(\widehat{x}_{\text{OMLE}}) \right| = 0.$$

Now, recall that from Theorem 11 we have

$$R_m(\widehat{x}_{\text{OMLE}}) - \frac{c}{\sqrt{m}} \leq R_m(\widehat{x}_{\text{PI}}) \leq R_m(\widehat{x}_{\text{OMLE}}) + \frac{c}{\sqrt{m}},$$

where $c$ is a constant. Using Theorem 11 we get

$$\lim_{m \to \infty} \left| R_m(\widehat{h}_{t'}) - R_m(\widehat{x}_{\text{PI}}) \right| = 0$$

and recall that $R_m(\widehat{h}_{t'}) \leq R_m(\widehat{x}_{\text{MLE}})$, with this we have $R_m(\widehat{x}_{\text{PI}}) \leq R_m(\widehat{x}_{\text{MLE}}) + \frac{c'}{\sqrt{m}}$ where $c'$ is a constant. This concludes the proof of Theorem 13.

**Proof of Lemma 27**

For this, we consider two cases: $t_{\text{OMLE}} = 1$ and $t_{\text{OMLE}} = 2$.

**Case 1:** $t_{\text{OMLE}} = 1$**.** Substituting this $t_{\text{OMLE}}$ in (3.7) with $k = 2$, we have

$$1 = \max\left(\left\lceil \frac{1}{2}\left(2 - \frac{\log \frac{p_H^*}{1-p_H^*}}{\log \frac{p_L^*}{1-p_L^*}}\right)\right\rceil, 0\right) + 1.$$

With simple algebraic manipulation, this implies

$$(p_L^*)^2(1-p_H^*) \le p_H^*(1-p_L^*)^2. \tag{B.35}$$

Now, observe that for any $y_{Li}, y_{Hi} \in \{(2,0),(0,1)\}$ we have $f_2(y_{Li}, y_{Hi}) \neq f_1(y_{Li}, y_{Hi})$. Specifically, we have $f_2(2,0) = 1, f_2(0,1) = 0, f_1(2,0) = 0, f_1(0,1) = 1$. In addition, for all $(y_L, y_H) \in \{0,1,2\} \times \{0,1\} \backslash \{(2,0),(0,1)\}$, we have $f_1(y_L, y_H) = f_2(y_L, y_H)$. Now without loss of generality, we consider $x_i^* = 1 \; \forall \; i \in [m_1]$ and $x_i^* = 0 \; \forall \; i \in [m] \backslash [m_1]$, where $0 \le m_1 \le m$. With this, and substituting $t' = 2, t_{\text{OMLE}} = 1$, we have

$$\begin{aligned} Z_2(\vec{y}_L, \vec{y}_H) &= \frac{1}{m}\sum_{i=1}^{m}\left(\mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right) - \mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right)\right) \\ &= \frac{1}{m}\left(\sum_{i=1}^{m_1}\left(\mathbb{1}(y_{Li}, y_{Hi} = 0,1) - \mathbb{1}(y_{Li}, y_{Hi} = 2,0)\right)\right. \\ &\qquad \left. + \sum_{i=m_1+1}^{m}\left(\mathbb{1}(y_{Li}, y_{Hi} = 2,0) - \mathbb{1}(y_{Li}, y_{Hi} = 0,1)\right).\right) \end{aligned}$$

Observe that under $x_i^* = 1$, we have $\mathbb{P}(y_{Li}, y_{Hi} = 0,1) = (1-p_L^*)^2 p_H^*$ and $\mathbb{P}(y_{Li}, y_{Hi} = 2,0) = (p_L^*)^2(1-p_H^*)$, and under $x_i^* = 0$, we have $\mathbb{P}(y_{Li}, y_{Hi} = 0,1) = (p_L^*)^2(1-p_H^*)$ and $\mathbb{P}(y_{Li}, y_{Hi} = 2,0) = (1-p_L^*)^2 p_H^*$. Thus, $Z_2(\vec{y}_L, \vec{y}_H)$ is the sample mean of the random variable $z_2$ distributed as

$$z_2 = \begin{cases} -1, & \text{wp } (p_L^*)^2(1-p_H^*) \\ 0, & \text{wp } 1 - p_L^2(1-p_H) - (1-p_L^*)^2 p_H^* \\ 1, & \text{wp } (1-p_L^*)^2 p_H^*. \end{cases}$$

From (B.35) we have $c_p = (1-p_L^*)^2 p_H^* - (p_L^*)^2(1-p_H^*) \ge 0$. Let $c_p > 0$. Now, using Hoeffding's inequality, where $\mathbb{E}[Z_2(\vec{y}_L, \vec{y}_H)] = c_p > 0$, we have (note we replace $Z_2(\vec{y}_L, \vec{y}_H)$ with $Z_2$ for clarity of explanation)

$$\mathbb{P}(Z_2 \le 0) = \mathbb{P}(Z_2 - \mathbb{E}[Z_2] \le -\left((1-p_L^*)^2 p_H^* - (p_L^*)^2(1-p_H^*)\right)) \le \exp\left(-mc_p^2/2\right).$$

Next consider $c_p = \mathbb{E}[Z_2] = 0$. Then we have

$$\mathbb{P}(Z_2 \le -1/\sqrt{m\log m}) = \mathbb{P}(Z_2 - \mathbb{E}[Z_2] \le -1/\sqrt{m\log m}) \le \exp\left(-\log m/2\right) = \frac{1}{\sqrt{m}}.$$

**Case 2:** $t_{\text{OMLE}} = 2$**.** Similarly to case 1, in this case (B.35) becomes

$$(p_L^*)^2(1-p_H^*) > p_H^*(1-p_L^*)^2.$$

Thus, we have

$$Z_1(\vec{y}_L, \vec{y}_H) = \frac{1}{m}\sum_{i=1}^{m} z_{1_i} = \frac{1}{m}\sum_{i=1}^{m} \left(\mathbb{1}\left(f_1(y_{Li}, y_{Hi}) \neq x_i^*\right) - \mathbb{1}\left(f_2(y_{Li}, y_{Hi}) \neq x_i^*\right)\right),$$

where for all $i \in [m]$, it holds that $z_1$ is distributed as

$$z_1 = \begin{cases} -1, & \text{wp } (1-p_L^*)^2 p_H^* \\ 0, & \text{wp } 1 - p_L^2(1-p_H) - (1-p_L^*)^2 p_H^* \\ 1, & \text{wp } (p_L^*)^2(1-p_H^*). \end{cases}$$

