Learning When to Advise Human Decision Makers

Gali Noti* Yiling Chen†

September 28, 2022

Abstract

Artificial intelligence (AI) systems are increasingly used for providing advice to facilitate human decision making. While a large body of work has explored how AI systems can be optimized to produce accurate and fair advice and how algorithmic advice should be presented to human decision makers, in this work we ask a different basic question: When should algorithms provide advice? Motivated by limitations of the current practice of constantly providing algorithmic advice, we propose the design of AI systems that interact with the human user in a two-sided manner and provide advice only when it is likely to be beneficial to the human in making their decision. Our AI systems learn advising policies using past human decisions. Then, for new cases, the learned policies utilize input from the human to identify cases where algorithmic advice would be useful, as well as those where the human is better off deciding alone. We conduct a large-scale experiment to evaluate our approach by using data from the US criminal justice system on pretrial-release decisions. In our experiment, participants were asked to assess the risk of defendants to violate their release terms if released and were advised by different advising approaches. The results show that our interactive-advising approach manages to provide advice at times of need and to significantly improve human decision making compared to fixed, non-interactive advising approaches. Our approach has additional advantages in facilitating human learning, preserving complementary strengths of human decision makers, and leading to more positive responsiveness to the advice.

1 Introduction

Artificial intelligence (AI) is increasingly being used to support human decision making in high-stake settings in which the human operator, rather than the AI algorithm, needs to make the final decision. For example, in the criminal justice system, algorithmic risk assessments are being used to assist judges in making pretrial-release decisions and at sentencing and parole [14, 28, 17, 13]; in healthcare, AI algorithms are being used to assist physicians to assess patients' risk factors and to target health inspections and treatments [48, 18, 56, 35]; and in human services, AI algorithms are being used to predict which children are at risk of abuse or neglect, in order to assist decisions made by child-protection staff [58, 12].

In such systems, decisions are often based on assessments of risk, and machine-learning algorithms' abilities to excel at prediction tasks [46, 15, 25, 50, 47] are leveraged to provide predictions as advice to human decision makers [34]. For example, the decision that judges make on whether it is safe to release a defendant until his trial, is based on their assessment of how likely this defendant is, if released, to violate his release terms, i.e., to commit another crime until his trial or to fail to appear in court for his trial. For making such risk predictions, judges in the US are assisted by a "risk score" predicted for the defendant by a machine-learning algorithm [14, 28, 17].

Research on such AI-assisted decision making has mostly addressed two questions. The first is what advice should AI systems provide? The line of research that addresses this question places

*Harvard University and the Hebrew University of Jerusalem. Email: galinoti@seas.harvard.edu
†Harvard University. Email: yiling@seas.harvard.edu

emphasis on the machine-learning algorithms and focuses on optimizing and evaluating their success in comparison to human predictions, based on statistical metrics related to considerations such as prediction accuracy and fairness [33, 3, 27, 11]. The implicit expectation is that better algorithmic advice will lead to better human decisions.

The second question is how to present algorithmic advice to human decision makers? This question has been addressed by a recent line of work that emphasizes the role of the human as the one who eventually makes the actual decision. Instead of evaluating the algorithmic performance in isolation, these works concentrate on studying the effect of the algorithmic input on the decisions that humans make [21, 22, 1, 57, 38, 64, 6, 63] and hence term the perspective "AI-in-the-loop human decision making"[21]. These studies typically show—both with human experts such as judges or clinicians and with non-experts in experimental settings—that providing the algorithmic assessment indeed significantly improves human decision makers' prediction performance, and that different ways of providing the algorithmic input to human decision makers, as well as different algorithmic accuracy or error patterns, can have a significant impact on their decisions.

Situated in the framework of AI-in-the-loop human decision making, this work aims to answer a different important question: *when should algorithms provide advice?* The current practice in applications and in prior studies, is that algorithms provide advice to the human decision maker in every prediction problem. We explore whether AI systems can be trained to automatically identify the cases where advice is most useful, and those where the human decision maker is better off deciding without any algorithmic input, and whether such an approach that provides the algorithmic advice only when it is needed indeed manages to assist humans in improving their decisions.

Our approach is motivated by several observations from prior work and current practice. First, in prior studies on AI-assisted human decision making, the AI component is completely oblivious of the human decision maker: the human always receives advice from an algorithm, but, importantly, the algorithm is not aware of its human counterpart and whether its advice may actually be helpful to him. This is despite the fact that human decision makers have their own strengths and sometimes reach better decisions on their own, without the algorithmic input, and the computational methods have their own limitations and can have errors and biases (as was studied in recent literature on human-AI complementary performance [24, 60, 43, 31, 7, 55]), and so algorithmic advice may not always be helpful. For example, in a recent experiment that studied human prediction in the pretrial-release decision setting [22], the most accurate human prediction performance was achieved in an "Update" treatment, in which the human decision makers first made a risk prediction on their own and only then observed the algorithmic prediction and were allowed to update their prediction if they wished. However, in this dataset we found that in 66% of the predictions, the human's initial prediction (before observing the algorithmic input) was already equal to or better than the algorithm's prediction. Moreover, in 36% of the predictions, humans' initial prediction was strictly more accurate than the algorithm's, and after showing them the algorithmic prediction their prediction performance deteriorated 32% of these times.

An additional important point that arises when a human decision maker is assisted by an (inevitably) imperfect AI system, is that the human is de-facto expected to monitor the algorithm, i.e., to identify when the algorithm is wrong so as to override its prediction [20]. However, there is a large body of empirical evidence showing that such monitoring is a challenging task for humans: recent studies demonstrate that people do poorly in judging the quality of algorithmic predictions and determining when to override those predictions, and that these judgments are often incorrect and biased [21, 22, 23, 57, 32, 62, 7, 59], even when presented with clear interpretable models [52]. This suggests to consider alternative designs of decision pipelines in which the monitoring task, which is less suitable for human decision makers, is transferred to the AI.

Moreover, even if the algorithm were perfect, it is not clear whether the constant advising approach used in prior work is the optimal way to interact with human decision makers and to inform them so as to improve their decisions. Specifically, it may be that providing the advice

in every prediction will result in advice discounting or even disregard in the decision maker's judgment. Such behaviors have been demonstrated in other settings of users' interactions with technology [30, 2], and are related to the study of habituation [53], but have not been studied in behavioral literature on advice utilization.

Finally, in the experiment of [22] it is intriguing to see that while humans made significantly better predictions when they were assisted by the algorithm compared to making predictions without any assistance, their performance was still far worse than that of the algorithm alone. This is despite the fact that the human decision makers constantly received the algorithmic prediction, and, in principle, could just have adopted its predictions and reached the algorithmic performance. This observation that a human assisted by an algorithm is still inferior to the algorithm alone is in fact typical in AI-assisted human decision making settings (e.g., [38, 37, 29]) and suggests that there is room to improve and extract more value from the interaction between the human and the AI.

In this work we propose to replace the constant advising approach with an algorithmic assistant that interacts with the human decision maker and takes an active part in the decision making process, aiming to improve the human's decisions. Specifically, our algorithmic assistant applies a *learned advising policy* that depends on input from the human decision maker and provides advice only when it is likely to improve his decision. Thus, in this human-AI team, information does not only flow from the algorithm to the human as in prior work, but instead there is a *two-sided interaction*: the algorithmic assistant's advice depends on the human's input, and the human's final decision, in turn, depends on the input he receives from his algorithmic assistant.

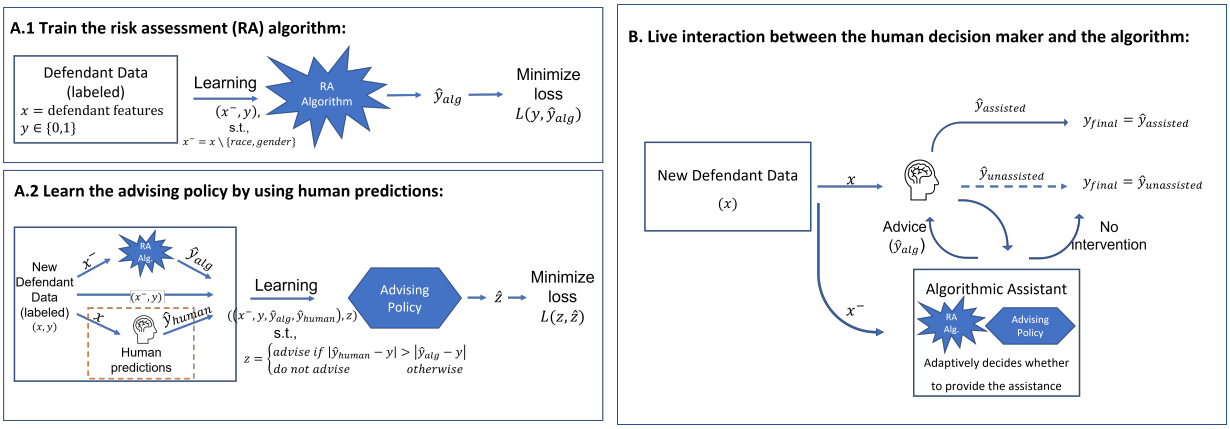
We consider a simple form of these two-sided interactions, in which the input from the human to the algorithmic assistant is the human's (initial, unassisted) risk prediction, and the algorithmic assistant's advising policy determines whether or not to advise the human, providing advice only when it identifies that its advice is likely to improve the human's prediction. Thus, our human-AI collaboration is designed such that the human decision maker is operating on his own and makes predictions, while the learned advising policy is there to optimize the added value that the human can extract from his interaction with the AI advising system.

Figure 1 presents a diagram of the advising-policy approach that we take for AI-assisted decision making, which we demonstrate in the pretrial-release decision setting. We first learn an advising policy by using predictions that humans made in previous experiments, about how likely a criminal defendant is to violate his release terms if released. Then, we conduct a large-scale experiment on Mechanical Turk to evaluate human prediction performance when they interact with our learned advising policy. Our experimental results show that an advising policy is indeed learnable from data and that humans assisted by this learned advising policy make significantly more accurate predictions than human decision makers assisted by the constant advising policy, and achieve comparable performance to the risk-assessment algorithm. We further explore how our interactive-advising approach affects human learning, human decision makers' responsiveness to algorithmic advice, and the performance of human decision makers with respect to defendants' racial groups.

2 Our Interactive Algorithmic Advising Approach

The decision pipeline that we consider is illustrated in Figure 1b. It is composed of two components: a human decision maker and an algorithmic assistant. The algorithmic assistant is in turn composed of a risk-assessment algorithm and an advising policy.[1] When a new criminal defendant arrives, the

[1]Note that in principle, an algorithmic assistant could be implemented as a single algorithmic component trained end-to-end. However, such an approach would not allow to isolate the contribution of the advising policy to human prediction performance. Furthermore, an important advantage of decoupling the risk-assessment algorithm from the advising policy is in reliability: such a separation constrains the risk-assessment algorithm to be trained only to optimize the quality of its risk assessments, rather than providing biased assessments that aim to affect the human decision maker, as in strategic settings where an algorithm designs the advice to serve its goals [5, 4].



(a) Training the algorithmic component

(b) Live interaction

Figure 1: An interactive advising approach for AI-assisted human decision making.

human observes a description of the defendant and predicts the defendant's likelihood to violate his release terms if released, $\hat{y}_{unassisted}$. Then, given the description of the defendant (excluding race and gender to match common practice among risk-assessment developers and previous experiments [40, 42, 13, 21, 22]) and the prediction that the human made, the algorithmic assistant generates an algorithmic risk assessment, $\hat{y}_{alg}$, and provides the assessment to the human according to the advising policy. The advising policy that we wish to learn aims to provide the algorithmic risk assessment to the human only when it is likely to improve the human's prediction. In cases in which the advice is not provided, the final prediction $\hat{y}_{final}$ is set to the human's unassisted prediction $\hat{y}_{unassisted}$, while in cases that the advice is provided, the human observes the advice and can update his prediction if he wishes (to any prediction value), and the final prediction is then set to the updated value $\hat{y}_{assisted}$.

