Testing weak nulls in matched observational studies

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Abstract

We develop sensitivity analyses for weak nulls in matched observational studies while allowing unit-level treatment effects to vary. In contrast to inference in randomized experiments, we show for general matched designs that over a large class of test statistics, any valid sensitivity analysis for the weak null must be unnecessarily conservative if Fisher’s sharp null of no treatment effect for any individual also holds. We present a sensitivity analysis valid for the weak null, and illustrate why it is conservative if the sharp null holds through connections to inverse probability weighted estimators. An alternative procedure is presented that is asymptotically sharp if treatment effects are constant, and is valid for the weak null under additional assumptions which may be deemed reasonable by practitioners. Simulation studies demonstrate that this alternative procedure results in a valid sensitivity analysis for the weak null hypothesis under many reasonable data-generating processes. With binary outcomes, a modification of the alternative procedure results in a test that is valid over the entirety of the weak null without further restrictions. The methods may be applied to matched observational studies constructed using any optimal without-replacement matching algorithm, allowing practitioners using even the most flexible forms of matching to assess robustness to hidden bias while allowing for treatment effect heterogeneity.

1 Introduction

Matching provides an appealing framework for inferring treatment effects in observational studies. Through the solution of an optimization problem, matching partitions individuals who have self-selected into treatment or control into matched sets on the basis of observed covariate information. In so doing, one tries to create a fair comparison between treatment and control individuals, with the hope that after matching observed discrepancies in outcomes may be attributed to differences in treatment status rather than differences in confounding factors. Should the observational study be free of hidden bias, a successful application of matching would justify modes of inference valid for finely stratified experiments (Fogarty, 2018). Matching throws into stark relief the fundamental differences between randomized experiments and observational studies: in randomized experiments such modes of inference are valid by design, whereas in observational studies the same modes of inference require an assumption that is both untestable and untenable.

In a sensitivity analysis, the practitioner assesses the magnitude of hidden bias that would be required to overturn an observational study’s finding of a treatment effect. Studies vary considerably their sensitivity to hidden bias: smoking causing lung cancer is an extremely robust finding, while it would not take much in the way of hidden bias to suggest that coffee does not increase the risk of heart attacks despite the two being associated (Rosenbaum, 2002, §4). Methods for sensitivity analysis in matched observational studies have traditionally focused on tests of sharp

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null hypotheses, i.e. hypotheses which impute the missing values for the potential outcomes. Perhaps the most famous sharp null, Fisher’s sharp null, states that the treatment has no effect for any individual. The restrictiveness of this null hypothesis has been a point of contention in the analysis of both randomized experiments and observational studies, with researchers in many domains instead desiring inference for Neyman’s weak null that the average of the treatment effects equals zero (Neyman, 1935). See Sabbaghi and Rubin (2014) and Wu and Ding (2018) for more on arguments surrounding tests of sharp versus weak nulls.

The observed treated-minus-control difference in means has an expectation equal to the zero under both the sharp and weak null in a completely randomized design; however, the variance for the difference in means computed under the assumption of the sharp null may be too large or too small if instead only the weak null holds (Ding, 2017; Loh et al., 2017). A single mode of inference that is both exact for the sharp null and asymptotically correct for the weak null is attained by instead employing a studentized randomization distribution. Rather than permuting the difference in means, one instead permutes the difference in means divided by a suitable standard error estimator. Fundamentally, studentization only succeeds in unifying the modes of inference in randomized experiments because the expectations are equal under both null hypotheses. In an observational study, the treated-minus-control difference in means under either the sharp or weak null in the presence of hidden bias. When conducting a sensitivity analysis under a sharp null, one calculates the worst-case expectation for the test statistic by employing the asymptotically separable algorithm of Gastwirth et al. (2000), reviewed in §3. This calculation makes explicit use of the sharp null hypothesis, and one may be concerned that the worst-case expectation when allowing for heterogeneous effects would be materially larger than that under the sharp null. Were this the case, studentization alone would be insufficient for aligning the methods for sensitivity analysis.

Fogarty (2019) shows that for paired observational studies using the difference in means, the worst-case expectation assuming the sharp null also bounds the worst-case expectation under the weak null for any pattern of heterogeneous treatment effects. Once again, an appropriate studentization then creates a unified mode of inference for the sharp and weak null. That said, pair matching is but one of the matching algorithms available to practitioners, and is the least flexible of all matching algorithms. Alternatives include fixed ratio matching, variable ratio matching and full matching; see Rosenbaum (2010, §6-13) for a comprehensive overview and worked examples of various matching algorithms. These methods produce post-stratifications with a common structure: within each matched set, there is either one treated individual and many controls, or one control individual and many treated individuals, and each individual appears in at most one matched set. Of these methods, full matching makes use of the largest percentage of data while maintaining balance on observed covariates (Hansen, 2004). Rosenbaum (1991) further illustrates that full matching enjoys desirable optimality properties in terms of minimizing aggregate covariate distance without discarding individuals. In contrast, pair matching can be unnecessarily wasteful of data: if there are many more controls than treated individuals, a sizeable percentage of control individuals will be dropped from the analysis.

Given their prevalence and attractiveness, this work investigates modes of inference for flexible matched designs in the potential presence of heterogeneous treatment effects. In contrast to the paired case, we show in §4 that a sensitivity analysis based upon the difference in means and assuming constant effects does not generally bound the worst-case expectation when effects are heterogeneous for other matched designs. Moreover, for general matched designs we show in Theorem 1 that over a large class of test statistics, no method of sensitivity analysis that tightly bounds the expectation assuming constant effects can also bound the worst-case expectation over the weak null. Unlike with randomized experiments, in matched observational studies it is impossible to
unify modes of inference for the weak null and the sharp null in a satisfactory way: any sensitivity analysis that is valid for the weak null must be unduly conservative for the sharp null, and any sensitivity analysis sharply bounding the expectation under constant effects must only be valid over a subset of the weak null.

Motivated by this impossibility result, we develop two sensitivity analyses for the sample average treatment effect in general matched designs with heterogeneous effects. The first, developed in §5, is always valid for the weak null but conservative for the sharp null, while the second, developed in §6, tightly bounds the expectation under constant effects and retains validity over various interpretable restrictions on the weak null. We further explore the fundamental source of divergence between the modes of inference through connections between matching estimators and inverse probability weighted estimators. A modification unique to binary outcomes is presented in §8, where it is shown that one can explicitly bound the worst-case expectation under the weak null. Simulations presented in §9 highlight our main results, and showcase the validity of the test in §6 under many reasonable data generating models. The methods developed herein enable practitioners using observational data to assess the robustness of their findings to hidden bias while accommodating effect heterogeneity in any matched design, providing methods for sensitivity analysis when practitioners are interested in average treatment effects.

2 Notation for stratified experiments and observational studies

2.1 Notation for finely stratified designs

There are $B$ independent matched sets formed on the basis of observed pretreatment covariates, the $i$th of which contains $n_i$ individuals. There are $N = \sum_{i=1}^{B} n_i$ total individuals in the study. Each matched set contains one treated unit and $n_i - 1$ control units. Let $Z_{ij}$ be an indicator of whether or not the $j$th individual in block $i$ received the treatment, such that $\sum_{i=1}^{n_i} Z_{ij} = 1$ for all matched sets $i = 1, ..., B$. Along with a vector of measured covariates $x_{ij}$, each individual also has an unobserved covariate $0 \leq u_{ij} \leq 1$. Let $r_{Tij}$ and $r_{Cij}$ denote the potential outcomes under treatment and control respectively for individual $ij$. The observed response is $R_{ij} = r_{Tij}Z_{ij} + r_{Cij}(1 - Z_{ij})$, and the individual-level treatment effect $\tau_{ij} = r_{Tij} - r_{Cij}$ is not observable for any individual. Collect the potential outcomes, observed covariates and unobserved covariate for each individual into the set $\mathcal{F} = \{r_{Cij}, r_{Tij}, x_{ij}, u_{ij} : i = 1, ..., B; j = 1, ..., n_i\}$, the contents of which will be conditioned upon in the forthcoming developments as fixed properties of the observational study at hand. Boldface will be used to reflect lexicographically ordered column vectors. For instance, $R = (R_{11}, R_{12}, ..., R_{Bn_B})^T$, and $R_i = (R_{i1}, ..., R_{in_i})^T$.

In studies employing full matching (Hansen, 2004), there may instead be one control individual and $n_i - 1$ treated individuals in certain matched sets. All developments for matching with multiple controls extend in a straightforward way to full matching, such that for notational convenience we proceed under the assumption that $\sum_{j=1}^{n_i} Z_{ij} = 1$ for all matched sets; see Rosenbaum (2002, §4, Problem 12) for details.

2.2 Treatment assignments in experiments and observational studies

Let $\Omega = \{z : \sum_{i=1}^{B} z_{ij} = 1, i = 1, ..., B\}$ be the set of possible values of $Z = (Z_{11}, Z_{12}, ..., Z_{Bn_B})^T$ under a finely stratified design where one individual receives the treatment and $n_i - 1$ receive the control, and let $\mathcal{Z}$ denote the event $\{Z \in \Omega\}$. In a finely stratified experiment (Fogarty, 2018; Pashley and Miratrix, 2019), $\text{pr}(Z = z \mid \mathcal{F}, \mathcal{Z}) = \text{pr}(Z = z \mid \mathcal{Z}) = |\Omega|^{-1}$, and
\[ \text{pr}(Z_{ij} = 1 \mid \mathcal{F}, \mathcal{Z}) = \text{pr}(Z_{ij} = 1 \mid \mathcal{Z}) = 1/n_i, \] where the notation \(|A|\) denotes the cardinality of the set \(A\).

Before matching, individuals are assigned to treatment independently with unknown probabilities \(\pi_{ij} = \text{pr}(Z_{ij} = 1 \mid \mathcal{F})\). While one may hope that \(\pi_{ij} \approx \pi_{ij}'\) after matching, proceeding as such may produce misleading inference due to the potential presence of unmeasured confounding. The model of Rosenbaum (2002, Chapter 4) states that individuals in the same matched set may differ in their odds of assignment to treatment by at most a factor of \(\Gamma\),

\[ \frac{1}{\Gamma} \leq \frac{\pi_{ij}(1 - \pi_{ij}')}{\pi_{ij}'(1 - \pi_{ij})} \leq \Gamma, \quad i = 1, \ldots, B; j, j' = 1, \ldots, n_i. \quad (1) \]

The parameter \(\Gamma\) controls the degree to which unmeasured confounding may have impacted the treatment assignment process. The value \(\Gamma = 1\) returns a finely stratified experiment, while \(\Gamma > 1\) allows the unobserved covariates to tilt the randomization distribution to a degree controlled by \(\Gamma\). Returning attention to the matched structure by conditioning on \(\mathcal{Z}\), this model is equivalent to assuming

\[ \text{pr}(Z = z \mid \mathcal{F}, \mathcal{Z}) = \frac{\exp(\gamma z^T u)}{\sum_{b \in \Omega} \exp(\gamma b^T u)} = \prod_{i=1}^{B} \frac{\exp\left(\gamma \sum_{j=1}^{n_i} z_{ij} u_{ij}\right)}{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})}, \]

where \(\gamma = \log(\Gamma)\) and \(u\) lies in \(\mathcal{U}\), the \(N\)-dimensional unit cube; see Rosenbaum (2002, §4) for a proof of this equivalence.

2.3 The sample average treatment effect

The sample average treatment effect is the average of the individual-level treatment effects for the \(N\) individuals in the study population, \(\bar{\tau} = N^{-1} \sum_{i=1}^{B} \sum_{j=1}^{n_i} \tau_{ij} = \sum_{i=1}^{B} (n_i/N) \bar{\tau}_i\). At \(\Gamma = 1\) the conventional unbiased estimator for \(\bar{\tau}_i\), the average treatment effect for individuals in block \(i\), is simply the observed difference in means between the treated and control individuals in block \(i\),

\[ \hat{\tau}_i = \sum_{j=1}^{n_i} \{Z_{ij}r_{Tij} - (1 - Z_{ij})r_{Cij}/(n_i - 1)\}. \]

An unbiased estimator for the sample average treatment effect in a finely stratified experiment is \(\hat{\tau} = \sum_{i=1}^{B} (n_i/N) \hat{\tau}_i\); however, this estimator may be substantially biased in the presence of unmeasured confounding. In what follows, it will be useful to define an additional quantity \(\delta_{ij}\) \((i = 1, \ldots, B; j = 1, \ldots, n_i)\) representing what the treated-minus-control difference in means would have been in stratum \(i\) had individual \(j\) received the treatment,

\[ \delta_{ij} = r_{Tij} - \sum_{j' \neq j} r_{Cij'}/(n_i - 1), \]

such that \(\bar{\tau}_i = \bar{\delta}_i\) and \(\hat{\tau}_i = \sum_{j=1}^{n_i} Z_{ij} \delta_{ij}\).

3 Asymptotic separability and sensitivity analysis with constant effects

Suppose interest lies in testing the null of a constant treatment effect at \(\tau_0\),

\[ H^{(\tau_0)}_F : r_{Tij} = r_{Cij} + \tau_0 \quad (i = 1, \ldots, B; j = 1, \ldots, n_i), \]
with a greater-than alternative, and consider using $\hat{\tau} - \tau_0$ as a test statistic. $H_F^{(0)}$ is Fisher’s sharp null hypothesis of no treatment effect for any individual in the study. Under $H_F^{(\tau_0)}$, the adjusted responses $R_{ij} - Z_{ij}\tau_0$ equal $r_{Cij}$, imputing the missing values for the potential outcomes. Consequently, the values $\delta_{ij} = r_{Cij} - \sum_{j'\neq j} r_{Cij'}/(n_i - 1) = R_{ij} - Z_{ij}\tau_0 - \sum_{j'\neq j}(R_{ij'} - Z_{ij'}\tau_0)/(n_i - 1)$ are known under $H_F^{(\tau_0)}$ for all $ij$.

For a particular $\Gamma \geq 1$ and vector of unmeasured confounders $\mathbf{u}$, the test statistic’s null distribution is

$$
pr_{\mathbf{u}}\{\hat{\tau} - \tau_0 \geq k \mid \mathcal{F}, \mathcal{Z}\} = \sum_{\mathbf{z} \in \Omega} \left\{ \sum_{i=1}^{B} (n_i/N) \sum_{j=1}^{n_i} z_{ij}(\delta_{ij} - \tau_0) \geq k \right\} \prod_{i=1}^{B} \exp \left( \gamma \sum_{j=1}^{n_i} z_{ij}u_{ij} \right),$$

(2)

where $\mathbb{1}(A)$ is an indicator that the condition $A$ is true. At $\Gamma = 1$ (2) is simply the proportion of treatment assignments where the test statistic is greater than or equal to $k$, returning the usual randomization inference for $\hat{\tau}$, and $\mathcal{F}$ is Fisher’s sharp null hypothesis for $\hat{\tau}$ in a finely stratified experiment. While the values $\delta_{ij} - \tau_0$ are known under $H_F^{(\tau_0)}$, for $\Gamma > 1$ (2) remains unknown because of its dependence on the unmeasured covariates $\mathbf{u}$. A sensitivity analysis using $\hat{\tau} - \tau_0$ proceeds for a particular $\Gamma$ with the intent of finding the worst-case $p$-value over all possible $\mathbf{u}$ based on the randomization distribution (2), $p^*_\Gamma(\tau_0) = \max_{\mathbf{u} \in \mathcal{U}} pr_{\mathbf{u}}\{\hat{\tau} - \tau_0 \geq \hat{\tau}^{obs} - \tau_0 \mid \mathcal{F}, \mathcal{Z}\}$, where $\hat{\tau}^{obs}$ is the observed value of $\hat{\tau}$. The practitioner then increases $\Gamma$ until the test no longer rejects the null hypothesis, finding $\inf\{\Gamma : p^*_\Gamma(\tau_0) \geq \alpha\}$. This changepoint value of $\Gamma$ attests to the study’s robustness to unmeasured confounding.