Then, similarly to case 1, we define $c_p = (p_L^*)^2(1-p_H^*) - (1-p_L^*)^2 p_H^* \geq 0$. Then for $c_p > 0$. we have

$$\mathbb{P}(Z_1 \leq 0) = \mathbb{P}(Z_1 - \mathbb{E}[Z_1] \leq -\left((1-p_L^*)^2 p_H^* - (p_L^*)^2(1-p_H^*)\right)) \leq \exp\left(-mc_p^2/2\right),$$

and for $c_p = \mathbb{E}[Z_1] = 0$ we have

$$\mathbb{P}(Z_1 \leq -1/\sqrt{m\log m}) = \mathbb{P}(Z_1 - \mathbb{E}[Z_1] \leq -1/\sqrt{m\log m}) \leq \exp\left(-\log m/2\right) = \frac{1}{\sqrt{m}}.$$

This concludes our proof for Lemma 27.

# Appendix C

# To ArXiv or not to ArXiv

## C.1 Survey details for Q2.

**Target audience selection** Recall that our objective in target audience selection is to find reviewers for each paper whose research interests intersect with the paper, so that we can survey these reviewers about having seen the corresponding papers outside of reviewing contexts. We describe the exact process for target audience selection in EC and ICML.

In EC, the number of papers posted online before the end of the review process was small. To increase the total number of paper-reviewer pairs where the paper was posted online and the reviewer shared similar research interests with the paper, we created a new paper-reviewer assignment. For the new paper-reviewer assignment, for each paper we considered at most 8 members of the reviewing committee that satisfied the following constraints as its target audience—(1) they submitted a positive bid for the paper indicating shared interest, (2) they are not reviewing the given paper.

In ICML, a large number of papers were posted online before the end of the review process. So, we did not create a separate paper-reviewer assignment for surveying reviewers. Instead, in ICML, we consider a paper's reviewers as its target audience and queried the reviewers about having seen it, directly through the reviewer response form.

**Survey question.** For research question Q2, we conducted a survey to measure the visibility of papers submitted to the conference and posted online before or during the review process. We describe the details of the survey for EC 2021 and ICML 2021 separately. In EC 2021, we created a specialised reviewer-specific survey form shared with all the reviewers. Each reviewer was shown the title of five papers and asked to answer the following question for each paper:

"Have you come across this paper earlier, outside of reviewing contexts?"

In the survey form, we provided examples of reviewing contexts as "reviewing the paper in any venue, or seeing it in the bidding phase, or finding it during a literature search regarding another paper you were reviewing." The question had multiple choices as enumerated in Table C.1, and the reviewer could select more than one choice. If they selected one or more options from (b), (c), (d) and (e), we set the visibility to 1, and if they selected option (a), we set the visibility to 0. We did not use the response in our analysis, if the reviewer did not respond or only chose option

(f). In Table C.1, we also provide the number of times each choice was selected in the set of responses obtained.
In ICML 2021, we added a two-part question corresponding to the research question Q2 in the reviewer response. Each reviewer was asked the following question for the paper they were reviewing:

"Do you believe you know the identities of the paper authors? If yes, please tell us how."

Each reviewer responded either *Yes* or *No* to the first part of the question. For the second part of the question, table C.2 lists the set of choices provided for the question, and a reviewer could select more than one choice. If they responded *Yes* to the first part, and selected one or more options from (a), (d), (e) and (f) for the second part, then we set the visibility to 1, otherwise to 0. In Table C.2, we also provide the number of times each choice was selected in the set of responses that indicated a visibility of 1.

## C.2 Analysis procedure details

In this section we provide some more details of the analysis procedure.

### C.2.1 Kendall's Tau-b statistic

We describe the procedure for computing Kendall's Tau-b statistic between two vectors. Let $n$ denote the length of each vector. Let us denote the two vectors as $[x_1, x_2, \ldots, x_n] \in \mathbb{R}^n$ and $[y_1, y_2, \ldots, y_n] \in \mathbb{R}^n$. Let $P$ denote the number of concordant pairs in the two vectors, defined formally as

$$P = \sum_{\substack{(i,k) \in [n]^2 \\ i<k}} \left( \mathbb{I}\left(x_i > x_k\right) \mathbb{I}\left(y_i > y_k\right) + \mathbb{I}\left(x_i < x_k\right) \mathbb{I}\left(y_i < y_k\right) \right).$$

| List of choices for question in Q2 survey | Count |
|---|---|
| (a) I have NOT seen this paper before / I have only seen the paper in reviewing contexts | 359 |
| (b) I saw it on a preprint server like arXiv or SSRN | 51 |
| (c) I saw a talk/poster announcement or attended a talk/poster on it | 22 |
| (d) I saw it on social media (e.g., Twitter) | 4 |
| (e) I have seen it previously outside of reviewing contexts (but somewhere else or don't remember where) | 29 |
| (f) I'm not sure | 24 |

Table C.1: Set of choices provided to reviewers in EC in Q2 survey and the number of times each choice was selected in the responses obtained. There were 449 responses in total, out of which 92 responses indicated a visibility of 1.

| List of choices for question in Q2 survey | Count |
|---|---|
| (a) I was aware of this work before I was assigned to review it. | 373 |
| (b) I discovered the authors unintentionally while searching web for related work during reviewing of this paper | 47 |
| (c) I guessed rather than discovered whose submission it is because I am very familiar with ongoing work in this area. | 28 |
| (d) I first became aware of this work from a seminar announcement, Archiv announcement or another institutional source | 259 |
| (e) I first became aware of this work from a social media or press posting by the authors | 61 |
| (f) I first became aware of this work from a social media or press posting by other researchers or groups (e.g. a ML blog or twitter stream) | 52 |

Table C.2: Set of choices provided to reviewers in ICML in Q2 survey question and the number of times each choice was selected in the set of responses considered that self-reported knowing the identities of the paper authors outside of reviewing contexts. There were a total of 635 such responses that indicated a visibility of 1. Recall that for ICML, we consider the set of responses obtained for submissions that were available as preprints on arXiv. There were 1934 such submissions.

