

Worth Weighting? How to Think About and Use Sample Weights in Survey Experiments

Luke W. Miratrix*
Harvard University

Jasjeet S. Sekhon
University of California, Berkeley

Alexander G. Theodoridis
University of California, Merced

Luis F. Campos
Harvard University

Abstract

The popularity of online surveys has increased the prominence of sampling weights in claims of representativeness. Yet, much uncertainty remains regarding how these weights should be employed in analysis of survey experiments: Should they be used or ignored? If they are used, which estimators are preferred? We offer practical advice, rooted in the Neyman-Rubin model, for researchers producing and working with survey experimental data. We examine simple, efficient estimators (Horvitz-Thompson, Hájek, “double-Hájek”, and post-stratification) for analyzing these data, along with formulae for biases and variances. We provide simulations that examine these estimators and real examples from experiments administered online through YouGov. We find that for examining the existence of population treatment effects using high-quality, broadly representative samples recruited by top online survey firms, sample quantities, which do not rely on weights, are often sufficient. Sample Average Treatment Effect (SATE) estimates are unlikely to differ substantially from weighted estimates, and they avoid the statistical power loss that accompanies weighting. When precise estimates of Population Average Treatment Effects (PATE) are essential, we analytically show post-stratifying on survey weights and/or covariates highly correlated with the outcome to be a conservative choice.

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1 Introduction

Population-based survey experiments have become increasingly common in political science in recent decades ([Gaines, Kuklinski and Quirk, 2007](#); [Mutz, 2011](#); [Sniderman, 2011](#)). However, practical advice remains limited in the literature and uncertainty persists among scholars regarding the role of sampling weights in the analysis of survey experiments ([Franco et al., Forthcoming](#)). Should they be used or ignored? If they are to be used, which estimators are to be preferred? As [Mutz \(2011, 113-120\)](#) notes,

“there has been no systematic treatment of this topic to date, and some scholars have used weights while others have not ... the practice of weighting was developed as a survey research tool—that is, for use in observational settings. The use of experimental methodology with representative samples is not yet sufficiently common for the analogous issue to have been explored in the statistical literature.”

We seek to fill this void with a systematic treatment, based on sound statistical principals rooted in the Neyman-Rubin model, yielding practical advice for scholars seeking to make the best possible decisions when using (or electing not to use) sampling weights in their analysis of survey experiments. We explore the topic through a combination of formulae, simulation, and examination of real data. Taken together, these explorations lead to the conclusion that, for scholars examining population treatment effects using the high-quality, broadly representative samples recruited and delivered by top online survey firms, sample quantities, which do not rely on weights, are often sufficient. Sample Average Treatment Effect (SATE) estimates tend not to differ substantially from weighted estimates, and they avoid the statistical power loss that accompanies weighting. When precise estimates of Population Average Treatment Effects (PATE) are essential, we conclude that a “double-Hàjek” weighted estimator is a very straightforward and reliable option in many cases. We also analytically show that post-stratifying on survey weights and/or covariates highly correlated with the outcome is a conservative choice for precision improvement, because it is unlikely

to do harm and could be quite beneficial in certain circumstances.

The greater prevalence of online surveys has gone hand-in-hand with the boom in survey experiments. Firms such as YouGov (formerly Polimetrix) and Knowledge Networks (now owned by GfK) provide researchers platforms through which to run experiments. The firms offer representative samples generated through extensive panel recruitment efforts and sophisticated sample matching and weighting procedures. By reducing or eliminating costs, subsidized, grant-based and collective programs such as Time Sharing Experiments for the Social Sciences (TESS), the Cooperative Congressional Election Study (CCES), and Cooperative Campaign Analysis Project (CCAP) have further facilitated researchers' access to time on high-end online surveys. It should be noted that other firms and platforms, such as Survey Sampling International, Google Consumer Surveys ([Santoso, Stein and Stevenson, 2016](#)), and Amazon's Mechanical Turk ([Berinsky, Huber and Lenz, 2012](#)), offer even less costly access to large and diverse convenience samples on which researchers can also conduct survey experiments. Researchers using these sometimes generate their own weights to improve representativeness. However, because we view population inferences with such convenience samples as rather tenuous, our primary interest is in methods for analysis of data from sources, such as YouGov and Knowledge Networks, that actively recruit subjects and provide the researcher with sampling weights.

Survey experiments are a two-step process where a sample is first obtained from a parent population, and then that sample is randomized into different treatment arms. The sample selection and treatment assignment processes are generally independent of each other. Sampling procedures have changed in recent years because of increasing rates of non-response and new technologies. As a result, sampling weights can vary substantially across units, with some units having only a small probability of being in the sample. In contrast, the treatment assignment mechanisms are usually simple and relatively balanced, rendering the SATE straightforward to estimate. Estimating the PATE, however, is less so because these estimates need to incorporate the sampling weights, which introduces additional variance as well as a host of complexities. We encourage researchers choos-

ing between these approaches to first give serious thought to the types of inferences they will make. Do they simply wish to establish the presence or absence of an effect in a given population? If so, SATE may suffice. Or, do they hope to measure the magnitude of an effect that may or may not already be documented? In this case, the scholar may consider her options for weighted estimators.

In Section 2 we overview general survey methodology. In Section 3 we then formally consider survey experiments and relate them to the SATE. We formally define the PATE and some estimators of it in Section 4, and we introduce a post-stratification estimator in Section 5. We then investigate the performance of these estimators through simulation studies in Section 6, and analyze trends and features of real survey experimental data collected through YouGov in Section 7. We finally conclude with some advice and high-level pointers to applied practitioners.

2 Surveys and Survey Experiments through the Lens of Potential Outcomes

We formalize surveys and survey experiments in terms of the Neyman-Rubin model of potential outcomes (Splawa-Neyman, Dabrowska and Speed, 1923/1990). Assume we have a population of N units indexed as $i = 1, \dots, N$. We take a sample from this population using a sample selection mechanism, and we then randomly assign treatment in this sample using a treatment assignment mechanism. Both mechanisms will be formally defined in subsequent sections. Each unit i in the population has a pair of values, $(y_i(0), y_i(1))$, called its potential outcomes. Let $y_i(1) \in \mathbb{R}$ be unit i 's outcome if it were treated, and $y_i(0)$ its outcome if it were not. For each selected unit, we observe either $y_i(1)$ or $y_i(0)$ depending on whether we treat it or not. For any unselected unit, we observe neither.

We make the usual no-interference assumption that implies that treatment assignment for any particular unit has no impact on the potential outcomes of any other unit. This assumption is natural in survey experiments. The treatment effect Δ_i for unit i is then the difference in potential

outcomes, $\Delta_i \equiv y_i(1) - y_i(0)$. These individual treatment effects are deterministic, pre-treatment quantities.

Let \mathcal{S} be our sample of n units. Then the Sample Average Treatment Effect (SATE) is the mean treatment effect over the sample:

$$\tau_{\mathcal{S}} = \frac{1}{n} \sum_{i \in \mathcal{S}} \Delta_i = \frac{1}{n} \sum_{i \in \mathcal{S}} y_i(1) - \frac{1}{n} \sum_{i \in \mathcal{S}} y_i(0). \quad (1)$$

This is a parameter for the sample at hand, but is random in its own right if we view the sample as a draw from the larger population. By comparison, a parameter of interest in the population is the Population Average Treatment Effect (PATE) defined as

$$\tau = \frac{1}{N} \sum_{i=1}^N \Delta_i = \frac{1}{N} \sum_{i=1}^N y_i(1) - \frac{1}{N} \sum_{i=1}^N y_i(0).$$

In general $\tau_{\mathcal{S}} \neq \tau$, and if the sampling scheme is not simple (e.g., some types of units are more likely to be selected), then potentially $\mathbb{E}[\tau_{\mathcal{S}}] \neq \tau$.

We discuss some results concerning the sample selection mechanism in the next section. After that, we will combine the sample selection with the treatment assignment process.

2.1 Simple Surveys (No Experiments)

Let S_i be a dummy variable indicating selection of unit i into the sample, with $S_i = 1$ if unit i is in the sample, and 0 if not. Let \mathcal{S} be (S_1, \dots, S_N) , the vector of selections. In a slight abuse of notation, let \mathcal{S} also denote the random sample. Thus, for example, $i \in \mathcal{S}$ would mean unit i was selected into sample \mathcal{S} . Finally let the *selection probability* or *sampling probability* for unit i be

$$\pi_i \equiv \mathbf{P}\{S_i = 1\} = \mathbb{E}[S_i],$$

which is the probability of unit i being included in the sample. We assume $\pi_i > 0$ for all i , meaning every unit has some chance of being selected into \mathcal{S} . The π_i depend, among other things, on the desired size of sample $\mathbb{E}[n]$. We assume the π_i are fixed and known for all units in the population.

Consider the case where we have no treatment and we see $y_i \equiv y_i(0)$ for any selected unit. Our task is to estimate the mean of the population, $\mu = \frac{1}{N} \sum_{i=1}^N y_i$. Estimating the mean of a population with sampling has a long, rich history. We base our work on two estimators from that history here. The first is the Horvitz-Thompson estimator ([Horvitz and Thompson, 1952](#)), which is an inverse probability weighting estimator:

$$\hat{y}_{HT} = \frac{1}{N} \sum_{i=1}^N \frac{1}{\pi_i} S_i y_i.$$

Although unbiased, the Horvitz-Thompson estimator is well known to be highly variable. This variability comes from the weights; if you randomly get too many rare units in the sample, the inverse of their weights will inflate \hat{y}_{HT} , even if all y_i are the same. We are not controlling for the realized size of the sample. This is reparable.

Let $\bar{\pi} = \frac{1}{N} \sum \pi_i$ be the average sampling probability and $n = \sum S_i$ be the sample size, with $\mathbb{E}[n] = N\bar{\pi}$. Then let $w_i = \bar{\pi}/\pi_i$ be the *sampling weight*. These weights w_i are relative to a baseline of 1, which eases interpretability due to removing dependence on n . A weight of 1 means the unit stands for itself, a weight of 2 means the unit “counts” as 2 units, a weight of 0.5 means units of this type tend to be over-represented and so this unit counts as half, and so forth. The total weight of our sample is then

$$Z \equiv \sum_{i=1}^N \frac{\bar{\pi}}{\pi_i} S_i = \sum_{i=1}^N w_i S_i.$$

Z is random, but $\mathbb{E}[Z] = \mathbb{E}[n]$. We can re-express \hat{y}_{HT} as

$$\hat{y}_{HT} = \frac{1}{\mathbb{E}[n]} \sum_{i=1}^N \frac{\bar{\pi}}{\pi_i} S_i y_i = \frac{1}{\mathbb{E}[Z]} \sum_{i=1}^N S_i w_i y_i.$$

\hat{y}_{HT} is almost a weighted average of units, except it is normalized by the expected rather than realized weight of the sample.