Most test statistics for $H_F^{(\tau_0)}$ including $\hat{\tau} - \tau_0$ may be written in the form $\mathbf{Z}^T \mathbf{q}$ for some $\mathbf{q} = \mathbf{q}(\mathbf{R} - Z\tau_0, \mathbf{R} - Z\tau_0)$. Under $H_F^{(\tau_0)}$, $\mathbf{q}(\mathbf{R} - Z\tau_0, \mathbf{R} - Z\tau_0)$, such that $\mathbf{q}$ is determined by the observed data under the null. Rearrange the values $q_{ij}$ in each matched set such that $q_{i1} \leq q_{i2} \leq \ldots \leq q_{in_i}$. Rosenbaum and Krieger (1990) show that for a given $\Gamma$ in (1), the maximum value for $pr_{\mathbf{u}}(\mathbf{Z}^T \mathbf{q} \geq k \mid \mathcal{F}, \mathcal{Z})$ occurs at a vector $\mathbf{u}$ of the form $u_{i1} = \ldots = u_{ia_i} = 0$ and $u_{ia_i+1} = \ldots = u_{in_i} = 1$ for some $1 \leq a_i \leq n_i - 1$. Let $\mathcal{U}^+$ denote this collection of $n_i - 1$ binary vectors. Unfortunately, this still results in $\prod_{i=1}^{B} (n_i - 1)$ candidate values for the maximizer, rendering explicit calculation $p^*_\Gamma(\tau_0)$ infeasible for general matched designs. Gastwirth et al. (2000) show that under mild conditions an asymptotically valid upper bound to (2) can be attained by instead finding the vector $\mathbf{u}_i$ in each matched set that maximizes the expectation for $\mathbf{Z}_i^T \mathbf{q}_i$. If multiple $\mathbf{u}_i$ attain the same expectation, the one among these that maximizes the variance of $\mathbf{Z}_i^T \mathbf{q}_i$ is chosen.

Define $\mu_{\Gamma_i}$ as

$$
\mu_{\Gamma_i} = \max_{\mathbf{u}_i \in \mathcal{U}_i^+} \frac{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})q_{ij}}{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})},
$$

let $\mathcal{U}_i^+ \subseteq \mathcal{U}_i^+$ be the subset of binary vectors attaining the maximal expectation $\mu_{\Gamma_i}$, and define $\nu_{\Gamma_i}$ as

$$
\nu_{\Gamma_i} = \max_{\mathbf{u}_i \in \mathcal{U}_i^+} \frac{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})q_{ij}^2}{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})} - \mu_{\Gamma_i}.
$$

The asymptotic approximation to $\max_{\mathbf{u}_i \in \mathcal{U}} pr_{\mathbf{u}}(\mathbf{Z}_i^T \mathbf{q} \geq k \mid \mathcal{F}, \mathcal{Z}, H_F^{(\tau_0)})$ returned by this procedure,
known as the separable algorithm, is

\[ 1 - \Phi \left( \frac{Z^T q - \sum_{i=1}^B \mu_i \Gamma_i}{\sqrt{\sum_{i=1}^B \nu_i \Gamma_i}} \right), \]

where \( \Phi(\cdot) \) is the cumulative distribution function of the standard normal. Importantly, this asymptotic approximation reduces an optimization problem with \( \prod_{i=1}^B (n_i - 1) \) candidate solutions to \( B \) tractable optimization problems which may be solved in isolation, each requiring enumeration of only \( n_i - 1 \) candidate solutions.

The function \( q(r_T - \tau_0, r_C) \) giving rise to \( \hat{\tau} - \tau_0 \) is

\[ q(r_T - \tau_0, r_C)_{ij} = \left( \frac{n_i}{N} \right) \left( \delta_{ij} - \tau_0 \right) - \sum_{j' \neq j} \frac{r_{Cij'}}{n_i - 1}. \]

Let \( \tilde{p}_T(\tau_0) \) be the probability in (3) when the separable algorithm is applied to this choice of \( q \) at \( \Gamma \), such that \( \tilde{p}_T(\tau_0) \) provides an asymptotic approximation to the true worst-case \( p \)-value \( p^*_T(\tau_0) \) under the sharp null \( H_F^{(\tau_0)} \). The resulting large-sample sensitivity analysis using the difference in means is

\[ \varphi^{(\tau_0)}_{DiM}(\alpha, \Gamma) = 1 \{ \tilde{p}_T(\tau_0) \leq \alpha \}, \]

and Gastwirth et al. (2000) show that \( \lim_{B \to \infty} E \{ \varphi^{(\tau_0)}_{DiM}(\alpha, \Gamma) \mid F, Z \} \leq \alpha \) under mild conditions if (1) holds at \( \Gamma \) and \( H_F^{(\tau_0)} \) is true.

4 Challenges facing randomization inference and sensitivity analysis under effect heterogeneity

4.1 Can inference assuming constant effects also apply to average effects?

Suppose interest instead lies in the null hypothesis that the sample average of the treatment effects equals some value \( \tau_0 \),

\[ H_N^{(\tau_0)} : \bar{\tau} = \tau_0, \]

while leaving the individual treatment effects \( \tau_{ij} \) \((i = 1, ..., B; j = 1, ..., n_i)\) otherwise unspecified. \( H_N^{(0)} \) reflects Neyman’s weak null hypothesis of no treatment effect on average for the individuals in the study (Neyman, 1935; Ding, 2017). \( H_N^{(\tau_0)} \) is a composite null hypothesis, and \( H_F^{(\tau_0)} \subseteq H_N^{(\tau_0)} \) is one particular element. We begin with two questions:

(Q1) Assuming \( \Gamma = 1 \) as would be the case in a finely stratified experiment, does inference assuming effects are constant at \( \tau_0 \) and using \( \hat{\tau} \) as a test statistic control the Type I error rate in the limit if instead effects are heterogeneous?

(Q2) When conducting a sensitivity analysis at \( \Gamma > 1 \), does the worst-case expectation derived in §3 assuming constant effects also bound the worst-case expectation when effects are heterogeneous and average to \( \tau_0 \)?
To illustrate the answers to these questions, we consider the following generative model. There are $B = 500$ matched sets, each containing $n_i = 5$ individuals, and the sensitivity model (1) holds at some value $\Gamma = \exp(\gamma) \geq 1$. In each matched set, exactly one individual receives the treatment and the remaining 4 receive the control. In the $m$th of $M$ iterations, potential outcomes in each matched set are drawn as

$$r_{Ci j} \mid x_i \sim \mathcal{E}(1/15); \quad r_{T i j} \mid x_i, r_{Ci j} \sim r_{Ci j} + \mathcal{E}(1/30),$$

where $\mathcal{E}(\lambda)$ is an exponential distribution with rate $\lambda$. The treatment effects $\tau_{ij} = r_{T i j} - r_{Ci j}$ are heterogeneous and follow an exponential distribution with rate 1/30, and the sample variances for the potential outcomes under treatment, under control, and for the treatment effects have expectations $E(\hat{\sigma}_{Ci}^2 \mid Z) = 15^2$; $E(\hat{\sigma}_{Ti}^2 \mid Z) = 15^2 + 30^2$; and $E(\hat{\sigma}_{\tau i}^2 \mid Z) = 30^2$ respectively. Denote by $\tilde{\gamma}(m)$ the average of the treatment effects in iteration $m$. With the potential outcomes specified, worst-case unmeasured covariates $u$ are then constructed to maximize the expectation of $\tilde{\gamma}$ when (1) holds at $\Gamma$, and we calculate the conditional assignment probabilities

$$g_{ij} := \Pr(Z_{ij} = 1 \mid \mathcal{F}, Z) = \frac{\exp(\gamma u_{ij})}{\sum_{j' = 1}^{n_i} \exp(\gamma u_{ij'})}.$$

Using these probabilities, we assign exactly one individual in each matched set to the treatment and four to the control, resulting in a vector of observed response $R^{(m)}$. Finally, we conduct a sensitivity analysis at $\Gamma$ with $\tilde{\gamma} - \tilde{\gamma}(m)$ as the test statistic and assuming constant effects despite effects actually being heterogeneous. Using the separable algorithm described in §3, we calculate a candidate worst-case $p$-value $\tilde{p}_m\{\tilde{\gamma}(m)\}$, asymptotically valid under the assumption of constant effects, and record whether or not we rejected the null.

### 4.2 Improper variance in finely stratified experiments

We first consider a finely stratified experiment under the generative model (5), such that $\Gamma = 1$ and $\Pr(Z_{ij} = 1 \mid \mathcal{F}, Z) = 1/5$ in each matched set regardless of $u$. We address (Q1) through a comparison of the true variance of $\tilde{\gamma}$ across randomizations to the variance under the assumption of constant effects employed when calculating $\hat{p}_1(\tau_0)$. In matched designs, the true variance for $\sqrt{B} \tilde{\gamma}$ converges in probability to

$$\text{plim}_{B \to \infty} B \sum_{i = 1}^{B} \left( \frac{n_i}{N} \right)^2 \left\{ \hat{\sigma}_{Ci}^2/(n_i - 1) + \hat{\sigma}_{Ti}^2 - \hat{\sigma}_{\tau i}^2/n_i \right\},$$

which equals 1001.25 in the generative model (5); see §3.1 of Fogarty (2018) for a derivation. Meanwhile, the reference distribution assuming constant effects uses a pooled variance of the treated and control individuals constructed under the incorrect assumption that the variances are equal. The variance for $\sqrt{B} \tilde{\gamma}$ used by the permutational t-test instead limits to

$$\text{plim}_{B \to \infty} B \sum_{i = 1}^{B} \left( \frac{n_i}{N} \right)^2 \left\{ \frac{(n_i - 1)\hat{\sigma}_{Ci}^2 + \hat{\sigma}_{Ti}^2}{n_i - 1} \right\},$$

which equals 506.25 in the generative model (5). Recall that $\hat{p}_1(\tau_0)$ is the large-sample $p$-value generated using the distribution of $\tilde{\gamma} - \tau_0$ under the assumption of constant effects. We then see that the limiting probability of a Type I error if we reject when $\tilde{p}_1\{\tilde{\gamma}(m)\} \leq 0.05$ is actually $\Phi(\Phi^{-1}(0.05)\sqrt{506.25}/1001.25) = 0.12$. The answer to (Q1) is negative: even at $\Gamma = 1$, inference
assuming constant effects and using \( \hat{\tau} - \tau_0 \) as the test statistic need not control the asymptotic Type I error rate.

In finely stratified experiments, a straightforward fix is available. Instead of the randomization distribution of \( \hat{\tau} - \tau_0 \), simply consider the randomization distribution for \( (\hat{\tau} - \tau_0)/\text{se}(\hat{\tau}) \) for a suitably chosen standard error estimator. In matched designs assuming \( \Gamma = 1 \), any of the standard errors derived in Fogarty (2018) can be employed towards this end. Under suitable regularity conditions on \( \mathcal{F} \), the reference distribution generated in this manner under the assumption of constant effects converges to a standard normal regardless of whether or not the effects are actually constant. The true distribution of \( (\hat{\tau} - \tau_0)/\text{se}(\hat{\tau}) \) instead converges to a normal with a variance less than or equal to one, as \( E\{\text{se}^2(\hat{\tau}) \mid \mathcal{F}, \mathcal{Z}\} \geq \text{var}(\hat{\tau} \mid \mathcal{F}, \mathcal{Z}) \). This results in a single test that is both exact for \( H_F^{(\tau_0)} \), and asymptotically correct for \( H_N^{(\tau_0)} \). See Loh et al. (2017) and Wu and Ding (2018) for related developments.

### 4.3 Improper worst-case expectation in matched observational studies

We again consider the generative model (5), but now when (1) holds at \( \Gamma = 6 \). Recall that the true values for the unmeasured confounders \( \mathbf{u} \) in our simulation equal the unmeasured confounders yielding the worst-case (largest) expectation for \( \hat{\tau} - \tau_0 \). Once these confounders have been set, treatments are assigned in each set based upon the resulting probabilities \( q_i \), leading to a vector of observed responses \( \mathbf{R}^{(m)} \). We then proceed with a sensitivity analysis at \( \Gamma = 6 \) under the incorrect assumption of constant effects, using the separable algorithm in §3 with \( q(R^{(m)} - \mathbf{Z}_{\hat{\tau}}^{(m)}, R^{(m)} - \mathbf{Z}_{\hat{\tau}}^{(m)}) \) in (4). This results in candidate worst-case expectations \( \mu_{\Gamma_i}^{(m)} \) and variances \( \nu_{\Gamma_i}^{(m)} \) for each \( i \). Because the constant effects model is actually false, the candidate worst-case expectation generated by the separable algorithm need not equal the true worst-case expectation.

Under a Gaussian limit, it must be that \( E(\hat{\tau} - \tau_0 - \sum_{i=1}^B \mu_{\Gamma_i}^{(m)} \mid \mathcal{F}, \mathcal{Z}) \leq 0 \) for the sensitivity analysis assuming constant effects to also be valid under effect heterogeneity. Figure 4.3 shows the distribution of the worst-case standardized deviated generated by asymptotic separability across \( M = 1000 \) simulations, which would be asymptotically standard normal under constant effects. While it is still roughly normal with heterogeneous effects, the mean is 2.67 and the standard deviation is 0.89. That the true expectation is substantially above zero invalidates the permutational \( t \)-based sensitivity analysis under effect heterogeneity, which proceeds as though this distribution is standard normal. When rejecting when \( \bar{p}_R(\hat{\tau}^{(m)}) \leq 0.05 \) in our simulation the estimated Type I error rate is 0.80, revealing that the answer to (Q2) is also negative.

