DISCUSSION PAPER SERIES

DP13430

MODIFIED CAUSAL FORESTS FOR ESTIMATING HETEROGENEOUS CAUSAL EFFECTS

Michael Lechner

LABOUR ECONOMICS

ISSN 0265-8003

MODIFIED CAUSAL FORESTS FOR ESTIMATING HETEROGENEOUS CAUSAL EFFECTS

Michael Lechner

Discussion Paper DP13430
Published 06 January 2019
Submitted 22 December 2018

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programme in **LABOUR ECONOMICS**. Any opinions expressed here are those of the author(s) and not those of the Centre for Economic Policy Research. Research disseminated by CEPR may include views on policy, but the Centre itself takes no institutional policy positions.

The Centre for Economic Policy Research was established in 1983 as an educational charity, to promote independent analysis and public discussion of open economies and the relations among them. It is pluralist and non-partisan, bringing economic research to bear on the analysis of medium- and long-run policy questions.

These Discussion Papers often represent preliminary or incomplete work, circulated to encourage discussion and comment. Citation and use of such a paper should take account of its provisional character.

Copyright: Michael Lechner

MODIFIED CAUSAL FORESTS FOR ESTIMATING HETEROGENEOUS CAUSAL EFFECTS

Abstract

Uncovering the heterogeneity of causal effects of policies and business decisions at various levels of granularity provides substantial value to decision makers. This paper develops new estimation and inference procedures for multiple treatment models in a selection-on-observables framework by modifying the Causal Forest approach suggested by Wager and Athey (2018). The new estimators have desirable theoretical and computational properties for various aggregation levels of the causal effects. An Empirical Monte Carlo study shows that they may outperform previously suggested estimators. Inference tends to be accurate for effects relating to larger groups and conservative for effects relating to fine levels of granularity. An application to the evaluation of an active labour mar-ket programme shows the value of the new methods for applied research.

JEL Classification: C21, J68

Keywords: Causal machine learning, statistical learning, average treatment effects, conditional aver-age treatment effects, multiple treatments, selection-on-observable, causal forests

Michael Lechner - michael.lechner@unisg.ch
SEW - University of St. Gallen and CEPR

Acknowledgements
I am also affiliated with CEPR, London, CESIfo, Munich, IAB, Nuremberg, IZA, Bonn, and RWI, Essen. I thank Michael Knaus, Anthony Strittmatter and Michael Zimmert for many important comments and discussions on this and related topics. This paper also greatly benefited from the data preparation that was done for previous papers using the same data for their Empirical Monte Carlo studies. I am indebted to Martin Huber, Giovanni Mellace, Michael Knaus and Tony Strittmatter for this. I also thank Jonathan Chassot, Daniel Goller, and Gabriel Okasa for help with optimizing the code. A previous version of the paper was presented at research seminars at the University of St. Gallen and at Amazon, Seattle. I thank participants for helpful comments and suggestions. The usual disclaimer applies. The programmes are coded in Gauss 18.1.5. They are freely available at www.michael.lechner.eu/software and at my ResearchGate homepage.

Modified Causal Forests for Estimating Heterogeneous Causal Effects

Michael Lechner[*]

Swiss Institute for Empirical Economic Research

University of St.Gallen

1st version: December 2018

Date this version has been printed: ***22 December 2018***

Comments are very welcome

Abstract: Uncovering the heterogeneity of causal effects of policies and business decisions at various levels of granularity provides substantial value to decision makers. This paper develops new estimation and inference procedures for multiple treatment models in a selection-on-observables framework by modifying the Causal Forest approach suggested by Wager and Athey (2018). The new estimators have desirable theoretical and computational properties for various aggregation levels of the causal effects. An Empirical Monte Carlo study shows that they may outperform previously suggested estimators. Inference tends to be accurate for effects relating to larger groups and conservative for effects relating to fine levels of granularity. An application to the evaluation of an active labour market programme shows the value of the new methods for applied research.

Keywords: Causal machine learning, statistical learning, average treatment effects, conditional average treatment effects, multiple treatments, selection-on-observable, causal forests.

JEL classification: C21, J68.

Correspondence to: Michael Lechner, Professor of Econometrics, Swiss Institute for Empirical Economic Research (SEW), University of St. Gallen, Switzerland, Michael.Lechner@unisg.ch, www.michael-lechner.eu , www.researchgate.net/profile/Michael_Lechner .

[*] I am also affiliated with CEPR, London, CESIfo, Munich, IAB, Nuremberg, IZA, Bonn, and RWI, Essen. I thank Michael Knaus, Anthony Strittmatter and Michael Zimmert for many important comments and discussions on this and related topics. This paper also greatly benefited from the data preparation that was done for previous papers using the same data for their Empirical Monte Carlo studies. I am indebted to Martin Huber, Giovanni Mellace, Michael Knaus and Tony Strittmatter for this. I also thank Jonathan Chassot, Daniel Goller, and Gabriel Okasa for help with optimizing the code. A previous version of the paper was presented at research seminars at the University of St. Gallen and at Amazon, Seattle. I thank participants for helpful comments and suggestions. The usual disclaimer applies. The programmes are coded in Gauss 18.1.5. They will be freely available at www.michael.lechner.eu/software and at my *ResearchGate* homepage.

1 Introduction

Although science and the public celebrated the amazing predictive power of the new machine learning methods, many researchers are left with some unease, simply because prediction does not imply causation. The ability to uncover causal relations is, however, at the core of most questions concerning the *effects* of particular policies, medical treatments, marketing campaigns, business decisions, etc. (see Athey, 2017, for a recent discussion).

The recently rapidly expanding *causal* machine learning literature holds great promise for the improved estimation of causal effects by merging the statistics and econometrics literature on causality with the supervised statistical and machine learning (ML) literature focussing on prediction. The classical causality literature clarifies the conditions needed for being able to estimate causal effects. It also shows how to transform a counterfactual causal problem into specific prediction problems (e.g., Imbens and Wooldridge, 2009). The latter literature on ML provides tools that can be highly effective in solving prediction problems (e.g. Hastie, Tibshirani, and Friedman, 2009). Bringing those two literatures together can lead to more precise, less biased, and thus more reliable estimators of average causal effects. Furthermore, it appears now possible to uncover systematically their heterogeneity as well (for an overview, see Athey and Imbens, 2017).

However, although many methods have been proposed recently to estimate heterogeneous causal effects, knowledge about their comparative statistical and computational performance as well as their ease-of-use is limited.[1] While this holds true for the suggested point estimators, it is even truer for inference, which is usually not straightforward to conduct with ML methods. There is also the issue of relevant aggregation levels: Typically, the estimators are specialised either for detecting effects at the lowest possible aggregation level using all information available, or at the highest possible aggregation level, i.e. the population. However,

[1] For a recent comparative study, see Knaus, Lechner, and Strittmatter (2018).

in practice an empirical researcher may be interested in several aggregation levels (i.e. for the population at large, for some broader subgroups, for a certain number of medium sized groups, as well as for the finest possible level). Running different estimations for different aggregation levels may be undesirable because of the computational and research-time costs of running, monitoring, and fine-tuning many level-specific estimations. This is even more so, when the estimator itself is a multi-step estimator. Such multi-step estimators, though, are needed, so far, to successfully disentangle selection bias and effect heterogeneity, which is a major issue of every non-experimental causal study (see Knaus, Lechner and Strittmatter, 2018, KLS18 from now on).

This paper tries to fill some of these gaps by suggesting estimation and approximate inference procedures that are based on (i) one-step estimators for the lowest aggregation level and that (ii) allow for direct aggregation to higher levels of interest without the need for having full-blown ML estimations for all aggregation levels. The finite sample performance of the new procedures is compared to existing estimators in an Empirical Monte Carlo study. Subsequently, they are applied to an investigation of the employment effects of a Swiss job search programme.

The new estimators are based on two modifications of the Causal Forest estimator proposed by Wager and Athey (2018, WA18 henceforth). The splitting criterion used to build the underlying trees of these Causal Forests (originating from Athey and Imbens, 2016) is based on a particular approximation of the squared error of the comparison of the predicted causal effects with the (unobservable) true causal effects. Here, we change the perspective and focus on an approximation of the squared error of the prediction problem that results after applying the identification condition (i.e. selection-on-observables) to the causal problem. The latter leads to consider squared errors of predicting a difference of conditional expectations where the components of the difference are estimated in different samples. Furthermore, to deal with selection bias without the need of additional estimation steps or outcome transformations, we add a second term to the splitting criterion. This 'penalty term' penalises splits that lead to similar treatment shares. Therefore, by accounting explicitly for treatment probability heterogeneity it fosters the removal of selection bias coming from observable variables. The resulting estimator inherits many advantages of the original Causal Forest estimator at very low, if any, additional computational costs: (i) Predictive power of the Random Forest type algorithm; (ii) applicability of the theoretical asymptotic analysis by WA18 and the resulting theoretical guarantees; and (iii) representation of the estimator as difference of weighted means with weights coming directly from the forest algorithm. The latter property turns out to be very useful for estimating aggregated effects and derive approximate inference methods for those. In the simulation study, the new estimators turns out to be performing best when there is selection bias. In the experimental context, i.e. in the case without any selection bias, the cost of having an additional penalty term that accounts for potential selection bias appears to be low. The proposed approximate inference method tends to give inference that becomes more conservative the finer the granularity of the effects is.

In the next section, we introduce the related literature. In Section 3, we discuss the parameters of interest and their identification. Estimation and inference is discussed in Section 4. Section 5 contains the Empirical Monte Carlo study. Finally, Section 6 presents an empirical application and Section 7 concludes. Appendices A and B contain more details on the estimators and the simulation study. Appendix C contains the simulation results for the case without selection bias (experiment) and for the case with an alternative specification of the treatment effect as well as results for further estimators and sensitivity checks with respect to different values of the penalty term. Free computer code is provided in terms of Gauss programme files and will be downloadable from www.michael-lechner.eu/statistical-software and www.researchgate.net/project/Causal-Machine-Learning.

2 Literature

There is a considerable literature related to the estimation of effect heterogeneity by machine learning methods in observational studies within a selection-on-observables research design. Therefore, we will discuss the main papers only briefly and refer the reader to the much more in-depth discussion of KLS18. They also organize this literature along systematic criteria about the various aspects of the particular estimators proposed.

We start this review with the methodological literature concerned with estimating heterogeneous causal or treatment effects by machine learning methods followed by applications in economics and comparative studies.[2] Contributions to this literature come from various fields, like epidemiology, econometrics, statistics, and informatics. Proposed estimators are based on regression trees (Su, Tsai, Wang, Nickerson, and Li, 2009; Athey and Imbens, 2016), Random Forests (Athey, Tibshirani, and Wager, 2018; Friedberg, Tibshirani, Athey, and Wager, 2018; Oprescu, Syrgkanis and Wu, 2018; Wager and Athey, 2018), bagging nearest neighbour estimators (Fan, Lv and Wang, 2018), the least absolute shrinkage and selection operator (LASSO, Qian and Murphy, 2011; Tian, Alizadeh, Gentles, and Tibshirani, 2014), support vector machines (Imai and Ratkovic, 2013), boosting (Powers, Qian, Jung, Schuler, Shah, Hastie, and Tibshirani, 2018), neural networks (Ramachandra, 2018; Schwab, Linhardt, and Karlen, 2018; Shalit, Johansson and Sontag, 2017), and Bayesian machine learning methods (Hill, 2011; Wang, and Rudin, 2015; Taddy, Gardner, Chen, and Draper, 2016). Finally, Chen, Tian, Cai, and Yu (2017), Künzel, Sekhon, Bickel, and Yu (2018), and Nie and Wager (2018) propose general estimation approaches that are not specific to any particular ML method.

[2] Flexible **C**onditional **A**verage **T**reatment **E**ffect (CATE) estimation has also been discussed using non-machine learning methods. These are usually multi-step procedures based on a first step estimation of the propensity score (and possibly the expectation of the outcome given treatment and confounders) and a second non- or semi-parametric step to obtain a low-dimensional CATE function. Finally, this function is used for predicting CATEs (or aggregated versions thereof) in and out-of-sample. For example, Xie, Brand and B. Jann (2012) base their estimator on propensity score stratification and regression, while Abrevaya, Hsu and Lieli (2015) use propensity score weighting, and Lee, Okui, and Whang (2017) use a doubly robust estimation approach.

While there is a large number of proposed methods, in economics only few studies used these methods so far. Ascarza (2018) investigates retention campaigns for customers. Bertrand, Crépon, Marguerie, and Premand (2017) analyse active labour market programmes in a developing country. Davis and Heller (2017) investigate summer jobs in the US. Strittmatter (2018) reinvestigates a US welfare programme. Finally, Knaus, Lechner, and Strittmatter (2017) evaluate the heterogeneous effects of a Swiss job search programme for unemployed workers.

There are also a few simulation studies investigating the properties of the suggested methods. Of course, almost every methodological paper contains a simulation study. However, these studies tend to be very specific, and usually conclude that the estimator proposed in the particular paper performs very well. However, there are now four (up to our knowledge) studies that compare a larger number of estimators. Three of them have an epidemiological background. Two of them are using data generating process that are only to a limited extend informed by real data (Zhao, Runfold, and Kemper, 2017; Powers, Qian, Jung, Schuler, Shah, Hastie, and Tibshirani, 2018). The third study uses large medical databases to inform their simulation designs (Wendling, Callahan, Schuler, Shah, and Gallego, 2018). Since these data generating processes are very specific to biometrics, it appears to be hard to draw strong lessons for many applications in social sciences.

The fourth study by KLS18 is closely related to this paper as it uses the same data and similar simulation designs. KLS18 investigate various Random Forest and LASSO based estimators for causal heterogeneity in a selection-on-observables setting. Generally, they conclude that the Forest based versions, in particular the Generalized Forest by Athey, Tibshirani, and Wager (2018) belong to the best performing estimators if explicitly adjusted to take account of confounding. This is done in a first pre-estimation step (called ‘local centering’, a ML step to estimate the expectation of the treatment and the outcomes conditional on control variables).[3]

[3] Oprescu, Syrgkanis, and Wu (2018) suggested a related, computationally intensive adjustment procedure.

Perhaps somewhat surprisingly, the estimators predicting the conditional mean of the outcomes among the treated and among the controls by standard Random Forests and subsequently taking the difference of these predictions perform almost similar to some of the more sophisticated estimators explicitly optimized for causal estimation.

3 Causal framework

3.1 The potential outcome model

We use Rubin's (1974) potential outcome language to describe a multiple treatment model under unconfoundedness, or conditional independence (Imbens, 2000, Lechner, 2001). Let *D* denote the treatment that may take a known number of *M* different integer values from *0* to *M-1*. The (potential) outcome of interest that realises under treatment *d* is denoted by Y^d. For each observation, we observe only the particular potential outcome that is related to the treatment status the observation is observed to be in, $y_i = \sum_{j=0}^{M-1} \underline{1}(d_i = j) y_i^d$ ($\underline{1}(\cdot)$ denotes the indicator function, which is one if its argument is true).[4] There are two groups of variables to condition on, $\tilde{X}$ and *Z*. $\tilde{X}$ contains those features that are needed to correct for selection bias (confounders), while *Z* contains variables that define (groups of) population members for which an average causal effect estimate is desired. For identification, $\tilde{X}$ and *Z* may be discrete, continuous, or both (when estimators are considered below, we will consider discrete *Z* only). They may overlap in any way. In line with the ML literature, we call them 'features' from now on. Denote the union of the two groups of variables by *X*, $X = \{\tilde{X}, Z\}$, $\dim(X) = p$.[5]

Below, we investigate the following average causal effects:

---

[4] If not obvious otherwise, capital letters denote random variables, and small letter their values. Small values subscripted by '*i*' denote the value of the respective variable of observation '*i*'.

[5] To avoid complications, we assume *p* to be finite (although it may be very large).

$$IATE(m,l;x,\Delta) = E(Y^m - Y^l \mid X = x, D \in \Delta) \ ,$$

$$GATE(m,l;z,\Delta) = E(Y^m - Y^l \mid Z = z, D \in \Delta) = \int IATE(m,l;x,\Delta) f_{X|Z=z,D\in\Delta}(x)dx \ ,$$

$$ATE(m,l;\Delta) = E(Y^m - Y^l \mid D \in \Delta) = \int IATE(m,l;x,\Delta) f_{X|D\in\Delta}(x)dx \ .$$

The **I**ndividualized **A**verage **T**reatment **E**ffects (IATEs), $IATE(m,l;x,\Delta)$ measure the mean impact of treatment *m* compared to treatment *l* for units with features *x* that belong to treatment groups Δ, where Δ denotes all treatments of interest. The IATEs represent the causal parameters at the finest aggregation level of features available. On the other extreme, the **A**verage **T**reatment **E**ffects (ATE) represent the population averages. If Δ relates the population with *D*=*m*, then this is the **A**verage **T**reatment **E**ffect on the **T**reated (ATET) for treatment *m*. The ATE and ATET are the classical parameters investigated in many econometric causal studies. The **G**roup **A**verage **T**reatment **E**ffect (GATE) parameters are in-between those two extremes with respect to their aggregation levels.[6] The IATEs and the GATEs are special cases of the so-called **C**onditional **A**verage **T**reatment **E**ffects (CATEs).

3.2 Identifying assumptions

The classical set of unconfoundedness assumptions consists of four parts (see Imbens, 2000, Lechner 2001):[7]

6 Note that we presume that the analyst selects the variables *Z* prior to estimation (Z may be discrete or continuous). They are not assumed to be determined in a data driven way, e.g., by statistical variable selection procedures. However, the estimated IATE may be analysed by such methods to describe their dependence on certain features. See Section 6 for more details. Note that Abrevaya, Hsu and Lieli (2015) and Lee, Okui, and Whang (2017) introduce similar aggregated parameters that depend on a reduced conditioning set and discuss inference in the specific settings of their papers.

7 To simplify the notation, we take the strongest form of these assumptions. Some parameters are identified under weaker conditions as well (for details, see Lechner, 2001, or Imbens, 2000, 2004).

$$
\begin{array}{lll}
\{Y^0,...,Y^m,...,Y^{M-1}\} \coprod D \mid X=x, & \forall x \in \chi; & (CIA) \\
0 < P(D=d \mid X=x) = p_d(x), & \forall x \in \chi, \forall d \in \{0,...,M-1\}; & (CS) \\
Y = \sum_{d=0}^{M-1} \underline{1}(D=d) Y^d; & & (SUTVA) \\
X^d = X, & \forall d \in \{0,...,M-1\}. & (EXOG)
\end{array}
$$

The conditional independence assumption (CIA) implies that there are no features other than X that jointly influence treatment and potential outcomes (for the values of X that are in the support of interest, χ). The common support (CS) assumption stipulates that for each value in χ, there must be the possibility to observe all treatments. The stable-unit-treatment-value assumption (SUTVA) implies that the observed value of the treatment does not depend on the treatment allocation of the other population members (ruling out spillover and treatment size effects). Finally, the exogeneity assumptions (EXOG) imply that the observed values of X do not dependent on the treatment status (X^d denotes a 'potential' feature). Thus, a causal effect of D on X is ruled out. In addition to these *identifying* assumptions, assume that a large random sample of size N from the random variables Y, D, X, $\{y_i, d_i, x_i\}$, $i = 1,\ldots,N$, is available and that these random variables have at least first (and second) moments.[8]

If this set of assumption holds, then all IATE's are identified in the sense that they can be uniquely deduced from expectations of variables that have observable sample realisations (see Hurwicz, 1950):

$$
\begin{aligned}
IATE(m,l;x,\Delta) &= E(Y^m - Y^l \mid X=x, D \in \Delta) \\
&= E(Y^m - Y^l \mid X=x) \\
&= E(Y^m \mid X=x, D=m) - E(Y^l \mid X=x, D=l) \\
&= E(Y \mid X=x, D=m) - E(Y \mid X=x, D=l) \\
&= IATE(m,l;x); \qquad \forall x \in \chi, \forall m \neq l \in \{0,...,M-1\}.
\end{aligned}
$$

8 The identification results will also hold under weight-based and dependent sampling (if the dependence is not too large and certain additional regularity conditions are imposed), but for simplicity we stick to the i.i.d. case. Second moments are not needed for identification, but for the inference part below.

Note that one implication of these assumptions is that IATE does not depend on the conditioning treatment set, Δ. Since the distributions used for aggregation, $f_{X|Z=z,D\in\Delta}(x)$ and $f_{X|D\in\Delta}(x)$, relate to observable variable (*X, Z, D*) only, they are identified as well (under standard regularity conditions). This in turn implies that the GATE and ATE parameters are identified (their dependence on Δ remains, if the distribution of the features depends on Δ).

