Learning Risk Scores Robust to Unobserved Confounders

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We consider the problem of learning risk scores to prioritize individuals for scarce resources or interventions, from historical observational data affected by unobserved confounding. In settings such as public health and homelessness prevention, decisions about who receives a scarce resource (e.g., a hospital bed or housing) are often guided by a risk score assigned to each individual based on recorded characteristics, such as responses to a survey. These risk scores are increasingly being learned directly from observational data: historical records of individuals' characteristics, allocation decisions, and outcomes under received allocations. Standard methods such as inverse propensity weighting (IPW), which corrects for the bias introduced by the historical allocation policy, can be used to learn accurate risk scores if the historical decision process is fully explained by the recorded characteristics, an assumption known as unconfoundedness. In practice, however, historical decisions often depend on unrecorded information (e.g., details a caseworker learns in conversation but never logs), causing learned risk scores to systematically under-prioritize exactly the individuals whose unrecorded circumstances drove past prioritization. We propose a method for learning risk scores that are robust to this kind of unobserved confounding, building on IPW. Since propensity weights cannot be reliably estimated under unobserved confounding, we instead treat them as belonging to an uncertainty set determined by the observable data and domain-informed estimates of the degree of confounding, combining sensitivity analysis from causal inference with Wasserstein distributionally robust optimization. The resulting robust risk score learning problem admits a sample-based approximation that we reformulate as an exponential cone program compatible with off-the-shelf solvers. We demonstrate the effectiveness of our approach relative to IPW and other benchmarks on semi-synthetic data derived from datasets in the UCI Machine Learning Repository. Our method decreases calibration error by up to 28.9% over traditional benchmarks and up to 9.8% over the state of the art, without compromising other metrics.

Key words: robust optimization, unobserved confounders, robust machine learning, conic optimization, risk scores.

1. Introduction

In high-stakes resource allocation settings, decisions about who receives scarce resources are typically made by people — caseworkers, judges, clinicians — often guided by hand-crafted risk scores: mappings from individual characteristics to a measure of vulnerability that allow interpretable comparisons between different individuals' needs. Risk scores of

this kind have been deployed in fields such as criminal justice and public health, where they inform consequential decisions about parole and medical treatment (Angwin et al. 2016, Nemati et al. 2018). Risk scores have traditionally been hand-crafted from domain expertise. In homelessness prevention, for example, housing authorities use standardized vulnerability surveys such as the VI-SPDAT (OrgCode Consulting, Inc. and Community Solutions 2015), which score risk using hand-crafted rules for different sections of questions (e.g., if a participant answers YES to any of questions 1 through 4, increase risk by 1). In recent years, however, there is a growing interest in using machine learning to learn risk scores directly from historical data, with the goal of producing scores that are more accurate than what hand-crafted rules alone can achieve (Rice et al. 2023, Wang et al. 2023).

These approaches often rely on observational data — historical records of individuals' characteristics, the allocation decisions that were actually made under some prior policy, and the outcomes that followed — rather than data from randomized assignment. In the case of homelessness, this means historical records of individuals who filled out the VI-SPDAT, whether or not they received housing, and their observed outcomes in the system (e.g., remaining housed vs. returning to homelessness). Crucially, we only observe outcomes for each individual for the specific resource they were allocated, and do not observe other counterfactual outcomes. To learn accurate risk scores from observational data, existing methods typically work by debiasing the historical allocation policy itself. A standard tool for this is inverse propensity weighting (Rosenbaum and Rubin 1983), which reweighs observations by an estimated propensity score, representing the probability an individual would have received the resource under the historical policy given their recorded characteristics. This causes the reweighted data to behave as if historical allocation had been random.

These methods, however, typically require an unconfoundedness assumption that, conditional on the recorded characteristics, the historical allocation decision is independent of the outcome. In other words, everything relevant to both the decision and the outcome should be captured in the data. In practice, this assumption frequently fails, because historical decisions were often based on more information than was recorded. In homelessness prevention, for instance, the allocations we observe were shaped not only by survey responses but by factors caseworkers observed in person; factors such as social connections or perceived "resilience" may have no direct analogue in the recorded data. This means individuals who received housing may be systematically different from those who did not, in ways we cannot observe ex post

— so a risk score learned using a method that assumes unconfoundedness may not properly debias the historical allocation policy. This problem is not unique to homelessness: whenever historical policies depended on richer information than what was recorded, propensity scores like the one above cannot be reliably estimated, because they are themselves confounded by unobserved variables.

This gives rise to our research question: *how can we learn risk scores that are robust to unobserved confounding?* We treat the unidentifiable propensity scores as uncertain and maximize worst-case expected log-likelihood over an uncertainty set that (i) bounds confounding via the marginal sensitivity model of Tan (2006), (ii) enforces consistency with the observable data via a Wasserstein constraint, and (iii) can capture domain-specific logic such as historical rationality. The sample-based problem can be reformulated as an exponential cone program, and we evaluate it on semi-synthetic data from the UCI ML repository.

1.1. Related Work

Risk score learning as optimization. Risk scores are simple, additive scoring rules that let practitioners estimate risk with a handful of arithmetic operations, and have long been used in medicine and criminal justice (Angwin et al. 2016, Nemati et al. 2018). Ustun and Rudin (2019) argue that such scores, which were traditionally built ad hoc through expert judgment, are better obtained by directly formulating risk score construction as an optimization program that jointly balances calibration, sparsity, and operational constraints rather than treating them as separate heuristic stages. Our work adopts this same premise — that risk scores are best learned by directly optimizing an objective that reflects the desired properties of the score — but we aim to learn risk scores from observational data, so we only observe outcomes for individuals under the treatment they received. Since treatment assignments may be subject to unobserved confounding, we therefore extend this optimization-based framing to a robust formulation, optimizing against a worst-case realization of the confounder within a causal uncertainty set, rather than against a single fixed dataset. While our experiments focus on real-valued risk scores, our framework can easily accommodate integrality or sparsity constraints if desired.

Sensitivity analysis, causal uncertainty sets, and partial identification. Sensitivity analysis for unmeasured confounding originates with Cornfield et al. (1959) and Rosenbaum (2002), and was recast by Tan (2006) as the marginal sensitivity model (MSM), which

bounds the difference in odds ratios between the true and observed propensity scores. Bertsimas et al. (2023) connect the MSM to distributionally robust optimization, deriving sharp average treatment effect bounds by optimizing over the induced uncertainty set; Dorn and Guo (2023) sharpen this characterization by reducing average treatment effect bounds to a single quantile regression, and Dorn et al. (2025) establish closed-form solutions for expectation-type objectives under this set. Zhang et al. (2025) propose the enhanced MSM (eMSM), supplementing the MSM treatment sensitivity constraint with an outcome sensitivity constraint that bounds how much an unobserved confounder can shift treatment effects, yielding tighter sharp bounds on causal estimands. Our uncertainty set also builds on the MSM, but is shaped by a different modeling goal: rather than bounding a causal estimand such as the average treatment effect, we optimize a predictive risk score. Our formulation can accommodate general outcome sensitivity constraints in the style of eMSM, bounding the difference between true and observed treatment effects by an arbitrary interval. Historical rationality — the assumption that decision-makers, given access to richer information than what is recorded, used it on average to make better decisions than they would have without it — corresponds to the one-sided choice of this interval $[0, \infty)$. We adopt this realization specifically because, unlike eMSM's outcome constraint, it requires no additional sensitivity parameter to elicit or estimate.