Following this, we let the number of discordant pairs in the two vectors be denoted by $Q$, defined as

$$Q = \sum_{\substack{(i,k)\in[n]^2 \\ i<k}} \left(\mathbb{I}\left(x_i > x_k\right)\mathbb{I}\left(y_i < y_k\right) + \mathbb{I}\left(x_i < x_k\right)\mathbb{I}\left(y_i > y_k\right)\right).$$

Observe that the concordant and discordant pairs do not consider pairs with ties in either of the two vectors. In our data, we have a considerable number of ties. To account for ties, we additionally compute the following statistics. Let $A_x$ and $A_y$ denote the number of pairs in the two vectors tied in exactly one of the two vectors as

$$A_x = \sum_{\substack{(i,k)\in[n]^2 \\ i<k}} \mathbb{I}\left(x_i = x_k\right)\mathbb{I}\left(y_i \neq y_k\right) \qquad \text{and} \qquad A_y = \sum_{\substack{(i,k)\in[n]^2 \\ i<k}} \mathbb{I}\left(x_i \neq x_k\right)\mathbb{I}\left(y_i = y_k\right).$$

Finally, let $A_{xy}$ denote the number of pairs in the two vectors tied in both vectors, as

$$A_{xy} = \sum_{\substack{(i,k)\in[n]^2 \\ i<k}} \mathbb{I}\left(x_i = x_k\right)\mathbb{I}\left(y_i = y_k\right).$$

Observe that the five statistics mentioned above give a mutually exclusive and exhaustive count of pairs of indices, with $P+Q+A_x+A_y+A_{xy} = 0.5n(n-1)$. With this setup in place, we have the Kendall's Tau-b statistic between $[x_1, x_2, \ldots, x_n] \in \mathbb{R}^n$ and $[y_1, y_2, \ldots, y_n] \in \mathbb{R}^n$ denoted by $\tau$ as

$$\tau = \frac{P - Q}{\sqrt{\left(P + Q + A_x\right)\left(P + Q + A_y\right)}}. \tag{C.1}$$

This statistic captures the correlation between the two vectors.

**Input** : Samples $v_i, \alpha_i, \widetilde{t}_i$ for $i \in [N]$, iteration count $\gamma$.
**(1)** Compute the test statistic $T$ defined in (4.1).
**(2)** For $z \leftarrow 1$ to $\gamma$:
**(i)** For all $b \in \{1, 2, 3\}$: Let $V_b$ denote the number of responses with $v_i = 1$ in bin $b$. Take all the responses in bin $b$ and reassign each response's visibility to 0 or 1 uniformly at random such that the total number of responses with a visibility of 1 remains the same as $V_b$.
**(ii)** Using the new values of visibility in all bins, recompute the test statistic in (4.1). Denote the computed test statistic as $T_z$.
**Output** : *P* value $= \frac{1}{\gamma} \sum_{z=1}^{\gamma} \mathbb{I}(T_z - T > 0)$.

**Algorithm 6:** Permutation test for correlation between papers' visibility and rank.

### C.2.2 Permutation test

The test statistic $T$ in (4.1) gives us the effect size for our test. Recall from (4.1) that the test statistic $T$ is defined as:

$$T = \frac{N_1\,\tau_1 + N_2\,\tau_2 + N_3\,\tau_3}{N_1 + N_2 + N_3},$$

where for each bin value $b \in \{1, 2, 3\}$, we have $N_b$ as the number of responses obtained in that bin, and $\tau_b$ represents the Kendall Tau-b correlation between visibility and rank in the responses obtained in that bin. To analyse the statistical significance of the effect, we define some notation for our data. Let $N$ denote the total number of responses. For each response $i \in [N]$ we denote the visibility of the paper to the reviewer as $v_i \in \{0, 1\}$ and the rank associated with response $i$ as $\alpha_i \in \mathbb{N}_{<0}$. Finally, we denote the bin associated with response $i$ as $\widetilde{t}_i \in \{1, 2, 3\}$. With this, we provide the algorithm for permutation testing in Algorithm 6.

# Appendix D

# Cite-seeing and Reviewing

In this section we provide additional details on our analysis procedure.

## D.1 Controlling for confounding factors

As described in Section 5.3.2, our analysis relies on a number of characteristics (`quality`, `expertise`, `preference`, `seniority`) to account for confounding factors C2–C5. The value of `quality` is, of course, unknown and we exclude it from the analysis by focusing on differences in reviewers' evaluations made for the same submission (details in Appendix D.2.2). For the remaining characteristics, we use a number of auxiliary variables available to conference organizers to quantify these characteristics. These variables differ between conferences and Table D.1 summarizes the details for both venues.

| Characteristic | Auxiliary variable | EC 2021 | ICML 2021 |
|---|---|---|---|
| `expertise` | Self-reported expertise | In both venues, reviewers were asked to self-evaluate their ex post expertise in reviewing submissions using a 4-point Likert item. The evaluations were submitted together with initial reviews and higher values represent higher expertise. We encode these evaluations in a continuous variable `expertiseSRExp`. | |
| | Self-reported confidence | Not used in the conference. | Similar to expertise, reviewers were asked to evaluate their ex post confidence in their evaluation on a 4-point Likert item. We encode these evaluations in a continuous variable `expertiseSRConf`. |
| | Textual overlap | Not used in the conference. | TPMS measure of textual overlap (Charlin and Zemel, 2013) between a submission and a reviewer's past papers (real value between 0 and 1; higher values represent higher overlap). We denote this quantity `expertiseText`. Out of 3,335 (submission, reviewer) pairs that qualify for the analysis (before data filtering is executed), 439 pairs have the value of `expertiseText` missing due to reviewers not creating their TPMS accounts. Entries with missing values were removed from the analysis. |
| `preference` | Self-reported preference | Reviewers reported partial rankings of submissions in terms of their preference in reviewing them by assigning each submission a non-zero value from -100 to 100 (the higher the value the higher the preference; non-reported preferences are encoded as 0). In the automated assignment, reviewers were not assigned to papers with negative preferences. Assignment of submissions to reviewers who did not enter a preference was discouraged, but not forbidden. For analysis, we transform non-negative preferences into percentiles `prefPerc` (0 means top preference, 100 – bottom). | Reviewers bid on submissions by reporting a value from 2 (Not willing to review) to 5 (Eager to review). In the automated assignment, reviewers were not assigned to papers with bids of value 2. Assignment of submissions to reviewers who did not enter a bid was discouraged, but not forbidden. As a result, out of 3,335 (submission, reviewer) pairs that qualify for the analysis (before data filtering is executed), 159 pairs had the value of bid missing. Entries with missing values were removed from the analysis. Positive bids (3, 4, 5) are captured in the continuous variable `prefBid`. |
| | Missing preference | Out of 849 (submission, reviewer) pairs that qualify for the analysis (before data filtering is executed), 154 have the reviewer's preference missing. This missingness is captured in a binary indicator `missingPref`. | Not used in the analysis as data points with missing preferences are excluded from the analysis. |
| | | Program chairs split the reviewer pool in two groups: *curated* — reviewers with significant review experience or personally | Program chairs split the reviewer |