Our main focus is on learning such advising policies and evaluating their impact on the predictions made by human decision makers. As the risk-assessment component of the algorithmic assistant, we use the model of [22] that was trained on 47,141 defendant cases from a dataset collected by the U.S. Department of Justice [51] (top diagram in Figure 1a), all of which were released before trial and we thus know the ground-truth information about their pretrial-release outcome. That is, we know for each case whether eventually the defendant violated his release terms. Among the defendants in this dataset, 29.8% violated their pretrial-release terms. The model gets as input a description of a criminal defendant and outputs a risk assessment $\hat{y}_{alg} \in [0, 1]$ that represents the algorithm's predicted likelihood that this defendant will violate his release terms if released. In [22] it is shown that this model achieves comparable performance to widely used risk-assessment tools like COMPAS [49, 39] and the Public Safety Assessment [16]. See Section A in the Appendix and [22] for more details on the dataset and the model.

For learning the advising policy, we train a machine-learning model on experimental data of human predictions from [21]. See the bottom diagram in Figure 1a. Given a defendant case, the algorithmic risk assessment, and the prediction that the human made, the policy determines whether or not to advise the human. In the training process, the label of each such prediction example is set to 1 (i.e., do advise) if the algorithm's prediction is more accurate than that made by the human, and to 0 (i.e., do not advise) otherwise. The training data consist of 6,250 predictions made by 250 human participants, for 500 defendant cases. Each participant was asked to predict for a series of 25 defendants, the defendants' risk to violate their release terms if released. In this dataset, in 33.31% of the predictions the algorithm's prediction was more accurate than the human's prediction. To better adapt to our target domain, which is a new experiment in which humans interact with a learned advising policy rather than predict independently from it as in our training data, we train our model on an augmented version of the dataset. For more details on the learning process, see *Materials and Methods*.

3 Results

We conduct an experiment on Mechanical Turk to evaluate the quality of human predictions when assisted by our learned advising policy. In the experiment, each participant was randomly assigned to one of the experimental treatments, and was asked to predict the risk for a series of 50 defendants (from 0% to 100%, in intervals of 10%), to violate their release terms if released, according to the decision pipeline described above.

Our experimental design compares human prediction performance in five experimental treatments. The first three treatments compare human performance when assisted by advising policies of different learning quality: **"Learned,"** in which humans were assisted by the learned advising policy described above; **"Random,"** in which the subset of defendant cases for which the human received the algorithmic advice was chosen at random, in the same frequency in which the learned advising policy provided advice on the training data; **"Omniscient,"** in which humans were assisted by an advising policy that showed the advice exactly in those cases where the algorithmic risk assessment was more accurate than their initial (unassisted) prediction, based on the ground truth of the defendant case (i.e., whether the defendant eventually violated his release terms). This provides an upper bound for performance improvement that may be achieved by improving the learning quality of our advising policy.

In addition, we ran a **"No Advice"** treatment in which humans made the predictions on their own without observing the algorithmic risk assessment, and the **"Update"** treatment from [22] in which humans first made the prediction on their own and then always observed the algorithmic prediction and were allowed to update their prediction if they wished. The prediction structure in this Update treatment led to the best human prediction performance in [22], consistently with findings in other recent studies (e.g., [10, 24]) and with prior behavioral research that suggest the importance of forming a pre-advice independent opinion [59, 9, 54].

For comparability with the experimental results of [22], we used in the experiment the same set of 300 defendant cases that they used, which were sampled from the heldout dataset of the risk-assessment algorithm's training process, and followed their experimental setup and procedure. 200 people or more participated in each of the experimental treatments, to a total of 1,096 participants and 54,800 predictions. For more details on the experimental setup and design, see *Materials and Methods*, and see Table 1 in the Appendix for demographic and general details on the experiment.

Next, we describe the main experimental results. See Table 2 in the Appendix for an overview of the results according to the main metrics. All p-values and confidence intervals are generated on distribution of performance at the participant level, unless otherwise stated.

3.1 Learning Performance

Our analysis starts by evaluating the extent to which our learned advising policy managed to generalize from the fixed training data to the new domain of our experiment, which includes new participants, new defendant cases, and importantly, live interaction between the advising policy and the human decision maker. The experimental results show that our learned advising policy managed to provide the advice in the correct times, i.e., when the algorithmic risk assessment was more accurate than the human's initial risk prediction, significantly more frequently than all other treatments (except, of course, from the Omniscient treatment, which by definition has perfect accuracy), obtaining accuracy of 74.1±0.9% (see *Materials and Methods* for definitions). This is compared with 58.4±1.5% accuracy in the Random treatment in which the advice is given at random times; with 42.0±2.1% accuracy in the Update treatment which can be thought of as an "always advising policy;" and with 52.5±2.0% accuracy in the No Advice treatment which can be thought of as a "never advising policy." In the Learned treatment, in 37.5% of the predictions the algorithmic risk assessment was more accurate than the human's initial risk prediction, and our learned advising policy provided the advice in 37.0% of the predictions, thus achieving calibrated advice frequency. For further details, see Section B.2 in the Appendix.

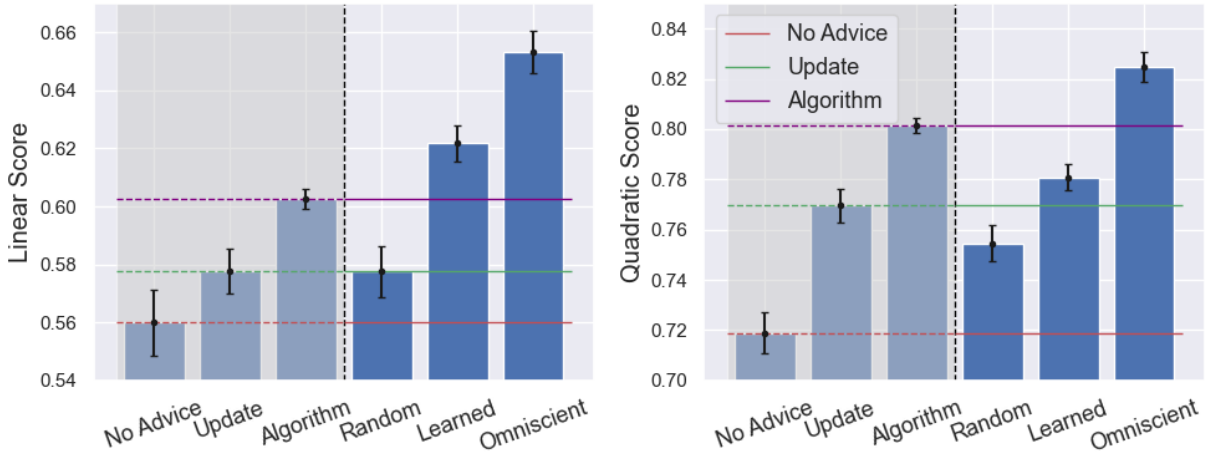


Figure 2: Participant performance in the different experimental treatments, and the algorithm's performance, according to both linear and quadratic scores. Error bars show 95% confidence intervals. The No Advice, Update, and algorithmic benchmarks are presented on the left of each figure, and for ease of comparison, their means continue as horizontal lines. The algorithm's performance is computed over its predictions for the cases given to participants in the Learned treatment.

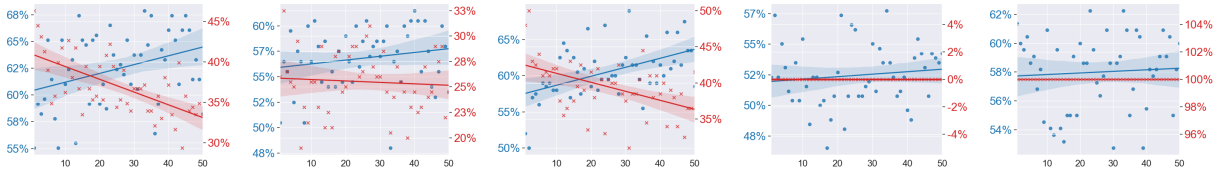
3.2 Impact on Human Prediction Performance

We turn to look at the actual impact of our learned advising policy on the quality of the final predictions of the human decision makers. For each risk prediction $\hat{y} \in \{0, 0.1, ..., 1\}$ with ground truth $y \in \{0, 1\}$ (0 for not violating the release terms or 1 otherwise), the prediction error is defined as $error = |y - \hat{y}|$. We evaluate the prediction performance primarily according to two measures that capture different error patterns: the linear score (i.e., $1 - error$) and the quadratic score (i.e., $1 - error^2$). Evaluation according to additional measures gives qualitatively similar results (see Section B.3 in the Appendix).

Figure 2 shows the prediction performance of the human participants in the experiment, and the algorithmic prediction performance, according to the linear score (left panel) and the quadratic score (right panel). Figure 6 in the Appendix shows the full performance distributions. According to both score measures, human predictions in the Learned treatment have a clear and statistically significant advantage over the No Advice, Random, and Update treatments, and specifically the ranking of performance, from best to worst, is: Omniscient, Learned, Update, Random, and No Advice. The advantage of the Learned treatment over the constant-advising Update treatment demonstrates the usefulness of our learned advising policy approach that considers input from the human decision maker, and provides advice that is focused only on those places where it is likely to be useful. The performance of the Random treatment shows that providing advice only in part of the predictions does not lead in itself to an improvement in the quality of human predictions, and that the learned advising approach is important to achieve this improvement. The large gap of the performance of Omniscient above all other treatments shows the potential for further improvement of human predictions by improving the learning quality of the advising policy (e.g., by utilizing more advanced computational methods or larger datasets).

A comparison with the algorithmic performance shows that according to the linear score human decision makers in the Learned treatment outperformed the algorithm, while according to the quadratic score the algorithm had better performance.[2] Thus, we conclude that the prediction performance of human decision makers when assisted by our learned advising policy was on par with the performance of the algorithm. Humans in the Omniscient treatment outperformed

[2]Note that the algorithm we use was trained to optimize quadratic score, and thus it could be expected that it will have an advantage according to this measure compared to other measures.



(a) Learned (b) Random (c) Omniscient (d) No Advice (e) Update

Figure 3: Human learning. In blue: the average frequency in which the human initial prediction was at least as accurate as the algorithm's prediction, as a function of the prediction period. In red: the average frequency in which the advice was provided as a function of the prediction period. The blue and red lines are the regression curves for the two measures. Only in the Learned and Omniscient treatments these frequencies are significantly correlated with prediction period, and this learning effect resembles a "training wheels" pattern: as the participants' initial predictions improve, the algorithmic advice is useful less often and the frequency in which it is provided decreases.

the algorithm, by a large gap, according to both measures, which again shows the potential for further gains from improving the learning quality. Reaching the algorithmic performance is a notable improvement compared to all prior advising methods in the human-AI collaboration in decision making setting that we consider that requires human agency, and in particular compared with the constant-advising Update treatment, in which human prediction performance is typically significantly inferior to that of the algorithm.

