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# The Balance-Sample Size Frontier in Matching Methods for Causal Inference\*

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## Abstract

We propose a simplified approach to matching for causal inference that simultaneously optimizes both balance (between the treated and control groups) and matched sample size. This procedure resolves two widespread tensions in the use of this popular methodology. First, current practice is to run a matching method that maximizes one balance metric (such as a propensity score or average Mahalanobis distance), but then to check whether it succeeds with respect to a different balance metric for which it was not designed (such as differences in means or  $L_1$ ). Second, current matching methods either fix the sample size and maximize balance (e.g., Mahalanobis or propensity score matching), fix balance and maximize the sample size (such as coarsened exact matching), or are arbitrary compromises between the two (such as calipers with ad hoc thresholds applied to other methods). These tensions lead researchers to either try to optimize manually, by iteratively tweaking their matching method and rechecking balance, or settle for suboptimal solutions. We address these tensions by first defining and showing how to calculate the *matching frontier* as the set of matching solutions with maximum balance for each possible sample size. Researchers can then choose one, several, or all matching solutions from the frontier for analysis in one step without iteration. The main difficulty in this strategy is that checking all possible solutions is exponentially difficult. We solve this problem with new algorithms that finish fast, optimally, and without iteration or manual tweaking. We also offer easy-to-use software that implements these ideas, along with analyses of the effect of sex on judging and job training programs that show how the methods we introduce enable us to extract new knowledge from existing data sets.

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# 1 Introduction

Matching has attained popularity among applied researchers as a statistically powerful and conceptually simple method of improving causal inferences in observational data analysis. It is especially simple in applied statistics when thought of as a nonparametric preprocessing step that identifies data subsets from which causal inferences can be drawn with greatly reduced levels of model dependence (Ho et al., 2007). Although successful applications of matching require both reduced imbalance (between the treated and control groups) and a sufficiently large matched sample, existing matching methods optimize with respect to only one of these two factors, with the required joint optimization performed by manually tweaking existing methods or ignored altogether. This is crucial since, if the subset identified by the matching method is too small, the reduction in model dependence (and hence bias) achieved will be counterbalanced by an unacceptably high variance. Similarly, the small variance associated with a large matched data subset may be counterbalanced by unacceptably high levels of imbalance (and thus model dependence and bias). Some of this problem may also be induced by current matching methods, which optimize with respect to one balance metric but cause researchers using them to check the level of balance achieved with respect to a different metric for which the method was not designed and does not optimize.

To remedy these problems, we introduce a procedure that enables researchers to define, estimate, visualize, and then choose from what we call the *matching frontier*, which fully characterizes the trade-off between imbalance (with a user-chosen metric) and the matched sample size. Unlike other approaches, we allow researchers to evaluate how much balance is achieved by pruning observations and simultaneously trade this off against the lower variance produced by larger matched sample sizes. At each location (denoted by the matched sample size) along the matching frontier, our approach offers a matched subset of the complete data such that no other possible subsets of the same size has lower imbalance. Any matching solution not on this frontier is suboptimal, in that a lower level of imbalance can be achieved for the same size data subset. This means that no matching method can outperform this approach, given a choice of imbalance metric. In

this sense, our approach achieves all of the benefits of any individual matching method, allows researchers to extract maximal causal information from their observational data, avoids many of the pitfalls and difficulties that lead researchers to ignore best practices in applications, is considerably easier to apply appropriately, and can reveal considerably more information about the data.

We begin by introducing the trade-off in matching between pruning observations to reduce model dependence and retaining observations to reduce variance (Section 2). We then detail our mathematical notation, goals, and assumptions (Section 3), the choices required for defining a matching frontier (Section 4), and a formal definition of and algorithms for calculating the frontier (Section 5). Finally, we offer several empirical examples (Section 6) and conclude (Section 7). Software to implement all the ideas in this paper can be found at <http://projects.iq.harvard.edu/frontier> (King, Lucas and Nielsen, 2015).

## 2 The Matching Frontier Trade-off

Matching methods selectively prune observations from a data set to reduce imbalance. A reduction in imbalance reduces, or reduces the bound on, the degree of model dependence, a result which has been shown both formally (King and Zeng, 2006; Imai, King and Stuart, 2008; Iacus, King and Porro, 2011a) and in real data (Ho et al., 2007). However, matching has a potential cost in that observations pruned from the data may increase the variance of the causal effect estimate. Although researchers using matching confront the same bias-variance trade-off as in most of statistics, two issues prevent one from optimizing on this scale directly. First, since matching is commonly treated as a preprocessing step, rather than a statistical estimator, particular points on the bias-variance frontier cannot be computed without also simultaneously evaluating the estimation procedure applied to the resulting matched data set. Second, best practice in matching involves avoiding selection bias by ignoring the outcome variable while matching (Rubin, 2008), the consequence of which is that we give up the ability to control either bias or variance directly.

Thus, instead of bias, matching researchers focus on reducing the closely related quantity, *imbalance*. The specific mathematical relationship between the two is given by Imai,

King and Stuart (2008) but conceptually, imbalance along with the relative importance of individual covariates determines bias. Researchers exclude relative importance because it cannot be estimated without the outcome variable (although scaling the covariates by prior expectations of importance is a common and valuable step). Similarly, instead of variance, researchers focus on the *matched sample size*. The variance is determined by the matched sample size along with the heterogeneity (i.e., residual unit-level variance) in the data. Researchers exclude heterogeneity because it can only be estimated by using the outcome variable.

Thus, the goal of matching involves the joint optimization of imbalance and matched sample size. Optimizing with respect to one, but not both, would be a mistake. Existing methods address the joint optimization by combining machine optimization of one of these factors with manual (human) optimization of the other. Of course, it is easy to see that optimizing by hand in this way is time consuming and usually produces results which are suboptimal.

Many good suggestions for ad hoc approaches to manual optimization of matching methods have appeared in the methodological literature (e.g., Austin, 2008; Caliendo and Kopeinig, 2008; Rosenbaum, Ross and Silber, 2007; Stuart, 2008). For example, Rosenbaum and Rubin (1984) detail their gradual refinement of an initial model by including and excluding covariates until they obtain a final model with 45 covariates, including 7 interaction degrees of freedom and 1 quadratic term. Ho et al. (2007, p.216) recommend trying as many matching solutions as possible and choosing the one with the best balance. Imbens and Rubin (2009) propose running propensity score matching, checking imbalance, adjusting the specification, and iterating until convergence, as well as manual adjustments. Applying most of these methods can be inconvenient, difficult to use optimally, and hard to replicate. With nearest neighbor and propensity score matching in particular, tweaking the procedure to improve imbalance with respect to one variable will often make it worse on others, and so the iterative process can be frustrating to apply in practice.

Because of these issues, following suggested best practices, such as these, in applied

literatures is rare. In what follows, we replace a machine-human optimization procedure with a machine-machine optimization procedure, thus guaranteeing optimal results in considerably less time.

### 3 Causal Inference Objectives

We define here our notation (Section 3.1) and choices for the causal quantity of interest (Section 3.2). We separate discussion of the necessary assumptions into those which are logically part of the notation and, in Section 3.3, assumptions which become necessary when trying to learn about the quantities of interest from the data.

#### 3.1 Notation and Basic Assumptions

For unit  $i$ , let  $T_i$  denote a treatment variable coded 1 for units in the treated group and 0 in the control group. Let  $Y_i(t)$  (for  $t = 0, 1$ ) be the (potential) value the outcome variable would take if  $T_i = t$ . Denote the treatment effect of  $T$  on  $Y$  for unit  $i$  as  $TE_i = Y_i(1) - Y_i(0)$ . However, for each  $i$ , either  $Y_i(1)$  or  $Y_i(0)$  is observed, but never both (which is known as the fundamental problem of causal inference; Holland 1986). This means we observe  $Y_i = T_i Y_i(1) + (1 - T_i) Y_i(0)$ . Finally, define a vector of  $k$  pre-treatment control variables  $X_i$ .

We simplify this general framework by restricting ourselves to studies or functions of TEs only for treated units. (Since the definition of which group is labeled treated is arbitrary, this does not restrict us much in practice.) In this situation, the only unobservables are  $Y_i(0)$  for units which received treatment  $T_i = 1$  (since  $Y_i(1) \equiv Y_i$  is observed for these units).

A coherent interpretation of this notation implies two assumptions (Imbens, 2004). First, is *overlap* (sometimes called “common support”):  $\Pr(T_i = 1|X) < 1$  for all  $i$  (see also Heckman, Ichimura and Todd, 1998, p.263). The idea here is that for treated units, where  $T_i = 1$ , it must be conceivable that before treatment assignment an intervention could have taken place that would have assigned unit  $i$  instead to the control group, while holding constant the values of  $X$ . If this could not have been possible, then  $Y_i(0)$ , which we need to define  $TE_i$ , does not even logically exist.

