Studentized sensitivity analysis for the sample average treatment effect in paired observational studies

Colin B. Fogarty *

Abstract

A fundamental limitation of causal inference in observational studies is that perceived evidence for an effect might instead be explained by factors not accounted for in the primary analysis. Methods for assessing the sensitivity of a study's conclusions to unmeasured confounding have been established under the assumption that the treatment effect is constant across all individuals. In the potential presence of unmeasured confounding, it has been argued that certain patterns of effect heterogeneity may conspire with unobserved covariates to render the performed sensitivity analysis inadequate. We present a new method for conducting a sensitivity analysis for the sample average treatment effect in the presence of effect heterogeneity in paired observational studies. Our recommended procedure, called the studentized sensitivity analysis, represents an extension of recent work on studentized permutation tests to the case of observational studies, where randomizations are no longer drawn uniformly. The method naturally extends conventional tests for the sample average treatment effect in paired experiments to the case of unknown, but bounded, probabilities of assignment to treatment. In so doing, we illustrate that concerns about certain sensitivity analyses operating under the presumption of constant effects are largely unwarranted.

1 Introduction

1.1 Constant effects, then and now

It is common to assume that treatment effects are constant across individuals when inferring both the existence and the magnitude of causal effects. Unease is sometimes expressed about the restrictiveness of this assumption. Indeed, the strength of the constant effects assumption (also called the assumption of additivity) was at the core of the "Neyman-Fisher controversy," with Fisher favoring the sharp null that the the treatment effect was zero for all individuals and Neyman recommending tests of the weaker null that the treatment effect was zero *on average* for the individuals in a given experiment. Neyman (1935) suggested that inference assuming additivity in Latin Square designs could be anti-conservative in the presence of effect heterogeneity, eliciting an acerbic response by Fisher (Fisher, 1935) and, in turn, irrevocably souring relations between the two; see Sabbaghi and Rubin (2014) for a detailed discussion of the controversy and its ramifications.

*Operations Research and Statistics Group, MIT Sloan School of Management, Massachusetts Institute of Technology, Cambridge MA 02142 (e-mail: cfogarty@mit.edu)

Evidently, the passage of time has done little to temper the debate, with both camps maintaining supporters. Those favoring Neyman's weak null of no effect on average focus on the seeming inadequacy of the constant effects assumption as a description of reality. Gelman writes that "the presumption of constant effects corresponds to a simplified view of the world that can impede research discussion" (Gelman, 2015, p. 636). Advocates of Fisher's sharp null focus, among many things, on the central role of hypothesis testing in empirical falsification. Cox (1958) and Rosenbaum (2002b, §2.4.5) discuss how rejection of the sharp null is in and of itself useful as a means of promoting future scientific inquiry, despite a rejection of the sharp null not implying the existence of a treatment effect that is predictably positive or negative. Such a rejection may well be indicative of underlying subject-by-treatment interactions, hence identifying the existence of patterns for effects which the current experiment can neither describe nor predict. Quoting Rosenbaum, "the variation we do not fathom today we intend to decipher tomorrow" (Rosenbaum, 2002b, p. 40). See Caughey et al. (2017, §2) for additional perspective and for further quotations supporting both sides.

1.2 Additivity in observational studies

In the context of observational studies, the restrictiveness of the constant effects assumptions faces additional scrutiny when assessing the robustness of a study's findings to unmeasured confounding through a *sensitivity analysis*. There is a perception that, with few exceptions, the methodology described in Rosenbaum (2002b, §4.2) requires the researcher to posit a sharp null hypothesis, and that the model may not readily extend to tests of average causal effects in the face of effect heterogeneity. Hill opines that when conducting a sensitivity analysis, "the focus on additive treatment effects...is potentially problematic" (Hill, 2002, p. 308), while Robins states that any gain from Rosenbaum's model for a sensitity analysis is offset "...by Rosenbaum's assumption that individual outcomes are deterministic and that an additive treatment effect model holds" (Robins, 2002, p. 310). There is concern, in particular, that sensitivity analyses conducted assuming constant effects may paint an overly optimistic picture of the study's sensitivity to hidden bias. The fear is that certain patterns of unmeasured confounding may conspire with the unidentified aspects of the constant effects model, rendering the analysis assuming constant effects inadequate. One particular argument is that in observational studies, individuals may tend to self-select into the treatment group which they know to be most beneficial to them. For a treated and control individual with the same observed covariate value x, one may then expect a difference in the observed response between the treated and control individuals due to this "essential heterogeneity," even if there truly was no average effect of the treatment at that point x. (Heckman et al., 2006).

