Covariate Selection for Generalizing Experimental Results∗

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Abstract

Scientists are interested in generalizing causal effects estimated in an experiment to a target population. However, analysts are often constrained by available covariate information, which has limited applicability of existing approaches that assume rich covariate data from both experimental and population samples. As a concrete context, we focus on a large-scale development program, called the Youth Opportunities Program (YOP), in Uganda. Although more than 40 pre-treatment covariates are available in the experiment, only 8 of them were also measured in a target population. To tackle this common issue of data constraints, we propose a method to estimate a separating set – a set of variables affecting both the sampling mechanism and treatment effect heterogeneity – and show that the population average treatment effect (PATE) can be identified by adjusting for estimated separating sets. Our approach has two advantages. First, our algorithm only requires a rich set of covariates in the experimental data, not in the target population. Second, the algorithm can estimate separating sets under researcher-specific constraints on what variables are measured in the population. Using the YOP experiment, we find that the proposed algorithm can allow for estimation of the PATE in situations where conventional methods fail due to data requirements.

Keywords: Causal inference, External validity, Generalization, Randomized experiments

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1 Introduction

Over the last few decades, social and biomedical scientists have developed and applied an array of statistical tools to make valid causal inference (Imbens and Rubin, 2015). In particular, randomized experiments have become the mainstay for estimating causal effects. Although many scholars agree upon the high internal validity of experimental results, there is a debate about how scientists should infer the impact of policies and interventions on broader populations (Imai et al., 2008; Angrist and Pischke, 2010; Bareinboim and Pearl, 2016; Deaton and Cartwright, 2018). This issue of generalizability is pervasive in practice because randomized controlled trials are often conducted on non-representative samples (Cook et al., 2002; Druckman et al., 2011; Allcott, 2015; Stuart et al., 2015).

Our motivating application is the Youth Opportunities Program (YOP) in Uganda, which aims to help the poor and unemployed become self-employed artisans and increase incomes. This large scale development program, involving more than 10,000 individuals from 454 communities, was designed, implemented and evaluated by the government of Uganda and the authors of Blattman et al. (2013) from 2008 to 2012. Young adults in Northern Uganda were invited to form groups and submit grant proposals for vocational training and to start independent trades. To evaluate the causal impact of the program, funding was randomly assigned among 535 eligible groups and a host of economic variables (e.g., employment and income) were measured.

The question of generalizability is of great importance in this application. The aim of such development programs is elegantly noted in Duflo and Kremer (2005), “the benefits of knowing which programs work and which do not extend far beyond any program or agency, and credible impact evaluations are global public goods in the sense that they can offer reliable guidance to international organizations, governments, donors, and nongovernmental organizations (NGOs) beyond national borders.” Researchers and policy makers are not just concerned to learn about the very individuals who participated in the trial. The ultimate goal is to learn whether and how much the program can improve economic conditions in a larger target population — more than 5 million people in Northern Uganda (Blattman et al., 2013).

Despite its importance, estimating population average treatment effects is not straightforward because we have to adjust for differences between experimental samples and the target population. One pervasive question is what covariates should and can we adjust for? Although previous research shows that adjusting for a set of variables explaining sampling mechanism or treatment heterogeneity is sufficient for generalization (Stuart et al., 2011; Bareinboim et al., 2014), researchers are often constrained by available covariate information in applied settings.

In this paper, we address this problem of covariate selection for estimating population average treatment effects. In particular, we develop a data-driven method to estimate a separating set — a set of variables affecting both sampling mechanism and treatment effect heterogeneity. Recent papers show that the population average treatment effect can be identified by adjusting for this separating set (Tipton, 2013; Pearl, 2015; Kern et al., 2016). In Section 3 we extend this result and show that
the separating set relaxes data requirements of conventional methods by generalizing
two widely-used covariate selection approaches: (1) a sampling set – a set of variables
explaining how units are sampled into a given experiment (Cole and Stuart 2010; Pressler and Kaizar 2013; Hartman et al. 2015; Buchanan et al. 2018) and (2) a
heterogeneity set: a set of variables explaining treatment effect heterogeneity (Kern et al. 2016; Nguyen et al. 2017).

In Section 4 we demonstrate that such separating sets are estimable from the experi-
mental data alone and then provide a new estimation algorithm based on Markov
random fields. This algorithm only requires that a sampling set be observed in the
experimental sample, not in the target population. We estimate a separating set as a
set that makes a sampling set conditionally independent of observed outcomes in the
experimental data. Therefore, in contrast to conventional methods, we can exploit
all covariates in the experiment to find necessary separating sets, even when there
are few variables measured in both the experimental and population data.

Our approach has two advantages. First, unlike existing approaches requiring
rich covariate information in both the experimental sample and the non-experimental
target population, we only require rich covariate data in the experimental sample.
This has practical implications because researchers often have more control over what
to measure on their experimental subjects, even when it is difficult to collect detailed
information about their target population. For example, the experimental data of
Blattman et al. (2013) contains about 40 pre-treatment covariates, even though only
8 of them are also measured in the target population. Second, we can incorporate
user-constraints on what variables can feasibly be collected in the target population.
If there are characteristics that cannot be measured in the target population, the
algorithm will identify a separating set subject to these constraints.

Our article builds on a growing literature on the population average treatment
effect, which has two general directions. First, many previous studies have focused
on articulating identification assumptions and proposing consistent estimators of the
population average treatment effect (e.g., Cole and Stuart 2010; Stuart et al. 2011;
Hartman et al. 2015; Buchanan et al. 2018). In particular, Tipton (2013) and Kern
et al. (2016) explicitly show that researchers have to jointly consider treatment effect
heterogeneity and the sampling mechanism. These existing approaches often assume
researchers have access to a large number of covariates in both the experimental
sample and the non-experimental target population. In contrast, we provide a new
data-driven covariate selection algorithm to find separating sets in situations where
researchers have data constraints in the target population. Our focus on covariate
selection is similar to recent influential work on causal directed acyclic graphs (causal
DAGs) (Bareinboim et al. 2014; Pearl 2015; Bareinboim and Pearl 2016). We differ
from the DAG-based approaches in that we empirically estimate separating sets under
assumptions about sampling and heterogeneity sets rather than analytically selecting
separating sets from fully specified causal DAGs. Although assumptions about the
entire causal DAGs are sufficient for covariate selection, the proposed algorithm can
estimate separating sets under weaker assumptions about sampling and heterogeneity
sets at the expense of statistical uncertainties.
Research in the second direction argues that the necessary assumptions for existing methods are often too strong in practice. Recent papers have explored methods for sensitivity analyses (Nguyen et al. 2017; Andrews and Oster 2017) and bounds (Chan 2017) to achieve partial identification under weaker assumptions. Our paper is complementary to these approaches. We instead focus on the point identification of the population average treatment effect and alleviate strong assumptions about data requirements by adding an additional step of estimating a separating set.

2 Youth Opportunities Program in Uganda

As well documented by the World Bank, a large number of young adults in developing countries are unemployed or underemployed (World Bank 2012). In addition to its direct implication to poverty, concerns for policy makers are that such large young and unemployed populations can increase risk of crime and social unrest (Blattman et al. 2013). Uganda, especially conflict-affected Northern Uganda, is not an exception. According to estimates from the government, two-thirds of northern Ugandans could not meet basic needs, about 50% were illiterate, and most were underemployed in subsistence agriculture in 2006 (Government of Uganda 2007).

In this paper, we study the Youth Opportunities Program (YOP) in Uganda, designed to help the poor and unemployed become self-employed artisans and increase incomes. This intervention is one example of widely used cash transfer programs in which participants are offered a certain amount of cash in the hope that they invest in training and start new, profitable enterprises. In 2008, the government invited young adults in Northern Uganda to form groups and submit grant proposals for how they would use a grant for vocational training and business start-up. Then, funding was randomly assigned among 535 screened, eligible applicant groups — 265 and 270 groups to treatment and control, respectively. Treatment groups received a one-time unsupervised grant worth $7,500 on average — about $382 per group member, roughly their average annual income. Following the original analysis, we focus on a binary treatment, whether they receive any grants or not through the YOP.