This discrepancy is corrected with the Hjek estimator, which takes the usual weighted average of the selected units, and likely reflects the approach used by most scholars:

$$\hat{y}_H = \frac{1}{Z} \sum_{i=1}^N w_i S_i y_i$$

The Hjek estimator is not unbiased, but it often has smaller MSE than Horvitz-Thompson (Hjek, 1958; Srndal, Swensson and Wretman, 2003). The bias, however, will tend to be negligible, as shown by the following lemma:

Lemma 2.1. *[A variation on Result 6.34 of Cochran (1977)] Under a Poisson selection scheme, i.e. units sampled independently with individual probability π_i , the bias of the Hjek estimator is $O(1/\mathbb{E}[n])$. In particular, the bias can be approximated as*

$$\mathbb{E}[\hat{y}_H] - \mu \doteq -\frac{1}{\mathbb{E}[n]} \left(\frac{1}{N} \sum_{i=1}^N (y_i - \mu) \frac{\bar{\pi}}{\pi_i} \right) = -\frac{1}{\mathbb{E}[n]} \text{Cov}[y_i, w_i].$$

See Appendix B for proof. The above shows that, for a fixed population, the bias decreases rapidly as sample size increases. Also, if we sample with equal probability or if the outcomes are constant, the bias is 0. However, if the covariance between the weights and y_i is large, the bias could potentially be large also. In particular, the covariance will be large if rare units (those with small π_i) systematically tend to be outliers with large $y_i - \mu$ because, as sampling weights are non-negative inverses of the π_i , their distribution can feature a long right tail that drives the covariance.

3 Survey Experiments and SATE

Survey experiments are surveys with an additional treatment assigned at random to all selected units. Independent of S_i let T_i be a treatment assignment, with $T_i = 1$ if unit i is treated, 0 otherwise. The most natural such assignment mechanism for our context is Bernoulli assignment, where each responding unit i is treated independently with probability p for some p . Another common mechanism is the classic complete randomization, when a np -sized simple random sample of the n units is treated. Regardless, we assume randomization is a separate process from selection. In particular, we assume that randomization does not depend on the sampling weights.

If our interest is in the SATE, then a natural estimator is Neyman’s difference-in-means estimator of

$$\hat{\tau}_{SATE} = \frac{1}{n_1} \sum_{i=1}^n T_i y_i - \frac{1}{n - n_1} \sum_{i=1}^n (1 - T_i) y_i, \quad (2)$$

with n_1 the (possibly random) number of treated units (see, [Splawa-Neyman, Dabrowska and Speed, 1923/1990](#)).

This estimator is unbiased¹ for the SATE ($\mathbb{E}[\hat{\tau}_{SATE} | \mathcal{S}] = \tau_{\mathcal{S}}$), but unfortunately, the SATE is not generally the same as the PATE and $\mathbb{E}[\tau_{\mathcal{S}}] \neq \tau$ in general. The bias, for fixed n , is

$$bias(\hat{\tau}_{SATE}) = \mathbb{E}[\hat{\tau}_{SATE}] - \tau = \frac{1}{N} \sum_{i=1}^N \left(\frac{\pi_i}{\bar{\pi}} - 1 \right) \Delta_i.$$

The derivation of this can be found in Appendix B. As units with higher π_i will be more likely to be selected into \mathcal{S} , the estimator will be biased toward the treatment effect of these units.

The variance of $\hat{\tau}_{SATE}$, conditional on the sample \mathcal{S} , is well known, but we include it here as we use it extensively.

¹Under randomizations where the estimator could be undefined (e.g., there is a chance of all units getting assigned to treatment, such as with Bernoulli assignment, this unbiasedness is conditional on the event of the estimator being defined. See [Miratrix, Sekhon and Yu \(2013\)](#) for further discussion.

Theorem 3.1. *Let sample \mathcal{S} be randomly assigned to treatment and control with $\mathbb{E}[T_i] = p$ for all i with either a complete randomization or Bernoulli assignment mechanism. The unadjusted simple-difference estimator $\hat{\tau}_{SATE}$ is unbiased² for the SATE, i.e. $\mathbb{E}[\hat{\tau}_{SATE}|\mathcal{S}] = \tau_{\mathcal{S}}$. Its variance is*

$$\text{Var}[\hat{\tau}_{SATE}|\mathcal{S}] = \frac{1}{n} [(\beta_1 + 1)\sigma_S^2(1) + (\beta_0 + 1)\sigma_S^2(0) + 2\gamma_S] \quad (3)$$

$$= \frac{1}{n} [\beta_1\sigma_S^2(1) + \beta_0\sigma_S^2(0) - \sigma_S^2(\Delta)] \quad (4)$$

where $\sigma_S^2(z)$ and $\sigma_S^2(\Delta)$ are the variances of the individual potential outcomes and treatment effects for the sample, and $\beta_\ell = \mathbb{E}[n/n_\ell]$ are the expectations (across randomizations) of the inverses of the proportion of units in the two treatment arms. If n_1 is fixed, such as with a completely randomized experiment, then $\beta_1 = 1/p$, $\beta_0 = 1/(1-p)$ and the above simplifies to Neyman's result of

$$\text{Var}[\hat{\tau}_{SATE}|\mathcal{S}] = \frac{1}{n} \left[\frac{1}{p}\sigma_S^2(1) + \frac{1}{1-p}\sigma_S^2(0) - \sigma_S^2(\Delta) \right]$$

For Bernoulli assignment, the β_ℓ are ugly, and there are also mild technical issues due to the estimator being undefined when, for example, $n_1 = 0$. These issues tend to be negligible, and a good simplification is to, implicitly conditioning on number of units treated, set $p = n_1/n$ and use Neyman's results. Conditioning is a reasonable choice and leads to a more accurate (and more clear) formula; for more detail, including formal definitions of the notation and the derivations, see [Miratrix, Sekhon and Yu \(2013\)](#).

It is important to underscore that any SATE analysis on the sample, given a truly modeled treatment assignment mechanism, is valid. I.e., such an analysis is estimating a true treatment effect parameter, the SATE. If $\mathbb{E}[\tau_{\mathcal{S}}] = \tau$ then any SATE analysis will be correct for PATE as well (although estimates of uncertainty may be too low if they do not account for variability in $\tau_{\mathcal{S}}$). In

²Nearly unbiased, that is. If n_1 is random, and $\mathbf{P}\{n_1 = 0\} > 0$ or $\mathbf{P}\{n_0 = 0\} > 0$ then there is potential for bias, but it is generally exponentially small. See [Miratrix, Sekhon and Yu \(2013\)](#).

particular, if there is a constant treatment effect, then $\tau_S = \tau$ for any sample, and the SATE will be the PATE, and all uncertainty estimates for SATE will be the same as for PATE.³ But a constant treatment effect is a large assumption.

4 Estimating the PATE

Imagine we had all the potential outcomes $y_i(1), y_i(0)$ for the sampled $i \in \mathcal{S}$. These would give us exact knowledge of the SATE, and we could also use this information, coupled with the sampling weights, to estimate the PATE. In particular, with knowledge of the $y_i(\ell)$ we have a sample of treatment effects:

$$\Delta_i = y_i(1) - y_i(0) \text{ for } i \in \mathcal{S}.$$

We can use this sample to estimate the PATE with, for example, a Hájek estimator:

$$\nu_S = \frac{1}{Z} \sum S_i \frac{\bar{\pi}}{\pi_i} \Delta_i = \frac{1}{Z} \sum S_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{Z} \sum S_i \frac{\bar{\pi}}{\pi_i} y_i(0). \quad (5)$$

This estimator is slightly biased, but the bias is small, giving $\mathbb{E}_S[\nu_S] \approx \tau$. If we wanted an unbiased estimator, we could use a Horvitz-Thompson estimator by replacing Z with $\mathbb{E}[n] = N\bar{\pi}$, the expected sample size.

Unfortunately, we do not, for a given sample \mathcal{S} , observe ν_S . We can, however, estimate it given the randomization and partially observed potential outcomes. Estimating the PATE is now implicitly a two-step process: estimate the sample dependent ν_S , which in turn estimates the population

³The estimated uncertainty will, however, depend on the sample \mathcal{S} . For example, if \mathcal{S} happens to have widely varying units, $\hat{\tau}_{SATE}$ will have high variance and the sample-dependent SATE SE estimate should generally reflect that by being large to give correct coverage for τ_S . Now, as this is true for any sample, the overall *process* will have correct coverage.

parameter τ . This gives an overall mean square error of

$$\text{MSE}[\hat{\tau}_{hh}] \approx \mathbb{E}_{\mathcal{S}}[\text{MSE}[\hat{\tau}_{hh}|\mathcal{S}]] + \text{MSE}[\nu_{\mathcal{S}}] \quad (6)$$

Appendix A provides a more formal treatment of the above. We then have two concerns. First, we want to accurately estimate $\nu_{\mathcal{S}}$ using all the tools available to simple randomized experiments such as adjustment methods or, if we can control the randomization, blocking. Second, we want to focus on a sample parameter $\nu_{\mathcal{S}}$ that is itself a good estimator of τ .

4.1 Estimating $\nu_{\mathcal{S}}$

Equation 5 shows that our estimator is the difference in weighted means of our treatment potential outcomes and our control potential outcomes. This immediately motivates estimating these means with the units randomized to each arm of our study, as with the following “double-Hàjek” estimator

$$\hat{\tau}_{hh} = \frac{1}{Z_1} \sum_{i=1}^N S_i T_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{Z_0} \sum_{i=1}^N S_i (1 - T_i) \frac{\bar{\pi}}{\pi_i} y_i(0) \quad (7)$$

with

$$Z_1 = \sum_{i=1}^N S_i T_i \frac{\bar{\pi}}{\pi_i} \text{ and } Z_0 = \sum_{i=1}^N S_i (1 - T_i) \frac{\bar{\pi}}{\pi_i}.$$

The Z_{ℓ} are the total sample masses in each treatment arm. $\mathbb{E}[Z_1] = pN\bar{\pi} = \mathbb{E}[n_1]$, the expected number of units that will land in treatment (similarly for control).

This estimator is two separate Hàjek estimators, one for the mean treatment outcome and one for the mean control. Each estimator adjusts for the total mass selected into that condition. It is the one naturally seen in the field, the difference of weighted means. It corresponds to the weighted OLS estimate regressing the observed outcomes Y^{obs} on the treatment indicators T with weights w_i . This equivalence is shown in Appendix B.