A second assumption implied by the notation is *stable unit treatment value* (SUTVA),

which can be thought of as logical consistency, so that each potential value is fixed even if  $T$  changes (or conceptualized differently, no interference and no versions of treatments) (VanderWeele and Hernan, 2012). As an example that would violate this assumption, suppose  $Y_i(0) = 5$  in the actual case for this treated unit where  $T_i = 1$ , but  $Y_i(0) = 8$  in the counterfactual condition if this same unit had instead been assigned to the control group. If overlap holds but SUTVA does not, then  $Y_i(0)$  and  $TE_i$  exist but are not fixed quantities to be estimated.

## 3.2 Quantities of Interest

From the basic definitions in Section 3.1, we can compute many quantities, based on the average of TE over given subsets of units. We focus on two in this paper. We first define these theoretically and then explain how they work in practice, followed by the assumptions necessary for identification.

First is the *sample average treatment effect on the treated*,  $SATT = \text{mean}_{i \in \{T=1\}}(TE_i)$ , which is TE averaged over the set of all treated units  $\{T = 1\}$  (Imbens, 2004). (Formally, for set  $S$  with cardinality  $\#S$ , define the average over  $i$  of function  $g(i)$  as  $\text{mean}_{i \in S}[g(i)] = \frac{1}{\#S} \sum_{i=1}^{\#S} g(i)$ .) If matching only prunes data from the control group, SATT is fixed throughout the analysis.

Second, since many real observational data sets contain some treated units without good matches, analysts often choose to compute a causal effect among only those treated observations for which good matches exist. We designate this as the *feasible sample average treatment effect on the treated* or FSATT.<sup>1</sup> Other possibilities include TE averaged over all observations, population average treatment effects, among others.

Although the distinction between SATT and FSATT is clear given a data set, the dis-

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<sup>1</sup>Using FSATT is common in the matching literature but may be seen as unusual elsewhere in that the quantity of interest is defined by the statistical procedure, but in fact it follows the usual practice in observational data analysis of collecting data and making inferences only where it is possible to learn something. The advantage here is that the methodology makes a contribution to a step previously considered outside the statistical framework (e.g., Crump et al., 2009; Iacus, King and Porro, 2011a), just as measurement error, missing data, selection bias, and other issues once were. As Rubin (2010, p.1993) puts it, “In many cases, this search for balance will reveal that there are members of each treatment arm who are so unlike any member of the other treatment arm that they cannot serve as points of comparison for the two treatments. This is often the rule rather than the exception, and then such units must be discarded. . . . Discarding such units is the correct choice: A general answer whose estimated precision is high, but whose validity rests on unwarranted and unstated assumptions, is worse than a less precise but plausible answer to a more restricted question.”

tion can blur in practice because in observational data analysis there often exists no “correct” or canonical definition of the target population. Usually if observational data analysts have access to more relevant data, they use it; if a portion of the data are not helpful or too difficult to use, because of measurement problems or the extreme nature of the counterfactual inferences required, they drop it. If we have a new more powerful telescope, we observe more — for that reason. Thus, since observational data analysis is, in practice, an opportunist endeavor, we must recognize first that even the choice of SATT as a quantity of interest always involves some feasibility restriction (quite like FSATT), either explicitly where we choose to make a SATT inference in a chosen subset of our data, or implicitly due to the choice of our data set to begin with. Thus, regardless of the definition of the units over which an average will take place, researchers must always be careful to characterize the resulting new estimand, for which we offer some tools below.

Suppose we insist on choosing to estimate SATT even though some counterfactuals are so far from our data that they have no reasonable matches and require (perhaps extreme) model-based extrapolation. If SATT really is the quantity of interest, this situation cannot be avoided (except when it is possible to collect more data). To understand this problem, we follow [Iacus, King and Porro \(2011a\)](#) and partition the  $N$  treated units into  $N_f$  treated units that can be well matched with controls (a “feasible estimation set”) and  $N_{nf}$  remaining treated units that cannot be well matched (a “nonfeasible estimation set”), such that  $N = N_f + N_{nf}$ . In this case, we express SATT as a weighted average of an estimator applied to each subset separately:

$$\text{SATT} = \frac{\text{FSATT} \cdot N_f + \text{NFSATT} \cdot N_{nf}}{N} \tag{1}$$

When estimating SATT in this unfortunate, but common, situation, it is often worthwhile to compute its two subcomponents separately since only FSATT will be estimatable without (much) model dependence. We refer to the subsets of observations corresponding to FSATT and NFSATT, respectively as the *overlap set* and *nonoverlap set*.<sup>2</sup>

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<sup>2</sup>Although these definitions borrow language from the related overlap assumption introduced in Section 3.1, the two are distinct: regardless of whether a matching counterfactual observation exists to estimate  $Y_i(0)$ , we need to ensure that the ex ante probability of an alternative treatment assignment would have been possible for observation  $i$ . However, if the overlap assumption is violated, it would be impossible to find a



### 3.3 Statistical Assumptions

To establish statistical properties for estimators of these quantities, statisticians typically posit an extra layer of complication by imagining a superpopulation from which the observed data are drawn repeatedly and then attempting to infer fixed population quantities from the average over hypothetical applications of an estimator to repeated hypothetical draws from the population. (Other, alternative sampling and modeling frameworks are sometimes suggested instead, but all add an extra imaginary layer of some kind.) We first explain the assumption necessary in this hypothetical situation, simplified for SATT, but then go a step further and establish conditions under which we would get the correct answer in our one sample, without an extra layer of complication. As it turns out, these conditions are simpler and easier to understand.

First, for formal statistical identification, we make an *ignorable treatment assignment* (ITA) assumption, which for SATT requires that the mechanism that produced the treatment assignment (i.e., the values of  $T$ ) be independent of the potential outcome  $Y_i(0)$  given  $X$ :  $T_i \perp Y_i(0) | X$  for all treated units (Rosenbaum and Rubin, 1983; Barnow, Cain and Goldberger, 1980). This independence assumption can be weakened, to mean independence and in other ways, that are not usually crucial distinctions in practice (Imbens, 2004). (This assumption has also been referred to as “selection on observables,” “unconfoundedness,” and “conditional independence”; special cases of it are referred to as “exogeneity,” “no omitted variable bias,” among others.) Perhaps the simplest way to satisfy this assumption is to include in  $X$  any variable which from prior research or theory is known to cause either  $Y$  or  $T$ , since if any subset of these variables satisfies ITA, this set will too (VanderWeele and Shpitser, 2011).

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suitable counterfactual observation.

In all cases, ways of estimating the overlap set (see Section 3.2) necessarily depend on substantive characteristics of the data, but methodologists usually attempt to offer some guidance on the basis of the data alone. The simplest and most stringent existing definition for the overlap region is exact matching (Manski, 1995). However, in part because in most applications this definition would result in almost all observations being in the nonoverlap set and in part because reasonable smoothness assumptions make extrapolating small distances over continuous space relatively safe (Zhao, 2004), most scholars choose more relaxed definitions. Some others include definitions based on nonparametric estimation of the propensity score, the quality of the worst individual matches (Imbens, 2004), and the convex hull (King and Zeng, 2006). In Section 4.3 we offer approaches that seem naturally implied by each imbalance metric. In this way, we reduce the number of adjustable parameters to be chosen or assumed in using our methodology.

Second, for clarity we pair down ITA and its superpopulation sampling framework to its essentials necessary for getting the correct answer in sample:  $\widehat{\text{SATT}} = \text{SATT}$ . We retain the counterfactual framework inherent in the definition of causality so that  $Y_i(0)$  is unobserved but defined for treated units. However, for point estimates, imagining infinite random draws from an invented population is unnecessary. And if we wish to consider this (such as for some types of uncertainty estimates) we shall follow the principle of privileging the sample in hand so that SATT is defined over the treated units in the only data set we actually observe.