1.3 A motivating example: alcohol consumption and genetic damage

Maffei et al. (2000) conducted an observational study on the impact of alcoholism on genetic damage, wherein 20 alcoholics were paired with 20 non-alcoholics on the basis of covariates such as gender, age, and smoking habit, with alcoholics having consumed > 120 grams of pure alcohol per day and the controls consuming between 8 and 13 grams per day. One reported measure of genetic damage was the frequency of micronuclei harboring whole chromosomes per

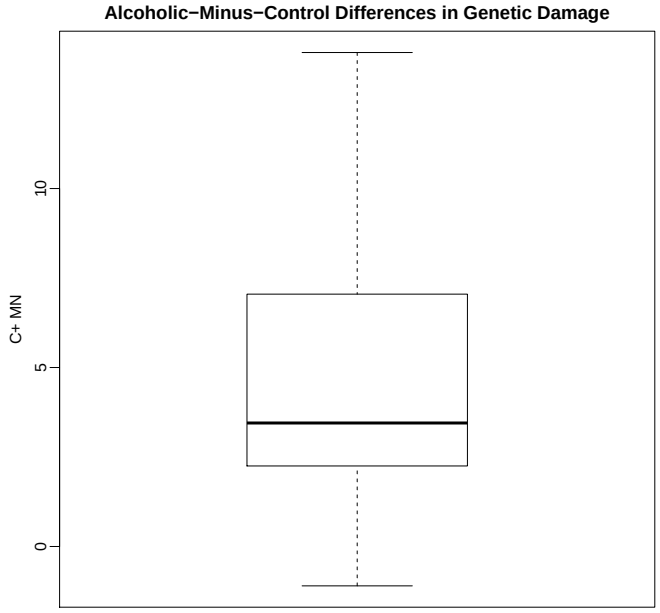

Figure 1: Evidence of skewness in the distribution of alcoholic-minus-control paired differences for C+ MN (a measure of genetic damage).

1,000 binucleated cells (C+ MN), about which a sensitivity analysis for the number of positive Walsh averages was previously reported in Rosenbaum (2003). Higher values for this measure indicate a larger degree of damage. The average of the alcoholic-minus-control paired differences was 4.75, and the standard deviation for the paired differences was 4.26. Under the framework presented in §§2.2-2.3, a sensitivity analysis utilizing the permutational t-statistic rejects Fisher's sharp null of no effect for any individual until the value $\Gamma = 5.04$ at $\alpha = 0.05$. In words, the value $\Gamma = 5.04$ means that we would reject Fisher's sharp null at $\alpha = 0.05$ for any pattern of unmeasured confounding which impacted the odds ratio of becoming an alcoholic by a factor of less than 5.04 in all pairs.

Figure 1 shows a boxplot of the 20 treated-minus-control pair differences, which provides evidence of substantial right skewness. Under no unmeasured confounding, such an asymmetric distribution would be precluded by a model of constant effects, as the paired differences would necessarily be distributed symmetrically about the true treatment effect. It seems likely, then, that the treatment effects are heterogeneous, violating the assumption underpinning the reported value of $\Gamma = 5.04$. How much would the reported robustness to unmeasured confounding change if we instead test the null that the sample average of the treatment effects for the 40 individuals in our study is zero while allowing for heterogeneous effects?

1.4 Studentization with hidden bias

Rather than taking a stance on which null hypothesis should be preferred, this work focuses primarily on the ramifications of the debate for the interpretations ascribed to sensitivity analyses. We have in mind a practitioner who recognizes the need for conducting a sensitivity analysis when treatment assignment is beyond their control, but who would ideally like the analysis to attest to the robustness of their findings in the presence of heterogeneous effects, thereby assuaging the potential fears of critics in their field. We specialize our treatment to the case of paired observational studies, and to inference conducted using the treated-minus-control difference in means as the test statistic. This amounts to a choice of the permutational t-statistic for conducting a sensitivity analysis in the model of Rosenbaum (2002b, §4.2) when assuming constant effects, which is reviewed in §§2.2 and 2.3.

In this setting, we assess (i) whether one can construct a valid sensitivity analysis for the sample average treatment effect in the presence of effect heterogeneity; (ii) if so, whether a sensitivity analysis for a constant treatment effect using the permutational t-statistic is one such method; and (iii), if (i) is true but (ii) is false, by what magnitude the methods can differ. Propositions 1 and 2 of §§3 and 4 validate two procedures which affirmatively answer (i). Proposition 3 of §5 answers (ii) negatively, in the sense that there exists patterns of effect heterogeneity which lead the test assuming constant effects to have an inflated Type I error rate in the presence of heterogeneous effects; however, Proposition 4 of §5 indicates that the answer to (iii) is, stated loosely, "not by much," a position to which the analysis of the data example in §1.3, concluded in §7, further attests. At its core, the claim stems from the realization that even under effect heterogeneity, the classical sensitivity analysis based on the permutational t-test creates a candidate worst-case distribution that correctly bounds the *expectation* of the test statistic's actual distribution (proven in Lemma 2) but which may have too small a variance (shown through a numerical counterexample in §5.1). As bias trumps variance in the context of a sensitivity analysis, the extent to which the analysis based on the permutational t-test can mislead is, thankfully, limited.