### 4.4 Understanding the bias under effect heterogeneity

To understand what went wrong in §4.3, under \( H_N^{(\tau_0)} \) \( R_{ij} - Z_{ij} \tau_0 \) does not generally equal \( r_{C_{ij}} \) when \( Z_{ij} = 1 \). Instead, it equals \( r_{C_{ij}} + \tau_{ij} - \tau_0 \). The true value of \( \delta_{ij} - \tau_0 \) is only known for the index \( k \) such that \( Z_{ik} = 1 \) in the observational study at hand. If \( Z_{ik} = 1 \), the separable algorithm in §3 proceeding under the assumption of constant effects generates a candidate worst-case expectation in stratum \( i \) using improperly imputed values for \( \delta_{ij}^{(k)} \)

\[
\delta_{ij}^{(k)} = \begin{cases} 
\delta_{ij} - \tau_0 & j = k \\
\delta_{ij} - \tau_{ij} - (\tau_{ik} - \tau_0)/(n_i - 1) & \text{otherwise},
\end{cases}
\]

such that \( \delta_{ij}^{(k)} - \tau_0 \neq \delta_{ij} - \tau_0 \) for \( j \neq k \) under \( H_N^{(\tau_0)} \). For each \( k = 1, \ldots, n_i \), let \( q_i^{(k)} \) be the probabilities giving rise to the worst-case expectation when the separable algorithm is applied with \( Z_{ik} = 1 \), and let \( q_{\Gamma i k} = \sum_{j=1}^{n_i} q_i^{(k)} \delta_{ij}^{(k)} \) be the worst-case expectation returned by the separable algorithm.
Histogram of Candidate Worst−Case Deviate

Visualizing the Bias with Heterogeneous Effects

Figure 1: (Left) The distribution of the candidate worst-case deviate with heterogeneous effects at $\Gamma = 6$. (Right) The underlying relationship between $\varrho_{ij}^*(1 - \varrho_{ij}^*)$ and $\tau_{ij} - \tau_0$.

assuming that treatment effects are constant at $\tau_0$ when $Z_{ik} = 1$. The random variable stemming from the asymptotically separable algorithm in the $i$th set is

$$\hat{T}_{\Gamma_1}^{(\tau_0)} = \sum_{j=1}^{n_i} Z_{ij}(\delta_{ij} - \tau_0 - \vartheta_{ij})$$

and let $\hat{T}_\Gamma^{(\tau_0)} = \sum_{i=1}^{B}(n_i/N)\hat{T}_{\Gamma_1}$ be their weighted average. Let $u$ and $\varrho$ be the true vectors of unmeasured confounders and the corresponding conditional probabilities. For the separable algorithm to furnish a valid asymptotic sensitivity analysis under effect heterogeneity and under a normal approximation, it must be that $E_u(\hat{T}_\Gamma^{(\tau_0)} | F, Z) \leq 0$ when (1) holds at $\Gamma$. Unfortunately, this need not be the case.

**Proposition 1.** Suppose (1) holds at $\Gamma$ and that $H_N^{(\tau_0)}$ is true. Then,

$$E(\hat{T}_\Gamma^{(\tau_0)} | F, Z) \leq \sum_{i=1}^{B} \frac{n_i^2}{N(n_i - 1)} \sum_{j=1}^{n_i} \varrho_{ij}(1 - \varrho_{ij})(\tau_{ij} - \tau_0).$$

Moreover, there exist allocations of potential outcomes satisfying $H_N^{(\tau_0)}$ with unmeasured confounders $u$ such that $E(\hat{T}_\Gamma^{(\tau_0)} | F, Z) > 0$ when (1) holds at $\Gamma$.

Proposition 1 is proved in the web-based supporting materials. At $\Gamma = 1$, $\varrho_{ij}(1 - \varrho_{ij}) = (n_i - 1)/n_i^2$, returning the usual result that $E(\hat{T}_\Gamma^{(\tau_0)} | F, Z) = \tilde{\tau}_i - \tau_0$ in a finely stratified experiment. For a paired observational study with $n_i = 2$, at the worst-case vector of assignment probabilities $\varrho^*$ we have that $\varrho_{ij}^*(1 - \varrho_{ij}^*) = \Gamma/(1 + \Gamma)^2$ for all $ij$. As a result, the worst-case expectation is bounded above by zero under $H_N^{(\tau_0)}$ as proved in Theorem 1 of Fogarty (2019). However, for $\Gamma > 1$ and $n_i > 2$, the unit-level treatment effects may be unequally weighted by the conditional selection probabilities even under the worst-case vector of unmeasured confounding. Should these selection probabilities correlate with the individual-level treatment effects in an adverse manner, the expectation may fail to be controlled.

Figure 4.3 shows the relationship between $\varrho_{ij}(1 - \varrho_{ij}) = \varrho_{ij}^*(1 - \varrho_{ij}^*)$ and $\tau_{ij} - \tau_0$ in the simulation reported in §4.3. We see that larger values for $\varrho_{ij}^*(1 - \varrho_{ij}^*)$ correspond to larger values for $\tau_{ij} - \tau_0$. This relationship leads to the sensitivity analysis justified under $H_F^{(\tau_0)}$ failing to control the Type I error rate under the weak null.
4.5 An impossibility result for general matched designs

The previous section illustrates that a sensitivity analysis using \( \hat{\tau} - \tau_0 \) as a test statistic under the assumption of constant effects need not yield a valid sensitivity analysis if effects are instead heterogeneous. While the difference in means cannot be employed to this end, one may hope that another test statistic could achieve this objective. This is not the case. Over a large class of test statistics that are functions of the stratum-wise treated-minus-control mean differences \( \hat{\tau}_i \), a sensitivity analysis assuming constant effects cannot be relied upon to bound the worst-case expectation if the treatment effects are instead heterogeneous.

For any collection of nondecreasing, nonconstant functions \( \{ h_{\Gamma n_i} \}_{n_i \geq 2} \), there exist combinations of stratum sizes \( n_i \) (\( i = 1, \ldots, B \)), degrees of hidden bias \( \Gamma \), and values for potential outcomes satisfying \( H_N^{(\tau_0)} \) such that if (1) holds at \( \Gamma \),

\[
\max_{u \in U} \sum_{i=1}^{B} E_u \{ h_{\Gamma n_i}(\hat{\tau}_i - \tau_0) - \mu_{\Gamma}(h_{\Gamma n_i}, R_i - Z_i \tau_0) \mid F, Z \} > 0.
\]

That is, no sensitivity analysis using the worst-case expectation under \( H_F^{(\tau_0)} \) can bound the worst-case expectation under \( H_N^{(\tau_0)} \) when effects are heterogeneous for all possible combinations of stratum sizes and degrees of hidden bias.

The proof is presented in the web-based supporting materials. It is constructive, creating for any \( n_i \geq 3 \) an allocation of potential outcomes satisfying \( H_N^{(\tau_0)} \), but where for any non-decreasing, non-constant function \( h_{\Gamma n_i}(\hat{\tau}_i - \tau_0) \) there exists a \( \Gamma \) such that the worst-case expectation under \( H_F^{(\tau_0)} \) fails to bound the worst-case expectation under \( H_N^{(\tau_0)} \). The theorem shows that for a large class of test statistics, including any weighted average of treated-minus-control differences in means across strata, it is in general impossible to devise an upper bound for the sum of expectations over all elements of the composite null \( H_N^{(\tau_0)} \) for general matched designs.

Theorem 1. For any collection of nondecreasing, nonconstant functions \( \{ h_{\Gamma n_i} \}_{n_i \geq 2} \), there exist combinations of stratum sizes \( n_i \) (\( i = 1, \ldots, B \)), degrees of hidden bias \( \Gamma \), and values for potential outcomes satisfying \( H_N^{(\tau_0)} \) such that if (1) holds at \( \Gamma \),

\[
\max_{u \in U} \sum_{i=1}^{B} E_u \{ h_{\Gamma n_i}(\hat{\tau}_i - \tau_0) - \mu_{\Gamma}(h_{\Gamma n_i}, R_i - Z_i \tau_0) \mid F, Z \} > 0.
\]

4.6 Towards sensitivity analysis for weak nulls

Suppose that (1) holds at \( \Gamma \) and that \( \bar{\tau} = \tau_0 \), such that \( H_N^{(\tau_0)} \) is true. Let \( L_\Gamma^{(\tau_0)} = B^{-1} \sum_{i=1}^{n} L_{\Gamma i}^{(\tau_0)} \) be any candidate test statistic, and let \( u \) be the true, but unknowable, vector of unmeasured
confounders. Under suitable regularity conditions on \( \mathcal{F} \) a central limit theorem would apply to \( L^{(\tau_0)}_\Gamma \), such that for any \( k \)

\[
\lim_{B \to \infty} \Pr \left( \frac{L^{(\tau_0)}_\Gamma - E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})}{\text{var}_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})^{1/2}} \geq k \right) = 1 - \Phi(k). \tag{6}
\]

The deviate within (6) cannot be used in practice, as the expectation and variance both depend upon the unmeasured confounders and the unknown potential outcomes. A straightforward observation provides that when \( L^{(\tau_0)}_\Gamma > 0 \),

\[
\frac{L^{(\tau_0)}_\Gamma - E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})}{\text{var}_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})^{1/2}} \geq \frac{L^{(\tau_0)}_\Gamma - \max_{u \in U} E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})}{\text{var}_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})^{1/2}}. \tag{7}
\]

The right-hand side of (7) simply replaces the true but unknowable expectation with its worst-case value at a given \( \Gamma \). When testing the sharp null \( H_F^{(\tau_0)} \), the separable algorithm of Gastwirth et al. (2000) can be used to construct this worst-case expectation. Unfortunately, this explicit construction requires assuming a sharp null so that the unknown potential outcomes are imputed. When testing a weak null hypothesis, the missing potential outcomes remain unspecified, such that the worst-case expectation cannot be calculated. Nonetheless, we will show that while it cannot be explicitly calculated, with a smart choice of test statistic the worst-case expectation \( \max_{u \in U} E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z}) \) may itself be bounded over the weak null \( H_N^{(\tau_0)} \) in a way that facilitates an implementable sensitivity analysis.

Theorem 1 presents two roads, diverging, both of which will be traveled. In §5, we present a test statistic whose worst-case expectation is tightly bounded above by zero under the weak null. We then illustrate precisely why this bound is conservative if effects are instead constant. In §6, we present an alternative test statistic whose expectation is tightly bounded above by zero over a subset of the weak null which may be viewed as reasonable by practitioners.

Even if we knew the worst-case expectation in the right-hand side of (7), the deviate could not be deployed in practice due to the unknown variance. We show in §7.1 that the observed data can be used to construct a conservative variance estimator, despite the true variance depending on both the missing potential outcomes and the unmeasured confounders. That is, for the statistics in §§5-6 we construct a sample-based standard error \( \text{se}(L^{(\tau_0)}_\Gamma) \) such that \( E\{\text{se}^2(L^{(\tau_0)}_\Gamma) \mid \mathcal{F}, \mathcal{Z}\} \geq \text{var}_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z}) \), and such that the squared standard error, suitably scaled, converges in probability to its expectation under mild conditions. That is, for \( L^{(\tau_0)}_\Gamma > 0 \) and using the squared standard error’s expectation,

\[
\frac{L^{(\tau_0)}_\Gamma - E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})}{\text{var}_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})^{1/2}} \geq \frac{L^{(\tau_0)}_\Gamma - \max_{u \in U} E_u(L^{(\tau_0)}_\Gamma \mid \mathcal{F}, \mathcal{Z})}{E\{\text{se}^2(L^{(\tau_0)}_\Gamma) \mid \mathcal{F}, \mathcal{Z}\}^{1/2}}. \tag{8}
\]

A deviate replacing the worst-case expectation in the right-hand side of (8) with a computable upper bound and replacing \( E\{\text{se}^2(L^{(\tau_0)}_\Gamma) \mid \mathcal{F}, \mathcal{Z}\}^{1/2} \) with the estimator \( \text{se}(L^{(\tau_0)}_\Gamma) \) would have its upper tail probability bounded above by a standard normal in the limit, in turn facilitating an asymptotically valid sensitivity analysis for \( H_N^{(\tau_0)} \).
5 A valid sensitivity analysis with heterogeneous effects

5.1 Interval restrictions on the stratum-wise assignment probabilities

For the time being, consider a further restriction on the allowed assignment probabilities in (1). Imagine that for each stratum \(i\) and for any \(\Gamma\), there exist known constants \(\kappa_{\Gamma_i}\) (\(i = 1, ..., B\)) such that

\[
\kappa_{\Gamma_i}^{-1} \leq \varrho_{ij} \leq \Gamma \kappa_{\Gamma_i}^{-1}.
\] (9)

The restriction (9) holding at \(\Gamma\) implies that Rosenbaum’s model (1) also holds at \(\Gamma\); however, the converse is not true for pre-specified values \(\kappa_{\Gamma_i}\). See Aronow and Lee (2012) and Miratrix et al. (2017) for a related model for finite population sampling with unknown but bounded probabilities of sample selection.

Suppose interest lies in the null hypothesis \(H_{N}^{(\tau_0)}\) with (9) holding at \(\Gamma\), and consider the random variable

\[
\hat{D}_{\Gamma_i}^{(\tau_0)} = \hat{\tau}_i - \tau_0 - \left(\frac{\Gamma - 1}{1 + \Gamma}\right) |\hat{\tau}_i - \tau_0|.
\] (10)

\(\hat{D}_{\Gamma_i}^{(\tau_0)}\) is precisely the treated-minus-control mean difference in stratum \(i\), subtracted by the worst-case expectation under a paired design for \(H_{F}^{(\tau_0)}\) when (1) holds at \(\Gamma\). Fogarty (2019) demonstrated that \(\hat{D}_{\Gamma_i}^{(\tau_0)}\) can be used to construct an asymptotically valid sensitivity analysis for \(H_{N}^{(\tau_0)}\) in paired observational studies. Here we show the importance of this random variable in general matched designs with heterogeneous effects.

**Proposition 2.** Suppose (9) holds at \(\Gamma\) for known values \(\kappa_{\Gamma_i}\) (\(i = 1, ..., B\)). Then,

\[
E(\hat{D}_{\Gamma_i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z}) \leq n_i \left(\frac{2\Gamma}{1 + \Gamma}\right) \frac{\hat{\tau}_i - \tau_0}{\kappa_{\Gamma_i}}.
\]

Consider the weighted average \(K_{\Gamma_i}^{(\tau_0)} = N^{-1} \sum_{i=1}^{B} \kappa_{\Gamma_i} \hat{D}_{\Gamma_i}^{(\tau_0)}\). From Proposition 2 we see \(E(K_{\Gamma_i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z}) \leq 2\{\Gamma/(1 + \Gamma)\}(\hat{\tau} - \tau_0)\) if (9) holds at \(\Gamma\) for known constants \(\kappa_{\Gamma_i}\), which equals zero under \(H_{N}^{(\tau_0)}\). Test statistics of the form \(K_{\Gamma_i}^{(\tau_0)}\) can control the worst-case expectation with heterogeneous effects under interval restrictions of the form (9).