To evaluate the impact of this intervention, Blattman et al. (2013) surveyed 5 people per group three times over four years, resulting in a panel of 2,598 individuals after removing 79 observations due to missing data. They measured 17 outcome variables across five dimensions — employment (7), income (2), investments (3), business formality (3), and urbanization (2). They find that the effects of the YOP are large across all dimensions. Notably, after two years, the treatment groups were 4.5 times more likely to have vocational training, 2.5 times more likely to engage with a skilled trade and had 16% more hours of employment and 42% higher earnings.

Although it is unambiguous that the YOP had large, persistent positive effects on experimental subjects, it is of great policy interest to empirically investigate how much these experimental estimates are generalizable to a larger population. Estimating population average treatment effects can inform which specific development policies governments should scale up. While the focus of the program was on Northern Uganda as a whole, participants of the YOP were inevitably not representative, as in many other development programs. To take into account differences between experimental
| Employment                                      | Control mean | PATE estimate | Investments                                      | Control mean | PATE estimate |
|------------------------------------------------|--------------|---------------|-------------------------------------------------|--------------|---------------|
| Average employment hours                       | 24.95        | 5.16          | Vocational training                             | 0.15         | 0.60          |
| (3.02)                                         |              |               | Hours of vocational training                    | 44.91        | 260.85        |
| Agricultural                                   | 14.04        | -1.17         | Business assets                                 | 289.16       | 301.28        |
| (1.87)                                         |              |               | (65.13)                                        |              |               |
| Nonagricultural                                | 10.91        | 6.33          |                                                 |              |               |
| (2.42)                                         |              |               |                                                 |              |               |
| Skilled trades only                            | 2.94         | 3.96          |                                                 |              |               |
| (1.85)                                         |              |               |                                                 |              |               |
| No employment hours                            | 0.10         | 0.02          | Maintain records                                | 0.30         | 0.18          |
| (0.04)                                         |              |               | (0.06)                                         |              |               |
| Any skilled trade                              | 0.18         | 0.26          | Registered                                      | 0.15         | 0.09          |
| (0.06)                                         |              |               | (0.06)                                         |              |               |
| Works mostly in a skilled trade                | 0.04         | 0.04          | Pays taxes                                      | 0.21         | 0.08          |
| (0.04)                                         |              |               | (0.05)                                         |              |               |
| Income                                         |              |               |                                                 |              |               |
| Cash earnings                                  | 33.86        | 16.88         | Changed parish                                  | 0.31         | 0.01          |
| (8.63)                                         |              |               | (0.06)                                         |              |               |
| Durable assets                                 | -0.09        | 0.16          | Lives in Urban area                             | 0.18         | 0.03          |
| (0.13)                                         |              |               | (0.07)                                         |              |               |

Table 1: Estimates of Population Average Treatment Effects based on the Original Eight Variables. *Note:* We estimated population average treatment effects of the above 17 outcomes using an inverse probability weighting estimator with standard errors clustered by group. Weights are estimated by a logistic regression including the eight variables additively. See details of the estimation in Section [6](#). As reference, means of control groups’ outcomes are computed based on experimental samples.

samples and Northern Uganda’s population, [Blattman et al. (2013)](#) merged their experimental samples with a 2008 population-based household survey, the Northern Uganda Survey (NUS). They adjusted for eight variables shared by experimental and population data: gender, age, urban status, marital status, school attainment, household size, durable assets, and district indicators. In Table [1](#) we report estimates based on an inverse probability weighting (IPW) estimator [Stuart et al. (2011)](#) that adjusts for the original eight variables. Although the original authors rely on weighted linear regression models in their paper, we focus on the IPW estimator widely studied in the literature of generalization (e.g., [Buchanan et al. (2018)](#)).

In this paper, we focus on a pervasive methodological challenge of covariate selection in generalizing experimental results. Although it is common to adjust for all observed covariates shared by experimental and population data, it is unclear whether such sets of covariates include all necessary covariates for generalization. In fact, the authors carefully pay attention to this point in the original paper; “young adults are selected into our sample because of unobserved initiative, connections or affinity for entrepreneurship” [Blattman et al. (2013)](#). It is also possible that the original analysis adjusted for unnecessary variables, resulting in inefficient estimators of population average treatment effects. We investigate necessary and sufficient sets of covariates for generalizing experimental estimates, called separating sets, and then provide a new
algorithm to empirically estimate such sets. We select the separating sets under several different assumptions and assess how estimates of population average treatment effects vary. Our reanalysis of this experiment appears in Section 6.

3 Separating Sets For Generalization

This section sets up the potential outcomes framework for studying population average treatment effects. We review a definition of a separating set — a set of variables affecting both the sampling mechanism and treatment effect heterogeneity, and then show that a sampling set and a heterogeneity set, the main focus of existing approaches, are special cases of the separating sets.

3.1 The Setup

We consider a scenario in which we have two data sets. Following Buchanan et al. (2018), we define the first sample of $n$ individuals to be participants in a randomized experiment (“Experimental Data”) and the second data set to be a random sample of $m$ individuals from the target population (“Population Data”). In our application, the experimental data has 2,598 individuals and the population data contains 21,348 individuals. We define a sampling indicator $S_i$ taking 1 if unit $i$ is in the experiment and 0 if unit $i$ is in the target population. We assume that every unit has non-zero probability of being in the experiment. Although experimental units can be randomly sampled from the target population in ideal settings, they often select into the experiment, as in the YOP, making the experimental sample non-representative. Note that we consider cases in which the experimental sample and the target population don’t overlap, but similar results hold for cases in which the experimental sample is a subset of the target population.

Let $T_i$ be a binary treatment assignment variable for unit $i$ with $T_i = 1$ for treatment and 0 for control. We define $Y_i(t)$ to be the potential outcome variable of unit $i$ if the unit were to receive the treatment $t$ for $t \in \{0, 1\}$. In this paper, we make a stability assumption, which states that there is neither interference between units nor different versions of the treatment, either across units or settings (Rubin, 1990; Tipton, 2013; Hartman et al., 2015). We define pre-treatment covariates $X_i$ to be any variables not affected by the treatment variable.

We are interested in estimating the average treatment effect in the target population. We call this causal estimand the population average treatment effect (PATE) and define it formally as follows.

\textbf{Definition 1 (Population Average Treatment Effect)}

$$\tau \equiv \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0].$$

The treatment assignment mechanism is controlled by researchers within the experiment ($S_i = 1$), but it is unknown for units in the target population ($S_i = 0$). Formally, we assume that the treatment assignment is randomized within the experiment.

\textbf{Assumption 1 (Randomization in Experiment)}

$$\{Y_i(1), Y_i(0), X_i\} \perp\!\!\!\!\perp T_i \mid S_i = 1.$$
For each unit in the experimental condition, only one of the potential outcome variables can be observed, and the realized outcome variable for unit \( i \) is denoted by \( Y_i = T_i Y_i(1) + (1 - T_i) Y_i(0) \).

### 3.2 Definition of Separating Sets

[Tipton (2013)] and [Kern et al. (2016)] show that the PATE can be identified by a set of variables affecting both treatment effect heterogeneity and the sampling mechanism (sample ignorability for treatment effects). In this paper, we refer to this set as a *separating set* and investigate its statistical properties. Formally, a separating set is any set that makes the sampling indicator and treatment effect heterogeneity conditionally independent.

**Definition 2 (Separating Set)**

A separating set is a set \( W \) that makes the sampling indicator and treatment effect heterogeneity conditionally independent.

\[
Y_i(1) - Y_i(0) \perp \! \perp S_i \mid W_i. \tag{1}
\]

This definition of a separating set contains two simple cases: (1) when no treatment effect heterogeneity exists and (2) when the experimental sample is randomly drawn from the target population. In both of these cases, \( W_i = \emptyset \). This separating set also encompasses two common approaches in the literature as special cases. First, researchers often employ statistical methods based on a *sampling set* – a set of all variables affecting the sampling mechanism (e.g., [Stuart et al., 2011]). Second, researchers might adjust for a *heterogeneity set* – a set of all variables governing treatment effect heterogeneity (e.g., [Kern et al., 2016]). Below, we formalize these sets based on the potential outcomes framework.