4 Estimation and inference

In this section, we discuss estimation and inference of the proposed Random Forest based estimators. The first subsection introduces the modified criteria for split selection of leaves in the regression trees that form the Causal Forest. Subsection 2 briefly reviews the theoretical guarantees and properties such estimators may have, followed by a suggestion of weights-based inference as a computationally convenient tool to conduct inference for all desired aggregation levels. The final subsection considers several issues related to the practical implementation of the estimator.

4.1 IATE: Towards a MSE minimal estimator

Denoting the conditional expectations of *Y* given *X* in the subpopulation *D* = *d* by $\mu_d(x)$ leads to the following expression of *IATE(m,l;x)* as a difference of $\mu_m(x)$ and $\mu_l(x)$:

$$IATE(m,l;x) = \mu_m(x) - \mu_l(x); \qquad \forall x \in \chi, \forall m \neq l \in \{0,...,M-1\}.$$

This estimation task is different to standard ML problems because the two conditional expectations have to be estimated in different, treatment-specific subsamples.[9] Thus, the ML prediction of the difference cannot be directly validated in a holdout sample. This observation

[9] This is implied by the fact that the causal effect is a hypothetical construct that is per se unobservable. Thus, in the words of Athey and Imbens (2016), the 'ground truth' is unobservable in causal analysis.

is indeed the starting point of the current causal machine learning literature. The papers then differ on how to tackle this issue.

An easy-to-implement estimator consists in estimating the two conditional expectations separately by standard ML tools, and taking a difference. Below, we denote this estimator as *basic*, $\widehat{IATE}^{basic}(m,l;x) = \hat{\mu}_m^{basic}(x) - \hat{\mu}_l^{basic}(x)$. This approach has the disadvantage that standard ML methods will try to maximise out-of-sample predictive power of the two estimators *separately*. More concretely, if a Random Forest is used, the difference of the predictions of the two different estimated forests (one estimated in subpopulation *m*, the other one estimated in subpopulation *l*) may suggest a variability of the IATE that is just estimation error due to (random) differences in the estimated forests. This problem can be particularly pronounced when the features are highly predictive for *Y*, but the IATEs are rather constant. Another example is the case of very unequal treatment shares. As (standard) tree-building uses a stopping rule defined in terms of minimum leaf size, there will be many more observations in treatment *m* compared to treatment *l*, even for similar values of *x*. Thus, the forest estimated for treatment *m* will be finer than the one estimated for treatment *l*. Again, this may lead to spurious effect heterogeneity. However, despite these methodological drawbacks, the large-scale EMCS of KLS18 finds that the Random Forest based *basic* estimator may perform well compared to technically more sophisticated approaches, in particular when the IATEs are large and vary strongly with the features.

An alternative approach is to use the same splitting rules in both subsamples in which $\mu_m(x)$ and $\mu_l(x)$ are estimated by $\hat{\mu}_m(x)$ and $\hat{\mu}_l(x)$, respectively. Of course, the key is then how to obtain a plausible splitting rule for this 'joint' forest. The dominant approach in the literature so far seems to consider the analogy to a classical random forest regression problem in which the 'ground truth', i.e. the individual treatment effect, would be observable. In this case, the Tree estimate of $IATE(m,l;x)$ would be equal to the mean of the 'observed' individual treatment effects in each leaf. For such a case, some algebra reveals that minimising the mean squared error of the prediction and maximising the variance of the predicted treatment effects leads to the same sample splits. Therefore, Athey and Imbens (2016) suggest for their causal CARTs to split the parent leave such as to maximise the heterogeneity of the estimated effects (subject to some adjustments for overfitting). This criterion is also used in one of the approaches of Wager and Athey (2018) and in Oprescu, Syrgkanis, and Wu (2018). However, in case of causal estimation, when the individual treatment effect is unobservable, there is no guarantee that the difference of the outcome means of treated and controls within all leaves equals the means of the true effects within all leaves. Without this condition, the maximisation of treatment effect heterogeneity is not equivalent to MSE minimisation of treatment effects prediction. The reason for the difference to the standard predictive case is due to potential selection bias. Intuitively, if selection bias does not matter, such an equality holds in expectation and this criterion should be a good approximation to minimizing the MSE of the estimated and true individual treatment effect. However, if selection bias is a relevant issue (as it is likely to be in the early splits of the tree), then the quality of this approximation may be questionable.

An alternative approach to derive a splitting rule is to consider the mean square error of the estimation problem directly:

$$
\begin{aligned}
MSE\left[\widehat{IATE}(m,l;x)\right] &= E\left\{\left[\widehat{IATE}(m,l;x) - IATE(m,l;x)\right]^2\right\} \\
&= E\left[\hat{\mu}_m(x)-\mu_m(x)\right]^2 + E\left[\hat{\mu}_l(x)-\mu_l(x)\right]^2 - 2E\left[\hat{\mu}_m(x)-\mu_m(x)\right]\left[\hat{\mu}_l(x)-\mu_l(x)\right] \\
&= MSE\left[\hat{\mu}_m(x)\right] + MSE\left[\hat{\mu}_l(x)\right] - 2\underbrace{E\left[\hat{\mu}_m(x)-\mu_m(x)\right]\left[\hat{\mu}_l(x)-\mu_l(x)\right]}_{MCE\left[\hat{\mu}_m(x),\hat{\mu}_l(x)\right]} \\
&= MSE\left[\hat{\mu}_m(x)\right] + MSE\left[\hat{\mu}_l(x)\right] - 2MCE\left[\hat{\mu}_m(x),\hat{\mu}_l(x)\right].
\end{aligned}
$$

This derivation of the mean square error is instructive.[10] It shows that the *basic* estimator fails to take into account that estimation errors may be correlated, conditional on the features.

[10] Note that in Random Forests, the expectation is taken with respect to the distribution of X in the training data.

Thus, there may be a big advantage of tying the estimators together in a way such that the correlation of their estimation errors becomes positive (and cancels to some extent). The complication here is that the **M**ean **C**orrelated **E**rror (MCE) is difficult to estimate.

For constructing estimators based on this criterion, the MSE of $\hat{\mu}_d(x)$ has to be estimated. This is straightforward, as the MSEs of all *M* functions $\hat{\mu}_d(x)$ can be computed in the respective treatment subsamples in the usual way. Denote by $N_{S_x}^d$ the number of observations with treatment value *d* in a certain stratum (leaf) S_x, which is defined by the values of the features *x*. Then, the following estimator is a 'natural' choice:

$$\widehat{MSE}_{S_x}\left[\hat{\mu}_d(x)\right] = \frac{1}{N_{S_x}^d}\sum_{i=1}^{N}\underline{1}(x_i \in S_x)\underline{1}(d_i \in d)\left[\hat{\mu}_d(x_i) - y_i\right]^2 .$$

Note that the overall MSE in S_x is the sum of the MSEs in the treatment specific subsamples of S_x, where each subsample receives the same weight (independent of the number of observations in that subsample), as implied by the above MSE formula for causal effect estimation.

In order to compute the correlation of the estimation errors, we need a proxy for cases when there are no observations with exactly the same values of *x* in all treatment states (as is always true for continuous features). In this case, we propose using the closest neighbour available instead.[11]

$$\widehat{MCE}(m,l;S_x) = \frac{1}{N_{S_x}^l + N_{S_x}^m}\sum_{i=1}^{N}\underline{1}(x_i \in S_x)\left[\underline{1}(d_i = m) + \underline{1}(d_i = l)\right]\left[\hat{\mu}_m(x_i) - \tilde{y}_{(i,m)}\right]\left[\hat{\mu}_l(x_i) - \tilde{y}_{(i,l)}\right],$$

$$\tilde{y}_{(i,m)} = \begin{cases} y_i & \text{if } d_i = m \\ y_{(i,m)} & \phantom{\text{if }} d_i \neq m \end{cases} .$$

[11] Closeness is based on a simplified Mahalanobis metric as in Abadie and Imbens (2006). This simplified version has the inverse of the variances of the features on the main diagonal. Off-diagonal elements are zero. The simplification avoids computational complications when inverting the variance-covariance matrix of potentially large-dimensional features at the cost of ignoring correlations between covariates.

While the splitting rule that minimizes $\widehat{MSE}[\widehat{IATE}(m,l,x)]$ is motivated by maximising the predictive power of the estimator, the causal ML literature suggests that the particular feature of causal problems (in observational studies), i.e. that the treatment is not randomly allocated, gives a special role to the propensity score, $P(D=d \mid X=x)$, in any estimation of IATEs. This is usually tackled by a first stage estimation of $P(D=d \mid X=x)$ (and sometimes also of $E(Y \mid X=x)$ or $E(Y \mid D=d, X=x)$) and treating it as a nuisance parameter in the Random Forest estimation of $\widehat{IATE}(m,l;x)$.[12] Here, we like to avoid computer-time-consuming additional estimation steps, but still improve on the robustness of the estimator with respect to selection bias, in particular in smaller samples where the Random Forests may not be automatically fine enough to remove all selection biases.

These considerations lead to a modification of the splitting rules. Denote by *leaf(x')* and *leaf(x'')* the values of the features in the daughter leaves resulting from splitting some parent leaf. We propose to add a penalty term to $\widehat{MSE}\left[\widehat{IATE}(m,l;x)\right]$ that punishes cases where the treatment probabilities in the possible splits are very similar (splits that are similar with respect to treatment shares will not be able to remove much selection bias, while if they are very different in this respect, they approximate differences in $P(D=m \mid X=x)$ well). In other words, the modified criterion prefers splits with high propensity score heterogeneity and puts explicit emphasis on tackling selection bias. In the simulations below, the following penalty function is used and added to splitting criteria of (some) estimators considered:

$$penalty(x', x'') = \lambda \left\{ 1 - \frac{1}{M} \sum_{d=0}^{M-1} [P(D=d \mid X \in leaf(x')) - P(D=d \mid X \in leaf(x''))]^2 \right\} .$$

[12] See for example Oprescu, Syrgkanis, and Wu (2018). There is also a substantial literature on how to exploit so-called double-robustness properties when estimating the causal effects at higher aggregation level, see, e.g., Belloni, Chernozhukov, Fernández-Val, and Hansen (2017) and the references therein.

This penalty term is zero if the split leads to a perfect prediction of the probabilities in the daughter leaves. It reaches its maximum value, λ, when all probabilities are equal. Thus, the algorithm prefers a split that is not only predictive for *y* but also for *D*. Of course, the choice of the exact form of this penalty function is arbitrary. Furthermore, there is the issue of how to choose λ (*without* expensive additional computations) which is taken up again in Section 4.5.

So far, we focused on the comparison of two treatments. When there more than two treatments, this algorithms can be implemented in at least two different ways. Based on the 'sample reduction properties' in Lechner (2001), the version will estimated all parameters in pair-wise comparisons independent of each other. This may become computationally cumbersome when there are many treatments. The second alternative is to build the same Trees for all treatments and then sum up their MSEs. If some effects are more important to a researcher than others are, such a summation may be based on some pre-specified weights. If this is not the case, it appear 'natural' to weight all MSE's equally.

4.2 Properties

Theorem 1 of WA18 shows that under certain conditions predictions from Random Forest based estimators scaled are normally distributed. The assumptions necessary for achieving this asymptotic distribution in the case of i.i.d. sampling require (i) Lipschitz continuity of lower and existence of some higher order moments of the outcome variable conditional on the features, (ii) using subsampling to form the training data for tree building (subsamples should increase with *N*, slower than *N*, but not too slow), (iii) conditions on the features (independent, continuous with bounded support, *p* is fixed, i.e. low-dimensional), as well as (iv) some conditions on how to build the trees. The latter conditions require (a) that building a tree is independent from computing its predictions (honesty), (b) that every feature has a large enough probability to be used for tree splitting, (c) that trees are fully grown up to a minimum leaf size, and

(d) that the minimum share of observations required at least to end up in the smaller daughter-leaf in each split is not more than 20% of the observations in the parent leaf.

The proposed estimators (see below) fulfil these conditions on tree building. However, some of these conditions are very specific and sometimes difficult to match (like covariates being independent), or impossible to verify (like the regularity conditions on the conditional outcome expectations). Nevertheless, the simulation results show that the predictions from all Random Forest based estimators appear to be very close to be normally distributed even for the smallest sample size investigated (*N=1000*).

4.3 Inference

There are several suggestions in the literature on how to conduct inference and how to compute standard errors of Random Forest based predictions (e.g., Wager, Hastie, and Efron, 2014; Wager and Athey, 2018; and the references therein). Although these methods can be used to conduct inference on the IATE, it is yet unexplored how these methods could be readily generalized to take account of the aggregation steps needed for the GATE and ATE parameters.

Therefore, we suggest an alternative inference method useful for estimators that have a representation as weighted averages of the outcomes. This perspective is attractive for Random Forest based estimators (e.g. Athey, Tibshirani, and Wager, 2018) as they consist of trees that first stratify the data (when building a tree), and subsequently average over these strata (when building the forest). Thus, we exploit the weights-based representation explicitly for inference (see also Lechner, 2002, and Abadie and Imbens, 2006, for related approaches).

Let us start with a general weights-based estimator. Denote by $\hat{w}_i$ the weight that the dependent variable y_i receives in the desired estimator, $\hat{\theta}$ (which could be one of the IATEs, or GATEs, or ATEs).

$$\hat{\theta} = \frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i; \qquad Var(\hat{\theta}) = Var\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i\right).$$

Next, we apply the law of total probability to the variance:[13]

$$\begin{aligned} Var\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i\right) &= E_{\hat{W}} Var\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i \mid \hat{w}_1,...,\hat{w}_N\right) + Var_{\hat{W}} E\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i \mid \hat{w}_1,...,\hat{w}_N\right) \\ &= E_{\hat{W}}\left(\frac{1}{N^2}\sum_{i=1}^{N}\hat{w}_i^2 Var(y_i \mid \hat{w}_1,...,\hat{w}_N)\right) \\ &+ Var_{\hat{W}}\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i E(y_i \mid \hat{w}_1,...,\hat{w}_N)\right). \end{aligned}$$

However, the large conditioning sets of $E(y_i \mid \hat{w}_1,...,\hat{w}_N)$ and $Var(y_i \mid \hat{w}_1,...,\hat{w}_N)$ makes it impossible to estimate these terms precisely without further assumptions. The conditioning sets can be drastically reduced, though, if observation '*i*' is not used to build the forest,[14] and the data used for the computations of the conditional mean and variance are an i.i.d. sample. To see this, recall that Random Forest weights are computed as functions of $\vec{X}^T = (x_1,...,x_{N^T})$ and $\vec{Y}^T = (y_1,...,y_{N_T})$ in the training sample with N_T training observations. These weights are then assigned to observation *i* based on the value of x_i only. Thus, the weights are functions of x_i and the training data, $\hat{w}_i = \hat{w}(x_i, \vec{X}^T, \vec{Y}^T)$. If observation '*i*' does not belong to the training data and there is i.i.d. sampling, y_i and $\hat{w}_j = \hat{w}(x_j, \vec{X}^T, \vec{Y}^T)$ are independent. Thus, we obtain $E(y_i \mid \hat{w}_1,...,\hat{w}_N) = E(y_i \mid \hat{w}_i) = \mu_{Y|W}(\hat{w}_i)$ and $Var(y_i \mid \hat{w}_1,...,\hat{w}_N) = Var(y_i \mid \hat{w}_i) = \sigma^2_{Y|W}(\hat{w}_i)$. This leads to the following expression of the variance of the proposed estimators:

---

[13] Letting *A* and *B* two random variables, then $Var(A) = E_B Var(A \mid B) + Var_B E(A \mid B)$.

[14] Note that this condition goes beyond 'honesty' (i.e. continuously switching the role of observations used for tree building and effect estimation in Random Forests), because even if honesty is used, each weight may still depend on many observations. Clearly, the price to pay for sample splitting is a loss of precision (as so-called 'cross-fitting' does also not appear to work in this set-up without further adjustments).

$$Var\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i y_i\right) = E_{\hat{W}}\left(\frac{1}{N^2}\sum_{i=1}^{N}\hat{w}_i^{\,2}\sigma^2_{Y|\hat{W}}(\hat{w}_i)\right) + Var_{\hat{W}}\left(\frac{1}{N}\sum_{i=1}^{N}\hat{w}_i\mu_{Y|\hat{W}}(\hat{w}_i)\right).$$

The above expression suggests using the following estimator:

$$\widehat{Var(\hat{\theta})} = \frac{1}{N^2}\sum_{i=1}^{N}\hat{w}_i^2\hat{\sigma}^2_{Y|\hat{W}}(\hat{w}_i) + \frac{1}{N(N-1)}\sum_{i=1}^{N}\left[\hat{w}_i\hat{\mu}_{Y|\hat{W}}(\hat{w}_i) - \frac{1}{N}\sum_{i=1}^{N}\hat{w}_i\hat{\mu}_{Y|\hat{W}}(\hat{w}_i)\right]^2.$$

The conditional expectations and variances may be computed by standard non-parametric or ML methods, as this is a one-dimensional problem for which many well-established estimators exist. Bodory, Camponovo, Huber, and Lechner (2018) investigate *k*-nearest neighbour estimators to obtain these weights. They found good results in a binary treatment setting for the ATET. The same method is used here.[15] Finally, note that because of the weighting representation, this approach can also be readily used to account for, e.g., clustering, which is a common feature in economics data, or to conduct joint tests of several linear hypothesis, such that several groups have the same or no effect leading to Wald-type statistics. It is, however, beyond the scope of this paper to rigorously analyse the exact statistical conditions needed for this estimator to lead to valid inference.

4.4 GATE and ATE

Estimates for GATEs and ATE are most easily obtained by averaging the IATEs in the respective subsamples defined by z (assuming discrete Z) and Δ. Although estimating ATEs and GATEs directly instead of aggregating IATEs may lead to more efficient and more robust estimators (e.g. Belloni, Chernozhukov, Fernández-Val, and Hansen, 2017), the computational burden would also be higher, in particular if the number of GATEs of interest is large, as is common in many empirical studies. Therefore, letting $\widehat{IATE}(m,l;x)$ be an estimator of

[15] They also found a considerable robustness on how exactly to compute the conditional means and variances. Note that since their results relate to aggregate results, their generalisability to the level of IATE's is unclear.

$IATE(m,l;x)$, we suggest to estimate the GATEs and ATEs as appropriate averages of $\widehat{IATE(m,l;x)}$'s:

$$\begin{aligned}
\widehat{GATE}(m,l;z,\Delta) &= \frac{1}{N^{z,\Delta}}\sum_{i=1}^{N}\underline{1}(z_i = z, d_i \in \Delta)\widehat{IATE}(m,l;x_i) \\
&= \frac{1}{N^{z,\Delta}}\sum_{i=1}^{N}\underline{1}(z_i = z, d_i \in \Delta)\frac{1}{N}\sum_{j=1}^{N}\hat{w}_j^{IATE(m,l;x_i)}y_j \\
&= \frac{1}{N}\sum_{i=1}^{N}\left(\frac{1}{N^{z,\Delta}}\sum_{j=1}^{N}\underline{1}(z_j = z, d_j \in \Delta)\hat{w}_i^{IATE(m,l;x_j)}\right)y_i \\
&= \frac{1}{N}\sum_{i=1}^{N}\hat{w}_i^{GATE(m,l;z,\Delta)}y_i; \\
\hat{w}_i^{GATE(m,l;z,\Delta)} &= \frac{1}{N^{z,\Delta}}\sum_{j=1}^{N}\underline{1}(z_j = z, d_j \in \Delta)\hat{w}_j^{IATE(m,l;x_i)}; \qquad N^{z,\Delta} = \sum_{i=1}^{N}\underline{1}(z_i = z, d_i \in \Delta).
\end{aligned}$$

$$\begin{aligned}
\widehat{ATE}(m,l;\Delta) &= \frac{1}{N^{z}}\sum_{i=1}^{N}\underline{1}(d_i \in \Delta)\widehat{IATE}(m,l;x_i) \\
&= \frac{1}{N}\sum_{i=1}^{N}\hat{w}_i^{ATE(m,l;\Delta)}y_i; \\
\hat{w}_i^{ATE(m,l;z,\Delta)} &= \frac{1}{N^{\Delta}}\sum_{j=1}^{N}\underline{1}(d_j \in \Delta)\hat{w}_j^{IATE(m,l;x_i)}; \qquad N^{\Delta} = \sum_{i=1}^{N}\underline{1}(d_i \in \Delta).
\end{aligned}$$

From this expression, it is clear that ATEs and GATEs have the same type of weight-based representation as the IATEs. Hence, inference can be conducted in the same way as for the IATEs, just using different weights.

