Doing Great at Estimating CATE? On the Neglected Assumptions in Benchmark Comparisons of Treatment Effect Estimators

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Abstract

The machine learning toolbox for estimation of heterogeneous treatment effects from observational data is expanding rapidly, yet many of its algorithms have been evaluated only on a very limited set of semi-synthetic benchmark datasets. In this paper, we show that even in arguably the simplest setting – estimation under ignorability assumptions – the results of such empirical evaluations can be misleading if (i) the assumptions underlying the data-generating mechanisms in benchmark datasets and (ii) their interplay with baseline algorithms are inadequately discussed. We consider two popular machine learning benchmark datasets for evaluation of heterogeneous treatment effect estimators – the IHDP and ACIC2016 datasets – in detail. We identify problems with their current use and highlight that the inherent characteristics of the benchmark datasets favor some algorithms over others – a fact that is rarely acknowledged but of immense relevance for interpretation of empirical results. We close by discussing implications and possible next steps.

1. Introduction

Successful estimation of treatment effects (TEs) from observational data is contingent on a range of assumptions on underlying data-generating processes (DGPs). Arguably the most essential class of assumptions in causal inference is concerned with identification of effects. Such assumptions, e.g. the strong ignorability conditions [34] or the backdoor criterion [31], render a TE identifiable – yet they usually have no testable implications, making their verification a task requiring domain expertise [32]. Another class of assumptions, which is less often the source of controversy, is statistical in nature and mainly linked to estimation performance; e.g. assumptions on overlap, error terms, and – in particular – the form and complexity of underlying regression functions in the DGP determine the (expected) relative performance of different algorithms on a dataset. While the former class thus fundamentally ensures that any estimate can be interpreted as causal, the latter class of assumptions is crucial in practice when it comes to choosing an algorithm from the ever-expanding machine learning (ML) toolbox for heterogeneous treatment effect (HTE) estimation.

When new algorithms are proposed within the ML community, it is common to evaluate their performance against existing baseline algorithms on benchmark datasets, which, in other areas of ML, often consist of real data e.g. ImageNet [11]. When trying to evaluate estimators of HTEs on real data, the fundamental problem is that – even if identifying assumptions hold – ground truth individual TEs are never observed [19]. Thus, to provide proof-of-concept, papers proposing new (H)TE estimators have largely relied on synthetic or semi-synthetic datasets to showcase their properties. Possibly to discourage authors to invent DGPs that put their own algorithms in the best light, a few of these (semi-synthetic) datasets have emerged as benchmarks which have been used in a wide range of ML papers in the last years. The most prominent example of this is [17]’s IHDP dataset; since its first use in [21, 36], it has become the standard benchmark dataset in the ML HTE literature (used in e.g. [3, 1, 15, 16, 40, 4, 9, 41]). Unfortunately, this reliance on standard benchmark datasets and common baseline algorithms has seemingly removed the pressure on authors to (i) examine assumptions underlying such DGPs and (ii) argue why a specific benchmark comparison is fair and insightful in any given context.

Motivating example. A popular baseline algorithm for HTE estimation from the statistics community is [38, 6]’s Causal Forest (CF), which – unlike most ML estimators – targets the TE directly, comes with a set of theoretical guarantees and has already been applied in real empirical studies [5, 10]. Nonetheless, [36] (and many extensions, e.g. [4, 22, 39]) show that the neural network (NN) based TARNet and its many variants outperform CF by lengths on the IHDP dataset. We asked ourselves a simple question: Why? Is it because TARNet is a uniformly better estimator?
Or is it because TARNet is a NN while CF is a random forest (RF)? Is it maybe because TARNet models the potential outcomes, while CF models the TE directly? Or is it something else inherent to DGP or implementation? Such questions are by no means unique to this specific example and arise in many more ML HTE papers – in fact, missing insights into the sources of performance differences is a problem of the ML community at large [26]. Therefore, these questions only illustrate that it is crucial to build better understanding of how to interpret empirical findings in light of the assumptions underlying both benchmark datasets and baseline algorithms.