We define a *sampling set* as a set of variables that determines the sampling mechanism by which individuals come to be in the experimental sample. For example, when a researcher implements stratified sampling based on gender and age, the sampling set consists of those two variables. When researchers control the sampling mechanism, a sampling set is known by design. However, when samples are selected without such an explicit sampling design, a sampling set is unknown and in practice, researchers must posit a sampling mechanism. For example, [Blattman et al. (2013)] assume that a sampling set consists of eight variables: gender, age, urban status, marital status, school attainment, household size, durable assets, and district indicators. Formally, we can define a sampling set \( X^S \) as follows.

\[
\{Y_i(1), Y_i(0), X_i^{-S}\} \perp \! \perp S_i \mid X_i^S \tag{2}
\]

where \( X^{-S} \) is a set of pre-treatment variables that are not in \( X^S \). This conditional independence means that the sampling set is a set that sufficiently explains the sampling mechanism. Given the sampling set, the sampling indicator is independent of the joint distribution of potential outcomes and all other pre-treatment covariates. We refer to variables in the sampling set as sampling variables.

The other popular approach is to adjust for a set of all variables explaining treatment effect heterogeneity, which we call a *heterogeneity set*. Formally, we can define a heterogeneity set \( X^H \) as follows.
\[ Y_i(1) - Y_i(0) \perp \{S_i, X_i^{-H}\} \mid X_i^H, \]  

where \( X^{-H} \) is a set of pre-treatment variables that are not in \( X^H \). In this case, because a heterogeneity set fully accounts for treatment heterogeneity, \( Y_i(1) - Y_i(0) \) is independent of all other variables. We refer to variables in the heterogeneity set as heterogeneity variables. In our application, Blattman et al. (2013) discuss at least two heterogeneity variables, gender and initial credit constraints.

We want to emphasize that a sampling set and a heterogeneity set are special cases of a separating set in the sense that both sets satisfy Equation (1). Yet, there may exist many other separating sets, which we explore in Section 4.

Although often implicit in many empirical studies, it is critical to be explicit about what variables are measured in the experimental sample and the population data. We distinguish between different research scenarios, or “settings,” researchers may find themselves in depending on the available covariate information.

We begin with the most demanding scenario – when a separating set is known and measured in both the experimental sample and the population data. If this is the case, the PATE is nonparametrically identified.

**Setting 1 (A Separating Set is Observed in Experiment and in Target Population)**

A separating set \( W_i \) is observed in both the experimental sample \((S_i = 1)\) and the target population \((S_i = 0)\).

**Result 1 (Identification of the PATE (Bareinboim et al., 2014))** In Setting 1, the PATE is identified with separating set \( W_i \) under Assumption 1:

\[ \tau = \int \left\{ \mathbb{E}[Y_i \mid T_i = 1, S_i = 1, W_i = w] - \mathbb{E}[Y_i \mid T_i = 0, S_i = 1, W_i = w] \right\} \, dF_{W_i|S_i=0}(w), \]

where \( F_{W_i|S_i=0}(w) \) is the cumulative distribution function of \( W \) conditional on \( S_i = 0 \).

As sampling and heterogeneity sets are special cases of a separating set, the PATE is also identified under the same set of assumptions, when a sampling set \( X_i^S \) or a heterogeneity set \( X_i^H \) is observed in both the experimental sample and the target population.

## 4 Identification and Estimation of Separating Sets

The PATE is identified when we measure a separating set in both the experimental sample and target population. An advantage over existing approaches based only on sampling and heterogeneity sets is that there may exist, potentially many, other separating sets and hence, researchers can choose a set subject to their data constraints. For example, researchers might want to measure as few variables as possible in the target population. Or researchers might already know they cannot measure certain covaraites, e.g., social connections and initial motivation for entrepreneurship, in the target population of Blattman et al. (2013).

In this section, we propose a novel data-driven method to select separating sets. In Section 4.1, we demonstrate that separating sets are estimable from the experimental data alone. Then, in Section 4.2, we propose an algorithm to estimate separating sets using Markov random fields.
4.1 Identification of Separating Sets

First, we show that a separating set is estimable from the experimental data. In settings where both a sampling set and a heterogeneity set are observed in the experimental data, we can estimate an exact separating set. A key advantage of this result is that we only require the experimental data, not the target population data, to discover separating sets, should they exist.

In many applied research contexts, however, the heterogeneity set is not readily available even in the experimental data because it is inherently unobservable. The fundamental problem of causal inference (Holland, 1986) states that only one of two potential outcomes are observable, which implies that the causal effect is unobserved at unit level and thus so is the heterogeneity set. For example, in our application, although Blattman et al. (2013) discuss two specific heterogeneity variables (gender and initial credit constraints), it might be unreasonable to assume away the existence of other potential heterogeneity variables.

We therefore develop an additional method to find a variant of a separating set, which we call a marginal separating set, using only knowledge of a sampling set. We show that a marginal separating set can also be discovered using only the experimental data, and it is sufficient for identifying the PATE. Importantly, this approach requires measuring the sampling set in the experimental data, but not in the target population. Although this data requirement might still be stringent in some contexts, it is much weaker than the one necessary for widely-used existing approaches.

4.1.1 Identification of Exact Separating Sets

We begin with settings in which a sampling set and a heterogeneity set are observed in the experimental sample. In this setting, we can use the experimental data to identify exact separating sets. Although this data requirement is still restrictive, we emphasize that it does not require rich data on the target population.

Setting 2 (Sampling and Heterogeneity Sets are Observed in Experiment)

Sampling set $X^S$ and heterogeneity set $X^H$ are observed in the experiment ($S_i = 1$).

In this setting, a separating set is estimable as a set that makes the sampling set and the heterogeneity set conditionally independent within the experimental data.

Theorem 1 (Identification of Separating Sets in Experiment)
In Setting 2, for a set of pre-treatment variables $W$, under Assumption 1

\[
\tilde{X}_i^H \perp \perp \tilde{X}_i^S \mid W_i, T_i, S_i = 1 \implies Y_i(1) - Y_i(0) \perp \perp S_i \mid W_i,
\]

where $\tilde{X}^H$ and $\tilde{X}^S$ are the set difference $X^H \setminus W$ and $X^S \setminus W$, respectively.

We provide the proof in Appendix A.1. Theorem 1 states that as long as we can find a set that satisfies the testable conditional independence on the left hand side, the discovered set is guaranteed to be a separating set. That is, we can identify an exact
separating set from the experimental data alone. Note that when $X^H$ and $X^S$ share some variables, those variables should always be in $W$. Using the selected separating set, researchers can identify the PATE based on Result [1].

An intuition behind this theorem has straightforward two steps. First, because a heterogeneity set $X^H$ fully explains treatment effect heterogeneity $Y(1) - Y(0)$, $S$ and $Y(1) - Y(0)$ are conditionally dependent only when $S$ and $X^H$ are conditionally dependent. In addition, because a sampling set $X^S$ fully explains the sampling indicator $S$, $S$ and $X^H$ are conditionally dependent only when $X^S$ and $X^H$ are conditionally dependent. Taken together, $S$ and $Y(1) - Y(0)$ are conditionally dependent only when $X^S$ and $X^H$ are conditionally dependent.

4.1.2 Identification of Marginal Separating Sets

While Theorem [1] allows us to discover separating sets using the experimental data, a key challenge would be to measure both a sampling set and a heterogeneity set in the experimental data. In particular, it is often difficult to measure the heterogeneity set in practice because it is inherently unobservable. We show that a modified version of a separating set – a marginal separating set – is estimable from the experimental data under a much weaker assumption. We define a marginal separating set as follows.

**Definition 3 (Marginal Separating Set)**

A marginal separating set is set $W$ that makes the sampling indicator and marginal distributions of potential outcomes conditionally independent.

\[ Y_i(t) \perp \perp S_i \mid W_i \quad \text{for} \quad t = \{0, 1\}. \tag{5} \]

We refer to this as a *marginal* separating set since it renders the marginal, not the joint, distribution of potential outcomes conditionally independent of the sampling process.