Because this is a Hàjek estimator, there is bias for $\hat{\tau}_{hh}$ in the randomization step as well as the

selection step because the Z_ℓ depend on the realized randomization. Again, this bias is small, which means the expected value of our actual estimator, conditional on the sample, is approximately ν_S , our Hájek “estimator” of the population τ : $\mathbb{E}[\hat{\tau}_{hh}|\mathcal{S}] \approx \nu_S$. (For unbiased versions, see Appendix A.)

We can obtain approximate results for the population variance of $\hat{\tau}_{hh}$ if we view the entire selection-and-assignment process as drawing two samples from a larger population. We ignore the finite-sample issues of no unit being able to appear in both treatment arms (i.e., we assume a large population) and use approximate formula based on sampling theory. For a Poisson selection scheme and Bernoulli assignment mechanism we then have:

Theorem 4.1. *The approximate variance (AV) of $\hat{\tau}_{hh}$ is*

$$AV(\hat{\tau}_{hh}) \approx \frac{1}{p \mathbb{E}[n]} \frac{1}{N} \sum_{j=1}^N w_j (y_j(1) - \mu(1))^2 + \frac{1}{(1-p) \mathbb{E}[n]} \frac{1}{N} \sum_{j=1}^N w_j (y_j(0) - \mu(0))^2,$$

with $\mu(z) = \frac{1}{N} \sum_{i=1}^N y_i(z)$. This formula assumes the π_j are small; see Appendix for a more exact form. This variance can be estimated by

$$\hat{V}(\hat{\tau}_{sd}) = \frac{1}{Z_1^2} \sum_{j=1}^N S_i T_i w_j^2 (y_j(1) - \hat{\mu}(1))^2 + \frac{1}{Z_0^2} \sum_{j=1}^N S_i (1 - T_i) w_j^2 (y_j(0) - \hat{\mu}(0))^2,$$

where $\hat{\mu}(1) = \frac{1}{Z_1} \sum_{i=1}^N S_i T_i w_i y_i(1)$ and $\hat{\mu}(0) = \frac{1}{Z_0} \sum_{i=1}^N S_i (1 - T_i) w_i y_i(0)$.

See Appendix B for the derivation, which also gives more general formulae that can be adapted for other selection mechanisms. For related work and similar derivations, see [Aronow and Middleton \(2013\)](#) and [Wood \(2008\)](#).

5 Post-Stratification for PATE in Survey Experiments

Viewing estimation as a two step process motivates looking for estimators of ν_S that have higher precision. Post-stratification is one way to get them. Say we had a categorical covariate b associated with our outcomes. We can then express our overall estimand τ as:

$$\tau = \sum_{k=1}^K \frac{N_k}{N} \left(\frac{1}{N_k} \sum_{i:b_i=k} (y_i(1) - y_i(0)) \right) = \sum_{k=1}^K f_k \tau_k$$

with N_k being the number of units in the population in stratum k and $f_k = N_k/N$ being the proportion of the population in stratum k . We could then estimate the population τ_k with strata level estimators of

$$\nu_{Sk} = \frac{1}{Z_k} \sum_{i:b_i=k} w_i (y_i(1) - y_i(0)).$$

As before, we would then need to estimate these ν_{Sk} .

This motivates a post-stratified estimator as a combination of estimates of population strata size estimates and population strata effect estimates:

$$\hat{\tau}_{ps} = \sum_{k=1}^K \hat{f}_k \hat{\tau}_k$$

where $\hat{f}_k = Z_k/Z$ estimates f_k , with the Z being the total weight in the sample and the

$$Z_k = \sum_{i:b_i=k} w_i S_i \text{ for } k = 1, \dots, K$$

being the total weights of the strata. These are not dependent on the randomization so

$$\mathbb{E}[\hat{\tau}_{ps} | \mathcal{S}] = \sum_{k=1}^K \hat{f}_k \mathbb{E}[\hat{\tau}_k | \mathcal{S}].$$

If we had population knowledge we might actually know the f_k and simply plug them in; this

connects to the generalization of experiments. See, for example, [Tipton \(2013\)](#).

For the τ_k we have several options. Arguably the most natural is the double-Hàjek estimator of

$$\hat{\tau}_k = \frac{1}{Z_{k1}} \sum_{i:b_i=k} S_i T_i w_i y_i(1) - \frac{1}{Z_{k0}} \sum_{i:b_i=k} S_i (1 - T_i) w_i y_i(0)$$

with Z_{k1} being the total sample weight in the treatment group in stratum k , and similarly for the control. The $\hat{\tau}_k$ will have the usual bias from being Hájek estimators. Here, however, this bias is of order n_k , not n (see Lemma 2.1), and so could potentially be larger than one might expect.

Regardless, combining gives our final

$$\hat{\tau}_{ps} = \sum_{k=1}^K \frac{Z_k}{Z} \left(\frac{1}{Z_{k1}} \sum_{i:b_k=k} S_i T_i w_i y_i(1) - \frac{1}{Z_{k0}} \sum_{i:b_k=k} S_i (1 - T_i) w_i y_i(0) \right). \quad (8)$$

If we want to avoid bias, we could instead use a single-Hájek estimator in each strata:

$$\hat{\tau}_k^{(h)} = \frac{n_k}{Z_k} \left(\frac{1}{n_{Tk}} \sum_{i:b_i=k} T_i w_i y_i(1) - \frac{1}{n_k - n_{Tk}} \sum_{i:b_i=k} (1 - T_i) w_i y_i(0) \right).$$

For the single-Hájek, we immediately have $\mathbb{E}[\hat{\tau}_k^{(h)} | \mathcal{S}] = \nu_{S_k}$, i.e., unbiasedness in the randomization step. This also causes the Z_k to cancel. If the weights within strata are generally homogenous, the single-Hájek will be essentially the same as the double. And if b is built by stratifying on weights, as we discuss next, then we would indeed expect such homogeneity.

5.1 How to Stratify

One can stratify on anything pre-treatment. One natural choice is the sampling weights themselves. To post-stratify on weights, simply build K strata pre-randomization (but not necessarily pre-sampling) by, e.g., taking the K weighted quantiles of the $1/\pi_i$ as the strata.

Generally, adjustments such as post-stratification are quite effective at increasing precision

when using covariates that are strongly associated with outcomes. In particular, the more the mean potential outcomes vary between strata, the greater the gain in precision. And given that it is precisely when the weights and outcomes are correlated that we worry about the weights, it is natural to consider when post-stratifying on them could help.

With that in mind, we can consider the ν_S (and strata-level ν_{S_k} s) to be the SATEs for different, rescaled, potential outcomes. In particular, if we define

$$u_i(\ell) \equiv \frac{n}{Z} \frac{\bar{\pi}}{\pi_i} y_i(\ell) = \frac{n}{Z} w_i y_i(\ell),$$

then

$$\nu_S = \frac{1}{n} \sum_{i=1}^n (u_i(1) - u_i(0)).$$

The $u_i(\ell)$ are fixed, post-sampling, pre-randomization values, i.e., our original potential outcomes scaled by the total sample mass selected. These are new potential outcomes and ν_S is their SATE.

We would expect post-stratification on weights to be beneficial when the $u_i(\ell)$ are associated with the π_i . For generally positive $y_i(\ell)$, such an association is built in due to the $1/\pi_i$ term in the $u_i(\ell)$: large values of π_i would easily be associated with small values of $u_i(\ell)$, and vice-versa. This is spurious, however, and is due to these estimators not being scale-invariant: if we recenter the $y_i(\ell)$ around 0, this association is largely broken, leaving us with a notable overall association only if the underlying $y_i(\ell)$ and π_i are themselves associated.⁴ We advocate, therefore, centering the outcome and post-stratifying on weights particularly if it is believed that the weights correlate with outcome.

Regardless, post-stratification on weights could limit the impact of high-weight units. In particular, post-stratification ensures that, even if we get a few extra high-weight units in one treatment arm, the overall contribution of the other units in that arm are not too discounted. For example,

⁴To see this, consider the covariance between w_i and $w_i(\bar{y}(\ell) + r_i)$ with the y_i being broken down into a mean and residual. The first term induces a correlation and depends on the mean of the y , the second is more structural to the underlying variables.

if there were four units with extreme weights, three of which were in treatment, then without post-stratification, the overall treatment average would be overweighted toward these three units, relative to the control. With post-stratification, this imbalance would be restricted to the high-weight strata, and the estimate of effects for the low-weight strata would not be impacted. Note how the variance of the $u_i(\ell)$ will tend to increase, relative to the raw $y_i(\ell)$, for units with large weights, unless the weights are negatively correlated with the $y_i(\ell)$.

We use post-stratification because it relies on very weak modeling assumptions. While typically beneficial, post-stratification could harm precision if, for example, the sampling probabilities are proportional to the outcomes, i.e., when sampling proportional to size. In this case, the $u_i(\ell)$ could be quite similar and stratifying on π_i would not be helpful. Other estimators that rely on regression and other forms of modeling are also possible, see [Zheng and Little \(2003\)](#) or, more recently, [Si, Pillai and Gelman \(2015\)](#).

5.2 Bootstrap Variance Estimates

In general, the post-stratification step can be sample-dependent. For example, if the units are divided into K quantiles by survey weight, the cut-points of those quantiles depend on the realized weights of the sample. Because this is still pre-randomization, this does not impact the validity of the variance and variance-estimation formulae of the SATE estimate of τ_S or re-weighted SATE estimate of ν_S . It does, however, make generating appropriate population variance formulae difficult. Furthermore, even if the strata are pre-defined, the sampling formulae of Theorem 4.1 are actually for a linearized version of the ratio estimators, and as the strata are smaller than the overall sample, one might be concerned that these approximations are not that good.

An alternate approach is to use the bootstrap. Here we can incorporate the re-estimation of strata in the bootstrap to take into account any additional variability induced by this stage being sample-dependent. Bootstrap is also natural in that in survey experiments we are pulling units from a large population, and so simulating independent draws is reasonable. While a technical analysis

of this approach is beyond the scope of this paper, simulation studies, shown in the next section, show excellent coverage rates.

Appropriate implementation of the bootstrap deserves some discussion. Bootstrap is a “by analogy” technique. To obtain the variability of an estimator we repeatedly simulate obtaining a sample from some population using our hypothesized sampling mechanism, randomizing it into treatment, and estimating the treatment effect using our estimator on that sample. We first, therefore, need to have a population to sample from. Our best estimate of this population is the sample weighted by the sampling weights. We then take a size- n i.i.d. sample from this population with probability proportional to the inverse of these weights. The treatment assignment being Bernoulli means we take a case-wise bootstrap, bootstrapping the original treatment assignment along with the outcome. This avoids any need to impute the missing potential outcomes.