## D.2 Details of the parametric inference

Conceptually, the parametric analysis of both EC 2021 and ICML 2021 data is similar up to the specific implementation of our model (5.1) in these venues. In this section, we specify this parametric model for both venues using variables introduced in Table D.1 (Section D.2.1), and also introduce the procedure used to eliminate the `quality` variable whose values are unobserved (Section D.2.2).

### D.2.1 Specification of parametric model

We begin by specifying the model (5.1) to each of the venues we consider in the analysis.

**ICML** With auxiliary variables introduced in Table D.1, our model (5.1) for ICML reduces to the following specification:

$$\texttt{score} \sim \alpha_0 + \alpha_1 \cdot \texttt{quality} + \alpha_2^{(1)} \cdot \texttt{expertiseSRExp} + \alpha_2^{(2)} \cdot \texttt{expertiseSRConf} + \alpha_2^{(3)} \cdot \texttt{experti}$$
$$+ \alpha_3 \cdot \texttt{prefBid} + \alpha_4 \cdot \texttt{seniority} + \alpha^* \cdot \texttt{citation}.$$

**EC** An important difference between our ICML and EC analyses is that in the latter we do not remove entries with missing values of auxiliary variables but instead incorporate the data missingness in the model. For this, recall that in EC, the only source of missingness is reviewers not reporting their `preference` in reviewing submissions. To incorporate this missingness, we enhance the model by an auxiliary binary variable `missingPref` that equals one when the `preference` is missing and enables the model to accommodate associated dynamics:

$$\texttt{score} \sim \alpha_0 + \alpha_1 \cdot \texttt{quality} + \alpha_2 \cdot \texttt{expertiseSRExp} + \alpha_3^{(1)} \cdot \texttt{prefPerc} + \alpha_3^{(2)} \cdot \texttt{missingPref}$$
$$+ \alpha_4 \cdot \texttt{seniority} + \alpha^* \cdot \texttt{citation}.$$

### D.2.2 Elimination of submission quality from the model

Having the conference-specific models defined, we now execute the following procedure to exclude the unobserved variable `quality` from the analysis. For ease of exposition, we illustrate the procedure on the model (5.1) as details of this procedure do not differ between conferences.

**Step 1. Averaging scores of CITED and UNCITED reviewers** Each submission used in the analysis is assigned to at least one CITED and at least one UNCITED reviewer. Given that there may be more than one reviewer in each category, we begin by averaging the scores given by CITED and UNCITED reviewers to each submission. The linear model assumptions behind our model (5.1) ensure that for each submission, averaged scores $\texttt{score}_{\text{ctd}}$ and $\texttt{score}_{\text{unctd}}$ also adhere to the following linear models:

$$\texttt{score}_{\text{ctd}} \sim \alpha_0 + \alpha_1 \cdot \texttt{quality} + \alpha_2 \cdot \texttt{expertise}_{\text{ctd}} + \alpha_3 \cdot \texttt{preference}_{\text{ctd}} + \alpha_4 \cdot \texttt{seniority}_{\text{ctd}} + \alpha^*, \tag{D.1a}$$

$$\texttt{score}_{\text{unctd}} \sim \alpha_0 + \alpha_1 \cdot \texttt{quality} + \alpha_2 \cdot \texttt{expertise}_{\text{unctd}} + \alpha_3 \cdot \texttt{preference}_{\text{unctd}} + \alpha_4 \cdot \texttt{seniority}_{\text{unctd}}. \tag{D.1b}$$

In these equations, subscripts "ctd" and "unctd" represent means of the corresponding values taken over CITED and UNCITED reviewers, respectively. Variances of the corresponding Gaussian noise in these models are inversely proportional to the number of CITED reviewers (D.1a) and the number of UNCITED reviewers (D.1b).

**Step 2. Taking difference between mean scores** Next, for each submission, we take the difference between mean scores $\texttt{score}_{\text{ctd}}$ and $\texttt{score}_{\text{unctd}}$ and observe that the linear model assumptions again ensure that the difference ($\texttt{score}_{\Delta}$) also follows the linear model:

$$\texttt{score}_{\Delta} \sim \alpha_2 \cdot \texttt{expertise}_{\Delta} + \alpha_3 \cdot \texttt{preference}_{\Delta} + \alpha_4 \cdot \texttt{seniority}_{\Delta} + \alpha^*. \quad \text{(D.2)}$$

Subscript $\Delta$ in this equation denotes the difference between the mean values of the corresponding quantity across CITED and UNCITED conditions: $X_{\Delta} = X_{\text{ctd}} - X_{\text{unctd}}$. Observe that by taking a difference we exclude the original intercept $\alpha_0$ and the unobserved `quality` variable from the model. Thus, all the variables in the resulting model (D.2) are known and we can fit the data we have into the model. Each submission used in the analysis contributes one data point that follows the model (D.2) with a submission-specific level of noise:

$$\sigma^2 = \sigma_0^2 \left( \frac{1}{\#\text{CITED}} + \frac{1}{\#\text{UNCITED}} \right),$$

where $\sigma_0^2$ is the level of noise in the model (5.1) that defines individual behavior of each reviewer.

**Step 3. Fitting the data** Having removed the unobserved variable `quality` from the model, we use the weighted linear regression algorithm implemented in the R `stats` package (R Core Team, 2013) to test for significance of the target coefficient $\alpha^*$.