5.2 A connection with inverse probability weighted estimators

We now draw connections between the random variable \(\hat{D}_{\Gamma_i}^{(\tau_0)}\) and an inverse probability weighted (IPW) estimator under the restriction (9). Consider the random variable

\[
W_{\Gamma_i}^{(\tau_0)} = \min_{\kappa_{\Gamma_i}^{-1} \leq p_i \leq \Gamma \kappa_{\Gamma_i}^{-1}} \left(\frac{\hat{\tau}_i - \tau_0}{p_i}\right),
\]

which weights \(\hat{\tau}_i - \tau_0\) in the worst possible way for inference with a greater-than alternative under the interval restriction (9). If we had access to the true treatment assignment probabilities \(\varrho_i\), classical results on IPW estimators (Horvitz and Thompson, 1952) show

\[
E \left\{ \frac{\hat{\tau}_i - \tau_0}{\sum_{j=1}^{n_i} Z_{ij} \varrho_{ij}} | \mathcal{F}, \mathcal{Z} \right\} = \hat{\tau}_i - \tau_0.
\]
As we do not have access to $\varrho_i$ due to its dependence on the unmeasured confounders $u_i$, the statistic $W_i^{(\tau_0)}$ instead weights $(\hat{\tau}_i - \tau_0)$ by the worst-case conditional probability under (9), weighting by the largest probability if $\hat{\tau}_i - \tau_0 \geq 0$ and by the smallest probability when $\hat{\tau}_i - \tau_0 < 0$, resulting in an expectation no larger than $\hat{\tau}_i - \tau_0$. In the web-based supporting materials, we further show

$$W_i^{(\tau_0)} = \frac{\kappa_i}{n_i} \left( \frac{1 + \Gamma}{2\Gamma} \right) \hat{D}_i^{(\tau_0)}.$$  \hspace{1cm} (11)

This connection to an IPW estimator provides intuition for why $\hat{D}_i^{(\tau_0)}$ can be used to control the worst-case expectation in arbitrary matched designs, despite its functional form having been motivated by sensitivity analyses in paired designs.

### 5.3 A valid sensitivity analysis for the sample average treatment effect under Rosenbaum’s model

Suppose instead one wants to perform a sensitivity analysis for the sample average treatment effect without imposing a particular interval restriction as in (9). That is, one simply desires a sensitivity analysis for the sample average treatment effect under the assumption that (1) holds at $\Gamma$. For a given stratum size $n_i$, under (1) the resulting conditional assignment probabilities are bounded as

$$\frac{1}{\Gamma(n_i - 1) + 1} \leq g_{ij} \leq \frac{\Gamma}{(n_i - 1) + \Gamma}.$$  \hspace{1cm} (12)

Defining $\tilde{k}_{\Gamma n_i} = \Gamma(n_i - 1) + 1$ and $\Gamma_{n_i} = \Gamma(\Gamma(n_i - 1) + 1)/(n_i - 1 + \Gamma)$, we can express the constraint on $g_{ij}$ imposed by (1) holding at $\Gamma$ as

$$\tilde{k}_{\Gamma n_i}^{-1} \leq g_{ij} \leq \Gamma_{n_i} \tilde{k}_{\Gamma n_i}^{-1}.$$  \hspace{1cm} (12)

For matched pairs, $\Gamma_{n_i} = \Gamma$; however, for $n_i > 2$, $\Gamma_{n_i} > \Gamma$ if $\Gamma > 1$, and in fact $\Gamma_{n_i} \to \Gamma^2$ as $n_i \to \infty$. Consider the random variable

$$\tilde{D}_i^{(\tau_0)} = N^{-1} \sum_{i=1}^{B} \tilde{k}_{\Gamma n_i} \hat{D}_i^{(\tau_0)},$$

where

$$\hat{D}_i^{(\tau_0)} = \hat{\tau}_i - \tau_0 - \left( \frac{\Gamma_{n_i} - 1}{1 + \Gamma_{n_i}} \right) |\hat{\tau}_i - \tau_0|.$$  \hspace{1cm} (12)

**Theorem 2.** Suppose (1) holds at $\Gamma$ and that $H_i^{(\tau_0)}$ is true. Then

$$E(\tilde{D}_i^{(\tau_0)} \mid F, Z) \leq 0,$$

such that the worst-case bias is controlled through this test statistic. Further, there exist allocations of potential outcomes within $H_i^{(\tau_0)}$ and vectors of unmeasured confounders $u$ such that $E(\tilde{D}_i^{(\tau_0)} \mid F, Z) = 0$.

Theorem 2, proved in the web-based supporting materials, demonstrates that the random variable $\tilde{D}_i^{(\tau_0)}$ has a known upper bound on the worst-case expectation when the sample average treatment effect equals $\tau_0$ and (1) holds at $\Gamma$, even when allowing for heterogeneous effects. Further, it states that the bound is tight for certain elements of $H_i^{(\tau_0)}$, such that it cannot be improved upon without further restrictions on $H_i^{(\tau_0)}$ or on the unmeasured confounders $u$. The example used to prove Theorem 1 represents an instance where the worst-case expectation equals zero.
5.4 Explaining the divergent methods for sharp and weak nulls: Incompatibility

Theorem 1 implies a necessary divergence between sensitivity analyses assuming constant effects and those allowing for heterogeneous effects. In particular, we know that because $\hat{D}_{1}^{(\tau_{0})}$ has an expectation bounded above by zero under $H_{N}^{(\tau_{0})}$ when (1) holds at $\Gamma$ by Theorem 2, it is necessary that $E(\hat{D}_{1}^{(\tau_{0})} \mid \mathcal{F}, \mathcal{Z}) < 0$ under $H_{F}^{(\tau_{0})}$ when (1) holds at $\Gamma$ unless we have a paired observational study. The following proposition helps explain why this occurs.

**Proposition 3.** Consider the random variable

$$A_{i} = \sum_{j=1}^{n_{i}} Z_{ij} \left\{ q_{ij} - \left( \frac{\Gamma - 1}{1 + \Gamma} \right) |q_{ij}| \right\},$$

where $q_{ij}$ are any constants such that $\sum_{j=1}^{n_{i}} q_{ij} = 0$. Suppose (1) holds at $\Gamma$. Then,

$$E(A_{i} \mid \mathcal{F}, \mathcal{Z}) \leq 0.$$

Moreover, $E(A_{i} \mid \mathcal{F}, \mathcal{Z}) = 0$ for $u_{ij} = 1(q_{ij} \geq 0)$.

**Proof.** For any vector of unmeasured confounders $u_{i}$,

$$E_{u_{i}}(A_{i} \mid \mathcal{F}, \mathcal{Z}) = \frac{\sum_{j=1}^{n_{i}} \exp(\gamma u_{ij}) |q_{ij} - \{(\Gamma - 1)/(1 + \Gamma)\} |q_{ij}|}{\sum_{j=1}^{n_{i}} \exp(\gamma u_{ij})} \quad (13)$$

When $q_{ij} \geq 0$, $q_{ij} - \{(\Gamma - 1)/(1 + \Gamma)\} |q_{ij}|$ equals $2q_{ij}/(1+\Gamma)$, and it equals $2\gamma q_{ij}/(1+\Gamma)$ for $q_{ij} < 0$. It is clear that the numerator in (13) is maximized by setting $u_{ij} = 1(q_{ij} \geq 0)$. Under this choice, the numerator becomes $2\gamma/(1+\Gamma) \sum_{j=1}^{n_{i}} q_{ij} = 0$ as $\sum_{j=1}^{n_{i}} q_{ij} = 0$. The choice $u_{ij} = 1(q_{ij} \geq 0)$ thus also maximizes (13). □

Under $H_{F}^{(\tau_{0})}$, we know that $\sum_{i=1}^{n_{i}} (\delta_{ij} - \tau_{0}) = 0$. Therefore, under constant effects, Proposition 3 implies that $E(\hat{D}_{1}^{(\tau_{0})} \mid \mathcal{F}, \mathcal{Z}) \leq 0$ even when (1) holds at $\Gamma_{n_{i}} = \Gamma\{n_{i} - 1\}/\{n_{i} - 1 + \Gamma\}$, which is strictly greater than $\Gamma$ for $n_{i} > 2$ and $\Gamma > 1$. Proceeding as though the worst-case expectation for $\hat{D}_{1}^{(\tau_{0})}$ is zero would be unnecessarily conservative under $H_{F}^{(\tau_{0})}$, and one could find a less conservative bound for $E(\hat{D}_{1}^{(\tau_{0})} \mid \mathcal{F}, \mathcal{Z})$ under $H_{F}^{(\tau_{0})}$ by applying the separable algorithm in §3.

Interpreting $\hat{D}_{1}^{(\tau_{0})}$ as an IPW estimator is helpful in understanding both the source of the conservativeness when assuming constant effects and the reason it cannot be overcome when effects are heterogeneous. For each $ij$, we know that value $\hat{\theta}_{ij}$ minimizing $(\delta_{ij} - \tau_{0}) / \hat{\theta}_{ij}$ subject to (12) is $\hat{\theta}_{ij} = \Gamma/(n_{i} - 1 + \Gamma)$ if $(\delta_{ij} - \tau_{0}) > 0$; $\hat{\theta}_{ij} = 1/(\Gamma(n_{i} - 1) + 1)$ if $(\delta_{ij} - \tau_{0}) < 0$; and is any feasible $\hat{\theta}_{ij}$ if $(\delta_{ij} - \tau_{0}) = 0$. Letting $\hat{\theta}_{ij}$ constructed in this manner for each matched set, we have that $\sum_{j=1}^{n_{i}} \hat{\theta}_{ij}$ need not equal 1. That is, the vector $\hat{\theta}_{i}$ need not be a valid probability distribution for $\hat{Z}_{i} \mid \mathcal{F}, \mathcal{Z}$, and it will not be when $n_{i} > 2$ so long as $\delta_{ij}$ is not constant at $\tau_{0}$ for all $j = 1, \ldots, n_{i}$.

When assuming the sharp null $H_{F}^{(\tau_{0})}$ all possible values of $\delta_{ij} - \tau_{0}$ are known based upon the observed data, such that the separable algorithm can be used to produce a worst-case expectation based upon a valid probability distribution. Under $H_{N}^{(\tau_{0})}$, only the $\delta_{ij} - \tau_{0}$ for which $Z_{ij} = 1$ in the observed study is known, leaving $n_{i} - 1$ values for $\delta_{ij} - \tau_{0}$ unknown. Despite knowing that $\hat{\theta}_{i}$ will generally not correspond to a valid probability distribution, one cannot act upon this knowledge when conducting inference for $H_{N}^{(\tau_{0})}$ without risking an anti-conservative procedure. This issue is referred to as *incompatibility* and occurs in other methods for sensitivity analysis: the worst-case unmeasured confounder may not be compatible with any probability distribution. See Zhao et al. (2019) for a related discussion in the context of sensitivity analysis for IPW estimators.

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(13)
6 A concordant mode of inference with additional restrictions

6.1 A statistic with known worst-case expectation under constant effects

Consider instead the alternative weighted average

\[ \hat{D}_{\Gamma}^{(\tau_0)} = \sum_{i=1}^{B} \left( \frac{n_i}{N} \right) \hat{D}_{\Gamma_i}^{(\tau_0)}, \] (14)

where \( \hat{D}_{\Gamma_i}^{(\tau_0)} \) is defined as in (10). This statistic weights each set’s contribution as is typical in finely stratified experiments for inference on the sample average effect, and does not modify the value of \( \Gamma \) based upon \( n_i \).

**Theorem 3.** Let \( u \) be any vector of unmeasured confounders and suppose (1) holds at \( \Gamma \). Then, if \( H_F^{(\tau_0)} \) holds,

\[ E_u(\hat{D}_{\Gamma}^{(\tau_0)} | F, Z) \leq 0, \]

and \( E_u(\hat{D}_{\Gamma}^{(\tau_0)} | F, Z) = 0 \) for \( u_{ij} = 1 \{ \delta_{ij} \geq \tau_0 \} \). If instead \( H_N^{(\tau_0)} \) holds but \( H_F^{(\tau_0)} \) does not, we have

\[ E_u(\hat{D}_{\Gamma}^{(\tau_0)} | F, Z) \leq \sum_{i=1}^{B} (n_i/N) E_{u_i}(\hat{D}_{\Gamma_i}^{(\tau_0)} | F, Z) \]

\[ + \frac{2\Gamma}{1+\Gamma} \sum_{i=1}^{B} (n_i/N) \left\{ 1 + \frac{1-\Gamma}{\Gamma} pr_{u_i}(\hat{\tau}_i \geq \bar{\tau}_i | F, Z) \right\} (\bar{\tau}_i - \tau_0), \]

where \( \sum_{i=1}^{B} (n_i/N) E_{u_i}(\hat{D}_{\Gamma_i}^{(\tau_0)} | F, Z) \leq 0. \)

That the worst-case expectation is bounded by zero under \( H_F^{(\tau_0)} \) follows from Proposition 3, while the general form is proved in the web-based supporting materials. For matched designs besides pair matching, the right-hand side of (15) under the weak null need not be less than or equal to zero even if the sample average treatment effect equals \( \tau_0 \). The inequality (15) may be strict, sometimes by a considerable margin as the simulations in §9 reveal, such that the right-hand side being larger than zero does not imply that the worst-case expectation itself lies above zero. The inequality does provide a useful necessary condition for the worst-case expectation to lie above zero.

**Corollary 1.** Let \( u \) be the true, but unknowable, vector of unmeasured confounders and let \( u^* \) be the vector of unmeasured confounders yielding the worst-case expectation. Suppose (1) holds at \( \Gamma \) and that \( \bar{\tau} = \tau_0 \). Then, a necessary condition for \( E(\hat{D}^{(\tau_0)} | F, Z) > 0 \) is that the following inequalities both hold:

\[ \text{cov}\left\{ pr_{u_i}(\hat{\tau}_i \geq \bar{\tau}_i | F, Z), n_i(\bar{\tau}_i - \tau_0) \right\} < 0, \]

\[ \text{cov}\left\{ pr_{u^*_i}(\hat{\tau}_i \geq \bar{\tau}_i | F, Z), n_i(\bar{\tau}_i - \tau_0) \right\} < 0, \]

where \( \text{cov}(\cdot) \) is the usual sample covariance.

Comparing the bounds in Theorem 3 for \( \hat{D}_{\Gamma}^{(\tau_0)} \) and Proposition 1 for \( \hat{T}_F^{(\tau_0)} \), the permutational \( t \) test, reveals a fundamental difference in the conditions required for the bias to be controlled under
the weak null. When considering a sensitivity analysis using the permutational $t$-statistic as in 
§§3-4, one would need to impose conditions on the relationship between individual-level treatment 
effects $\tau_{ij}$ and individual-level unmeasured confounders $u_{ij}$. This appears unattractive, as many 
natural patterns of unmeasured confounding such as essential heterogeneity directly link individual-
level treatment effects to their unmeasured confounders. In contrast, the assumptions required for 
the bias of $\hat{D}_{\Gamma}^{(\tau_0)}$ to be controlled pertain instead to a relationship between the average of the 
treatment effects in set $i$ and aggregate function of the unmeasured confounders in set $i$. We now 
develop further intuition for Corollary 1, and shed light upon the relationships between potential 
outcomes and hidden bias for which it may be satisfied.