In addressing these questions, we illustrate that while a randomization-based sensitivity analysis for the permutational t-test may have the improper size in the presence of heterogeneous effects, the issue is avoided through an appropriate "studentization" of the test-statistic while employing the same worst-case distribution for treatment assignments, aligning with work on robust permutation tests by Janssen (1997) and Chung and Romano (2013). Under no unmeasured confounding, the studentization employed is none other than that recommended by Gosset himself: the observed difference-in-means is simply divided by the classical standard error estimator for paired studies, hence yielding the usual non-permutational t-statistic. When hidden bias is allowed to corrupt inference, finding both the appropriate initial test statistic and the appropriate standard deviation by which to studentize is itself non-trivial, and is presented in §3.2. The studentized procedure has many attractive features beyond asymptotically maintaining the desired size in the presence of unmeasured confounding, which are discussed in detail in §§5 and 6. Proposition 5 of §6 shows that the design sensitivity for the studentized procedure is identical to that of the permutational t-test, indicating that the procedure provides Type I error control for heterogeneous effects without sacrificing power. The procedure operates within the familiar model for biased treatment assignments of (Rosenbaum, 2002b, §4.2) and is straightfor-

ward to implement, with an `R` script provided in the supplementary materials. The studentized procedure thus empowers researchers with a sensitivity analysis for the sample average treatment effect valid in the face of effect heterogeneity, while researchers having conducted a sensitivity analysis using the permutational-t in the past can be reasonably assured that their results do not materially overstate insensitivity to hidden bias.

2 Sensitivity analysis for constant effects

2.1 Notation and review

There are I independent matched pairs. In the i^{th} matched pair there is one individual who receives the treatment, $Z_{ij} = 1$, and one who receives the control, $Z_{ij'} = 0$, such that $Z_{i1}+Z_{i2} = 1$ for each i. These pairs are formed on the basis of observed pre-treatment covariates x_{ij} so that $x_{i1} = x_{i2}$ in each pair i; however, individuals may differ on the basis of an unobserved covariate $0 \leq u_{ij} \leq 1$, such that $u_{i1} \neq u_{i2}$. Each individual has a potential outcome under treatment, r_{Tij}, and under control, r_{Cij}. Implicit in this description of the potential outcomes is the stable unit-treatment value assumption, or SUTVA (Rubin, 1980). Let $\mathcal{F}_I = \{(r_{Tij}, r_{Cij}, x_{ij}, u_{ij}), i = 1, ..., I, j = 1, 2\}$.

The fundamental problem of causal inference is that the pair of potential outcomes (r_{Tij}, r_{Cij}) is not jointly observable, and hence we cannot observe the individual treatment effect $\tau_{ij} = r_{Tij} - r_{Cij}$ for any individual (Holland, 1986). Instead, we observe the response $R_{ij} = r_{Tij}Z_{ij} + r_{Cij}(1-Z_{ij})$, and the observed treated-minus-control paired differences $Y_i = (Z_{i1}-Z_{i2})(R_{i1}-R_{i2})$. See Neyman (1923) and Rubin (1974) for more on the potential outcomes framework. Let $\Delta_i = (\tau_{i1} + \tau_{i2})/2$ be the average of the two treatment effects in pair i; let $\ell_{ij} = (r_{Tij} + r_{Cij})/2$ be the average of the potential outcomes for individual j in pair i (also referred to as the "level" of the potential outcomes); and let $\eta_i = \ell_{i1} - \ell_{i2}$ be the difference between the potential outcome levels of individuals 1 and 2 in pair i. Write $V_i = Z_{i1} - Z_{i2}$, such that $V_i = 1$ if the first unit in a pair received the treatment and equals -1 if the second unit received the treatment. The i^{th} treated-minus-control paired difference Y_i can then be expressed as

$$Y_i = \Delta_i + V_i\eta_i. \tag{1}$$

2.2 A Model for Hidden Bias in Observational Studies

Let Ω_I be the set of 2^I possible values of Z under the matched pairs design, i.e. $\Omega_I = \{z : z_{i1} + z_{i2} = 1, i = 1, ..., I\}$. In a paired randomized experiment, each $z \in \Omega_I$ has probability 2^{-I} of being selected. Let $\mathcal{Z}_I$ denote the event $\{z \in \Omega_I\}$. For a paired randomized experiment, $\pi_i = \text{pr}(Z_{i1} = 1 \mid \mathcal{F}_I, \mathcal{Z}_I) = 1/2$. Both Δ_i and η_i are functions of $\mathcal{F}_I$, so are themselves fixed by conditioning upon $\mathcal{F}_I$.