Now we turn to our final setting researchers may find themselves in – that the sampling set is observed only in the experimental data. Previous work using the sampling set assumes it is measured in both the experimental sample and the target population (e.g., Cole and Stuart, 2010; Tipton, 2013; Hartman et al., 2015; Buchanan et al., 2018). Since researchers often have much more control over what data is collected in the experiment, this final setting greatly relaxes the data requirements of the previous literature.

**Setting 3 (A Sampling Set is Observed in Experiment)**

Sampling set $X^S$ is observed in the experimental data ($S_i = 1$).

**Theorem 2 (Identification of Marginal Separating Sets in Experiment)**

In Setting 3, for a set of pre-treatment variables $W$, under Assumption 1,

\[ Y_i \perp \perp X_i \mid W_i, T_i, S_i = 1 \quad \Rightarrow \quad Y_i(t) \perp \perp S_i \mid W_i. \tag{6} \]

We provide the proof in Appendix A.2. Theorem 2 states that as long as we can find a set that makes the observed outcome $Y$ conditionally independent of the sampling
Table 2: Identifying the PATE under different research settings about data requirements. **Note:** Many previous approaches assume that a sampling set or a heterogeneity set is measured in both the experimental sample and the target population (Setting 1). Our approach (Settings 2 and 3) relaxes data requirements for the target population by introducing an additional step of estimating separating sets.

| Setting          | Data Requirements | Identification |
|------------------|-------------------|----------------|
| **Setting 1**    |                   | Result 1       |
| (Special Case 1.1) | Separating set    |                |
| (Special Case 1.2) | Sampling set      |                |
|                  | Heterogeneity set |                |
| **Setting 2**    |                   | Result 1 and Theorem 1 |
|                  | Sampling Set      | User Specified Constraints |
|                  | Heterogeneity Set | User Specified Constraints |
| **Setting 3**    |                   | Result 2 and Theorem 2 |
|                  | Sampling set      | User Specified Constraints |

Result 2 (Identification of the PATE with Marginal Separating Sets) When a marginal separating set $W$ is observed both in the experimental sample and the target population, the PATE is identified with the marginal separating set $W$ under Assumption 1.

$$
\tau = \int \left\{ \mathbb{E}[Y_i \mid T_i = 1, S_i = 1, W_i = w] - \mathbb{E}[Y_i \mid T_i = 0, S_i = 1, W_i = w] \right\} dF_{W_i|S_i=0}(w).
$$

We omit the proof because it is straightforward from the one of Result 1 (Bareinboim et al., 2014).

In Table 2, we summarize three research settings we have discussed. Previous approaches (Setting 1) assume a separating set is observed both in the experimental sample and the target population. Most common special cases are methods based on a sampling set (Special Case 1.1) or a heterogeneity set (Special Case 1.2). Although the identification of the PATE in this setting is straightforward (Result 1), it requires rich covariate information from the experimental sample and more importantly from the target population. Our approach relaxes data requirements for the target population by introducing an additional step of estimating separating sets. In Setting 2 where we observe both a sampling set and a heterogeneity set in the experimental sample, we can identify exact separating sets from the experimental data alone (Theorem 1). Setting 3 only requires observing a sampling set in the experimental sample and
we can identify marginal separating sets (Theorem 2). For both settings, the next subsection will introduce an algorithm that can estimate separating sets subject to user specified data constraints in the target population.

4.2 Estimation of Separating Sets

Here, we propose an estimation algorithm to find a marginal separating set. As shown in Theorem 2, the goal is to find a set that makes a sampling set and observed outcomes conditionally independent within the experimental data. We show how to apply Markov random fields (MRFs) to encode conditional independence relationships among observed covariates and then select a separating set. A similar algorithm can be used for finding an exact separating set.

Our estimation algorithm consists of four simple steps. We provide a brief summary here and then describe each step in order. Step 1: specify all variables in sampling set $X^S$ based on domain knowledge, some of which might not be measured in the population data. Step 2: using the experimental data alone, estimate a Markov random field over an outcome, a treatment, the sampling set and observed pre-treatment covariates. Step 3: enumerate all simple paths$^1$ from $Y$ to $X^S$ in the estimated Markov graph. Step 4: find sets that block all the simple paths from $Y$ to $X^S$ in the estimated Markov graph.

**Estimating Markov Random Fields** Theorem 2 implies that we can find a marginal separating set by estimating a set of variables $W$ that satisfies the conditional independence, $Y_i \perp \perp X^S_i \mid W_i, T_i, S = 1$. To estimate this set, we employ a Markov random field (MRF). MRFs are statistical models that encode the conditional independence structure over random variables via graph separation rules. For example, suppose there are three random variables $A, B$ and $C$. Then, $A \perp \perp B \mid C$ if there is no path connecting $A$ and $B$ when node $C$ is removed from the graph (i.e., node $C$ separates nodes $A$ and $B$), so-called the global Markov property (Lauritzen, 1996). Using the general theory of MRFs, the estimation of a separating set can be recast as the problem of finding a set of covariates separating outcome variable $Y$ and a sampling set $X^S$ in an estimated Markov graph. Therefore, we can find a separating set that satisfies the desired conditional independence as far as we can estimate the MRF over $\{Y, T, X^S, X_0\}$ within the experimental data where we define $X_0$ to be all pre-treatment variables measured both in the experimental and population data. We define $Z \equiv \{X^S, X_0\}$ to be pre-treatment covariates from which we select a separating set. Note that MRFs (also known as undirected graphical models) are used here to estimate conditional independence relationships as an intermediate step of estimating separating sets; they are not used to estimate the underlying causal directed acyclic graphs (causal DAGs). In addition, we emphasize that we differ from recent causal DAGs-based approaches (Bareinboim et al., 2014; Pearl and Bareinboim, 2014) in that we only rely on domain knowledge about sampling sets and we do not require full knowledge about underlying causal DAGs.

We use a mixed graphical model (Yang et al., 2015), which allows for both con-

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$^1$A simple path is a path in a Markov graph that does not have repeating nodes.
tinuous and categorical variables. More concretely, we assume that each node can be modeled as the exponential family distribution using the remaining variables.

\[
\Pr(G_r \mid G_{-r}) = \exp \left\{ \alpha_r G_r + \sum_{h \neq r} \theta_{r,h} G_r G_h + \varphi(G_r) - \Phi(G_{-r}) \right\}, \quad (7)
\]

where \(G_{-r}\) is a set of all random variables in a Markov graph except for variable \(G_r\), base measure \(\varphi(G_r)\) is given by the chosen exponential family, and \(\Phi(G_{-r})\) is the normalization constant. For example, for a Bernoulli distribution, the conditional distribution can be seen as a logistic regression model.

\[
\Pr(G_r \mid G_{-r}) = \frac{\exp(\alpha_r + \sum_{h \neq r} \theta_{r,h} G_h)}{\exp(\alpha_r + \sum_{h \neq r} \theta_{r,h} G_h) + 1}. \quad (8)
\]

In general, we model each node using a generalized linear model conditional on the remaining variables. Using this setup, we can estimate the structure of the MRF by estimating parameters \(\{\theta_{r,h}\}_{h \neq r}; \theta_{r,h} \neq 0\) for variable \(G_h\) in the neighbors of variable \(G_r\) and \(\theta_{r,h} = 0\) otherwise. We estimate each generalized linear model with \(\ell_1\) penalty to encourage sparsity [Meinshausen and Bühmann (2006)]. Finally, using the AND rule, an edge is estimated to exist between variables \(G_r\) and \(G_h\) when \(\theta_{r,h} \neq 0\) and \(\theta_{h,r} \neq 0\). Researchers can also use an alternative OR rule (an edge exists when \(\theta_{r,h} \neq 0\) or \(\theta_{h,r} \neq 0\)) and obtain the same theoretical guarantee of graph recovery.

**Estimating Separating Sets** Given the estimated graphical model, we can enumerate many different separating sets. First, we focus on the estimation of a separating set of the smallest size because it requires the smallest number of variables to be measured in the target population. It is important to note that this separating set might not be the smallest with respect to the underlying DAG because MRFs don’t encode all conditional independence relationships between variables. It is the smallest size among all separating sets estimable from MRFs.