The idea of (exact) matching is to replace the unobserved  $Y_i(0)$  for each treated unit  $i$  with an observed  $Y_j(0) \equiv Y_j$  for a control unit (i.e.,  $T_j = 0$ ) with matching covariate values ( $X_i = X_j$ ). A sufficient (but not necessary) condition for  $\widehat{\text{SATT}} = \text{SATT}$  is  $(Y_i(0)|T_i = 1, X_i) = (Y_j(0)|T_j = 0, X_i)$  for all treated units  $i$  (with matching controls  $j$ ). However, if any  $Y_j(0)$  from individual matches does not equal  $Y_i(0)$ , we can still estimate SATT correctly so long as the estimates of  $Y_i(0)$  are right *on average* over treated units. Note that this sounds like, but is distinct from, the concept of “unbiasedness,” which refers to averages over hypothetical repeated draws from an imaginary superpopulation, rather than our need for being correct on average over the real in-sample treated units. We formalize this idea with the less restrictive *uncorrelated treatment assignment* (UTA) assumption, which is that  $Y(0)$  is uncorrelated with  $T$ , within strata of  $X$ , or equivalently but more simply:  $\text{mean}_i(Y_i(0)|T = 0, X) = \text{mean}_j(Y_j(0)|T = 1, X)$ , which means that within strata defined by  $X$ , the average of  $Y_i(0)$  for (unobserved) treated and (observed) control units is the same.

An even easier way to understand the UTA assumption is to consider the case with a data set composed of one-to-one exact matches. Exact matching is equivalent to conditioning on  $X$  and one-to-one matching means that weighting within strata is not required (see Section 4.1). In this simpler situation,  $X$  is irrelevant and the assumption is simply  $\text{mean}(Y_i(0)|T = 1) = \text{mean}(Y_j(0)|T = 0)$ , which reduces to  $\text{mean}_i(Y_i(0)|T_i = 1) = \text{mean}_j(Y_j|T_j = 0)$  since the second term is fully observed.

When used in practice, applying UTA (or ITA) requires both (a) choosing and measur-

ing the correct variables in  $X$  and (b) using an analysis method that controls sufficiently for the measured  $X$  so that  $T$  and  $Y(0)$  are sufficiently close to unrelated that any biases that a relationship generates can for practical purposes be ignored (i.e., such as if they are much smaller than the size of the quantities being estimated). Virtually all observational data analysis approaches, including matching and modeling methods, assume (a) holds as a result of choices by the investigator. This includes defining which variables are included in  $X$  and ensuring that the definition of each of the variables has a meaningful scaling or metric. Then, given the choice of  $X$ , the methods distinguish themselves by how they implement approximations to (b) given the choice for the definition of  $X$ .

## 4 Matching Frontier Components

The matching frontier (defined formally in Section 5) requires the choice of options for four separate components. In addition to the quantity of interest (Section 3.2), they include and we now describe fixed- v. variable-ratio matching (Section 4.1), a definition for the units to be dropped (Section 4.2), and the imbalance metric (Section 4.3).

### 4.1 Fixed- or Variable-Ratio Matching

Some matching methods allow the ratio of treated to control units to vary, whereas others restrict them to have a fixed ratio throughout a matched data set. Fixed-ratio matching can be less efficient than variable ratio matching because some pruning usually occurs solely to meet this restriction. However, an important goal of matching is simplicity and encouraging researchers to understand and use this powerful procedure, and so the ability to match without having to modify existing analysis procedures, remains popular. (Fixed-ratio matching is also useful in large data sets where the primary goal is reducing bias.) Indeed, most applications involve the even more restrictive requirement of 1-to-1 matching, or sometimes 1-to- $p$  matching with larger  $p$ .

In fixed-ratio matching, SATT can be estimated by a simple difference in means between the treated and control groups:  $\text{mean}_{i \in \{T=1\}}(Y_i) - \text{mean}_{j \in \{T=0\}}(Y_j)$ .

In variable-ratio matching, we can estimate the TE within each matched stratum  $s$  by a simple difference in means:  $\text{mean}_{i \in s, \{T=1\}}(Y_i) - \text{mean}_{j \in s, \{T=0\}}(Y_j)$ . However, aggregating up to SATT requires weighting, with the stratum-level TE weighted according to

the number of treated units. Equivalently, a weighted difference in means can be computed, with weights  $W$  such that each treated unit  $i$  receives a weight of  $W_i = 1$ , and each control unit  $j$  receives a weight of  $W_j = (m_0/m_1)[(m_{s_1})/(m_{s_0})]$  where  $m_0$  and  $m_1$  are respectively the number of control and treated units in the data set, and  $m_{s_1}$  and  $m_{s_0}$  are the number of treated and control units in the stratum containing observation  $j$ .<sup>3</sup>

## 4.2 Defining the Number of Units

The size and construction of a matched data set influences the variance of the causal effect estimated from it. Under SATT, the number of treated units remain fixed and so we measure the data set size by the number of control units. For FSATT, we measure the total number of observations .

For both quantities of interest, we will ultimately use an estimator equal to or a function of the difference in means of  $Y$  between the treated and control groups. The variance of this estimator is proportional to  $\frac{1}{n_T} + \frac{1}{n_C}$ , where  $n_T$  and  $n_C$  are the number of treated and control units in the matched set. Thus, the variance of the estimator is largely driven by  $\min(n_T, n_C)$ , and so we will also consult this as an indicator of the size of the data set.

To simplify notation in these different situations, we choose a method of counting from those above and let  $N$  denote the number of these units in the original data and  $n$  the number in a matched set, with  $n \leq N$ . In our graphs, we will represent this information as the number of units pruned, which is scaled in the same direction as the variance.

## 4.3 Imbalance Metrics

An *imbalance measure* is a (nondegenerate) indicator of the difference between the multivariate empirical densities of the  $k$ -dimensional covariate vectors of treated  $X_1$  and control  $X_0$  units for any data set (i.e., before or after matching). Our concept of a matching frontier, which we define more precisely below, applies to any imbalance measure a researcher may choose. We ease this choice by narrowing down the reasonable possibilities from measures to metrics and then discuss five examples of continuous and discrete families of these metrics. For each example, we give metrics most appropriate for FSATT and SATT when feasible.

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<sup>3</sup>See [j.mp/CEMweights](https://j.mp/CEMweights) for further explanation of these weights. Also, for simplicity, we define any reuse of control units to match more than one control as variable-ratio matching.

**Measures v. Metrics** To narrow down the possible measures to study, we restrict ourselves to the more specific concept of an *imbalance metric*, which is a function  $d : [(m_0 \times k) \times (m_1 \times k)] \rightarrow [0, \infty]$  with three properties, required for a generic semi-distance:

1. **Nonnegativeness:**  $d(X_0, X_1) \geq 0$ .
2. **Symmetry:**  $d(X_0, X_1) = d(X_1, X_0)$  (i.e., replacing  $T$  with  $1 - T$  will have no effect).
3. **Triangle inequality:**  $d(X_0, X_1) + d(X_1, Z) \geq d(X_0, Z)$ , given any  $k$ -vector  $Z$ .

Imbalance measures that are not metrics have been proposed and are used sometimes, but they add complications such as logical inconsistencies without conferring obvious benefits. Fortunately, numerous imbalance metrics have been proposed or could be constructed.

**Continuous Imbalance Metrics** The core building block of a continuous imbalance metric is a (semi-)distance  $D(X_i, X_j)$  between two  $k$ -dimensional vectors  $X_i$  and  $X_j$ , corresponding to observations  $i$  and  $j$ . For example, the Mahalanobis distance is  $D(X_i, X_j) = \sqrt{(X_i - X_j)S^{-1}(X_i - X_j)}$ , where  $S$  is the sample covariance matrix of the original data  $X$ . The Euclidean distance would result from redefining  $S$  as the identity matrix. Numerous other existing definitions of continuous metrics could be used instead. Although one can always define a data set that will produce large differences between any two metrics, in practice the differences among the choice of these metrics are usually not large or at least not the most influential choice in most data analysis problems (Zhao, 2004; Imbens, 2004). We use the most common choice of the Mahalanobis distance below for illustration, but any of the others could be substituted. In real applications, scholars should choose variables, the coding for the variables, and the imbalance metric together to reflect their substantive concerns: with this method as with most others, the more substantive knowledge one encodes in the procedure, the better the result will be.

To get from a distance between two individual observations to an imbalance metric comparing two sets of observations, we need to aggregate the distance calculations over observations in one of two ways. One way to do this is with the *Average Mahalanobis*

*Imbalance* (AMI) metric, which is the distance between each unit  $i$  and the closest unit in the opposite group, averaged over all units:  $D = \text{mean}_i[D(X_i, X_{j(i)})]$ , where the closest unit in the opposite group is  $X_{j(i)} = \arg \min_{X_j | j \in \{1-T_i\}}[D(X_i, X_j)]$  and  $\{1 - T_i\}$  is the set of units in the (treatment or control) group that does not contain  $i$ .