3.3 Interaction Between the Human and the Algorithm

Human Responsiveness to the Algorithmic Advice. We now look at the responses of the human decision makers in our experiment to the algorithmic advice in those cases in which the advice was given. We observe a clear pattern, which we term a "scarcity effect": as the advice is given less frequently, it tends to be followed by a stronger response on the human decision maker's part. Specifically, we look at the Random treatment, in which advice is given at random times and thus there is a natural variance in the frequencies in which participants received the advice. We find that the advice frequency is negatively correlated with the responsiveness of participants to the advice, as measured by the advice acceptance rate ($\rho = -0.23$, $p < 0.001$) and by the influence measure [61, 22] which quantifies the extent to which the human prediction after observing the advice changed from its initial value in the direction of the value of the advice ($\rho = -0.29$, $p < 0.0001$). See *Materials and Methods* for definitions and Figure 10 in the Appendix. Additionally, we observed that human responsiveness to the advice in the partial-advising treatments (Random, Learned, and Omniscient) was stronger, by a large gap, than the responsiveness in the Update treatment in which algorithmic risk assessment was provided for all predictions (Figure 9 in the Appendix). While the scarcity effect we observed in the Random treatment is sufficiently strong to explain such a gap (by extrapolating the correlation pattern to an advice frequency of 100%), this gap could also result from the algorithmic assistant framing, and thus our experimental design does not isolate the sources for the gap in human responses between these treatments. See Section B.4 in the Appendix for more details.

Indication of Human Learning. In order to see whether our human decision makers managed to learn and improve over the course of the experiment, we analyze the quality of participants' initial (i.e., unassisted) prediction in comparison with the algorithm's prediction (which is the only type of feedback that the participants received in the experiment). Note that in all treatments participants had the same information when making their initial predictions, and so differences between treatments in the quality of these predictions are a result of some learning process from the interaction with the different advising policies.

The results show that in all experimental treatments participants managed to learn and im-

prove their initial predictions relative to the No Advice benchmark, and suggest that the informed advising policies, namely Learned and Omniscient, better facilitate human learning. Specifically, our first indication of human learning is that the overall frequency in which the human initial prediction was at least as accurate as that of the algorithm, was significantly higher than the No Advice benchmark in all experimental treatments, and was the highest in the Learned and Omniscient treatments (see Figure 11a in the Appendix). Second, looking over time, we find that this frequency significantly increased with prediction period only in the Learned and Omniscient treatments (see Figure 11b and analysis in the Appendix). While these observations show a clear learning effect with respect to the algorithmic feedback that participants received, we find that this effect was only weakly translated to an improvement in the quality of the initial predictions with respect to the ground truth. See Section B.5 in the Appendix for further details.

Figures 3a and 3c show the learning effect in the Learned and Omniscient treatments alongside the response of the advising policies to this effect, and demonstrate the advantage of our two-sided interaction approach: as the human initial prediction improves compared to the algorithmic prediction, the learned advising policy identifies more cases in which the algorithmic advice is not needed, and as a consequence provides significantly less advice. We note that the better learning observed in the Learned and Omniscient treatments may result from a combination of several effects, which their impact on human learning is not isolated in our experimental design; e.g., the higher informativeness of the given advice and the higher responsiveness to the advice in these treatments. Further studying the factors that facilitate human learning is a broad and interesting direction for future work.

Tension between imitating the algorithm and preserving complementary human strengths. The results so far show that participants managed to learn from the algorithmic feedback and improve their initial predictions, and that this improvement was more substantial in the Learned and Omniscient treatments than in the Random and Update treatments. Now we turn to look directly at how the distributions of initial predictions differ between the different treatments. We demonstrate that this learning phenomenon raises a tension between the extent to which humans learn from the algorithmic advice and their ability to preserve their own relative prediction strengths.

A comparison of the distribution of human initial predictions and the algorithmic predictions shows that, as expected, in the No Advice treatment, in which human predictions are completely independent of the algorithmic predictions, the distance between these two distributions is the largest (as measured by KL divergence [36], see Table 2 in the Appendix). The distribution of initial predictions in the Update treatment was the closest to the algorithmic predictions, and the treatments in which the advice was provided only in part of the predictions had intermediate KL divergence values. This suggests that in the Update treatment, in which participants constantly observed the algorithmic risk assessment, the participants learned to predict similar values to the feedback that they observed, while in the partial advice settings this imitation effect was moderated.

A closer look suggests that the partial advice has an advantage in preserving human prediction behavior that is complementary to the predictions of the algorithm. A notable example is that in the always-advising Update treatment participants learned to almost never predict a certain low-risk value of zero – a value that was never predicted by the algorithm,[3] but was predicted by human participants in the No Advice treatment in 10.5% of all predictions. This is despite the fact that in the subset of instances in which humans predicted zero risk, their predictions were significantly more accurate than their average prediction performance. By contrast, in the Learned treatment participants preserved this relative strength and predicted a risk of zero for 11.0% of the predictions, and similarly to the No Advice treatment, with a higher accuracy in those predictions relative to their average performance. See more details in Section B.6 in the Appendix.

[3]Recall that the algorithm was optimized for minimizing quadratic error, and so avoiding predictions of extreme values is a typical outcome of such an optimization process.

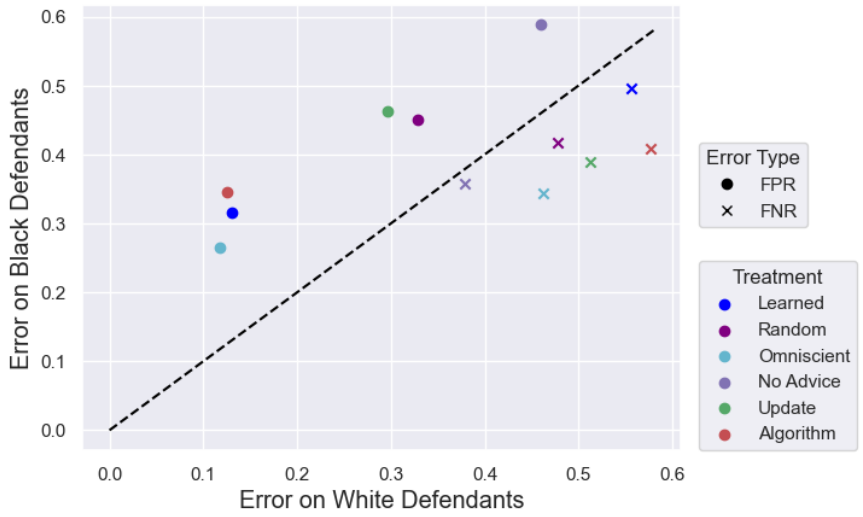


Figure 4: False-positive rates (FPR) and false-negative rates (FNR) for black and white defendants in each experimental treatment and for the algorithm's predictions.

3.4 Fairness

We further examine how human decision makers assisted by our advising policies perform with respect to defendants' racial groups. We start by comparing the false-positive rates (FPR) and false-negative rates (FNR) in our experiment for black and white defendants (see *Materials and Methods* for the definitions). First, the results show that the FPR in the Learned treatment, for both black and white defendants, is substantially lower than the FPR in the Update, Random, and No Advice treatments, but at the cost of higher FNR (Figure 4 and Section B.7 in the Appendix). Second, in terms of FPR and FNR disparities between black and white defendants, we find that the learned advising policy is comparable to the Update treatment, and has a significant advantage compared with the risk-assessment algorithm. For more details, see Section B.7 in the Appendix.

Figure 4 shows that according to both FPR and FNR, all treatments have an error that is biased to the same direction which gives harsher predictions for black defendants. We quantify this discrimination by defining "classification disparity" (see the formal definition in *Materials and Methods*), which weighs utility gaps between black and white defendants that are in favor of white defendants. We find that, interestingly, the algorithm has the highest classification disparity (this is despite the fact that the algorithm did not directly observe the defendant's race whereas the human participants did), the No Advice and Random treatments have the least classification disparity, and Omniscient, Learned, and Update have intermediate disparity levels with an advantage to Omniscient and Learned (Figure 13c in the Appendix). When considering the accuracy-fairness tradeoff the results show that Learned and Omniscient Pareto dominate the Update treatment (Figure 14 in the Appendix). This tradeoff suggests that the use of informed advising policies in Learned and Omniscient allowed the human decision makers on the one hand to extract gains from the high performance of the algorithm, while on the other hand to moderate its racial disparity.

Finally, in Section B.7 in the Appendix we analyze interaction disparity according to the two measures studied in [22], to evaluate whether participants responded to the risk assessment in a racially biased manner. In [22], every experimental treatment exhibited disparate interactions, including the Update treatment (which is identical to the Update treatment in our experiment) that yielded the smallest disparity. Our experiment replicates the results for the first "influence disparity" measure for the Update treatment, but this influence disparity was eliminated in the Learned and Omniscient treatments. The second "deviation disparity" observed in [22] was not replicated in our experiment, including in our Update treatment. See Section B.7 in the Appendix for more details.

4 Discussion

What is the best way to use algorithms to advise human decision makers? Existing methods constantly provide advice and focus on optimizing the algorithmic advice itself or its presentation or explanation to the human user. Motivated by limitations of the constant-advising approach that frequently advises the users in redundant or even harmful times, and by the complementary abilities of humans and algorithms that have been demonstrated in many settings, this paper proposes a responsive advising approach, in which the algorithm interacts with the human user and provides advice only when it is most beneficial for the human in making his decision.

We analyzed over fifty thousand human predictions in five experimental treatments that compared our new interactive-advising approach to the constant-advising approach and to other benchmarks. Our analysis shows that people assisted by interactive advising policies succeeded in making predictions that are more accurate, more fair, and better preserve human relative strengths, compared with people who constantly received the algorithmic advice. Also, human predictions when assisted by our advising policy achieved comparable performance to the algorithm's predictions in terms of accuracy, which, as discussed, is a significant improvement over prior methods, and had a significant advantage over the algorithm in terms of fairness measures. Importantly, we showed that such an advising policy that identifies when (and when not) to provide advice to the human user, based on input from the user, can be automatically learned from existing data.

One basic explanation for the advantage of our approach in terms of accuracy, is that our learned advising policies managed to utilize, for every given prediction problem, both the inputs from the algorithmic risk assessment and the input from the human user. This resulted in providing the advice in more informative times, and specifically, providing the advice when the algorithmic assessment was more accurate than the human initial prediction and refraining from providing misleading advice. Notably, in our implementation, the input from the user was composed of solely the human's initial prediction. The results show that this single additional bit of information that the AI system received already enabled this significant advantage. Future work will determine whether more complex inputs from the human users can further improve the quality of the advising policies and their usefulness for the users (e.g., by using active queries to the users, individualized analyses of their historical behavior, or signals that indicate their levels of confidence or engagement).

Aside from the direct impact on performance, our analysis raises two concerns about the longer-term impact on human decisions from constantly receiving algorithmic inputs. First, in the treatment where humans received algorithmic advice all the time, this advice was followed by a weak response, and importantly humans often failed to identify those cases where this advice was especially useful for them. By contrast, in the treatments where advice was given only in selected times, this advice was followed by higher responsiveness on the side of the human decision makers. Indeed, in a within-treatment analysis in the Random treatment, we find a clear connection between the frequency in which the advice is provided and human responsiveness to the advice, which we term the "scarcity effect": When advice is given less frequently, it tends to be followed by stronger responses. We conjecture that observing advice more frequently leads to habituation in human responses, while scarce advice are perceived as more valuable, however further research is needed in order to explain the behavioral source of this effect. More broadly, our study suggests the importance of studying the effect of partial or conditional advising on advice utilization, which in contrast to various other factors (see, e.g., review in [9]) has not yet received attention in behavioral literature.