We estimate this separating set from pre-treatment covariates \(Z\) as an optimization problem. A separating set should block all simple paths between outcome \(Y\) and variables in the sampling set \(X^S\). Therefore, we first enumerate all simple paths between \(Y\) and \(X^S\) and then find a minimum set of variables that intersect all paths.

Define \(q\) to denote the number of variables in \(Z\). We then define \(d\) to be a \(q\)-dimensional decision vector with \(d_j\) taking 1 if we include the \(j\)th variable of \(Z\) into a separating set and taking 0 otherwise. We use \(P\) to store all simple paths from \(Y\) to each variable in \(X^S\) where each row is a \(q\)-dimensional vector and its \(j\)th element takes 1 if the path contains the \(j\)th variable. With this setup, the estimation of the separating set of the smallest size is equivalent to the following linear programming problem given the estimated graphical model.

\[
\min_d \sum_{j=1}^{q} d_j \quad \text{s.t., } \quad Pd \geq 1.
\]

\(^2\)Other principled methods for choosing a set include precision in the PATE estimate or the costs associated with collecting target population information about the separating set.
where $\mathbf{1}$ is a vector of ones. The constraints above ensure that all simple paths intersect with at least one variable in a selected separating set, and the objective function just counts the total number of variables to be included into a separating set. Therefore, by optimizing this problem, we can find a set of variables with the smallest size that is guaranteed to block all simple paths.

It is important to emphasize that the estimation of the Markov graph is subject to uncertainty as any other statistical methods. In our application, we incorporate uncertainties about set estimation through bootstrap. We also investigate accuracy of the proposed algorithm in simulation studies (Section 5).

**Incorporating Users’ Constraints** One advantage of our approach is that we can allow the flexibility for researchers to explicitly specify variables that they cannot measure in the target population. This is important in practice because it is often the case that researchers can measure a large number of covariates in the experimental data but they can collect relatively few variables in the target population. We can easily adjust the previous optimization problem to account for this restriction. Define $\mathbf{u}$ to be a $q$-dimensional vector with $u_j$ taking 1 if we want to exclude the $j$th variable of $\mathbf{Z}$ from a separating set and taking 0 otherwise. As we define $\mathbf{X}_0$ to be those variables observed in both the experimental sample and the target population, $\mathbf{u}$ will place constraints on those covariates in $\mathbf{X}_S$ that are unobservable. Then, the optimization problem above changes as follows.

$$\min_d \sum_{j=1}^{q} d_j \quad \text{s.t., } \mathbf{P}d \geq \mathbf{1} \quad \text{and} \quad \mathbf{u}^\top d = 0$$

In practice, it is possible that there exists no separating set, subject to user constraints. For example, a true separating set could include social connections, which are not measured in the Northern Uganda Survey (the population data). In this case, there is no feasible separating set and our algorithm finds no separating set.

### 4.3 Estimation of Population Average Treatment Effect

To estimate the PATE with estimated separating sets, we use an inverse probability weighting estimator. First, we estimate a probability of being in the experiment $\Pr(S_i = 1 \mid \mathbf{W}_i)$, for example, using a logistic regression [Stuart et al., 2011, Westreich et al., 2017]. Following [Buchanan et al., 2018], we stack the experimental data and the population data, and $S_i = 1$ ($S_i = 0$) indicates that unit $i$ belongs to the experimental data (the population data). We can then estimate weights as

$$\pi_i = \frac{1}{\Pr(S_i = 1 \mid \mathbf{W}_i)} \times \frac{\Pr(S_i = 0 \mid \mathbf{W}_i)}{\Pr(S_i = 0)}, \quad (9)$$

where a usual inverse probability is adjusted by $\Pr(S_i = 0|\mathbf{W}_i)/\Pr(S_i = 0)$ because the PATE is defined only with the population data, i.e., $\mathbb{E}[Y_i(1) - Y_i(0)|S_i = 0]$. Finally, we compute the inverse probability weighting estimator [Stuart et al., 2011],

$$\hat{\tau} \equiv \frac{\sum_{i:S_i=1} \pi_i p_i T_i Y_i}{\sum_{i:S_i=1} \pi_i p_i T_i} - \frac{\sum_{i:S_i=1} \pi_i (1-p_i)(1-T_i) Y_i}{\sum_{i:S_i=1} \pi_i (1-p_i)(1-T_i)}, \quad (10)$$
Figure 1: Causal DAG underlying the simulation study. Note: We consider three conceptually distinct sets (1) a sampling set, $X_4$ and $X_5$ (green), (2) a heterogeneity set, $X_2$ and $X_3$ (orange) and (3) the minimum separating set, $X_1$ (purple). Three root nodes $X_1$, $X_6$, $X_7$ are normally distributed and other pre-treatment covariates are linear functions of their parents.

where $p_i \equiv \Pr(T_i = 1 \mid S_i = 1, W_i)$ is known by the experimental design. We prove its consistency in the supplementary material.

5 Simulation Studies

We turn now to simulations to explore how well the proposed algorithm can recover the PATE. We first verify that our proposed algorithm can obtain a consistent estimator of the PATE. More importantly, we find that estimators based on estimated separating sets often have similar standard errors to the ones based on the true sampling set. Although our approach introduces an additional estimation step of finding separating sets to relax data requirements for the target population, it does not suffer from substantial efficiency loss. Both results hold with and without user constraints on what variables can be measured in the target population.

5.1 Simulation Design

In this subsection, we articulate our simulation design step by step. See the supplementary material for all the details on the simulation design.

Pre-treatment Covariates and Potential Outcome Model To consider different types of separating sets, we assume the causal directed acyclic graph (DAG) in Figure 1 that encodes causal relationships among the outcome, the sampling indicator, and pre-treatment covariates. In this DAG, there are three conceptually distinct sets that we consider – (1) a sampling set, $X_4$ and $X_5$, depicted in green, (2) a heterogeneity set, $X_2$ and $X_3$, depicted in orange, and (3) the minimum separating set, $X_1$, highlighted in purple. Three root nodes $X_1$, $X_6$, $X_7$ are normally distributed...
and other pre-treatment covariates are linear functions of their parents in the DAG. The true PATE is set to 5.00.

**Sampling Mechanism and Treatment Assignment** We randomly sample a set of \( n \) units for a randomized experiment. The sampling mechanism is a logit model based on the sampling set, \( X4 \) and \( X5 \). The treatment assignment mechanism is defined only for the experimental sample (\( S_i = 1 \)). After being sampled into the experiment, every unit has the same probability of receiving the treatment \( \Pr(T_i = 1 \mid S_i = 1) = 0.5 \). For the sake of simplicity, we omit an arrow from the sampling indicator \( S \) to the treatment \( T \) in Figure 1.

**Simulation Procedure** We conduct 5,000 simulations for each of six experimental sample sizes, \( n = \{100, 200, 500, 1000, 2000, 3000\} \). Within each simulation, we first randomly sample \( n \) units for the experiment based on the sampling mechanism and randomly assign units to treatment according to the specified treatment assignment mechanism. We also randomly sample a target population of size \( m = 10,000 \). We then estimate both an exact and a marginal separating set using the experimental data. Here, we use the causal DAG to clarify our simulation settings. In the estimation, we don’t estimate this underlying causal DAG, but rather estimate the separating set using an MRF in order to find a set that meets the left-hand side criterion in Theorems 1 or 2. An advantage of our method is that researchers can specify variables that cannot be measured in the target population. To illustrate this benefit, we also estimate a marginal separating set with a constraint that variable \( X1 \) is unmeasurable in the target population, thus making the minimal separating set unobservable in the target population. We compare these sets to an oracle sampling set, oracle heterogeneity set, and oracle minimum separating set.

For each estimated and oracle set, we compute the PATE using the inverse probability weighting estimator described in Section 4.3. In the supplementary material, we repeat these simulations with a calibration estimator discussed in Hartman et al. (2015), and a linear regression projection estimator.