## D.3 Details of the non-parametric inference

Non-parametric analysis conducted in ICML 2021 consists of two steps that we now discuss.

**Step 1. Matching** First, we conduct matching of (submission, reviewer) pairs by executing the following procedure separately for each submission. Working with a given submission $\mathcal{S}$, we consider two groups of reviewers assigned to $\mathcal{S}$: CITED and UNCITED. Next, we attempt to find CITED reviewer $\mathcal{R}_{\text{ctd}}$ and UNCITED reviewer $\mathcal{R}_{\text{unctd}}$ that are similar in terms of `expertise`, `preference`, and `seniority` characteristics. More formally, in terms of variables we introduced in Table D.1, reviewers $\mathcal{R}_{\text{ctd}}$ and $\mathcal{R}_{\text{unctd}}$ should satisfy *all of the following criteria* with respect to $\mathcal{S}$:

- Self-reported expertise of reviewers in reviewing submission $\mathcal{S}$ is the same:

$$\texttt{expertiseSRExp}_{\text{ctd}} = \texttt{expertiseSRExp}_{\text{unctd}}$$

- Self-reported confidence of reviewers in their evaluation of submission $\mathcal{S}$ is the same:

$$\texttt{expertiseSRConf}_{\text{ctd}} = \texttt{expertiseSRConf}_{\text{unctd}}$$

- Textual overlap between submission $\mathcal{S}$ and papers of each of the reviewers differ by at most $0.1$:

$$|\texttt{expertiseText}_{\text{ctd}} - \texttt{expertiseText}_{\text{unctd}}| \leq 0.1$$

- Reviewers' bids on sibmission $\mathcal{S}$ satisfy one of the two conditions:
  1. Both bids have value $3$ ("In a pinch"):

$$\texttt{prefBid}_{\text{ctd}} = \texttt{prefBid}_{\text{unctd}} = 3$$

  2. Both bids have values greater than $3$ (4-"Willing" or 5-"Eager"):

$$\texttt{prefBid}_{\text{ctd}} \in \{4, 5\} \quad \text{and} \quad \texttt{prefBid}_{\text{unctd}} \in \{4, 5\}$$

- Reviewers belong to the same seniority group:

$$\texttt{seniority}_{\text{ctd}} = \texttt{seniority}_{\text{unctd}}$$

We run this procedure for all submissions in the pool. If for submission $\mathcal{S}$ there are no reviewers $\mathcal{R}_{\text{ctd}}$ and $\mathcal{R}_{\text{unctd}}$ that satisfy these criteria, we remove submission $\mathcal{S}$ from the non-parametric analysis. Overall, we let $K$ denote the number of such 1-1 matched pairs obtained and introduce the set of triples that the remaining analysis operates with:

$$\left\{ \left[ (\mathcal{S}^{(i)}, \mathcal{R}^{(i)}_{\text{ctd}}, \mathcal{R}^{(i)}_{\text{unctd}}) \right] \right\}_{i=1}^{K}. \tag{D.3}$$

Each triple in this set consists of submission $\mathcal{S}$ and two reviewers $\mathcal{R}_{\text{ctd}}$ and $\mathcal{R}_{\text{unctd}}$ that (i) are assigned to $\mathcal{S}$ and (ii) satisfy the aforementioned conditions with respect to $\mathcal{S}$. Within each submission, each reviewer can be a part of only one triple.

Let us now consider two (submission, reviewer) pairs associated with a given triple. Observe that these pairs share the submission, thereby sharing the value of unobserved characteristic `quality`. Additionally, the criteria used to select reviewers $\mathcal{R}_{\text{ctd}}$ and $\mathcal{R}_{\text{unctd}}$ ensures that characteristics `expertise`, `preference`, and `seniority` are also similar across these pairs. Crucially, while being equal on all four characteristics, these pairs have different values of the `citation` indicator.

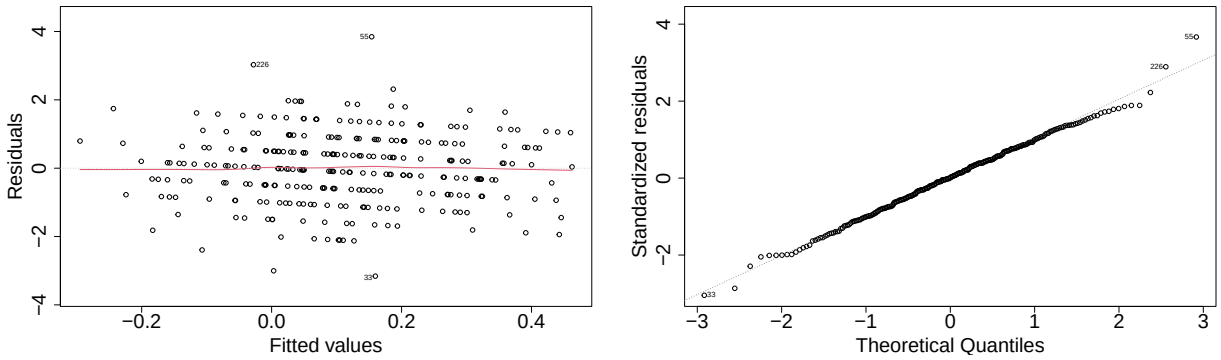
**Step 2. Permutation test** Having constructed the set of triples (D.3), we now compare scores given by CITED and UNCITED reviewers within these triples. Specifically, consider triple $i \in \{1, \ldots, K\}$ and let $Y^{(i)}_{\text{ctd}}$ (respectively, $Y^{(i)}_{\text{unctd}}$) be the score given by CITED reviewer $\mathcal{R}^{(i)}_{\text{ctd}}$ (respectively, UNCITED reviewer $\mathcal{R}^{(i)}_{\text{unctd}}$) to submission $\mathcal{S}^{(i)}$. Then the test statistic $\tau$ of our analysis is defined as follows:

$$\tau = \frac{1}{K} \sum_{i=1}^{K} \left( Y^{(i)}_{\text{ctd}} - Y^{(i)}_{\text{unctd}} \right). \tag{D.4}$$

To quantify the significance of the difference between scores given by CITED and UNCITED reviewers, we execute the permutation test (Fisher, 1935). Specifically, at each of the 10,000 iterations, we independently permute the `citation` indicator within each triple $i \in \{1, \ldots, K\}$. For each permuted sample, we recompute the value of the test statistic (D.4) and finally check whether the actual value of the test statistic $\tau$ appears to bee "too extreme" for the significance level $0.05$.