More broadly, a large literature derives bounds on causal estimands under a variety of sensitivity models (Tan 2006, Dorn and Guo 2023, Dorn et al. 2025, Zhang et al. 2025, Yadlowsky et al. 2022, Jin et al. 2025) that acknowledge the need for distributional consistency between the true and observed distributions. This consistency is generally enforced by either normalizing on a per-covariate basis or analytically constructing a worst-case consistent distribution that lies within the given uncertainty set, where the worst case is in relation to the bound being derived (usually average treatment effect). Our method instead enforces distributional consistency via a Wasserstein bound on the unknown distribution. At the population level, this bound is equivalent to bounding on a per-covariate basis, but the bound can also be relaxed to account for settings where the distributions being compared do not have the same support, which can arise in sample approximation settings. Additionally, our consistency bound is independent of the objective being optimized, whereas the constructions above are tied to the specific causal estimand whose bound is being derived.

Robust prediction under distributional shift. Shimodaira (2000) shows that under covariate shift, reweighting examples by the ratio between test and training densities and maximizing the resulting weighted log-likelihood yields asymptotically optimal estimates of a model's regression coefficients, akin to inverse propensity weighting. In our setting, unobserved confounding means that we cannot exactly know these ratios, motivating worst-case optimization over a causal uncertainty set rather than point-estimate reweighting. Shafieezadeh Abadeh et al. (2015) show that Wasserstein-robust logistic regression admits an exact reformulation as a norm-regularized logistic regression; Esfahani and Kuhn (2018) later generalize this result, establishing that minimax optimization over worst-case distributions with Wasserstein uncertainty sets can be reformulated as tractable convex programs across a broader class of losses. While we leverage Wasserstein distance to establish a consistency constraint, our uncertainty set is defined by propensity score bounds from the MSM rather than direct bounds on the distribution of the observed data, yielding a causally interpretable robustness parameter Γ that practitioners can reason about directly. Sahoo et al. (2026) similarly optimize a predictive model against a worst-case distribution within a Γ-bounded uncertainty set with odds-ratio form identical to the MSM, but their Γ bounds outcome-dependent selection into the observed sample rather than outcome-dependent treatment assignment. Chen et al. (2016) and Liu and Ziebart (2017) extend minimax prediction to general losses, further validating the robustness-over-reweighting philosophy we adopt.

Robust policy learning. Kallus and Zhou (2021) use MSM-derived uncertainty sets for confounding-robust policy improvement in offline settings, optimizing worst-case policy value. Their target — expected reward under a treatment rule — differs fundamentally from ours. Risk scores are upstream of policy: they produce vulnerability estimates that inform human decision-making rather than prescribe treatment directly, a distinction that matters in social services settings where interpretability and human oversight are desired. Additionally, our uncertainty set includes additional consistency constraints. We show through an example that using the uncertainty set of Kallus and Zhou causes the uncertainty set to include distributions incompatible with the observed data, and show experimentally that this leads to worse predictions.

1.2. Notation

All random variables are defined as measurable functions on an abstract probability space $(\Omega, \mathcal{F}, \mathbb{P})$ and are capitalized (e.g., X), while their realizations are denoted by the same

symbols in lowercase letters (e.g., x). For any random vector X, $\mathbb{P}_{\mathrm{x}}$ denotes its marginal distribution. All (in)equalities involving random variables should be understood to hold almost surely. The family of all bounded Borel-measurable functions from a Borel set $\mathcal{A} \in \mathcal{B}(\mathbb{R}^\ell)$ to another $\mathcal{C} \in \mathcal{B}(\mathbb{R}^k)$ is denoted by $\mathcal{L}(\mathcal{A}, \mathcal{C})$. For a finite set $\mathcal{Y}$, $\Delta(\mathcal{Y})$ denotes the probability simplex over $\mathcal{Y}$. The indicator function is denoted by $\mathbb{1}(\cdot)$, taking the value 1 if the argument is true, 0 otherwise. Finally, $\|\cdot\|$ refers to the Euclidean norm, with the understanding that other choices of norms do not change the theoretical conclusions of the work.

2. Problem Description

Individuals are characterized by their observed covariates $X \in \mathcal{X} \subseteq \mathbb{R}^{d_{\mathrm{x}}}$, unobserved confounders $U \in \mathcal{U} \subseteq \mathbb{R}^{d_{\mathrm{u}}}$, and potential outcomes $\{Y^t\}_{t \in \mathcal{T}} \in \mathcal{Y} = \{-1, 1\}^{m+1}$, where $\mathcal{T} = \{0, 1, \ldots, m\}$ is the set of treatments, and Y^t is the potential outcome under $t \in \mathcal{T}$ (Hernan and Robins 2023). We consider $Y^t = -1$ to be the *adverse outcome*, and use $t = 0$ to denote the no-treatment case. We denote by $\mathbb{P}$ the distribution of $(X, U, \{Y^t\}_{t \in \mathcal{T}})$.

The distribution $\mathbb{P}$ is unknown. Instead, we have access to historical data of the form $\{x_i, t_i, y_i\}_{i=1}^n$, where t_i is the treatment historically assigned to i according to an (unknown) historical policy $\pi_{\mathrm{h}} \in \mathcal{L}(\mathcal{X} \times \mathcal{U}, \Delta(\mathcal{T}))$, and $y_i = y_i^{t_i}$. Here, $\pi_{\mathrm{h}}^t(x, u)$ denotes the probability of assigning treatment t to an individual with covariates x and unobserved confounders u. In the observed data, neither the confounders u_i nor the counterfactual outcomes $\{y_i^t\}_{t \in \mathcal{T} \setminus \{t_i\}}$ are available. We make the following assumption throughout.

Assumption 1. *We assume (i) consistency ($Y = Y^T$), (ii) positivity ($\pi_{\mathrm{h}}^t(X, U) \geq \delta > 0$ for all $t \in \mathcal{T}$), and (iii) conditional exchangeability given both observed covariates and unobserved confounders ($Y^t \perp T \mid (X, U)$ for all $t \in \mathcal{T}$).*

Assumption 1 (i) and (ii) are standard in the causal inference literature (Hernan and Robins 2023). Assumption 1 (iii) is not restrictive, unlike the standard conditional exchangeability assumption (a.k.a. unconfoundedness), which requires $Y^t \perp T \mid X$ for all $t \in \mathcal{T}$. Indeed, Assumption 1 (iii) holds without loss of generality because U is defined broadly and may represent any collection of missing confounders required to have conditional exchangeability.

Problem formulation. We aim to learn the parameters β of a risk score $s(x) = \beta^\top x$ that, given observable covariates x, estimates the probability of an adverse outcome ($Y^0 = -1$) under no treatment ($t = 0$) using the logistic link

$$\Pr_\beta(Y^0 = -1 \mid X = x) = (1 + \exp(-\beta^\top x))^{-1}.$$

Under this model, higher risk score corresponds to a higher probability of an adverse outcome. β is assumed to lie in some set $\mathcal{B} \subseteq \mathbb{R}^{d_X}$, where $\mathcal{B}$ can be chosen to capture additional constraints such as integrality or sparsity.

If $\mathbb{P}$ were known, we could try to solve the following problem to find the coefficients that best explain the relationship between X and Y^0:

$$\sup_{\beta \in \mathcal{B}} \mathbb{E}_{\mathbb{P}} \left[\log \Pr_\beta(Y^0 \mid X) \right] \tag{1}$$

The objective function of (1) is the expected log-likelihood under $\mathbb{P}$. Problem (1) cannot be solved because $\mathbb{P}$ is unknown. If the data were fully observable, a standard sample average approximation (SAA) approach would replace the unknown distribution with the empirical distribution induced by the sample. However, this approach is also infeasible with observational data because we observe y_i^0 only when $t_i = 0$, and therefore cannot estimate the joint distribution of (X, Y^0) or the expectation in (1) from the data. A common approach in this case is to rely on so-called propensity scores (Rosenbaum and Rubin 1983), i.e., $\mu_i = \mathbb{P}(T = t_i \mid X = x_i, U = u_i)$. However, these scores also cannot be estimated from the data when there are unobserved confounders. For this reason, we establish a robust framework to model this uncertainty. To this end, using standard arguments, we next reformulate (1) in terms of $\mathbb{P}(T = 0 \mid X, U)$.[1]

Lemma 1. *It holds that*

$$\begin{aligned} &\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y^0 \mid X)] \\ =&\mathbb{E}_{\mathbb{P}} \left[\frac{\log \Pr_\beta(Y \mid X)}{\mathbb{P}(T = 0 \mid X, U)} \,\middle|\, T = 0 \right] \mathbb{P}(T = 0). \end{aligned} \tag{2}$$

The right-hand side of (2) can be interpreted as an expectation under a distribution obtained by reweighting the untreated population inversely to its probability of receiving no treatment, thereby correcting for treatment-selection bias. The idea is that this reweighting recovers the distribution of (X, Y^0) by reweighting the conditional distribution of $(X, Y) \mid T = 0$. We make this argument more explicit later in this section.