The up-weighting and subsequent weighted sampling steps collapse to generating a bootstrap sample by taking a classic with-replacement unweighted sample (i.e., a case-wise bootstrap) from the original sample of the triples (Y_i^{obs}, Z_i, w_i) .

6 Simulation Studies

We here present a series of simulation studies to assess the relative performance of the respective estimators. We also assess the performance of the bootstrap estimates of the standard errors.

Our simulation studies are as follows: we generate a large population of size $N = 10,000$ with the two potential outcomes and a sampling weight for each unit. Using this population, we repeatedly conduct a sampling and subsequent experiment and record the treatment effect estimates for the different estimators. In particular, we first select a sample of size n , sampling without replacement but with probabilities of selection inversely proportional to the sampling weights.⁵ Once we have obtained the final sample, we randomly assign treatment and estimate the treatment

⁵We ignore a mild technical issue of the π_i not being exactly proportional to the weights due to not sampling with replacement.

effect. After doing this 10,000 times we estimate the overall mean, variance, and MSE of the different estimators to compare their performance to the PATE. For our initial simulations we also calculate bootstrap standard error estimates for all the estimators using the case-wise bootstrap scheme discussed in Section 5.2.

	Estimator	Mean	Bias	SE	RMSE	boot SE	Coverage
A							
1	τ_S	40.36	7.77	1.35	7.89		
2	ν_S	32.58	0.00	1.84	1.84		
3	$\hat{\tau}_{SATE}$	40.37	7.78	3.14	8.39	3.12	30%
4	$\hat{\tau}_{hh}$	32.62	0.03	3.91	3.91	3.87	95%
5	$\hat{\tau}_{ps}$	32.60	0.01	2.67	2.67	2.69	95%
B							
1	τ_S	30.00	0.00	0.00	0.00		
2	ν_S	30.00	0.00	0.00	0.00		
3	$\hat{\tau}_{SATE}$	30.01	0.01	2.58	2.58	2.58	95%
4	$\hat{\tau}_{hh}$	30.03	0.03	3.35	3.35	3.31	95%
5	$\hat{\tau}_{ps}$	30.02	0.02	3.32	3.32	3.29	95%

Table 1: Simulations A & B. Performance of different estimators as estimators for the PATE for (A) a heterogenous treatment effect scenario with $\tau = 32.58$ and (B) a constant treatment effect of $\tau = 30$. For each estimator, we have, from left to right, its expected value, bias, standard error, root mean squared error, average bootstrap SE estimate and coverage across 10,000 trials.

Simulation A. Our first simulation is for a population with a heterogeneous treatment effect that varies in connection to the sampling weight. See Appendix C for some simple plots showing the structure of the population and a single sample. Our treatment effect, outcomes and sampling probabilities are all strongly related. We first randomly generated the original population, obtaining a PATE of 32.58. We then took samples of a specified size from this fixed population, and examined the performance of our estimators as estimators for the PATE.

Results for $n = 500$ are on Table 1. Other sample sizes such as $n = 100$, not shown, are substantively the same. The first two lines of the table show the performance of the two “oracle” estimators τ_S (Equation 1) and ν_S (Equation 5), which we could use if all of the potential outcomes

were known. For τ_S there is bias because the treatment effect of a sample is not generally the same as the treatment effect of the population. The Hjek approach of ν_S , second line, is therefore superior despite the larger SE. Line 3 is the simple estimate of the SATE from Equation 2. Because it is estimating τ_S , it has the same bias as line 1, but because it only uses observed outcomes, the SE is larger. Line 4 uses the “double-Hjek” estimator shown in Equation 7. This estimator is targeting ν_S , reducing bias, but has a larger SE relative to line 3 due to the fact that we are incorporating weights. Line 5 is the post-stratified “double-Hjek” of Equation 8. Units were stratified by their survey weight, with $K = 7$ equally sized (by weight) strata. For this scenario, post-stratifying helps, as illustrated by the smaller SE and RMSE, compared to $\hat{\tau}_{hh}$.

An inspection of the coverage rates reflects what we have already discussed: The estimate $\hat{\tau}_{SATE}$ does not target the PATE while the other two sample estimates, $\hat{\tau}_{hh}$ and $\hat{\tau}_{ps}$, do. Therefore it has terrible coverage. Furthermore, the latter two estimates give correct coverage, which is reflective of the bootstrap SE estimates hitting their mark.

Simulation B. As a second simulation we kept the original structure between $Y(0)$ and w , but set a constant treatment effect of 30 for all units. Results are on the bottom half of Table 1. Here, $\tau_S = \tau$ for any sample S , so there is no error in either estimate with known potential outcomes (lines 1–2). This also means that $\hat{\tau}_{SATE}$ is a valid estimate of the PATE and this is reflected in the lack of bias and nominal coverage rate (line 3). The increase of SE of the weighted and post-stratified estimators (lines 4–5) reflects the use of weights when they are in fact unnecessary. Overall the SATE estimate is the best, as expected in this situation.

Simulation C. In our final simulation, we systematically varied the relationship between sampling weight and outcomes while maintaining the same marginal distributions in order to examine the benefits post-stratification.

In our DGP we first generate a bivariate normal pair of latent variables with correlation γ ,

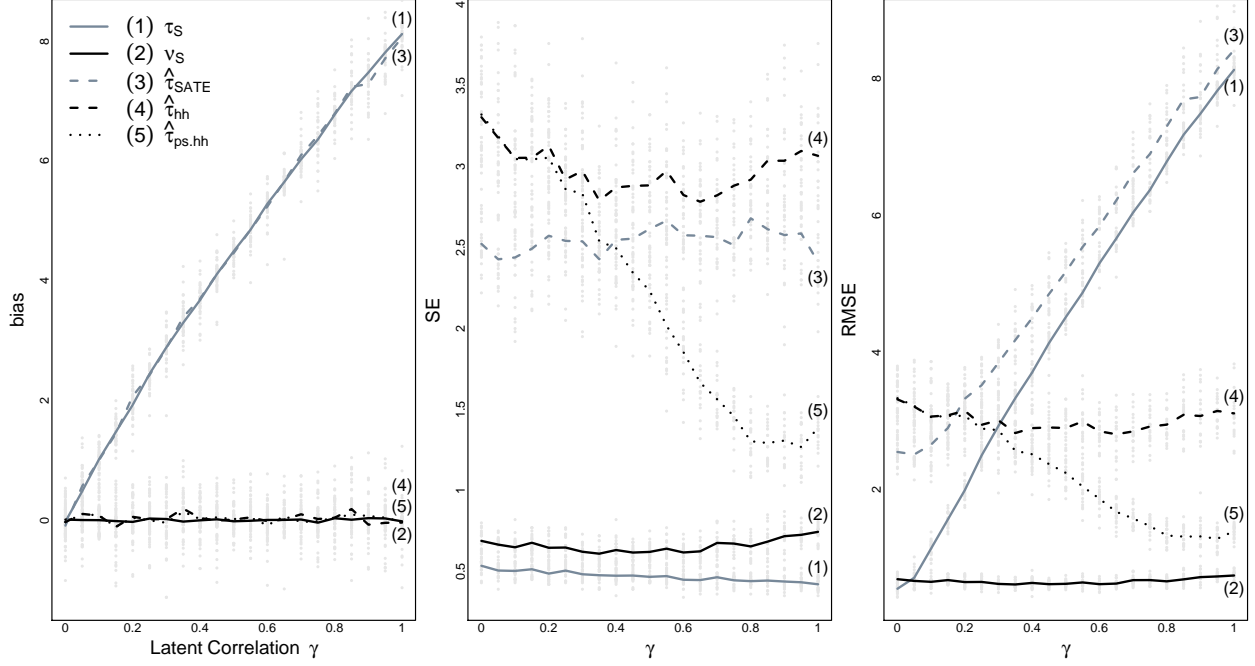


Figure 1: Simulation C: (left to right) SE, bias and RMSE of estimates when the sampling weight is increasingly related to the potential outcomes. Grey are SATE-targeting, black PATE-targeting. Solid are oracle estimators using all potential outcomes of the sample, dashed are actual estimators. The thicker lines are averages over the 20 simulated populations in light gray.

and then generate the sampling weights as a function of the first variable and the outcomes as a function of the second. Then, by varying γ we can vary the strength of the relationship between outcome and weight. (See Appendix C for the particulars.) When $\gamma = 1$, which corresponds to Simulation A, w_i and $(Y_i(0), Y_i(1))$ have a very strong relationship and we benefit greatly from post-stratification. Conversely when $\gamma = 0$, w_i and $(Y_i(0), Y_i(1))$ are unrelated and there will be no such benefit.

For each γ we generated 20 populations, conducting a simulation study within each population. We then averaged the results and plotted the averages against γ on Figure 1. The solid lines give the performance of the oracle estimators τ_S and ν_S , and the non-solid lines are the estimators. The gray lines are estimators that do not incorporate the weights, and the black lines are estimators that do. The light grey points show the individual population simulation studies; they vary due to the variation in the finite populations.

We first see that, because both the double-Hàjek and its post-stratified version are targeting ν_S , which in turn estimates the PATE, they remain unbiased regardless of the latent correlation. On the other hand, the SATE and its estimator, $\hat{\tau}_{SATE}$, are affected. The bias continually increases as the relationship between weight and treatment effect increases.

As expected, the SE of the estimators that do not use weights, τ_S and $\hat{\tau}_{SATE}$, stay the same regardless of γ because the marginal distributions of the outcomes are the same across γ . The estimators that do use weights but only use them to adjust for sampling differences, ν_S and $\hat{\tau}_{hh}$, also remain the same, although their SEs are larger than for τ_S and $\hat{\tau}_{SATE}$ because of incorporating the weights. We pay for unbiasedness with greater variability. The post-stratified estimator $\hat{\tau}_{ps}$, however, sees continual gains as the weights are increasingly predictive of treatment effect. For low γ , it has roughly the same uncertainty as $\hat{\tau}_{hh}$, but is soon the most precise of all (non-oracle) estimators.

These conclusions are tied together in the right-most panel of Figure 1, showing the RMSE, which gives the combined impact of bias and variance on performance. As γ increases, the RMSE of $\hat{\tau}_{SATE}$ steadily climbs due to bias, eventually being the worst at $\gamma = 0.2$. Meanwhile, the post-stratified estimator that exploits weights, $\hat{\tau}_{ps}$, performs better and better. Overall, if weights are important then 1) the bias terms can be too large to be ignored, and 2) there is something to be gained by adjusting the estimates of treatment effects with those weights beyond simple re-weighting. Otherwise, SATE estimators are superior, as incorporating weights can be costly.