## D.4 Model Diagnostics

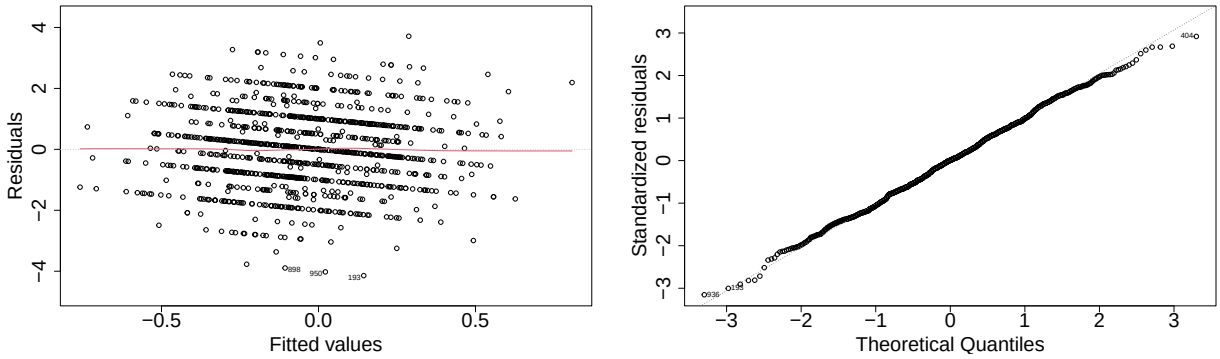
Conclusions of our parametric analysis depend on the linear regression assumptions that we cannot a priori verify. To get some insight on whether these assumptions are satisfied, we conduct basic model diagnostics. Visualizations of these diagnostics are given in Figure D.1 (EC 2021) and Figure D.2 (ICML 2021). Overall, the diagnostics we conduct do not reveal any critical violations of the underlying modeling assumptions and suggest that our linear model (5.1) provides a reasonable fit to the data.




(a) Residuals vs Fitted

(b) Normal Q-Q

Figure D.1: Model diagnostics for the EC 2021 parametric analysis. Residuals do not suggest any critical violation of model assumptions.




(a) Residuals vs Fitted

(b) Normal Q-Q

Figure D.2: Model diagnostics for the ICML 2021 parametric analysis. Residuals do not suggest any critical violation of model assumptions.

# Appendix E

# Perceptions in NeurIPS 2021

## E.1 More details about the experiment

In this section, we provide details about the experiment, augmenting the details provided in Section 6.3. First, we focus on the release timeline of the surveys. Then we provide details about the content of the surveys, including the instructions provided.

*Timeline.* Phase 1 of the experiment was conducted soon after the paper submission deadline in order to obtain authors' perceptions of their submitted papers while the papers were still fresh on their minds. The paper submission deadline was on May 28, 2021. The Phase 1 survey was released shortly after, on June 1. Authors were invited to participate in the survey through June 11, after which the survey was closed. To increase participation in the survey, the program chairs sent a reminder email about the experiment on June 9.

Phase 2 of the experiment aimed at understanding the change in authors' perception of their papers after receiving the initial reviews. The authors received the initial set of reviews on August 3, 2021 and were able to provide their rebuttal (response to the initial reviews) any time until August 10. We invited authors to participate in the Phase 2 survey on August 12. The peer review process was concluded on September 28, 2021, with the announcement of final decisions.

*Instructions.* In both the Phase 1 and Phase 2 surveys, authors were provided information regarding the privacy and confidentiality of their survey responses. They were informed that during the review process, only the authors themselves could view their responses, in addition to the administrators of OpenReview.net (the conference management platform used by NeurIPS 2021). It was emphasised that authors' responses could not affect the outcome of the review process and that the responses would not be visible to their co-authors, reviewers, area chairs, or senior area chairs at any point of time. Regarding the analyses and following dissemination of the findings from the experiment, the survey mentioned that, "After the review process, the survey responses will be made available to the NeurIPS 2021 program chairs and Workflow chairs for statistical analyses. Any information shared publicly will be anonymized and only reported in an aggregated manner that protects your identities." For the purposes of analysis, responses and profiles were accessed algorithmically via the OpenReview api. Further, authors were also told,

"To allow authors to freely provide their opinions and keep samples as independent as possible, please do not discuss your answers to these survey questions with other NeurIPS 2021 authors (including your co-authors), or ask others about their responses."

In Phase 1 of the experiment, we asked authors with multiple submissions to rank their submissions. The instructions for providing ranking were as follows: "Rank your papers in terms of your own perception of their scientific contributions to the NeurIPS community, if published in their current form. Rank 1 indicates the paper with the greatest scientific contribution; ties are allowed, but please use them sparingly. In the table entry for each submission below, there is a pull-down menu called "Paper Ranking." Please click on it and specify the rank for that submission."

Finally, among the 6237 authors with multiple submissions, 32 authors (0.5%) provided a ranking for only one of their submissions. We exclude these responses from the analysis of the ranking.

## E.2 More details about demographic analysis

In this section, we provide details about the analyses we conduct to test for significant difference in calibration error across demographic groups in Section 6.5.2. To describe the analysis, we first define some notation. Let $n$ denote the total number of responses obtained in Phase 1 of our experiment. We will use $i$ as an index over responses, where each response pertains to a single author-paper pair. For response $i$, let $p_i \in [0, 1]$ be the acceptance probability indicated by the author. The observed outcome of the associated paper is a binary indicator, denoted by $y_i \in \{0, 1\}$, where $y_i = 1$ if the paper is accepted and $y_i = 0$ if it is rejected. The self-reported gender of the associated author is denoted by $g_i \in G \coloneqq \{\text{Female, Male, Other, Unspecified}\}$. Note that there are responses where the associated authors did not provide a gender in their Open Review profile. All authors' seniority is classified into three types based on their reviewing participation, denoted by $s_i \in S \coloneqq \{\text{Meta-reviewer, Reviewer, Neither}\}$.