**Outlook.** In this paper, we argue that the recent ML literature on HTE estimation has largely neglected how assumptions, inherent to the semi-synthetic DGPs used for testing, influence benchmark comparisons of newly proposed and baseline algorithms. To substantiate this, we present two case studies using popular benchmark datasets – the IHDP dataset [17] and the ACIC2016 simulations [12] – in which we compare the empirical performance of a number of RF- and NN-based HTE estimators as illustrative examples. By doing so, we aim to understand to what extent some observed performance differences are simply a product of the interplay between (i) the assumptions underlying the semi-synthetic DGPs of the datasets and (ii) the assumptions underlying the compared algorithms, giving some algorithms an expected advantage that should be disclosed if the goal is fair comparison. In passing, we also highlight other problems with the current execution of benchmark comparisons. Ultimately, we aim to build greater understanding of what such (semi-synthetic) benchmark comparisons can and cannot establish, and therefore close by discussing practical implications for the ML HTE literature.

### 2. Problem Setup, Assumptions and Their Interplay with Learning Algorithms

In this paper, we operate under the standard setup in the potential outcomes (PO) framework [35]. That is, we assume that any individual, associated with (pre-treatment) covariates $X \in \mathcal{X}$, has two potential outcomes $Y(0)$ and $Y(1)$ of which only $Y = Y(W)$, the outcome associated with the administered (binary) treatment $W \in \{0, 1\}$, is observed. We are interested in $\tau(x)$, the conditional average treatment effect (CATE), which is the expected difference between an individual’s POs (conditional on covariates), i.e.

$$\tau(x) = \mathbb{E}[Y(1) - Y(0) | X = x] = \mu_1(x) - \mu_0(x) \quad (1)$$

where $\mu_w(x) = \mathbb{E}[Y(w) | X = x]$ is the expected PO.

**Identifying assumptions.** We aim to reason about the expected properties of a number of ML-based (or nonparametric) estimators of CATE, all of which rely on the effect being identified. As our main interest here lies not in identification but in a different class of assumptions (see below), we therefore rely on the strong ignorability conditions [34] for convenience (with the understanding that they may be limiting in practical applications).

#### 2.1. Key assumptions on the POs

Instead, our core interest lies in how assumptions on the form of and relationship between the two PO regression surfaces affect the relative performance of different estimators. In DGPs used for testing CATE estimators, such assumptions manifest in how the $\mu_w(x)$ are modeled. In most general form, one can always let

$$\mu_w(x) = \begin{cases} f_0(x) & \text{if } w = 0 \\ f_1(x) & \text{if } w = 1 \end{cases} \quad (2)$$

for $f_0, f_1$ some functions, which could be arbitrarily different. While possible in theory, having no relationship between the expected outcomes under different treatments seems highly unrealistic in practice. In medicine, for example, one often assumes that some biomarker information is *prognostic* of outcome regardless of treatment status, such that only a subset of markers is *predictive* of effect heterogeneity [7]. A common approach to simulating response surfaces is therefore to simply use additive effects,

$$\mu_w(x) = f_0(x) + w f_\tau(x) \quad (3)$$

that is, to assume that the treated regression surface $f_1(x)$ in (2) can be *additively decomposed* into a component shared with the control group ($f_0(x)$) and $f_\tau(x)$, an offset function determining treatment effect and heterogeneity, which could be simpler (e.g. smoother or sparser) than $f_1(x)$ itself.

Instead of additive transformations, it would also be possible that the PO regression surfaces have more general relationships, e.g. $f_1(x) = g(f_0(x))$ with $g(\cdot)$ some transformation function – in the IHDP dataset, for example, $g(\cdot)$ is a logarithmic transformation – however, this specification seems less popular than additive parametrizations used in most DGPs in related work.

#### 2.2. Learning Algorithms and the interplay with DGPs

A plethora of ML-based CATE estimators have been proposed in recent years. Here, we distinguish them along two key axes most relevant to our problem: (i) the underlying ML method and (ii) the estimation strategy. The former is straightforward and refers simply to the ML method used to implement an algorithm, e.g. a NN or a RF. The latter is crucial in the CATE context but has received relatively little explicit attention in the ML literature. As in [9], we distinguish between two types of estimation strategy: *indirect* estimators that target the POs, i.e. first obtain estimates $\hat{\mu}_w(x)$ and then simply set $\hat{\tau}(x) = \hat{\mu}_1(x) - \hat{\mu}_0(x)$, and estimators
that target CATE directly, e.g. by using pseudo-outcome regression or other two-step procedures [25, 30, 23, 9].