Without control over the assignment mechanism, the probabilities $\pi_i, i = 1, ..., I$, are unknown to the researcher in an observational study. A concern in the analysis of observational studies is that $\pi_i \neq 1/2$ in a given pair i despite $x_{i1} = x_{i2}$ due to latent discrepancies between the unobserved covariates u_{i1} and u_{i2}. Through a sensitivity analysis, one seeks to assess the robustness of a study's finding to deviations from a paired experiment caused by unmeasured confounding. A

sensitivity analysis places bounds on the allowable departure from a pair-randomized experiment. We use the model of Rosenbaum (1987) and Rosenbaum (2002b, §4.2), which controls the allowable departure from a paired randomized experiment through a parameter $\Gamma = \exp(\gamma) \geq 1$. In each pair, the model relates u_{i1} and u_{i2} to π_i by $\pi_i = \exp(\gamma u_{i1})/(\exp(\gamma u_{i1}) + \exp(\gamma u_{i2}))$, which implies that $1/(1+\Gamma) \leq \pi_i \leq \Gamma/(1+\Gamma)$. The resulting model for biased treatment assignment in a paired observational study (Rosenbaum, 1987) with sensitivity parameter Γ is

$$\text{pr}(Z = z \mid \mathcal{F}_I, \mathcal{Z}_I) = \prod_{i=1}^{I} \pi_i^{z_{i1}}(1-\pi_i)^{1-z_{i1}}, \quad \frac{1}{1+\Gamma} \leq \pi_i \leq \frac{\Gamma}{1+\Gamma} \quad (i = 1, ..., I). \tag{2}$$

$\Gamma = 1$ recovers a paired randomized experiment, while $\Gamma > 1$ encodes a family of departures from unbiased assignments within each pair. Let $\theta_\Gamma = \Gamma/(1+\Gamma)$ be the corresponding upper bound on π_i for a particular value of Γ.

2.3 Sensitivity analysis under additivity

Suppose interest lies in testing the hypothesis

$$H_\tau : \tau_{ij} = r_{Tij} - r_{Cij} = \tau \quad \text{for all } i, j,$$

implying that the treatment effect takes a common value τ for the $2I$ individuals in the study. Consider the adjusted responses $\tilde{R}_{ij} = R_{ij} - Z_{ij}\tau$. Under H_τ, $\tilde{R}_{ij} = r_{Cij}$ for each individual. We further see from (1) that, conditional upon H_τ, $Y_i - \tau = (Z_{i1} - Z_{i2})(r_{Ci1} - r_{Ci2})$, and as a result that $|Y_i - \tau|$ is fixed at $|r_{Ci1} - r_{Ci2}|$ across randomizations under H_τ given $\mathcal{F}_I$ and $\mathcal{Z}_I$. Consider the test statistic $T(Z, R - Z\tau) = I^{-1}\sum_{i=1}^{I}(Y_i - \tau) = \bar{Y} - \tau$, the average of the I adjusted responses commonly referred to as the permutational t-statistic (Rosenbaum, 2007). Under H_τ, the function $T(z, R - z\tau) = T(z, r_c)$ is computable for any $z \in \Omega_I$. The distribution of $T(Z, R - Z\tau)$ under H_τ can then be expressed as, for any scalar a,

$$\text{pr}\left(I^{-1}\sum_{i=1}^{I}(Y_i - \tau) \geq a \mid \mathcal{F}_I, \mathcal{Z}_I, H_\tau\right) = \sum_{z \in \Omega_I} \chi\left\{T(z, r_C) \geq a\right\} \prod_{i=1}^{I} \pi_i^{z_{i1}}(1-\pi_i)^{1-z_{i1}}, \tag{3}$$

where $\chi\{A\}$ is an indicator that the event A occurred. While $T(z, r_C)$ is known for each $z \in \Omega_I$ under H_τ, (3) remains unknown as it depends on the unknown probabilites π_i. A sensitivity analysis proceeds by, for a given value of Γ in (2), finding the values for π_i yielding the worst-case p-value in (3), thus specifying the worst-case reference distribution for the desired inference. One then iteratively increases the value of Γ until the null hypothesis can no longer be rejected. This changepoint Γ serves as a measure of the robustness of the study's findings to unmeasured confounding.

As an illustration, suppose we sought to maximize (3), the p-value with a greater than alternative, subject to (2) holding at a particular value Γ. Consider the I random variables $\tilde{V}_{i,\Gamma}|Y_i - \tau|$, where $\tilde{V}_{i,\Gamma}$ are conditionally independent given $\mathcal{F}_I$ and $\mathcal{Z}_I$ and take the values -1 and 1 with probability $(1-\theta_\Gamma)$ and θ_Γ respectively. Rosenbaum (2007) illustrates that under H_τ,

$Y_i - \tau$ is stochastically dominated by $\tilde{V}_{i,\Gamma}|Y_i - \tau| = \tilde{V}_{i,\Gamma}|r_{Ci1} - r_{Ci2}|$. $T(Z, r_c)$ is then stochastically dominated by $I^{-1}\sum_{i=1}^{I}\tilde{V}_{i,\Gamma}|Y_i - \tau|$, whose randomization distribution is computable under H_τ. In summary, for any scalar a,

$$\begin{aligned}\text{pr}\left(\bar{Y} - \tau \geq a \mid \mathcal{F}_I, \mathcal{Z}_I, H_\tau\right) &\leq \text{pr}\left(I^{-1}\sum_{i=1}^{I}\tilde{V}_{i,\Gamma}|Y_i - \tau| \geq a \mid \mathcal{F}_I, \mathcal{Z}_I, H_\tau\right) \\ &= \sum_{z\in\Omega_I}\chi\left\{I^{-1}\sum_{i=1}^{I}(z_{i1} - z_{i2})|Y_i - \tau| \geq a\right\}\prod_{i=1}^{I}\theta_\Gamma^{z_{i1}}(1-\theta_\Gamma)^{1-z_{i1}}. \quad (4)\end{aligned}$$

In the preceding illustration, we heavily utilized the assumption that the individual-level treatment effects were specified under H_τ. In what follows, we evaluate whether or not (4) also bounds the maximal tail probability when τ is instead the *average* of the $2I$ possibly heterogeneous treatment effects.