4.5 Implementation

Beyond the *basic* estimator, we investigate five different estimators, all based on using the same single forest for all treatment subsamples. They differ in their complexity. *OneF* ignores MCE in constructing the Tree und is solely based on sum of the treatment specific MSEs of estimating $\hat{\mu}_d(x)$. *OneF.MCE* estimates the MCE in a computationally not too expensive way by using nearest neighbours. *OneF.VarT* is the Causal Forest estimator of Wager and Athey (2018) based on maximising treatment effect heterogeneity. *OneF.MCE.Penalty* and

OneF.VarT.Penalty use the same basic splitting criteria as *OneF.MCE* and *OneF.VarT* but add an additional penalty term to the splitting rule to reduce potential selection biases.

The main elements of the algorithms used for estimating the results in the simulation and application part are the following:

1) Split the estimation sample randomly into two parts of equal size (sample A and sample B)

2) Estimate the trees that define the random forest in sample A.

   a. *Basic*: Estimate Random Forests for each treatment state in the subsamples defined by treatment state. The splitting rule consists of independently minimizing the mean squared prediction error within each subsample.

   b. *OneF*: Estimate the same forest for all treatment states jointly. The splitting rule is based on minimising the sum of the MSE's for all treatment state outcome predictions (MCEs are set to 0).

   c. *OneF.MCE*: Same as b), but before building the trees, for each observation in each treatment state, find a close 'neighbour' in every other treatment state and save its outcome (to estimate MCE). The splitting rule is based on minimising the overall MSEs, taking account of all MCEs.

   d. *OneF.VarT*: Same as b), but the splitting is based on maximising estimated treatment effect heterogeneity.

   e. *OneF.MCE.Penalty*: Same as c) but a penalty term penalizing propensity score homogeneity is added.[16]

[16] In the simulations below, setting λ to *Var(Y)* works well. *Var(Y)* corresponds to the MSE when the effects are estimated by the sample mean without any splits. Thus, it provides some benchmark for plausible values of λ. In small-scale experiments with values smaller and larger than *Var(Y)* the MSE shows little sensitivity for values half as well as twice the size of *Var(Y)* (see Appendix C.2.1). Generally, decreasing the penalty increases biases and reduces variances, et vice versa. As will be seen below in the simulations, biases are more likely to occur when selection is strong. Thus, if a priori knowledge about the importance of selectivity is available, then the researcher might increase (strong selectivity) or decrease (weak selectivity or experiment) the penalty term accordingly.

f. *OneF.VarT.Penalty*: Same as d) but a penalty term penalizing propensity score homogeneity is added.[17]

3) Apply the sample splits obtained in sample A to all subsamples (by treatment state) of sample B and take the mean of the outcome in the respective leaf as the prediction that comes with this Random Forest.

4) Obtain the weights from the estimated Random Forest by counting how many times an observation in sample B is used to predict IATE for a particular value of x.

5) Aggregate the IATEs to GATEs by taking the average over observations in sample B that have the same value of z and treatment group Δ. Take the average of all IATE in treatment group Δ to get ATE. Do the same aggregation with the weights to obtain the new weights valid for the GATEs.

6) Do the same steps as in 5) to obtain the ATEs, but average over all observations in treatment group Δ.

7) Compute weight-based standard errors as described above. Use the estimated standard errors together with the quantiles from the normal distribution to obtain critical values (p-values).

While Appendix A details the implementation further, at least two points merit some more discussion. The efficiency loss inherent in the two-sample approach could be avoided by cross-fitting, i.e. by repeating the estimation with exchanged roles of the two samples and averaging the two estimates.[18] However, in such a case it is unclear on how to compute the weight-based inference for the averaged estimator as the two components of this average will be correlated.

[17] Setting λ to the square of the sum of the differences of the treatment means corresponds to the intuition used for *OneF.MCE.Penalty*. However, in the simulations below it appeared that such a value is far too small to reduce biases significantly when there is selectivity (see Appendix C.2.2). Therefore, a value corresponding to 100 x that value is used below.

[18] However, note that the simulations below comparing the two-sample estimator with a one-sample-with-honesty strategy suggest that the efficiency loss is minor, if existent at all.

A second issue concerns the fact of forming the neighbours by simplified Mahalanobis matching, which has the issue of being potentially large-dimensional. A lower dimensional alternative might be to estimate a prognostic score, $\left[\hat{\mu}_0(x),....,\hat{\mu}_{M-1}(x)\right]$, by ML methods and then use this score instead.[19] While this is a viable alternative, it requires again (costly) nuisance parameter estimation which we want to avoid with the suggested estimator.

5 Simulation results of an Empirical Monte Carlo Study

5.1 Data base, simulation concept, and data generating processes

It is a general problem of simulation results that they depend on the particular design of the data generating process (DGP) chosen by the researcher, which might reduce their generalizability to specific empirical applications. While this may be innocuous if simulations are used to investigate specific theoretical properties of estimators or test statistics only (e.g. analysing what happens if the correlation of features increases), it is more problematic when simulations are used to investigate the suitability of estimators for particular applications. In this case, a simulation environment that closely mimics real applications is advantageous. In that way, the results generalize more easily to applications with a similar data structure.

Huber, Lechner, and Wunsch (2013) and Lechner and Wunsch (2013) proposed a specific data driven method for simulation analyses which they called Empirical Monte Carlo Study (EMCS). The main idea of an EMCS is to have a large real data set from which to draw random subsamples using as much information from the real data as is possible. In the simulations below, we follow this approach (see Appendix B for the algorithm used).[20]

[19] Note that propensity scores will not be helpful as the intended correction is not directly related to selection bias.

[20] Note the similarity of the concept of EMCS with the very recently proposed method of Synth Validation (Schuler, Jung, Tibshirani, Hastie, and Shah, 2017). Although the latter method is intended to select methods for a particular datasets (in the machine learning spirit of comparing predictions with observed variables), both methods could be used for method

We base the simulations on Swiss social security data previously used to evaluate active labour market policies. More precisely, as in Huber, Lechner, and Mellace (2017) for analysing a mediation framework, and in KLS18 for comparing different estimators of the IATE, we consider the effects of a job search programme on employment outcomes. This data is well suited for this type of analysis, as it is long and wide (about 95'000 observations, more than 50 base covariates). Furthermore, the programme is one for which the literature argues that a selection-on-observable assumption is plausible when rich social security data are available (see, e.g. Lechner and Gerfin, 2002, for the Swiss case, and the survey by Card, Kluve, and Weber, 2017).[21]

The main steps are the following: Using the initial data, we estimate the propensity score, $p(x)$. This estimation uses the same specification as KLS18. It depends on 77 features that enter a logit model estimated by maximum likelihood.[22] This estimated propensity score plays the role of the true selection process in the following steps. Next, the treated are removed from the data. This means that for all remaining observations (approx. 84'000) we observe Y^0, the non-treatment outcome (measured as number of months in employment in the next 33 months after the programme starts), the features, and the propensity score. This information is used to simulate the individual treatment effects (ITE), to compute the IATEs, and to compute Y^1 as the sum of Y^0 and the ITE. In the next step, we draw randomly a validation data set with 5'000 observations and remove it from the main data. From the remaining data, we draw random samples of size $N = 1'000$ or $N = 4'000$, simulate a treatment status for each observation using the 'true'

selection as well as for method validation. Advani, Kitagawa, and Słoczyński (2018) point to the potential limits of generalizing results from such simulation exercises.

[21] Our implementation follows closely KLS18. Therefore, for the sake of brevity, we do not repeat their extensive documentation of all steps that lead to the final sample and their descriptions of the estimation sample. The reader interested in more details is referred to KLS18.

[22] The number is larger than the number of base covariates due to the addition of some transformations of the base covariates, such as dummy variables.

propensity score (shifted such that a treatment share is about 50%), and take the potential outcome that corresponds to the simulated treatment as observed outcome, *Y*. These random samples are the training data for the algorithms, while their performance is measured out-of-sample on the 5'000 observations of the validation sample.

To be specific, we specify the IATE as non-linear function of the propensity score:

$$\xi(x) = \sin\left(1.25\,\pi \frac{p(x)}{\max_{i=1:N} p(x_i)}\right),$$

$$IATE(x) = \alpha \frac{\xi(x) - \bar{\xi}}{SD(\xi)}; \qquad \bar{\xi} = \frac{1}{N}\sum_{i=1}^{N} \xi(x_i), \quad SD(\xi) = \sqrt{\frac{1}{N}\sum_{i=1}^{N}\left[\xi(x_i) - \bar{\xi}\right]^2} \; .$$

Note that the parameter α determines the variability of the IATE. Due to the non-linear way in which the features enter the IATEs via the propensity score, it is a difficult task for every estimator not to confuse selection effects with heterogeneous treatment effects.

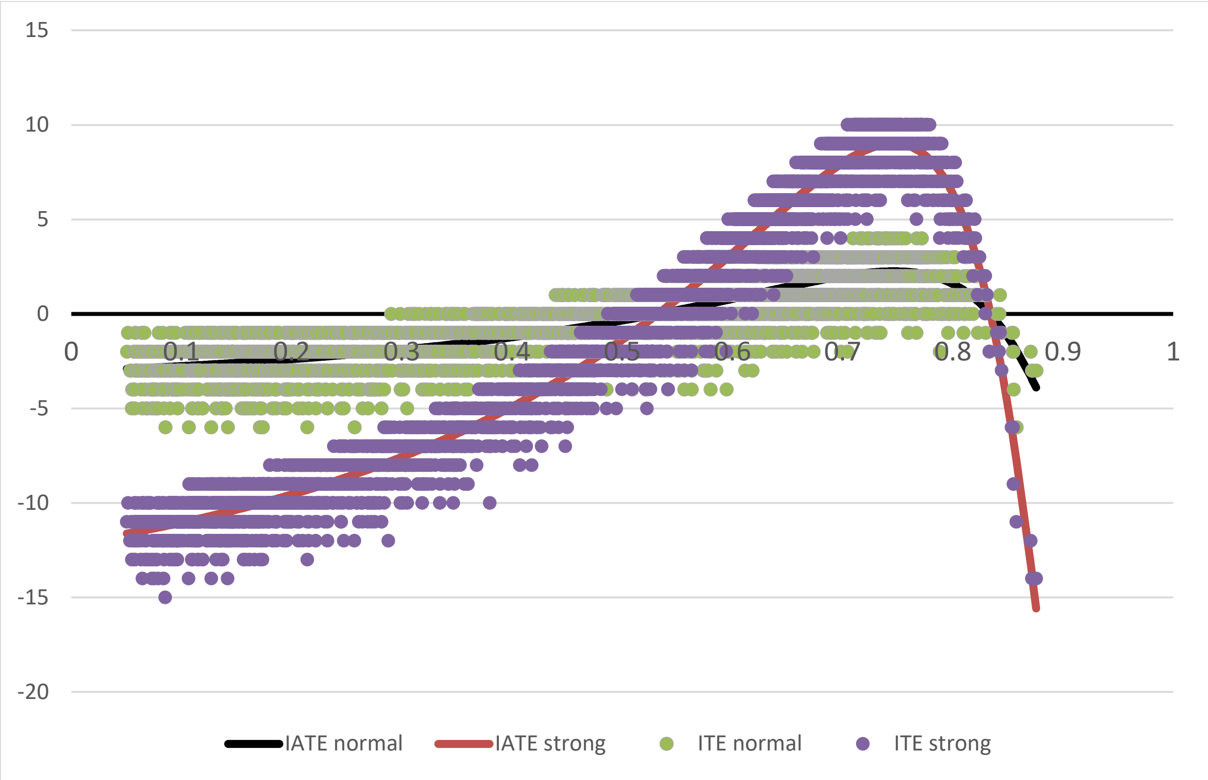
Adding two independent random components to the IATE leads to the ITE. The first random term is a (minus) Poisson (1) variate adjusted to have mean zero. The second random term ensures that the ITE, and thus Y^1, keeps its character as integer (month) in a way that the rounding 'error' is independent of the IATEs:

$$\begin{aligned} ITE(x) &= IATE(x) + (1-u) + v; \\ & u \sim Poisson(1); \\ & v^* \sim Uniform[0,1]; \\ & v^{diff} = IATE(x) + u - floor(IATE(x) + u); \\ & v = \underline{1}(v^* > v^{diff})(-v^{diff}) + \underline{1}(v^* \le v^{diff})(1 - v^{diff}). \end{aligned}$$

Floor denotes the integer part of *IATE(x).* In the simulations, we consider three types of ITEs: (i) *ITE = 0;* (ii) $\alpha = 2$ ('normal' heterogeneity); and (iii) $\alpha = 8$ ('strong' heterogeneity). The 'normal' IATE has a standard deviation of about 1.7, while the 'strong' IATE has a standard deviation of about 6.8, compared to a standard deviation of the outcome of about 12.9. The ‘normal’ as well as the ‘strong’ IATE are well predictable (if observed). For example, plain-vanilla Random Forests have out-of-sample R^2’s above 99%. Naturally, the predictability of the ITE is smaller with out-of-sample R^2’s of about 76% (‘normal’) and 92% (‘strong’), respectively.

Figure 1 shows the ITE (as bands at integer values of the outcome) and the IATE (as continuous line) as functions of the propensity score. The figures emphasise the non-linearities, the strong relation to the propensity score, and the fact that the effects change sign. Obviously, these features are much stronger for the ‘strong’ effects, than for the ‘normal’ ones.

Figure 1: The relation of the IATEs and ITEs to the propensity score



Note: Results are obtained from validation data (N=5’000).

These IATEs are also used to compute the ATE and the GATEs. The true ATE is taken as average of the IATEs in the validation sample, while the true GATEs are taken as averages of the IATEs in the respective groups. There are two types of GATEs considered: The first type consists of two GATEs, one for man (56%) and one for women (44%). The second type of GATEs considers 32 yearly age categories (24-55). As younger individuals are more likely to become unemployed in Switzerland, the largest, i.e. youngest, age group has about three times as many observations as the smallest, i.e. oldest, age group.

In addition to varying the IATEs and the corresponding aggregate effects, we also vary the assignment process by considering random assignment of the treatment as in a randomized control trial (RCT). Furthermore, we vary the sample size. Overall, this leads to 12 different DGPs in total (3 IATEs x 2 sample sizes x 2 selection processes). In addition to the estimators introduced in Section 4, we consider *basic* also in a version without the a priori sample split but with honest trees instead.[23] Finally, for the smaller sample, *N = 1'000*, estimation and inference is repeated on 1000 independent random training samples (replications, *R*). Since computation time is a constraint and because the larger sample produces estimates with lower variability, only 250 replications are used for *N = 4'000*.

5.2 Results

5.2.1 General remarks

For each replication, we obtain 5'035 results in the validation sample, consisting of 5000 estimates of different IATE, 32 and 2 estimates of the GATEs, and 1 estimate of the ATE. For each of these parameters, we compute the usual measures for point estimators, like bias (col. (1) of the following tables), standard deviation (col. (8)), mean squared error (MSE, col. (4)), skewness (col. 5), and kurtosis (col. 6). The Jarque-Bera statistic (JB, col. (7)) summarizes these moments. These three measures are used to check whether the estimators are normally distributed (JB is $\chi^2(2)$ distributed when the estimators are normally distributed, and shifts to the right when they are not; the 5% and 95%-critical values are 6 and 9.2, respectively). Finally, we compute the bias of the estimated standard errors (col. 9) and the coverage probability of the

[23] So that for each random subsample used for a particular tree the data is split randomly into two parts: Half the data is used to build the tree, and half the data is used to estimate the effects given the tree.

90% confidence interval (col. 10).[24] For the MSE, we also compute measures for its variability across replications to assess the simulation error (contained in footnote of tables).

It is nonsensical to report these measures for all 5035 parameters. While we report the measures for the ATE directly, we aggregate the measures for the IATE and the GATEs by taking their average (or their median to check the relevance of outliers) across groups. Note that due to the way the estimators are constructed, the bias of the ATE and the average biases of the IATEs are identical. Since this type of cancellation of biases for the IATEs is undesirable in their quality measure (as a negative bias is as undesirable as a positive one), we also report their average absolute bias (col. 1). Finally, we report the standard deviation of all the true (2) and estimated GATEs and IATEs (3) to see how the estimators capture the cross-sectional variability of the effect heterogeneity.

5.2.2 Detailed results

We begin the discussion of the simulation results by comparing *basic*, *OneF.VarT*, and *OneF.MCE* for the three DGPs with selection-on-observables in Table 1 (*N*=1’000) and Table 2 (*N*=4’000). These tables are followed by Table 3 (*N*=1’000) and 4 (*N*=4’000) with the penalized estimators (*OneF.VarT.Penalty*, *OneF.MCE.Penalty*). A symmetric set of results for the DGPs without selectivity, i.e. the experimental setting, is contained in Appendix C.1. Appedix C.2 presents results for an alternative specification of the IATE, while Appendix C.3 presents results showing how the performance of the penalised estimators changes with the magnitude of the penalty terms. Two further appendices contain the results for the one-sample version of *basic* (C.4) and for *OneF* (C.5), which turned out to be clearly dominated by several of the other estimators.

---

[24] This is the share of replications for which the true value was included in the 90% confidence interval. 90% instead of the more common 95% is used as evaluation quantile, because the number of replications may be too small (in particular for the larger sample) to estimate the more extreme tail probability precisely. Since all estimators are normally distributed, the particular tail quantile should hardly affect the conclusions.

Considering the overall relative performance of the unpenalised estimators based on their MSE for the IATE, for the smaller sample (Table 1) it is difficult to rank the three estimators. While *basic* dominates when effects are strong, it performs worst when there are no effects at all. This is to some extent expected: *Basic* is very flexible in the sense of using different forests for the estimation of $\mu(1,x)$ and $\mu(0,x)$. It appears that this flexibility leads to lower bias when $\mu(1,x)$ and $\mu(0,x)$ differ substantially (DGP with strong effects), while when these two conditional expectations are very similar, there is an efficiency loss due to the unnecessary flexibility. *OneF.VarT* and *OneF.MCE* perform broadly similarly for this sample size.

Since all estimators are substantially biased and fail to recover most of the cross-sectional heterogeneity of the IATE, one may conclude that a sample size of *N*=1'000 is too small for a reliable performance of these estimators. Nevertheless, it is interesting to note that even in this case, all estimators appear to be normally distributed, and that the estimated standard errors are in line with the true standard errors for the aggregated effects, while they are too large for the IATEs. These features about the distribution of the estimators and the bias of the standard errors are common to all estimators considered (the one-sample version of the *basic* estimator being the exception) for all DGPs. Thus, they will not be repeated in the discussions that follow.

Most of the coverage probabilities appear to be far too small (with one exception), in particular when the true effect size is non-zero. This is however not the result of an underestimation of the standard errors, but comes from the bias of the point estimators.