From a theoretical viewpoint, we expect that the type of underlying DGP will (at least partially) determine which combination of ML method and estimation strategy will be most successful on any benchmark dataset. To see this, let \( \epsilon_{SQ}(\hat{f}(X)) = E[(\hat{f}(X) - f(X))^2] \) denote the expected MSE for an estimate \( \hat{f}(x) \) of a function \( f(x) \) and consider the behaviour of \( \epsilon_{SQ}(\hat{f}(X)) \) for different strategies. For indirect estimators, we have that \( \epsilon_{SQ}(\hat{f}(X)) = 2(\epsilon_{SQ}(\hat{f}_1(X)) + \epsilon_{SQ}(\hat{f}_0(X))) \), so that the error rate on the more complex of the regression surfaces will determine performance [1, 9]. Some direct estimators, on the other hand, can instead reach the same performance as a supervised learning algorithm with (hypothetical) target \( Y(1) - Y(0) \), so that they can attain the error rate associated with \( f_r(x) \) (see [9, 23, 25]). Whenever \( f_r(x) \) is easier to estimate than the more complex of \( f_1(x) \) and \( f_0(x) \) (e.g. due to being sparser or smoother [9, 23]), direct learners thus have a clear theoretical advantage – which diminishes as \( f_1(x) \) and \( f_0(x) \) become less similar (making \( f_r(x) \) increasingly complex). Further, the properties of the underlying ML method used to implement any estimation strategy will determine how well different types of functions \( f_w(x) \) and/or \( f_r(x) \) can be fit using a finite sample of observed data from a specific DGP.

3. Empirical investigation

In this section we present two case studies comparing the empirical performance of estimators relying on (i) different ML-methods (NNs and RFs) and (ii) different estimation strategies (direct and indirect). We begin with our motivating example and examine the sources of performance differences between the RF-based, direct estimator Causal forest (CF) [38, 6] and the NN-based, indirect estimator TARNet [36] on the IHDP dataset. Second, we consider relative performance of the same algorithms on a subset of the ACIC2016 simulations.

Models and implementation. We consider two (model-agnostic) indirect estimation strategies which are commonly referred to as T- and S-learner [25]: the former fits two separate regression surfaces for each treatment arm, while the latter fits a single model in which \( W \) is included as a standard covariate. We consider standard RF-based implementations (TRF and SRF) and a NN-based T-learner (TNet). Instead of a standard NN-based S-learner we use TARNet (as it can be seen as a hybrid of S- and T-learner [9]). As direct estimators, we consider [6]’s CF, which relies on a two-stage procedure solving a local moment equation inspired by the Robinson transformation [33], and, as a NN-based variant, we use [30]’s R-learner (RNet), which relies on the same principle. For all forest-based methods we use the R-package grf [37] and for all NN-based methods we use the python-implementation catenets [9]. We use all models off-the-shelf; for all RFs this entails using 2000 trees, and all NNs have hyperparameter settings similar to those used in [36] for the IHDP experiments. For further details, refer to the the Appendix.

3.1 Case study 1: IHDP dataset

The IHDP benchmark dataset uses a semi-synthetic DGP on top of the covariates \( n = 747, d = 25 \) and treatments of the Infant Health and Development Program, a randomized experiment targeting an intervention at premature infants with low birth weight. [17] introduced selection bias and imbalance \( (n_0 = 608, n_1 = 139) \) by excluding a non-random proportion of treated individuals (those with nonwhite mothers), leading to incomplete overlap for the control group. The popular semi-synthetic DGP (setup ‘B’ in [17]) uses \( \mu_0(x) = \exp((x + A)(\beta)) \) and \( \mu_1(x) = x\beta - \omega; \) \( \beta \) is a coefficient vector with entries sampled from \( (0, 0.1, 0.2, 0.3, 0.4) \) with probabilities \( (0.6, 0.1, 0.1, 0.1, 0.1, 0.1) \), \( A \) is a fixed offset matrix, and \( \omega \) is set uniquely in every simulation run, ensuring that the average treatment effect on the treated (ATT), which was the main estimand of interest in [17], is equal to 4. Note that this DGP was not created to mimic a specific realistic response surface, rather it was one of multiple DGPs used in [17] to compare different estimators. Below, we use [36]’s IHDP-100 dataset (100 draws of the DGP) and report performance on the pre-determined 10% hold-out sample. We repeat each run 5 times with different seeds for all models.