Finally, we include the geographical region associated with the author, denoted by $r_i$. To assign a geographical region to each author, we use the institutional domain of the author's primary affiliation. We classify the geographical regions using the geographical region division provided by the United Nations Statistics Division (UNSD). Within their division of regions, we further break each region with more than 100 responses in our survey into sub-regions listed by UNSD. This yields the following set of regions denoted by $R \coloneqq \{$Africa, North America, Latin America and the Caribbean, Central Asia, Eastern Asia, South-eastern Asia, Southern Asia, Western Asia, Eastern Europe, Northern Europe, Southern Europe, Western Europe, Oceania$\}$.

To measure accuracy, we use the Brier score (i.e., squared loss). For response $i$, the Brier score is given by $(y_i - p_i)^2$. With this notation, we define the average calibration error for a gender-based subgroup. To account for confounding by authors' seniority and geographical region, we bin all responses based on their corresponding seniority and geographical region, and compute their prevalence rate in the population. This gives the weight to be assigned to each

response to compute the average calibration error for gender-based subgroups as

$$M_g = \sum_{r \in R} \sum_{s \in S} \left( \frac{\sum_{i \in [n]} \mathbb{I}(g_i = g, r_i = r, s_i = s)(y_i - p_i)^2}{\sum_{i \in [n]} \mathbb{I}(g_i = g, r_i = r, s_i = s)} \times \frac{\sum_{i \in [n]} \mathbb{I}(r_i = r, s_i = s)}{n} \right), \quad \text{(E.1)}$$

where $\mathbb{I}(\cdot)$ is the indicator function. Using this definition of calibration error of a gender subgroup, we derive 95% confidence intervals using bootstrapping (Efron and Tibshirani, 1986). We now move on to our hypothesis comparing miscalibration between male authors and female authors. Formally, in terms of (E.1), the hypothesis is stated as:

$$H_0 : M_{\text{male}} = M_{\text{female}},$$
$$H_1 : M_{\text{male}} \neq M_{\text{female}}.$$

To test this hypothesis, we conduct a permutation test to obtain its significance ($p$-value). In the permutation test, we permute our data within each demographic subgroup of seniority and geographical region. From the permutation test, we obtain a $p$-value of $0.0006$. To account for multiple testing we use the Benjamini-Hochberg procedure (Benjamini and Hochberg, 1995), which gives a final $p$-value of $0.0012$.

Similarly, we compute the average calibration error for seniority-based subgroups, while accounting for confounding by gender and geographical region. In this analysis, we filter out the responses by authors who did not report their gender. Since the set of authors who did not report their gender may be a heterogeneous set, including this set in the analysis for seniority will violate the exchangeability assumption of the permutation test. Thus, the total number of responses considered in the seniority analysis, denoted by $n_{g \in G}$, is given by $\sum_{i \in [n]} \mathbb{I}(g_i \in G)$. With this, the average calibration error corresponding to each seniority level, for $s \in S$, is given by

$$M_s = \sum_{r \in R} \sum_{g \in G} \left( \frac{\sum_{i \in [n]} \mathbb{I}(s_i = s, r_i = r, g_i = g)(y_i - p_i)^2}{\sum_{i \in [n]} \mathbb{I}(s_i = s, r_i = r, g_i = g)} \times \frac{\sum_{i \in [n]} \mathbb{I}(r_i = r, g_i = g)}{n_{g \in G}} \right). \quad \text{(E.2)}$$

We use bootstrapping to compute 95% confidence intervals. Further, we conduct a permutation test to compare the miscalibration by meta-reviewers (ACs and SACs) and other reviewers. This hypothesis is stated as

$$H_0 : M_{\text{meta-reviewer}} = M_{\text{reviewer}},$$
$$H_1 : M_{\text{meta-reviewer}} \neq M_{\text{reviewer}},$$

where $M_{\text{meta-reviewer}}$ and $M_{\text{reviewer}}$ are as defined in (E.2). The permutation test yields a $p$-value of $0.055$. Accounting for multiple testing using the Benjamini-Hochberg procedure does not alter the $p$-value.

# Appendix F

# Anonymity in Reviewer Discussions

## F.1 Assessing politeness of discussion posts

To assign a politeness score to each text, without compromising the privacy of the peer review data, we used a local implementation of a large language model. Specifically, we implemented the most recent and quantized version of the largest variant of Vicuna, `vicuna-13B-v1.5-GPTQ`,[1] with a context length of 4,096 tokens. We set the temperature to be 0.7, and limit the generation output to numbers $\{1, 2, 3, 4, 5\}$.

To run the Vicuna model, we first downloaded its model weights using the `TheBloke/vicuna-13B-v1.5` model checkpoint from huggingface. To run the model in inference mode in order to generate the politeness scores, we used the Python package `ExLlama`.[2]

We carefully craft a few-shot learning based prompt with three examples chosen from the politeness dataset provided in Bharti et al. (2023). Our overall prompt design consists of the elements described in Appendix F.1. Each prompt concatenates all the elements in order, namely Instruction + Examples + Query. The discussion text to be assessed is added in place of `[post]` to complete the prompt. Further, we limit the generation output to integers 1–5, by disallowing tokens other than 1–5 using the function "`ExLlamaGenerator.disallow_tokens()`."

To be robust to the biases shown by generative models in their output, due to the ordering of the few-shot examples, we do the following. We create six paraphrases for each post by alternating the order of the three examples used in the prompt. That is, we concatenate the three examples in six different ways: (1) Examples 1 + 2 + 3, (2) Examples 1 + 3 + 2, (3) Examples 2 + 1 + 3, (4) Examples 2 + 3 + 1, (5) Examples 3 + 1 + 2, and (6) Examples 3 + 2 + 1. As we average over the outcomes of each of these paraphrases, we ensure that the final generated outcome is not biased due to the ordering of the examples.