Table 1: Simulation results for DGP with selectivity: Basic, OneF.VarT, OneF.MCE, N=1000

			True & estimated effects			**Estimation error of effects (averages)**					**Estimation of std. error**	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No effect, with selectivity, *N* = 1000**						
ATE	*1*	*Basic*	1.13	-	-	2.64	0.1	3.3	3-4	1.17	0.04	77
GATE	*2*		1.13	-	-	2.71	0.1	3.2	3.7	1.19	0.05	78
GATE	*32*		1.09	0	0.12	2.78	0.1	3.3	3.9	1.25	0.08	80
IATE	*5000*		1.14	0	0.60	5.11	0.0	3.1	2.4	1.86	0.42	90
ATE	*1*	*OneF.*	1.23	-	-	2.80	-0.1	3.0	0.8	1.12	0.11	76
GATE	*2*	*VarT*	1.24	-	-	2.84	-0.1	3.0	0.9	1.14	0.13	76
GATE	*32*		1.23	0	0.04	2.85	-0.1	3.0	0.8	1.15	0.15	78
IATE	*5000*		1.24	0	0.39	3.98	0.0	3.0	1.9	1.51	0.53	91
ATE	*1*	*OneF.*	1.11	-	-	2.53	-0.1	2.9	2.0	1.14	0.06	77
GATE	*2*	*MCE*	1.11	-	-	2.58	-0.1	2.9	2.1	1.15	0.08	78
GATE	*32*		1.10	0	0.06	2.64	-0.1	2.9	1.7	1.20	0.13	81
IATE	*5000*		1.11	0	0.33	3.87	0.0	2.9	2.0	1.59	0.56	93
	DGP:					**Normal effect, with selectivity, *N* = 1000**						
ATE	*1*	*Basic*	1.90	-	-	4.92	-0.1	2.7	5.4	1.14	0.08	53
GATE	*2*		1.90	-	-	4.96	-0.1	2.7	4.7	1.16	0.09	54
GATE	*32*		1.87	0.17	0.09	4.98	-0.1	2.8	4.2	1.22	0.13	60
IATE	*5000*		1.91	1.72	0.96	8.47	-0.1	3.1	3.4	1.86	0.46	79
ATE	*1*	*OneF.*	2.09	-	-	5.64	-0.1	3.0	0.7	1.14	0.11	48
GATE	*2*	*VarT*	2.09	-	-	5.67	-0.1	3.0	0.7	1.15	0.12	50
GATE	*32*		2.06	0.17	0.07	5.60	-0.1	3.0	0.8	1.16	0.15	53
IATE	*5000*		2.09	1.72	0.75	8.01	0.0	3.0	3.0	1.55	0.54	74
ATE	*1*	*OneF.*	1.97	-	-	5.28	-0.1	3.1	1.6	1.18	0.04	50
GATE	*2*	*MCE*	1.97	-	-	5.31	-0.1	3.1	2.2	1.19	0.06	52
GATE	*32*		1.94	0.16	0.06	5.32	-0.1	3.0	2.0	1.24	0.11	56
IATE	*5000*		1.99	1.72	0.55	8.43	0.0	3.0	2.0	1.63	0.56	75
	DGP:					**Strong effect, with selectivity, *N* = 1000**						
ATE	*1*	*Basic*	3.22	-	-	12.01	0.1	2.7	4.0	1.26	0.01	20
GATE	*2*		3.22	-	-	11.99	0.1	2.7	3.8	1.28	0.03	22
GATE	*32*		3.18	0.66	0.39	11.98	0.1	2.8	2.5	1.34	0.06	27
IATE	*5000*		3.33	6.87	3.91	28.63	0.0	2.9	2.9	2.24	0.35	65
ATE	*1*	*OneF.*	6.47	-	-	15.62	-0.1	2.9	2.8	1.46	-0.08	18
GATE	*2*	*VarT*	3.66	-	-	15.53	-0.1	2.9	2.5	1.47	-0.07	19
GATE	*32*		3.61	0.66	0.25	15.39	-0.1	2.9	2.4	1.48	-0.02	22
IATE	*5000*		4.52	6.87	2.81	36.76	-0.1	2.9	6.7	2.13	0.23	51
ATE	*1*	*OneF.*	3.48	-	-	14.09	-0.2	3.1	1.4	1.40	-0.01	20
GATE	*2*	*MCE*	3.46	-	-	13.99	-0.2	3.2	1.4	1.41	0.01	21
GATE	*32*		3.42	0.66	0.25	13.94	-0.2	3.1	1.4	1.42	0.05	25
IATE	*5000*		4.26	6.87	2.56	37.01	-0.1	3.0	2.0	2.04	0.35	57

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.10-0.16 for ATE/GATE and 0.14-0.43 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

Table 2: Simulation results for DGP with selectivity: Basic, OneF.VarT, OneF.MCE, N=4000

	Groups #	Est.	True & estimated effects: Avg. bias	X-sectional std. dev. true	X-sectional std. dev. est.	Estimation error of effects (averages): MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Estimation of std. error: Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No effect, with selectivity, *N* = 4000**						
ATE	*1*	*Basic*	0.84	-	-	1.09	0.0	2.8	0.2	0.63	0.00	60
GATE	*2*		0.85	-	-	1.15	0.0	2.8	0.9	0.65	0.01	63
GATE	*32*		0.78	0	0.17	1.18	-0.1	2.9	1.1	0.73	0.06	73
IATE	*5000*		1.00	0	0.86	3.84	0.0	3.0	2.4	1.54	0.24	87
ATE	*1*	*OneF.*	1.05	-	-	1.43	0.1	2.7	1.5	0.58	0.09	55
GATE	*2*	*VarT*	1.05	-	-	1.47	0.1	2.7	1.6	0.60	0.11	58
GATE	*32*		1.03	0	0.06	1.44	0.1	2.8	1.4	0.61	0.12	61
IATE	*5000*		1.06	0	0.57	2.60	0.0	2.9	1.7	1.08	0.53	89
ATE	*1*	*OneF.*	0.86	-	-	1.06	-0.1	2.8	0.6	0.57	0.05	59
GATE	*2*	*MCE*	0.87	-	-	1.08	-0.1	2.8	0.7	0.58	0.07	63
GATE	*32*		0.84	0	0.08	1.13	-0.1	2.9	1.1	0.64	0.14	75
IATE	*5000*		0.87	0	0.43	2.18	0.0	3.0	2.4	1.12	0.52	94
	DGP:					**Normal effect, with selectivity, *N* = 4000**						
ATE	*1*	*Basic*	1.39	-	-	2.30	0.3	3.0	2.9	0.62	0.02	31
GATE	*2*		1.39	-	-	2.35	0.2	3.1	2.4	0.64	0.03	35
GATE	*32*		1.35	0.17	0.12	2.40	0.2	2.9	3.3	0.74	0.06	48
IATE	*5000*		1.48	1.72	1.30	5.71	0.0	3.0	2.3	1.58	0.24	78
ATE	*1*	*OneF.*	1.78	-	-	3.57	-0.2	3.0	1.0	0.63	0.05	17
GATE	*2*	*VarT*	1.78	-	-	3.58	-0.1	3.0	1.4	0.65	0.06	18
GATE	*32*		1.75	0.17	0.09	3.52	-0.1	3.0	1.0	0.66	0.08	22
IATE	*5000*		1.79	1.72	1.13	5.31	-0.1	3.1	5.7	1.14	0.50	73
ATE	*1*	*OneF.*	1.46	-	-	2.50	-0.6	3.7	20.1	0.61	0.04	22
GATE	*2*	*MCE*	1.45	-	-	2.50	-0.6	3.6	17.4	0.63	0.06	27
GATE	*32*		1.43	0.17	0.07	2.53	-0.5	3.4	11.9	0.67	0.12	40
IATE	*5000*		1.50	1.72	0.79	4.87	-0.1	3.0	3.2	1.16	0.50	76
	DGP:					**Strong effect, with selectivity, *N* = 4000**						
ATE	*1*	*Basic*	2.19	-	-	5.21	0.1	3.0	0.8	0.64	0.04	5
GATE	*2*		2.20	-	-	5.28	0.1	3.0	1.3	0.66	0.05	5
GATE	*32*		2.16	0.66	0.52	5.28	0.1	3.2	2.4	0.74	0.08	15
IATE	*5000*		2.38	6.87	5.12	14.78	0.0	3.0	2.4	1.69	0.30	67
ATE	*1*	*OneF.*	2.69	-	-	8.29	-0.1	2.7	1.4	1.01	-0.23	10
GATE	*2*	*VarT*	2.68	-	-	8.23	-0.1	2.7	1.3	1.01	-0.21	10
GATE	*32*		2.64	0.66	0.38	8.13	-0.1	2.7	1.5	1.03	-0.17	14
IATE	*5000*		3.38	6.87	4.49	19.78	-0.1	2.9	3.5	1.74	0.06	51
ATE	*1*	*OneF.*	1.91	-	-	4.24	0.5	3.2	8.9	0.77	0.08	28
GATE	*2*	*MCE*	1.90	-	-	4.23	0.4	3.3	10.1	0.78	0.09	29
GATE	*32*		1.87	0.66	0.44	4.21	0.4	3.2	8.3	0.80	0.13	37
IATE	*5000*		2.38	6.87	4.49	13.54	0.2	3.3	7.3	1.45	0.42	68

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.05-0.13 for ATE/GATE and 0.09-0.23 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

When the sample size quadruples (Table 2), the biases and variances of all estimators shrink substantially. While for the ATE and GATEs these reductions are compatible with a parametric convergence rate, for the IATE the reduction is substantially slower. The MSE based ranking of the estimators is now less ambiguous. For the IATE, *OneF.MCE* performs best in all three DGPs, while *basic* is only competitive when the effects are strongest. *OneF.VarT* performs worse than *OneF.MCE*, likely because it has the largest bias of these three estimators. A similar picture emerges for the aggregated effects: *OneF.VarT* appears always to be the worst estimator, again due to its larger bias. These results suggest that taking account of correlated estimation errors across treatment samples when building the Causal Forest has the desired effect. Coverage probabilities are all too small because to the biases of the estimators.

A comparison of these results with the results obtained for the one-sample version of *basic* with honesty (details in Appendix C.4) leads to two observations. A first observation is that despite the fact that it uses honesty (see WA18) instead of sample splitting, there appears to be no consistent gain in precision or in MSE compared to *basic*. The second observation is that, as expected, inference becomes problematic as estimated standard errors are somewhat biased downward and coverage consequently gets worse (at least for the larger sample).

OneF (details in Appendix C.5) performs well in the case when there is no effect, as is expected, because in this case the dependence of the conditional expectation of the outcome on covariates is the same among the treated and controls. In the other cases, one of the other estimators always dominates it.

When considering the estimators that account for propensity score heterogeneity by adding a penalty term to the splitting criterion, *OneF.VarT.Penalty* and *OneF.MCE.Penalty* (Tables 3 and 4), it is apparent that they dominate the estimators without penalty in terms of MSE. The MSE reduction comes almost entirely from a substantial reduction in the biases of the estimators (at the expense of some additional variance). Consequently, the reduction in the bias improves the coverage probabilities substantially bringing them close to or above their nominal level for all DGPs in the larger sample, while they are still somewhat too small when effects are strong (which goes along with a larger bias) in the smaller sample. The bias reduction also goes hand in hand with a better recovery of the cross-sectional heterogeneity of the GATEs and IATEs.

Table 3: Simulation results for DGP with selectivity: OneF.VarT.Penalty, OneF.MCE.Penalty, N=1000

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No effect, with selectivity, *N* = 1000**						
ATE	*1*	*OneF.*	0.96	-	-	2.46	-0.1	3.1	2.6	1.24	0.12	84
GATE	*2*	*VarT.*	0.96	-	-	2.48	-0.1	3.1	2.8	1.25	0.13	84
GATE	*32*	*Pen*	0.96	0	0.04	2.51	-0.1	3.1	2.9	1.26	0.14	85
IATE	*5000*		0.97	0	0.56	4.07	-0.1	3.0	11.3	1.68	0.57	93
ATE	*1*	*OneF.*	0.63	-	-	2.26	-0.1	3.1	0.9	1.37	0.16	89
GATE	*2*	*MCE.*	0.63	-	-	2.29	-0.1	3.1	1.0	1.38	0.17	90
GATE	*32*	*Pen*	0.63	0	0.06	2.34	-0.1	3.1	0.9	1.39	0.18	90
IATE	*5000*		0.88	0	0.73	4.70	-0.1	3.2	9.5	1.93	0.63	93
	DGP:					**Normal effect, with selectivity, *N* = 1000**						
ATE	*1*	*OneF.*	1.50	-	-	4.06	0.0	3.1	0.20	1.34	0.09	71
GATE	*2*	*VarT.*	1.50	-	-	4.09	0.0	3.1	0.21	1.35	0.10	72
GATE	*32*	*Pen*	1.49	0.17	0.16	4.06	0.0	3.1	0.53	1.36	0.12	73
IATE	*5000*		1.51	1.72	1.26	6.40	-0.1	3.5	42.49	1.89	0.53	89
ATE	*1*	*OneF.*	1.11	-	-	3.35	-0.1	2.9	2.6	1.46	0.10	81
GATE	*2*	*MCE.*	1.11	-	-	3.36	-0.1	3.1	3.1	1.46	0.11	82
GATE	*32*	*Pen*	1.10	0.17	0.13	3.37	-0.1	2.9	2.7	1.47	0.13	83
IATE	*5000*		1.12	1.72	1.50	5.87	-0.2	3.1	2.0	2.05	0.56	90
	DGP:					**Strong effect, with selectivity, *N* = 1000**						
ATE	*1*	*OneF.*	1.79	-	-	5.64	0.0	2.8	2.2	1.56	0.21	74
GATE	*2*	*VarT.*	1.79	-	-	5.62	0.0	2.8	2.2	1.56	0.21	75
GATE	*32*	*Pen*	1.75	0.66	0.48	5.58	0.0	2.8	2.2	1.57	0.23	76
IATE	*5000*		2.38	6.87	5.14	14.62	0.0	3.0	2.4	2.44	0.63	82
ATE	*1*	*OneF.*	2.29	-	-	8.01	0.0	2.8	1.8	1.66	-0.03	60
GATE	*2*	*MCE.*	2.28	-	-	7.97	0.0	2.8	1.7	1.66	-0.02	60
GATE	*32*	*Pen*	2.24	0.66	0.41	7.91	0.0	2.8	2.0	1.67	0.00	62
IATE	*5000*		2.96	6.87	4.42	19.76	0.1	2.9	9.6	2.47	0.30	72

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.10-0.16 for ATE/GATE and 0.14-0.43 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

With respect to ranking the two estimators, a similar picture appears as before: While for the smaller sample a ranking is difficult, for the larger sample, the ranking is the same as for *OneF.MCE* versus *OneF.VarT*: *OneF.MCE.Penalty* has a smaller MSE for all three DGPs.

Table 4: Simulation results for DGP with selectivity: OneF.VarT.Penalty, OneF.MCE.Penalty, N=4000

			True & estimated effects			**Estimation error of effects (averages)**					**Estimation of std. error**	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No effect, with selectivity, *N* = 4000**						
ATE	*1*	*OneF.*	0.86	-	-	1.22	0.0	3.0	0.0	0.69	0.13	77
GATE	*2*	*VarT.*	0.86	-	-	1.23	0.0	3.0	0.3	0.70	0.14	77
GATE	*32*	*Pen*	0.85	0	0.05	1.23	0.0	3.0	0.4	0.71	0.15	79
IATE	*5000*		1.00	0	0.71	2.96	0.0	3.0	1.3	1.30	0.62	92
ATE	*1*	*OneF.*	0.25	-	-	0.66	-0.1	2.8	1.0	0.77	0.20	94
GATE	*2*	*MCE.*	0.25	-	-	0.67	-0.1	2.8	0.9	0.78	0.20	95
GATE	*32*	*Pen*	0.26	0	0.05	0.70	-0.1	2.9	1.6	0.80	0.22	95
IATE	*5000*		0.62	0	0.59	2.42	0.0	2.9	1.6	1.41	0.66	96
	DGP:					**Normal effect, with selectivity, *N* = 4000**						
ATE	*1*	*OneF.*	1.21	-	-	1.93	0.2	3.6	5.6	0.70	0.12	58
GATE	*2*	*VarT.*	1.20	-	-	1.94	0.2	3.6	5.0	0.71	0.13	61
GATE	*32*	*Pen*	1.19	0.17	0.13	1.62	0.2	3.6	5.6	0.71	0.14	63
IATE	*5000*		1.24	1.72	1.61	3.78	0.0	2.9	1.7	1.34	0.62	88
ATE	*1*	*OneF.*	0.39	-	-	0.89	-0.1	2.9	0.9	0.86	0.10	90
GATE	*2*	*MCE.*	0.39	-	-	0.90	-0.1	2.9	0.8	0.87	0.11	90
GATE	*32*	*Pen*	0.38	0.17	0.16	0.93	-0.1	2.8	1.1	0.89	0.14	91
IATE	*5000*		0.51	1.72	1.89	2.83	-0.1	3.0	2.3	1.53	0.53	94
	DGP:					**Strong effect, with selectivity, *N* = 4000**						
ATE	*1*	*OneF.*	0.88	-	-	1.64	0.3	2.9	4.0	0.93	0.07	82
GATE	*2*	*VarT.*	0.87	-	-	1.64	0.3	2.9	4.0	0.93	0.09	82
GATE	*32*	*Pen*	0.85	0.66	0.56	1.64	0.3	2.9	3.7	0.95	0.11	82
IATE	*5000*		1.37	6.87	5.91	6.61	0.2	3.2	4.8	1.83	0.46	89
ATE	*1*	*OneF.*	0.94	-	-	1.56	0.3	3.7	10.0	0.82	0.13	80
GATE	*2*	*MCE.*	0.95	-	-	1.56	0.3	3.7	9.7	0.83	0.14	80
GATE	*32*	*Pen*	0.92	0.66	0.55	1.58	0.3	3.6	8.5	0.84	0.17	82
IATE	*5000*		1.50	6.87	5.74	6.39	0.1	3.1	4.8	1.59	0.47	85

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.05-0.13 for ATE/GATE and 0.09-0.23 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

It is interesting to see how the performance of these estimators changes when the values of the penalty term changes. In this exercise, the bias-variance trade-off becomes apparent (see Appendix C.3 for details). If there is selection on observables, increasing the penalty term reduces the bias (and thus improves inference) but at the same time increases the variance. However, when the penalty becomes too large, i.e. it dominates the splitting criterion, the bias no longer declines. Indeed, above a certain level a further increase of the penalty term increases both, bias and variance.

The simulations with random assignment in Appendix C.1 show the importance of the selection process for the differential performance of the estimators. The first finding for the experimental case is that the other four estimators considered here dominate *basic* (with the exception of the strong effect DGP in the small sample). The second finding is indeed a non-finding in the sense that it is very difficult to rank the other four estimators among themselves as they perform very similarly. The good news of this finding is that the penalty term does not inflict much harm when used in a case in which it is redundant (as there is no selection bias in these DGPs).

As a robustness check with respect to different specifications of the IATE, in Appendix C.2 we investigate the case when $\xi(x)$ is a linear function of the (insured) earnings of the unemployed instead of the propensity score, thus weakening the link between effect heterogeneity and selection bias. The results show that for both sample sizes *OneF.MCE* is the dominant estimator of the IATE while *OneF.MCE.Penalty* is the best estimator for all other aggregation levels. Thus, one the one hand, this confirm the importance of taken the identification condition (and thus MCE) into account when constructing the splitting rule. On the other hand, it also shows that there may cases with selection bias where penalty term is not so important, at least for IATE.

In conclusion, when there is selection on observables, the penalized estimators based on the MCE-based splitting rules have generally lower bias (which is important for inference) as well as lower MSE. In this case, *OneF.MCE.Penalty* outperforms the other estimators in the larger samples. In the smaller samples, as well as in the experimental context, the rankings are less clear, at least among the estimators based on using the same forests for the different treatment subpopulations. Generally, inference tends to be conservative when biases are low and samples are large, in particular for the IATEs.

6 Application to the evaluation of a job search programme

This section shows how the estimator that came out best in the Empirical Monte Carlo study, i.e. *OneF.MCE.Penalty*, can be productively applied in empirical studies. To this extent, we rely on the data set that formed the basis of the Empirical Monte Carlo study above, but we use a more homogeneous subsample of men living in cantons where German is the dominant language (about 38'000 observations). As before, we investigate the effect of participating in a job search programme (share of participants is 8%) on months of employment accumulated over 9 months, as well as over 3 years, respectively. Based on a previous reanalysis of the effects of this programme in Knaus, Lechner, and Strittmatter (2017), we expect to find essentially zero effects with very limited heterogeneity over three years, but substantial heterogeneity over the first 9-month period, the so-called lock-in period. On top of this, we expect selective programme participation (see below). Thus, this is a challenging setting.[25]

The estimation with *OneF.MCE.Penalty* is based on 1'000 subsampling replications (50% subsampling share). The minimum leaf size N^{min} = (13, 50) and the number of coefficients used for leaf splitting M = (4, 10, 27) (out of 31 ordered and 8 unordered variables;[26] 6 variables have been deleted a priori as their unconditional correlation with both the outcome and programme participation is below 1%) are treated as tuning parameters. N^{min} = 13 and M = 27 are chosen by out-of-bag minimisation of the optimization criterion of this estimator.

The matching literature has shown that it may be important to ensure that common support actually holds post-estimation (e.g. Imbens, 2004). This issue has not yet been discussed

[25] This section is merely a demonstration of possible findings. Therefore, for the sake of brevity, it is written very densely. For more details on programmes, institutional details, data, the reader is referred to the previous papers using this setting, e.g. Huber, Lechner, and Mellace (2017), Knaus, Lechner, and Strittmatter (2017, 2018) and the references therein.

[26] *k-means-clustering* is used to deal with unordered variables (as proposed in Chou, 1991, and Hastie, Tibshirani, and Friedman, 2009).

in the context of machine learning methods of the type proposed here.[27] The key issue is that for every combination of values of *X* that is relevant for estimation, there should be enough treated and non-treated observations for reliable estimation of the effects. While this has been shown to be relevant for the (conventional) estimation of *ATE* and related aggregated parameters (e.g. Lechner and Strittmatter, 2017), it is even more important when estimating *IATE(x)*, which will be particularly sensitive to support violations close to their evaluation points. Our proposal is to predict the propensity scores using the trees of the already estimated (outcome) forest in a first step. In a second step, all values of *X* related to very few predicted treated or controls are deleted. Here, we deleted all values of *X* with propensity scores outside the [5%, 95%]-interval. This led to discarding about 0.5% (9 months) and 1.1% (3 years) of the observations. In the discarded group, almost 80% of the observations have a native language that is different from any of the main Swiss official languages (German, French, and Italian).