Discussion. As expected, estimating the SATE is a superior choice in terms of RMSE only in certain cases, e.g. constant treatment effects or effects uncorrelated with the sampling mechanism. When treatment effect varies with weight, however, the SATE can be quite a bit off. Furthermore, since bias is not estimated in classic measures of uncertainty, inference using the SATE can be overly optimistic and coverage can be poor even in circumstances with low RMSE, if there is such bias. This would suggest that we should generally incorporate weights unless our focus really

is the SATE. Unfortunately, however, for most of our simulations incorporating the weights does have its price: the standard errors generally increased substantially.

7 Real Data Application

To better understand the overall trade offs involved in using weighted estimators of PATE versus simply estimating the SATE on actual survey experiments we analyzed 87 different survey experiments embedded in 7 separate surveys fielded by us through YouGov over the course of 7 years. All of the studies examined were conducted in the United States and focused on topics related to partisanship. Sample sizes for the experiments range from 201 to 504. Because the studies were targeted, 71 of them (82%) showed SATEs that were significantly different from zero. However, once the sampling weights provided by YouGov were taken into account to estimate the PATE (via the double-Hàjek estimate) only 59 experiments (68%) had significant effects. This difference highlights the importance of 1) clarity and justification regarding a scholar’s interest in SATE or PATE, and 2) the importance of examining both so that the sources of any divergence can be better understood.

Our clearest finding is that incorporating weights substantially increases the standard errors. Figure 2(a) shows a 35.6% average increase in variance of $\hat{\tau}_{hh}$ over $\hat{\tau}_{SATE}$ across experiments.

We next examined whether there is evidence of some experiments having a PATE substantially different from the SATE. To do this, we calculated bootstrap estimates of the standard error for the difference in the estimators, and calculated a standardized difference in estimates of $\hat{\delta} = (\hat{\tau}_{SATE} - \hat{\tau}_{hh}) / \widehat{SE}$. If there were no difference between the SATE and the PATE, the $\hat{\delta}$ s should be roughly distributed as a standard Normal. To examine this, we compared our 87 $\hat{\delta}$ values to the standard normal with a qq-plot (Figure 2(b)). While there is a somewhat suggestive tail departing from the expected line, we can see that the bulk of the experiments follow the standard normal distribution relatively closely, suggesting that the SATE and PATE were generally quite similar relative to their

estimation uncertainty.

Finally, we examined whether post-stratification on weights improved precision. Generally, it did not: the estimated SEs of $\hat{\tau}_{ps}$ are very similar to those for $\hat{\tau}_{hh}$, with an average increase of about 0.5%. Further examination offers a hint as to why post-stratification did not yield benefits: the weights generated by YouGov for these samples do not correlate meaningfully with the outcomes of interest in the experiments. In no case did the magnitude of the correlation between weights and outcome exceed 0.23. While this does not imply that scholars need not consider post-stratifying on weights, it does show that outcomes of interest in political science studies are not necessarily going to be correlated with sampling weights. This makes clear the importance of researchers understanding, and reporting, the process used to generate weights and being aware of the covariates with which those weights are likely to be highly correlated (for online surveys, such a list would often includes certain racial and education-level categories).

To further highlight this point, we examine the effects of post-stratification on a covariate, respondent party identification, which is highly correlated with the outcomes in this collection of experiments (and many experiments in American political behavior). Here, our SATE estimators saw an average of a 7.8 percent reduction in the standard errors, with improvement in 93 percent of the experiments, from post-stratification on respondent party identification. Our “double-Hàjek” (both post-stratified on weights and not) also saw standard errors shrink by an average of 6.3 and 5.4 percent respectively, with 75 percent of our experiments seeing improvement.

Discussion. Overall, it appears that in this context and for these experiments, the survey weights significantly increase uncertainty, and that there is little evidence that the RMSE (which includes the SATE-PATE bias) for estimating the PATE is improved by estimators that include these weights. Furthermore, the weights are not predictive enough of outcome to help the post-stratified estimator. With regards to post-stratification, we note that in practice the analysis of any particular experiment would likely be improved by post-stratifying on known covariates predictive of outcome rather than

naïvely on the sampling weights.

In understanding these findings, it is useful to consider the ways in which data from these leading online survey firms (in this case, YouGov) may differ from more convenience-based online samples. Even unweighted, datasets from firms like YouGov tend to be more representative. This is because they often engage in extensive panel recruitment and retention efforts and assign subjects from their panels to client samples through mechanisms such as block randomization. As a result, the unweighted data are often largely representative of the overall population along many relevant dimensions. Relatedly, firms may use a clean-up matching step, such as the one employed by YouGov, where they down-sample their data to generate more uniform weights ([Rivers, 2006](#)). This will likely increase the heterogeneity of the final sample, which could decrease precision.⁶ We recommend, therefore, that researchers request the original, pre-weighted data, in order to work with a larger and more homogeneous sample. For the SATE the gains are immediate. For the PATE, one might generate weights for the full sample by extrapolating from the weights assigned in the trimmed sample or by contracting with the survey firm to provide weights for this full unmatched sample. Then, by post-stratifying on the weights, the researcher can take advantage of the additional units to increase precision in some strata without increasing variability in the others. For both SATE and PATE estimation, power would be generally improved.

⁶Consider a standard scenario wherein a researcher purchases a sample of 1000 respondents. To generate these data, the survey firm might recruit 1400 respondents, all of whom participate in the study. Two datasets result from this. The first contains all 1400 respondents. The second is a trimmed version, where the firm drops 400 of the most overrepresented respondents (which is tantamount to assigning these respondents a weight of 0). This second set, which comes with weights assigned to each observation, is what many scholars analyze. Some firms will, upon request, also provide the full data set, but these data do not generally include sampling weights, as the process for generating these weights is combined with the procedure for trimming down the larger data set by matching it to some frame based upon population characteristics. The weights will be less extreme than they would have been had the entire original sample been included, and the trimmed sample will be more heterogeneous, as many similar observations will be purged. This will make it more difficult to estimate its SATE compared to the full set (do note the SATEs could differ). Furthermore, post-stratification shows that estimators that include weights for the trimmed set will also be less variable than for the same estimators on the full dataset (assuming weights could be obtained), even though the trimmed dataset weights will be less variable. Consider a case with two classes of respondents, reluctant and eager, equally represented in the population. The trimmed sample will have fewer eager respondents. Then, compared to the full data set, we will have a less precise estimate of the eager respondents in the trimmed data set. The precision for the reluctant respondents would be the same. Overall, our combined estimate will be, therefore, less precise.

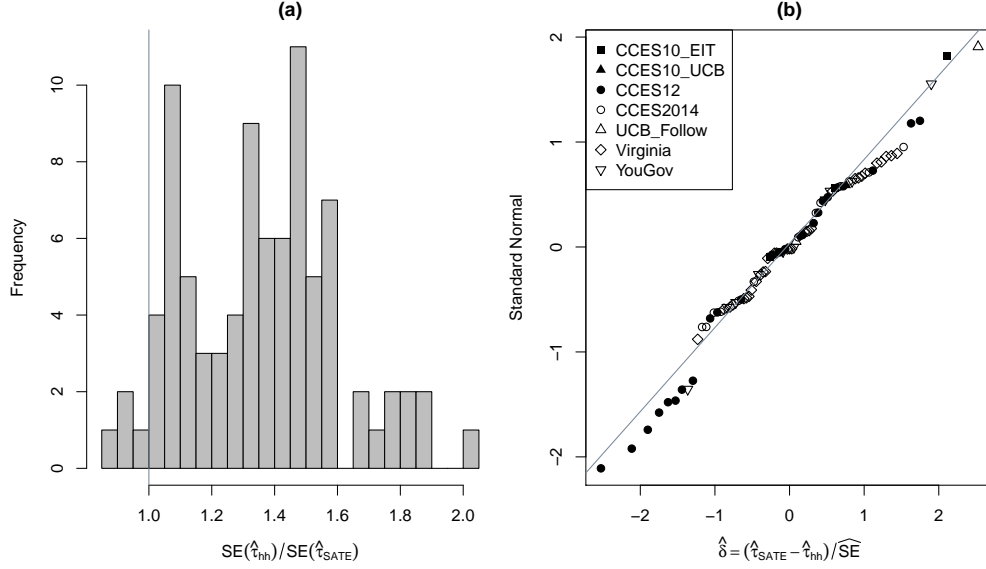


Figure 2: (a) Standardized efficiency of estimates of SATE, $\hat{\tau}_{SATE}$ and PATE, $\hat{\tau}_{hh}$ as well as a comparison to a standard normal (b) via a quantile-quantile comparison plot of the *relative difference* $\hat{\delta}$ of an estimate for the 68 experiments grouped into the larger surveys they are part of.

8 Conclusions

We investigate the use of sampling weights in survey experiments from a potential outcomes framework. We focus on two styles of estimator, those that incorporate these weights to take the sampling mechanism into account, and those that ignore weights and instead focus on estimating the SATE.

Our investigation highlights a few key features of population-based survey experiments. First, because the randomization of units into treatment is independent of the sampling weights, any inference conditional on the sample and the weights is a valid inference: we are estimating the treatment effect for a hypothetical population defined by the weights and sample, even if it does not correspond to the actual population. In particular, if we find a treatment effect in our weighted sample, we know the treatment does work for at least some. The challenge is then connecting this hypothetical population to the real population. This is the only place in which inaccuracies in the sampling weights could matter.

We also find that incorporating sampling weights, even when they are exactly known, substan-

tially decreases precision. Because of this, researchers are faced with a trade-off: more powerful estimates for the SATE, or more uncertain estimates of the PATE. Ignoring the weights means the researcher is putting focus on the sample average treatment effect, and most likely implicitly assuming that this found effect would generalize to the population. This may be reasonable if, for example, it is believed that the treatment effect does not vary much across the population. An alternative justification may be made if it is believed the treatment effect does not vary in any way connected to individual's propensity to participate in or be selected into the trial. These tend to be strong assumptions.

Interestingly, our examination of real survey data found no strong connection between the weights and outcomes, meaning that the SATE and PATE estimators tended to be similar. It should be emphasized that this will not always be the case, as our simulations demonstrate. If the treatment effect correlates with weights then the SATE and PATE could be substantially different. Also, when the outcome of interest is meaningfully correlated with an attribute (our simulations suggest that benefits are greatest when correlations exceed 0.4), post-stratification is likely to be beneficial. This second point was further demonstrated in our analysis of real-data examples with outcomes highly correlated with respondent party identification.