3 Large sample sensitivity analysis for the sample average treatment effect under effect heterogeneity

3.1 Neyman's notion of an overall treatment effect

The sample average treatment effect in a paired experiment or observational study, $\bar{\Delta}$, is defined as the average of the treatment effects τ_{ij} for the $2I$ individuals in our study, that is $\bar{\Delta} = I^{-1}\sum_{i=1}^{I}\Delta_i = (2I)^{-1}\sum_{i=1}^{I}\sum_{j=1}^{2}\tau_{ij}$. Forthcoming developments will focus on developing a valid level-α sensitivity analysis for the null hypothesis

$$\bar{H}_\tau : \bar{\Delta} = \tau$$

when (2) is assumed to hold at a particular value for Γ. Valid tests of both H_τ and $\bar{H}_\tau$ can, in the usual way, be inverted to form confidence intervals for $\bar{\Delta}$ (Lehmann and Romano, 2005, §5.4). If we were to further assume that the treatment effect was constant for all individuals, the null hypotheses $\bar{H}_\tau$ and H_τ would be equivalent, and the sensitivity analysis through the permutational t-test described in §2.3 would be applicable, yielding both exact tests of hypotheses and exact confidence intervals. In general, the null hypothesis $\bar{H}_\tau$ is composite, and there are infinitely many allocations for τ_{ij}, $i = 1, ..., I$, $j = 1, 2$ which satisfy $\bar{H}_\tau$. The pattern of treatment effects specified by H_τ is simply one element of this composite null, which may or may not yield the worst-case p-value over all patterns of treatment effects in $\bar{H}_\tau$.

3.2 A Neyman-style sensitivity analysis

For any constant τ and any constant $\Gamma \geq 1$, define the random variable $D_{i,\tau,\Gamma}$ by

$$D_{i,\tau,\Gamma} = Y_i - \tau - (2\theta_\Gamma - 1)|Y_i - \tau|$$

At $\Gamma = 1$, $D_{i,\tau,1}$ equals $Y_i - \tau$, the average observed treated-minus-control difference in response minus τ. Observe that while $|Y_i - \tau|$ is fixed at $|r_{Ci1} - r_{Ci2}|$ under H_τ, $|Y_i - \tau|$ is generally a random variable for other elements of $\bar{H}_\tau$. Capturing the impact of $|Y_i - \tau|$ on the overall variation of $D_{i,\tau,\Gamma}$ when $\Gamma > 1$ is essential in what follows. $\bar{D}_{\tau,\Gamma}$ is simply the test statistic used when conducting a sensitivity analysis through the permutational t-test as described in §2.3 minus the expectation of the stochastically bounding distribution for $\bar{Y} - \tau$ under H_τ. We now seek to understand, and subsequently bound, the distribution of $\bar{D}_{\tau,\Gamma}$ under effect heterogeneity.

Towards that end, let S_x^2 denote the conventional estimator of the variance for the sample mean based upon a vector of observations x of length I. For example,

$$S^2_{D_{\tau,\Gamma}} = \frac{1}{I(I-1)} \sum_{i=1}^{I} (D_{i,\tau,\Gamma} - \bar{D}_{\tau,\Gamma})^2.$$

At $\Gamma = 1$, $S^2_{D_{\tau,1}}$ does not depend on τ and is simply the classical variance estimator for the sample average treatment effect estimator $\bar{Y}$ in a paired experiment. Fix α with $0 < \alpha \leq 0.5$. Consider a candidate level-α test that the sample average treatment effect equals τ with a greater-than alternative with allowable degree of bias controlled by Γ in (2) of the form

$$\varphi_N(\tau, \alpha, \Gamma) = \chi\{\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}} \geq \Phi^{-1}(1-\alpha)\}$$

where $\Phi(\cdot)$ is the cumulative distribution function of the standard normal.