We can estimate the conditional distribution of $(X, Y) \mid T = 0$ from the data. However, U is never observed, and its meaning and support $\mathcal{U}$ are unknown. As a result, $\mathbb{P}(T = 0 \mid X, U)$ is not identifiable from the data. To address this uncertainty, we adopt a robust formulation

[1] All proofs can be found in Appendix EC.2.

for learning risk scores that perform well under all plausible $\mathbb{P}(T=0 \mid X,U)$. Since neither U nor $\mathcal{U}$ is known, we introduce a random variable $Z:\Omega\to[\delta,1]$, representing the unknown $\mathbb{P}(T=0 \mid X,U)$, and model it as belonging to an uncertainty set $\mathcal{S}_{\mathrm{z}}$ of measurable functions from Ω to $[\delta,1]$. We will impose additional structure on $\mathcal{S}_{\mathrm{z}}$ to ensure consistency with what can be estimated from the observable data and our modeling goals. We are therefore interested in the following robust optimization problem (constant $\mathbb{P}(T=0)$ can be dropped from the objective function).

$$\sup_{\beta\in\mathcal{B}} \inf_{Z\in\mathcal{S}_{\mathrm{z}}} \mathbb{E}_{\mathbb{P}}\left[\frac{\log \Pr_\beta(Y \mid X)}{Z}\,\middle|\, T=0\right] \tag{3}$$

This optimization problem should be interpreted as maximizing the objective of (1) under an adversarial realization of Z that lies in our uncertainty set, thereby ensuring robustness under all possible realizations of Z.

Uncertainty set. We require the following conditions on $\mathcal{S}_{\mathrm{z}}$. These conditions are satisfied by the (unknown) true $\mathbb{P}(T=0 \mid X,U)$, i.e., when $Z=\mathbb{P}(T=0 \mid X,U)$. We require *consistent inflation of the untreated population*, i.e.,

$$\mathbb{E}_{\mathbb{P}}\left[1/Z \mid T=0\right] = 1/\mathbb{P}(T=0) \tag{C1}$$

and *consistent coverage of the covariate distribution*, i.e.,

$$\mathbb{W}(\mathbb{P}_{\mathrm{x}}, \mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}) = 0. \tag{C2}$$

Here, $\mathbb{W}$ is the 1-Wasserstein distance induced by $\|\cdot\|$, intuitively, the amount of work needed to transform one distribution into another by moving probability mass (formally defined in Appendix EC.1.1), and $\mathbb{Q}^{\mathrm{z}}$ is the $1/Z$-reweighted distribution among the untreated population:

$$\mathbb{Q}^{\mathrm{z}}(A) = \frac{\mathbb{E}_{\mathbb{P}}[\mathbb{1}(A)/Z \mid T=0]}{\mathbb{E}_{\mathbb{P}}[1/Z \mid T=0]} \quad \forall A\in\mathcal{F}.$$

Recalling that Z represents the unknown $\mathbb{P}(T=0 \mid X,U)$, $\mathbb{Q}^{\mathrm{z}}$ is a distribution obtained by reweighting the untreated population inversely to its probability of receiving no treatment. Note that this is precisely the distribution referred to in the discussion following Lemma 1; the right-hand side of (2) is exactly equal to $\mathbb{E}_{\mathbb{Q}^{\mathrm{z}}}[\log \Pr_\beta(Y^0 \mid X)]$ when $Z=\mathbb{P}(T=0 \mid X,U)$. Put differently, the $1/Z$-reweighting aims to make the untreated population (i.e., the conditional distribution of $(X,Y) \mid T=0$) representative of the entire population (i.e., the distribution of (X,Y^0)).

Condition (C1) ensures that the reweighted untreated population is inflated to the correct full-population scale. For example, if the untreated group makes up 20% of the population, so that $\mathbb{P}(T=0)=0.2$, then the average weight in this group must be $1/0.2=5$ to scale the untreated group back to the full population size. Condition (C2) ensures that the reweighting is performed so as to recover the estimable full-population distributional property. In particular, even with abundant data, only the marginal distribution of X can be estimated. We use Wasserstein distance to formulate this requirement because, in finite samples, the covariate supports of the treated and untreated groups often do not perfectly overlap. Wasserstein distance can compare distributions with different supports, unlike alternatives such as ϕ-divergences.

The next proposition shows that when $Z=\mathbb{P}(T=0\mid X,U)$, the two conditions are satisfied, ensuring consistency with the (unobservable) ground truth distribution.

PROPOSITION 1. *If $Z=\mathbb{P}(T=0\mid X,U)$, then* (C1) *and* (C2) *are satisfied.*

We further impose the following *odds ratio bound* condition on $\mathcal{S}_{\mathrm{z}}$ to characterize the plausible effects of unobserved confounding:

$$\Gamma^{-1}\leq\frac{\mathbb{P}(T=0|X)}{1-\mathbb{P}(T=0|X)}\frac{1-Z}{Z}\leq\Gamma \tag{C3}$$

Condition (C3) is based on Tan's marginal sensitivity model (Tan 2006) and has been used to capture uncertainty due to unobserved confounding, including in minimax regret policy optimization (Kallus and Zhou 2021). It requires that the true odds of receiving no treatment cannot differ from the observable odds based only on X by more than a multiplicative factor of $\Gamma\geq 1$. The factor Γ quantifies the degree of hidden bias induced by unobserved confounding, and can be calibrated by domain experts (Hsu and Small 2013). $\Gamma=1$ corresponds to the absence of unobserved confounding, while larger values of Γ allow for a greater potential effect of unobserved confounding.

To emphasize the importance of Condition (C2), consider an example where the true population distribution is such that $\mathbb{P}(X=-1)=\mathbb{P}(X=1)=0.5$, and $\mathbb{P}(T=0\mid X=-1)=\mathbb{P}(T=0\mid X=1)=0.5$. Consider a selection of the unknown Z such that $Z\mid(X=-1)=0.25$ and $Z\mid(X=1)=0.75$. Such a Z satisfies (C1) and (C3). However, under such a Z, we have $\mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}(X=-1)=0.75$ and $\mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}(X=1)=0.25$, so this reweighting selection results in a covariate distribution significantly different from the true covariate distribution overall, and it fails to

satisfy (C2). This failure is important because the point of reweighting the untreated group is to recover the overall distribution across all treatment groups, but we cannot recover even the covariate distribution.

Uncertainty set augmentation. The previous conditions are those that must be satisfied by the true underlying distribution. However, domain knowledge can also be leveraged to impose additional conditions on the uncertainty set when applicable. Frequently, experts are aware of what certain confounders may be, even if they are not recorded. For example, from conversations with domain experts in homelessness, unobserved confounders could capture a history of domestic violence that is revealed in conversation with a case worker (and thus influences the treatment decision) but not recorded in the data. To encode this idea, we consider an additional *historical rationality* condition:

$$\mathbb{E}_{\mathbb{P}}[Y^0/Z \mid T=0] \leq \mathbb{E}_{\mathbb{P}}[Y^0/\mathbb{P}(T=0|X) \mid T=0] \tag{C4}$$

Condition (C4) states that historical treatment decisions should, on average, not have been worse than what would be explained using only observed covariates. This reflects the above intuition that unobserved confounders can provide additional information about $\mathbb{E}[Y^0]$ and be used to better prioritize the most vulnerable individuals for treatment. These bounds can be adapted to reflect alternative historical decision logics if needed. For example, one may consider inverting the above condition if domain knowledge changes over time, or dropping the condition if there is no knowledge at all about the nature of the unobserved confounders.