**5.2 Results**

We present results in Figure 2. Not shown in the graph are the results for the naive difference-in-means, which has significant bias (-1.0). As expected, we see that the bias is tends to zero for the oracle and estimated separating sets, and that the estimators are consistent for the PATE. More importantly, we see that estimators based the selected marginal separating sets (red), exact separating sets (dark blue), and marginal separating set with user constraints (pink) have similar standard errors to the oracle sampling set (green) and the oracle minimum separating set (purple). An estimator based on the heterogeneity set (orange) has smaller standard errors than other estimators partly because it contains variables which are direct predictors of outcomes. However, this estimator might be unavailable in practice as discussed in Section 4 because a heterogeneity set is inherently unobservable.

Figure 3 shows the breakdown of types of estimated separating sets. We group sets that are conceptually similar, and the frequency with which each set is chosen
Figure 2: Simulation Results. Note: The left figure shows bias for the PATE and the right figure presents standard error estimates. As expected, bias is close to zero for all estimators. More importantly, estimators based the estimated separating sets (red) and estimated separating sets with user constraints (pink) have similar standard errors to the oracle sampling set (green) and the oracle minimum separating set (purple).

Figure 3: Types of Estimated Separating Sets. Note: We present the frequency of estimated separating sets by conceptual type. While the algorithm picks an inappropriate set when the sample size is small, as $n$ increases, the most likely set is the minimal separating set.
is presented. For example, if our algorithm selects the variables in the sampling set ($X_4$ and $X_5$) as well as an additional variable, we group these as “similar to” the sampling set. As can be seen, in these simulations as $n$ gets large, over 75% of the time, the minimal separating set (purple) is selected. Small sample size can lead misestimation of the MRF, and therefore selection of inappropriate sets (gray) which do not remove bias – however the rate at which inappropriate sets are selected drops off rapidly with sample size. In the supplementary material, we show that, when incorporating user constraints that make adjustment by the minimum separating set infeasible, the algorithm selects sets similar to the sampling and heterogeneity sets with higher frequency.

6 Empirical Analysis

We apply our proposed approach to the YOP described in Section 2. Our focus is on a central methodological challenge of covariate selection. In the original analysis, the authors adjusted for all eight variables shared by the experimental and population data. However, as noted in the original paper, it is unknown whether the original eight variables is a separating set necessary for estimating PATEs. To tackle this pervasive concern, we employ the proposed approach and select a separating set under two different assumptions about a sampling set and a heterogeneity set.

First, we incorporate domain knowledge about a heterogeneity set, while we maintain the original assumption about a sampling set. As explained in Section 3, by combining substantive information about a sampling set and a heterogeneity set, we can find a separating set, which can be much smaller than each one of the two. Relying on this smaller separating set, we find that point estimates are similar to estimates based on the original sampling set, but standard errors of the proposed approach are much smaller for 16 out of 17 outcomes that the original analysis studied. Incorporating domain knowledge about a heterogeneity set can help us find a smaller set of variables sufficient for the PATE estimation, thereby improving efficiency.

Second, we relax the original assumption about a sampling set — the shared eight variables contain all relevant variables, and we allow for two additional unobserved variables. In the conventional approach based on a sampling set, researchers cannot estimate PATEs under this assumption. In contrast, the proposed approach estimated appropriate separating sets for 4 out of 17 outcomes and we find that PATE estimates for those outcomes are robust to the two additional unobserved sampling variables. At the same time, we reveal that estimated PATEs are sensitive to the original assumption about the sampling mechanism for the other 13 outcomes.

6.1 Incorporating Domain Knowledge on Heterogeneity Set

To begin with, we maintain an assumption about a sampling set in the original analysis, i.e., $X^S = \{\text{Gender, Age, Urban, Marital status, School attainment, Household size, Durable assets, District}\}$. Although the original analysis relies only on this knowledge of the sampling set for the PATE estimation, the authors also carefully discuss a heterogeneity set in their paper. In particular, they discuss two variables: gender and initial credit constraints. Although not perfect, we use
**Durable assets** as a proxy for initial credit constraints. We, therefore, assume that $X^H = \{\text{Gender, Durable assets}\}$. Under these assumptions, the minimum separating set is a union of the sampling set and the heterogeneity set, i.e., $W = \{\text{Gender, Durable assets}\}$. If assumptions about the sampling set and the heterogeneity set hold, estimators based on the original sampling set $X^S$ and on the minimum separating set $W$ are both consistent. However, standard errors of the latter might be smaller because corresponding estimated weights might be more stable.

To estimate the PATEs, we use the inverse probability weighting estimator proposed in Section 4.3. First, we estimate weights using the following logistic regression.

$$ \logit\{\Pr(S_i = 1 \mid C_i)\} = \alpha_0 + C_i^\top \beta, $$

where $C = X^S$ for the estimator based on the original sampling set and $C = W$ for our proposed estimator. We stack the experimental data (sample size = 2,598) and the population data (sample size = 21,348) and $S_i = 1 (S_i = 0)$ indicates that unit $i$ belongs to the experimental data (the population data). We can then estimate weights as $\hat{\pi}_i = 1/\hat{\Pr}(S_i = 1 \mid C_i) \times \hat{\Pr}(S_i = 0 \mid C_i)/\hat{\Pr}(S_i = 0)$, as proposed in Section 4.3. Note that treatment assignment probability in the experiment $\Pr(T_i = 1 \mid S_i = 1, W_i)$ is equal to $\Pr(T_i = 1 \mid S_i = 1, D_i)$ where $D_i$ is a vector indicating 14 districts, because the treatment randomization was stratified by districts (Blattman et al., 2013). Standard errors are clustered by group as done in the original analysis. Note that the difference between the estimator based on the original sampling set and our proposed estimator only comes from the selection of covariates $C$ in the estimation of weights.

We report results in Table 3. Effects of the YOP are large and positive across many outcomes even among the broader target population. For example, the average employment hours would increase by 4.99 hours (20% increase compared to the control group), monthly cash earnings would increase by 12,760 Uganda shilling (38% increase), and a proportion of people enrolled in vocational training would increase by 53 percentage points (350% increase). Comparing estimates based on the original sampling set and those based on the proposed separating set, we reveal that point estimates are similar in that differences between them are not statistically significant at the conventional 0.05 level. This is expected because both estimators are consistent under the assumption that both specified sampling and heterogeneity sets are correct. More interestingly, Figure 4 shows how much smaller standard errors are under the proposed estimator. On average, standard errors of the proposed approach are 51% of those based on the original sampling set. For the outcome “Works mostly in a skilled trade,” the standard error is about a fourth of the original one. This shows that by incorporating domain knowledge about heterogeneity sets, we can find smaller separating sets, which often improve efficiency.

### 6.2 Estimating Marginal Separating Sets

In the previous analysis, we maintained the original authors’ assumption about the sampling set and additionally take into account the assumption about the heterogeneity set. Here, we focus on estimating PATEs under weaker assumptions and
Table 3: Estimates of Population Average Treatment Effects based on the Original Set and the Proposed Separating Set. Note: We estimated population average treatment effects of 17 outcomes using weights based on the original eight variables ("Original estimate") and the minimum separating set ("Sep. Set estimate"). Standard errors of the proposed estimators are smaller for 16 out of 17 outcomes.

Figure 4: Comparing Standard Errors of Population Average Treatment Effects Estimates based on the Original Set and the Proposed Separating Set.

directly address a concern noted in the original paper that the shared eight variable might not contain all relevant variables. In particular, [Blattman et al. 2013] discuss
two potentially problematic variables. First, the authors are concerned that when the government screened applications at the village level, people with more social connections may have received some privilege. Second, people with “affinity for entrepreneurship” (Blattman et al., 2013) might have been more likely to apply for the program in the first place. To account for these two sources of sample selection, we assume that a true sampling set contains two additional variables: (1) Connection, the number of community groups that a respondent belongs to, as a measure of social connections, and (2) Experience, previous experience of vocational training, as a measure of initial motivation and affinity for entrepreneurship. Importantly, both of these two variables are not measured in the population data. Therefore, \( X^S = \{ \text{Gender, Age, Urban, Marital status, School attainment, Household size, Durable assets, District, Connection, Experience} \} \) where the last two variables are measured only in the experiment and not in the population data. Moreover, we don’t make any assumption about heterogeneity sets. Under this assumption, the current practice based on sampling sets or heterogeneity sets cannot estimate any PATEs; weights can be estimated only when sampling sets or heterogeneity sets are measured in both the experimental and population data. In contrast, the proposed estimation algorithm can select appropriate separating sets, should they exist, under such data constraints.