Proposition 1. *Suppose that treatment assignment satisfies (2) for a particular $\Gamma \geq 1$ and that τ equals the true sample average treatment effect, $\bar{\Delta}$. If there exist constants $C > 0$, μ_m and μ_a such that as $I \to \infty$*

$$I^{-1} \sum_{i=1}^{I} |\eta_i| > C, \quad I^{-1} \sum_{i=1}^{I} \eta_i^2 > C, \tag{5}$$

$$I^{-2} \sum_{i=1}^{I} \eta_i^2 \to 0, \quad I^{-2} \sum_{i=1}^{I} \eta_i^4 \to 0, \quad I^{-2} \sum_{i=1}^{I} \Delta_i^4 \to 0, \tag{6}$$

$$I^{-1} \sum_{i=1} (2\pi_i - 1)\eta_i \to \mu_m, \quad I^{-1} \sum_{i=1}^{I} \pi_i |\Delta_i - \tau + \eta_i| + (1-\pi_i)|\Delta_i - \tau - \eta_i| \to \mu_a, \tag{7}$$

then

$$\lim_{I \to \infty} E(\varphi_N(\tau, \alpha, \Gamma) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \leq \alpha.$$

The conditions (5) - (7) serve to preclude certain pathological sequences for the constants η_i and Δ_i, ensuring, for example, that a central limit theorem holds for $\bar{D}_{\tau,\Gamma}$. Under these conditions, Proposition 1 implies that for sufficiently large I, if we reject the null hypothesis $\bar{H}_\tau$ when $\bar{D}_{\tau,\Gamma}/S_{\tau,\Gamma} \geq \Phi^{-1}(1-\alpha) \Leftrightarrow \varphi_N(\tau, \alpha, \Gamma) = 1$, then asymptotically we will incorrectly reject a true null hypothesis with probability at most α when (2) holds at Γ. That is, despite the allowed heterogeneity under $\bar{H}_\tau$, $\varphi_N(\tau, \alpha, \Gamma)$ provides an asymptotically valid level-α sensitivity

analysis for the sample average treatment effect.

The proof is divided into several lemmas, each of which illustrates an important component of the procedure. Those most essential to the result are presented here, while those stemming from standard derivations are deferred to the appendix. Lemma 1 constructs a new random variable $\bar{U}_{\tau,\Gamma}$ that stochastically bounds $\bar{D}_{\tau,\Gamma}$ for any sample size I; however, its randomization distribution is not directly useful as it depends on the unknown values of Δ_i and η_i. Lemma 2 shows that the conditional expectation of $\bar{U}_{\tau,\Gamma}$, and hence of $\bar{D}_{\tau,\Gamma}$, is bounded above by 0 when the sample average treatment effect equals τ and (2) holds at Γ. Lemma 3 illustrates that $S^2_{D_{\tau,\Gamma}}$ yields an estimator of $\text{var}(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$ which is conservative in expectation regardless of the values for the unknown probabilities π_i. Lemma 4 shows that the expectation $S^2_{D_{\tau,\Gamma}}$ is also larger than $\text{var}(\bar{U}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$ when (2) holds at Γ. Together, these results bound, in expectation, the moments of the stochastically dominating random variable $\bar{U}_{\tau,\Gamma}$ by quantities computable from the observational study at hand. In the appendix, we illustrate that under conditions (5) - (7), this sharp bounding random variable has a distribution which is asymptotically normal. We then demonstrate that despite the true moments for the bounding random variable being unknown, the true expectation of $\bar{U}_{\tau,\Gamma}$ can be safely replaced by zero, and the true variance of $\text{var}(\bar{U}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$ similarly replaced by $S^2_{D_{\tau,\Gamma}}$ without corrupting the asymptotic size of the procedure.

Define I new random variables $U_{i,\tau,\Gamma} = \Delta_i - \tau + \tilde{V}_{i,\Gamma}|\eta_i| - (2\theta_\Gamma - 1)\{(1 + \tilde{V}_{i,\Gamma})|\Delta_i - \tau + |\eta_i|| + (1 - \tilde{V}_{i,\Gamma})|\Delta_i - \tau - |\eta_i||\}/2$, where the random variables $\tilde{V}_{i,\Gamma}$ are defined as in §2.3.

Lemma 1. *Suppose treatment assignment satisfies (2) at Γ. Then, for any constant τ and any constant k,*

$$pr(\bar{D}_{\tau,\Gamma} \geq k \mid \mathcal{F}_I, \mathcal{Z}_I) \leq pr\left(\bar{U}_{\tau,\Gamma} \geq k \mid \mathcal{F}_I, \mathcal{Z}_I\right) \tag{8}$$

Moreover, the upper bound is sharp in that sense that (8) holds if $\pi_i = \theta_\Gamma$ and $\eta_i \geq -\eta_i$ for $i = 1, ..., I$