3. Sample-Based Approximation

Given that $\mathbb{P}$, or even its marginals that can be estimated from data, are unknown, we cannot solve Problem (3) directly. However, unlike Problem (1), Problem (3) admits a sample-based approximation.

The following additional notation will be useful throughout this section. Recall that $\mu_i = \mathbb{P}(T=t_i \mid X=x_i, U=u_i)$ and let $\hat{\mu}_i$ represent an estimate of $\mathbb{P}(T=t_i|X=x_i)$, which can be learned from the data. Let $\hat{w}_i = 1/\hat{\mu}_i$, $w_i = 1/\mu_i$ and $w = (w_1, w_2, \ldots, w_n)$. As w is unknown and cannot be estimated from data, we treat it as an uncertain parameter, analogous to before, belonging to an uncertainty set that we will define shortly. Lastly, let $\mathcal{N} = \{1, \ldots, n\}$ be the index set of the data samples, and let $\mathcal{N}_0 = \{i \in \mathcal{N} : t_i = 0\}$ be the subset of indices with $t_i = 0$. Then, the sample-based approximation of (3) is given by

$$\sup_{\beta\in\mathcal{B}} \inf_{w\in\mathcal{U}(\widehat{\mathbb{P}}_n;\Gamma,\varepsilon)} \sum_{i\in\mathcal{N}_0} w_i \log \Pr_\beta(y_i \mid x_i), \tag{4}$$

where $\widehat{\mathbb{P}}_n = \frac{1}{n}\sum_{i\in\mathcal{N}} \delta_{x_i}$ is the empirical distribution, δ_{x_i} is the Dirac point mass at observation x_i, and

$$\mathcal{U}(\widehat{\mathbb{P}}_n;\Gamma,\varepsilon) = \left\{ \begin{array}{l} w \in \mathbb{R}_+^n : \\ \mathbb{Q} = \sum_{i\in\mathcal{N}_0} \frac{w_i}{\sum_{k\in\mathcal{N}_0} w_k} \delta_{x_i} \\ \frac{1}{n}\sum_{i\in\mathcal{N}_0} w_i = 1 \\ \mathbb{W}(\widehat{\mathbb{P}}_n, \mathbb{Q}) \le \varepsilon \\ \Gamma^{-1} \le \frac{1/\hat{w}_i}{1-1/\hat{w}_i} \frac{1-1/w_i}{1/w_i} \le \Gamma \ \ \forall i \in \mathcal{N}_0 \\ \sum_{i\in\mathcal{N}_0} y_i w_i \le \sum_{i\in\mathcal{N}_0} y_i \hat{w}_i \end{array} \right\}$$

for some $\Gamma \ge 1$ and $\varepsilon \ge 0$. $\mathcal{U}(\widehat{\mathbb{P}}_n;\Gamma,\varepsilon)$ encapsulates the conditions posed on the population level uncertainty set: the first two constraints capture (C2), the third captures (C1), the fourth captures (C3), and the fifth captures (C4). We relax (C2) to $\mathbb{W} \le \epsilon$ because, as noted above, empirical supports can differ even without confounding. The inner infimum problem in (4) is guaranteed to be feasible when $\varepsilon \ge \mathbb{W}(\widehat{\mathbb{P}}_n, \widehat{\mathbb{Q}})$, where $\widehat{\mathbb{Q}} = \sum_{i\in\mathcal{N}_0} \frac{\hat{w}_i}{\sum_{k\in\mathcal{N}_0} \hat{w}_k} \delta_{x_i}$, and the weights $\hat{w}$ are self-normalized, for example by using the Hájek IPW estimator (Hájek 1971, Hernan and Robins 2023), and greater than 1.

By recasting the Wasserstein distance as a transportation problem and employing strong duality arguments, we formulate problem (4) as an exponential cone program amenable to off-the-shelf solvers in the following proposition.

PROPOSITION 2. *Problem* (4)*, given that the inner infimum is feasible and* $\hat{w}_i \ge 1$ *for all* $i \in \mathcal{N}_0$*, admits an equivalent reformulation as the following exponential cone program:*

$$\begin{aligned} \sup \ & \sum_{i\in\mathcal{N}_0} \hat{w}_i \left(\Gamma^{-1}\underline{\theta}_i - \Gamma\overline{\theta}_i - \kappa y_i \right) \\ & + \sum_{i\in\mathcal{N}_0} \left((1-\Gamma^{-1})\underline{\theta}_i - (1-\Gamma)\overline{\theta}_i \right) \\ & -\varepsilon\tau + \frac{1}{n}\sum_{j\in\mathcal{N}} \varphi_j + n\rho \\ \text{s.t.} \ & \log \Pr_\beta(y_i \mid x_i) - \underline{\theta}_i + \overline{\theta}_i + \kappa y_i + \frac{\psi_i}{n} \ge \rho \ \ \forall i \in \mathcal{N}_0 \end{aligned} \tag{5}$$

$$\varphi_j + \psi_i \leq \|x_i - x_j\|\tau \;\; \forall i \in \mathcal{N}_0, j \in \mathcal{N}$$
$$\beta \in \mathcal{B},\, \underline{\theta}, \overline{\theta} \in \mathbb{R}^n_+,\, \tau, \kappa \in \mathbb{R}_+$$
$$\rho \in \mathbb{R},\, \varphi \in \mathbb{R}^n,\, \psi \in \mathbb{R}^n$$

Proposition 2 allows us to leverage off-the-shelf solvers to compute robust risk scores for certain choices of $\mathcal{B}$. For our experiments we consider continuous risk score coefficients, and implement and solve the exponential cone program using MOSEK ApS (2026). The number of decision variables grows linearly in $\max(d_{\mathrm{x}}, n)$, while the number of constraints grows quadratically in n. While this is considered tractable from a theoretical point of view, the quadratic number of constraints can practically create computational issues for datasets larger than those used in our experiments. In this case, the computation time can be improved via standard delayed constraint generation techniques.

4. Experiments

To demonstrate the effectiveness of our method, we run experiments on several datasets from the UCI machine learning repository (Dua and Karra Taniskidou 2017). We begin with a standard binary classification dataset with labels $y_i \in \{-1, 1\}$; note that such datasets are not observational and do not contain a treatment assignment component t_i. To convert this into a semi-synthetic *observational* dataset, we introduce a binary treatment variable $t_i \in \{0, 1\}$, and while doing so, systematically introduce unobserved confounding. We adapt datasets in this way so that we have access to both the ground truth and a confounding sample for each dataset; to the best of our knowledge there is no pre-existing repository of intentionally confounded observational datasets. By considering the labels y_i in the dataset to be the outcome y_i^0 under no treatment as well as an unobserved confounder, we generate a treatment policy $\pi^0(x, y^0)$ of the form

$$\log\left(\frac{\pi^0(x, y^0)}{1 - \pi^0(x, y^0)}\right) = f(x) + \gamma y^0,$$

where $f : \mathcal{X} \to \mathbb{R}$ and $\gamma \geq 0$, which indicates that the probability of no treatment to a unit with fixed x and a non-adversarial outcome $y^0 = 1$ is at least as high as that for the case of an adversarial outcome $y^0 = -1$. We take f to be an affine function of the observed covariates, and in the experiments we sample the corresponding coefficients $\lambda_1, \ldots, \lambda_{d_{\mathrm{x}}}$ independently and identically from a uniform distribution, i.e., $\lambda_1, \ldots, \lambda_{d_{\mathrm{x}}} \sim \mathrm{U}(-0.1, 0.1)$.