6.2 Interpreting the necessary condition under a particular pattern of adversarial bias

For $n_i > 2$, a peculiar feature of the worst-case confounder $\arg\max_{u_i \in U} E_u(\hat{D}_{\Gamma_i}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})$ is its 
dependence upon the particular value for $\tau_0$ being tested. As a result, in any matched set $i$, it may 
be that $\arg\max_{u_i \in U} E_u(\hat{D}_{\Gamma_i}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) \neq \arg\max_{u_i \in U} E_u(\hat{D}_{\Gamma_i}^{(\tau_i)} \mid \mathcal{F}, \mathcal{Z})$, where $\tau_i$ is the true value for 
the sample average treatment effects in matched set $i$. When testing the composite null $H_N^{(\tau_0)}$, 
a sensitivity analysis under (1) allows nature to choose a pattern of hidden bias that is different for 
stratum $i$ from the one that would have been chosen under $H_N^{(\tau_i)}$. To allow for this suggests that 
nature had knowledge of which null hypothesis we would be testing when deciding upon the values 
of the unmeasured confounders, and that nature would have acted differently had we tested $H_N^{(\tau_0)}$ 
and $H_N^{(\tau_2)}$ for $\tau_0 \neq \tau_2$. Catering the choice of $u_i$ to the hypothesized value $\tau_0$ rather than to $\tau_i$ 
may be viewed as affording nature unrealistic clairvoyance in selecting the worst-case confounder.

Suppose instead that nature chooses the worst-case unmeasured confounder based not upon the 
postulated value for $\tau = \tau_0$ being tested, but rather by maximizing the expectation of $\hat{D}_{\Gamma_i}^{(\tau_i)}$ in 
each matched set $i$. This pattern of bias yields the worst-case expectation when the null hypothesis is 
true within set $i$. This choice always maximizes $\text{pr}_{u_i}(\tau_i \geq \bar{\tau}_i)$, and corresponds to the worst-case 
confounder for $E(\hat{D}_{\Gamma_i}^{(\tau_i)} \mid \mathcal{F}, \mathcal{Z})$ under $H_N^{(\tau_0)}$ for matched pairs. Let $u^{**}$ denote the corresponding 
pattern of hidden bias. From Proposition 3, the resulting worst-case confounder has an intuitive 
form: $u_{ij}^{**} = \mathbb{1}(\delta_{ij} \geq \bar{\tau}_i) = \mathbb{1}\{\bar{\tau}_ij + r_{ij}/(n_i - 1) \geq \bar{\tau}_i + \bar{r}_{ij}/(n_i - 1)\}$. That is, in each 
matched set $i$, treatment assignments for which $\bar{\tau}_i \geq \bar{\tau}_i$ are given higher probability. Any choice of $u_i$ 
that differs from $u_{ij}^{**}$ must necessarily ascribe higher probability to observing a treated-minus-control 
difference in means that falls below $\bar{\tau}_i$.

The first term in (15) is maximized at $u_{ij}^{**}$ and equals zero, while the second equals

$$\frac{2\Gamma}{1 + \Gamma} \sum_{i=1}^B \frac{n_i}{N} \left\{ \frac{\sum_{j=1}^{n_i} \mathbb{1}(\delta_{ij} < \bar{\tau}_i) + \Gamma \mathbb{1}(\delta_{ij} \geq \bar{\tau}_i)}{n_i} \right\} (\bar{\tau}_i - \tau_0),$$

and the necessary condition for $E_{u_{ij}^{**}}(\hat{D}_{\Gamma_i}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) > 0$ under $H_N^{(\tau_0)}$ is

$$\text{cov} \left\{ \frac{\sum_{j=1}^{n_i} \mathbb{1}(\delta_{ij} < \bar{\tau}_i) + \Gamma \mathbb{1}(\delta_{ij} \geq \bar{\tau}_i)}{n_i} n_i (\bar{\tau}_i - \tau_0) \right\} > 0. \quad (16)$$

The first term in the covariance (16) is a function of the stratum size $n_i$ and $\sum_{j=1}^{n_i} \mathbb{1}(\delta_{ij} \geq \bar{\tau}_i)$, the 
number of potential treated-minus-control differences in means that exceed the average treatment 
effect in each stratum. Imagine conducting a linear regression of $\delta = (\delta_{11}, \delta_{12}, ..., \delta_{Bn_B})^T$ on an 
$N \times B$ matrix whose $i$th column contains an indicator of membership in the $i$th matched set; call
the resulting matrix \( X \). Let \( H = X(X^T X)^{-1}X^T \) be the hat matrix associated with the design matrix containing the matched set indicators, such that \( H\delta \) are the fitted values and \((I - H)\delta\) are the residuals. The fitted value for individual \( ij \) would be \((H\delta)_{ij} = \tau_i\), while the residual for individual \( ij \) would be \(((I - H)\delta)_{ij} = \delta_{ij} - \tau_i\). The first term in (16) is a function of these residuals, while the second term is a function of the fitted values. By standard results from multiple regression, we have that \( \text{cov}\{(H\delta)_{ij}, (((I - H)\delta)_{ij}) = 0 \). The covariance in (16) involves a covariance between functions of \( H\delta \) and functions of \((I - H)\delta\). This serves to highlight what must occur in order for \( E_{\Omega^+}(\hat{D}_{\Gamma}^{(\tau_0)} \mid F, Z) > 0 \): despite the fact that the residuals are uncorrelated with the fitted values, the functions of the residuals and functions of the fitted values represented in (16) must be positively associated. In particular, the proportion of positive residuals in matched set \( i \) must carry information about the fitted value in matched set \( i \). This can occur when the degree of skewness in the distribution of \( \delta_{ij} \) in each matched set varies as a function of \((\bar{\tau}_i - \tau_0)\). For \( n_i > 2 \), \( n_i^{-1} \sum_{i=1}^{n_i} \mathbb{1}(\delta_{ij} \geq \bar{\tau}_i) \) will tend to be larger under left-skewness than under right-skewness, so if \( \delta_{ij} \) are right-skewed for \( \bar{\tau}_i - \tau_0 \geq 0 \) and left-skewed otherwise, the covariance in (16) may be positive. In the absence of varying skewness as a function of treatment effect size, a positive covariance becomes more difficult to consistently attain. While it is possible mathematically that (16) holds, it remains to be seen whether the conditions giving rise to such an occurrence are of practical concern.

6.3 Validity of \( \hat{D}_{\Gamma}^{(\tau_0)} \) with heterogeneous effects for a subset of Rosenbaum’s model

Section 5.1 describes a general construction of test statistics that bound the worst-case expectation under \( H_N^{(\tau_0)} \) under interval restrictions on the conditional probabilities. We now show that the test statistic \( \hat{D}_{\Gamma}^{(\tau_0)} \) has expectation bounded above by zero under a particular subset of Rosenbaum’s model. Consider the restriction

\[
\frac{2}{n_i(1 + \Gamma)} \leq g_{ij} \leq \frac{2\Gamma}{n_i(1 + \Gamma)}. \tag{17}
\]

When \( n_i = 2 \), (17) is equivalent to the original sensitivity model (1). For \( n_i > 2 \), this restriction is strict subset of Rosenbaum’s model at \( \Gamma \). Applying Propositon 2 shows that \( E(\hat{D}_{\Gamma}^{(\tau_0)} \mid F, Z) \leq \bar{\tau}_i - \tau_0 \) under (17), such that \( E(\hat{D}_{\Gamma}^{(\tau_0)} \mid F, Z) \leq 0 \) when \( \bar{\tau} = \tau_0 \).

7 Variance estimation and performing the sensitivity analysis

7.1 Constructing conservative standard errors for sensitivity analysis

Little attention has been paid until now to the variance of \( \hat{D}_{\Gamma}^{(\tau_0)} \) in §5.3 or of \( \hat{D}_{\Gamma}^{(\tau_0)} \) in §6.1; rather, the discussion has focused on the the extent to which the worst-case expectations of these test statistics may be understood and bounded under \( H_N^{(\tau_0)} \). Here we show that the variance estimators for finely stratified designs developed in Fogarty (2018) also yield conservative variance estimators for use in sensitivity analysis with heterogeneous effects. We cater the following construction to \( \hat{D}_{\Gamma}^{(\tau_0)} \), but the analogous result holds for \( \hat{D}_{\Gamma}^{(\tau_0)} \). Let \( Q \) be any \( B \times p \) matrix that is constant over \( z \in \Omega \) with \( B > p \), and let \( H_Q = Q(Q^T Q)^{-1}Q^T \) be its hat matrix. Let \( Y_{\Gamma_i} = B(n_i/N)\hat{D}_{\Gamma_i}^{(\tau_0)}/\sqrt{1 - h_{Qii}} \) where
Proposition 4. Regardless of the true value of $\Gamma$ for which (1) holds,

$$E\{se^2(\hat{D}_{\Gamma}^{(\tau_0)}) \mid \mathcal{F}, \mathcal{Z}\} - \text{var}(\hat{D}_{\Gamma}^{(\tau_0)}) = \frac{1}{B^2} E(\hat{Y}_\Gamma \mid \mathcal{F}, \mathcal{Z})^T(I - H_Q) E(\hat{Y}_\Gamma \mid \mathcal{F}, \mathcal{Z}) \geq 0$$

Further, under suitable regularity conditions, then conditional upon $\mathcal{F}$ and $\mathcal{Z}$

$$\lim_{B \to \infty} \frac{se^2(\hat{D}_{\Gamma}^{(\tau_0)})}{\text{var}(\hat{D}_{\Gamma}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})} \geq 1.$$

Proposition 4 is a straightforward extension of Proposition 1 and Theorem 2 in Fogarty (2018), and the proof is omitted. For matched designs, a natural choice for $Q$ would be the vector containing weights $B(n_i/N)$ in the $i$th entry; this choice is used in the simulations in §9. As described in Fogarty (2018), other choices for $Q$ using covariate information from the matched sets can reduce the conservativeness of the resulting estimator. This positive bias is not reflective of a deficiency in the estimators, but rather is a fundamental feature of variance estimation under the finite population model: in general, it is impossible to consistently estimate the variance of the sample average treatment effect when effects are heterogeneous.

7.2 Large-sample reference distributions for sensitivity analysis

Suppose that $H_N^{(\tau_0)}$ is true and that (1) holds at $\Gamma > 1$. Then, from developments in §5.3, we know that $\max_{u \in \mathcal{U}} E_u(\hat{D}_{\Gamma}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) \leq 0$. Further, from §6.1 and §6.3, any of the following conditions are sufficient for $\max_{u \in \mathcal{U}} E_u(\hat{D}_{\Gamma}^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) \leq 0:

1. Potential outcomes and the true vector of hidden bias $u$ satisfy
   \[ \text{cov}\{\text{pr}(\hat{\tau}_i \geq \bar{\tau}_i \mid \mathcal{F}, \mathcal{Z}), n_i(\bar{\tau}_i - \tau_0)\} \geq 0. \] (§6.1)

2. Potential outcomes and the vector of hidden bias leading to the worst-case expectation $u^*$ satisfy
   \[ \text{cov}\{\text{pr}(\hat{\tau}_i \geq \bar{\tau}_i \mid \mathcal{F}, \mathcal{Z}), n_i(\bar{\tau}_i - \tau_0)\} \geq 0. \] (§6.1)

3. Conditional probabilities $\text{pr}(Z_{ij} = 1 \mid \mathcal{F}, \mathcal{Z})$ satisfy the interval restriction (17) at $\Gamma$. (§6.3)

Consider a candidate level-$\alpha$ sensitivity analysis using $\hat{D}_{\Gamma}^{(\tau_0)}$ through

$$\hat{\varphi}^{(\tau_0)}(\alpha, \Gamma) = 1 \left\{ \frac{\hat{D}_{\Gamma}^{(\tau_0)}}{se_Q(\hat{D}_{\Gamma}^{(\tau_0)})} \geq \Phi^{-1}(1 - \alpha) \right\},$$

and let $\hat{\varphi}^{(\tau_0)}(\alpha, \Gamma)$ be the analogous test based upon $\hat{D}_{\Gamma}^{(\tau_0)}$. The following theorems summarize our findings for sensitivity analyses using $\hat{D}_{\Gamma}^{(\tau_0)}$ and $\hat{D}_{\Gamma}^{(\tau_0)}$. 


Theorem 4. Suppose (1) holds at $\Gamma$, and consider any $\alpha \leq 0.5$. If the weak null $H_N^{(\tau_0)}$ is true, and any of the sufficient condition (a), (b), or (c) are satisfied, then under suitable regularity conditions
\[
\lim_{B \to \infty} E\{\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma) \mid \mathcal{F}, \mathcal{Z}\} \leq \alpha,
\]
and equality is possible. If the constant effect model $H_F^{(\tau_0)}$ holds, then sufficient conditions (a) and (b) are both satisfied, such that
\[
\lim_{B \to \infty} E\{\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma) \mid \mathcal{F}, \mathcal{Z}\} \leq \alpha,
\]
with equality possible in any matched design.

Theorem 5. Suppose (1) holds at $\Gamma$, and consider any $\alpha \leq 0.5$. If the weak null $H_N^{(\tau_0)}$ is true, then under suitable regularity conditions
\[
\lim_{B \to \infty} E\{\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma) \mid \mathcal{F}, \mathcal{Z}\} \leq \alpha,
\]
and equality is possible. If the constant effect model $H_F^{(\tau_0)}$ also holds, $\Gamma > 1$, and we do not have a paired design,
\[
\lim_{B \to \infty} E\{\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma) \mid \mathcal{F}, \mathcal{Z}\} < \alpha.
\]

For paired observational studies or if $\Gamma = 1$, $\hat{D}_\Gamma^{(\tau_0)}$ and $\hat{D}^{(\tau_0)}_\Gamma$ are equivalent, implying no divergence between these modes of inference. For general matched designs with $\Gamma > 1$, sensitivity analyses based upon $\hat{D}^{(\tau_0)}_\Gamma$ are valid with heterogeneous effects, but are unduly conservative for constant effects. Sensitivity analyses with $\hat{D}^{(\tau_0)}_\Gamma$ are valid and asymptotically sharp if treatment effects are constant, but require additional, potentially plausible, assumptions to guarantee validity if treatment effects are instead heterogeneous.

Regularity conditions on $\mathcal{F}$ are discussed in the web-based supporting materials. They are needed ensure that a central limit theorem holds for our test statistics, and that our standard error estimators have limits in probability; given these, the proofs are straightforward. In the web-based supporting materials, we also describe a modification of $\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma)$ which replaces critical values from a standard normal with critical values from a biased randomization distribution. At $\Gamma = 1$, this modification results in a test that is both exact for $H_F^{(\tau_0)}$ and asymptotically correct for $H_N^{(\tau_0)}$.