For SATT, it is helpful to have a way to identify the overlap and nonoverlap sets. A natural way to do this for continuous metrics is to define the nonoverlap region as the set of treated units for which no control unit has chosen it as a match. More precisely, denote the (closest) treated unit  $i$  that control unit  $j$  matches to by  $j(i) \equiv \arg \min_{i | i \in \{T=1\}}[D(X_i, X_j)]$ . Then define the overlap and nonoverlap sets, respectively, as

$$\mathcal{O} \equiv \{j(i) \mid j \in \{T = 0\}\} \quad (2)$$

$$\mathcal{NO} \equiv \{i \mid i \in \{T = 1\} \wedge \{i \notin \mathcal{O}\}\} \quad (3)$$

where  $\wedge$  means “and,” connecting two statements required to hold.

**Discrete Imbalance Metrics** Discrete imbalance metrics indicate the difference between the multivariate histograms of the treated and control groups, defined by fixed bin sizes  $H$ . Let  $f_{\ell_1 \dots \ell_k}$  be the relative empirical frequency of treated units in a bin with coordinates on each of the  $X$  variables as  $\ell_1 \dots \ell_k$  so that  $f_{\ell_1 \dots \ell_k} = n_{T_{\ell_1 \dots \ell_k}} / n_T$  where  $n_{T_{\ell_1 \dots \ell_k}}$  is the number of treated units in stratum  $\ell_1 \dots \ell_k$  and  $n_T$  is the number of treated units in all strata. We define  $g_{\ell_1 \dots \ell_k}$  similarly among control units. Then, among the many possible metrics built from these components, we consider two:

$$L_1(H) = \frac{1}{2} \sum_{(\ell_1 \dots \ell_k) \in H} |f_{\ell_1 \dots \ell_k} - g_{\ell_1 \dots \ell_k}| \quad (4)$$

and

$$L_2(H) = \frac{1}{2} \sqrt{\sum_{(\ell_1 \dots \ell_k) \in H} (f_{\ell_1 \dots \ell_k} - g_{\ell_1 \dots \ell_k})^2} \quad (5)$$

To remove the dependence on  $H$ , [Iacus, King and Porro \(2011a\)](#) define  $L_1$  as the median value of  $L_1(H)$  from all possible bin sizes  $H$  in the original unmatched data (approximated by random simulation); we use the same value of  $H$  to define  $L_2$ . The typically numerous empty cells of each of the multivariate histograms do not affect  $L_1$  and  $L_2$ , and so the summation in (4) and (5) each have at most only  $n$  nonzero terms.

When used for creating SATT frontiers, these discrete metrics suggest a natural indicator of the nonoverlap region: all observations in bins with either one or more treated units and no controls or one or more control units and no treated.

With variable-ratio matching, and the corresponding weights allowed in the calculation of  $L_1$ , the metric is by definition 0 in the overlap region. With fixed-ratio matching,  $L_1$  will improve as the heights of the treated and control histogram bars within each bin in the overlap region equalize. (In other words, what the weights, included for variable-ratio matching, do is to equalize the heights of the histograms without pruning observations.)

**Adjusting Imbalance Metrics for Relative Importance** Every imbalance metric is conditional on a definition of the variables in  $X$ , and so researchers must think carefully about what variables may be sufficient in their application. This choice also involves carefully defining the measurement or scaling of each covariate so that it makes sense (e.g., ensuring interval-level measurement for continuous distance metrics) and reflects prior information about its importance in terms of its relationship with  $Y|T$ . In almost all areas of applied research, the most important covariates are well known to researchers — such as age, sex, and education in public health, or partisan identification and ideology in political science. Since bias is a function of both importance and imbalance, and matching can only affect the latter (Imai, King and Stuart, 2008), we will want to require better matches on important covariates. This is easy to do in the context of most imbalance metrics by adjusting weights used in continuous metrics (such as  $S$  in average Mahalanobis or Euclidean distance; see Greevy et al. 2012) or variable-specific coarsening used in discrete metrics (such as  $H$  in  $L_1$ ).

## 5 Constructing Frontiers

Now that we have given our notation and components of a frontier, we offer a formal definition along with algorithms for calculating the (theoretical) frontier directly.

### 5.1 Definition

Begin by choosing an imbalance metric  $d(x_0, x_1)$  (see Section 4.3), a quantity of interest  $Q$  (SATT or FSATT described in Section 3.2), whether to use weights (to allow variable-ratio matching) or no weights (as in fixed-ratio matching)  $R$  (Section 4.1), and a definition

for the number of units  $U$  (Section 4.2). We will consider all matched data set sizes from the original  $N$ , all the way down  $n = N, N - 1, N - 2 \dots, 2$ .

For quantity of interest SATT, where only control units are pruned, denote  $\mathcal{X}_n$  as the set of all  $\binom{N}{n}$  possible data sets formed by taking every combination of  $n$  rows (observations) from the  $(N \times k)$  control group matrix  $X_0$ . Then denote the combined set of all sets  $\mathcal{X}_n$  as  $\mathcal{X} \equiv \{\mathcal{X}_n \mid n \in \{N, N - 1, \dots, 1\}\}$ . This combined set  $\mathcal{X}$  is (by adding the null set) the *power set* of rows of  $X_0$ , containing (a gargantuan)  $2^N$  elements. For example, if the original data set contains merely  $N = 300$  observations, the number of elements of this set exceeds current estimates of the number of elementary particles in the universe. The task of finding the frontier requires identifying a particular optimum over the entire power set. As it turns out, with our new algorithms, this task, as we show below, can often be accomplished fast and efficiently.

To be more specific, first identify an element (i.e., data set) of  $\mathcal{X}_n$  with the lowest imbalance for a given matched sample size  $n$ , and the choices of  $Q, U$ , and  $R$ :

$$x_n = \arg \min_{x_0 \in \mathcal{X}_n} d(x_0, x_1), \text{ given } Q, R, \text{ and } U. \quad (6)$$

where for convenience when necessary we define the  $\arg \min$  function in the case of nonunique minima as a random draw from the set of data sets with the same minimum imbalance. We then create a set of all these minima  $\{x_N, x_{N-1}, \dots, 1\}$ , and finally define the *matching frontier* as the subset  $\mathcal{F}$  of these minima after imposing monotonicity, which involves eliminating any element which has higher imbalance with fewer observations:

$$\mathcal{F} \equiv \{x_n \mid (n \in \{N, N - 1, \dots, 1\}) \wedge (d_{n-1} \leq d_n)\} \quad (7)$$

where  $d_n = d(x_n, x_1)$ . We represent a frontier by plotting the number of observations pruned  $N - n$  horizontally and  $d_n$  vertically.

For simplicity, we will focus on SATT here, but our description also generalizes to FSATT by defining  $\mathcal{X}_n$  as the set of all combinations of the entire data matrix  $(X_0', X_1)'$  taken  $n$  at a time.



## 5.2 Algorithms

Calculating the frontier requires finding a data subset of size  $n$  with the lowest imbalance possible chosen from the original data of size  $N$  for each possible  $n$  ( $N > n$ ) — given choices of the quantity of interest (and thus the definition of the units to be pruned), fixed- or variable-ratio matching, and an imbalance metric, along with the monotonicity restriction.

Adapting existing approaches to algorithms for this task is impractical. The most straightforward would involve directly evaluating the imbalance of the power set of all possible subsets of observations. For even moderate data set sizes, this would take considerably longer than the expected lives of most researchers (and for reasonably sized data sets far longer than life has been on the planet). Another approach could involve evaluating only a sample of all possible subsets, but this would be biased, usually not reaching the optimum (for the same reason that estimating a maximum by sampling is an underestimate). Finally, adapting general purpose numerical optimization procedures designed for similar but different purposes, such as [Diamond and Sekhon \(2012\)](#), would take many years and in any event are not guaranteed to reach the optimum.

The contribution of our algorithms, then, is not that they can find the optimum, but that they can find it fast. Our solution is to offer analytical rather than numerical solutions to this optimization problem cases by leveraging the properties of specific imbalance metrics. The key to each is developing greedy algorithms that return optimal results.

We now outline algorithms we developed for calculating each of four families of matching frontiers, with many possible members of each. We leave to future research the derivation of algorithms for other families of matching frontiers (defined by choosing among permutations of the choices defined in [Section 5.1](#), and finding a feasible algorithm). In all cases with SATT frontiers, we first remove the nonoverlap set and then compute the remaining frontier within the overlap set. FSATT frontiers do not require this separate step.