Note that under this policy, if two individuals i, j have identical observable features $x_i = x_j = x$, we have

$$\frac{\pi^0(x, y_i^0)}{1 - \pi^0(x, y_i^0)} \frac{1 - \pi^0(x, y_j^0)}{\pi^0(x, y_j^0)} = e^{\gamma(y_i^0 - y_j^0)}. \tag{6}$$

Thus, for two units with the same observable features, the difference in treatment odds relies only on γ and the difference in outcomes. Generating treatments in this manner gives us a reasonable intuition of how γ relates to Γ: since the difference in outcomes is bounded above and below by 2 and -2 respectively, we can approximately bound the ratio of the odds under the true propensity score μ_i and that under the estimated score $\hat{\mu}_i$ from below and above by $e^{-2\gamma}$ and $e^{2\gamma}$, respectively (see Appendix EC.3.3 for details).

We choose 15 classification datasets from the UCI repository for our experiments: details on dataset selection and preprocessing can be found in Appendix EC.3.2. 1500 training points and 500 test points are sampled from each dataset, and historical treatments are generated for the training set according to the above model for various choices of γ. We estimate propensity scores using logistic regression, gradient boosting, and a support vector machine, and let $\hat{\mu}$ denote the self-normalized scores from the model with the highest classification accuracy on a held-out validation set of 300 points. Specifically, we normalize such that the inverse weights $\hat{w}$ sum up to n and are all at least 1. Setting ε to be 1.025 times the Wasserstein distance between the entire training population and the $1/\hat{\mu}$-reweighted distribution among the untreated population, we then compute our robust risk scores by solving problem (5) with $\mathcal{B} = \mathbb{R}^{d_{\mathbf{x}}}$. We additionally run the following methods for comparison:

- Full Information: logistic regression on all data with access to counterfactual outcomes.
- Direct Method: logistic regression on $t_i = 0$ data with no propensity weighting.
- IPW: logistic regression on $t_i = 0$ data, weighted by $\hat{\mu}$.
- Doubly Robust[2] (DR): a combination of direct and IPW estimation (Robins et al. 1994).
- Robust Baseline: the exponential cone program reformulation of problem (4) without Wasserstein or historical rationality constraints, preserving (C1) and (C3).

We note that the Robust Baseline invokes an uncertainty set that consists purely of the odds-ratio bound, matching the uncertainty set used by Kallus and Zhou (2021) for policy regret minimization. To the best of our knowledge, this is the state of the art benchmark for our framework, as methods from e.g., Dorn et al. (2025) and Zhang et al. (2025) are

[2] Not to be confused with robustness to unobserved confounders.

γ		0.0			1.0			1.5			2.0		
Metric	Γ	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑
Full information		.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878
IPW		.255	.061	.787	.127	.146	.673	−.035	.201	.582	−.312	.253	.489
Direct		*.266*	*.059*	*.798*	.125	.157	.663	−.054	.221	.555	−.322	.279	.455
Doubly Robust		.249	.066	.776	.144	.139	.661	.017	.187	.573	−.219	.235	.483
Robust Baseline	e^1	.225	.094	.769	.141	.132	.648	.054	.170	.565	−.124	.215	.483
	e^2	.091	.177	.727	.084	.150	.622	.065	.156	.548	−.011	.180	.476
	e^3	.030	.207	.714	.032	.174	.610	.028	.160	.530	−.018	.167	.459
	e^4	.015	.211	.708	.011	.185	.603	.005	.169	.520	−.033	*.163*	.446
Robust	e^1	.238	.074	.795	*.174*	*.119*	*.700*	.073	.164	.612	−.109	.211	.504
	e^2	.186	.109	.768	.160	.121	.697	*.107*	*.145*	*.617*	.004	.177	*.511*
	e^3	.162	.117	.751	.144	.129	.692	.098	*.145*	.613	*.014*	.170	.508
	e^4	.156	.115	.742	.139	.131	.690	.092	.146	.608	.010	.167	.503

Table 1 Aggregate results across γ values for the following metrics: log likelihood improvement over random guess, expected calibration error, and Spearman's rank correlation with optimal policy. Italicized values indicate the best result for that metric within each γ group, excluding Full information: higher values are better for Improvement and Rank Corr., while lower values are better for Cal. Error. Results are reported for each choice of Γ. In all cases, $\Gamma = e^0$ reduces to the IPW method, and are excluded. Shaded cells mark the a priori choice $\Gamma = e^{2\gamma}$ for the Robust Baseline and Robust methods.

analytically tailored for bounding treatment effects. A complete set of ablations is reported in Appendix EC.4.1.

For coefficients β generated by any of the above methods, performance is evaluated using the following metrics:

- Log-likelihood improvement compared to a method that predicts a risk of 1/2 for all individuals. A value of 1 indicates that β yields zero log-loss, and 0 or below indicates that β is no better or worse than random guessing:

$$\text{Improvement} = 1 - \frac{\sum_{i \in \mathcal{N}_{\text{test}}} \log \Pr_\beta(y_i \mid x_i)}{|\,\mathcal{N}_{\text{test}}\,| \log 0.5}$$

- Expected calibration error, reported over 10 bins of equal width. A value of 0 indicates perfect calibration, while 1 indicates total miscalibration.
- Spearman's rank correlation between the rankings generated by β and those generated by the test-data-optimal risk scores, computed via logistic regression with full visibility into the test data, including access to all y_0 outcomes. A value of 1 indicates that β and the test-data-optimal scores generate identical rankings, whereas −1 indicates completely opposite rankings.

Results for 15 independent replications per dataset are collected in Table 1, aggregated via 10% trimmed mean across all runs. Further statistical analysis is reported in Appendix

EC.4.1. We first note that the full information benchmark has identical results regardless of γ, since this method has access to counterfactuals and can ignore the effect of confounders on treatment assignment. As γ increases, all other methods decrease in performance, as the treatment assignments (and thus the set of observations the methods have access to) become more confounded. We find that in general, setting Γ somewhere between e^{γ} and $e^{2\gamma}$ leads to our robust method having the best performance. For these choices of Γ, our robust method outperforms the non-full information benchmarks in all metrics; most notably, we see up to a 28.9% relative reduction in calibration error compared to the DR benchmark. We also see up to an 9.8% relative reduction in calibration error compared to the robust baseline, displaying the benefits of distributional consistency and historical rationality constraints.

Additional experiments that adjust the treatment generation method are reported in Appendix EC.4.2. We find that our method still performs well under a noisy nonlinear relation between y^0 and log treatment odds, and that C4 can be safely ablated in settings where historical decision makers did not use unobserved confounders to properly prioritize those most at risk.

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E-Companion

EC.1. Additional definitions

EC.1.1. 1-Wasserstein distance (Kantorovich and Rubinstein 1958)

The Wasserstein distance $\mathbb{W}(\mathbb{Q}_1, \mathbb{Q}_2)$ between two distributions $\mathbb{Q}_1$ and $\mathbb{Q}_2$ on $\Xi \subseteq \mathbb{R}^m$ is defined as

$$\mathbb{W}(\mathbb{Q}_1, \mathbb{Q}_2) = \inf_{\mathbb{M} \in \mathcal{M}(\mathbb{Q}_1, \mathbb{Q}_2)} \int_{\Xi^2} \|\xi_1 - \xi_2\| \, \mathbb{M}(\mathrm{d}\xi_1, \mathrm{d}\xi_2),$$

where $\mathcal{M}(\mathbb{Q}_1, \mathbb{Q}_2)$ denotes the set of all joint probability distributions of ξ_1 and ξ_2 with marginals $\mathbb{Q}_1$ and $\mathbb{Q}_2$, respectively.