8 Bounding the worst-case expectation with binary outcomes

For continuous potential outcomes, it is generally impossible to explicitly calculate $\max_{u \in \mathcal{U}} E_u(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})$ over $H_N^{(\tau_0)}$ despite that $\max_{u \in \mathcal{U}} E_u(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) = 0$ under $H_F^{(\tau_0)}$ when (1) holds at $\Gamma$. With binary outcome variables the model of constant effects is particularly unsanitary, as the only feasible value for a constant effect is $\tau_0 = 0$. Fortunately, with binary outcomes it is possible to bound the worst-case expectation for $\hat{D}_\Gamma^{(\tau_0)}$ under $H_F^{(\tau_0)}$ when (1) is assumed to hold at $\Gamma$ using the approach of Fogarty et al. (2017). That work develops an integer program for calculating the worst-case expectation for a wide range of test statistics suitable for binary outcomes under the sensitivity model (1). The approach proceeds by finding the worst-case expectation over all values for $u$ and all values for the missing potential outcomes such that $H_F^{(\tau_0)}$ holds. The formulation is such that the number of decision variables does not scale linearly in the number of observations. Instead,
the problem scales in accord with the number of unique observed $2 \times 2$ tables for each $n_i$, with treatment status on one margin and outcome on the other. For each of these observed $2 \times 2$ tables, the formulation considers all possible $2 \times 2$ tables with potential outcomes under treatment on one margin and under control on the other. For each table, one can then calculate the vector $\mathbf{u}_i$ yielding the worst-case expectation through the separable algorithm in §3.

Let $\mathcal{I}_\Gamma^{(n)}(\mathbf{R}, \mathbf{Z})$ be the solution to this integer program, which upper bounds
\[
\max_{\mathbf{u} \in U} E_{\mathbf{u}}(\hat{D}_\Gamma^{(n)} | \mathcal{F}, \mathcal{Z}).
\]
Consider the candidate sensitivity analysis
\[
\hat{\varphi}^{(n)}(\alpha, \Gamma) = 1 \left\{ \frac{\hat{D}_\Gamma^{(n)} - \mathcal{I}_\Gamma^{(n)}(\mathbf{R}, \mathbf{Z})}{\text{se}_Q(\hat{D}_\Gamma^{(n)})} \geq \Phi^{-1}(1 - \alpha) \right\}.
\]

**Theorem 6.** Suppose (1) holds at $\Gamma$ and that outcomes are binary. If $H_N^{(n)}$ holds, then under suitable regularity conditions
\[
\lim_{B \to \infty} E\{\hat{\varphi}^{(n)}(\alpha, \Gamma) | \mathcal{F}, \mathcal{Z}\} \leq \alpha.
\]

Consider a paired observational study. There are four potentially observed $2 \times 2$ tables corresponding to the possible combinations of $(Z_{ij}, R_{ij})$ that could be observed in each pair. For each of these observed tables, there are four possible $2 \times 2$ tables of potential outcomes based on the potential values for the unobserved potential outcomes. This results in a total of at most 16 decision variables for any sample size $N$. Had we instead encoded the unobserved binary potential outcome as the decision variables there would have been $N$ decision variables, rendering the problem infeasible for moderately sized $N$. See Fogarty et al. (2017, §§4-5) for a detailed description of the integer program, and for computational experiments highlighting the strength of the underlying formulation.

**9 Simulations with worst-case hidden bias**

**9.1 The generative model**

We compare the candidate modes of sensitivity analyses with potential outcomes generated from various distributions. For continuous outcomes, we compare $\hat{\varphi}^{(n)}(\alpha, \Gamma)$ and $\hat{\varphi}^{(n)}(\alpha, \Gamma)$ to $\varphi^{(n)}(\alpha, \Gamma)$, the permutational $t$ sensitivity analysis described in §§3-4. For binary outcomes, we compare $\hat{\varphi}^{(n)}(\alpha, \Gamma)$, $\varphi^{(n)}(\alpha, \Gamma)$ and $\hat{\varphi}^{(n)}_{\text{binary}}(\alpha, \Gamma)$.

There are $B$ matched sets in each iteration, and matched set sizes are drawn iid with $n_i \sim 2 + \text{Poisson}(2)$ to mimic an observational study using variable ratio matching. In each matched set, exactly one individual receives the treatment and the remaining $n_i - 1$ receive the control. In the $m$th of $M$ iterations, potential outcomes are drawn independently for distinct individuals as
\[
\tilde{r}_{Cij} | \mathbf{x}_i = \varepsilon_{Cij}; \tilde{r}_{Tij} | \mathbf{x}_i, \varepsilon_{Cij} \sim \tilde{r}_{Cij} + \beta_i + \varepsilon_{Tij},
\]
where $\beta_i \overset{\text{IID}}{\sim} F_{\beta}(\cdot)$, $\varepsilon_{Cij} \overset{\text{indep}}{\sim} F_{\varepsilon_{Cij}}(\cdot | \beta_i)$, $\varepsilon_{Tij} \overset{\text{indep}}{\sim} F_{\varepsilon_{Tij}}(\cdot | \beta_i)$, $\varepsilon_{Cij} \perp \varepsilon_{Tij}$, and $E(\beta_i) = E(\varepsilon_{Cij}) = E(\varepsilon_{Tij}) = 0$. The variance of $\beta_i$ affects the across-set effect heterogeneity (potentially reflecting the impact of effect modifiers $\mathbf{x}_i$), while $\varepsilon_{Tij}$ affects the within-set effect heterogeneity. Note the potential dependence of $F_{\varepsilon_{Cij}}(\cdot | \beta_i)$ and $F_{\varepsilon_{Tij}}(\cdot | \beta_i)$ on $\beta_i$. For continuous outcomes, we set $r_{Tij} = \tilde{r}_{Tij}$ and $r_{Cij} = \tilde{r}_{Cij}$. For binary outcomes, we instead set $r_{Tij} = 1(\tilde{r}_{Tij} > 0)$, and set $r_{Cij} = 1(\tilde{r}_{Cij} > 0)$. The sample average treatment effect is then $N^{-1} \sum_{i=1}^{B} \sum_{j=1}^{n_i} (r_{Tij} - r_{Cij}) = \hat{\tau}^{(m)}$. 

20
Table 1: Generative models for the simulation study. \( \mathcal{N}(\mu, \sigma^2) \) is a normal with mean \( \mu \) and variance \( \sigma^2 \), \( \mathcal{E}(\lambda) = \mathcal{E}(\lambda) - 1/\lambda \) is an exponential with rate \( \lambda \) minus its expectation, and \( V_i = \{ 2I(\beta_i \geq 0) - 1 \} \).

<table>
<thead>
<tr>
<th>( \beta_i )</th>
<th>( \varepsilon_{Cij} )</th>
<th>( \varepsilon_{Tij} )</th>
<th>( \beta_i )</th>
<th>( \varepsilon_{Cij} )</th>
<th>( \varepsilon_{Tij} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a) 0</td>
<td>( \mathcal{E}(1/10) )</td>
<td>0</td>
<td>(f) -( \mathcal{E}(1) )</td>
<td>-( \mathcal{E}(1/10) )</td>
<td>-( \mathcal{E}(1/10) )</td>
</tr>
<tr>
<td>(b) \mathcal{N}(0, 1)</td>
<td>\mathcal{N}(0, 10^2)</td>
<td>\mathcal{N}(0, 10^2)</td>
<td>(g) -( \mathcal{E}(1/10) )</td>
<td>-( \mathcal{E}(1/10) )</td>
<td>-( \mathcal{E}(1/10) )</td>
</tr>
<tr>
<td>(c) \mathcal{N}(0, 10^2)</td>
<td>\mathcal{N}(0, 10^2)</td>
<td>\mathcal{N}(0, 10^2)</td>
<td>(h) \mathcal{N}(0, 1)</td>
<td>( V_i{ \mathcal{E}(1/10) } )</td>
<td>\mathcal{N}(0, 1)</td>
</tr>
<tr>
<td>(d) ( \mathcal{E}(1) )</td>
<td>( \mathcal{E}(1/10) )</td>
<td>( \mathcal{E}(1/10) )</td>
<td>(i) \mathcal{N}(0, 5^2)</td>
<td>( V_i{ \mathcal{E}(1/10) } )</td>
<td>\mathcal{N}(0, 1)</td>
</tr>
<tr>
<td>(e) ( \mathcal{E}(1/10) )</td>
<td>( \mathcal{E}(1/10) )</td>
<td>( \mathcal{E}(1/10) )</td>
<td>(j) \mathcal{N}(0, 1)</td>
<td>-( V_i{ \mathcal{E}(1/10) } )</td>
<td>\mathcal{N}(0, 1)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(k) \mathcal{N}(0, 5^2)</td>
<td>-( V_i{ \mathcal{E}(1/10) } )</td>
<td>\mathcal{N}(0, 1)</td>
</tr>
</tbody>
</table>

Once the potential outcomes are fixed, we construct the three vectors of unmeasured confounders resulting in the worst-case expectations for \( \hat{D}_\Gamma^{(\tau_0)} \), \( \hat{T}_\Gamma^{(\tau_0)} \) and \( \hat{V}_\Gamma^{(\tau_0)} \) when (1) holds at \( \Gamma \) and with \( \tau_0 = \hat{\tau}^{(m)} \); these worst-case vectors of hidden covariates may be different. For each test, we generate a single treatment assignment vector through (1) using the worst-case vector of unmeasured confounders for that test. We finally conduct the sensitivity analysis at \( \Gamma \) for the null hypothesis \( \hat{\tau} = \hat{\tau}^{(m)} \).

We set \( B = 500 \), draw \( M = 5000 \) data sets for each of 11 simulation settings, generate biased treatment assignment probabilities such that (1) holds at \( \Gamma = 5 \), and attempt to control the Type I error rate at \( \alpha = 0.1 \). For each simulation setting, we report the estimated Type I error rate. We further include the expected value of the difference between the test statistic employed and its candidate worst-case expectation, scaled by the test statistic’s standard deviation across simulations. Finally, we report upper bounds given in Theorem 2 for \( \hat{D}_\Gamma^{(\tau_0)} \), Theorem 3 for \( \hat{T}_\Gamma^{(\tau_0)} \), and Proposition 1 for \( \hat{V}_\Gamma^{(\tau_0)} \), scaled again by the test statistic’s standard deviation. For the integer-programming approach in §3, no analytical bound is available. We instead report the average time required to solve the integer program.

Table 1 provides the 11 combinations of distributions for \( \beta_i, \varepsilon_{Cij}, \) and \( \varepsilon_{Tij} \) used in the simulation. Under choice (a), \( H^{(0)}_F \) holds in all simulations with continuous and binary outcomes. With continuous outcomes, choices (b) and (c) lead to a symmetric distribution for \( \delta_{ij} \) within each matched set, with more across-set treatment effect heterogeneity in (c) than (b). Choices (d) and (e) lead to right-skewed distributions for \( \delta_{ij} \) with more heterogeneity of effects in (e), and (f) and (g) lead to left-skewed distributions for \( \delta_{ij} \) with more heterogeneity in (g). Choices (h) and (i) result in distributions for \( \delta_{ij} \) that are right-skewed when \( \beta_i \geq 0 \) and left-skewed otherwise, while for (j) and (k) it is reversed. Choice (i) has more across-set effect heterogeneity than (h), and (k) has more across-set heterogeneity than (j). For the simulations with binary outcomes, the comparisons between degrees of effect heterogeneity remain the same, but the degrees of skewness may differ due to the descriptions in Table 1 reflecting distributions of the latent variables, rather than the observed binary outcomes.

9.2 Results

Table 2 contains the results of the simulation study. The first set of columns show that \( \hat{D}_\Gamma^{(\tau_0)} \) was valid in all settings as predicted by Theorem 5. That said, the procedure was conservative even when effects were heterogeneous: in no simulation setting did the actual Type I error rate exceed 0.03, despite that we sought to control the rate at 0.1. This conservativeness was present even in setting (a) where the sharp null holds, reflecting the consequences of Theorem 1. The second set of columns summarize findings for \( \hat{D}_\Gamma^{(\tau_0)} \). As Theorem 4 predicts, under the sharp null the size was
Table 2: Results from the simulation study with continuous and binary outcomes. The sensitivity model (1) holds at $\Gamma = 5$, and the desired size was $\alpha = 0.10$. Presented are the estimated Type I error rate for each method, along with the actual bias in the test statistic and the upper bound on the bias in units of the statistic’s standard deviation. For the method using an integer program, solution time in seconds is instead reported.

### Continuous Outcomes

<table>
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<th>Size</th>
<th>$\tilde{D}_\Gamma$</th>
<th>Bias</th>
<th>Bound</th>
<th>Size</th>
<th>$\tilde{D}_\Gamma$</th>
<th>Bias</th>
<th>Bound</th>
<th>Size</th>
<th>$\hat{T}_\Gamma$</th>
<th>Bias</th>
<th>Bound</th>
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<td></td>
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<td></td>
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<td></td>
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<td></td>
</tr>
<tr>
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<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>(b)</td>
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<td>-1.607</td>
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### Binary Outcomes

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<th>Bias</th>
<th>Bound</th>
<th>Size</th>
<th>$\tilde{D}_\Gamma$</th>
<th>Bias</th>
<th>Bound</th>
<th>Size</th>
<th>IP$_T\Gamma$</th>
<th>Bias</th>
<th>Time (s)</th>
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controlled at 0.10. With continuous outcomes, we see that the procedure was conservative for all settings except (h), and was appreciably less conservative than \( \tilde{D}_{\Gamma}^{(\tau_0)} \) as predicted by Theorem 3. That the method failed for simulation (h) further highlights Theorem 1: because the procedure is sharp under constant effects, there must be situations where the method is invalid under the weak null. The bound on the worst-case bias can be quite conservative in many simulation settings with heterogeneous effects. For settings (a)-(g), (j)-(k) the necessary conditions in Corollary 1 for the procedure to be invalid were not satisfied, while in simulation (i) we see that they despite being satisfied the procedure remained valid. That is, the necessary conditions may be from sufficient in many circumstances. For binary outcomes, the method controlled the Type I error rate in every setting spare (h) and (j), both of which are settings where the degree of skewness in \( \delta_{ij} \) strongly correlates with the sample average treatment effects.

With continuous outcomes, the permutational \( t \)-test in the third set of columns correctly controlled the size of the procedure when the sharp null held as expected, but was anti-conservative by a sizeable margin in many plausible simulation settings where effects were heterogeneous. For binary outcomes, the third set of columns shows the approach using integer programming to bound the worst-case expectation. We see that it correctly controlled the Type I error rate in all simulation settings. Comparing the relative biases, it was less conservative than \( \tilde{D}_{\Gamma}^{(\tau_0)} \) in settings (a)-(f), but more conservative in (h)-(k) where skewness varied with the average treatment effects. For all simulation settings, the integer program solved in well under half a second on a personal laptop with a 3.5 GHz processor and 16.0 GB RAM using the \texttt{gurobi} package within \texttt{R}.