### 5.2.1 Continuous, FSATT, Variable Ratio

Our first family of algorithms is based on the average continuous imbalance metric for FSATT with variable ratio matching. We illustrate this with the AMI metric, although any other continuous metric could be substituted. Define  $N$  as the number of units in the original data,  $n$  as the number that have not yet been pruned, and  $D_n$  as the current matched data set. Then our algorithm requires five steps:

1. Begin with  $N = n$ .
2. Match each observation  $i$  ( $i = 1, \dots, N$ ) to the nearest observation  $j(i)$  in the opposite treatment condition  $\{1 - T_i\}$ ; this matched observation has index  $j(i) = \arg \min_{j \in \{1 - T_i\}} [D(X_i, X_j)]$ , and distance  $d_{j(i)} \equiv D(X_i, X_{j(i)})$ .
3. Calculate AMI for  $D_n$ .
4. Prune from  $D_n$  the unit or units with distance  $d_{j(i)}$  equal to  $\max(d_{j(i)} | i \in D_n)$ . Redefine  $n$  as the number of units remaining in newly pruned data set  $D_n$ .
5. If  $n > 2$  and  $\text{AMI} > 0$ , go to Step 3; else stop.

This is a greedy algorithm, but we now prove that it is optimal by proving two claims that, if true, are sufficient to establish optimality. Below, we offer a simple example.

**Claim 1.** *For each unit  $i$  remaining in the data set, the nearest unit (or units) in the opposite treatment regime  $j(i)$  (defined in Step 2) remains unchanged as units are pruned in Step 4.*

**Claim 2.** *The subset  $D_n$  with the smallest AMI, among all  $\binom{N}{n}$  subsets of  $D_N$ , is that defined in Step 4 of the algorithm.*

Given Claim 1, Claim 2 is true by definition since  $D_n$  includes the  $n$  smallest distances within  $D_N$ .

We prove Claim 1 by contradiction. First assume that the next unit to be pruned is the nearest match for a unit not yet pruned (hence requiring the data set to be rematched). However, this is impossible because the unit not yet pruned would have had to have a

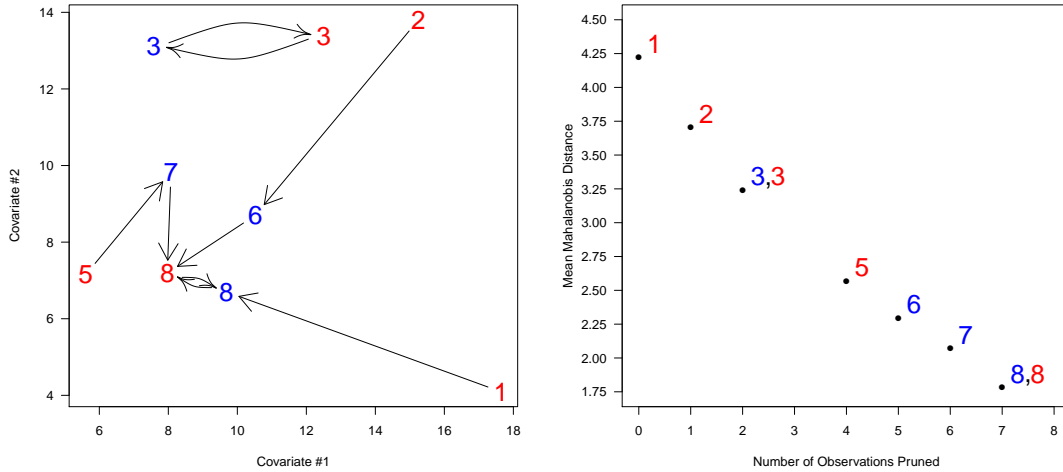


Figure 1: The left panel gives an example data set with two covariates, one on each axis. The numbers indicate the order in which observations are pruned, the arrows denote the observation in the opposite treatment regime to which each unit is matched, and the color denotes treatment regime (treatment or control). The right panel displays the frontier for this data set, where points are labeled to correspond with the left plot.

distance to the pruned unit lower than the maximum, in which case it would have been matched to that unit originally, which is not the case. This proves Claim 1. By induction, we have the proof of optimality.

To illustrate, consider the simple data set in the left panel of Figure 1, with four treated units (blue numbers) and five control units (red numbers), each measured on two covariates (one on each axis). We denote, with an arrow coming from each unit, the closest unit (measured in Mahalanobis distance) in the opposite treatment regime.

The algorithm is illustrated by removing the observations in numerical order in the left panel of Figure 1 (starting with 1); the right panel gives the frontier for these data, numbered with the observation to be removed next.

The figure also illustrates the special case of “mutual minima,” which is a treated unit that has as its nearest match a control unit which, in turn, has as its closest match the same treated unit. In this situation, we remove both (or more if they are also tied) units at the same time, as per Step 4. This is illustrated in the left panel of Figure 1 by two arrows between the pair of observations marked 3 (and also 8, when only 2 units remain). Technically, this means that some points on the frontier are not represented, such as if we

wished to prune exactly 3 observations in this simple example. Although we could fill in these missing points by enumerating and checking all possible data sets this size, we find that omitting them, and thus saving substantial computational time, is almost costless from a practical point of view: a researcher who wants a data set of a particular sample size would almost always be satisfied with a very slightly different sample size. With a realistic sized data set, the missing points on the frontier, like discrete points in a (near) continuous space, are graphically invisible and for all practical purposes substantively irrelevant.<sup>4</sup>

Put differently, the intuition as to why the greedy algorithm is also optimal is that at every point along the frontier, the closest match for each observation in the remaining set is the same as that in the full set, which implies that (1) each observation contributes a fixed distance to the average distance for the entire portion of the frontier in which it remains in the sample and (2) observations do not need to be re-matched as others are pruned.

### 5.2.2 Continuous, SATT, Variable Ratio

The SATT frontier is identical to the FSATT frontier when the SATT requirement of keeping all treated units is not binding. This occurs when the  $\max_i d_{j(i)}$  calculation in the FSATT algorithm leads us to prune only control units. Usually, this is the case for some portion of the frontier, but not the whole frontier. When the calculable part of this SATT frontier is not sufficient, part of each matched data set along the rest of the frontier will have a nonoverlap region, requiring extrapolation. In these situations, we recommend using the FSATT algorithm for the overlap region, extrapolating to the remaining nonoverlap region and combining the results as per Equation 1.

### 5.2.3 Discrete, FSATT, Variable Ratio

As a third family of frontiers, we can easily construct a discrete algorithm for FSATT with variable ratio matching, using  $L_1$  for exposition. With variable ratio matching in

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<sup>4</sup>It is important to note that the missing points on the frontier cannot violate the monotonicity constraint. If the point after the gap contains  $n$  observations, those  $n$  observations are the  $n$  observations with the closest matches. Therefore, there can be no set of  $n + 1$  observations (say in the gap) with a lower mean distance than that in the  $n$  observations contained at the defined point, because the additional observation must have a distance equal to or greater than the greatest distance in the existing  $n$  observations.

discrete metrics, weighting eliminates all imbalance in the overlap set, making this frontier a simple step function with only two steps. The first step is defined by  $L_1$  for the original data and the second is  $L_1$  after the nonoverlap region has been removed. Within each step, we could remove observations randomly one at a time, but since imbalance does not decline as a result it makes more sense to only define the frontier for only these two points on the horizontal axis.

If the binning  $H$  is chosen to be the same as the coarsening in coarsened exact matching (CEM), the second step corresponds exactly to the observations retained by CEM (Iacus, King and Porro, 2011a,b).

#### 5.2.4 Discrete, SATT, Fixed-Ratio

A final family of frontiers is for discrete metrics, such as  $L_1$  or  $L_2$ , for quantity of interest SATT with fixed-ratio matching. To define the algorithm, first let  $b_{iT}$  and  $b_{iC}$  be the numbers, and  $p_{iT} = b_{iT}/n_T$  and  $p_{iC} = b_{iC}/n_C$  be the proportions, of treated and control units in bin  $i$  ( $i \in \{1, \dots, B\}$ ) in the  $L_1$  multivariate histogram, where  $n_T = \sum_{i=1}^B b_{iT}$  and  $n_C = \sum_{i=1}^B b_{iC}$  are the total numbers of treated and control units.

To prune  $k$  observations optimally (that is with minimum  $L_1$  imbalance) from a data set of sized  $N$ , we offer this algorithm:

1. Define  $p'_{iC} = b_{iC}/(n_C - k)$ .
2. Prune up to  $k$  units from any bin  $i$  where after pruning  $p'_{iC} \geq p_{iT}$  holds.
3. If  $k$  units have not been pruned in step 2, prune the remaining  $k'$  units from the bins with the  $k'$  largest differences  $p_{iC} - p'_{iT}$ .

An optimal frontier can then be formed by applying this algorithm with  $k = 1$  pruned and increasing until small numbers of observations results in nonmonotonicities in  $L_1$ .