EC.2. Proofs

EC.2.1. Proof of Lemma 1

We have

$$\begin{aligned}
&\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y^0 \mid X)] \\
=&\mathbb{E}_{\mathbb{P}}[\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y^0 \mid X) \mid X, U]] \\
=&\mathbb{E}_{\mathbb{P}}[\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y^0 \mid X) \mid X, U, T = 0]] \\
=&\mathbb{E}_{\mathbb{P}}[\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y \mid X) \mid X, U, T = 0]],
\end{aligned}$$

where the second and third equalities follow from Assumption 1 (iii) and (i), respectively. We further have

$$\begin{aligned}
&\mathbb{E}_{\mathbb{P}}[\mathbb{E}_{\mathbb{P}}[\log \Pr_\beta(Y \mid X) \mid X, U, T = 0]] \\
=&\mathbb{E}_{\mathbb{P}}\left[\frac{\mathbb{E}_{\mathbb{P}}\left[\log \Pr_\beta(Y \mid X) \mathbb{1}(T = 0) \mid X, U\right]}{\mathbb{P}(T = 0 \mid X, U)}\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[\frac{\log \Pr_\beta(Y \mid X) \mathbb{1}(T = 0)}{\mathbb{P}(T = 0 \mid X, U)}\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[\frac{\log \Pr_\beta(Y \mid X)}{\mathbb{P}(T = 0 \mid X, U)} \,\middle|\, T = 0\right] \mathbb{P}(T = 0),
\end{aligned}$$

where equalities follow from standard properties of conditional expectations.

EC.2.2. Proof of Proposition 1

Suppose that $Z = \mathbb{P}(T = 0 \mid X, U)$. By the definition of conditional expectation, we have

$$\mathbb{E}_{\mathbb{P}}\left[1/Z \mid T = 0\right] = \frac{\mathbb{E}_{\mathbb{P}}\left[\mathbb{1}(T = 0)/Z\right]}{\mathbb{P}(T = 0)}.$$

By $Z = \mathbb{P}(T=0 \mid X,U)$ and the properties of conditional expectation, we further have

$$\begin{aligned}
&\mathbb{E}_{\mathbb{P}}\left[\mathbb{1}(T=0)/Z\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[\mathbb{E}_{\mathbb{P}}\left[\mathbb{1}(T=0)/Z \mid X,U\right]\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[\mathbb{E}_{\mathbb{P}}\left[\mathbb{1}(T=0)/\mathbb{P}(T=0 \mid X,U) \mid X,U\right]\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[(1/\mathbb{P}(T=0 \mid X,U))\mathbb{E}_{\mathbb{P}}\left[\mathbb{1}(T=0) \mid X,U\right]\right] \\
=&\mathbb{E}_{\mathbb{P}}\left[(1/\mathbb{P}(T=0 \mid X,U))\mathbb{P}(T=0 \mid X,U)\right] = 1.
\end{aligned}$$

This shows that (C1) is satisfied.

For (C2), note that, by the definition of $\mathbb{Q}^{\mathrm{z}}$, for all $A \in \mathcal{B}(\mathcal{X})$,

$$\mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}(A) = \frac{\mathbb{E}_{\mathbb{P}}[\mathbb{1}(X \in A)/Z \mid T=0]}{\mathbb{E}_{\mathbb{P}}[1/Z \mid T=0]}.$$

Following similar steps to before, we obtain

$$\mathbb{E}_{\mathbb{P}}[\mathbb{1}(X \in A)/Z \mid T=0] = \frac{\mathbb{E}_{\mathbb{P}}[\mathbb{1}(X \in A)\mathbb{1}(T=0)/Z]}{\mathbb{P}(T=0)}$$

and

$$\begin{aligned}
&\mathbb{E}_{\mathbb{P}}[\mathbb{1}(X \in A)\mathbb{1}(T=0)/Z] \\
=&\mathbb{E}_{\mathbb{P}}[\mathbb{E}_{\mathbb{P}}[\mathbb{1}(X \in A)\mathbb{1}(T=0)/Z \mid X,U]] \\
=&\mathbb{P}(X \in A).
\end{aligned}$$

Therefore,

$$\begin{aligned}
\mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}(A) &= \frac{1}{\mathbb{E}_{\mathbb{P}}[1/Z \mid T=0]} \frac{\mathbb{P}(X \in A)}{\mathbb{P}(T=0)} \\
&= \mathbb{P}(T=0)\frac{\mathbb{P}(X \in A)}{\mathbb{P}(T=0)} = \mathbb{P}(X \in A) = \mathbb{P}_{\mathrm{x}}(A).
\end{aligned}$$

This equality holds for all $A \in \mathcal{B}(\mathcal{X})$ so the two probability distributions $\mathbb{Q}^{\mathrm{z}}_{\mathrm{x}}$ and $\mathbb{P}_{\mathrm{x}}$ are equal. Because of this and by the definition of the Wasserstein distance, (C2) is therefore satisfied.

EC.2.3. Proof of Proposition 2

As $\widehat{\mathbb{P}}_n$ and $\mathbb{Q}$ are both discrete distributions, the Wasserstein distance between them can be computed via the following optimal transport problem:

$$\begin{array}{lll}
\inf\limits_{\zeta} & \displaystyle\sum_{i \in \mathcal{N}_0}\sum_{j \in \mathcal{N}} \|x_i - x_j\|\zeta_{ij} & \\
\text{s. t.} & \displaystyle\sum_{i \in \mathcal{N}_0} \zeta_{ij} = \frac{1}{n} & \forall j \in \mathcal{N}
\end{array}$$

$$\begin{aligned}
&\sum_{j\in\mathcal{N}} \zeta_{ij} = \frac{1}{n} w_i && \forall i \in \mathcal{N}_0 \\
&\frac{1}{n}\sum_{i\in\mathcal{N}_0} w_i = 1 \\
&w \in \mathbb{R}^n_+,\, \zeta \in \mathbb{R}^{n\times n}_+,
\end{aligned}$$

where ζ_{ij} represents the amount of probability mass transported from x_i to x_j in order to transform $\mathbb{Q}$ to $\widehat{\mathbb{P}}_n$. Note that the third constraint above requires that any feasible realization of weights reweight the untreated population to the correct full-population scale, thereby requiring a sample-based version of (C1) to hold.

We therefore have the following equivalent reformulation of problem (4).

$$\begin{aligned}
\sup_{\beta\in\mathcal{B}} \inf_{w,\zeta} \quad & \sum_{i\in\mathcal{N}_0} w_i \log \Pr_\beta(y_i \mid x_i) \\
\text{s.t.} \quad & \Gamma^{-1}(\hat{w}_i - 1) \le w_i - 1 \le \Gamma(\hat{w}_i - 1) && \forall i \in \mathcal{N}_0 \\
& \sum_{i\in\mathcal{N}_0}\sum_{j\in\mathcal{N}} \|x_i - x_j\| \zeta_{ij} \le \varepsilon \\
& \sum_{i\in\mathcal{N}_0} \zeta_{ij} = \frac{1}{n} && \forall j \in \mathcal{N} \\
& \sum_{j\in\mathcal{N}} \zeta_{ij} = \frac{1}{n} w_i && \forall i \in \mathcal{N}_0 \\
& \frac{1}{n}\sum_{i\in\mathcal{N}_0} w_i = 1 \\
& \sum_{i\in\mathcal{N}_0} y_i w_i \le \sum_{i\in\mathcal{N}_0} y_i \hat{w}_i \\
& w \in \mathbb{R}^n_+,\, \zeta \in \mathbb{R}^{n\times n}_+
\end{aligned}$$

Notably, the inner infimum is now linear in w and ζ. Applying strong duality to the inner infimum gives a supremum problem which can be merged with the outer supremum, thus resulting in an exponential cone problem. Introducing dual variables $\underline{\theta}_i, \overline{\theta}_i, \tau, \varphi_j, \psi_i, \kappa$ for each constraint, respectively (noting that the first constraint corresponds to both θ terms), and replacing the inner infimum with its dual supremum yields the problem stated in Proposition 2.