Second, our analysis of the initial predictions given by the participants in the experiment (i.e., before observing the algorithmic risk assessment), shows that people who constantly observe the algorithmic advice learn to imitate the algorithm's past predictions. Arguably, this is instead of focusing on making their own judgments for the problems at hand. By contrast, in the interactive advising treatments, in which advice was provided in only about one third of the times, this imitation effect was moderated, and the distributions of human-predicted risk assessments pre-

served features that were unique to human judgments (and not to the algorithm), which almost completely disappeared in the constant-advising treatment. This empirical observation of the imitation effect raises a concern, which may in fact be inherent to any algorithmic advising setting: on the one hand algorithmic advice assists humans to improve their decisions, but on the other hand, through repeated exposure to the algorithm, human decision makers may also internalize its biases and weaknesses into their own judgments. Our results suggest that the advising-policy approach manages to balance this tradeoff to a good extent.

One limitation of the present study is that the findings are based on predictions made by Mechanical Turk workers in controlled experimental settings, rather than on decisions made in practice by real human experts like judges or clinicians. While controlled experiments with lay decision makers are useful in isolating and suggesting human behavioral tendencies that are then identified in practice [26, 8], the effects in the "real world" may differ from those in experimental settings due to the experimental abstracted context and the decision makers' domain knowledge and levels of expertise [57, 64]. Thus, continued research is important in order to study the extent in which the findings generalize to human-algorithm interactions in practice, and specifically to test the usefulness of our interactive algorithmic-advising approach in real AI-assisted decision making scenarios.

Another potential limitation worth mentioning is that the improvement achieved by using the learned advising policy comes at some cost of a more complex design in comparison to the simple constant-advising approach, as can be seen in Figure 1. There are two main practical aspects of complexity to consider: (i) implementation of the advising pipeline; (ii) data collection for learning the advising policy. However, neither of these points should pose a substantial limitation for implementation in AI advising platforms. Specifically, while our approach may require a shift of perspective by the platform designer, it is general and straightforward to implement in different domains, and in particular can be easily adjusted for requirements of specific platforms (e.g., by adding special conditions where an advice must (or must not) be given, or by allowing users to also manually query the algorithm). The kind of data that is required for learning advising policies is the record of past decisions that users made in the platform and the statistics of their outcomes, both of which are data that many platforms already collect.

Algorithmic advising systems are becoming increasingly prevalent in situations in which human judgment is important and cannot be replaced by an algorithm. Such systems provide advice to human decision makers in high-stake domains ranging from criminal justice to finance and healthcare, as well as in day-to-day applications such as personal assistants and recommendation systems. The way in which we choose to design such algorithmic advising tools shape our lives and may have broad implications to society. The present study points to the importance of asking *when* algorithms should provide advice. The findings show that an interactive approach that considers human input and provides advice that is *focused* on those places where it is most needed can better assist humans in making their decisions. Future work will study how to best apply this approach in current AI-assisted decision making systems, aiming to create better human-AI collaboration that will efficiently harness AI strengths to assist humans in making better decisions.

Materials and Methods

Additional details on learning the advising policy

We learn the advising policy by using data from the experiment of [21]. The data consist of 6,250 predictions made by 250 human participants on Mechanical Turk, for 500 defendant cases, in the control treatment in which the participants did not observe the algorithmic risk assessment. To better adapt to our target domain, which is a new experiment in which humans interact with a learned advising policy rather than predict independently from it, we train our model on an augmented version of this dataset. We augment the dataset in two steps: First, we added simulated

defendant cases, such that each original defendant record was duplicated six times on all attributes except for the age attribute that was set to ± 3 years from the original age value (but still restricting the age to be above 18). This step augmented the dataset from 6250 predictions for 500 cases to 40,529 predictions for 3251 cases. This augmentation is based on the assumption, which we also verified in the data, that a slight variation in a defendant age would barely affect the prediction. Second, for each defendant case in the augmented set of cases, we simulated human predictions by sampling from a smoothed version of the empirical distribution of human predictions on this case. This step doubled the size of the dataset, from 40,529 to 81,058 predictions.

We trained a random forest binary classifier on these augmented data by using the *scikit-learn* python package. An optimization process of the model's hyperparameters to obtain the best accuracy in a 20-fold cross-validation on the training data gave the following hyperparameters: `n_estimators` = 400, `min_samples_splits` = 100, `min_samples_leaf` = 50, and `max_features` = 4. We also fitted the model's threshold, and use a value of 0.42, which calibrates the advice frequency on the training data. That is, by using a model's threshold of 0.42, the model's advice frequency on the training data matched the 33.3% advice frequency according to the ground truth.

Additional details on the experimental setup

This study was reviewed and approved by the Harvard University Institutional Review Board (IRB) and the National Archive of Criminal Justice Data (NACJD). We recruited participants on Amazon Mechanical Turk, restricting the participation to Mechanical Turk workers inside the United States who had an historical acceptance rate of at least 75%. All Mechanical Turk workers are at least 18 years old.

The experimental procedure is similar to that of [22]. Upon arrival, participants read a brief description of "what to expect" in the experiment, and were asked to sign a consent form. Then, they were randomly assigned to one of the five experimental treatments: Learned, Omniscient, Random, No Advice, or Update. The experiment started with a tutorial, followed by a short intro survey, the primary prediction task of predicting risk for a series of 50 criminal defendants, and an exit survey to obtain participants' reflection on the task. The series of 50 defendants was drawn at random for each participant, from the same sample of 300 defendant cases that was used in the experiment of [22]. The tutorial was visible at the bottom of the screen throughout the entire prediction task so that participants could look up background information and the definitions of key terms.

In the primary prediction task, participants were presented with descriptions of the 50 defendants, one by one. Each description included seven features: age, gender, race, offense type, number of prior arrests, number of prior convictions, and previous failure to appear. For example: "*Defendant #1 is a 35 year old black male. He was arrested for a property crime. The defendant has previously been arrested 10 times. The defendant has previously been released before trial, and has never failed to appear. He has previously been convicted 3 times.*" Then, they were asked to predict the defendant's risk on a scale from 0% to 100%, in intervals of 10%: "*How likely is this defendant to fail to appear in court for trial or get arrested before trial?*"

In the three algorithmic-assistant treatments—Learned, Omniscient, and Random—after making each prediction, the algorithmic assistant decided whether to advise the participant with the algorithmic risk assessment, as follows. In Learned, our learned advising policy determined whether or not to advise based on its prediction of how likely the algorithmic risk assessment is to be more accurate than the participant's prediction. In Omniscient, an advising policy that uses hindsight information on the defendant's true pretrial-release outcome decided to show the advice exactly in those cases where the algorithmic risk assessment was more accurate than the participant's prediction. In Random, an advising policy decided to show the advice with probability of 30%, which corresponds to the advice frequency of our learned advising policy on the training data, and did not provide advice when the algorithmic risk assessment and the participant's prediction were identical. The instructions informed the participants that the set of cases in which the algorithmic

assistant provides the advice is randomized. When the decision was not to advise, the participant continued directly to the next defendant case. When the decision was to advise, the participant was presented with a message such as: "*Your algorithmic assistant identifies that your current prediction is likely to have high error, and advises you to improve the prediction to 40%.*[4]" In Random, the message was: "*Your algorithmic assistant predicts that this person is 40% likely to fail to appear in court for trial or get arrested before trial.*" Then, the participant was asked to make his final prediction, by either accepting the algorithmic advice or editing his prediction. In the No Advice treatment there was no additional information regarding the risk assessment, and after making each prediction the participant continued directly to the next defendant case. The Update treatment was identical to that of [22].

Upon completion of the experimental trial, the participants received a base payment of \$2 plus a performance-dependent bonus of up to \$3. The average duration of an experimental trial was 21.6 minutes. The bonus was computed additively over the predictions. The reward for each prediction was determined according to a Brier score function: $score = 1-(prediction-outcome)^2$, normalized such that a perfect predictive accuracy on all 50 predictions would yield a total bonus of \$3. The average bonus in the experiment is \$2.31. The Brier score is a proper score function [19], and thus incentivizes the users to report their true risk estimates. In the tutorial we explained this truthfulness property, and also included a comprehension question about it to verify understanding.

The intro and exit surveys included a simple attention question, and we excluded from our analysis participants who failed to answer correctly both attention questions. Also, we included in our analysis only participants who completed the prediction task for the full series of 50 defendant cases, and allowed a single participation for each worker. In total, 1,096 workers are included in the analysis. See Table 1 in the Appendix for more details on the participants in each treatment.

Additional details on performance measures

The *accuracy* of an advising policy is defined as the percentage of predictions in which the advising policy correctly determined whether the algorithmic risk assessment is more accurate than the human initial risk prediction.

We measure human responsiveness to the algorithmic advice, in those cases where the advice was given, by two measures:

1. *Advice influence* [22]: for each prediction $\hat{y}^k_{unassisted}$ for which an advice $\hat{y}^k_{alg}$ was given, the influence is defined by $I^k = (\hat{y}^k_{assisted} - \hat{y}^k_{unassisted})/(\hat{y}^k_{alg} - \hat{y}^k_{unassisted})$. This measure quantifies the extent to which the human prediction after observing the advice changed from its initial value in the direction of the value of the advice. It is similar to the "weight of advice" measure [61]: when the final (assisted) prediction falls within the initial (unassisted) prediction and the advice, the influence reflects the weight that a participant assigns to the advice. Influence of 0 means that the participant ignored the advice, while an influence of 1 means that the participant adopted the advice completely. The influence values ranged in $[-6, 5]$, with 86.2% of the predictions in $[0, 1]$.

2. *Advice acceptance rate*: considering predictions where the initial (unassisted) prediction is different than the algorithmic risk assessment, the advice acceptance rate is the frequency in which the advice is exactly followed. I.e., $Pr(\hat{y}^k_{assisted} = \hat{y}^k_{alg} | \hat{y}^k_{unassisted} \neq \hat{y}^k_{alg}$ and $\hat{z} = 1)$.

The analysis with respect to defendants' racial groups is based on *false-positive rates* (FPR) and *false-negative rates* (FNR) for black and white defendants. For each racial group, the group FPR is the rate in which defendants from that group did not violate their release terms but were wrongly classified as high-risk defendants, and the group FNR is the rate in which defendants

[4] As in [22], because the participants predicted risk in increments of 10%, we rounded the algorithmic risk predictions to the nearest 10% when presenting them to participants and in the analysis of the results.

from the group violated their release terms but were wrongly classified as low-risk defendants. The decision threshold is the value that optimizes F-score [65], which was 0.3 for each treatment as well as for the algorithm, such that predictions above 0.3 are classified as high-risk decisions and otherwise are classified as low-risk decisions. We note that the same threshold is also obtained by taking the fraction of high-risk defendants, which is 0.326 in our dataset (and since risk predictions are in multiples of 0.1).

The *classification disparity* is defined for a treatment by: $Pr(Y=0)(FPR_{Black}-FPR_{White})+Pr(Y=1)(FNR_{White}-FNR_{Black})$. The classification disparity weighs the discrimination of black compared to white non-risky defendants (the first term), and the bias in favor of white compared to black risky defendants (the second term). See more details in Section B.7 in the Appendix.

Acknowledgments

This project is partially supported by U.S. National Science Foundation under grant No. IIS 2007887 and grant No. IIS 2147187. The project has also received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 740282).

References

[1] A. Albright. If you give a judge a risk score: evidence from kentucky bail decisions. *Harvard John M. Olin Fellow's Discussion Paper*, 85:16, 2019.

[2] B. Anderson, A. Vance, B. Kirwan, D. Eargle, and S. Howard. Users aren't (necessarily) lazy: Using neurois to explain habituation to security warnings. 2014.

[3] J. Angwin, J. Larson, S. Mattu, and L. Kirchner. Machine bias. *ProPublica, May*, 23(2016):139–159, 2016.