From the point of view of eliminating selection bias, any good estimator in this setting should ensure covariate balance. Table 5 therefore shows the means and standardized differences for selected covariates (mostly directly related to the pre-specified heterogeneity shown in Table 6 below) prior to the estimation. To see how the different forests related to the two respective outcome variable change the balancing, the same covariates are predicted using the estimated forests (and their implied weights; columns headed by 'post-estimation'). In both cases, the balancing improves considerably.

[27] For a general discussion on the implications of the overlap assumption in a high-dimensional covariate space, see D'Amour, Ding, Feller, Lei, and Sekhon (2018).

Table 5: Pre- and post-estimation balancing of covariates

	Pre-estimation				**Post-estimation**	
Features	Mean *d=1*	Mean *d=0*	Differ-ence	Stand. diff. (%)	Difference	
Outcome: Months of employment in					**9 month**	**3 years**
# of pre UE employment episodes (last 2 years)	0.09	0.12	-0.027	20	-0.019	-0.016
Foreign native language	0.24	0.32	-0.08	19	-0.05	-0.05
Employability (1, 2, 3)	2.01	1.93	0.09	18	0.02	0.05
Pre-UE monthly earnings (in CHF)	5435	4899	537	26	50	26

Note: The standardized difference (stand. diff.) is defined as $Stand.diff = \frac{\left|\bar{x}^1 - \bar{x}^0\right|}{\sqrt{[Var(x^1) + Var(x^0)]/2}}$, where the superscripts 1 and 0 denote the subpopulations of treated and controls. Post-estimation results are obtained by treating the variables as outcomes and running them through the estimated forests. Since these forests differ for the 9 and the 36 month outcomes, the post-estimation results differ with respect to the outcome variable used to train the forests.

Tables 6 and 7 contain the results of the estimation for the ATE (upper panel), the various GATEs that appeared to be of a priori interest (in the middle of the table), as well as summary statistics for the IATEs (lower panel). The GATEs relate to effect heterogeneity in the number of unemployment episodes in the 2 years prior to the unemployment spell analysed (12 categories), the native language being a 'Swiss' one or a different one (2 cat.), the employability index (3 cat.), and the economic sector of the last occupation (16 cat.). As mentioned before, all effects have the interpretation of additional months of regular employment due to programme participation.

For the average treatment effect, the first four columns show the mean of the potential outcomes as well as their estimated standard errors. The remaining columns show the average effects, their estimation error, and the p-value for the hypothesis that the average effects are zero. The results imply that over nine months (3 years), these individuals work 1.6 (16.5) month if they participate in the programme and 2.1 (16.8) months if not. This negative short-run effect is statistically significant at conventional levels (despite inference being likely conservative), while the smaller 3-year effect is not.

Table 6: Average, group effects, and individualized effects: 9 months

Average potential outcomes				Average effects		
Treated		Controls				
Expectation	Stand. err.	Expectation	Stand. err.	ATE	Stand. err.	p-val. in %
1.59	0.06	2.09	0.02	-0.52	0.06	0.0

Group average treatment effects (GATEs)

Group		Expect.	Est. std.	p-val. in %	**Group**	Expect.	Est. std.
# E spells	**0**	-0.52	0.06	93	**Sector 1**	-0.56	0.07
# E spells	**1**	-0.51	0.06	37	**Sector 2**	-0.64	0.07
# E spells	**2**	-0.50	0.06	93	**Sector 3**	-0.51	0.06
# E spells	**3**	-0.50	0.07	43	**Sector 4**	-0.41	0.09
# E spells	**4**	-0.50	0.07	55	**Sector 5**	-0.44	0.07
# E spells	**5**	-0.49	0.07	87	**Sector 6**	-0.45	0.07
# E spells	**6**	-0.49	0.08	39	**Sector 7**	-0.50	0.07
# E spells	**7**	-0.51	0.07	30	**Sector 8**	-0.50	0.06
# E spells	**8**	-0.48	0.08	59	**Sector 9**	-0.53	0.06
# E spells	**9**	-0.51	0.08	76	**Sector 10**	-0.50	0.06
# E spells	**10**	0.53	0.10	-	**Sector 11**	-0.64	0.09
# E spells joint test (p-val in %)				94	**Sector 12**	-0.49	0.07
Swiss lang. native		-0.51	0.06	68	**Sector 13**	-0.54	0.07
Swiss lang. not nat.		-0.53	0.06	-	**Sector 14**	-0.45	0.07
Swiss joint test				68	**Sector 15**	-0.46	0.07
Employability good		-0.49	0.06	11	**Sector 16**	-0.47	0.07
Employabil. middle		-0.52	0.06	52	**Sector** joint test (p-val in %)		0.1
Employability bad		-0.54	0.07	-			
Employability joint test (p-val in %)				17			

Individualized average treatment effects (IATEs)

Mean	Standard deviation	Share < 0 in %	Share > 0 in %	Average standard error of estimate	Share significant at 5% level in %
-0.52	0.15	100	0	0.15	92

Note: *Expect.* and *est. std.* is defined for potential outcomes, effects, and their respective estimation errors. The p-values relate to hypothesis tests based of the (asymptotic) t- or Wald-type. For the ATE, it is the *t*-test that the effect is zero. For the GATEs, it is the *t*-test that the adjacent values are identical (therefore it is not given for the sectors that are not in any natural order) as well as the Wald test that all GATEs relating to this variables are identical. Therefore, this test has (# of groups-1) degrees of freedom.

The middle part of the tables contains the group-mean effects and their standard errors for the four different GATEs relating to the variables discussed above. The p-values shown, however, do not relate to the hypothesis that the particular effects are zero but to the hypothesis that the differences of the effects minus the ones in the next row are zero. Such a test contains relevant information only if the heterogeneity variable is ordered. Since this is not true for the economic sectors, the p-values are not shown for those GATEs. Finally, the last row for each GATE shows the p-value of a Wald-test (again, based on the weighted representation of the estimator, which allows uncovering the full variance-covariance matrix of the estimated

GATEs) with degrees of freedom equal to the number of different categories for the particular GATE for the hypothesis that all GATEs relating to this variable are equal.

No significant heterogeneity appears for the 3-year outcome, neither in a statistical nor in a substantive sense. However, for the 9-month employment outcome there appears to be heterogeneity with respect to the economic sector. Here, the Wald test clearly rejects homogeneity and the various negative effects differ substantially.[28]

While the estimated GATEs are presented in total since they are not based on too many different groups, this is impossible for the estimated IATEs. Thus, for a better description we may want to condense their information further. The first part of this exercise is reported in the lower panel of Table 6 and 7. The first two columns of these tables show the average and their standard deviation. Next, there is the share of IATEs with positive and negative values. Finally, we report the average of their estimated standard errors and the share of IATE's that are significantly different from zero (at the 5% level). For the 9-month outcome, all effects are negative and more than 90% significantly so (in a statistical sense). Their standard deviation across individuals is about 4 days. For the 3-year outcome, their standard deviation is larger, but this is essentially so because the potential outcomes are about 10 x larger. About two thirds of the IATEs are smaller than zero, and one third larger. However, only 2% of them are statistically significantly different from zero.

[28] Given the space constraints of this paper, we refrain from analysing and interpreting this heterogeneity further.

Table 7: Average, group effects, and individualized effects: 36 months

Average potential outcomes				**Average effects**		
Treated		Controls				
Expectation	Stand. err.	Expectation	Stand. err.	ATE	Stand. err.	p-val. in %
16.50	0.33	16.79	0.099	-0.30	0.34	39

Group average treatment effects (GATEs)

Group		Expect.	Est. std.	p-val. in % of diff	Group	Expect.	Est. std.
# E spells	**0**	-0.55	0.37	12	**Sector 1**	0.19	0.47
# E spells	**1**	-0.19	0.36	12	**Sector 2**	-0.62	0.42
# E spells	**2**	0.04	0.40	16	**Sector 3**	-0.30	0.37
# E spells	**3**	0.16	0.44	43	**Sector 4**	0.77	0.67
# E spells	**4**	0.22	0.50	9	**Sector 5**	0.04	0.42
# E spells	**5**	0.34	0.55	96	**Sector 6**	-0.13	0.40
# E spells	**6**	0.34	0.50	37	**Sector 7**	-0.27	0.46
# E spells	**7**	0.15	0.50	65	**Sector 8**	-0.25	0.39
# E spells	**8**	0.27	0.55	92	**Sector 9**	-0.44	0.44
# E spells	**9**	0.24	0.60	37	**Sector 10**	-0.32	0.37
# E spells	**10**	-0.16	0.60	-	**Sector 11**	-0.45	0.58
# E spells joint test				28	**Sector 12**	-0.25	0.42
Swiss lang. native		-0.22	0.40	59	**Sector 13**	-0.42	0.37
Swiss lang. not nat.		-0.33	0.36	35	**Sector 14**	-0.17	0.44
Swiss joint test				66	**Sector 15**	-0.29	0.38
Employability good		-0.09	0.38	81	**Sector 16**	-0.19	0.40
Employabil. middle		-0.33	0.35	35	**Sector joint test** (p-val in %)		41
Employability bad		-0.44	0.38	25			
Employability joint test (p-val in %)				34			

Individualized average treatment effects (IATEs)

Mean	Standard deviation	Share < 0 in %	Share > 0 in %	Average standard error of estimate	Share significant at 5% level in %
-0.30	0.86	64	36	1.10	0.02

Note: *Expect.* and *est. std.* is defined as potential outcomes and effects and their estimation errors. The p-values relate all to the hypothesis test based of the (asymptotic) t- or Wald-type statistic. For the ATE, it is the *t*-test that the effect is zero. For the GATEs, it is the *t*-test that the adjacent values are identical (therefore it is not given for the sectors that are not in any natural ordered) as well as the Wald test that all GATEs relating to this variables are identical. Therefore, this test has (# of groups-1) degrees of freedom.

The final step consists in a post-estimation analysis of the IATEs in order to be able to *describe* the correlation of the estimated IATE with exogenous variables. In principle, all standard descriptive tools could be used for this exercise. In Table 8, we report binary correlations of covariates with the estimated effects for variables for which the absolute level of such correlations is larger than 10%. In a multivariate analysis, there is the difficulty that the covariate space is very large. Thus, we use a dimension reduction tool, here the LASSO, and perform OLS estimation with the covariates that had a non-zero coefficient in the LASSO estimation (this is also called Post-LASSO; for theoretical properties of the Post-LASSO and its advantages over LASSO see, e.g., Belloni, Chernozhukov and Wei, 2016).[29] Since model selection (LASSO) and coefficient estimation are performed on two random, non-overlapping subsamples (50% each), the OLS standard errors remain valid given the selected model. However, of course they do not explicitly account for the fact that the dependent variable is an estimated quantity. Table 8 shows a selection of variables only, to serve as examples. Here, heterogeneity appears for both outcome variables, but related to variables not specified in the GATEs, like age difference of case worker and unemployed, age, number of employment spells, pre-unemployment time in employment and earnings, as well as the employability according to the judgement of the case worker. While these are highly significant predictors for the 3-year outcome, their 9-month effects are significant at approximately the 5% level.

Table 8: Selected coefficients of post-lasso OLS estimation of IATE

Variable	Coefficient	Standard error	t-value	p-value	Uncond. correlation (if > 10%) in %
Outcome: Months of employment in first 6 months					
|Age case worker – age UE| in years	-0.002	0.001	-2.0	4.7	-24
|Age case worker – age UE| < 5	not	selected	-	-	-
Age	0.45	0.22	2.0	4.6	27
# E spells	0.10	0.05	2.0	4.6	-
Time in employment before (share)	-0.10	0.05	-2.0	4.5	-16
Earnings before	-0.04	0.02	-1.9	5.2	-
Employability	-0.004	0.002	1.7	9.5	-
Outcome: Months of employment in first 3 years					
|Age case worker – age UE| in years	not	selected	-	-	11
|Age case worker – age UE| < 5	-0.12	0.03	-4.1	< 1%	-
Age	-0.01	0.001	-7.8	< 1%	-20
# E spells	0.12	0.01	14	<1%	26
Time in employment before (share)	-1.20	0.06	-21	<1%	-32
Earnings before	-0.008	0.0006	-14	<1%	-
Employability	-0.17	0.03	-7.3	<1%	-

Note: OLS regression. Dependent variable is the estimated IATE. Independent variables are those with non-zero coefficients in the LASSO estimation. OLS and LASSO were estimated on randomly splitted subsamples (to facilitate the use of ordinary, robust OLS standard errors). The penalty term of the LASSO estimation was chosen by minimization of the 10-fold cross-validated mean square prediction error and the one-standard error rule. R^2 = 71% (9 months), 20% (3 years). *N*=8990. A constant term and further variables capturing caseworker, local and individual characteristics are included as well.

[29] This approach is in spirit very similar to Zhao, Small, and Ertefaie (2017).

In summary, this section gave a first indication about the usefulness of the new methods in applications and provides some suggestions on how to deal with some practical issues (i.e. common support, balancing of covariates, describing the IATE) that arise when applying such machine learning tools in a causal framework.

7 Conclusion

In this paper, we develop and apply new estimators to estimate heterogeneous treatment effects for multiple treatments in a selection-on-observables setting. We compare them to existing estimation approaches in an empirically informed simulation study, and apply the best performing estimator to an empirical programme evaluation study. The new estimators are based on the idea of the Causal Forest approach proposed by Wager and Athey (2018). Causal Forests performed well in comparative simulation studies, once they are purged from the effects of confounding feature by additional estimation and transformation steps (see Knaus, Lechner, and Strittmatter, 2018).

The new estimators deviate from the original Causal Forest in that a new splitting criterion is proposed to build the trees that form the forest. The new splitting criterion has two components: First, we approximate the mean square error of the causal effects by combining the causal problem with the particular identification strategy directly. Second, a penalty term is added that favours splits that are heterogeneous with respect to the implied treatment propensity to reduce selection bias directly. It turned out that both changes usually worked as intended and that they can improve the performance of the Causal Forest approach substantially.

An additional advantage of the Causal Forest structure is that the proposed estimators have a representation of weighted means of the outcome variables. This makes them particularly amendable to using a common inference framework for estimation effects at various aggregation levels, like for the average treatment effect (ATE), the group treatment effect

(GATE), or the individualized treatment effect (IATE). The advantage is that ATE and GATEs can be obtained directly from aggregating the estimated IATEs without the need for different additional machine learning estimation at the various aggregation levels of interest.

Despite the good performance of the proposed methods in the simulation study and the application, many issues are still unresolved and deserve further inquiry: One open issue is how to choose the value of the penalty term in an optimal way (without having to pre-estimate a propensity score). Another issue is to explore alternative methods to estimate the mean correlated errors, which is the main innovation in the new splitting rule. Three other issues concern the proposed inference methods: First, it would be desirable to have a more solid theoretical justification why the inference methods work so well, e.g. by deriving explicitly the regularity conditions the weights have to fulfil to lead to a consistent standard error estimator. Second, this paper explored just one way on how to implement this weight based inference. It seems worthwhile to investigate alternatives, also with the goal of tackling the issue that the current standard errors for the IATEs in particular seem to be too large. Third, it would be useful to understand better the relation of the many other tuning parameters that come with these types of Random or Causal Forests (e.g., number of coefficients to be randomly chosen, minimum leaf size, subsampling size) to the quality of inference (and point estimation).

The Gauss code of the estimator is available at the websites of the author at *ResearchGate* and at www.michael-lechner.eu/statistical-software. A release of the code in other computer languages (R/Python) is planned.

References

Abadie, A., and G. W. Imbens (2006): Large Sample Properties of Matching Estimators for Average Treatment Effects, *Econometrica*, 74(1), 235-267.

Advani, A., T. Kitagawa, and T. Słoczyński (2018): Mostly Harmless Simulations? On the Internal Validity of Empirical Monte Carlo Studies, IZA DP No. 11862.

Abrevaya, J., Y.-C. Hsu, and R. P. Lieli (2015): Estimating Conditional Average Treatment Effects, *Journal of Business & Economic Statistics,* 33:4, 485-505, doi: 10.1080/07350015.2014.975555.

Ascarza, E. (2018): Retention Futility: Targeting High Risk Customers Might Be Ineffective. *Journal of Marketing Research*, 55(1), 80–98.

Athey, S. (2017): Beyond prediction: Using big data for policy problems, *Science* 355,483–485.

Athey, S., and G. W. Imbens (2016): Recursive Partitioning for Heterogeneous Causal Effects, *Proceedings of the National Academy of Sciences of the United States of America*, 113(27), 7353–7360.

Athey, S., and G. W. Imbens (2017): The State of Applied Econometrics: Causality and Policy Evaluation, *Journal of Economic Perspectives*, 31 (7). 3-32.

Athey, S., J. Tibshirani, and S. Wager (2018): Generalized Random Forests, *arXiv*: 1610.01271v4, forthcoming in the *Annals of Statistics*.

Belloni, A., V. Chernozhukov, and Y. Wei (2016): Post-Selection Inference for Generalized Linear Models with Many Controls, *Journal of Business & Economic Statistics*, 34, 606–619.

Belloni, A., V. Chernozhukov, I. Fernández-Val, and C. Hansen (2017): Program Evaluation and Causal Inference with High-Dimensional Data, *Econometrica*, 85, 233–298.

Bertrand, M., B. Crépon, A. Marguerie, and P. Premand (2017): Contemporaneous and Post- Program Impacts of a Public Works Program: Evidence from Côte d'Ivoire. Washington D.C.

Breiman, L. (2001): Random Forests, *Machine Learning*, 45(1), 5–32.

Breiman, L., J. Friedman, C. J. Stone, and R. A. Olshen (1984): Classification and regression trees. *CRC press*.

Chen, S, L. Tian, T. Cai, and M. Yu (2017): A General Statistical Framework for Subgroup Identification and Comparative Treatment Scoring. *Biometrics*, 73, 1199–1209.

Chou, P. A. (1991): Optimal Partitioning for Classification and Regression Trees, IEEE Transactions on Pattern Analysis and Machine Intelligence, 13 (4), 340-354.

D'Amour, A., P. Ding, A. Feller, L. Lei, and J. Sekhon (2018): Overlap in observational studies with high-dimensional covariates, *arXiv:* 1711.02582v3.

Davis, J. M., and S. B. Heller (2017): Using causal forests to predict treatment heterogeneity: An application to summer jobs, *American Economic Review*, 107, 546–550.

Denil, M., D. Matheson, N. de Freitas (2014): Narrowing the Gap: Random Forests In Theory and In Practice, *Proceedings of the 31 st International Conference on Machine Learning*, Beijing, China, 2014. JMLR: W&CP, vol. 32.

Fan, Y., J. L. and J. Wang (2018): DNN: A Two-Scale Distributional Tale of Heterogeneous Treatment Effect Inference, *arXiv*: 1808.08469v1.

Friedberg, R., J. Tibshirani, S. Athey, and S. Wager (2018): Local Linear Forests, *arXiv*: 1807.11408v1.

Gerfin, M., and M. Lechner (2002): Microeconometric Evaluation of the Active Labour Market Policy in Switzerland, *The Economic Journal*, 112, 854-893.

Hastie, T., R. Tibshirani, and J. Friedman (2009): *The Elements of Statistical Learning: Data Mining, Inference, and Prediction*, 2nd edition, Springer (10th printing with corrections, 2013).

Hill, J. L. (2011): Bayesian Nonparametric Modelling for Causal Inference, *Journal of Computational and Graphical Statistics*, 20(1), 217–240.

Huber, M., M. Lechner, and C. Wunsch (2013): The performance of estimators based on the propensity score, *Journal of Econometrics*, 175 (1), 1-21.

Huber, M., M. Lechner, and G. Mellace (2017): Why Do Tougher Caseworkers Increase Employment? The Role of Programme Assignment as a Causal Mechanism, *Review of Economics and Statistics*, 99 (1), 180-183.

Hurwicz, L. (1950): Generalization of the Concept of Identification, in *Statistical Inference in Dynamic Economic Models*, Cowles Commission Monograph, 10, New York: Wiley.

Imai, K., and M. Ratkovic (2013): Estimating treatment effect heterogeneity in randomized program evaluation, *Annals of Applied Statistics*, 7(1), 443–470.

Imbens, G. W. (2000): The Role of the Propensity Score in Estimating Dose-Response Functions, *Biometrika*, 87, 706-710.