Our examination leads to several general pieces of practical guidance: 1) When analyzing survey experiments using high quality, broadly representative samples, like those recruited and provided by firms like YouGov and Knowledge Networks, SATE estimates will generally be sufficient for most purposes. This is largely because adding weights is costly in terms of precision, and we find that, in practice, weighted estimates do not deviate greatly from simple sample averages. 2) If a particular research question calls for estimates of PATE, a “double-Hàjek” is probably the most straightforward (and a defensible) approach, unless weights are highly correlated with the outcomes variables. 3) If weights are strongly correlated with a study's outcome(s) of interest, post-stratification on the weights (and/or other covariates highly correlated with the outcomes ([Miratrix, Sekhon and Yu, 2013](#))) is a simple and effective approach for scholars seeking to draw

population inferences, so long as the strata-level sample sizes are not too small.

Overall, we advocate a two-stage approach: first focus on the SATE using the entire, unweighted sample and determine whether the treatment had impact. This will generally be the most powerful strategy for detecting an effect, as the weights, being set aside, will not inflate uncertainty estimates. Then, once a treatment effect is established, generate a point estimate of the PATE by using the sampling weights. This stage is an assessment of the magnitude of an effect in the population once an effect on at least some members of the population has been established. The wider confidence intervals one will obtain due to the weighting captures the difficulty in generalization. Generally, scholars should recognize that estimating the PATE is more difficult than the SATE.

With this work we do not address a third piece of uncertainty, that of the sampling weights themselves. That being said, survey experiments are closely related to generalizing randomized trials to wider populations, especially when the weights themselves are estimates of the tendency of individuals to be selected into the sample for the experiment. An increasing body of work is focused on this problem, such as discussed in [Hartman, Grieve and Ramsahai \(2015\)](#) and [Imai, King and Stuart \(2008\)](#). Here, the approach is generally to weight units with their propensity for inclusion into the experiment in order to estimate population characteristics. These propensities of inclusion are usually estimated by borrowing from the propensity score literature for observational studies. This general idea is discussed in [Stuart et al. \(2010\)](#). One nice aspect of this approach is it provides diagnostics in the form of a placebo test. In particular, the characteristics of the re-weighted control group of the randomized experiment should match the characteristics of the population of interest (see [Cole and Stuart \(2010\)](#) for a discussion along with an in-depth case study). For example, [O’Muircheartaigh and Hedges \(2014\)](#) and [Tipton \(2013\)](#) propose post-stratified estimators, stratifying on these estimated weights. In their case, however, they also have the population proportions of the strata as given, which allows for simpler variance expressions. Furthermore, they do not incorporate the unit-level weights once they stratify. [Tipton \(2013\)](#) in-

investigates the associated bias-variance trade-offs due to stratification, and gives advice as to when stratification will be effective. Similar methods could be used for survey experiments by, for example, post-stratifying and then ignoring the weights within the strata. Similarly, the results here potentially could, conditioning on the propensity scores, be used to motivate variance expressions for generalization.

Regardless of choices made, we advocate greater transparency in how work is presented as it pertains to these issues. The suitability of the sample and analysis techniques for population inferences, the claims, both explicit and implied, made by scholars in the presentation of these data, and the way in which findings are subsequently interpreted by other scholars are not always consistent. Some might dismiss concerns regarding population inferences by arguing that their analysis is merely designed to examine the possibility of a treatment effect. If findings are to truly be interpreted, by both the authors and readers, in such a starkly limited way, then we agree that the representativeness of the sample is of no consequence. However, this should be made abundantly clear in any description of findings, and scholars should take care not to overstate the scope of results.

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Appendix A: A general class of estimators

When estimating the PATE, our overall estimation error is a combination of our error due to the randomized experiment for estimating ν_S and the difference between our survey-sampling estimate ν_S and the PATE τ . We can break this error down for any estimator $\hat{\tau}_*$ of ν_S . First, given $\hat{\tau}_*$, we have $\mathbb{E}[\hat{\tau}_*|\mathcal{S}] = \nu_S + b_S$, with b_S being a bias term. Then

$$\begin{aligned} \text{MSE}[\hat{\tau}_*] &= \mathbb{E}[(\hat{\tau}_* - \tau)^2] \\ &= \mathbb{E}[(\hat{\tau}_* - \nu_S)^2] + \mathbb{E}[(\nu_S - \tau)^2] + 2 \mathbb{E}_S[b_S(\nu_S - \tau)] \\ &= \mathbb{E}_S[\text{MSE}[\hat{\tau}_*|\mathcal{S}]] + \text{MSE}[\nu_S] + 2 \mathbb{E}_S[b_S(\nu_S - \tau)] \end{aligned} \tag{9}$$

Given a choice of ν_S , the first term is the expected MSE of the estimator for estimating ν_S when we consider all possible *randomizations* of treatment assignment on the given sample \mathcal{S} . The second term is the MSE of ν_S as an estimator for τ across all samples. The third term is a cross-bias term; it depends on how the bias of a sample is correlated with the error of its ν_S . We generally assume it is small and ignore it.

The first term will tend to be a function of the randomization method used and sample-dependent parameters such as $\sigma_S^2(1)$, $\sigma_S^2(0)$, $\sigma_S^2(\Delta)$, and, importantly, the choice of estimator $\hat{\tau}_*$. For a given choice of ν_S , if we reduce this inner term, we reduce the expectation and therefore increase the overall precision of the estimator for PATE. We reduce this term with better estimators, e.g., ones that exploit covariates; this is the goal of the post-stratification approach discussed in Section 5.

The sampling scheme and choice of ν_S governs the second term. If we reduce it by changing ν_S , we increase precision. The main way to do this is to sample better, e.g., move closer to equal probability sampling.

Alternate estimators. Given the above, our primary “double-Hàjek” estimator $\hat{\tau}_{hh}$ can be viewed as doubly biased: the expected value across randomizations is approximately ν_S , and the expected

value of ν_S is approximately τ . We could instead use Horvitz-Thompson style estimators at either or both levels to remove these biases. In particular, if we select an estimator that is unbiased at the randomization level, i.e. $\mathbb{E}[\tau_*|\mathcal{S}] = \nu_S$, then we have

$$\text{MSE}[\hat{\tau}_*] = \mathbb{E}_S[\text{Var}[\hat{\tau}_*|\mathcal{S}]] + \text{Var}_S[\nu_S] + (\mathbb{E}_S[\nu_S] - \tau)^2$$

One such estimator is the “single-Hàjek” estimator of

$$\hat{\tau}_h = \frac{1}{Zp} \sum_{i=1}^N S_i T_i w_i y_i(1) - \frac{1}{Z(1-p)} \sum_{i=1}^N S_i (1 - T_i) w_i y_i(0).$$

This estimator is tied to double-Hàjek by $\mathbb{E}[Z_1|\mathcal{S}] = pZ$ and $\mathbb{E}[Z_0|\mathcal{S}] = (1-p)Z$. It is a Horvitz-Thompson estimator with respect to the randomization for the two parts of our estimand ν_S . Interestingly, this estimator has the same asymptotic variance expression found in Theorem 4.1 as $\hat{\tau}_{hh}$.

Finally, if $\mathbb{E}_S[\nu_S] = \tau$ we have

$$\text{MSE}[\hat{\tau}_*] = \mathbb{E}_S[\text{Var}[\hat{\tau}_*|\mathcal{S}]] + \text{Var}_S[\nu_S].$$

For fixed n , we have such an estimator as

$$\hat{\tau}_{sd} = \frac{1}{n_1} \sum_{i \in \mathcal{S}} T_i w_i y_i(1) - \frac{1}{n - n_1} \sum_{i \in \mathcal{S}} (1 - T_i) w_i y_i(0). \quad (10)$$

This estimator generally pays a large price for unbiasedness with high variance.

Appendix B: Derivations

In the following we derive the bias of the Hájek estimator, show that it is small, and show that τ_{SATE} does not estimate the PATE. After this we show how a weighted OLS regression can be used in practice to estimate the double-Hájek. Finally, we derive the sampling properties of the unstratified PATE estimators.

Hájek Bias.

The proof of Lemma 2.1, that the bias of a Hájek estimator is $O(1/\mathbb{E}[n])$, follows a similar strategy to the proof of Result 6.34 in Cochran (1977). That result is of the bias of a general ratio estimator for a fixed sample size under simple random sampling. We adapt this result to the Hájek estimator (also a ratio estimator) under independent Poisson random sampling with variable sample size. A fixed sample size correction is possible, but is not needed for our purposes.

We extend the notation described in Section 2.1. Denote $Z_y = \sum_{i=1}^N \frac{\bar{\pi}}{\pi_i} S_i y_i$ so that we can write $\hat{y}_H = \frac{Z_y}{Z}$. The expected values of both the numerator and denominator are

$$\mathbb{E}[Z_y] = N\bar{\pi}\mu, \tag{11}$$

$$\mathbb{E}[Z] = N\bar{\pi}.$$

These results alone should motivate why the Hájek estimator should be approximately unbiased, but let us be a bit more rigorous. By first manipulating the difference of the estimator and its target and then applying the first order Taylor approximation, $(1 + A)^{-1} \doteq (1 - A)$, we can get the

approximate difference.

$$\begin{aligned}
\hat{y}_H - \mu &= \frac{Z_y}{Z} - \mu = \frac{Z_y - \mu Z}{Z} = (Z_y - \mu Z) \frac{1}{Z} \\
&= (Z_y - \mu Z) \frac{1}{N\bar{\pi}} \frac{N\bar{\pi}}{Z} = (Z_y - \mu Z) \frac{1}{N\bar{\pi}} \left(\frac{Z}{N\bar{\pi}} \right)^{-1} \\
&= (Z_y - \mu Z) \frac{1}{N\bar{\pi}} \left(\frac{N\bar{\pi} + (Z - N\bar{\pi})}{N\bar{\pi}} \right)^{-1} \\
&= (Z_y - \mu Z) \frac{1}{N\bar{\pi}} \left(1 + \frac{Z - N\bar{\pi}}{N\bar{\pi}} \right)^{-1} \\
&\doteq (Z_y - \mu Z) \frac{1}{N\bar{\pi}} \left(1 - \frac{Z - N\bar{\pi}}{N\bar{\pi}} \right)
\end{aligned}$$

Taking expectations and noting that $\mathbb{E}[Z_y - \mu Z] = 0$ by Equation 11 leads to the approximate bias:

$$\begin{aligned}
\mathbb{E}[\hat{y}_H] - \mu &\doteq - \frac{1}{(N\bar{\pi})^2} \mathbb{E}[(Z_y - \mu Z)(Z - N\bar{\pi})] \\
&= - \frac{1}{(N\bar{\pi})^2} \left(\mathbb{E}[Z_y Z] - N\bar{\pi} \mathbb{E}[Z_y] + N\bar{\pi} \mu \mathbb{E}[Z] - \mu \mathbb{E}[Z^2] \right).
\end{aligned} \tag{12}$$

These expanded terms can be calculated individually for our estimator using properties of variance

and covariance.