[1] https://huggingface.co/TheBloke/vicuna-13B-v1.5-GPTQ
[2] https://github.com/turboderp/exllama

| Element Type | Text |
|---|---|
| Instruction | We are scoring reviews based on their politeness on a scale of 1-5, where 1 is highly impolite and 5 is highly polite. |
| Example 1 | Review: Please say in the main text that details in terms of architecture and so on are given in the appendix.<br>Politeness score: 5 |
| Example 2 | Review: From this perspective, the presented comparison seems quite inadequate.<br>Politeness score: 3 |
| Example 3 | Review: Please elaborate on how you end up in this mess.<br>Politeness score: 1 |
| Query | Review: `[post]`<br>Politeness score: |

Table F.1: Our prompt to query the politeness score of a review consists of the following: (1) an overall instruction to describe this politeness scoring task, (2) three examples for LLM to understand this task better, and (3) query of the politeness score given a new review.

## F.2 Mann-Whitney test details

### F.2.1 Mann-Whitney test for survey responses

To test for difference in the self-reported experiences of reviewers discussed in Section 7.4.4, we utilize the Mann-Whitney $U$ test. For each survey question, given the responses collected, we want to test the hypothesis described next. Let $X^a$ indicate the random variable sampled as a response from a reviewer in the non-anonymous condition, and $X^{\tilde{a}}$ indicate the random variable sampled as a response from a reviewer in the anonymous condition. We define ordinal comparisons between the responses as

Strongly agree $>$ Somewhat agree $>$ Neither agree nor disagree $>$ Somewhat disagree $>$ Strongly disagree.

Under this ordering, for each survey question, we want to test the hypothesis:

$$\begin{aligned} H_0 &: \mathbb{P}(X^a > X^{\tilde{a}}) = \mathbb{P}(X^a < X^{\tilde{a}}) \\ H_1 &: \mathbb{P}(X^a > X^{\tilde{a}}) \neq \mathbb{P}(X^a < X^{\tilde{a}}). \end{aligned} \tag{F.1}$$

Let us denote the response data available for a question in the anonymous and the anon-anonymous condition as: $\{x_1^a, \cdots, x_{n_a}^a\}$ and $\{x_1^{\tilde{a}}, \cdots, x_{n_{\tilde{a}}}^{\tilde{a}}\}$ respectively, where $n_a, n_{\tilde{a}}$ indicate the number of respondents in the corresponding condition, and $x_i^a$ and $x_i^{\tilde{a}}$ indicate the $i^{\text{th}}$ responses therein. With this notation, we lay out the step-wise procedure for conducting the Mann-Whitney $U$ test for (F.1).

- Step 1: Compute the Mann-Whitney $U$-statistic. This statistic measures the frequency of a response in one condition being higher than a response in the other condition with ties counted as half. This is encapsulated in the following function

$$S(a,b) = \begin{cases} 1 \text{ if } a > b, \\ 0.5 \text{ if } a = b, \\ 0 \text{ if } a < b. \end{cases} \tag{F.2}$$

Note that this is a non-symmetric function, since $S(a,b) = 1 - S(b,a)$. The Mann-Whitney $U$ statistic is defined symmetrically as

$$U = \min \left\{ \sum_{i=1}^{n_a} \sum_{j=1}^{n_{\widetilde{a}}} S(x_i^a, x_j^{\widetilde{a}}), \sum_{i=1}^{n_a} \sum_{j=1}^{n_{\widetilde{a}}} S(x_j^{\widetilde{a}}, x_i^a) \right\}. \tag{F.3}$$

We compute the normalized $U$-statistic as $\dfrac{U}{n_{\widetilde{a}} n_a}$.

- Step 2: Compute the p-value using a permutation test.

Let $\gamma$ indicate the number of iterations. We set $\gamma = 100000$. For each iteration, we randomly shuffle the data across the two groups $\{x_1^a, \cdots, x_{n_a}^a, x_1^{\widetilde{a}}, \cdots, x_{n_{\widetilde{a}}}^{\widetilde{a}}\}$ such that we have $n_a$ and $n_{\widetilde{a}}$ samples respectively in the hidden and the shown group. Then for $k \in \{1, 2, \ldots, \gamma\}$, compute $U_k$ based on shuffled data according to (F.3). Then, we compute the two-tailed p-value as

$$p = \frac{2}{\gamma} \min \left\{ \sum_{k=1}^{\gamma} \mathbb{I}(U_k > U) \quad , \quad \sum_{k=1}^{\gamma} \mathbb{I}(U_k < U) \right\} \tag{F.4}$$

### F.2.2 Mann-Whitney test for politeness scores

To test for difference in politeness of discussion posts across the two conditions, we consider posts written by senior reviewers separately and junior reviewers separately to account for selection bias. Let $P^a \in \{p_1^a, p_2^a, \ldots\}$ denote the set of politeness scores assigned to anonymous discussion posts by senior reviewers, and similarly $P^{\widetilde{a}} \in \{p_1^{\widetilde{a}}, p_2^{\widetilde{a}}, \ldots\}$ denotes scores for non-anonymous discussions. The set of scores assigned to posts by junior reviewers are denoted by $Q^a \in \{q_1^a, q_2^a, \ldots\}$ and $Q^{\widetilde{a}} \in \{q_1^{\widetilde{a}}, q_2^{\widetilde{a}}, \ldots\}$ for the anonymous and non-anonymous condition respectively. To account for difference in behaviour across seniority groups, we define the normalised $U$-statistic as

$$U_{PQ} = \frac{\left( \sum_{p^a \in P^a} \sum_{p^{\widetilde{a}} \in P^{\widetilde{a}}} \mathbb{I}\left(p^a > p^{\widetilde{a}}\right) + 0.5\mathbb{I}\left(p^a = p^{\widetilde{a}}\right) \right) + \sum_{q^a \in Q^a} \sum_{q^{\widetilde{a}} \in Q^{\widetilde{a}}} \left( \mathbb{I}\left(q^a > q^{\widetilde{a}}\right) + 0.5\mathbb{I}\left(q^a = q^{\widetilde{a}}\right) \right)}{|P^a||P^{\widetilde{a}}| + |Q^a||Q^{\widetilde{a}}|}, \tag{F.5}$$

where $\mathbb{I}(\cdot)$ denotes the indicator function. To derive the significance of the test, we conduct a permutation test as described in Step 2 in Section F.2.1 except when the data is shuffled in each iteration, the elements of $P^a$ are shuffled at random with elements of $P^{\widetilde{a}}$ and the elements of $Q^a$ are shuffled at random with $Q^{\widetilde{a}}$.

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