EC.3. Experimental Details

EC.3.1. Compute resources

All experiments are implemented on a cloud computing platform with a mixture of the following compute nodes:

- AMD EPYC 7513 CPU @ 2.60 GHz, 248 GB RAM
- AMD EPYC 7542 CPU @ 2.90 GHz, 248 GB RAM
- INTEL XEON Silver 4116 CPU @ 2.10 GHz, 185 GB RAM
- INTEL XEON Silver 4116 CPU @ 2.10 GHz, 89 GB RAM

EC.3.2. Datasets

To choose datasets to run experiments on, we select classification datasets from the UCI repository for which a logistic relation is a decent, but not perfect explanation (i.e., cross-validated log loss between 0.35 and $\log 2$, the log-loss of random guess). If a classification dataset is already extremely well explained by a logistic relation, then baseline methods that do not leverage propensity scores would already suffice to explain the corresponding observational dataset, with no dependence on the historical treatment policy. Since this would not be compatible with a setting where unobserved confounders influenced both the historical treatment policy and the outcomes, datasets with log loss below 0.35 are not considered. We thus acquire the following datasets:

Abalone, Adult, Connect-4, Covertype, Default of Credit Card Clients, EEG Eye State, Electrical Grid Stability Simulated Data, NHANES Age Prediction Subset, In-Vehicle Coupon Recommendation, Letter Recognition, MAGIC Gamma Telescope, Online News Popularity, Pen-Based Recognition of Handwritten Digits, Spambase, Wine Quality.

For each dataset, target classes are relabeled with $\{-1, 1\}$: datasets with more than 2 classes accordingly have certain classes merged to reflect binary outcomes, choosing to merge similar classes on a per-domain basis when possible. Categorical features are binarized, numerical features are scaled to $[0, 1]$, then all features are centered around their mean. Doing this prevents the treatment model from having a very large random intercept on datasets with many features.

EC.3.3. Approximately relating Γ to γ

Under the treatment generation model of our experimental section, we have

$$\begin{aligned}\frac{\mu_i^0}{1-\mu_i^0}\frac{1-\hat{\mu}_i^0}{\hat{\mu}_i^0} \approx & \frac{\pi^0(x_i, y_i^0)}{1-\pi^0(x_i, y_i^0)}\frac{1-\mathbb{E}\left[\pi^0(x_i, Y^0)\right]}{\mathbb{E}\left[\pi^0(x_i, Y^0)\right]} \\ \leq & \frac{\pi^0(x_i, y_i^0)}{1-\pi^0(x_i, y_i^0)}\mathbb{E}\left[\frac{1-\pi^0(x_i, Y^0)}{\pi^0(x_i, Y^0)}\right] \\ = & \mathbb{E}\left[e^{\gamma(y_i^0 - Y^0)}\right] \leq e^{2\gamma}\end{aligned}$$

and

$$\begin{aligned}\frac{\mu_i^0}{1-\mu_i^0}\frac{1-\hat{\mu}_i^0}{\hat{\mu}_i^0} =& 1 \Big/ \frac{1-\mu_i^0}{\mu_i^0}\frac{\hat{\mu}_i^0}{1-\hat{\mu}_i^0} \\ \approx& 1 \Big/ \frac{1-\pi^0(x_i,y_i^0)}{\pi^0(x_i,y_i^0)}\frac{\mathbb{E}\left[\pi^0(x_i,Y^0)\right]}{1-\mathbb{E}\left[\pi^0(x_i,Y^0)\right]} \\ \geq& 1 \Big/ \frac{1-\pi^0(x_i,y_i^0)}{\pi^0(x_i,y_i^0)}\mathbb{E}\left[\frac{\pi^0(x_i,Y^0)}{1-\pi^0(x_i,Y^0)}\right] \\ =& \mathbb{E}\left[e^{-\gamma(y_i^0-Y^0)}\right] \geq e^{-2\gamma},\end{aligned}$$

where all expectations are conditional on $X = x_i$, and the inequalities follow from Jensen's inequality and $0 < \pi^0 < 1$. Thus, we expect $e^{2\gamma}$ to be the highest reasonable value (and $e^{-2\gamma}$ the lowest) for Γ in the uncertainty set $\mathcal{U}$.

EC.4. Further experimental results

EC.4.1. Ablations and statistical reporting

We report the full set of results from the experimental setup in the main paper. For ablations, we consider the removal of C2, C4, and both simultaneously (referred to as Robust Basline in the main paper). For statistical reporting, we first aggregate each metric across datasets for each random seed, then compute standard deviations over the aggregate metrics. Doing this restricts the statistical analysis to replication noise - if we were to compute standard deviations over all results separately, the values would be dominated by dataset heterogeneity.

Results are presented in Table EC.1. We find that removing either C2 or C4 degrade performance, as the uncertainty sets become larger, leading to more conservative predictions. In particular, we find that the ablations deteriorate more quickly when Γ is poorly calibrated: when $\gamma = 1$, setting $\Gamma = e^4$ causes the methods without C2 to produce a log likelihood nearly as poor as that of random guessing.

EC.4.2. Different historical treatment policies

The experiments in the main paper assume a positive linear relation between the unobserved confounder and the log odds ratio of treatment. In this section, we consider two alternative historical policies. In the first setting, the term γ is instead sampled from the normal distribution $\mathcal{N}(\gamma, \sigma^2)$ for a given spread σ. In the second setting, the term γ is instead replaced with $-\gamma$ with probability p. For each of these experiments, we fix $\gamma = 1$ and $\Gamma = e$, and inspect the effect of the hyperparameters σ and p respectively.

γ		0.0			1.0			1.5			2.0		
Metric	Γ	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑	Imp.↑	Cal.↓	Corr.↑
Full information		.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878
		.009	.003	.011	.009	.003	.011	.009	.003	.011	.009	.003	.011
IPW		.255	.061	.787	.127	.146	.673	−.035	.201	.582	−.312	.253	.489
		.023	.004	.021	.074	.023	.044	.119	.028	.052	.208	.033	.055
Direct		*.266*	*.059*	*.798*	.125	.157	.663	−.054	.221	.555	−.322	.279	.455
		.018	*.004*	*.019*	.065	.023	.043	.112	.030	.054	.183	.034	.054
Doubly Robust		.249	.066	.776	.144	.139	.661	.017	.187	.573	−.219	.235	.483
		.025	.006	.022	.069	.018	.044	.098	.023	.051	.208	.029	.055
Robust	e^1	.238	.074	.795	*.174*	*.119*	*.700*	.073	.164	.612	−.109	.211	.504
		.019	.005	.023	*.045*	*.017*	*.040*	.079	.024	.054	.146	.027	.056
	e^2	.186	.109	.768	.160	.121	.697	*.107*	*.145*	*.617*	.004	.177	*.511*
		.022	.006	.027	.034	.010	.040	*.054*	*.016*	*.057*	.096	.020	*.053*
	e^3	.162	.117	.751	.144	.129	.692	.098	*.145*	.613	*.014*	.170	.508
		.017	.006	.026	.028	.008	.041	.047	*.014*	.056	*.081*	.018	.054
	e^4	.156	.115	.742	.139	.131	.690	.092	.146	.608	.010	.167	.503
		.015	.005	.027	.027	.007	.041	.046	.013	.055	.080	.017	.056
Robust w/o C2	e^1	.231	.079	.789	.172	*.119*	.697	.073	.163	.611	−.110	.210	.499
		.021	.006	.024	.046	*.017*	.041	.080	.024	.055	.148	.028	.055
	e^2	.132	.120	.735	.126	.132	.680	.088	*.145*	.602	.003	.173	.496
		.031	.011	.033	.037	.010	.038	.053	*.013*	.056	.089	.018	.056
	e^3	.081	.124	.704	.074	.149	.661	.050	.150	.586	−.008	.162	.491
		.017	.013	.032	.031	.010	.038	.046	.010	.058	.072	.011	.062
	e^4	.069	.121	.692	.052	.155	.652	.026	.158	.579	−.025	*.159*	.482
		.011	.013	.030	.024	.009	.033	.042	.010	.057	.067	*.007*	.064
Robust w/o C4	e^1	.233	.088	.773	.145	.130	.656	.055	.170	.570	−.121	.215	.488
		.020	.008	.021	.044	.014	.043	.073	.019	.048	.142	.025	.050
	e^2	.151	.142	.745	.122	.137	.641	.088	.155	.566	−.009	.183	.490
		.027	.010	.024	.027	.006	.039	.044	.010	.047	.092	.017	.048
	e^3	.115	.161	.729	.105	.145	.633	.079	.155	.559	.005	.176	.484
		.020	.008	.024	.022	.006	.040	.037	.009	.047	.078	.014	.048
	e^4	.108	.164	.724	.100	.147	.632	.073	.156	.555	.001	.173	.479
		.017	.007	.021	.020	.006	.040	.036	.008	.047	.076	.013	.049
Robust w/o C2, C4	e^1	.225	.094	.769	.141	.132	.648	.054	.170	.565	−.124	.215	.483
		.022	.009	.022	.043	.013	.043	.072	.018	.048	.143	.025	.049
	e^2	.091	.177	.727	.084	.150	.622	.065	.156	.548	−.011	.180	.476
		.038	.016	.025	.030	.007	.038	.043	.008	.047	.084	.014	.048
	e^3	.030	.207	.714	.032	.174	.610	.028	.160	.530	−.018	.167	.459
		.020	.008	.022	.024	.010	.041	.034	.007	.054	.066	.008	.052
	e^4	.015	.211	.708	.011	.185	.603	.005	.169	.520	−.033	.163	.446
		.013	.007	.021	.017	.009	.038	.030	.007	.055	.061	.006	.052