There are two questions of interest for each outcome; (1) Can we find a separating set and estimate the PATE? (2) If we can estimate the PATE, is an estimate different
Figure 6: Estimates of Population Average Treatment Effects based on the Original Sets and Estimated Marginal Separating Sets. Note: We estimated population average treatment effects for 4 outcomes that have estimated proportions of infeasible solutions below 30%. Weights are based on the original eight variables (“Original”) and estimated marginal separating sets (“Estimated Separating Set”).

from the one based on the original eight variables? We estimate marginal separating sets using the proposed algorithm. For each outcome Y, we first estimate a Markov random field and then select a separating set that makes outcome Y and sampling set \( X^S \) conditionally independent under a constraint that the two unobserved variables (Connection, Experience) cannot be selected. When the algorithm can find no separating set under the constraint, we call it an “infeasible solution.” To take into account uncertainties over this covariate selection, we estimate Markov random fields and select separating sets in each of 1000 bootstrap samples.

We begin by computing proportions of infeasible solutions among the 1000 bootstraps (Figure 5). Proportions vary across outcomes, ranging from 9.9% (“Agricultural”) to 89.4% (“Any skilled trade”), and on average, 48%. Given that the current practice just based on sampling or heterogeneity sets cannot estimate PATEs for any outcomes, it is interesting that the proportions of infeasible solutions are below 30% for four outcomes (“Average employment hours” [25.0%], “Employment in agriculture” [9.9%], “Works mostly in a skilled trade” [25.1%], “Changed parish” [11.1%]).

For these four variables, we also report estimates with 95% confidence intervals in Figure 6. We find that point estimates are similar to the original estimates and yet, standard errors are sometimes smaller than the original ones. That is, estimates of the PATEs are robust to alternative separating sets, i.e., even if the sampling set includes additional unobserved variables, substantive conclusions are similar. This result demonstrates that the proposed algorithm of selecting separating sets allows researchers to estimate PATEs in situations where previous methods could not.

7 Concluding Remarks

The increased emphasis on well-identified causal effects in the social and biomedical sciences can sometimes lead researchers to narrow the focus of their research question and limit their findings to the experimental sample. However, primary research questions are often driven by the need to discover the impact of an intervention on
a broader population. The extant literature has focused on the mathematical underpinnings concerning the generalizability of experimental evidence. The aim of this paper is to provide applied researchers with a means for uncovering a separating set using the experimental data alone.

Building on previous approaches to generalization, we clarify the role of the separating set – and its relationship to the sampling mechanism and treatment effect heterogeneity – in identification of population average treatment effects. This framework makes clear that there are many possible covariate sets researchers can use for the recovery of population effects, and it allows us to develop a new algorithm that can incorporate researchers’ data constraints on the target population.

As a concrete context, we focus on the YOP in Uganda, designed to help the poor and unemployed become self-employed artisans and increase their incomes. For these types of large-scale development programs, potential benefits and necessity of generalization are well known among researchers and policy makers. However, analysts are often constrained by available covariate information, which limits applicability of existing approaches that assume rich covariate data from both the experimental and population samples. Our proposed algorithm can help researchers to estimate appropriate separating sets, if any should exist, even under such data constraints. We find that by incorporating domain knowledge about heterogeneity sets, which is often overlooked in the PATE estimation, we can substantially improve efficiency. We also reveal that the proposed algorithm can find separating sets for 4 out of 17 outcomes, even if we allow for two additional sampling variables that are not measured in the population.

Identifying population effects remains a challenging task for experimental researchers. The results here suggest researchers can increase a chance of generalization by collecting rich covariate information on their experimental subjects, even when their capacity of the population data collection is limited.
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Appendix

A Proof of Theorems

Here, we provide proofs for the theorems presented in the paper.

A.1 Proof of Theorem 1

In this proof, we assume that the separating set $W$ is disjoint with the sampling set $X^s$ and the heterogeneity set $X^H$ for simpler notations. The same proof applies to the case in which some variables of the sampling set or the heterogeneity set are in the separating set. First, we have

$$X^H \perp \perp X^s \mid W, T, S = 1. \quad (12)$$

From Random Treatment Assignment (Assumption 1), we have

$$T \perp \perp X^s \mid W, S = 1. \quad (13)$$

Combining equations (12) and (13) (Contraction in Pearl (2000)),

$$\{X^H, T\} \perp \perp X^s \mid W, S = 1,$$

which implies $X^H \perp \perp X^s \mid W, S = 1$. Given that the conditional independence structure of $(X^H, X^s, X)$ is the same under $S = 1$ and $S = 0$, (because $S$ only changes the treatment assignment), we have

$$X^H \perp \perp X^s \mid W, S. \quad (14)$$

From the definition of the sampling variable,

$$X^H \perp \perp S \mid W, X^s. \quad (15)$$

Combining equations (14) and (15) (Intersection (Pearl 2000)), we have

$$X^H \perp \perp \{S, X^s\} \mid W,$$

which implies

$$X^H \perp \perp S \mid W. \quad (16)$$

Additionally, based on the definition of the heterogeneity set,

$$Y(1) - Y(0) \perp \perp S \mid W, X^H. \quad (17)$$

Therefore, by combining equations (16) and (17) based on Contraction in Pearl (2000),

$$\{Y(1) - Y(0), X^H\} \perp \perp S \mid W,$$

which implies $Y(1) - Y(0) \perp \perp S \mid W$. \hfill \Box
A.2 Proof of Theorem 2

First, we have

\[ Y \perp \perp X^S \mid W, T, S = 1. \]  \hspace{1cm} (18)

From Random Treatment Assignment (Assumption 1), we have

\[ T \perp \perp X^S \mid W, S = 1. \]  \hspace{1cm} (19)

Combining equations (18) and (19) (Contraction in Pearl (2000)),

\[ \{Y, T\} \perp \perp X^S \mid W, S = 1, \]

which implies

\[ Y(t) \perp \perp X^S \mid W, S = 1. \]  \hspace{1cm} (20)

Given that the conditional independence structure of \((Y(1), Y(0), X^S, X)\) is the same under \(S = 1\) and \(S = 0\), (because \(S\) only changes the treatment assignment, relationship for potential outcomes and pre-treatment variables would not change), we have

\[ Y(t) \perp \perp X^S \mid W, S, \]  \hspace{1cm} (21)

for \(t = \{0, 1\}\).

From the definition of the sampling variable, for \(t = \{0, 1\}\),

\[ Y(t) \perp \perp S \mid W, X^S. \]  \hspace{1cm} (22)

Combining equations (21) and (22) (Intersection in Pearl (2000)), we have

\[ Y(t) \perp \perp \{S, X^S\} \mid W, \]

which implies

\[ Y(t) \perp \perp S \mid W \]

for \(t = \{0, 1\}\). This completes the proof. \(\Box\)
Supplementary Material:

Covariate Selection for Generalizing Experimental Results

SI-1 IPW Estimator

Here, we show that \( \hat{\tau} \xrightarrow{p} \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0] \).