Proof. $V_i\eta_i = \pm|\eta_i|$ and, by (2), $1 - \theta_\Gamma \leq \text{pr}(V_i\eta_i = |\eta_i| \mid \mathcal{F}_I, \mathcal{Z}_I) \leq \theta_\Gamma$. Now, $\tilde{V}_{i,\Gamma}|\eta_i| = \pm|\eta_i|$, and $\text{pr}(\tilde{V}_{i,\Gamma}|\eta_i| = |\eta_i| \mid \mathcal{F}_I, \mathcal{Z}_I) = \theta_\Gamma$, so that $\tilde{V}_{i,\Gamma}|\eta_i|$ stochastically dominates $V_i\eta_i$. As the function $x - (2\theta_\Gamma - 1)|x|$ is monotone nondecreasing for all x and for any Γ, we have that $\Delta_i - \tau + |\eta_i| - (2\theta_\Gamma - 1)|\Delta_i - \tau + |\eta_i|| \geq \Delta_i - \tau - |\eta_i| - (2\theta_\Gamma - 1)|\Delta_i - \tau - |\eta_i||$. The random variable $D_{i,\tau,\Gamma}$ is thus stochastically dominated by $U_{i,\tau,\Gamma}$ given $\mathcal{F}_I$ and $\mathcal{Z}_I$ for each i, and (8) then follows from a standard probability inequality (see, e.g., Ahmed et al., 1981, Lemma 3.3). □

Lemma 2. *For any constant τ, if (2) holds at Γ, then*

$$E(\bar{U}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 4\theta_\Gamma(1 - \theta_\Gamma)I^{-1}\sum_{i=1}^{I}(\Delta_i - \tau)$$

In particular, if τ is set to the true sample average treatment effect $\bar{\Delta}$, then $E(\bar{U}_{\bar{\Delta},\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 0$.

Proof. For each i, $E(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = (2\theta_\Gamma - 1)|\eta_i| + \Delta_i - \tau - (2\theta_\Gamma - 1)\{\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1 - \theta_\Gamma)|\Delta_i - \tau - |\eta_i||\}$. Since $1/2 \leq \theta_\Gamma \leq 1$, the inequality $\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1 - \theta_\Gamma)|\Delta_i - \tau - |\eta_i|| \geq$

$|\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau)$ always holds (we show this in the appendix for completeness). Hence, $E(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq (2\theta_\Gamma - 1)|\eta_i| + \Delta_i - \tau - (2\theta_\Gamma - 1)\{|\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau)\}$
$= 4\theta_\Gamma(1 - \theta_\Gamma)(\Delta_i - \tau)$. As this holds for all i, the result for the average follows. □

Lemma 3. *For any constant τ, and any constant $\Gamma \geq 1$*

$$E(S^2_{D_{\tau,\Gamma}} \mid \mathcal{F}_I, \mathcal{Z}_I) - var(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$$
$$= \frac{1}{I(I-1)} \sum_{i=1}^{I} \left(E(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) - E(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)\right)^2 \geq 0.$$

Proof. The proof is similar to that of Proposition 1 of Imai (2008), and is deferred to the appendix. □

Lemma 4. *If (2) holds at Γ, then for any constant τ*

$$var\left(\bar{U}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I\right) \leq E(S^2_{D_{\tau,\Gamma}} \mid \mathcal{F}_I, \mathcal{Z}_I)$$

Proof. By Lemma 3, it suffices to show that $\text{var}(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \geq \text{var}(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$ for all i. But $\text{var}(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = \pi_i(1-\pi_i)\{2\eta_i - (2\theta_\Gamma - 1)(|\Delta_i - \tau + \eta_i| - |\Delta_i - \tau - \eta_i|)\}^2 \geq \theta_\Gamma(1-\theta_\Gamma)\{2|\eta_i| - (2\theta_\Gamma - 1)(|\Delta_i - \tau + |\eta_i|| - |\Delta_i - \tau - |\eta_i||)\}^2 = \text{var}(U_i \mid \mathcal{F}_I, \mathcal{Z}_I)$ since (2) holds at Γ, proving the result. □

4 A studentized sensitivity analysis

The conclusion of Proposition 1 yields the following corollary:

Corollary 1. *Let $\hat{G}_I(\cdot)$ be a sequence of random distribution functions such that, conditional upon $\mathcal{F}_I$ and $\mathcal{Z}_I$, $\hat{G}_I(t) \xrightarrow{p} \Phi(t)$ for all points t as $I \to \infty$. Then, under the conditions of Proposition 1,*

$$\lim_{I\to\infty} pr(\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}} \geq \hat{G}_I^{-1}(1-\alpha) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \leq \alpha,$$

where $\hat{G}_I^{-1}(1-\alpha) = \inf\{t : \hat{G}_I(t) \geq 1-\alpha\}$ is the $(1-\alpha)$ quantile of $\hat{G}_I(\cdot)$.

Proof. The result is an immediate consequence of Proposition 1 along with Lemma 11.2.1 of Lehmann and Romano (2005). □

While any sequence of random distribution functions $\hat{G}_I(t)$ would do, the utility of the above corollary is that it allows for sequences of distribution functions which better reflect the finite-sample distribution of the random variable of interest, here $\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}}$. As simulations in §5.4 illustrate, seeking improvements in finite sample performance over that imparted by $\varphi_N(\cdot)$ is undoubtedly warranted. We now demonstrate that while the conventional worst-case distribution utilized by the permutational t-based sensitivity analysis need not satisfy the conditions of Corollary 1, the issue can be alleviated by means of an appropriate studentization.