$$\mathbb{E}[Z_y Z] = Cov(Z_y, Z) + \mathbb{E}[Z_y] \mathbb{E}[Z] \quad (13)$$

$$\begin{aligned} &= \sum_{i=1}^N \sum_{j=1}^N \frac{\bar{\pi}^2}{\pi_i \pi_j} y_i Cov(S_i, S_j) + (N\bar{\pi}\mu)(N\bar{\pi}) \\ &= \sum_{i=1}^N \frac{\bar{\pi}^2}{\pi_i^2} y_i Var(S_i) + N^2 \bar{\pi}^2 \mu \\ &= \bar{\pi}^2 \sum_{i=1}^N \frac{1 - \pi_i}{\pi_i} y_i + N^2 \bar{\pi}^2 \mu \\ &= \bar{\pi}^2 \sum_{i=1}^N \frac{y_i}{\pi_i} - N\bar{\pi}^2 \mu + N^2 \bar{\pi}^2 \mu \end{aligned}$$

$$\mathbb{E}[Z^2] = Var(Z) + \mathbb{E}[Z]^2 \quad (14)$$

$$\begin{aligned} &= \sum_{i=1}^N \frac{\bar{\pi}^2}{\pi_i^2} var(S_i) + N^2 \bar{\pi}^2 \\ &= \bar{\pi}^2 \sum_{i=1}^N \left(\frac{1}{\pi_i} - 1 \right) + N^2 \bar{\pi}^2 \\ &= \bar{\pi}^2 \sum_{i=1}^N \frac{1}{\pi_i} - N\bar{\pi}^2 + N^2 \bar{\pi}^2 \end{aligned}$$

By substituting Equations 11, 13 and 14 in Equation 12 we can simplify the approximate bias, rendering our desired result.

$$\begin{aligned}
\mathbb{E}[\hat{y}_H] - \mu &\doteq -\frac{1}{(N\bar{\pi})^2} \left(\bar{\pi}^2 \sum_{i=1}^N \frac{y_i}{\pi_i} - N\bar{\pi}^2\mu + N^2\bar{\pi}^2\mu \right. \\
&\quad \left. - N^2\bar{\pi}^2\mu + N^2\bar{\pi}^2\mu \right. \\
&\quad \left. - \mu\bar{\pi}^2 \sum_{i=1}^N \frac{1}{\pi_i} + N\bar{\pi}^2\mu + N^2\bar{\pi}^2 \right) \\
&= -\frac{1}{N\bar{\pi}} \left(\bar{\pi} \frac{1}{N} \sum_{i=1}^N \frac{y_i}{\pi_i} - \mu\bar{\pi} \frac{1}{N} \sum_{i=1}^N \frac{1}{\pi_i} \right) \\
&= -\frac{1}{\mathbb{E}[n]} \left(\bar{\pi} \frac{1}{N} \sum_{i=1}^N \frac{y_i - \mu}{\pi_i} \right) \\
&= -\frac{1}{\mathbb{E}[n]} \left(\frac{1}{N} \sum_{i=1}^N (y_i - \mu) \frac{\bar{\pi}}{\pi_i} \right).
\end{aligned}$$

We then use the relation

$$\text{Cov}[A, B] = \mathbb{E}[(A - \bar{A})(B - \bar{B})] = \mathbb{E}[(A - \bar{A})B] - \mathbb{E}[(A - \bar{A})\bar{B}] = \mathbb{E}[(A - \bar{A})B]$$

to get our final covariance formulation.

We have ignored a mild technical issue of an undefined estimator with probability $\mathbf{P}\{Z = 0\}$. For the Poisson selection scheme, with the S_i independent, $\mathbf{P}\{Z = 0\} = \prod (1 - \pi_i)$ which will be exponentially small in n . Letting the estimator be defined as 0 under this circumstance gives a bounded, exponentially small term far less in magnitude than other bias terms.

Bias of SATE

To see that $\hat{\tau}_{SATE}$ is a biased estimate for PATE, assume fixed sample size n to obtain:

$$\begin{aligned}\mathbb{E}[\hat{\tau}_{SATE}] &= \mathbb{E}_{\mathcal{S}}[\mathbb{E}[\hat{\tau}_{SATE}|\mathcal{S}]] = \mathbb{E}_{\mathcal{S}}[\tau_{\mathcal{S}}] = \mathbb{E}_{\mathcal{S}}\left[\frac{1}{n} \sum_{i=1}^N S_i(y_i(1) - y_i(0))\right] \\ &= \frac{1}{N} \sum_{i=1}^N \frac{N\pi_i}{n} (y_i(1) - y_i(0)).\end{aligned}$$

For a random sample size, we still have a bias term. However, we can see that the above is a first order approximation of that bias by replacing $\mathbb{E}_{\mathcal{S}}[S_i/n]$ with $\mathbb{E}_{\mathcal{S}}[S_i]/\mathbb{E}_{\mathcal{S}}[n]$. The difference in these terms is of order $1/n$, as with our bias lemma.

The double-Hàjek as Weighted OLS

In Section 4.1 we introduced the “double-Hàjek” estimator. Here we will show that this estimate is equivalent to a weighted OLS where the weights are $w_i = \frac{\pi}{\pi_i}$ and we regress on the treatment indicator. In other words we fit the model

$$y_i = \alpha + \tau T_i + \varepsilon_i$$

with weights w_i for our sample $\{i \in S\}$. The weighted OLS estimates $\hat{\alpha}$ and $\hat{\tau}$ are the solutions to the normal equations:

$$\sum_{i \in S} w_i (y_i - \hat{\alpha} - \hat{\tau} T_i) = 0, \tag{15}$$

$$\sum_{i \in S} w_i T_i (y_i - \hat{\alpha} - \hat{\tau} T_i) = 0. \tag{16}$$

These are gotten by taking derivatives with respect to α and τ of the weighted sum of squares, $\sum_{i \in S} w_i (y_i - \alpha - \tau T_i)^2$, and setting them to 0. Grouping by treatment indicators, we get the

following

$$\begin{aligned} \sum_{i:T_i=1} w_i(y_i - \hat{\alpha} - \hat{\tau}) + \sum_{i:T_i=0} w_i(y_i - \hat{\alpha}) &= 0, \\ \sum_{i:T_i=1} w_i(y_i - \hat{\alpha} - \hat{\tau}) &= 0. \end{aligned}$$

Taking the difference of these equations implies that

$$\hat{\alpha} = \frac{\sum_{i:T_i=0} w_i y_i}{\sum_{i:T_i=0} w_i}.$$

To make the connection to the “double-Hàjek” estimate, denote $Z_0 = \sum_{i:T_i=0} w_i$ and $Z_1 = \sum_{i:T_i=1} w_i$, as before. If we distribute the summation in the second normal equation (Equation 16), we get

$$\begin{aligned} \sum_{i:T_i=1} w_i y_i - \hat{\alpha} Z_1 - \hat{\tau} Z_1 &= 0 \\ \sum_{i:T_i=1} w_i y_i - \frac{Z_1}{Z_0} \sum_{i:T_i=0} w_i y_i - \hat{\tau} Z_1 &= 0 \\ \hat{\tau} &= \frac{1}{Z_1} \sum_{i:T_i=1} w_i y_i - \frac{1}{Z_0} \sum_{i:T_i=0} w_i y_i \end{aligned}$$

Written in the most general sense and replacing the weights, we get back our “double-Hàjek” estimate.

$$\hat{\tau}_{hh} = \frac{1}{Z_1} \sum_{i=1}^N S_i T_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{Z_0} \sum_{i=1}^N S_i (1 - T_i) \frac{\bar{\pi}}{\pi_i} y_i(0)$$

Hence one way of calculating $\hat{\tau}_{hh}$ is by fitting a weighted OLS regression onto the treatment indicator and inspecting the coefficients.

Properties of $\hat{\tau}_{hh}$

Our estimator can be expressed as

$$\begin{aligned}\hat{\tau}_{hh} &= \frac{1}{Z_1} \sum_{i=1}^N S_i T_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{Z_0} \sum_{i=1}^N S_i (1 - T_i) \frac{\bar{\pi}}{\pi_i} y_i(0) \\ &= \hat{\mu}(1) - \hat{\mu}(0).\end{aligned}$$

For expectation, we have

$$\begin{aligned}\mathbb{E}[\hat{\tau}_{hh}|\mathcal{S}] &\approx \mathbb{E}\left[\frac{1}{\mathbb{E}[Z_1|\mathcal{S}]} \sum_{i=1}^N S_i T_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{\mathbb{E}[Z_0|\mathcal{S}]} \sum_{i=1}^N S_i (1 - T_i) \frac{\bar{\pi}}{\pi_i} y_i(0) | \mathcal{S}\right] \\ &= \frac{1}{Z} \sum_{i=1}^N S_i \frac{\bar{\pi}}{\pi_i} y_i(1) - \frac{1}{Z} \sum_{i=1}^N S_i \frac{\bar{\pi}}{\pi_i} y_i(0) = \nu_{\mathcal{S}}\end{aligned}$$

For variance we use results and notation from [Särndal, Swensson and Wretman \(2003\)](#) to obtain approximate variance terms.

Define $\tilde{S}_i = S_i T_i$ as the event of unit i being selected and also treated. We then have $\tilde{\pi}_i = \mathbb{E}[\tilde{S}_i] = p\pi_i$ and the probability that units j and k are both selected and treated is

$$\tilde{\pi}_{jk} = \mathbb{E}[\tilde{S}_j = 1 \text{ and } \tilde{S}_k = 1] = \mathbf{P}\{T_j = 1 \text{ and } T_k = 1 | S_j = 1, S_k = 1\} \pi_{jk}$$

For the treatment group specifically we have

$$\hat{\mu}(1) = \frac{\bar{\pi} \sum_{i=1}^N S_i T_i \frac{y_i(1)}{p\pi_i}}{\bar{\pi} \sum_{i=1}^N S_i T_i \frac{1}{p\pi_i}} = \frac{\sum_{i=1}^N S_i T_i \frac{y_i(1)}{p\pi_i}}{\sum_{i=1}^N S_i T_i \frac{r_i}{p\pi_i}} = \frac{\sum_{\tilde{S}} \check{y}_i}{\sum_{\tilde{S}} \check{r}_i} = \frac{\hat{t}_y}{\hat{t}_r}.$$

with $r_i = 1$. The check notation denotes a value divided by its sampling probability: $\check{a}_i = a_i/\pi_i$.

The above is a classic ratio estimator with selection probabilities of $\tilde{\pi}_j$ for the ratio of

$$R = \frac{t_y}{t_r} = \frac{\sum_{i=1}^N y_i(1)}{\sum_{i=1}^N r_i} = \frac{\sum_{i=1}^N y_i(1)}{N} = \mu(1)$$

since $t_r = \sum_{i=1}^N r_i = N$.