A pattern observed throughout the simulation study is that all candidate modes of sensitivity analysis rejected in the null less frequently in the presence of strong across-set effect heterogeneity (captured by the variance of \( \beta_i \)) than in the presence of mild across-set heterogeneity. The gaps between the bounds on the worst-case bias and the actual degree of bias were also larger with a strong degree of heterogeneity, indicating that the upper bounds become more conservative as the degree of effect heterogeneity increases. To explain this for \( \hat{D}_{\Gamma}^{(\tau_0)} \), the function \( E_{u_i}(\hat{D}_{\Gamma_i}^{(\tau_0)} | F, Z) \) is concave in \( \tau_0 \) for fixed values \( u_i \), and the bound in Theorem 3 stems from a Taylor expansion about the point \( \bar{\tau}_i \). When effects are severely heterogeneous across matched sets but \( \bar{\tau} = \tau_0 \), the value \( \tau_0 \) may be quite far from \( \bar{\tau}_i \), which in turn increases the conservativeness of the bound based upon the Taylor expansion. Extreme effect modification across matched sets promotes conservativeness, and \( \hat{D}_{\Gamma_i}^{(\tau_0)} \) is least conservative when across-set effect modification is limited.

## 10 Discussion

For pair matching, the choice of sensitivity analysis for testing the weak null is straightforward: \( \hat{D}_{\Gamma}^{(\tau_0)} \) and \( \tilde{D}_{\Gamma}^{(\tau_0)} \) are equivalent, and the studentized sensitivity analysis described in detail in Fogarty (2019) using these statistics provides a sensitivity analysis for the weak null that remains sharp if effects are constant. For testing weak nulls in more flexible matched designs, the researcher must instead weigh the benefits and downsides of a few competing methods. Should bounding the worst-case expectation over the entirety of the weak null be valued above all else, even if doing so typically results in conservative analyses? Are we worried about patterns of hidden bias that satisfy the necessary condition in \$6.1 \) for \( \hat{D}_{\Gamma}^{(\tau_0)} \) to yield potentially invalid inference, keeping in mind that the necessary condition can be far from sufficient? We end here with a collection of facts, observations, and general commentary on the candidate sensitivity analyses for the weak null.

- The permutational \( t \)-based sensitivity analysis, \( \varphi^{(\tau_0)}_{DiM}(\alpha, \Gamma) \), should not be used if effect heterogeneity is suspected. Even in a finely stratified experiment, \( \Gamma = 1 \), the reference distribution
employed by the test may have too small a variance in the presence of heterogeneous effects as described in §4.2, which corrupts the size of the procedure even in the limit. When $\Gamma > 1$, the test can fail to bound the worst-case expectation under plausible patterns of hidden bias as suggested by Proposition 1 and confirmed by the simulations in §9. The permutational $t$-based sensitivity analysis can be invalid under many reasonable data generating processes.

- If bounding the worst-case expectation over the entirety of the weak null and for all patterns of hidden bias is deemed essential and non-negotiable, the sensitivity analysis using $\tilde{D}_{\Gamma}^{(\tau_0)}$ is appropriate. That said, the sensitivity analysis can be very conservative under reasonable data-generating processes such as those in §9. The examples yielding exactness rather than conservativeness such as the one in Theorem 1 might be viewed as pathological, unlikely to represent real-world observational studies. The source of the conservativeness, incompatibility, is described in §5.4 through the lens of a worst-case inverse probability weighted estimator.

- A common recommendation when using a flexible form of matching is to place upper and lower bounds on the allowed ratio of the number of treated to control individuals in any matched set. Matching with large imbalances in the number of controls across sets can have reduced efficiency at $\Gamma = 1$ (Kalton, 1968; Hansen, 2004), and there are diminishing returns to scale for the power of a sensitivity analysis as the number of controls increase in each matched set (Rosenbaum, 2013). The value $\Gamma_{n_i}$ used in the construction of $\tilde{D}_{\Gamma}^{(\tau_0)}$ provides further support for this recommendation. $\Gamma_{n_i} = \frac{\Gamma(n_i - 1) + 1)}{(\Gamma + n_i - 1)} \geq \Gamma$ can be thought of as the effective value of $\Gamma$ for which the sensitivity analysis using $\tilde{D}_{\Gamma}^{(\tau_0)}$ is being conducted if treatment effects are actually constant. $\Gamma_{n_i} = \Gamma$ when $n_i = 2$, but becomes increasingly large as $n_i$ increases when $\Gamma > 1$, tending towards $\Gamma^2$ and in turn contributing more and more to the conservativeness of $\tilde{D}_{\Gamma}^{(\tau_0)}$. Inference using $\tilde{D}_{\Gamma}^{(\tau_0)}$ will be less conservative when $n_i$ is kept small than when $n_i$ is allowed to take on large values, and will also be less conservative when (1) holds at smaller values of $\Gamma$ than when it holds for larger values of $\Gamma$.

- The test $\tilde{\varphi}^{(\tau_0)}(\alpha, \Gamma)$ using the statistic $\tilde{D}_{\Gamma}^{(\tau_0)}$ employs an upper bound on the worst-case expectation that is tight under constant effects. As the simulations in §9 reflect, with continuous outcomes the sensitivity analysis was conservative for testing the weak null in all circumstances except setting (h). Unlike with the examples breaking the permutational $t$-test, it is less obvious that setting (h) reflects a circumstance of practical concern. The necessary conditions within Corollary 1, in concert with the discussion in §6.2, give a sense of what must go wrong for $\tilde{D}_{\Gamma}^{(\tau_0)}$ to fail: despite residuals being uncorrelated with fitted values in linear regression, a function of the residuals must be strongly correlated with a function of the fitted values. Those considerations were exploited in the formulation of setting (h), but it may be difficult to imagine a pattern of hidden bias affecting an observational study in this way being of scientific interest. Is setting (h), where $\delta_{ij}$ are right-skewed when average effects tend to be positive but left-skewed when average effects tend to be negative, of practical concern? As setting (i) further illustrates, the bias does not increase with the degree of across-set heterogeneity within this setting. In fact, the opposite occurs, and even with an adversarial skewness this test is violated only when effects are heterogeneous, but not too heterogeneous.

- With binary outcomes, the test using $\tilde{D}_{\Gamma}^{(\tau_0)}$ and computing a bound on the worst-case expectation through the integer program in §8, $\tilde{\varphi}^{(\tau_0)}_{\text{binary}}(\alpha, \Gamma)$, correctly controlled the Type I error rate as predicted by Theorem 6. Comparing the relative biases, it less conservative than the
test using $\hat{D}_{\Gamma}^{(\tau_0)}$ in the more realistic settings (a)-(f), suggesting that it integer programming approach may be preferable to $\hat{\phi}^{(n)}(\alpha, \Gamma)$ with binary outcomes. That said, $\hat{D}_{\Gamma}^{(\tau_0)}$ itself resulted in a valid and even less conservative test, valid for all circumstances spare settings (h) and (j). This again calls for a critical appraisal of whether we should be concerned about the circumstances invalidating the sensitivity analysis $\hat{\phi}^{(n)}(\alpha, \Gamma)$.

A Proof of Proposition 1

Proof. For each $i$,

$$E(\hat{T}^{(n)}_{\Gamma_i} | F, Z) = \sum_{i=1}^{n_i} \theta_{ij}(\delta_{ij} - \tau_0 - \vartheta_{ij})$$

$$= \sum_{j=1}^{n_i} \theta_{ij} \left\{ \delta_{ij} - \tau_0 - \sum_{k=1}^{n_i} \theta_{ik}^{(j)}(\delta_{ik}^{(j)} - \tau_0) \right\}$$

$$\leq \sum_{j=1}^{n_i} \theta_{ij} \left\{ \delta_{ij} - \tau_0 - \sum_{k=1}^{n_i} \theta_{ik}^{(j)}(\delta_{ik}^{(j)} - \tau_0) \right\}$$

$$= \sum_{j=1}^{n_i} \theta_{ij} \{(1 - \theta_{ij})(\tau_{ij} - \tau_0) + (1 - \theta_{ij})(\tau_{ij} - \tau_0)/(n_i - 1)\}$$

$$= \frac{n_i}{n_i - 1} \sum_{j=1}^{n_i} \theta_{ij}(1 - \theta_{ij})(\tau_{ij} - \tau_0).$$

Taking the weighted average of these terms yields the result. $\blacksquare$

B Proof of Theorem 1

Proof. Without loss of generality, assume $h_{\Gamma n_i}(\tilde{\tau}_i - \tau_0)$ passes through the origin; otherwise simply define $\tilde{h}_{\Gamma n_i}(\tilde{\tau}_i - \tau_0) = h_{\Gamma n_i}(\tilde{\tau}_i - \tau_0) - h_{\Gamma n_i}(0)$. We first consider the case $n_i = 3$, and suppress the potential dependence of $h_{\Gamma n_i}$ on $n_i$ in what follows. Suppose there are $B = 3$ matched sets, with potential outcomes and hidden covariates of the following form

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<th>$r_C$</th>
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<tr>
<td></td>
<td>$a$</td>
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<td>1</td>
</tr>
</tbody>
</table>

Assume $c > 0$, and observe $\tilde{\tau} = 0$. It is clear that if the second or third individual in any set receives the treatment, the observed treated-minus-control paired difference will be zero, and the worst-case expectation generated under the assumption of Fisher’s sharp null will also be zero.
We focus attention on what occurs if the first individual receives the treatment. If this occurs in set one, then \( \hat{\tau}_1 = \delta^{(1)}_{11} = c \). Further, assuming Fisher’s sharp null, the unobserved treated-minus-control paired differences would be improperly imputed as \( \delta^{(1)}_{12} = \delta^{(1)}_{13} = -c \), while in reality they are \( \delta_{12} = \delta_{13} = 0 \). In sets two and three, if the first individual receives the treatment then \( \hat{\tau}_i = \delta_{i1} = -c \), and under the assumption of the sharp null the worst-case expectation would be calculated assuming the unobserved treated-minus-control paired differences were \( \delta^{(1)}_{i2} = \delta^{(1)}_{i3} = c/2 \), while in reality \( \delta_{i2} = \delta_{i3} = 0 \) for \( i = 2, 3 \).

In the first set, if the first individual received the treatment, the worst-case confounder under Fisher’s sharp null would instead assign the treatment to individual 1 with probability \( \Gamma/(2 + \Gamma) \), and to individuals 2 and 3 with probability \( 1/(2 + \Gamma) \) each, which align with the true probabilities of assignment to treatment under the true vector \( u_1 \). For matched set 1, the expectation of \( h_\Gamma(\hat{\tau}_i) - \mu_{\Gamma i} \) is

\[
\frac{\Gamma}{2 + \Gamma} \left\{ h_\Gamma(2c) - \left( \frac{\Gamma h_\Gamma(2c)}{2 + \Gamma} + \frac{h_\Gamma(-c)}{2 + \Gamma} + \frac{h_\Gamma(-c)}{2 + \Gamma} \right) \right\} \\
= \frac{\Gamma}{(2 + \Gamma)^2} \left\{ 2h_\Gamma(2c) - 2h_\Gamma(-c) \right\}
\]

For sets two and three, if the first individual receives the treatment then the worst-case confounder under Fisher’s sharp null would instead assign the treatment to individual 1 with probability \( 1/(2\Gamma + 1) \), and to individuals 2 and 3 with probability \( \Gamma/(2\Gamma + 1) \). These probabilities also align with the true assignment probabilities under \( u_2 \) and \( u_3 \). The expectation of \( h_\Gamma(\hat{\tau}_i) - \mu_{\Gamma i} \) for either of these sets would be

\[
\frac{1}{2\Gamma + 1} \left\{ h_\Gamma(-c) - \left( \frac{h_\Gamma(-c)}{2\Gamma + 1} + \frac{\Gamma h_\Gamma(c/2)}{2\Gamma + 1} + \frac{\Gamma h_\Gamma(c/2)}{2\Gamma + 1} \right) \right\} \\
= \frac{\Gamma}{(2\Gamma + 1)^2} \left\{ 2h_\Gamma(-c) - 2h_\Gamma(c/2) \right\}.
\]

The sum of the expectations across the three matched sets is

\[
\frac{\Gamma}{(2 + \Gamma)^2} \left\{ 2h_\Gamma(2c) - 2h_\Gamma(-c) \right\} + \frac{2\Gamma}{(2\Gamma + 1)^2} \left\{ 2h_\Gamma(-c) - 2h_\Gamma(c/2) \right\},
\]

and for the expectation to be positive for some \( \Gamma \), it suffices to show that there exists a \( \Gamma \) such that

\[
\{2h_\Gamma(2c) - 2h_\Gamma(-c)\} > \left( \frac{2 + \Gamma}{2\Gamma + 1} \right)^2 \{ -4h_\Gamma(-c) + 4h_\Gamma(c/2) \}
\]

Take \( \Gamma = 1 + 3/\sqrt{2} + \epsilon \) for some \( \epsilon > 0 \), which in turn implies \( (2 + \Gamma)^2/(2\Gamma + 1)^2 = 1/2 - 1/4\epsilon \) for some \( 0 < \epsilon < 1 \). Recalling that \( h_\Gamma \) passes through the origin and is non-decreasing, we have \( h_\Gamma(-c) \leq 0 \leq h_\Gamma(c/2) \leq h_\Gamma(2c) \). We then have

\[
\{2h_\Gamma(2c) - 2h_\Gamma(-c)\} + (2 + \Gamma)^2/(2\Gamma + 1)^2 \{ 4h_\Gamma(-c) - 4h_\Gamma(c/2) \}
= \{2h_\Gamma(2c) - 2h_\Gamma(-c)\} + (1/2 - 1/4\epsilon) \{ 4h_\Gamma(-c) - 4h_\Gamma(c/2) \}
= 2h_\Gamma(2c) - \epsilon 2h_\Gamma(-c) + (2 - \epsilon)h_\Gamma(c/2)
\geq -\epsilon 2h_\Gamma(-c) + (4 - \epsilon)h_\Gamma(c/2).
\]
By assumption $h_{\Gamma}(\cdot)$ is nondecreasing and not constant. Therefore, there exists a $c$ such that $h_{\Gamma}(-c) < h_{\Gamma}(c/2)$ Choosing this value of $c$, we then have $-\epsilon_c2h_{\Gamma}(-c) + (4 - \epsilon_c)h_{\Gamma}(c/2) > 0$, proving the result.

The construction naturally extends to other values of $n_i$. Choose $n_i$ strata, each of size $n_i$, where the first has one individual with $r_{Tij} = a + (n_i - 1)c$, $r_{Cij} = a$ and the rest have $r_{Tij} = r_{Cij} = a$. Let the remaining $n_i - 1$ strata each have one individual with $r_{Tij} = a - c$, $r_{Cij} = a$ and the rest $r_{Tij} = r_{Cij} = a$.