[4] A. Azaria, Y. Gal, S. Kraus, and C. V. Goldman. Strategic advice provision in repeated human-agent interactions. *Autonomous Agents and Multi-Agent Systems*, 30(1):4–29, 2016.

[5] A. Azaria, Z. Rabinovich, C. V. Goldman, and S. Kraus. Strategic information disclosure to people with multiple alternatives. *ACM Transactions on Intelligent Systems and Technology (TIST)*, 5(4):1–21, 2014.

[6] G. Bansal, B. Nushi, E. Kamar, W. S. Lasecki, D. S. Weld, and E. Horvitz. Beyond accuracy: The role of mental models in human-ai team performance. In *Proceedings of the AAAI Conference on Human Computation and Crowdsourcing*, volume 7, pages 2–11, 2019.

[7] G. Bansal, T. Wu, J. Zhou, R. Fok, B. Nushi, E. Kamar, M. T. Ribeiro, and D. Weld. Does the whole exceed its parts? the effect of ai explanations on complementary team performance. In *Proceedings of the 2021 CHI Conference on Human Factors in Computing Systems*, pages 1–16, 2021.

[8] N. C. Barberis. Thirty years of prospect theory in economics: A review and assessment. *Journal of Economic Perspectives*, 27(1):173–96, 2013.

[9] S. Bonaccio and R. S. Dalal. Advice taking and decision-making: An integrative literature review, and implications for the organizational sciences. *Organizational behavior and human decision processes*, 101(2):127–151, 2006.

[10] Z. Buçinca, M. B. Malaya, and K. Z. Gajos. To trust or to think: cognitive forcing functions can reduce overreliance on ai in ai-assisted decision-making. *Proceedings of the ACM on Human-Computer Interaction*, 5(CSCW1):1–21, 2021.

[11] A. Chouldechova. Fair prediction with disparate impact: A study of bias in recidivism prediction instruments. *Big data*, 5(2):153–163, 2017.

[12] A. Chouldechova, D. Benavides-Prado, O. Fialko, and R. Vaithianathan. A case study of algorithm-assisted decision making in child maltreatment hotline screening decisions. In *Conference on Fairness, Accountability and Transparency*, pages 134–148. PMLR, 2018.

[13] T. H. Cohen, C. T. Lowenkamp, and W. E. Hicks. Revalidating the federal pretrial risk assessment instrument (ptra): A research summary. *Fed. Probation*, 82:23, 2018.

[14] N. J. Courts. Annual report to the governor and the legislature, 2020.

[15] R. M. Dawes, D. Faust, and P. E. Meehl. Clinical versus actuarial judgment. *Science*, 243(4899):1668–1674, 1989.

[16] S. L. Desmarais, K. L. Johnson, and J. P. Singh. Performance of recidivism risk assessment instruments in us correctional settings. *Psychological services*, 13(3):206, 2016.

[17] equivant. Practitioner's guide to compas core. *equivant*, 2019.

[18] C. Garcia-Vidal, G. Sanjuan, P. Puerta-Alcalde, E. Moreno-García, and A. Soriano. Artificial intelligence to support clinical decision-making processes. *EBioMedicine*, 46:27–29, 2019.

[19] T. Gneiting and A. E. Raftery. Strictly proper scoring rules, prediction, and estimation. *Journal of the American statistical Association*, 102(477):359–378, 2007.

[20] B. Green. The flaws of policies requiring human oversight of government algorithms. *Available at SSRN*, 2021.

[21] B. Green and Y. Chen. Disparate interactions: An algorithm-in-the-loop analysis of fairness in risk assessments. In *Proceedings of the conference on fairness, accountability, and transparency*, pages 90–99, 2019.

[22] B. Green and Y. Chen. The principles and limits of algorithm-in-the-loop decision making. *Proceedings of the ACM on Human-Computer Interaction*, 3(CSCW):1–24, 2019.

[23] N. Grgić-Hlača, C. Engel, and K. P. Gummadi. Human decision making with machine assistance: An experiment on bailing and jailing. *Proceedings of the ACM on Human-Computer Interaction*, 3(CSCW):1–25, 2019.

[24] M. Groh, Z. Epstein, C. Firestone, and R. Picard. Deepfake detection by human crowds, machines, and machine-informed crowds. *Proceedings of the National Academy of Sciences*, 119(1), 2022.

[25] W. M. Grove, D. H. Zald, B. S. Lebow, B. E. Snitz, and C. Nelson. Clinical versus mechanical prediction: a meta-analysis. *Psychological assessment*, 12(1):19, 2000.

[26] C. Guthrie, J. J. Rachlinski, and A. J. Wistrich. Inside the judicial mind. *Cornell L. Rev.*, 86:777, 2000.

[27] H. A. Haenssle, C. Fink, R. Schneiderbauer, F. Toberer, T. Buhl, A. Blum, A. Kalloo, A. B. H. Hassen, L. Thomas, A. Enk, et al. Man against machine: diagnostic performance of a deep learning convolutional neural network for dermoscopic melanoma recognition in comparison to 58 dermatologists. *Annals of Oncology*, 29(8):1836–1842, 2018.

[28] P. J. Institute. Scan of pretrial practices. *Pretrial Justice Institute*, 2019.

[29] M. Jacobs, M. F. Pradier, T. H. McCoy, R. H. Perlis, F. Doshi-Velez, and K. Z. Gajos. How machine-learning recommendations influence clinician treatment selections: the example of antidepressant selection. *Translational psychiatry*, 11(1):1–9, 2021.

[30] M. Kalsher and K. Williams. Behavioral compliance: Theory, methodology, and results.(chap. 23) in ms wogalter (ed.) handbook of warnings (pp. 313-329), 2006.

[31] E. Kamar, S. Hacker, and E. Horvitz. Combining human and machine intelligence in large-scale crowdsourcing. In *AAMAS*, volume 12, pages 467–474, 2012.

[32] A. Kiani, B. Uyumazturk, P. Rajpurkar, A. Wang, R. Gao, E. Jones, Y. Yu, C. P. Langlotz, R. L. Ball, T. J. Montine, et al. Impact of a deep learning assistant on the histopathologic classification of liver cancer. *NPJ digital medicine*, 3(1):1–8, 2020.

[33] J. Kleinberg, H. Lakkaraju, J. Leskovec, J. Ludwig, and S. Mullainathan. Human Decisions and Machine Predictions*. *The Quarterly Journal of Economics*, 133(1):237–293, 08 2017.

[34] J. Kleinberg, J. Ludwig, S. Mullainathan, and Z. Obermeyer. Prediction policy problems. *American Economic Review*, 105(5):491–95, 2015.

[35] I. Kononenko. Machine learning for medical diagnosis: history, state of the art and perspective. *Artificial Intelligence in medicine*, 23(1):89–109, 2001.

[36] S. Kullback. *Information theory and statistics*. Courier Corporation, 1997.

[37] V. Lai, H. Liu, and C. Tan. " why is' chicago'deceptive?" towards building model-driven tutorials for humans. In *Proceedings of the 2020 CHI Conference on Human Factors in Computing Systems*, pages 1–13, 2020.

[38] V. Lai and C. Tan. On human predictions with explanations and predictions of machine learning models: A case study on deception detection. In *Proceedings of the conference on fairness, accountability, and transparency*, pages 29–38, 2019.

[39] J. Larson, S. Mattu, L. Kirchner, and J. Angwin. How we analyzed the compas recidivism algorithm. *ProPublica (5 2016)*, 9(1):3–3, 2016.

[40] Laura and J. A. Foundation. Public safety assessment: Risk factors and formula, 2016.

[41] H. Liu, V. Lai, and C. Tan. Understanding the effect of out-of-distribution examples and interactive explanations on human-ai decision making. *Proceedings of the ACM on Human-Computer Interaction*, 5(CSCW2):1–45, 2021.

[42] C. T. Lowenkamp. The development of an actuarial risk assessment instrument for us pretrial services. *Fed. Probation*, 73:33, 2009.

[43] D. Madras, T. Pitassi, and R. Zemel. Predict responsibly: improving fairness and accuracy by learning to defer. *Advances in Neural Information Processing Systems*, 31, 2018.

[44] C. Marx, F. Calmon, and B. Ustun. Predictive multiplicity in classification. In *International Conference on Machine Learning*, pages 6765–6774. PMLR, 2020.

[45] R. McGill, J. W. Tukey, and W. A. Larsen. Variations of box plots. *The american statistician*, 32(1):12–16, 1978.

[46] P. E. Meehl. Clinical versus statistical prediction: A theoretical analysis and a review of the evidence. 1954.

[47] S. Mullainathan and J. Spiess. Machine learning: an applied econometric approach. *Journal of Economic Perspectives*, 31(2):87–106, 2017.

[48] M. A. Musen, B. Middleton, and R. A. Greenes. Clinical decision-support systems. In *Biomedical informatics*, pages 795–840. Springer, 2021.

[49] I. Northpointe. Compas risk and need assessment system. *Northpointe, Inc.*, 2017.

[50] Z. Obermeyer and E. J. Emanuel. Predicting the future—big data, machine learning, and clinical medicine. *The New England journal of medicine*, 375(13):1216, 2016.

[51] U. S. D. o. J. O. o. J. P. B. of Justice Statistics. State court processing statistics, 1990-2009: Felony defendants in large urban counties, 2014.

[52] F. Poursabzi-Sangdeh, D. G. Goldstein, J. M. Hofman, J. W. Wortman Vaughan, and H. Wallach. Manipulating and measuring model interpretability. In *Proceedings of the 2021 CHI conference on human factors in computing systems*, pages 1–52, 2021.

[53] C. H. Rankin, T. Abrams, R. J. Barry, S. Bhatnagar, D. F. Clayton, J. Colombo, G. Coppola, M. A. Geyer, D. L. Glanzman, S. Marsland, et al. Habituation revisited: an updated and revised description of the behavioral characteristics of habituation. *Neurobiology of learning and memory*, 92(2):135–138, 2009.

[54] J. A. Sniezek and T. Buckley. Cueing and cognitive conflict in judge-advisor decision making. *Organizational behavior and human decision processes*, 62(2):159–174, 1995.

[55] M. Steyvers, H. Tejeda, G. Kerrigan, and P. Smyth. Bayesian modeling of human–ai complementarity. *Proceedings of the National Academy of Sciences*, 119(11):e2111547119, 2022.

[56] N. Tomašev, X. Glorot, J. W. Rae, M. Zielinski, H. Askham, A. Saraiva, A. Mottram, C. Meyer, S. Ravuri, I. Protsyuk, et al. A clinically applicable approach to continuous prediction of future acute kidney injury. *Nature*, 572(7767):116–119, 2019.

[57] P. Tschandl, C. Rinner, Z. Apalla, G. Argenziano, N. Codella, A. Halpern, M. Janda, A. Lallas, C. Longo, J. Malvehy, et al. Human–computer collaboration for skin cancer recognition. *Nature Medicine*, 26(8):1229–1234, 2020.

[58] R. Vaithianathan, E. Putnam-Hornstein, N. Jiang, P. Nand, and T. Maloney. Developing predictive models to support child maltreatment hotline screening decisions: Allegheny county methodology and implementation. *Center for Social data Analytics*, 2017.

[59] L. M. Van Swol and J. A. Sniezek. Factors affecting the acceptance of expert advice. *British journal of social psychology*, 44(3):443–461, 2005.

[60] B. Wilder, E. Horvitz, and E. Kamar. Learning to complement humans. In *Proceedings of the Twenty-Ninth International Joint Conference on Artificial Intelligence, IJCAI-20*, pages 1526–1533. International Joint Conferences on Artificial Intelligence Organization, 7 2020.