The discreteness of the  $L_1$  imbalance metric means that multiple data sets have equivalent values of the imbalance metric for each number of units pruned. Indeed, it is possible with this general algorithm to generate one data set with  $k - 1$  pruned and another with  $k$  pruned that differ with respect to many more than one unit. In fact, even more complicated is that units can be added, removed, and added back for different adjacent points on the

frontier. This of course does not invalidate the algorithm, but it would make the resulting frontier difficult and use in applied research. Thus, to make this approach easier to use, we choose a greedy algorithm which is a special case of this general optimal algorithm. The greedy algorithm is faster but more importantly it, by definition, never needs to put a unit back in a data set once it had been pruned.

Our greedy (and also optimal) algorithm is as follows. Starting with the full data set  $N = n$ ,

1. Compute and record the value of  $L_1$  and the number of units  $n$ .
2. Prune a control unit from the bin with the maximum difference between the proportions of treated and control units, such that there are more controls than treated.

That is, prune a unit from bin  $f(i)$ , where

$$f(i) = \arg \max_{i \in \{n_C > n_T\}} |p_{ic} - p_{it}| \quad (8)$$

3. If  $L_1$  is larger than the previous iteration, stop; otherwise go to step 1.

To understand this algorithm intuitively, first note that deleting a control unit from any bin with more controls than treateds changes  $L_1$  by an equal amount (because we are summing over bins normalized by the total number of controls, rather than by the number of controls in any particular bin). When we delete a control unit one bin, the relative size of all the other bins increase slightly, because all the bins must always sum to 1. Deleting controls from bins with the greatest relative difference, as we do, prevents the relative number of treated units from ever overtaking the relative number of controls in any bin, and guarantees that this greedy algorithm is optimal.

To illustrate the greedy version of this optimal algorithm, Figure 2 gives a simple univariate example. Panel 0 in the top left represents the original data set with a histogram in gray for controls and black for treateds. The  $L_1$  imbalance metric for Panel 0 is reported in the frontier (Figure 2, bottom right) marked as “0”. The red unit in Panel 0 is the next control unit to be removed, in this case because it is in a bin without any treated units.

Then, Panel 1 (i.e., where 1 observation has been pruned) removes the red unit from Panel 0, and renormalizes the height of all the bars with at least some control units so

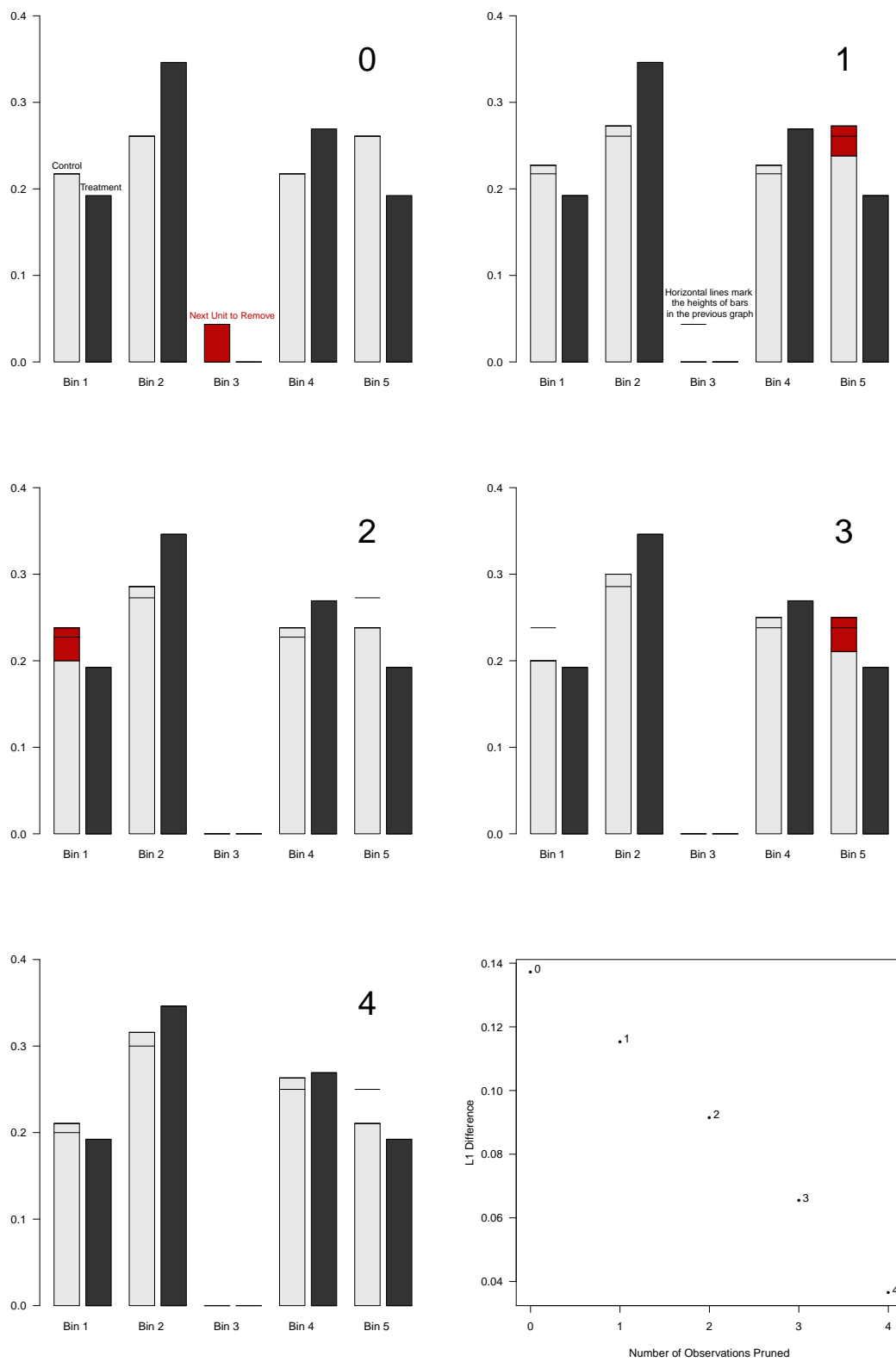


Figure 2: A demonstration of the L1 SATT algorithm

that they still sum to one. As indicated by the extra horizontal lines, reflecting the heights of control histogram bars from the previous panel, the height of each of the remaining the gray control histogram bars have increased slightly. The “1” point in the bottom right panel in Figure 2 plots this point on the frontier. The red piece of Bin 5 in Panel 1 refers to the next observation to be removed, in this case because this bin has the largest difference between control and treateds, among those with more controls than treateds (i.e., Equation 8). Panels 2-4 repeat the same process as in Panel 1, until we get to Panel 4 where no additional progress can be made and the frontier is complete.

## 6 Applications

We now apply our approach in two important applications. The first uses a SATT frontier, so a direct comparison can be made between experiment and observational data sets. The second uses a FSATT frontier in a purely observational study, and thus we focus on understanding the new causal quantity being estimated.

### 6.1 Job Training

We begin with a data set compiled from the National Supported Work Demonstration (NSWD) and the Current Population Survey (Lalonde, 1986; Dehejia and Wahba, 2002). The first was an experimental intervention, while the second is a large observational data collection appended to the 185 experimental treated units and used in the methodological literature as a mock control group to test a method’s ability to recover the experimental effect from observational data. Although our purpose is only to illustrate the use of the matching frontier, we use the data in the same way.

As is standard in the use of these data, we match on age, education, race (black or Hispanic), marital status, whether or not the subject has a college degree, earnings in 1974, earnings in 1975, and indicators for whether or not the subject was unemployed in 1974 and 1975. Earnings in 1978 is the outcome variable. To ensure a direct comparison, we estimate the SATT fixed ratio frontier, and thus prune only control units. We give the full matching frontier in the top-left panel of Figure 3, and, in the two lower panels, the effect estimates for every point on the frontier (the lower-left panel displays estimates over the full frontier and the lower-right panel zooms in on the final portion of the frontier).