3.1.1. Empirical results

• Finding (i): Reporting simple averages of RMSE across simulation runs appears inappropriate. We begin with a general observation we consider crucial for anyone using the IHDP dataset for benchmark comparisons. In Fig. 2 we plot a histogram of the average RMSE across the 100 realizations of the simulation, in which it is obvious that the right tail of scores very long and heavy. Averaging over such scores gives ex-
Finding (ii): Indirect strategies perform best on this dataset and NNS outperform RFs. In the left panel of Fig. 1(a), we plot RMSE by $\sigma_T$ for TARNet, TNet, TRF and CF on IHDP-100. We omit RNet and SRF for readability, refer to the appendix for full results. We observe that the direct estimators consistently perform worse than their indirect alternatives. This is expected given the DGP and the theoretical arguments made in the previous section: $\mu_0(x)$ and $\mu_1(x)$ are not similar (on an additive scale) in the underlying DGP, making $\tau(x)$ a difficult function to estimate directly. Further, considering only the left panel of Fig. 1(a), it appears as if NN-based estimators have a clear advantage on this dataset and the underlying ML-method seems to matter much more than the CATE estimation strategy used. TARNet also consistently outperforms TNet; this is not surprising given that the linear predictor $X\beta$ is shared across both outcome surfaces, making for a perfect shared representation that TARNet can exploit.

Finding (iii): Relative performance systematically differs across runs. Note that simulation runs in which more covariates have (large) nonzero coefficients will have higher TE heterogeneity due to the exponential specification. In the left panel of Fig. 1(a), it is obvious that as the measured variation in CATE increases, the absolute discrepancy between methods becomes more extreme. Due to high differences in magnitude, this perspective masks more interesting differences that become most apparent when considering relative differences using only the simulation runs with less extreme $\sigma_T$. In the right panel of Fig. 1(a) we observe that the forest-based estimators perform better than the NN-based versions for $\sigma_T$ small, and that the discrepancy between direct and indirect learners is less extreme.

Finding (iv): Tree-based methods suffer in the tails due to the exponential. The performance differences highlighted above let us speculate that the poor performance of the forest-based estimators (which can be thought of as adaptive nearest neighbor methods [38]) may be partially caused by boundary bias on exponential specifications. We confirm this in Fig. 3 where we plot T-learner estimates of $\tau(x)$ for the simulation runs at the 10, 50 and 90th percentiles of $\sigma_T$. We observe that for large $\sigma_T$, major performance discrepancies indeed arise only for the small subset of individuals with the largest linear predictor, which is where the exponential is the steepest.

Finding (v): Tweaking the DGP slightly by creating an additive TE leads to completely different results. Finally, to further test finding (ii), i.e. whether the observed performance differences across strategies are indeed due to the CATE function being as difficult to estimate as the $\mu(x)$, we slightly alter the original IHDP simulation. We use $\mu_0(x) = \mu_0(x)$ and $\mu_1(x) = \mu_1(x) + \mu_0(x)$; i.e. the treatment effect is now additive and simple (linear). In Fig. 1(b) we report RMSE of estimating CATE by $\sigma_{\mu_0}$ (as we found the variance induced by the exponential specification in $\mu_0(x)$, and not $\sigma_T$, to drive variation across runs). We observe that in this setting, almost all conclusions on relative performance are indeed reversed from what we observed in 1(a): CF performs best throughout, direct learners perform better and NNs no longer have a clear advantage over RFs (possibly because the boundary bias now appears on both regression surfaces, which can difference out).

true and predicted CATE for TNet and TRF, on 3 datasets with $\sigma_T$ at 10, 50 and 90th percentile across IHDP runs.
3.1.2. Conclusion case study 1

The empirical investigation above allows us to resolve our questions asked in the motivating example: The advantage of TARNet over CF on the IHDP dataset has multiple sources; the DGP underlying the IHDP dataset indeed favours both (i) NNs over RFs and (ii) indirect over direct estimators. Further, CF performs worse on runs with extreme $\sigma_{\tau}$, which effectively get much higher weight than the runs with very low $\sigma_{\tau}$ where it performs best. When tweaking the DGP slightly to create a simple and additive treatment effect, the relative performance reverses, highlighting that performance is indeed determined by the interplay between assumptions underlying a learning algorithm and the DGP.