[61] I. Yaniv. Receiving other people's advice: Influence and benefit. *Organizational Behavior and Human Decision Processes*, 93(1):1–13, 2004.

[62] M. Yeomans, A. Shah, S. Mullainathan, and J. Kleinberg. Making sense of recommendations. *Journal of Behavioral Decision Making*, 32(4):403–414, 2019.

[63] M. Yin, J. Wortman Vaughan, and H. Wallach. Understanding the effect of accuracy on trust in machine learning models. In *Proceedings of the 2019 chi conference on human factors in computing systems*, pages 1–12, 2019.

[64] Y. Zhang, Q. V. Liao, and R. K. Bellamy. Effect of confidence and explanation on accuracy and trust calibration in ai-assisted decision making. In *Proceedings of the 2020 Conference on Fairness, Accountability, and Transparency*, pages 295–305, 2020.

[65] Q. Zou, S. Xie, Z. Lin, M. Wu, and Y. Ju. Finding the best classification threshold in imbalanced classification. *Big Data Research*, 5:2–8, 2016.

APPENDICES

A The Pretrial-Release Decision Setting and Dataset

We demonstrate our approach in the pretrial-release decision setting. When a person in the United States is arrested and brought in front of a judge, the judge has to decide whether to release or detain the defendant until his trial. This decision is based on the judge's assessment of how likely is the defendant, if released, to violate his release terms, i.e., to commit another crime until his trial or to fail to appear in court for his trial. In the US, judges are assisted by risk-assessment algorithms for making this important decision [14, 28, 17], and prior research have studied the algorithms' prediction performance [33, 3] as well as the impact of using algorithmic risk assessment on human decision makers – both with judges and with nonexperts in experimental settings [21, 22, 1].

We use a dataset collected by the U.S. Department of Justice, that contains records of 151,461 criminal defendants who were arrested between the years 1990 and 2009, in 40 of the 75 most populous counties in the United States [51]. This dataset was also used in previous studies (e.g., in [21, 22, 44, 41]). The data include information about arrest charges, demographic characteristics, criminal history, pretrial release and detention, adjudication, and sentencing. We use the cleaned version of the dataset generated and used by [21, 22], that contains records of 47,141 defendants that remained after restricting the analysis to defendants who were released before trial, who were at least 18 years old, and whose race was recorded as either black or white. Thus, the dataset we use includes only defendants who were released before trial and we thus know the ground truth information about their pretrial-release outcome, i.e., we know for each case whether eventually the defendant was violating his release terms. Among the defendants in this dataset, 29.8% violated their pretrial-release terms.

B Results: Additional Details

B.1 Overview

Table 1 presents demographic and general details and Table 2 presents an overview of the results of the main metrics in the five experimental treatments.

Table 1: Demographic and general details of the participants in the experiment.

	Learned N=218	Random N=200	Omniscient N=200	No Advice N=258	Update N=220
Male	64.2%	61.0%	64.5%	64.3%	65.5%
Black	12.4%	7.5%	16.0%	10.9%	10.0%
White	81.2%	77.0%	76.5%	76.4%	80.0%
18-24 years old	4.1%	4.5%	3.5%	3.1%	1.4%
25-34 years old	41.3%	42.0%	42.0%	42.2%	46.8%
35-59 years old	50.0%	48.5%	49.0%	48.4%	49.1%
60+ years old	4.6%	5.0%	5.5%	6.2%	2.7%
College degree or higher	81.7%	87.0%	85.0%	90.3%	83.6%
Criminal justice familiarity (1-5 scale)	3.3	3.2	3.5	3.3	3.2
Machine learning familiarity (1-5 scale)	3.3	3.1	3.4	3.2	3.1
Participant payment	$4.35	$4.27	$4.45	$4.17	$4.30
Experiment duration (minutes)	19.9	22.3	23.2	20.1	23.3
Experiment clarity (1-5 scale)	4.33	4.28	4.25	4.41	4.39
Experiment enjoyment (1-5 scale)	3.83	3.85	3.82	3.90	3.87

Table 2: Experimental results: Overview of main metrics.

	Learned N=218	Random N=200	Omniscient N=200	No Advice N=258	Update N=220
Advising policy accuracy	74.1% (±0.9%)	58.4% (±1.5%)	100.0% (±0.0%)	52.5% (±2.0%)	42.0% (±2.1%)
Quadratic score	0.781 (±0.005)	0.755 (±0.007)	0.825 (±0.006)	0.719 (±0.008)	0.770 (±0.007)
Algorithm's quadratic score	0.801 (±0.003)	0.800 (±0.003)	0.803 (±0.003)	0.805 (±0.003)	0.802 (±0.003)
Linear score	0.622 (±0.006)	0.578 (±0.009)	0.653 (±0.007)	0.560 (±0.011)	0.578 (±0.008)
Algorithm's linear score	0.603 (±0.003)	0.602 (±0.004)	0.604 (±0.004)	0.607 (±0.003)	0.603 (±0.003)
Advice influence	0.810 (±0.036)	0.769 (±0.040)	0.787 (±0.041)	–	0.321 (±0.040)
Advice acceptance rates	0.735 (±0.043)	0.683 (±0.048)	0.723 (±0.045)	–	0.305 (±0.043)
Human initial risk prediction that is at least as accurate as the algorithmic risk assessment	62.47% (±1.66%)	56.84% (±2.14%)	60.60% (±1.89%)	52.46% (±1.99%)	58.00% (±2.06%)
KL divergence between the distributions of human initial risk prediction and the algorithmic risk assessment	0.47	0.52	0.41	0.92	0.32
False-positive rates (FPR)	22.88% (±1.82%)	39.39% (±3.45%)	19.63% (±2.30%)	52.64% (±4.18%)	38.04% (±3.39%)
False-negative rates (FNR)	51.39% (±1.99%)	43.55% (±2.89%)	37.51% (±2.72%)	36.28% (±3.33%)	41.83% (±2.62%)
Classification disparity	0.145	0.102	0.138	0.094	0.152

B.2 Learning Performance

Figure 5 shows the confusion matrices in the different experimental treatments. By summing the left column for each treatment, we obtain the empirical frequency in which the risk assessment was more accurate than the initial predictions made by human participants in the treatment. Interestingly, it can be seen that this rate is significantly lower in the Learned and Omniscient treatments than in all other treatments. This shows differences in behavior of the participants in these treatments when they gave their initial predictions, i.e., before they observed any algorithmic input (and specifically, an improvement in their prediction relative to the algorithm), which is an indication of human learning. Later in our analysis (in the main text and in Section B.5) we further analyze this difference in behavior, and show that the use of informed advising policies facilitates human learning.

	True	False
True	0.24	0.13
False	0.13	0.50

(a) Learned

	True	False
True	0.13	0.12
False	0.30	0.45

(b) Random

	True	False
True	0.39	0
False	0	0.61

(c) Omniscient

	True	False
True	0	0
False	0.48	0.52

(d) No Advice

	True	False
True	0.42	0.58
False	0	0

(e) Update

Figure 5: Confusion matrices. For each treatment, the left and right columns show the ground-truth rates of whether or not the algorithmic risk prediction was more accurate than the human initial prediction, respectively, and the top and bottom rows show the rates in which the advising policy determined to provide or not to provide the advice, respectively.

B.3 Impact on Human Prediction Performance

In the main text we evaluate the prediction success of the human decision makers in our experiment according to the linear score and the quadratic score. Figure 6 presents the distributions of the average scores of the participants in the different experimental treatments and of the algorithmic predictions. In addition, Figures 7 and 8 present the performance according to other measures: the AUC and the logarithmic score, respectively. For each measure, the figures present the full distributions of participant and algorithmic performance, via standard box plots as in Figure 6, as well as bar charts with error bars that indicate 95% confidence intervals.

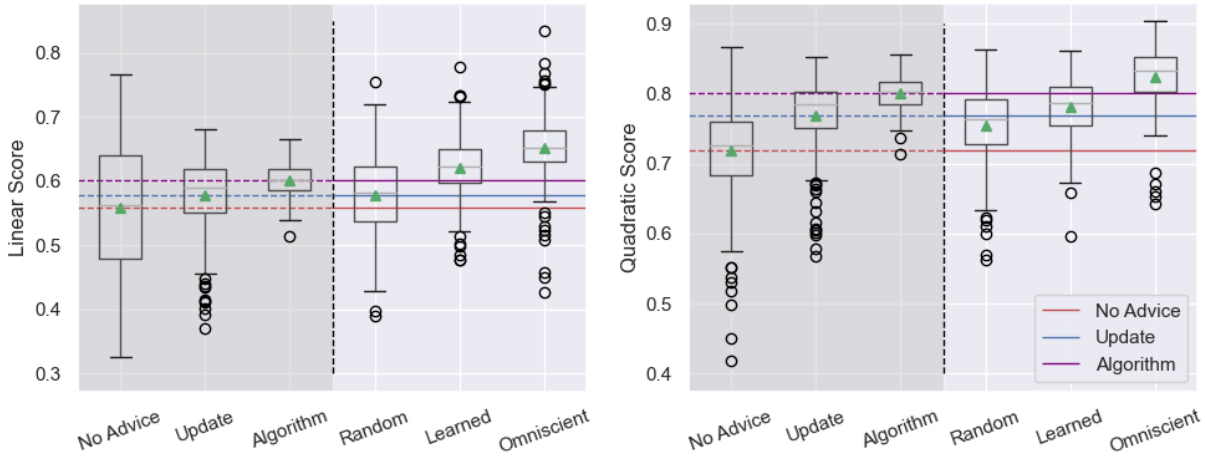
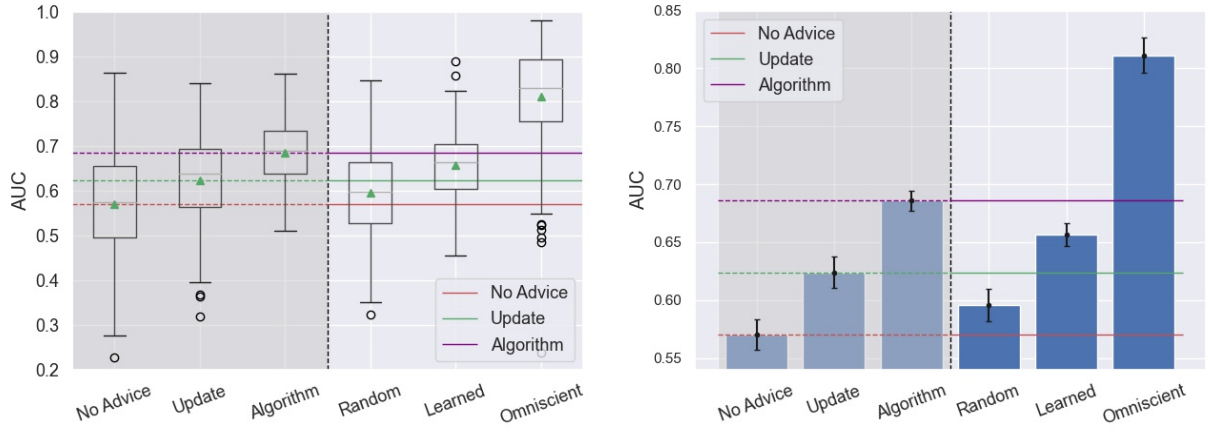