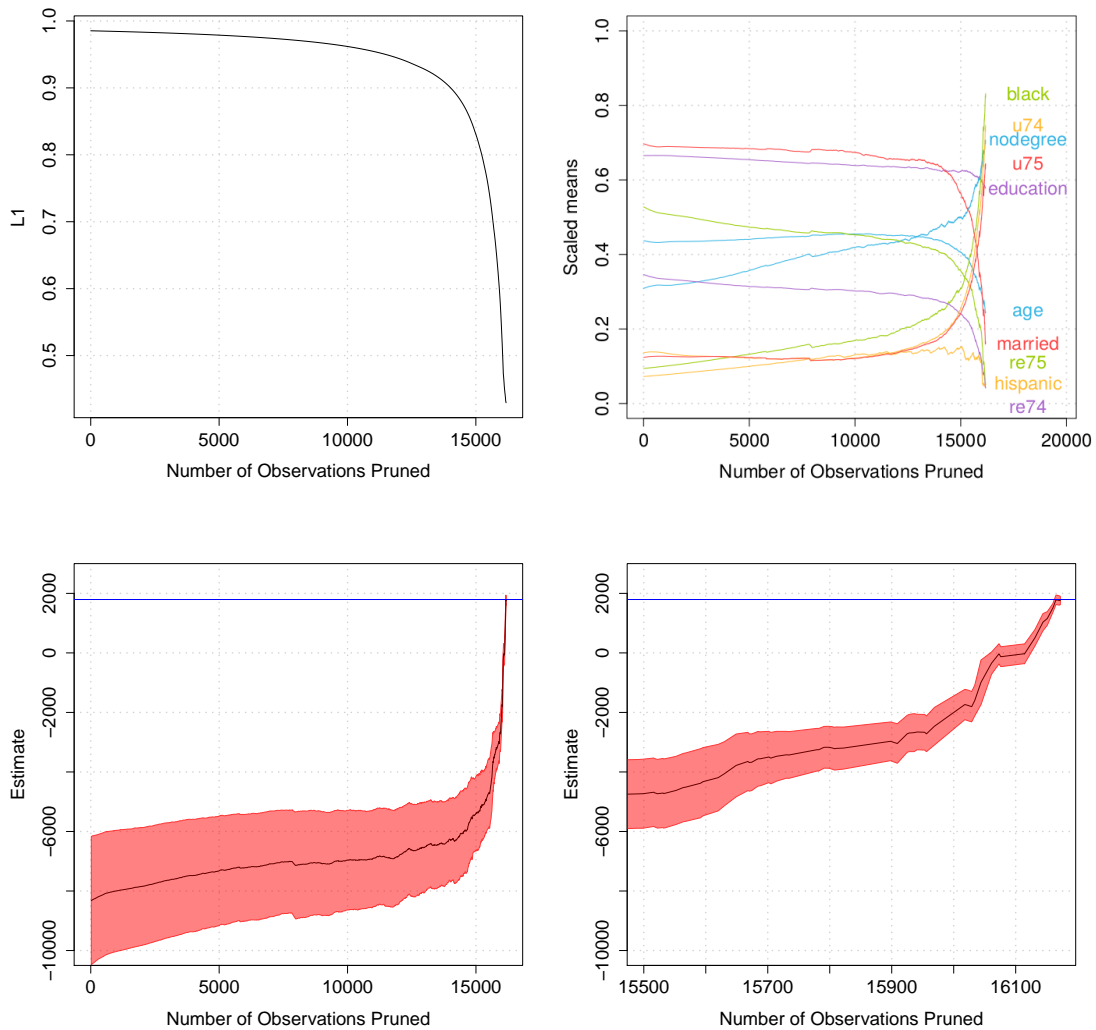


Figure 3: The top-left panel is the  $L_1$  frontier for the job training data, beginning at the left before pruning with  $n = 16,252$  (nonexperimental) control units. The top-right panel displays covariate means across the frontier. The bottom-left panel displays causal effect estimates along that frontier, with Athey-Imbens model dependence intervals calculated at each point. The bottom-right panel displays the final points on the frontier, effectively “zooming” in on the portion where imbalance improves rapidly. The horizontal blue line is the estimated treatment effect from the experimental sample.

There is no risk of selection bias in choosing a point on the frontier based on the estimates, so long as the entire set is presented, as we do here.

These data include 185 treated (experiment) units and 16,252 control units and so pruning even the vast majority of the control units will not reduce the variance much and could help reduce imbalance (see Section 4.2). In the present data, this insight is especially important given that the largest trade-off between pruning and imbalance occurs

after most of the observations are pruned; this can be seen at the right of the left panel of Figure 3, where the frontier drops precipitously.

The right panel gives the causal effect estimates, where the apparent advantages of pruning most of the cases can be seen. Over most of the range of the frontier, causal estimates from the (badly matched) data do not move much from the original unmatched estimate,  $-\$8,334$ , which would indicate that this job training program was a disaster. Then after the fast drop in imbalance (after pruning about 15,000 mostly irrelevant observations), the estimates rise fast, and ultimately intersect with the experimental estimate of the training program producing a benefit of  $\$1,794$  per trainee (denoted by the red horizontal line). Overall, the frontier reveals the whole range of possible conclusions as a function of the full bias-variance trade off. The right panel also gives Athey-Imbens model dependence intervals (Athey and Imbens, 2015) around the point estimates<sup>5</sup>; the width of these are controlled by the model dependence remaining in the data and so decrease as balance improves across the frontier. Correspondingly, the largest change in model dependence occurs near the end of the frontier, where imbalance improves the most.

## 6.2 Sex and Judging

For our second example, we replicate Boyd, Epstein and Martin (2010), who offer a rigorous analysis of the effect of sex on judicial decision making. They first review the large number of theoretical and empirical articles addressed to this question and write that “roughly one-third purport to demonstrate clear panel or individual effects, a third report mixed results, and the final third find no sex-based differences whatsoever.” These prior articles all use similar parametric regression models (usually logit or probit) and related data sets. To tame this remarkable level of model dependence, they introduce nearest neighbor propensity score matching to this literature, and find no effects of sex on judging in every one of 13 policy areas except for sex discrimination, which makes good sense substantively. The authors also offer a spirited argument for bringing “promising devel-

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<sup>5</sup>Athey and Imbens (2015) propose “a scalar measure of the sensitivity of the estimates to a range of alternative models.” To compute this measure, investigators estimate the quantity of interest with a base model, after which the quantity of interest is estimated in subsamples divided according to covariate values. The deviation of these subsample estimates from the base estimate is then a measure of model dependence.

opments in the statistical sciences” to important substantive questions in judicial politics, and so we follow their lead here too. We thus follow their inclinations but with methods developed after the publication of their article, by seeing whether our more powerful approach can detect results not previously possible.

Boyd, Epstein and Martin (2010) motivate their study by clarifying four different mechanisms by which sex might influence judicial outcomes, each with distinct empirical implications. First, *different voice*, in which “males and females develop distinct worldviews and see themselves as differentially connected to society.” This account suggests that males and females should rule differently across a broad range of issues, even those with no clear connection to sex. Second, *representational*, which posits that “female judges serve as representatives of their class and so work toward its protection in litigation of direct interest.” This theory predicts that males and females judge differently on issues of immediate concern to women. Third, *informational*, which argues that “women possess unique and valuable information emanating from shared professional experiences.” Here, women judge differently on the basis of their unique information and experience, and so might differ from men on issues over which they have distinct experiences, even if not related to sex. Finally, *organizational*, in which “Male and female judges undergo identical professional training, obtain their jobs through the same procedures, and confront similar constraints once on the bench.” This theory predicts that men and women do not judge differentially.

Boyd, Epstein and Martin (2010) argue that their results — that male and females judge differently *only* in sex discrimination cases — are “consistent with an information account of gendered judging.” Of course, their results are also consistent with representational theories. Indeed, as Boyd, Epstein and Martin (2010) argue, women might judge differently on sex discrimination because they have different information as a result of shared experiences with discrimination. But it is also possible that women judge differently on sex discrimination as a way to protect other women, consistent with representational accounts.

One way to use our new methodological approach is to attempt to distinguish between

these conflicting interpretations of the effects of sex. To do so, we analyze cases on an issue in which we expect to observe a difference if and only if the *informational* account is true. That is, an issue area where the unique experiences of women might lead to informational differences between men and women but that nonetheless does not directly concern the interests of women. For this analysis, we consider cases related to discrimination on the basis of race. Because women have shared experiences with discrimination, they have informational differences from men relevant to this issue area. However, judgements on racial discrimination do not have direct consequences for women more broadly. This issue area is the only such issue area that allows us to distinguish between these two accounts and for which we also have a suitable amount of available data.

Thus, using their data, we reanalyze race discrimination cases made on the basis of Title VII. In their original analysis, [Boyd, Epstein and Martin \(2010\)](#) found a null effect with this issue area, both before and after matching. We now show, with our method which enables us to analyze data with less dependence and bias than previous matching approaches, that female judges rule differently on race discrimination cases. We show that differences in male and female judgements are at least in part due to informational differences.