Imbens, G. W. (2004): Nonparametric Estimation of Average Treatment Effects under Exogeneity: A Review, *The Review of Economics and Statistics*, 86, 4–29.

Card, D., J. Kluve, and A. Weber (2017): What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations, forthcoming in the *Journal of the European Economic Association*.

Knaus, M. C., M. Lechner, and A. Strittmatter (2017): Heterogeneous Employment Effects of Job Search Programs: A Machine Learning Approach, *arXiv: 1709.10279v2*.

Knaus, M. C., M. Lechner, and A. Strittmatter (2018): The Performance of Machine Learning Approaches to Estimate Heterogeneous Policy Effects, *arXiv: 1810.13237v1*.

Künzel, S. R., J. S. Sekhon, P. J. Bickel, and B. Yu (2018): Meta-learners for Estimating Heterogeneous Treatment Effects using Machine Learning, *arXiv*: *1706.03461v4.*

Lechner, M. (2001): Identification and Estimation of Causal Effects of Multiple Treatments under the Conditional Independence Assumption, in: M. Lechner and F. Pfeiffer (eds.), *Econometric Evaluation of Active Labour Market Policies*, 43-58, Heidelberg: Physica.

Lechner, M., and A. Strittmatter (2017): Practical Procedures to Deal with Common Support Problems in Matching Estimation, forthcoming in *Econometric Reviews*, doi: 10.1080/07474938.2017.1318509.

Lechner, M., and C. Wunsch (2013): Sensitivity of matching-based program evaluations to the availability of control variables, *Labour Economics: An International Journal*, 21, 111-121.

Lee, S., R. Okui, and Y.-J. Whang (2017): Doubly robust uniform confidence band for the conditional average treatment effect function, *Journal of Applied Econometrics*, 32:1207–1225, doi: 10.1002/jae.2574.

Nie, X., and S. Wager (2018): Quasi-Oracle Estimation of Heterogeneous Treatment Effects, *arXiv*: *1712.04912v2*.

Oprescu, M., V. Syrgkanis, and Z. S. Wu (2018): Orthogonal Random Forest for Heterogeneous Treatment Effect Estimation, *arXiv*: *1806.03467v2*.

Powers, S., J. Qian, K. Jung, A. Schuler, N. H. Shah, T. Hastie, and R. Tibshirani (2018): Some methods for heterogeneous treatment effect estimation in high dimensions, *Statistics in Medicine*, 37(11), 1767–1787.

Qian, M., and S. A. Murphy, (2011): Performance Guarantees for Individualized Treatment Rules, *Annals of Statistics*, 39(2), 1180-1210.

Ramachandra, V. (2018): Deep Learning for Causal Inference, *arXiv*: *1803.00149v1*.

Rubin, D. B. (1974): Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies, *Journal of Educational Psychology*, 66, 688-701.

Schuler, A., K. Jung, R. Tibshirani, T. Hastie, and N. Shah (2017): Synth Validation: Selecting the Best Causal Inference Method for a Given Dataset, *arXiv*: *1711.00083v1*.

Schwab, P., L. Linhardt, and W. Karlen (2018): Perfect Match: A Simple Method for Learning Representations for Counterfactual Inference with Neural Networks, *arXiv*: *1810.00656v2*.

Shalit, U., F. D. Johansson, and D. Sontag (2017): Proceedings of the 34th International Conference on Machine Learning, Sydney, Australia, PMLR 70.

Strittmatter, A. (2018): What is the Value Added of Using Causal Machine Learning Methods in the Evaluation of a Welfare Experiment? , mimeo.

Su, X., C.-L. Tsai, H. Wang, D. M. Nickerson, and B. Li (2009): Subgroup analysis via recursive partitioning, *Journal of Machine Learning Research*, 10, 141–158.

Taddy, M., M. Gardner, L. Chen, and D. Draper (2016): A nonparametric Bayesian analysis of heterogeneous treatment effects in digital experimentation, *Journal of Business and Economic Statistics*, 34(4), 661–672.

Tian, L., A. A. Alizadeh, A. J. Gentles, and R. Tibshirani (2014): A Simple Method for Estimating Interactions between a Treatment and a Large Number of Covariates, *Journal of the American Statistical Association*, 109 (508), 1517–1532.

Wager, S., and S. Athey (2018): Estimation and Inference of Heterogeneous Treatment Effects using Random Forests, forthcoming in the *Journal of the American Statistical Society*.

Wager, S., T. Hastie, and B. Efron (2014): Confidence Intervals for Random Forests: The Jackknife and the Infinitesimal Jackknife, *Journal of Machine Learning Research*, 15, 1625-1651.

Wang, F., and C. Rudin (2015): Causal Falling Rule Lists, *Journal of Machine Learning Research, 38, 1013-1022*.

Wendling, T., K. Jung, A. Callahan, A. Schuler, N. H. Shah, B. Gallego (2018): Comparing methods for estimation of heterogeneous treatment effects using observational data from health care databases, *Statistics in Medicine*, 1–16.

Xie, Y., J. E. Brand, and B. Jann (2012): Estimating Heterogeneous Treatment Effects with Observational Data, *Sociological Methodology*, Volume 42, 314–347, doi: 10.1177/0081175012452652.

Zhao, Q., D. S. Small, and A. Ertefaie (2017): Selective Inference for Effect Modification via the LASSO, *arXi*v: *1705.08020v2*.

Appendix A: More details on estimation and inference

Here, we present some more details of the Causal Forest algorithms used in the EMCS.

The number of randomly chosen variables to form the next split in a tree (*V*) is either chosen from a *min(58,1+Poisson(5))* process or from a *min(58,1+Poisson(38))* process (58 variables in total). The means of the two Poisson processes are considered as the only tuning parameters. They are chosen based on the out-of-bag value of the objective function of the particular estimator. The motivation for using a random number of features is to foster the independence of the trees that appear in the Random Forest.[30]

The minimum leaf size in each tree equals 5. The number of trees contained in any forest equals 1000. Trees are formed on random subsamples drawn without replacement (subsampling) with a sample size of 50% of the size of sample A.

The nonparametric regressions that enter the estimation of the standard errors are based on *k*-NN estimation with number of neighbours equal to *2 sqrt(N)*.

Appendix B: Protocol of EMCS

The Empirical Monte Carlo study follows almost exactly the one used in KLS18. The main differences are in how the effects are generated, as explained in the main body of the text. In this appendix, we repeat their protocol for completeness, with some small adjustments.

1) Take the full sample and estimate the propensity score, $p^f(x)$. We use the specification of Huber, Lechner, and Mellace (2017) which is based on a logit model.

2) Remove all treated from the dataset and keep only the N^{nt} non-treated observations (Y^0).

3) Specify the true ITE. Add them to Y^0 to obtain Y^1 for all observations.

[30] This has also been suggested by Denil, Matheson, and de Freitas (2014) for regression forests.

4) Remove N^v*=5000* observations from the main sample to form the validation sample.

5) Calculate the true GATEs and ATEs by aggregating the true IATEs in the validation sample.

6) Modify $p^f(x)$ such that it equals approximately 50% and compute its value for all N units.

7) Draw random samples of size *N=1'000* or *N=4'000* from the (N^{nt} - N^v) observations.

8) Assign a treatment state based on the outcome of a draw in the Bernoulli distribution with this modified probability as parameter.

9) Depending on the assigned treatment state, use the value Y^0 or Y^1 as observable outcome variable Y.

10) Use these N observations as training sample to compute all effects.

11) Predict all effects in the validation sample.

12) Predict the quality measures in the validation sample for each parameter to estimate.

13) Repeat steps 7 to 12 1'000 (*N=1000*) or 250 (*N=4000*) times.

14) Calculate quality measures by aggregating over all estimated effects.

Appendix C: Further simulation results

C.1 No selectivity – experimental setting

The tables in this appendix follow exactly the structure of those in Section 5 of the main body of the text. The difference to Section 5 is that here we consider only the case without selection bias, i.e. where treatment is fully randomized with a propensity score of 0.5 for all observations.

Table C.1: Simulation results for DGP without selectivity: Basic, OneF.VarT, OneF.MCE, N=1000

			True & estimated effects			**Estimation error of effects (averages)**					**Estimation of std. error**	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt- osis	JB- Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:				**No effect, without selectivity, *N* = 1000**							
ATE	*1*	*Basic*	0.02	-	-	1.27	-0.1	3.0	1.8	1.13	0.04	91
GATE	*2*		0.02	-	-	1.32	-0.1	3.0	1.7	1.15	0.05	91
GATE	*32*		0.02	0	0.01	1.47	-0.1	3.0	2.8	1.21	0.09	92
IATE	*5000*		0.04	0	0.04	3.36	-0.1	3.1	3.1	1.83	0.45	96
ATE	*1*	*OneF.*	0.00	-	-	1.26	0.0	3.1	0.7	1.12	0.04	92
GATE	*2*	*VarT*	0.00	-	-	1.29	0.0	3.1	1.1	1.14	0.07	92
GATE	*32*		0.02	0	0.01	1.34	0.0	3.1	0.9	1.16	0.09	92
IATE	*5000*		0.02	0	0.03	2.36	0.0	3.0	1.9	1.53	0.53	97
ATE	*1*	*OneF.*	-0.04	-	-	1.24	0.0	3.2	2.3	1.11	0.05	92
GATE	*2*	*MCE*	0.04	-	-	1.28	0.0	3.2	2.0	1.13	0.07	92
GATE	*32*		0.04	0	0.01	1.38	0.0	3.2	2.8	1.18	0.13	93
IATE	*5000*		0.04	0	0.03	2.56	0.0	3.1	2.0	1.59	0.60	97
	DGP:				**Normal effect, without selectivity, *N* = 1000**							
ATE	*1*	*Basic*	-0.01	-	-	1.23	-0.2	3.1	7.3	1.11	0.07	92
GATE	*2*		0.03	-	-	1.29	-0.2	3.1	7.1	1.13	0.09	92
GATE	*32*		0.07	0.17	0.12	1.42	-0.2	3.1	5.1	1.19	0.12	93
IATE	*5000*		0.82	1.72	0.83	4.49	-0.1	3.1	5.1	1.89	0.43	92
ATE	*1*	*OneF.*	-0.02	-	-	1.28	0.1	2.9	2.7	1.13	0.05	92
GATE	*2*	*VarT*	0.05	-	-	1.31	0.1	2.9	2.5	1.15	0.07	92
GATE	*32*		0.10	0.17	0.06	1.39	0.1	2.9	2.0	1.17	0.09	92
IATE	*5000*		1.10	1.72	0.51	3.98	0.1	2.9	2.2	1.56	0.54	91
ATE	*1*	*OneF.*	-0.05	-	-	1.41	-01	3.1	1.4	1.19	-0.01	91
GATE	*2*	*MCE*	0.06	-	-	1.45	-0.1	3.1	1.3	1.20	0.01	91
GATE	*32*		0.10	0.17	0.06	1.54	-0.1	3.1	1.1	1.23	0.07	92
IATE	*5000*		1.04	1.72	0.57	4.19	0.0	3.0	1.9	1.67	0.57	92
	DGP:				**Strong effect, without selectivity, *N* = 1000**							
ATE	*1*	*Basic*	0.04	-	-	1.27	0.1	3.1	1.8	1.12	0.15	93
GATE	*2*		0.04	-	-	1.31	0.1	3.1	2.0	1.14	0.17	93
GATE	*32*		0.12	0.67	0.62	1.45	0.1	3.1	3.1	1.19	0.21	94
IATE	*5000*		1.27	6.87	5.93	8.04	0.0	3.1	3.1	2.20	0.41	86
ATE	*1*	*OneF.*	-0.09	-	-	1.30	-0.1	3.3	4.3	1.14	0.14	93
GATE	*2*	*VarT*	0.10	-	-	1.34	-0.1	3.3	4.2	1.15	0.16	94
GATE	*32*		0.23	0.67	0.43	1.46	-0.1	3.2	3.4	1.18	0.19	93
IATE	*5000*		2.23	6.87	4.50	11.25	0.0	3.0	1.8	2.00	0.50	77
ATE	*1*	*OneF.*	-0.02	-	-	1.22	0.0	2.9	0.7	1.11	0.18	95
GATE	*2*	*MCE*	0.09	-	-	1.25	0.0	2.9	0.9	1.12	0.19	95
GATE	*32*		0.21	0.67	0.45	1.39	0.0	2.9	0.8	1.15	0.23	95
IATE	*5000*		2.01	6.87	4.67	10.77	0.0	2.9	2.3	2.08	0.51	79

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.06 for ATE/GATE and 0.06-0.16 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

Table C.2: Simulation results for DGP without selectivity: Basic, OneF.VarT, OneF.MCE, N=4000

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No effect, without selectivity, *N* = 4000**						
ATE	*1*	*Basic*	-0.01	-	-	0.28	0.1	2.8	1.4	0.53	0.05	93
GATE	*2*		0.01	-	-	0.30	0.1	2.8	1.4	0.55	0.07	92
GATE	*32*		0.02	0	0.01	0.47	0.1	2.7	1.9	0.67	0.08	94
IATE	*5000*		0.07	0	0.09	2.28	0.0	3.0	2.1	1.50	0.29	94
ATE	*1*	*OneF.*	-0.02	-	-	0.28	0.2	3.1	2.0	0.58	0.05	94
GATE	*2*	*VarT*	0.02	-	-	0.30	0.2	3.0	1.5	0.62	0.08	94
GATE	*32*		0.02	0	0.01	0.32	0.2	3.1	1.8	0.66	0.10	95
IATE	*5000*		0.05	0	0.05	1.06	0.1	3.0	1.9	1.55	0.53	98
ATE	*1*	*OneF.*	0.07	-	-	0.30	-0.2	2.7	2.0	0.54	0.04	94
GATE	*2*	*MCE*	0.07	-	-	0.32	-0.2	2.8	2.1	0.56	0.06	94
GATE	*32*		0.07	0	0.02	0.40	-0.2	2.9	2.4	0.63	0.13	95
IATE	*5000*		0.08	0	0.06	1.26	0.0	3.0	1.8	1.11	0.57	98
	DGP:					**Normal effect, without selectivity, *N* = 4000**						
ATE	*1*	*Basic*	0.06	-	-	0.29	0.2	3.0	1.9	0.53	0.06	94
GATE	*2*		0.06	-	-	0.31	0.2	3.0	2.0	0.55	0.07	94
GATE	*32*		0.07	0.17	0.15	0.43	0.1	3.0	1.0	0.65	0.10	95
IATE	*5000*		0.44	1.72	1.34	2.81	0.0	3.0	2.2	1.56	0.28	92
ATE	*1*	*OneF.*	-0.1	-	-	0.34	-0.1	2.7	1.5	0.58	0.01	90
GATE	*2*	*VarT*	0.03	-	-	0.36	-0.2	2.7	1.7	0.60	0.03	91
GATE	*32*		0.08	0.17	0.08	0.39	-0.1	2.7	2.6	0.62	0.05	92
IATE	*5000*		0.82	1.72	0.84	2.10	0.0	3.0	1.9	1.10	0.51	93
ATE	*1*	*OneF.*	-0.01	-	-	0.32	0.1	2.9	0.3	0.57	0.02	92
GATE	*2*	*MCE*	0.05	-	-	0.34	0.1	3.0	0.6	0.58	0.03	92
GATE	*32*		0.06	0.17	0.09	0.41	0.1	2.9	0.9	0.63	0.12	94
IATE	*5000*		0.72	1.72	0.93	2.15	0.0	3.0	2.1	1.19	0.54	94
	DGP:					**Strong effect, without selectivity, *N* = 4000**						
ATE	*1*	*Basic*	0.01	-	-	0.29	-0.1	2.6	1.6	0.54	0.10	95
GATE	*2*		0.06	-	-	0.32	-0.1	2.6	1.9	0.56	0.11	97
GATE	*32*		0.16	0.67	0.74	0.48	-0.1	2.7	1.7	0.66	0.13	94
IATE	*5000*		1.10	6.87	6.53	5.24	0.0	3.0	2.2	1.64	0.33	84
ATE	*1*	*OneF.*	0.00	-	-	0.24	-0.1	2.9	0.5	0.50	0.14	98
GATE	*2*	*VarT*	0.05	-	-	0.27	-0.1	2.8	0.6	0.51	0.15	98
GATE	*32*		0.13	0.67	0.56	0.32	0.0	2.8	1.0	0.55	0.18	97
IATE	*5000*		1.23	6.87	5.77	4.62	0.0	2.9	1.9	1.35	0.50	86
ATE	*1*	*OneF.*	0.05	-	-	0.29	-0.1	3.1	0.7	0.54	0.10	94
GATE	*2*	*MCE*	0.05	-	-	0.31	-0.1	3.1	0.8	0.55	0.11	94
GATE	*32*		0.13	0.67	0.56	0.39	-0.1	3.1	2.0	0.60	0.17	95
IATE	*5000*		1.11	6.87	5.93	4.36	0.0	3.0	1.9	1.35	0.60	89

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.03 for ATE/GATE and 0.03-0.0.07 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

Table C.3: Simulation results for DGP without selectivity: OneF.VarT.Penalty, OneF.MCE.Penalty, N=1000

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	X-sectional std. dev. est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:				**No effect, without selectivity, *N* = 1000**							
ATE	*1*	*OneF.*	-0.01	-	-	1.19	-0.1	3.1	4.9	1.09	0.07	93
GATE	*2*	*VarT.*	0.01	-	-	1.23	-0.1	3.3	4.6	1.11	0.09	93
GATE	*32*	*Pen*	0.01	0	0.00	1.28	-0.1	3.3	3.9	1.13	0.12	93
IATE	*5000*		0.03	0	0.03	2.25	0.0	3.0	2.6	1.49	0.56	98
ATE	*1*	*OneF.*	0.03	-	-	1.32	0.0	2.9	0.9	1.15	0.01	91
GATE	*2*	*MCE.*	0.03	-	-	1.35	0.0	2.9	1.0	1.16	0.03	91
GATE	*32*	*Pen*	0.03	0	0.05	1.43	0.0	2.9	0.6	1.20	0.09	93
IATE	*5000*		0.03	0	0.03	2.54	0.0	2.9	1.9	1.59	0.57	97
	DGP:				**Normal effect, without selectivity, *N* = 1000**							
ATE	*1*	*OneF.*	-0.03	-	-	1.24	-0.1	3.1	1.3	1.11	0.07	92
GATE	*2*	*VarT.*	0.05	-	-	1.28	-0.1	3.1	1.3	1.13	0.09	92
GATE	*32*	*Pen*	0.10	0.17	0.05	1.34	-0.1	3.1	1.9	1.15	0.11	93
IATE	*5000*		1.11	1.72	0.51	3.92	0.0	3.0	1.9	1.53	0.55	91
ATE	*1*	*OneF.*	0.02	-	-	1.28	-0.1	2.8	4.2	1.13	0.05	91
GATE	*2*	*MCE.*	0.05	-	-	1.32	-0.1	2.8	3.6	1.15	0.07	91
GATE	*32*	*Pen*	0.10	0.17	0.06	1.41	-0.1	2.9	4.0	1.18	0.12	93
IATE	*5000*		1.06	1.72	0.54	4.03	0.0	3.0	2.2	1.60	0.58	92
	DGP:				**Strong effect, without selectivity, *N* = 1000**							
ATE	*1*	*OneF.*	-0.06	-	-	1.31	-0.1	2.9	1.3	1.14	0.13	93
GATE	*2*	*VarT.*	0.11	-	-	1.35	-0.1	2.9	1.4	1.15	0.15	94
GATE	*32*	*Pen*	0.23	0.67	0.42	1.47	-0.1	2.9	2.5	1.18	0.18	93
IATE	*5000*		2.28	6.87	4.44	11.52	0.0	2.9	1.9	2.00	0.46	76
ATE	*1*	*OneF.*	0.00	-	-	1.33	0.0	2.9	0.8	1.28	0.13	94
GATE	*2*	*MCE.*	0.09	-	-	1.37	0.0	2.9	1.1	1.31	0.14	93
GATE	*32*	*Pen*	0.21	0.67	0.45	1.49	0.0	2.9	0.8	1.37	0.18	94
IATE	*5000*		1.97	6.87	4.47	10.59	0.0	2.9	3.1	2.54	0.46	79

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.06 for ATE/GATE and 0.06-0.16 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

Table C.4: Simulation results for DGP without selectivity: OneF.VarT.Penalty, OneF.MCE.Penalty, N=4000