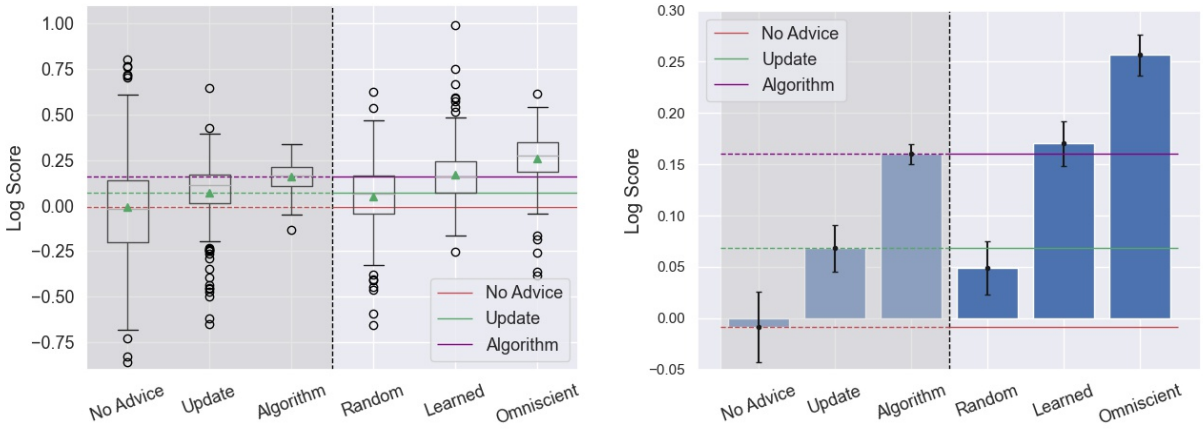
Figure 6: Distributions of participant performance in the different experimental treatments, and of the algorithm's performance, according to both linear and quadratic scores. The performance is presented via standard box plots (Tukey style [45]), where the box extends from the first quartile to the third quartile of the data, the gray line indicates the median and the green triangle indicates the mean. The No Advice, Update, and algorithmic benchmarks are presented on the left of each figure, and for ease of comparison, their means continue as horizontal lines. The algorithm's performance is computed over its predictions for the cases given to participants in the Learned treatment.



(a) Performance distributions.

(b) 95% confidence intervals.

Figure 7: Performance according to AUC.



(a) Performance distributions.

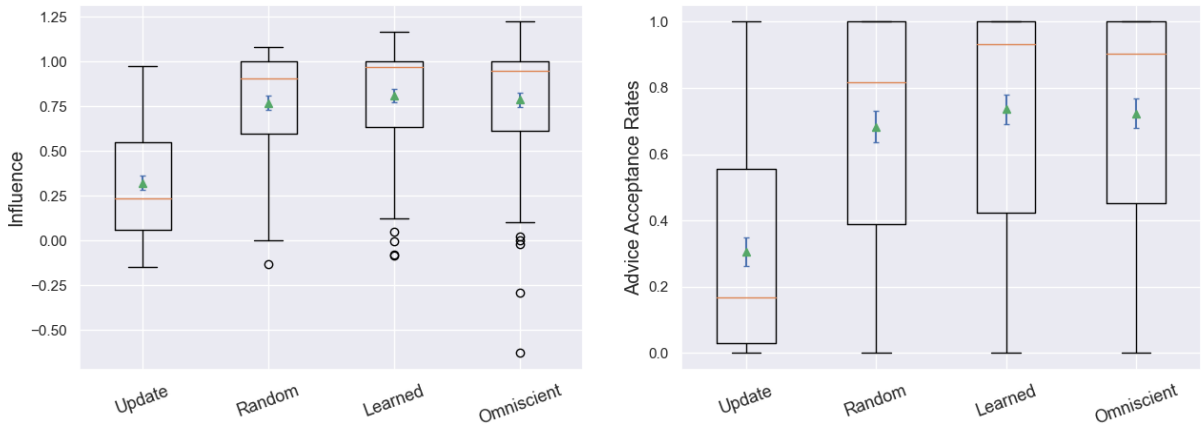
(b) 95% confidence intervals.

Figure 8: Performance according to the logarithmic score.

B.4 Human Responsiveness to the Algorithmic Advice

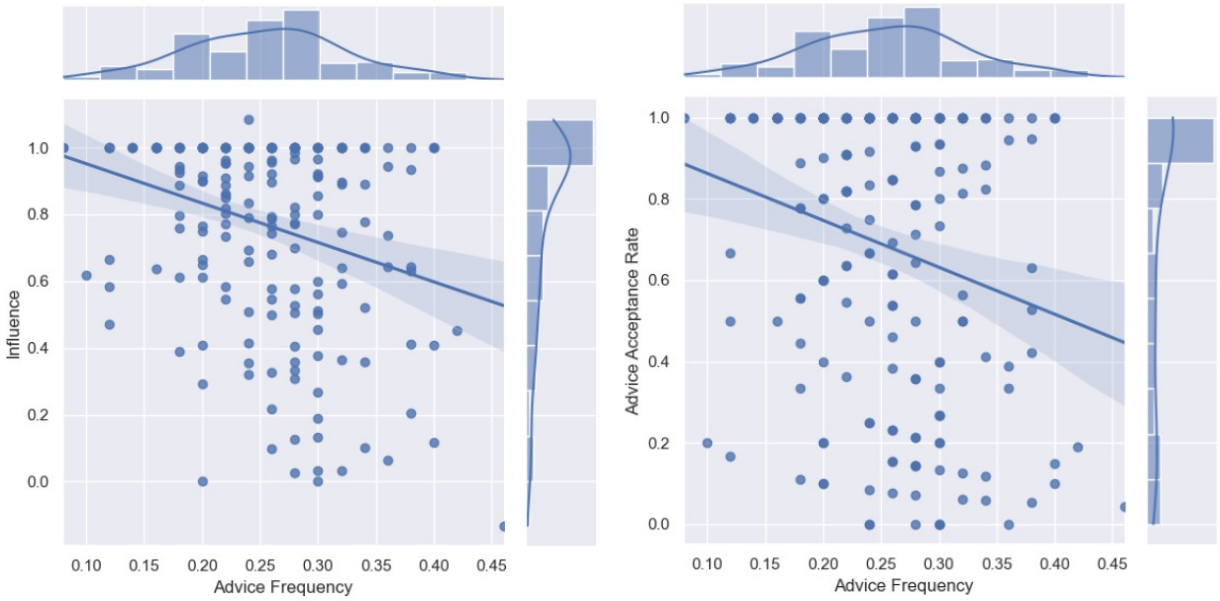
We measure human responsiveness to the algorithmic advice by the advice influence and the advice acceptance rate, as defined in *Materials and Methods*. Figures 9a and 9b show the distributions of the advice influence and the advice acceptance rate measures, respectively, over participants, in each of the four treatments in which the advice is given. As can be seen, according to both measures, the responsiveness to the advice is much stronger in all the three algorithmic-assistant treatments, in which the advice was provided only for part of the predictions (i.e., Random, Learned, and Omniscient), compared with the Update treatment in which the algorithmic risk assessment was provided for all predictions. Although the responsiveness to the advice is lower in Random than in Learned and Omniscient, the differences between these three treatments are not statistically significant.

The large gap between Update and the three experimental treatments could result from two potential sources: the partial advice and a potential framing in the algorithmic-assistant setting. To isolate the effect of the partial advising, we look within the Random treatment. As mentioned in the main text, in the Random treatment (unlike the Learned and Omniscient treatments) the variance in the frequencies in which participants received the advice comes from getting the advice with a fixed probability in each prediction. Figure 10 shows a significant pattern, according to both responsiveness measures, which we term a "scarcity effect": human responsiveness to the advice tends to increase with the scarcity of the advice. Specifically, the advice frequency is negatively correlated with the responsiveness of participants to the advice, as measured by the advice acceptance rate ($\rho = -0.23$, $p < 0.001$) and by the advice influence measure ($\rho = -0.29$, $p < 0.001$).



(a) (b)

Figure 9: Human responsiveness to the algorithmic advice.



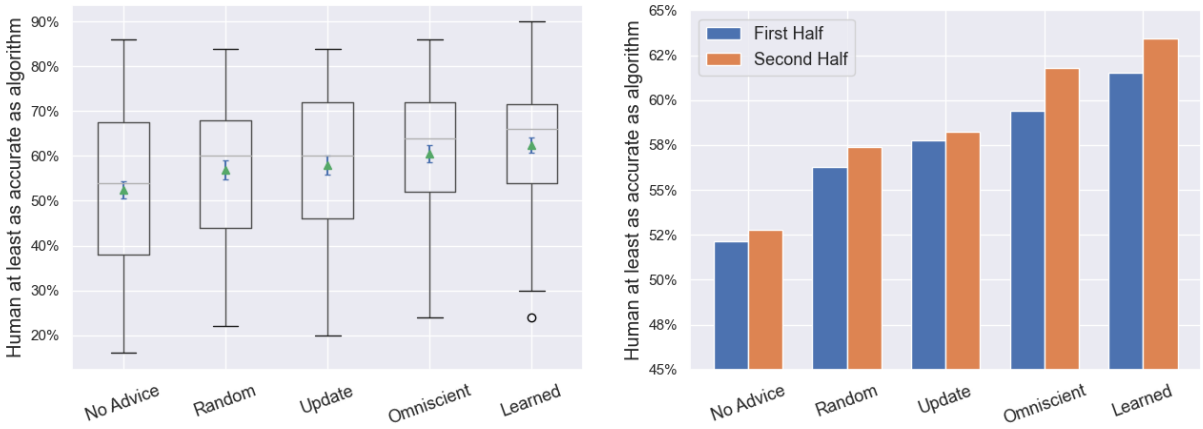
(a) Advice influence vs. advice frequency.

(b) Advice acceptance rate vs. advice frequency.

Figure 10: The scarcity effect. The figures show for participants in the Random treatment, scatter plots of the advice influence (Figure 10a) and the advice acceptance rate (Figure 10b) vs. the advice frequency, alongside the marginal distributions and the regression lines.

B.5 Indication of Human Learning

Figure 11 shows the performance of participants' initial (i.e., unassisted) predictions in terms of the frequency of initial predictions that were at least as accurate as the algorithmic prediction. Figure 11a presents the performance distributions over participants in each treatment. In all experimental treatments this frequency is significantly higher than in the No Advice treatment ($p < 0.01$). In the Learned and Omniscient treatments, human initial predictions were at least as accurate as the algorithmic risk score in 62.5% and 60.6% of the predictions, respectively, which is significantly higher than in all other treatments: 58.0%, 56.8%, and 52.5%, in Update, Random, and No Advice treatments, respectively ($p < 0.02$, except for the comparison of Omniscient and Update for which $p = 0.072$). The advantage of Update over the No Advice benchmark is consistent with the indication of learning in Update compared to No Advice that was observed in [22].



(a) Full experiment.

(b) First vs. second halves of the experiment.

Figure 11: Indication of human learning. The y-axis is the frequency of participants' initial predictions that were at least as accurate as the algorithmic prediction. (11a) Distributions over participants in each treatment. (11b) Average frequency in first and second halves of the experiment in each treatment. Differences between first and second halves are statistically significant only for Learned and Omniscient (paired t-test, $p < 0.04$ and $p < 0.01$, respectively).