3.2. Case study 2: ACIC2016

The datasets used in the Atlantic Causal Inference Competition (ACIC) 2016 are based on real covariates ($n = 4802, d = 58$) from the Collaborative Perinatal Project. The competition organizers created 77 simulation settings which varied in the functional form of the response surfaces, and the degrees of confounding, overlap and TE heterogeneity (see [12] for more detail); also here the main goal was to estimate the ATT. These datasets were used in e.g. [2, 4, 20, 28] to evaluate CATE estimators. Once more, our main interest lies in how the PO specification in the DGP influences the observed relative performance of algorithms. Therefore, we consider only a subset of settings and fix all ‘experimental knobs’ except for the degree of TE heterogeneity (which determines the similarity of the POs). Here, we focus on settings 2, 26 and 7, which have exponentials in their response surfaces and differ only in that they have no, low and high TE heterogeneity, respectively. For each setting, we present out-of-sample results for the first 10 (out of 100) simulation runs provided by [18], where we use the first 4000 observations for training and the remaining 802 for testing. Because of their higher variability, we average all NN results across 10 replications instead of 5.

3.2.1. Empirical results

- **Finding (i): There is substantial variation in absolute performance across different runs of the same setting.** We again make a general observation on distribution of RMSE scores. Similar to the IHDP dataset, there is substantial variation in absolute performance of algorithms across different runs of the same setting. As can be seen in Fig. 4, when considering the performance of the forest-based estimators, the differences in absolute performance of the same algorithm across different runs are often larger than the differences between different strategies (using the same underlying ML method) on the same run. Similar to the IHDP dataset, simple averaging across runs can thereforemask differences that are visible mainly on the run-level.

- **Finding (ii): Relative performance of direct and indirect learners across heterogeneity settings varies as expected.** In Fig. 4, we observe that there is a trend across the three heterogeneity settings: In absence of heterogeneity, direct learners have a clear advantage and S-learners outperform T-learners. For low heterogeneity, all methods show similar performance. For high heterogeneity, the observations are reversed and indirect learners generally perform best. This difference is much more consistent and clear for the tree-based estimators than for the NN-based estimators. Further, TARNet, as a hybrid between S- and T-learner strategy, seems to inherit both their advantages, and can even match the performance of RNet on the setting without HTE.

- **Finding (iii): Underlying simulation favors tree-based methods.** Despite looking at a setting with exponential response surfaces and abundant data, we observe that in Fig. 4, RFs outperform the NNs – which stands in apparent contrast to the findings in Case study 1. Finally, we therefore investigated whether there is a reason for this, hidden in the DGP. We found that the simulated response surfaces are not created using the raw data as input, instead the 27 count variables are dichotomized by [12] before they are used in the DGP. This is the most natural pattern to represent for a RF, but we speculated that the NNs may struggle with this. To test this hypothesis, we feed the transformed data

\[\text{\textsuperscript{3}}\text{We chose this triplet as it is the only one which has all three heterogeneity settings available.}\]

\[\text{\textsuperscript{4}}\text{We conjecture that this is because [12] randomly sample terms which enter the response surfaces in each run, making some runs randomly harder than others.}\]
We found that, in the considered simulations of ACIC2016, the relative performance of different learning strategies across heterogeneity settings is also in line with expectations. As the 77 settings provided by [12] consist of only 2 settings without heterogeneity, 32 with low and 43 with high heterogeneity, we would expect that – on average – indirect learners will generally be favoured on this benchmark. Further, we found that some aspects of the underlying DGP may inherently favor tree-based methods.

3.2.2. Conclusion Case study 2

We found that, in the considered simulations of ACIC2016, the relative performance of different learning strategies across heterogeneity settings is also in line with expectations. As the 77 settings provided by [12] consist of only 2 settings without heterogeneity, 32 with low and 43 with high heterogeneity, we would expect that – on average – indirect learners will generally be favoured on this benchmark. Further, we found that some aspects of the underlying DGP may inherently favor tree-based methods.

4. Conclusions and Implications

The main goal of this paper was to raise awareness of the limitations inherent to the current use of generic semi-synthetic benchmark comparisons in the ML HTE literature. Our case studies highlighted that for semi-synthetic benchmark datasets in which (a component of) the DGP is known, some estimators have an expected advantage over others, due to a better fit of underlying ML method and/or estimation strategy with the assumptions underlying the generation of the POs. Future research could consolidate these findings by investigating further datasets, other estimators and the effect of hyperparameters.