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:				**No effect, without selectivity, *N* = 4000**							
ATE	*1*	*OneF.*	0.03	-	-	0.32	0.1	2.7	1.4	0.57	0.01	91
GATE	*2*	*VarT.*	0.03	-	-	0.34	0.1	2.7	1.5	0.58	0.04	91
GATE	*32*	*Pen*	0.03	0	0.01	0.37	0.1	2.7	2.0	0.60	0.06	93
IATE	*5000*		0.05	0	0.06	1.12	0.0	2.9	1.8	1.05	0.51	98
ATE	*1*	*OneF.*	-0.02	-	-	0.28	0.1	2.7	0.9	0.53	0.05	93
GATE	*2*	*MCE.*	0.02	-	-	0.30	0.1	2.8	1.0	0.55	0.07	93
GATE	*32*	*Pen*	0.02	0	0.01	0.37	0.1	2.8	0.8	0.61	0.14	96
IATE	*5000*		0.05	0	0.06	1.16	-0.1	3.0	2.9	1.07	0.57	98
	DGP:				**Normal effect, without selectivity, *N* = 4000**							
ATE	*1*	*OneF.*	-0.05	-	-	0.30	0.1	3.1	0.3	0.54	0.04	92
GATE	*2*	*VarT.*	0.06	-	-	0.31	0.1	3.0	0.4	0.56	0.07	93
GATE	*32*	*Pen*	0.10	0.17	0.07	0.36	0.1	3.0	0.7	0.58	0.09	94
IATE	*5000*		0.84	1.72	0.83	2.07	0.0	3.0	1.9	1.07	0.53	93
ATE	*1*	*OneF.*	0.03	-	-	0.32	0.0	2.8	0.4	0.57	0.02	90
GATE	*2*	*MCE.*	0.04	-	-	0.34	0.0	2.9	0.6	0.58	0.04	91
GATE	*32*	*Pen*	0.08	0.17	0.08	0.41	-0.1	2.9	0.9	0.63	0.11	93
IATE	*5000*		0.78	1.72	0.86	2.17	0.0	3.0	2.0	1.15	0.52	93
	DGP:				**Strong effect, without selectivity, *N* = 4000**							
ATE	*1*	*OneF.*	0.03	-	-	0.30	-0.2	2.6	2.9	0.54	0.09	96
GATE	*2*	*VarT.*	0.06	-	-	0.32	-0.2	2.6	3.1	0.56	0.11	95
GATE	*32*	*Pen*	0.13	0.17	0.55	0.38	-0.2	2.7	2.9	0.59	0.14	95
IATE	*5000*		0.03	1.72	5.80	4.56	-0.1	2.9	2.1	1.33	0.50	86
ATE	*1*	*OneF.*	0.06	-	-	0.29	0.0	2.8	0.3	0.53	0.10	95
GATE	*2*	*MCE.*	0.06	-	-	0.30	0.0	2.8	0.4	0.54	0.12	95
GATE	*32*	*Pen*	0.13	0.67	0.57	0.36	0.0	2.7	1.3	0.58	0.18	96
IATE	*5000*		1.14	6.87	5.89	4.44	0.0	3.0	1.7	1.34	0.55	87

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. The simulation errors of the mean MSEs are 0.03 for ATE/GATE and 0.03-0.0.07 for IATE. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

C.2 Alternative specification of IATE

Here, we consider a 'normal' strength IATE with selectivity but with a different specification of the functions $\xi(x)$. Instead of its dependency on a non-linear function of *p(x)*, it now depends linearly on the insured earnings of the unemployed. The latter is an officially defined pre-unemployment earnings measure used to compute unemployment benefits. Although this variable is related to the selection into programmes as well, the link is much weaker.

Table C.5: Simulation results for IATE linear in insured earnings with selectivity

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*Est.*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias in %	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**With selectivity, *N* = 1000**						
ATE	*1*	*Basic*	1.20	-	-	2.87	0.0	3.1	0.3	1.21	0.01	74
GATE	*2*		1.23	-	-	3.10	0.0	3.1	0.3	1.25	0.02	74
GATE	*32*		1.14	0.35	0.20	2.98	0.0	3.1	0.6	1.28	0.72	79
IATE	*5000*		1.28	1.71	1.39	6.38	0.0	3.1	3.6	2.00	0.34	87
ATE	*1*	*OneF.*	1.18	-	-	2.83	-0.1	2.8	3.8	1.20	0.06	75
GATE	*2*	*VarT*	1.25	-	-	3.24	-0.1	2.8	3.7	1.21	0.08	73
GATE	*32*		1.11	0.35	0.08	2.81	-0.1	2.8	3.5	1.22	0.10	78
IATE	*5000*		1.53	1.71	0.67	5.88	-0.1	3.0	2.4	1.58	0.50	83
ATE	*1*	*OneF.*	1.18	-	-	2.77	0.1	2.9	0.9	1.17	0.04	76
GATE	*2*	*MCE*	1.24	-	-	3.09	0.0	2.9	1.1	1.19	0.07	74
GATE	*32*		1.13	0.35	0.13	2.83	0.1	3.0	1.5	1.22	0.11	80
IATE	*5000*		1.41	1.71	0.80	5.48	0.0	3.1	2.8	1.65	0.54	87
ATE	*1*	*OneF.*	0.97	-	-	2.47	-0.1	2.7	5.9	1.23	0.14	83
GATE	*2*	*VarT.*	1.04	-	-	2.88	-0.1	2.7	5.6	1.25	0.15	81
GATE	*32*	*Pen*	0.90	0.35	0.09	2.47	-0.1	2.7	5.9	1.26	0.16	85
IATE	*5000*		1.47	1.71	0.78	6.17	-0.1	3.1	7.5	1.71	0.58	85
ATE	*1*	*OneF.*	0.56	-	-	2.09	-0.1	3.0	1.9	1.33	0.23	92
GATE	*2*	*MCE.*	0.64	-	-	2.44	-0.1	3.0	1.8	1.34	0.24	89
GATE	*32*	*Pen*	0.50	0.35	0.11	2.15	-0.1	2.9	1.8	1.35	0.25	92
IATE	*5000*		1.37	1.71	1.05	6.70	-0.1	3.0	4.1	1.94	0.67	89
	DGP:					**With selectivity, *N* = 4000**						
ATE	*1*	*Basic*	0.94	-	-	1.20	-0.3	3.2	4.8	0.56	0.07	54
GATE	*2*		0.96	-	-	1.27	-0.3	3.1	3.8	0.59	0.08	58
GATE	*32*		0.91	0.35	0.28	1.30	-0.2	3.1	4.0	0.67	0.12	69
IATE	*5000*		1.09	1.71	1.82	4.36	-0.1	3.0	2.2	1.61	0.23	85
ATE	*1*	*OneF.*	1.06	-	-	1.55	0.3	3.0	5.2	0.61	0.08	56
GATE	*2*	*VarT*	1.14	-	-	1.86	0.3	3.0	3.8	0.63	0.09	54
GATE	*32*		1.02	0.35	0.13	1.53	0.3	3.0	3.5	0.65	0.11	64
IATE	*5000*		1.40	1.71	0.93	4.24	0.0	3.3	11.2	1.15	0.48	79
ATE	*1*	*OneF.*	0.87	-	-	1.03	-0.2	2.9	1.2	0.53	0.10	62
GATE	*2*	*MCE*	0.90	-	-	1.17	-0.2	2.9	1.6	0.55	0.12	65
GATE	*32*		0.83	0.35	0.20	1.08	-0.1	2.8	1.5	0.60	0.17	74
IATE	*5000*		1.03	1.71	1.22	2.78	0.0	3.0	2.0	1.13	0.54	90
ATE	*1*	*OneF.*	0.75	-	-	1.00	-0.3	2.9	4.1	0.82	0.16	79
GATE	*2*	*VarT.*	0.81	-	-	1.32	-0.3	3.0	4.5	0.84	0.16	74
GATE	*32*	*Pen*	0.68	0.35	0.12	1.01	-0.3	2.9	3.5	0.86	0.17	83
IATE	*5000*		1.40	1.71	1.04	4.63	0.0	3.0	1.6	1.93	0.65	83
ATE	*1*	*OneF.*	0.21	-	-	0.75	-0.3	2.9	3.3	0.84	0.13	93
GATE	*2*	*MCE.*	0.41	-	-	0.96	-0.3	2.9	3.1	0.85	0.14	89
GATE	*32*	*Pen*	0.21	0.35	0.15	0.83	-0.3	2.9	3.3	0.87	0.16	93
IATE	*5000*		1.10	1.71	1.05	4.33	-0.1	3.0	2.2	1.54	0.56	89

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. *OneF.VarT.Penalty:* Baseline penalty multiplied by 100.

C.3 Sensitivity of estimators to different values of the penalty term

This section contains an investigation of the sensitivity of the estimators with respect to different values of the penalty term. For computational reasons, all results are based on 200 (*N=1'000*) or 50 (*N=4'000*) replications only. The trade-off between bias and variance when changing the penalty term is obvious as long as the penalty does not become too large.

C.3.1 OneF.MCE.Penalty

Table C.6: Sensitivity checks for DGP with selectivity, normal random IATE's, N=1000: Different values of penalty term for OneF.MCE.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias in %
Estimator:			***OneF.MCE* (penalty = 0)**									
ATE	*1*	*1000*	1.97	-	-	5.28	-0.1	3.1	1.6	1.18	0.04	3
GATE	*2*		1.97	-	-	5.31	-0.1	3.1	2.2	1.19	0.06	5
GATE	*32*		1.94	0.16	0.06	5.32	-0.1	3.0	2.0	1.24	0.11	8
IATE	*5000*		1.99	1.72	0.55	8.43	0.0	3.0	2.0	1.63	0.56	34
Estimator:			***OneF.MCE.Penalty* (penalty = 0.1)**									
ATE	*1*	*1000*	1.67	-	-	4.11	0.1	3.1	0.3	1.14	0.14	12
GATE	*2*		1.67	-	-	4.13	0.1	3.1	0.7	1.16	0.15	13
GATE	*32*		1.64	0.17	0.06	4.16	0.1	3.0	0.3	1.20	0.18	15
IATE	*5000*		1.69	1.72	0.70	6.70	0.0	3.1	2.4	1.60	0.60	37
Estimator:			***OneF.MCE.Penalty* (penalty = 0.5)**									
ATE	*1*	*1000*	1.27	-	-	3.38	-0.1	3.5	2.2	1.46	0.06	4
GATE	*2*		1.23	-	-	3.40	-0.1	3.5	2.6	1.46	0.07	5
GATE	*32*		1.11	0.17	0.12	3.38	-0.1	3.5	3.0	1.47	0.10	7
IATE	*5000*		1.14	1.72	1.46	5.74	-0.2	3.2	3.2	2.01	0.53	27
Estimator:			***OneF.MCE.Penalty* (penalty = 1)**									
ATE	*1*	*1000*	1.11	-	-	3.35	-0.1	2.9	2.6	1.46	0.10	7
GATE	*2*		1.11	-	-	3.36	-0.1	3.1	3.1	1.46	0.11	7
GATE	*32*		1.10	0.17	0.13	3.37	-0.1	2.9	2.7	1.47	0.13	9
IATE	*5000*		1.12	1.72	1.50	5.87	-0.2	3.1	2.0	2.05	0.56	27
Estimator:			***OneF.MCE.Penalty* (penalty = 2)**									
ATE	*1*	*1000*	1.14	-	-	3.20	-0.2	2.6	3.1	1.38	0.23	17
GATE	*2*		1.14	-	-	3.21	-0.2	2.6	3.1	1.39	0.24	17
GATE	*32*		1.13	0.17	0.15	3.24	-0.2	2.7	2.9	1.40	0.25	18
IATE	*5000*		1.15	1.72	1.60	5.81	-0.2	3.1	2.0	2.04	0.69	34
Estimator:			***OneF.MCE.Penalty* (penalty = 10)**									
ATE	*1*	*1000*	0.98	-	-	3.02	-0.2	3.2	1.6	1.44	0.20	14
GATE	*2*		0.98	-	-	3.04	-0.2	3.2	1.5	1.45	0.21	14
GATE	*32*		0.97	0.17	0.14	3.05	-0.2	3.2	1.5	1.47	0.22	15
IATE	*5000*		0.99	1.72	1.59	5.89	-0.1	3.2	4.3	2.14	0.68	32

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). Note that due to the lower number of simulations (200), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *penalty* denotes a multiplier of *Var(y)*.

Table C.7: Sensitivity checks for DGP with selectivity, normal random IATE's, N=4'000: Different values of penalty term for OneF.MCE.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias in %
Estimator:			***OneF.MCE*** **(penalty = 0)**									
ATE	*1*	*4000*	1.46	-	-	2.50	-0.6	3.7	20.1	0.61	0.04	7
GATE	*2*		1.45	-	-	2.50	-0.6	3.6	17.4	0.63	0.06	9
GATE	*32*		1.43	0.17	0.07	2.53	-0.5	3.4	11.9	0.67	0.12	17
IATE	*5000*		1.50	1.72	0.79	4.87	-0.1	3.0	3.2	1.16	0.50	43
Estimator:			***OneF.MCE.Penalty*** **(penalty = 0.1)**									
ATE	*1*	*4000*	0.82	-	-	1.17	-0.1	1.9	2.3	0.71	0.11	14
GATE	*2*		0.82	-	-	1.18	-0.1	1.9	2.4	0.72	0.11	16
GATE	*32*		0.82	0.17	0.14	1.23	-0.1	2.0	2.1	0.76	0.14	18
IATE	*5000*		0.83	1.72	1.45	2.50	-0.1	2.6	1.6	1.26	0.53	42
Estimator:			***OneF.MCE.Penalty*** **(penalty = 0.5)**									
ATE	*1*	*4000*	0.57	-	-	0.79	0.7	4.7	10.0	0.68	0.24	35
GATE	*2*		0.57	-	-	0.79	0.6	4.5	9.0	0.69	0.24	35
GATE	*32*		0.57	0.17	0.15	0.81	0.6	4.3	7.6	0.70	0.28	41
IATE	*5000*		0.61	1.72	1.69	2.36	0.0	3.0	2.1	1.32	0.65	49
Estimator:			***OneF.MCE.Penalty*** **(penalty = 1)**									
ATE	*1*	*4000*	0.39	-	-	0.89	-0.1	2.9	0.9	0.86	0.10	12
GATE	*2*		0.39	-	-	0.90	-0.1	2.9	0.8	0.87	0.11	12
GATE	*32*		0.38	0.17	0.16	0.93	-0.1	2.8	1.1	0.89	0.14	15
IATE	*5000*		0.51	1.72	1.89	2.83	-0.1	3.0	2.3	1.53	0.53	35
Estimator:			***OneF.MCE.Penalty*** **(penalty = 2)**									
ATE	*1*	*4000*	0.55	-	-	1.03	0.5	2.9	2.4	0.85	0.16	19
GATE	*2*		0.55	-	-	1.01	0.5	3.0	2.2	0.85	0.16	19
GATE	*32*		0.55	0.17	0.16	1.03	0.4	2.9	1.9	0.86	0.19	22
IATE	*5000*		0.58	1.72	1.72	2.82	0.1	2.7	1.3	1.53	0.63	41
Estimator:			***OneF.MCE.Penalty*** **(penalty = 10)**									
ATE	*1*	*4000*	0.56	-	-	0.92	-0.1	2.8	0.1	0.78	0.25	31
GATE	*2*		0.56	-	-	0.92	-0.1	2.8	0.2	0.79	0.25	31
GATE	*32*		0.56	0.17	0.18	0.95	-0.1	2.7	0.7	0.80	0.26	33
IATE	*5000*		0.66	1.72	2.01	3.23	-0.1	2.8	2.3	1.61	0.67	42

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). Note that due to the lower number of simulations (50), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *Penalty* denotes a multiplier of *Var(y)*.

Table C.8: Sensitivity checks for DGP without selectivity, normal random IATE's, N=1'000: Different values of penalty term for OneF.MCE.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias in %
Estimator:			***OneF.MCE* (penalty = 0)**									
ATE	*1*	*1000*	-0.05	-	-	1.41	-01	3.1	1.4	1.19	-0.01	-1
GATE	*2*		0.06	-	-	1.45	-0.1	3.1	1.3	1.20	0.01	1
GATE	*32*		0.10	0.17	0.06	1.54	-0.1	3.1	1.1	1.23	0.07	6
IATE	*5000*		1.04	1.72	0.57	4.19	0.0	3.0	1.9	1.67	0.57	34
Estimator:			***OneF.MCE.Penalty* (penalty = 0.1)**									
ATE	*1*	*1000*	-0.01	-	-	1.19	-0.1	2.8	0.4	1.09	0.09	8
GATE	*2*		0.06	-	-	1.24	-0.1	2.9	0.5	1.12	0.10	9
GATE	*32*		0.08	0.17	0.08	1.33	-0.1	2.9	1.6	1.15	0.16	14
IATE	*5000*		1.02	1.72	0.60	3.98	0.0	3.0	3.0	1.62	0.62	38
Estimator:			***OneF.MCE.Penalty* (penalty = 0.5)**									
ATE	*1*	*1000*	-0.07	-	-	1.27	-0.2	3.4	2.5	1.13	0.05	5
GATE	*2*		0.07	-	-	1.31	-0.2	3.4	2.7	1.14	0.07	6
GATE	*32*		0.11	0.17	0.05	1.42	-0.1	3.3	1.6	1.19	0.12	10
IATE	*5000*		1.05	1.72	0.57	4.07	0.0	3.0	1.8	1.63	0.62	37
Estimator:			***OneF.MCE.Penalty* (penalty = 1)**									
ATE	*1*	*1000*	0.02	-	-	1.28	-0.1	2.8	4.2	1.13	0.05	4
GATE	*2*		0.05	-	-	1.32	-0.1	2.8	3.6	1.15	0.07	6
GATE	*32*		0.10	0.17	0.06	1.41	-0.1	2.9	4.0	1.18	0.12	10
IATE	*5000*		1.06	1.72	0.54	4.03	0.0	3.0	2.2	1.60	0.58	37
Estimator:			***OneF.MCE.Penalty* (penalty = 2)**									
ATE	*1*	*1000*	0.09	-	-	1.26	-0.3	2.9	2.5	1.12	0.06	5
GATE	*2*		0.10	-	-	1.29	-0.3	2.9	2.4	1.13	0.08	7
GATE	*32*		0.11	0.17	0.07	1.37	-0.3	2.9	2.3	1.17	0.12	11
IATE	*5000*		1.11	1.72	0.51	3.98	-0.1	2.9	1.5	1.49	0.57	37
Estimator:			***OneF.MCE.Penalty* (penalty = 10)**									
ATE	*1*	*1000*	0.10	-	-	1.49	0.0	3.2	0.3	1.22	-0.04	-3
GATE	*2*		0.09	-	-	1.52	0.0	3.2	0.3	1.23	-0.03	-2
GATE	*32*		0.12	0.17	0.05	1.59	0.0	3.1	0.3	1.26	0.01	1
IATE	*5000*		1.14	1.72	0.45	4.30	0.0	3.1	1.6	1.61	0.45	27

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight).
Note that due to the lower number of simulations (50), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *Penalty* denotes a multiplier of *Var(y)*.