We arranged the data from [Boyd, Epstein and Martin \(2010\)](#) so that the unit of analysis is the appellate court case, the level at which decisions are made. For example, the fourth observation in our data set is *Swinton v. Potomac Corporation*, which was decided in 2001 by the Ninth Circuit Court of Appeals and at the time was composed of Judges William A. Fletcher, Johnnie B. Rawlinson, and Margaret M. McKeown. Our treatment is whether or not at least one female judge was included in the three-judge panel. In *Swinton v. Potomac Corporation*, Judges Rawlinson and McKeown are female and so this observation is in the treatment group. For each appellate court case, we use the following covariates: (1) median ideology as measured by Judicial Common Space scores ([Epstein et al., 2007](#); [Giles, Hettinger and Peppers, 2001](#)), (2) median age, (3) an indicator for at least one racial minority among the three judges, (4) an indicator for ideological direction of the lower court's decision, (5) an indicator for whether a majority of the judges on the three-

judge panel were nominated by Republicans, and (6) an indicator for whether a majority of the judges on the panel had judicial experience prior to their nomination. In *Swinton v. Potomac Corporation*, for example, the median ideology was at the 20<sup>th</sup> percentile in the distribution of judges who ruled on a Title VII-race discrimination case and in the 10<sup>th</sup> percentile in the distribution of median ideologies, where lower scores are more liberal and higher scores are more conservative. This is unsurprising, as all three judges were nominated by President Clinton in either 1998 or 2000. Our outcome is the ideological direction of the decision — either liberal or conservative — and unsurprisingly, the ruling on *Swinton v. Potomac Corporation* was a liberal one. For our analysis, we use these six covariates to construct a Mahalanobis frontier for the estimation of FSATT.

We present the matching frontier in Figure 4, panel (a); in contrast to our previous example, most of the reduction in imbalance for observations pruned happens early on, but substantial imbalance reduction continues through the entire range. Our substantive results can be found in panel (b), which indicates that having a female judge increases the probability of a liberal decision, over the entire range. The vertical axis quantifies the substantial effect we see in terms of the reduction in probability, from about 0.05 with few observations pruned and higher levels of model dependence to 0.25 with many pruned and lower levels. Most importantly in this case, the point estimate of the causal effect increases as balance improves and we zero in on a data subset more closely approximating a randomized experiment. Correspondingly, model dependence, measured with red intervals around each line, decreases.

Because we pruned both treated and control units in this example, we must carefully consider how the quantity of interest changes as balance improves. Interestingly, the best balance exists within more conservative courts (and thus the effect we find in Figure 4 is among these courts). To see this, note that in Figure 5, which plots the means of each covariate as observations are pruned, variables associated with the court ideology changed the most, and all moved in a conservative direction.

More specifically, *Republican majority* has the largest difference in means, followed by *median ideology* and *median age*. That makes sense, as we expect all-male panels

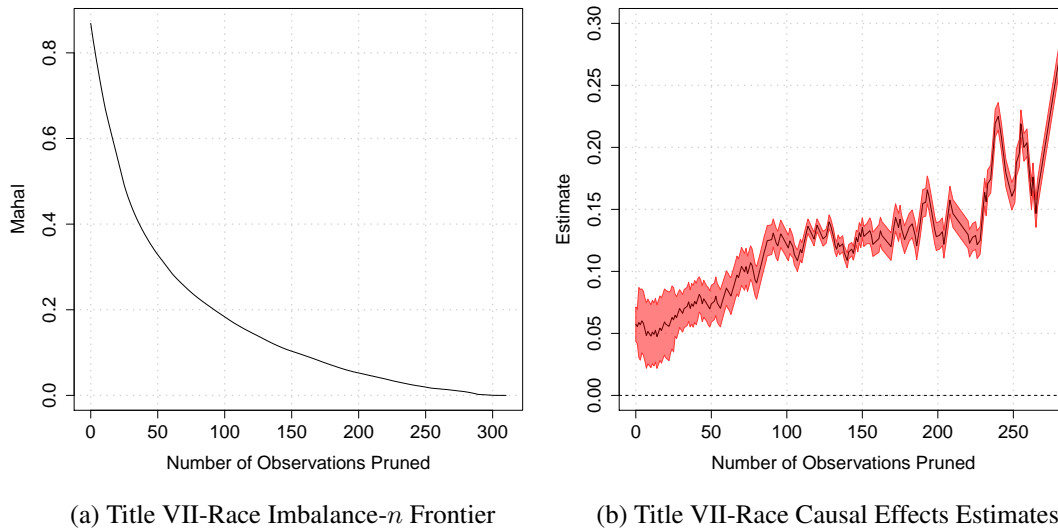


Figure 4: The Effect of Sex on Judges in Title VII-Race Discrimination Cases. Panel (a) displays the imbalance- $n$  frontier and panel (b) shows estimates of the causal effect across that same frontier. The region shaded in red is the Athey-Imbens model dependence interval.

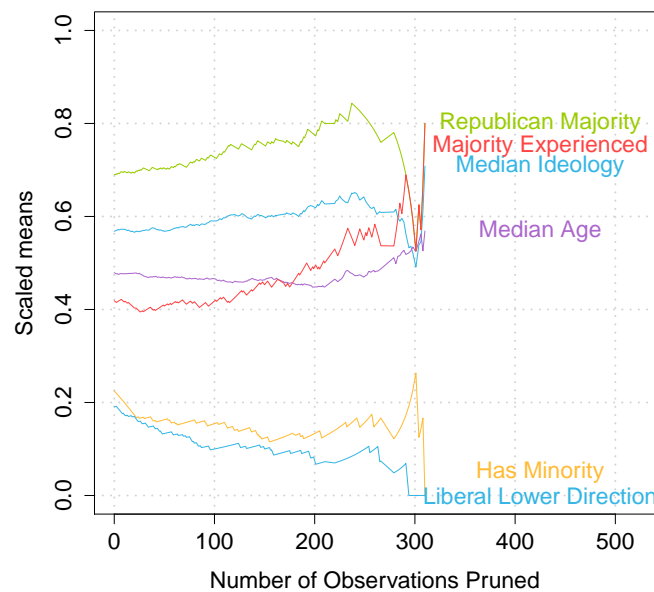


Figure 5: Covariate Means Across the Title VII-Race Discrimination Frontier. Displays the means of each covariate as observations are pruned and balance improves. Note the large change in variables measuring the ideology of the court.

(those assigned to control) to be more conservative on average, if only because Democratic presidents are more likely to appoint women than Republicans. In our data, 7% of judges appointed by Republicans are women, compared with 25% of judges appointed by Democrats. The key issue is that liberal courts with no female judges is rare and so the data do not admit reasonable inferences among this subset of data (for instance, 31% of courts with a Republican majority have at least one woman compared with 50% of courts without a Republican majority).

Our matching technique thus successfully identifies a subset of data with balance, and this subset is where women are assigned to conservative courts as well as a suitable comparison group of conservative courts without a woman. The causal effect of sex on judging in conservative courts is not the only question of interest in the literature, but it is certainly one question of interest, and, as it happens, it is one in these data that offer a reasonably secure answer. Thus, for example, after pruning 210 appellate court-cases, the difference in the median Judicial Common Space score for treated and control units is 0.02, compared to 0.1 in the full data. However, interestingly, the average median score for controls is essentially constant as we prune observations, changing from 0.123 to 0.163. In contrast, the average median score for treated units (courts with at least one woman) goes from 0.02 to 0.143, which is to say it rises to the level of conservatism displayed by courts assigned to the control group.

These results suggest an especially interesting conclusion to supplement the original findings in [Boyd, Epstein and Martin \(2010\)](#), namely that the mechanism for differences in judicial decision making is at least in part informational, perhaps in addition to being representational, (even) among the most conservative courts.

## **7 Concluding Remarks**

Matching methods are on the one hand simple and easy to understand and on the other provide a powerful nonparametric way to control for measured covariates. However, in practice, implementing matching methods can be frustrating because they optimize with respect to one imbalance metric or the matched sample size and then need to be evaluated with respect to another measure or sample size. The matching frontier we describe here

offers the first simultaneous optimization of both criteria while retaining much of the simplicity that made matching attractive in the first place.

With the approach offered here, once a researcher chooses an imbalance metric and set of covariates, all analysis is automatic. This is in clear distinction to the best practices recommendations for prior matching methods. However, although the choice of a particular imbalance metric does not usually matter that much, the methods offered here do not free one from the still crucial task of choosing the right set of pre-treatment control variables, coding them appropriately so that they measure what is necessary to achieve ignorability or, in the case of FSATT, from understanding and conveying clearly to readers the quantity being estimated.

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