Overall we do not consider our findings surprising – they follow directly from theoretical reasoning about the properties of different algorithms. Nonetheless, such arguments are rarely taken into account (or are at least not explicitly discussed) when benchmark datasets and baseline algorithms are selected for use in related work. Therefore, we close by discussing implications, presented as ‘food-for-thought’ in form of questions to the audience below.

•What are sensible baseline algorithms for a proposed estimator? Because many DGPs will favour one class of algorithms over another, we consider it important for authors to provide principled instead of ‘apples to oranges’ baseline comparisons – that latter may mask the sources of performance gain. That is, we consider some baseline comparisons more insightful than others. We would, for example,

What are insightful experimental knobs for a proposed estimator? Because it is often possible to predict under which circumstances an algorithm will perform best, authors should be incentivized to demonstrate how the performance of their proposed algorithm changes as experimental knobs, capturing the relevant dimension, change from most to least favourable settings. Requiring the use of ‘standard’ benchmark datasets can be detrimental in this context; especially when there are no relevant experimental knobs or settings (e.g. for evaluating direct estimators on the IHDP dataset). Conversely, when a standard benchmark dataset is used, a discussion of its inherent characteristics and their interplay with considered algorithms should be encouraged.

What does it mean for an algorithm to be ‘state-of-the-art’? As we highlighted in our experiments, the relative performance of different algorithms can change when they are compared under different DGPs/assumptions. This means that performance assessments based on a single dataset/DGP (e.g. IHDP) capture only one specific setting of many configurations of possible drivers of relative performance. We think that this warrants further discussion in the community reconsidering the meaning of the label ‘state-of-the-art’.

Can we create better benchmark datasets? Here, we considered only two (highly popular) benchmark datasets, partially because good benchmark datasets for CATE estimation are rare. In addition to considering further semi-synthetic (ACIC) datasets with simulated outcomes [13, 8] (or using real outcomes for the untreated and simulating only TEs as in [24]), a promising alternative to hand-crafting (possibly unrealistic) DGPs could be to create benchmarks using a generative approach as proposed in e.g. [29]. Nonetheless, we would expect that the resulting benchmarks would then inherently favor estimators that are most similar to the methods having generated the data. Such ‘credibility’ problems do not arise in standard supervised ML, as they can be overcome by simply evaluating methods on real data – yet, in the treatment effect context, this is usually prohibited by the absence of counterfactuals. The Twins dataset used in [27, 40], in which twins represent counterfactuals, presents an interesting exception. With this in mind, we consider further curation and provision of publicly available datasets, in which (proxies for) counterfactuals can reasonably be inferred from real data, to be a fruitful and crucial opportunity for collaboration of the ML HTE community with data owners and domain experts from more applied fields.
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A. Additional implementation details

We use all random forests with standard hyperparameters as implemented in [37]; in particular this entails using a very large number of trees (2000). All neural networks are used with standard hyperparameters and components as implemented in catenets5 for [9]; these are in turn based on the hyperparameters used in [36] for the IHDP experiments. In particular, all networks use dense layers with exponential linear units (ELU) as nonlinear activation functions, are trained with Adam, minibatches of size 100, and use early stopping based on a 30% validation split. All estimators have 3 representation layers of 200 units, 2 hypothesis layers with 100 units and a final prediction layer, and a small l2-penalty is applied to all weights. Refer to [9] for further detail. Finally, we use RNet without cross-fitting.

We retrieved the IHDP-100 data from https://www.fredjo.com/. We retrieved the ACIC2016 competition data from https://jenniferhill7.wixsite.com/acic-2016/competition; the transformations for Fig. 5 were performed using the script https://github.com/vdorie/aciccomp/blob/master/2016/R/transformInput.R from the competition R-package.

B. Additional results

In Fig. 6 (RFs) and 7 (NNs) we present full results on the two IHDP settings, which we left out of the main text for readability. Similar to the ACIC experiments, performance differences are more striking for RF-based than for NN-based methods.

5Available at https://github.com/AliciaCurth/CATENets
Figure 6. Out-of-sample RMSE of CATE estimation across 100 IHDP draws (original and modified setting) for forest-based estimators. Averaged across 5 runs, bar indicates one standard error. Shaded area in left plots indicates area which are zoomed on in right plots.

Figure 7. Out-of-sample RMSE of CATE estimation across 100 IHDP draws (original and modified setting) for NN-based estimators. Averaged across 5 runs, bar indicates one standard error. Shaded area in left plots indicates area which are zoomed on in right plots.