Table C.9: Sensitivity checks for DGP without selectivity, normal random IATE's, N=4'000: Different values of penalty term for OneF.MCE.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias in %
Estimator:			***OneF.MCE*** **(penalty = 0)**									
ATE	*1*	*4000*	-0.01	-	-	0.32	0.1	2.9	0.3	0.57	0.02	3
GATE	*2*		0.05	-	-	0.34	0.1	3.0	0.6	0.58	0.03	7
GATE	*32*		0.06	0.17	0.09	0.41	0.1	2.9	0.9	0.63	0.12	19
IATE	*5000*		0.72	1.72	0.93	2.15	0.0	3.0	2.07	1.19	0.54	45
Estimator:			***OneF.MCE.Penalty*** **(penalty = 0.1)**									
ATE	*1*	*4000*	-0.09	-	-	0.25	-0.2	2.7	0.6	0.49	0.10	20
GATE	*2*		0.09	-	-	0.27	-0.2	2.8	0.6	0.51	0.11	22
GATE	*32*		0.10	0.17	0.11	0.33	0.0	3.0	0.4	0.56	0.19	33
IATE	*5000*		0.77	1.72	0.92	2.02	0.1	2.8	1.6	1.11	0.61	55
Estimator:			***OneF.MCE.Penalty*** **(penalty = 0.5)**									
ATE	*1*	*4000*	0.01	-	-	0.31	0.1	2.2	1.5	0.56	0.03	5
GATE	*2*		0.06	-	-	0.32	0.1	2.2	1.5	0.57	0.05	9
GATE	*32*		0.07	0.17	0.09	0.39	0.0	2.3	1.5	0.63	0.11	18
IATE	*5000*		0.78	1.72	0.89	2.12	0.0	2.8	1.5	1.14	0.56	49
Estimator:			***OneF.MCE.Penalty*** **(penalty = 1)**									
ATE	*1*	*4000*	0.03	-	-	0.32	0.0	2.8	0.4	0.57	0.02	3
GATE	*2*		0.04	-	-	0.34	0.0	2.9	0.6	0.58	0.04	7
GATE	*32*		0.08	0.17	0.08	0.41	-0.1	2.9	0.9	0.63	0.11	17
IATE	*5000*		0.78	1.72	0.86	2.17	0.0	3.0	2.0	1.15	0.52	45
Estimator:			***OneF.MCE.Penalty*** **(penalty = 2)**									
ATE	*1*	*4000*	0.06	-	-	0.28	0.1	2.3	0.9	0.53	0.05	10
GATE	*2*		0.06	-	-	0.29	0.1	2.4	0.8	0.54	0.07	13
GATE	*32*		0.09	0.17	0.09	0.35	0.0	2.5	0.6	0.59	0.14	23
IATE	*5000*		0.90	1.72	0.73	2.29	0.0	2.9	1.1	1.72	0.59	54
Estimator:			***OneF.MCE.Penalty*** **(penalty = 10)**									
ATE	*1*	*4000*	0.09	-	-	0.35	-0.2	2.8	0.6	0.59	0.00	0
GATE	*2*		0.08	-	-	0.36	-0.2	2.8	0.7	0.60	0.02	3
GATE	*32*		0.11	0.17	0.07	0.43	-0.2	2.8	1.0	0.64	0.04	6
IATE	*5000*		1.00	1.72	0.65	2.57	0.0	2.7	1.5	1.12	0.55	49

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). Note that due to the lower number of simulations (50), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *Penalty* denotes a multiplier of *Var(y)*.

C.3.2 OneF.VarT.Penalty

Table C.10: Sensitivity checks for DGP with selectivity, normal random IATE's, N=1000: Different values of penalty term for OneF.VarT.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups	*N*	Avg. bias	X-sectional std. dev.		MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias
	#			true	est.							in %
Estimator:			***OneF.VarT*** **(penalty = 0)**									
ATE	*1*	*1000*	2.09	-	-	5.64	-0.1	3.0	0.7	1.14	0.11	10
GATE	*2*		2.09	-	-	5.67	-0.1	3.0	0.7	1.15	0.12	11
GATE	*32*		2.06	0.16	0.07	5.60	-0.1	3.0	0.8	1.16	0.15	13
IATE	*5000*		2.09	1.72	0.75	8.01	0.0	3.0	3.0	1.55	0.54	35
Estimator:			***OneF.VarT.Penalty*** **(penalty = 1)**									
ATE	*1*	*1000*	1.77	-	-	4.67	0.1	2.6	1.3	1.23	0.02	2
GATE	*2*		1.77	-	-	4.69	0.1	2.6	1.4	1.25	0.04	3
GATE	*32*		1.75	0.16	0.06	4.68	0.1	2.6	1.3	1.27	0.05	4
IATE	*5000*		1.78	1.72	0.77	7.04	0.0	2.8	1.5	1.64	0.47	29
Estimator:			***OneF.VarT.Penalty*** **(penalty = 10)**									
ATE	*1*	*1000*	1.65	-	-	4.31	-0.2	2.8	1.3	1.26	0.09	7
GATE	*2*		1.65	-	-	4.31	-0.2	2.8	1.3	1.26	0.10	8
GATE	*32*		1.63	0.16	0.09	4.29	-0.2	2.8	1.3	1.28	0.12	10
IATE	*5000*		1.66	1.72	1.14	6.52	-0.1	3.3	5.6	1.75	0.49	28
Estimator:			***OneF.VarT.Penalty*** **(penalty = 100)**									
ATE	*1*	*1000*	1.56	-	-	4.27	-0.4	3.3	7.1	1.36	0.06	5
GATE	*2*		1.55	-	-	4.28	-0.4	3.3	6.6	1.37	0.07	5
GATE	*32*		1.54	0.16	0.10	4.25	-0.4	3.2	6.7	1.37	0.09	7
IATE	*5000*		1.56	1.72	1.22	6.52	-0.4	3.5	13.4	1.86	0.54	29
Estimator:			***OneF.VarT.Penalty*** **(penalty = 1'000)**									
ATE	*1*	*1000*	1.25	-	-	3.33	0.0	2.7	1.0	1.33	0.16	12
GATE	*2*		1.25	-	-	3.34	0.0	2.7	1.0	1.34	0.17	13
GATE	*32*		1.24	0.16	0.11	3.34	0.0	2.6	1.1	1.35	0.18	13
IATE	*5000*		1.27	1.72	1.26	5.86	-0.2	3.3	4.0	1.92	0.60	32
Estimator:			***OneF.VarT.Penalty*** **(penalty = 10'000)**									
ATE	*1*	*1000*	1.34	-	-	3.50	-0.1	2.9	0.3	1.31	0.17	13
GATE	*2*		1.33	-	-	3.51	-0.1	2.9	0.5	1.32	0.18	14
GATE	*32*		1.32	0.16	0.11	3.51	-0.1	2.9	0.4	1.33	0.19	15
IATE	*5000*		1.35	1.72	1.30	5.94	-0.2	3.2	4.2	1.91	0.62	33
Estimator:			***OneF.VarT.Penalty*** **(penalty = 100'000)**									
ATE	*1*	*1000*	1.42	-	-	3.87	-0.3	3.7	14.9	1.36	0.13	9
GATE	*2*		1.42	-	-	3.89	-0.3	3.6	14.4	1.37	0.13	10
GATE	*32*		1.41	0.16	0.12	3.88	-0.3	3.7	15.5	1.38	0.14	10
IATE	*5000*		1.44	1.72	1.37	6.23	-0.3	3.5	18.9	1.94	0.60	30

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight).
Note that due to the lower number of simulations (200), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *Penalty* denotes a multiplier of *Var(y)*.

Table C.11: Sensitivity checks for DGP with selectivity, normal random IATE's, N=4'000: Different values of penalty term for OneF.VarT.Penalty

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	Avg. bias in %
Estimator:			***OneF.VarT*** **(penalty = 0)**									
ATE	*1*	*4000*	1.78	-	-	3.57	-0.2	3.0	1.0	0.63	0.05	7
GATE	*2*		1.78	-	-	3.58	-0.1	3.0	1.4	0.65	0.06	10
GATE	*32*		1.75	0.17	0.09	3.52	-0.1	3.0	1.0	0.66	0.08	13
IATE	*5000*		1.79	1.72	1.13	5.31	-0.1	3.1	5.7	1.14	0.50	43
Estimator:			***OneF.VarT.Penalty*** **(penalty = 1)**									
ATE	*1*	*4000*	1.61	-	-	2.99	0.0	3.2	0.4	0.64	0.06	9
GATE	*2*		1.61	-	-	3.01	0.0	3.1	0.6	0.65	0.08	12
GATE	*32*		1.58	0.17	0.10	2.97	0.1	3.1	1.1	0.67	0.09	13
IATE	*5000*		1.63	1.72	1.28	4.74	0.0	2.9	1.8	1.18	0.52	44
Estimator:			***OneF.VarT.Penalty*** **(penalty = 10)**									
ATE	*1*	*4000*	1.24	-	-	1.93	0.0	2.6	0.4	0.61	0.18	30
GATE	*2*		1.25	-	-	1.95	0.0	2.6	0.5	0.63	0.19	31
GATE	*32*		1.23	0.16	0.14	1.94	0.0	2.5	0.6	0.65	0.20	31
IATE	*5000*		1.28	1.72	1.69	3.84	0.0	2.7	1.5	1.29	0.59	46
Estimator:			***OneF.VarT.Penalty*** **(penalty = 100)**									
ATE	*1*	*4000*	1.24	-	-	2.05	0.0	2.4	0.7	0.71	0.10	14
GATE	*2*		1.24	-	-	2.05	0.0	2.4	0.7	0.73	0.11	15
GATE	*32*		1.23	0.16	0.13	2.05	-0.1	2.5	0.7	0.74	0.12	16
IATE	*5000*		1.27	1.72	1.51	3.75	0.0	2.7	1.3	1.31	0.65	50
Estimator:			***OneF.VarT.Penalty*** **(penalty = 1'000)**									
ATE	*1*	*4000*	1.26	-	-	2.02	0.3	1.9	0.7	0.66	0.17	25
GATE	*2*		1.26	-	-	2.04	0.3	2.0	0.7	0.67	0.17	26
GATE	*32*		1.25	0.16	0.14	2.00	0.3	2.0	0.7	0.66	0.20	30
IATE	*5000*		1.30	1.72	1.67	3.85	0.0	2.8	1.3	1.32	0.67	50
Estimator:			***OneF.VarT.Penalty*** **(penalty = 10'000)**									
ATE	*1*	*4000*	1.17	-	-	1.77	0.4	3.6	2.0	0.63	0.19	30
GATE	*2*		1.17	-	-	1.78	0.4	3.5	1.9	0.64	0.20	31
GATE	*32*		1.17	0.16	0.15	1.78	0.3	3.5	1.6	0.65	0.21	33
IATE	*5000*		1.21	1.72	1.61	3.55	0.0	2.7	1.3	1.29	0.68	52

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight).
Note that due to the lower number of simulations (50), the comparison of estimated and true standard errors and coverage probabilities may be less reliable. *Penalty* denotes a multiplier of *Var(y)*.

C.4 One-sample *basic* estimator with honesty

This section contains the results of the basic estimator in a configuration that might be considered standard: It uses the full sample for training, but when building each tree, the respective subsample is splitted and 50% is used for building the tree, and the other 50% is used for computing the effects. This procedure has been termed 'honesty' in the literature (e.g. Wager and Athey, 2018).

Table C.12: Simulation results for DGP without selectivity (Basic, 1 sample with honesty)

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No selectivity and no CATE**						
ATE	*1*	*1000*	0.01	-	-	0.64	0.11	2.9	2.5	0.80	0.02	91
GATE	*2*		0.01	-	-	0.68	0.11	3.0	2.5	0.82	0.02	91
GATE	*32*		0.01	0	0.01	0.97	0.09	3.2	4.8	0.94	0.00	90
IATE	*5000*		0.04	0	0.04	3.21	0.01	3.1	3.1	1.78	-0.10	87
ATE	*1*	*4000*	-0.03	-	-	0.12	0.0	2.41	3.6	0.35	0.06	96
GATE	*2*		0.03	-	-	0.15	0.0	2.52	2.7	0.38	0.05	94
GATE	*32*		0.03	0	0.02	0.32	0.1	2.96	2.9	0.55	0.01	91
IATE	*5000*		0.08	0	0.09	2.18	0.0	3.01	2.1	1.46	-0.14	85
	DGP:					**No selectivity and normal CATE**						
ATE	*1*	*1000*	0.01	-	-	0.60	0.0	3.0	0.1	0.77	0.06	92
GATE	*2*		0.01	-	-	0.65	0.0	2.9	0.3	0.81	0.05	92
GATE	*32*		0.03	0.17	0.13	0.85	0.0	3.0	1.0	0.92	0.04	91
IATE	*5000*		0.01	1.72	0.97	4.15	0.0	3.0	2.0	1.85	-0.13	83
ATE	*1*	*4000*	0.00	-	-	0.16	0.0	3.8	7.5	0.40	0.02	92
GATE	*2*		0.00	-	-	0.19	0.0	3.9	9.7	0.44	0.01	91
GATE	*32*		0.11	0.17	0.24	0.34	0.1	3.4	3.1	0.56	-0.01	90
IATE	*5000*		0.45	1.72	1.47	2.76	0.0	3.0	3.0	1.53	-0.18	81

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. DGP with strong selectivity omitted to save computational costs.

Table C.13: Simulation results for DGP with selectivity (Basic, 1 sample with honesty)

			True & estimated effects			**Estimation error of effects (averages)**					**Estimation of std. error**	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**With selectivity and no CATE**						
ATE	*1*	*1000*	1.01	-	-	1.68	0.0	3.2	0.7	0.81	0.86	69
GATE	*2*		1.02	-	-	1.75	0.0	3.2	1.6	0.84	0.88	69
GATE	*32*		0.97	0	0.21	1.89	0.0	3.1	0.9	0.96	0.97	74
IATE	*5000*		1.06	0	0.93	4.71	0.0	3.0	2.5	1.79	1.68	79
ATE	*1*	*4000*	0.85	-	-	0.89	0.3	3.0	4.0	0.41	0.04	39
GATE	*2*		0.86	-	-	0.94	0.3	3.1	3.6	0.44	0.03	43
GATE	*32*		0.79	0	0.21	1.04	0.1	3.1	2.3	0.61	-0.02	59
IATE	*5000*		1.05	0	0.93	3.89	0.0	3.0	2.4	1.51	-0.18	73
	DGP:					**With selectivity and normal CATE**						
ATE	*1*	*1000*	1.78	-	-	3.87	0.0	2.8	1.2	0.84	0.02	33
GATE	*2*		1.78	-	-	3.92	0.0	2.9	0.5	0.87	0.02	37
GATE	*32*		1.74	0.17	0.11	3.93	0.0	3.0	1.3	0.97	0.01	44
IATE	*5000*		1.80	1.72	1.08	8.00	0.0	3.1	3.2	3.27	-0.13	67
ATE	*1*	*4000*	1.45	-	-	2.30	0.2	2.8	1.3	0.43	0.03	15
GATE	*2*		1.46	-	-	2.34	0.1	2.9	1.1	0.45	0.03	19
GATE	*32*		1.42	0.17	0.13	2.41	0.1	2.9	1.7	0.60	-0.02	35
IATE	*5000*		1.56	1.72	1.38	6.06	0.0	3.0	2.3	1.58	-0.23	76
	DGP:					**With selectivity and strong CATE**						
ATE	*1*	*1000*	2.94	-	-	9.49	0.1	3.2	2.5	0.93	-0.03	33
GATE	*2*		2.94	-	-	9.54	0.1	3.1	1.6	0.95	-0.03	37
GATE	*32*		2.90	0.67	0.45	9.59	0.1	3.2	6.4	1.03	-0.03	44
IATE	*5000*		3.03	6.86	4.24	24.10	0.0	3.0	2.6	2.05	-0.18	67
ATE	*1*	*4000*	2.10	-	-	4.65	0.0	3.1	0.1	0.49	-0.01	1
GATE	*2*		2.11	-	-	4.72	0.0	3.1	0.6	0.51	-0.01	1
GATE	*32*		2.10	0.67	0.56	4.81	0.0	3.0	1.5	0.61	-0.01	4
IATE	*5000*		2.31	6.86	5.27	13.74	0.0	3.0	2.1	1.62	-0.18	57

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval.

C.5 One forest estimator without MCE correction (OneF)

The computation of *OneF* is identical to the one of the best performing *OneF.MCE.Penalty* when setting MCE as well as the penalty to zero. Therefore, it has some small computational advantages compared to *OneF.MCE* and *OneF.MCE.Penalty*.

Table C.14: Simulation results for DGP without selectivity (OneF)

			True & estimated effects			Estimation error of effects (averages)					Estimation of std. error	
	Groups #	*N*	Avg. bias	X-sectional std. dev. true	est.	MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Avg. bias	CovP (90) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**No selectivity and no CATE**						
ATE	*1*	*1000*	0.06	-	-	1.24	-0.2	2.9	5.1	1.11	0.06	92
GATE	*2*		0.06	-	-	1.28	-0.2	2.9	4.7	1.13	0.08	93
GATE	*32*		0.05	0	0.01	1.40	-0.2	2.9	4.8	1.18	0.16	94
IATE	*5000*		0.06	0	0.04	2.79	-0.1	3.0	2.4	1.67	0.68	98
ATE	*1*	*4000*	-0.04	-	-	0.33	0.1	2.8	0.6	0.57	0.01	90
GATE	*2*		0.04	-	-	0.034	0.1	2.9	1.7	0.58	0.03	92
GATE	*32*		0.04	0	0.01	0.44	0.0	2.9	1.1	0.66	0.14	95
IATE	*5000*		0.08	0	0.09	1.60	0.0	3.1	2.2	1.26	0.61	98
	DGP:					**No selectivity and normal CATE**						
ATE	*1*	*1000*	0.04	-	-	1.31	-0.1	2.8	2.5	1.14	0.04	91
GATE	*2*		0.05	-	-	1.35	-0.1	2.8	2.2	1.16	0.06	92
GATE	*32*		0.09	0.17	0.06	1.45	-0.1	2.9	1.4	1.20	0.14	93
IATE	*5000*		0.10	1.72	0.62	4.33	0.0	3.0	1.9	1.73	0.66	93
ATE	*1*	*4000*	0.01	-		0.32	-0.2	2.5	3.4	0.57	0.02	91
GATE	*2*		0.05	-		0.34	-0.1	2.6	2.7	0.58	0.04	92
GATE	*32*		0.06	0.17	0.10	0.42	-0.2	2.6	3.1	0.64	0.13	96
IATE	*5000*		0.72	1.72	0.97	2.35	0.0	2.9	1.9	1.26	0.64	95

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval. DGP with strong selectivity omitted to save computational costs.

Table C.15: Simulation results for DGP with selectivity (OneF)

	Groups #	N	True & estimated effects Avg. bias	X-sectional std. dev. true	est.	Estimation error of effects (averages) MSE	Skew ness	Kurt-osis	JB-Stat.	Std. err.	Estimation of std. error Avg. bias	CovP (90%) in %
	(1)		(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	DGP:					**With selectivity and no CATE**						
ATE	*1*	*1000*	1.08	-	-	2.34	-0.1	2.9	2.3	1.07	0.12	78
GATE	*2*		1.09	-	-	2.38	-0.1	2.9	2.0	1.09	0.14	79
GATE	*32*		1.08	0	0.07	2.49	-0.1	2.9	1.6	1.15	0.21	83
IATE	*5000*		1.09	0	0.43	4.04	-0.1	3.0	2.8	1.63	0.68	94
ATE	*1*	*4000*	0.91	-	-	1.16	-0.3	3.1	3.6	0.57	0.05	56
GATE	*2*		0.90	-	-	1.18	-0.3	3.0	3.7	0.59	0.06	56
GATE	*32*		0.91	-	-	1.24	-0.2	3.0	2.6	0.65	0.17	73
IATE	*5000*		0.91	-	0.51	2.59	-0.0	2.9	1.7	1.22	0.58	94
	DGP:					**With selectivity and normal CATE**						
ATE	*1*	*1000*	1.91	-	-	5.03	0.1	2.7	3.6	1.18	0.04	50
GATE	*2*		1.90	-	-	5.04	0.0	2.7	3.2	1.20	0.06	52
GATE	*32*		1.88	0.17	0.07	5.12	0.0	2.8	3.9	1.24	0.13	57
IATE	*5000*		1.94	1.72	0.59	8.64	0.0	3.0	1.8	1.71	0.63	76
ATE	*1*	*4000*	1.60	-	-	2.90	-0.2	3.2	2.5	0.58	0.07	15
GATE	*2*		1.60	-	-	2.90	-0.2	3.2	2.1	0.60	0.08	19
GATE	*32*		1.58	0.17	0.08	2.97	-0.1	3.2	1.7	0.67	0.16	35
IATE	*5000*		1.66	1.72	0.74	5.91	0.0	3.0	2.2	1.25	0.58	76
	DGP:					**With selectivity and strong CATE**						
ATE	*1*	*1000*	3.20	-	-	12.01	0.0	2.9	0.5	1.32	0.12	27
GATE	*2*		3.19	-	-	11.93	0.0	2.9	0.5	1.33	0.14	29
GATE	*32*		3.15	0.66	0.27	11.91	0.0	2.9	0.5	1.36	0.18	34
IATE	*5000*		3.98	6.87	2.75	34.22	0.0	3.0	2.1	2.04	0.54	61
ATE	*1*	*4000*	2.02	-	-	4.61	-0.1	2.9	0.7	0.71	0.14	18
GATE	*2*		2.02	-	-	4.58	-0.1	2.9	0.7	0.72	0.15	21
GATE	*32*		1.98	0.66	0.42	4.61	-0.1	2.9	1.4	0.76	0.19	29
IATE	*5000*		2.56	6.87	4.13	16.44	0.0	2.9	1.8	1.49	0.52	67

Note: For GATE and IATE the *average bias* is the absolute value of the bias for the specific group (GATE) / observation (IATE) averaged over all groups / observation (each group / observation receives the same weight). *CovP (90%)* denotes the (average) probability that the true value is part of the 90% confidence interval.