The approximate variance of a ratio estimator ([Särndal, Swensson and Wretman, 2003](#)) is:

$$\begin{aligned} AV(\hat{\mu}(1)) &= \frac{1}{t_r^2} \sum_{j=1}^N \sum_{k=1}^N \tilde{\Delta}_{jk} \frac{y_j(1) - Rr_j}{\tilde{\pi}_j} \frac{y_k(1) - Rr_k}{\tilde{\pi}_k} \\ &= \frac{1}{N^2} \sum \sum \tilde{\Delta}_{jk} \frac{y_j(1) - \mu(1)}{p\pi_j} \frac{y_k(1) - \mu(1)}{p\pi_k} \\ &= \frac{1}{N^2} \sum \sum \frac{\tilde{\Delta}_{jk}}{p^2\pi_j\pi_k} (y_j(1) - \mu(1)) (y_k(1) - \mu(1)) \end{aligned}$$

with

$$\tilde{\Delta}_{jk} \equiv \tilde{\pi}_{jk} - \tilde{\pi}_j\tilde{\pi}_k = \tilde{\pi}_{jk} - p^2\pi_j\pi_k.$$

We can estimate this variance with a sum over the treatment group of

$$\hat{V}(\hat{\mu}(1)) = \frac{1}{\hat{N}^2} \sum_{j=1}^N \sum_{k=1}^N S_j T_j S_k T_k \frac{\tilde{\Delta}_{jk}}{\tilde{\pi}_{jk} p^2 \pi_j \pi_k} (y_j(1) - \hat{\mu}(1)) (y_k(1) - \hat{\mu}(1))$$

with $\hat{\mu}(1) = \frac{1}{\hat{N}} \sum_{i=1}^N S_i T_i y_i(1)$ and $\hat{N} = \sum S_i T_i / \pi_i p$.

The Poisson-Bernoulli Model. Under Poisson selection we have $\pi_{jk} = \pi_j\pi_k$ for $j \neq k$ (with $\pi_{jj} = \pi_j$). With Bernoulli assignment we have $\tilde{\pi}_{jk} = p^2\pi_j\pi_k$ for $j \neq k$ (with $\tilde{\pi}_{jj} = p\pi_j$) giving

$\tilde{\Delta}_{jk} = 0$ for $j \neq k$ and $\tilde{\Delta}_{jj} = p\pi_j(1 - p\pi_j)$ for $j = k$. This gives

$$AV(\hat{\mu}(1)) = \frac{1}{N^2} \sum_{j=1}^N \frac{1 - p\pi_j}{p\pi_j} (y_j(1) - \mu(1))^2$$

and

$$\hat{V}(\hat{\mu}(1)) = \frac{1}{\hat{N}^2} \sum_{j=1}^N S_j T_j \frac{1 - p\pi_j}{p^2 \pi_j^2} (y_j(1) - \hat{\mu}(1))^2.$$

The above formula are problematic in that they depend on our π_j rather than the sampling weights $w_j = \bar{p}_i/\pi_j$). However, if we assume $N \gg n$ we can make progress. In particular, in this case, under mild regularity conditions on the sampling probabilities, we can assume $\pi_j \ll 1$ for all j . This means that $1 - p\pi_j \approx 1$. Couple this with $N\bar{\pi} = \mathbb{E}[n]$ to get a fairly tight upper bound on our two formula of

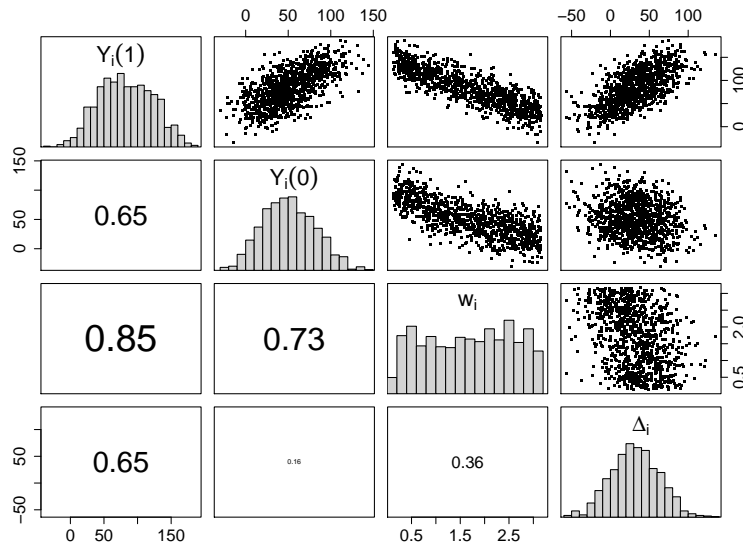
$$AV(\hat{\mu}(1)) \leq \frac{1}{p \mathbb{E}[n]} \frac{1}{N} \sum_{j=1}^N w_j (y_j(1) - \mu(1))^2$$

and, using $\hat{N} = Z_1/(\bar{\pi}p)$ with $Z_1 = \sum S_j T_j w_j$,

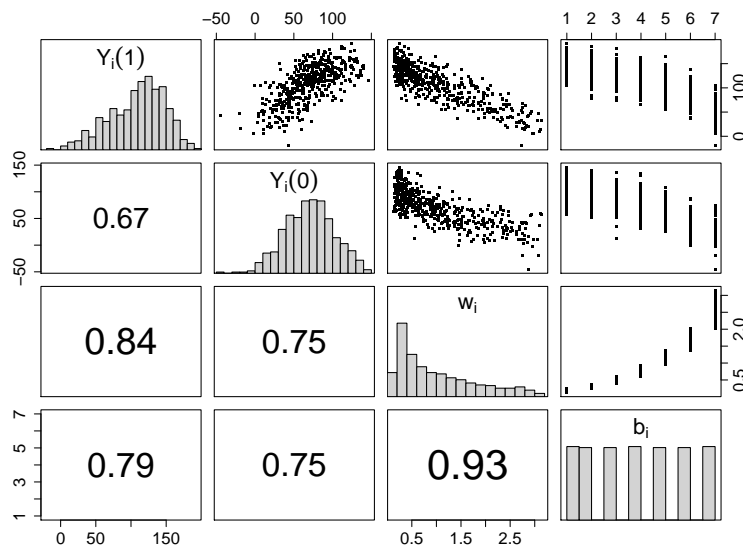
$$\begin{aligned} \hat{V}(\hat{\mu}(1)) &= \frac{\bar{\pi}^2}{Z_1^2} \sum_{j=1}^N S_j T_j \frac{1 - p\pi_j}{\pi_j^2} (y_j(1) - \hat{\mu}(1))^2 \\ &\leq \frac{1}{Z_1^2} \sum_{j=1}^N S_j T_j w_j^2 (y_j(1) - \hat{\mu}(1))^2. \end{aligned}$$

Finally, to get overall variance presented in Theorem 4.1 we first view the sample into the treatment arm as independent of the sample into the control arm, which is again motivated by the $N \gg n$ assumption. For the control arm, we then do the above derivation with $\tilde{S}_i = S_i(1 - T_i)$ and $\tilde{\pi}_i = (1 - p)\pi_i$. More lengthy derivations that account for the dependence structure will give higher-order terms which are in the end negligible. See Wood (2008) for an approach.

Appendix C: The simulation's DGP



(a) Population characteristics for Simulation A where the heterogeneous treatment effect varies in connection to the sampling weight.



(b) Characteristics of a sample from Simulation A. b_i is the post-stratification generated on this particular sample.

Figure 3: Characteristics of the Population and a Sample from Simulation A

In this section we provide additional simulation details and explanations of some of the choices

made throughout the simulations of Section 6.

To generate our populations we use the following algorithm: let $\gamma \in [0, 1]$ be a correlation measuring the strength of the relationship between the weights and outcomes. We then generate two latent parameters $(\varepsilon_i, \tilde{\varepsilon}_i)$ as a bivariate standard normal draw with correlation γ . (We do this by generating $\varepsilon_i \sim N(0, 1)$, and $\tilde{\varepsilon} = \gamma\varepsilon_i + \sqrt{1 - \gamma^2}\eta_i$, with $\eta_i \sim N(0, 1)$.)

We then generate uniformly distributed weights on pre-specified interval (a, b) by using the c.d.f. transformation:

$$w_i = a + b \Phi(\varepsilon_i),$$

where Φ is the standard normal c.d.f. We also generate shadow weights

$$\tilde{w}_i = a + b \Phi(\tilde{\varepsilon}_i),$$

also uniform, and with the same distribution as w_i .

Our potential outcomes are then a function of the shadow weights \tilde{w}_i :

$$Y_i(0) = 120 - 20\sqrt{\tilde{w}_i} + 5\epsilon_i$$

$$Y_i(1) = Y_i(0) + 10\sqrt{b - \tilde{w}_i}$$

with ϵ_i as independent Gaussian noise. The treatment potential outcomes are generated to give a non-linear heterogeneous treatment effect. When $\gamma = 1$, $\tilde{w}_i = w_i$, giving the strongest possible relationship between outcome and weight. Conversely when $\gamma = 0$ the weights are completely unrelated to the potential outcomes, so stratifying on them should not help improve estimation.

Once we have a population, we then sample inversely proportional to the weight w_i . For example, in Simulation A we take a fixed sample size of $n = 500$ (5% of the population). Our post-stratification estimator stratifies based on the weight w_i to increase precision. The stratifying variable b_i is defined in Section 5.

Simulation A has maximal covariance, with $\gamma = 1$. Figure 3 shows a subset of the population and a sample from this scenario to illustrate the structure of our DGP. Figure 3a shows the characteristics of the simulated population while Figure 3b shows how a weighted sample might look.

Overall, Figure 3 shows that the weight w_i and potential outcome distributions differ in the sample and population. Furthermore, because the potential outcomes are related to the weights they are consequently related to the post-stratification levels b_i in the sample.

For Simulation B we simply replace the formula for $Y_i(1)$ with a constant treatment effect of 30, so $Y_i(1) = Y_i(0) + 30$. We still have the sample general relationships between the sample and population, but as we see in Section 6 the estimators behave quite differently.

For Simulation C we varied γ , which controls the relationship between the weight and the potential outcomes. The top two right-most panels of Figure 3b show there is smaller variability within strata for $Y_i(0)$ and $Y_i(1)$ than if we consider the entire sample at once. As our weights become less predictive of outcome, this variability will increase. Our formulation, however, maintains the marginal distributions of w_i , $Y_i(0)$, and $Y_i(1)$ as γ changes so that any benefits we see from post-stratification can only be attributed to the changing relationship.