### C Proof of Proposition 2

**Proof.** Observe that $\sum_{j=1}^{n_i}(\delta_{ij} - \tau_0) = n_i(\bar{\tau}_i - \tau_0)$. Define $\tilde{\delta}_{ij}$ as

$$
\tilde{\delta}_{ij} = \begin{cases} 
2(\delta_{ij} - \tau_0)/(1 + \Gamma) & \delta_{ij} - \tau_0 \geq 0 \\
2\Gamma(\delta_{ij} - \tau_0)/(1 + \Gamma) & \delta_{ij} - \tau_0 < 0,
\end{cases}
$$

such that $\hat{D}_{\Gamma_i}(\tau_0) = \sum_{i=1}^{n_i}Z_{ij}\tilde{\delta}_{ij}$ and $E(\hat{D}_{\Gamma_i}(\tau_0) | F, Z) = \sum_{i=1}^{n_i}\theta_{ij}\tilde{\delta}_{ij}$. By ignoring the constraint that $\sum_{i=1}^{n_i}\theta_{ij} = 1$, it is clear that $E(\hat{D}_{\Gamma_i}(\tau_0) | F, Z)$ is upper bounded under (9) in the manuscript by setting

$$
\theta_{ij} = \begin{cases} 
\frac{\Gamma}{\kappa_{\Gamma_i}} & \tilde{\delta}_{ij} \geq 0 \\
\frac{1}{\kappa_{\Gamma_i}} & \tilde{\delta}_{ij} < 0.
\end{cases}
$$

Doing so yields

$$
E(\hat{D}_{\Gamma_i}(\tau_0) | F, Z) \leq \sum_{j=1}^{n_i} \theta_{ij}\left\{ \delta_{ij} - \tau_0 - \left(\frac{\Gamma - 1}{1 + \Gamma}\right) |\delta_{ij} - \tau_0| \right\} \\
\leq \left(\frac{2\Gamma}{1 + \Gamma}\right) \sum_{j=1}^{n_i}(\delta_{ij} - \tau_0) = n_i \left(\frac{2\Gamma}{1 + \Gamma}\right) \frac{(\bar{\tau}_i - \tau_0)}{\kappa_{\Gamma_i}}
$$

$\blacksquare$

### D Connections between $\hat{D}_{\Gamma_i}(\tau_0)$ and the inverse probability weighted estimator $W_{\Gamma_i}(\tau_0)$

We can write $W_{\Gamma_i}(\tau_0)$ in the form $\min_{\kappa_{\Gamma_i}^{-1} \leq \theta_{ij} \leq \Gamma\kappa_{\Gamma_i}^{-1}} \sum_{i=1}^{n_i}Z_{ij}(\delta_{ij} - \tau_0)/\theta_{ij}$. For each $ij$, we have

$$
\min_{\kappa_{\Gamma_i}^{-1} \leq \theta_{ij} \leq \Gamma\kappa_{\Gamma_i}^{-1}} \frac{\delta_{ij} - \tau_0}{\theta_{ij}} = \begin{cases} 
\frac{\kappa_{\Gamma_i}(\delta_{ij} - \tau_0)}{\theta_{ij}} & \delta_{ij} - \tau_0 \geq 0 \\
\frac{\kappa_{\Gamma_i}(\delta_{ij} - \tau_0)}{\theta_{ij}} & \delta_{ij} - \tau_0 < 0.
\end{cases}
$$

Comparing (20) to (19), we see that

$$
W_{\Gamma_i}(\tau_0) = \frac{\kappa_{\Gamma_i}}{n_i} \left(\frac{1 + \Gamma}{2\Gamma}\right) \hat{D}_{\Gamma_i}(\tau_0).
$$
E  Proof of Theorem 3

Suppose (1) holds at $\Gamma$, and let $u_i$ be a vector of unmeasured confounders. We first show that

$$E_u(\hat{D}_{1i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z}) \leq E_u(\hat{D}_{1i}^{(\tau_i)} | \mathcal{F}, \mathcal{Z}) + \left\{ \frac{2\Gamma}{1+\Gamma} \right\} \left\{ 1 + \frac{1-\Gamma}{\Gamma} \text{pr}_{u_i}(\tau_i \geq \bar{\tau}_i | \mathcal{F}, \mathcal{Z}) \right\} (\bar{\tau}_i - \tau_0).$$

Let $\varrho_{ij} = \exp(\gamma u_{ij})/\sum_{j=1}^{n_i} \exp(\gamma u_{ij})$. The function

$$f_i(t) := \sum_{j=1}^{n_i} \varrho_{ij} \left\{ \delta_{ij} - t - \left( \frac{\Gamma - 1}{1+\Gamma} \right) |\delta_{ij} - t| \right\}$$

is concave in $t$, and is differentiable everywhere except the points $\{\delta_{ij}, j = 1, ..., n_i\}$. Observe that $f_i(t) = E_u(\hat{D}_{1i}^{(t)} | \mathcal{F}, \mathcal{Z})$. By concavity, we have that for any superderivative $v$ of $f_i$ at $t = \bar{\tau}_i$,

$$E_u(\hat{D}_{1i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z}) \leq E_u(\hat{D}_{1i}^{(\tau_i)} | \mathcal{F}, \mathcal{Z}) + v(\tau_0 - \bar{\tau}_i).$$

One superderivative $v'$ of $f(t)$ at $f(\bar{\tau}_i)$ is

$$v' = -\frac{2\Gamma}{1+\Gamma} \sum_{j=1}^{n_i} \varrho_{ij} \left\{ 1(\delta_{ij} < \bar{\tau}_i) + 1(\delta_{ij} \geq \bar{\tau}_i)/\Gamma \right\}$$

$$= -\frac{2\Gamma}{1+\Gamma} \left( 1 + \frac{1-\Gamma}{\Gamma} \sum_{j=1}^{n_i} \varrho_{ij} 1(\delta_{ij} \geq \bar{\tau}_i) \right)$$

$$= -\frac{2\Gamma}{1+\Gamma} \left( 1 + \frac{1-\Gamma}{\Gamma} \text{pr}_{u_i}(\tau_i \geq \bar{\tau}_i | \mathcal{F}, \mathcal{Z}) \right),$$

and using this superderivative gives the desired inequality.

To see that $E_u(\hat{D}_{1i}^{(\tau_i)} | \mathcal{F}, \mathcal{Z}) \leq 0$ if (1) holds at $\Gamma$, note that $\sum_{i=1}^{n_u}(\delta_{ij} - \bar{\tau}_i) = 0$, and that $\hat{D}_{1i}^{(\tau_i)} = \sum_{j=1}^{n_i} Z_{ij} (\delta_{ij} - \bar{\tau}_i) - \{ (\Gamma - 1)/(1+\Gamma) \} |\delta_{ij} - \bar{\tau}_i|$. By Proposition 3, we then have that $f_i(\bar{\tau}_i) = E_u(\hat{D}_{1i}^{(\tau_i)} | \mathcal{F}, \mathcal{Z}) \leq 0$ if (1) holds at $\Gamma$. The suitably weighted average of these terms proves the theorem.

F  On regularity conditions for Proposition 4 and Theorems 4 - 6

Proposition 4 states that under suitable regularity conditions, then conditional upon $\mathcal{F}$ and $\mathcal{Z}$

$$\text{plim}_{B \to \infty} \frac{\text{se}_Q^2(\hat{D}_{1i}^{(\tau_0)})}{\text{var}(\hat{D}_{1i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z})} \geq 1.$$ 

This is required to justify replacing the true, but unknowable, value for $\text{var}(\hat{D}_{1i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z})$ with the conservative standard error $\text{se}_Q^2(\hat{D}_{1i}^{(\tau_0)})$ when performing inference. For the result to hold, it is sufficient to show that $B \times \text{se}_Q^2(\hat{D}_{1i}^{(\tau_0)})$ converges in probability to a limiting value as $B \to \infty$; and that $B \times \text{var}(\hat{D}_{1i}^{(\tau_0)} | \mathcal{F}, \mathcal{Z})$ converges to a limiting value as $B \to \infty$. For finely stratified experiments, Theorem 2 of Fogarty (2018) provides a proof of these details, with Conditions 1-3 therein providing sufficient conditions. Suitable modification of these regularity conditions also yields the result in a
sensitivity analysis with $\Gamma > 1$. The analogous derivation would follow through with $\hat{D}_\Gamma^{(\tau_0)}$ replaced with $\hat{D}_\Gamma^{(\tau_0)}$.

For Theorems 4 or 6 to hold, from arguments in §4.6 and armed with Proposition 4 we need only show that a central limit theorem holds when applied to the true, but unknowable, deviate

$$\frac{\hat{D}_\Gamma^{(\tau_0)} - E_u(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})}{\text{var}(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})^{1/2}}.$$

For Theorem 5 to hold, an analogous argument shows we simply need a central limit theorem to hold with $\hat{D}_\Gamma^{(\tau_0)}$ replaced with $\hat{D}_\Gamma^{(\tau_0)}$.

Sufficient conditions for a central limit theorem would include, for instance, satisfying the Lyapunov condition: for some $\delta > 0$,

$$\lim_{B \to \infty} \frac{\sum_{b=1}^{B} \left( \frac{n_i}{N} \right)^{2+\delta} E_u \{ | \hat{D}_\Gamma^{(\tau_0)} - E_u(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z}) |^{2+\delta} \mid \mathcal{F}, \mathcal{Z} \}}{\text{var}(\hat{D}_\Gamma^{(\tau_0)} \mid \mathcal{F}, \mathcal{Z})^{1+\delta/2}} = 0.$$

Many reasonable restrictions on $\mathcal{F}$ would satisfy this condition. For finely stratified experiments, Theorem 1 of Fogarty (2018) provides a proof that central limit theorem applies, with Conditions 1-3 therein providing sufficient conditions.

### G A reference distribution built from biased randomizations

In paired observational studies, Fogarty (2019) develops an alternative reference distribution for conducting a sensitivity analysis. Rather than using $\Phi(\cdot)$, one can instead consider constructing a reference distribution based upon the randomization distribution for $\hat{D}_\Gamma^{(\tau_0)} / \text{se}(\hat{D}_\Gamma^{(\tau_0)})$ under $H_F^{(\tau_0)}$ while using the vector of unmeasured confounders yielding the worst-case expectation when assuming constant effects at $\tau_0$. Explicitly, for a sensitivity analysis assuming (1) holds at $\Gamma$, consider constructing a reference distribution $\hat{G}_\Gamma(\cdot)$ as follows

1. For each $ij$, compute $a_{ij} = (R_{ij} - Z_{ij}\tau_0) - \sum_{j' \neq j} (R_{ij'} - Z_{ij'}\tau_0)/(n_i - 1)$

2. Set $u_{ij} = 1(a_{ij} \geq 0)$

3. Define $\hat{G}_\Gamma(\cdot)$ as

$$\hat{G}_\Gamma(k) = \sum_{z \in \Omega} \mathbb{1} \left\{ \frac{\bar{A}_\Gamma(z,a)}{\text{se}\{\bar{A}_\Gamma(z,a)\}} \leq k \right\} \prod_{i=1}^{B} \frac{\exp \left( \gamma \sum_{j=1}^{n_i} z_{ij} u_{ij} \right)}{\sum_{j=1}^{n_i} \exp(\gamma u_{ij})},$$

where $A_{\Gamma_i} = \sum_{j=1}^{n_i} z_{ij} (a_{ij} - \{(\Gamma - 1)/(1 + \Gamma)\}) |a_{ij}|$, $\bar{A}_\Gamma(z,a) = \sum_{i=1}^{B} (n_i/N) A_{\Gamma_i}$, and $\text{se}\{\bar{A}_\Gamma(z,a)\}$ is the corresponding standard error based on $(n_i/N) A_{\Gamma_i}, i = 1, \ldots, B$.

Observe that under $H_N^{(\tau_0)}$, $\hat{G}_\Gamma(\cdot)$ is random as $a_{ij} \neq r_{Cij} - \sum_{j' \neq j} r_{Cij'}/(n_i - 1)$. Consider replacing $\hat{\phi}^{(\tau_0)}(\alpha, \Gamma)$ with

$$\hat{\phi}_{\text{rand}}^{(\tau_0)}(\alpha, \Gamma) = 1 \left\{ \frac{\hat{D}_\Gamma^{(\tau_0)}}{\text{se}(\hat{D}_\Gamma^{(\tau_0)})} \geq \hat{G}_\Gamma^{-1}(1 - \alpha) \right\},$$

where $\hat{G}_\Gamma^{-1}(1 - \alpha) = \inf\{k : \hat{G}_\Gamma(k) \geq 1 - \alpha\}$ is the $1 - \alpha$ quantile of $\hat{G}_\Gamma(\cdot)$.
Proposition 5. Under suitable regularity conditions, for all points $k$ and conditional upon $Z$ and $F$.

$$
\hat{G}_\Gamma(k) \xrightarrow{D} \Phi(k).
$$

The flow of the proof is identical to that of Theorem 2 in Fogarty (2019), and a sketch is as follows. One considers the joint distribution of $\sqrt{B}A_\Gamma(z,a)$ and $\sqrt{B}A(z',a)$, for $z, z' \ iid$ from worst-case distribution for treatment assignments constructed above. One then shows that $\{\sqrt{B}A_\Gamma(z,a), \sqrt{B}A(z',a)\}$ are identically distributed and converge jointly in distribution to a multivariate normal, with mean zero and covariance zero, such that they are asymptotically independent. One further shows that $B \times \text{se}(\bar{A}_\Gamma(z,a))^2$ is consistent for the variance of $\sqrt{B}A_\Gamma(z,a)$, such that $\bar{A}(z,a)/\text{se}(\bar{A}(z,a))$ converges in distribution to a standard normal. Theorem 15.2.3 of Lehmann and Romano (2005) then completes the proof.

Proposition 5 implies that $\hat{\phi}^{(\tau_0)}(\alpha, \Gamma)$ may be replaced with $\hat{\phi}_{\text{rand}}^{(\tau_0)}(\alpha, \Gamma)$ in the statement of Theorem 4. At $\Gamma = 1$, we further have that if $H_F^{(\tau_0)}$ is true, $E\{\phi_{\text{rand}}^{(\tau_0)}(\alpha, 1) \mid F, Z\} \leq \alpha$ for all $B > p$, where $p$ was the dimension of the matrix $Q$ used to construct the standard error. That is, in finely stratified experiments, $\hat{\phi}^{(\tau_0)}(\alpha, 1)$ is exact under constant effects and asymptotically correct under $H_N^{(\tau_0)}$, thus extending the results of Loh et al. (2017) and Wu and Ding (2018) to this experimental design. Unfortunately, when (1) holds at $\Gamma > 1$ it need not be the case that $E\{\phi_{\text{rand}}^{(\tau_0)}(\alpha, \Gamma) \mid F, Z\} \leq \alpha$ in finite samples under $H_F^{(\tau_0)}$. This is because in using the pattern of unmeasured confounding that maximizes the worst-case expectation in constructing $\phi_{\text{rand}}^{(\tau_0)}(\alpha, \Gamma)$, we are already appealing to an asymptotic argument provided in Gastwirth et al. (2000) and outlined in §3 for how to approximate the worst-case $p$-value. That is, when conducting sensitivity analyses with matched structures beyond pair matching, one generally cannot calculate the true worst-case $p$-value even under constant effects. Nonetheless, one may expect $\phi_{\text{rand}}^{(\tau_0)}(\alpha, \Gamma)$ to better capture finite-sample departures from normality in the distribution of $\hat{D}^{(\tau_0)}_\Gamma/\text{se}(\hat{D}^{(\tau_0)}_\Gamma)$.

References