Table EC.1 Aggregate results (standard deviations below) across γ values for the following metrics: log likelihood improvement over random guess, expected calibration error, and Spearman's rank correlation with optimal policy. Italicized values indicate the best result for that metric within each γ group: higher values are better for Improvement and Rank Corr., while lower values are better for Cal. Error. Results are reported for each choice of Γ for each ablation. In all cases, $\Gamma = e^0$ reduces to the IPW method, and are excluded.

Results are reported in Tables EC.2 and EC.3. When tuning σ, we find that adding more noise to the system causes all predictions (except the full information oracle) to degrade in

σ / Metric	0.2			0.4			0.6			0.8			1.0		
	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑
Full information	.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878
	.009	.003	.011	.009	.003	.011	.009	.003	.011	.009	.003	.011	.009	.003	.011
IPW	.131	.141	.674	.121	.135	.673	.101	.134	.674	.066	.137	.668	.028	.142	.659
	.067	.022	.046	.082	.023	.043	.093	.025	.040	.115	.030	.047	.151	.034	.055
Direct	.132	.149	.667	.125	.143	.667	.104	.143	.666	.068	.147	.659	.026	.155	.649
	.060	.022	.045	.068	.023	.043	.079	.026	.041	.103	.032	.047	.137	.038	.058
Doubly Robust	.147	.135	.663	.147	.128	.667	.130	.128	.663	.102	.131	.659	.072	.135	.651
	.064	.018	.047	.072	.020	.041	.084	.023	.039	.112	.026	.047	.150	.030	.055
Robust	*.175*	*.116*	*.698*	*.169*	*.114*	*.698*	*.160*	*.115*	*.691*	*.133*	*.123*	*.678*	*.105*	*.129*	*.665*
	.040	*.016*	*.044*	*.052*	*.018*	*.041*	*.059*	*.020*	*.040*	*.081*	*.026*	*.050*	*.121*	*.031*	*.057*
Robust w/o C2	.173	*.116*	.696	.166	.115	.694	.158	.117	.688	.131	.124	.673	.103	.131	.661
	.040	*.016*	.044	.052	.018	.044	.059	.020	.041	.080	.026	.051	.121	.030	.060
Robust w/o C4	.150	.129	.658	.149	.127	.663	.144	.128	.662	.124	.131	.657	.102	.134	.652
	.039	.013	.044	.051	.015	.042	.057	.017	.041	.076	.021	.047	.115	.025	.054
Robust w/o C2, C4	.145	.131	.651	.145	.129	.656	.140	.130	.657	.121	.133	.652	.099	.137	.646
	.039	.013	.046	.051	.015	.041	.057	.017	.040	.074	.020	.047	.115	.024	.054

Table EC.2 Aggregate results (standard deviations below) for different noise parameters σ, reported at $\gamma = 1$ and $\Gamma = e^1$. When $\sigma = 0$, the results match those from the appropriate rows in Table EC.1.

performance, with the Robust method still performing the best. When tuning p, we find that Robust performs the best for low p, Robust w/o C4 performs the best for high p, and the baselines perform the best in between. We propose the following intuition: settings with low p are aligned with the historical rationality assumption, and settings with high p would reflect a "historical irrationality" setting. When $p \approx 0.5$, the effect of y^0 on the historical treatment decision is effectively random noise, leading all Robust methods to be overly conservative.

EC.5. Future work

Several aspects of our framework naturally point to directions for future work. The choice of $\mathcal{B}$ is intentionally modular, such that additional integrality or sparsity constraints can be included if desired. Adding these additional constraints may require additional computational strategies to efficiently solve the resulting problem. We also note that our framework allows for different objective functions beyond maximizing log-likelihood, which points to the possibility of tractably and robustly learning quantities beyond risk scores.

p		0.2			0.4			0.6			0.8			1.0	
Metric	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑	I↑	C↓	R↑
Full information	.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878	.329	.050	.878
	.009	.003	.011	.009	.003	.011	.009	.003	.011	.009	.003	.011	.009	.003	.011
IPW	.215	.098	.744	.255	.066	.777	.250	.065	.795	.215	.095	.768	.133	.137	*.712*
	.036	.012	.028	.023	.005	.022	.020	.005	.018	.028	.011	.022	.053	.018	*.032*
Direct	.222	.101	.745	*.262*	*.065*	.784	*.260*	*.063*	*.805*	.222	.098	*.770*	.127	.149	.702
	.030	.011	.027	*.019*	*.005*	.022	*.017*	*.004*	*.018*	.025	.012	*.024*	.053	.021	.037
Doubly Robust	.215	.096	.733	.249	.069	.767	.248	*.063*	.786	.226	*.080*	.762	.167	.119	.704
	.034	.008	.028	.022	.007	.022	.019	*.004*	.020	.028	*.007*	.021	.048	.015	.031
Robust	*.226*	*.085*	*.754*	.245	.074	*.795*	.234	.079	.785	.202	.108	.731	.137	.149	.650
	.026	*.006*	*.025*	.018	.004	*.019*	.017	.006	.024	.025	.011	.030	.046	.020	.040
Robust w/o C2	.221	.086	.751	.239	.078	.790	.226	.084	.778	.195	.112	.721	.131	.152	.642
	.027	.007	.025	.019	.005	.021	.019	.005	.025	.027	.012	.032	.048	.020	.043
Robust w/o C4	.201	.101	.729	.234	.088	.769	.237	.083	.782	*.227*	.086	.760	*.192*	*.111*	.702
	.028	.008	.029	.019	.006	.022	.014	.005	.019	*.019*	.006	.021	*.029*	*.012*	.030
Robust w/o C2, C4	.194	.105	.721	.226	.095	.762	.229	.088	.777	.221	.090	.752	.189	.113	.693
	.030	.008	.030	.021	.007	.021	.016	.006	.020	.020	.006	.022	.030	.012	.031

Table EC.3 Aggregate results (standard deviations below) for different negation chances p, reported at $\gamma = 1$ and $\Gamma = e^1$. When $p = 0$, the results match those from the appropriate rows in Table EC.1.