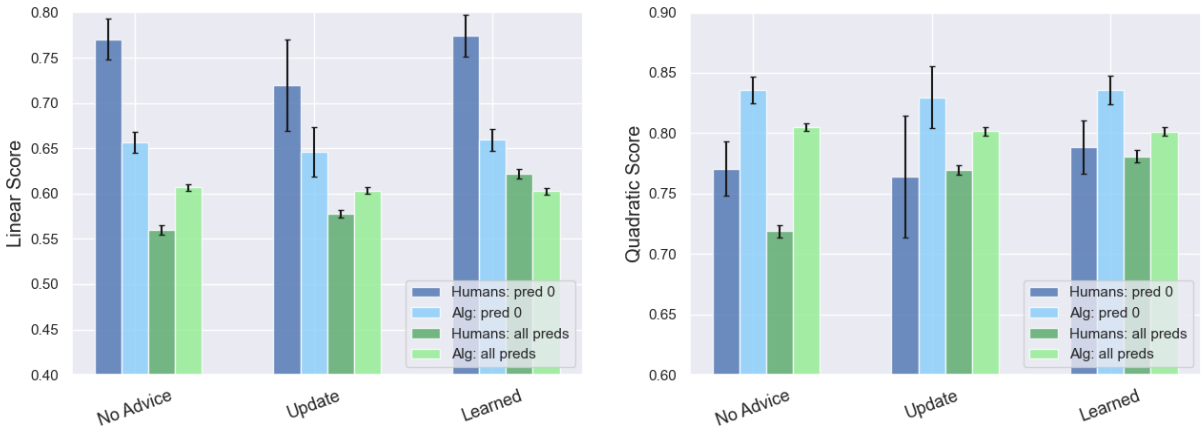
We find significant learning over time only in the Learned and Omniscient treatments. Specifically, we compared changes over time in the frequency in which the human initial prediction was at least as accurate as the algorithm's risk score. We found that only in the Learned and Omniscient treatments this frequency significantly improved between the first and second halves of the series of 50 predictions that participants made (paired t-test, $p < 0.04$, Figure 11b). In addition, the average frequency was positively correlated with the prediction period only in the Learned and Omniscient treatments ($\rho = 0.35$ $p < 0.02$, and $\rho = 0.49$ $p < 0.001$, respectively).

Finally, we find that the learning effect we observed was only weakly translated to an improvement in the quality of the initial predictions with respect to the ground truth. That is, testing over all predictions, only in the Learned and Omniscient treatments the initial predictions participants made outperformed those in the No Advice treatments according to both the linear and quadratic scores ($p < 0.01$), and testing for improvement over time we find that only the Omniscient treatment consistently improves such that both score measures were positively correlated with prediction period ($\rho = 0.33$ $p < 0.03$, and $\rho = 0.45$ $p < 0.01$, for linear and quadratic scores, respectively). This weaker effect in the comparison with the ground truth could be expected due to multiple noise sources in the learning process (humans imperfectly learn from imperfect algorithmic feedback about the ground truth), and it may also be that the time horizon is not long enough for the learning effect to become evident.

B.6 Tension between Imitating the Algorithm and Preserving Complementary Human Strengths

In the main text we focus on one behavior that we observed – predicting a risk of zero – that is very different between the humans and the algorithm. The algorithm never predicts a risk of zero, while in No Advice a risk of zero was predicted by human participants in 10.5% of all predictions. This behavior remains in Learned (11% of all initial predictions are zero), but in Update people learn not to predict this value (only 2.17% of all predictions are zero).

Figures 12a and 12b compare human and algorithmic performance according to the linear and quadratic scores, respectively, in the cases in which the human initial prediction is zero and over all predictions, in Learned, No Advice, and Update (the performance is according to the final prediction the human made). As can be seen, in No Advice and Learned when people predict zero



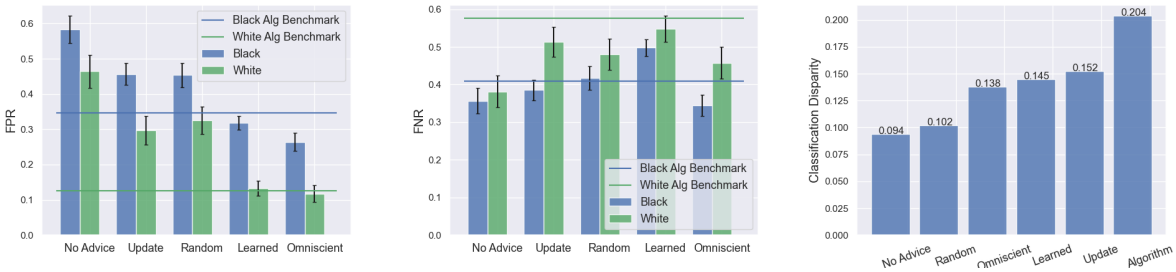
(a) (b)

Figure 12: A comparison of human and algorithmic performance according to the linear and quadratic scores, in the cases in which the human initial prediction is zero and over all predictions, in Learned, No Advice, and Update (the performance is according to the final prediction the human made).

they tend to do better than their overall performance and thus it seems that learning not to predict zero in Update hurts their performance. Also, the comparison of the algorithm with No Advice shows that when human initial prediction is zero (unlike when considering all predictions) it is not clear whether the algorithm is at all better than the human, and the question which behavior is more desirable depends on the measure we wish to optimize.

B.7 Fairness

Figures 13a and 13b show, for black and white defendants, the group false-positive rates (FPR) and group false-negative rates (FNR) of the predictions made by participants in the different experimental treatments, as well as those obtained from the algorithm, as defined in *Materials and Methods*. The results are also summarized in Table 2.



(a) (b) (c)

Figure 13: Performance with respect to defendants' racial groups. (a) False-positive rates (FPR); (b) False-negative rates (FNR); (c) Classification disparity.

We start by evaluating the levels of group FPR (Figure 13a) and group FNR (Figure 13b) in the Learned treatment. The results show that the FPR in the Learned treatment, for both black and white defendants, is substantially lower than the FPR in the Update, Random, and No Advice treatments, but at the cost of higher FNR. Specifically, the average FPR in the Learned treatment is 31.9% and 13.2% for black and white defendants, respectively, which is significantly lower than the FPR of 45.6% and 29.7%, respectively, obtained in the Update treatment ($p < 10^{-5}$). The FNR in the Learned treatment is 49.8% and 54.8% for black and white defendants, respectively,

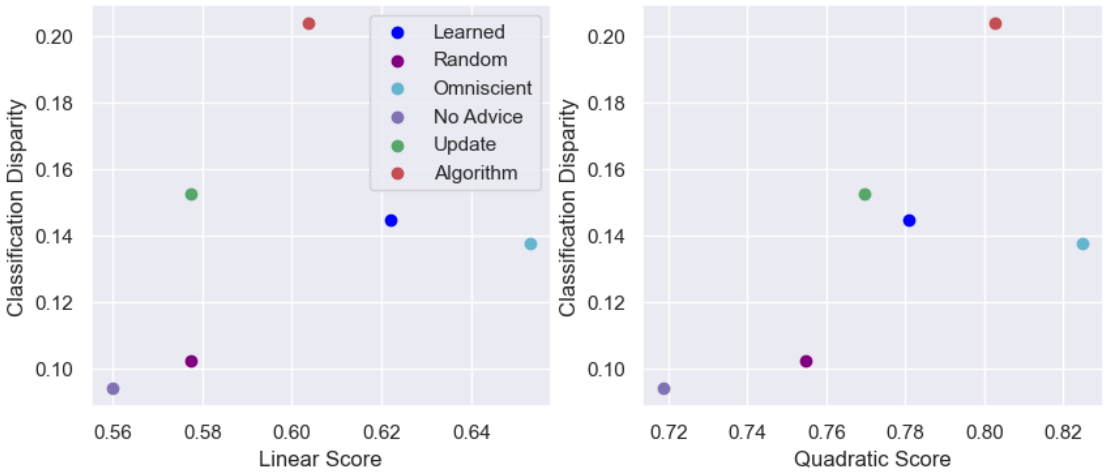


Figure 14: Accuracy-fairness tradeoff.

which is higher than the FNR of 38.6% and 51.3%, respectively, in the Update treatment, but the difference is statistically significant only for black defendants ($p < 10^{-5}$). A comparison of the Learned treatment with the risk-assessment algorithm shows that the FPR for black defendants is significantly lower in the Learned treatment (paired t-test, $p < 0.01$) but is comparable to the FPR of the algorithm for white defendants ($p = 0.34$), and that the FNR for black defendants is significantly higher in the Learned treatment ($p < 10^{-5}$) but is comparable to the FNR of the algorithm for white defendants ($p = 0.78$).

Next, we look at the disparity in FPR and FNR between black and white defendants in the Learned treatment. A comparison of the Learned and Update treatments shows significantly lower FNR disparity in the Learned treatment ($p < 0.01$). The FPR disparity in Learned is higher than that in Update, but the difference is not statistically significant ($p = 0.07$). A comparison of the Learned treatment with the risk-assessment algorithm shows that both the FPR and FNR disparities are significantly lower in the Learned treatment than those of the algorithmic predictions (paired t-test, $p < 10^{-5}$). Thus, in terms of FPR and FNR disparities, Learned has a significant advantage compared with the algorithm, and is comparable to or better than the Update treatment.

As shown in the main text, according to both FPR and FNR, all treatments have an error that is biased to the same direction which gives harsher predictions for black defendants. We quantify this discrimination by defining "classification disparity" for a treatment as: $Pr(Y = 0)(FPR_{Black} - FPR_{White}) + Pr(Y = 1)(FNR_{White} - FNR_{Black})$. The classification disparity weighs the discrimination of black compared to white non-risky defendants (the first term), and the bias in favor of white compared to black risky defendants (the second term). Equivalently, the classification disparity can be interpreted in terms of utility from the point of view of the defendant: the first term is the utility gap for non-risky defendants and the second term is the utility gap for risky defendants, both in favor of white defendants and are weighted by the overall frequencies of risky and non-risky defendants in the population. Figure 13c shows the classification disparity for all experimental treatments and for the risk-assessment algorithm. Interestingly, the algorithm has the highest bias although it did not directly observe race whereas humans did. Learned, Omniscient, and Update have intermediate classification disparity values, with an advantage to Learned and Omniscient. No Advice and Random have the least classification disparity.

Figure 14 shows that when considering the accuracy-fairness tradeoff, Learned and Omniscient Pareto dominate the Update treatment. Omniscient Pareto dominates the algorithm as well. Learned is comparable to the algorithm by accuracy performance (the performance ranking depends on the measure used), but is superior in terms of disparity. These results suggest that the informed advising policies (the Learned and Omniscient treatments) manage to balance the impact of algorithmic advice on human predictions between gaining from the high performance of

the algorithm, while moderating its racial disparity.

Finally, we analyze interaction disparity according to the two measures studied in [22], to evaluate whether participants responded to the risk assessment in a racially biased manner. In [22], every experimental treatment exhibited disparate interactions, including the Update treatment (which is identical to the Update treatment in our experiment) that yielded the smallest disparity. Our experiment replicates the results for the "influence disparity" measure for the Update treatment. However, in the Learned and Omniscient treatments this influence disparity was eliminated. Specifically, similar to [22], in our Update treatment when the risk assessment was higher than the human's initial prediction, its influence to increase risk in predictions about black defendants was significantly larger than in predictions about white defendants (influence of 0.35 vs. 0.27 for black and white defendants, respectively, paired t-test $p < 0.02$), but when the risk assessment was lower than the human's initial prediction, the inverse pattern emerged (though it did not reach statistical significance: influence of 0.28 vs. 0.31 for black and white defendants, respectively, paired t-test $p = 0.059$). By contrast, this bias was not observed in the Learned and Omniscient treatments, which as discussed above had substantially higher responsiveness to the algorithmic advice. Second, the "deviation disparity" observed in [22] was not observed in our experiment, including our Update treatment. Specifically, in [22] the results show that participants on average deviated positively (toward higher risk) for black defendants and negatively (toward lower risk) for white defendants. This effect was not observed in our experiment, and in fact participants on average deviated positively both for black and white defendants, and the deviation was larger for white defendants (though all differences were small).