**Proof** First, we rewrite the IPW estimator as follows.

\[
\hat{\tau} = \frac{1}{n+m} \sum_i S_i \pi_i p_i T_i Y_i - \frac{1}{n+m} \sum_i S_i \pi_i (1 - p_i)(1 - T_i) Y_i \tag{23}
\]

where \( n \) (\( m \)) is the sample size of the experimental data (the population data). By the law of large number,

\[
\frac{1}{n + m} \sum_i S_i \pi_i p_i T_i \xrightarrow{p} \mathbb{E}[S_i \pi_i p_i T_i] = \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) p_i \mathbb{P}(T_i = 1 \mid S_i = 1, W_i) \right\} = \mathbb{E} \left\{ \frac{\mathbb{P}(S_i = 0 \mid W_i)}{\mathbb{P}(S_i = 0)} \right\} = 1.
\]

Similarly, \( \frac{1}{n + m} \sum_i S_i \pi_i (1 - p_i)(1 - T_i) \xrightarrow{p} 1 \). Again, by the law of large number,

\[
\frac{1}{n + m} \sum_i S_i \pi_i (1 - p_i)(1 - T_i) Y_i \xrightarrow{p} \mathbb{E}[S_i \pi_i (1 - p_i)(1 - T_i) Y_i].
\]

Hence, \( \hat{\tau} \xrightarrow{p} \mathbb{E}[S_i \pi_i p_i T_i Y_i - S_i \pi_i (1 - p_i)(1 - T_i) Y_i] \). We focus on the term on the right.

\[
\mathbb{E} \left\{ \pi_i \left( S_i p_i T_i Y_i - S_i (1 - p_i)(1 - T_i) Y_i \right) \right\} = \mathbb{E} \left\{ \pi_i \mathbb{E}\left\{ S_i p_i T_i Y_i - S_i (1 - p_i)(1 - T_i) Y_i \mid W_i \right\} \right\}
\]

\[
= \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) \mathbb{E}\left\{ p_i T_i Y_i - (1 - p_i)(1 - T_i) Y_i \mid S_i = 1, W_i \right\} \right\}
\]

\[
= \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) \{ p_i \mathbb{E}[T_i Y_i \mid S_i = 1, W_i] - (1 - p_i) \mathbb{E}[(1 - T_i) Y_i \mid S_i = 1, W_i] \} \right\}
\]

\[
= \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) \left( \mathbb{E}[Y_i(1) \mid S_i = 1, W_i] - \mathbb{E}[Y_i(0) \mid S_i = 1, W_i] \right) \right\}
\]

\[
= \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 1, W_i] \right\} = \mathbb{E} \left\{ \pi_i \mathbb{P}(S_i = 1 \mid W_i) \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0, W_i] \right\}
\]

\[
= \mathbb{E} \left\{ \frac{\mathbb{P}(S_i = 0 \mid W_i)}{\mathbb{P}(S_i = 0)} \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0, W_i] \right\}
\]

\[
= \int_w \left\{ \frac{\mathbb{P}(S_i = 0 \mid W_i)}{\mathbb{P}(S_i = 0)} \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0, W_i] \right\} p(w)dw
\]

\[
= \int_w \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0, W_i] p(w \mid S_i = 0)dw = \mathbb{E}[Y_i(1) - Y_i(0) \mid S_i = 0],
\]

where the first equality follows from the law of conditional expectation given \( W \), the second from the conditional expectation given \( S \), the third from the linearity of
expectation, the fourth from the conditional expectation given $T$, the fifth from the linearity of expectation, the sixth from the definition of separating $W$, the seventh from the definition of $\pi$, the eight from the rule of expectation, the ninth from Bayes rule, and the tenth from the rule of expectation.

## SI-2 Details on Simulation

### SI-2.1 Simulation Design

**Pre-treatment Covariates** We first generate the population using the following data generating process.

\[
\begin{pmatrix}
X_1 \\
X_2 \\
X_3 \\
X_4 \\
X_5 \\
X_6 \\
X_7 \\
X_8 \\
X_9
\end{pmatrix} \sim N
\begin{pmatrix}
0 \\
0 \\
0 \\
0 \\
0 \\
0 \\
0 \\
0 \\
0
\end{pmatrix},
\begin{pmatrix}
1.00 & -0.70 & 0.70 & 0.70 & -0.20 & 0.00 & 0.00 & 0.50 & -0.70 \\
-0.70 & 1.00 & -0.50 & -0.50 & 0.15 & 0.00 & 0.00 & -0.70 & 0.50 \\
0.70 & -0.50 & 1.00 & 0.50 & -0.15 & 0.00 & 0.00 & 0.33 & -0.50 \\
0.70 & -0.50 & 0.50 & 1.00 & -0.15 & 0.00 & 0.00 & 0.33 & -0.50 \\
-0.21 & 0.15 & -0.15 & -0.15 & 1.00 & 0.00 & 0.00 & -0.10 & 0.30 \\
0.00 & 0.00 & 0.00 & 0.00 & 0.00 & 1.00 & 0.00 & 0.00 & 0.00 \\
0.00 & 0.00 & 0.00 & 0.00 & 0.00 & 0.00 & 1.00 & 0.00 & 0.00 \\
0.50 & -0.70 & 0.33 & 0.33 & -0.10 & 0.00 & 0.00 & 1.00 & -0.33 \\
-0.70 & 0.50 & -0.50 & -0.50 & 0.30 & 0.00 & 0.00 & -0.33 & 1.00
\end{pmatrix}
\]

**Potential Outcome Model** We draw the potential outcomes as follows.

\[Y_i(t) = 5T_i + 10 \times X_{3i} \times T + (-10) \times X_{2i} \times T + X_{6i} - 3 \times X_{8i} + \epsilon_i\]

where $\epsilon_i \sim N(0, 1)$.

**Sampling Mechanism** We then draw a sampling indicator $S_i$ as follows. The second step scales the probability to be bounded away from zero and one.

\[
S'_{i, lp} = -20 \times X_{4i} + 20 \times X_{5i}
\]

\[
S_{i, lp} = 0.25(S'_{i, lp} - \overline{S'}_{lp})/sd(S'_{lp})
\]

\[
S_i = \frac{1}{1 + e^{-S_{i, lp}}}
\]

Simulations are conducted in the DeclareDesign package in R [Blair et al. 2017].

### SI-2.2 Additional Simulation Results

In Section 5, we discussed the breakdown of the different types of estimated separating sets in the simulated data generating process. Here we show the breakdown of types of estimated separating sets when incorporating user constraints in Figure SI-7. In this case, $X1$, the alternative separating set, cannot be measured in the target population, so we see that the algorithm selects the sampling and heterogeneity sets with higher frequency.

Figure SI-8 presents the bias and standard error result by selected estimated separating set type. We refer to sets that are “similar to” different conceptual sets in order to group sets that control for a specific type of separating sets, but which may include extra variables. For example, if the estimated set includes $X4$, $X5$, and $X8$, we say this is similar to a sampling set ($X4$ and $X5$). As theorems tell us, it doesn’t matter what type of separating sets the algorithm estimates in the experimental data,
Figure SI-7: Type of Estimated Marginal Separating Set with User Constraints. Note: We present the frequency of estimated separating sets by conceptual type. With user constraints, the algorithm selects each of the other types of separating sets more frequently.

all of them produce unbiased estimates so long as the set is an appropriate separating set (see Figure [SI-8]). When an inappropriate set is chosen, which is common in the $n = 100$ case but rare as $n$ increases, we see that inappropriate sets do not reduce bias. As we expect, when estimated separating sets are similar to a heterogeneity set, standard errors are the smallest.

Figure SI-8: Simulation Results for Estimated Separating Set by Type. Note: The left figure shows bias for the PATE and the right figure presents standard error estimates. As expected, bias is close to zero for all estimators. Estimated sets are categorized by type: similar to oracle sampling set (green) and the oracle minimum separating set (purple) and oracle heterogeneity set (orange).
Finally, we present the simulation results for two alternative estimators in Figure SI-9: a calibration estimator and a linear regression projection. The calibration estimator matches population means for the estimated separating set using a maximum entropy (raking) algorithm (Hartman et al., 2015). The linear projection estimator estimates a fully interacted linear regression model using the estimated separating set, and projects the model on the target population.

![Figure SI-9: Simulation Results for Alternative Estimators. Note: The left figure shows bias for the PATE and the right figure presents standard error estimates. As expected, bias is close to zero for all estimators. More importantly, estimators based on the estimated separating sets (red) and estimated separating set with user constraints (pink) have similar standard errors to the oracle sampling set (green) and the oracle minimum separating set (purple). An estimator based on the heterogeneity set (orange) has significantly smaller standard errors than other estimators, but this estimator might be unavailable in practice.](image-url)