Towards that end, let $\tilde{V}_{i,\Gamma}$ be defined as before, and define the random variables $A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y)$, $i = 1, ..., I$, by

$$A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y) = \tilde{V}_{i,\Gamma}|Y_i - \tau| - (2\theta_\Gamma - 1)|Y_i - \tau|.$$

As shorthand, we will sometimes write $A_{i,\tau,\Gamma} = A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y)$ and $\bar{A}_{\tau,\Gamma} = \bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y)$. $A_{i,\tau,\Gamma}$ has expectation and variance

$$E(A_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = 0, \tag{9}$$

$$\text{var}(A_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = 4\theta_\Gamma(1-\theta_\Gamma)\left\{\pi_i(\Delta_i - \tau + \eta_i)^2 + (1-\pi_i)(\Delta_i - \tau - \eta_i)^2\right\}. \tag{10}$$

Define the random variable $S^2_{A_{\tau,\Gamma}} = (I(I-1))^{-1}\sum_{i=1}^I (A_{i,\tau,\Gamma} - \bar{A}_{\tau,\Gamma})^2$, i.e. the sample variance of the vector $A_{\tau,\Gamma}$ divided by I. For a given realization of Y, define biased randomization distributions $\hat{F}_{\tau,\Gamma}(x)$ and $\hat{G}_{\tau,\Gamma}(x)$ by

$$\hat{F}_{\tau,\Gamma}(x) = \sum_{z\in\Omega_I} \chi\{\bar{A}_{\tau,\Gamma}(z_1 - z_2, Y) \leq x\} \prod_{i=1}^I \theta_\Gamma^{z_{i1}}(1-\theta_\Gamma)^{1-z_{i1}}, \tag{11}$$

$$\hat{G}_{\tau,\Gamma}(t) = \sum_{z\in\Omega_I} \chi\left\{\frac{\bar{A}_{\tau,\Gamma}(z_1 - z_2, Y)}{S_{A_{\tau,\Gamma}(z_1-z_2,Y)}} \leq t\right\} \prod_{i=1}^I \theta_\Gamma^{z_{i1}}(1-\theta_\Gamma)^{1-z_{i1}}, \tag{12}$$

where $z_j = (z_{1j}, z_{2j}, ..., z_{Ij})$ contains the treatment assignments for the j^{th} unit in each pair. $\hat{F}_{\tau,\Gamma}(x)$ and $\hat{G}_{\tau,\Gamma}(t)$ are themselves random distribution functions, as they depend on the observed paired differences Y. $\hat{F}_{\tau,\Gamma}(x)$ is simply the left tail of the worst-case randomization distribution given in (4) shifted by $(2\theta_\Gamma - 1)I^{-1}\sum_{i=1}^I |Y_i - \tau|$. While $\hat{G}_{\tau,\Gamma}(t)$ utilizes the same biased distribution for treatment assignments as does $\hat{F}_{\tau,\Gamma}(x)$, it computes the randomization distribution of the studentized statistic $\bar{A}_{\tau,\Gamma}/S_{A_{\tau,\Gamma}}$ instead of the non-studentized version $\bar{A}_{\tau,\Gamma}$. Critically, both $\bar{A}_{\tau,\Gamma}(z_1 - z_2, Y)$ and $S_{A_{\tau,\Gamma}(z_1-z_2,Y)}$ vary across elements of Ω_I in (12).

Define a candidate level-α sensitivity analysis at Γ for $\bar{H}_\tau$ against the alternative that $\bar{\Delta} > \tau$ based on the permutational t-test by

$$\varphi_F(\tau, \alpha, \Gamma) = \chi\left\{\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}} \geq \hat{F}^{-1}_{\tau,\Gamma}(1-\alpha)/S_{D_{\tau,\Gamma}}\right\},$$

and based on its studentized version by

$$\varphi_S(\tau, \alpha, \Gamma) = \chi\left\{\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}} \geq \hat{G}^{-1}_{\tau,\Gamma}(1-\alpha)\right\}.$$

Proposition 2. *Suppose that, in addition to conditions (5) - (7), there exists a constant $\nu^2 > 0$ such that*

$$I^{-1}\sum_{i=1}^I \pi_i(\Delta_i - \tau + \eta_i)^2 + (1-\pi_i)(\Delta_i - \tau - \eta_i)^2 \to \nu^2, \tag{13}$$

and let $\nu_\Gamma^2 = 4\theta_\Gamma(1-\theta_\Gamma)\nu^2$. *Then, for all* a *and* t, *and conditional upon* $\mathcal{F}_I$ *and* $\mathcal{Z}_I$,

$$\hat{F}_{\tau,\Gamma}(a/I^{1/2}) \xrightarrow{p} \Phi(a/\nu_\Gamma)$$
$$\hat{G}_{\tau,\Gamma}(t) \xrightarrow{p} \Phi(t)$$

Proposition 2, in concert with Corollary 1, states that the studentized randomization distribution $\hat{G}_{\tau,\Gamma}(t)$ can be used as a null distribution for the test statistic $\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}}$ in order to conduct an asymptotically conservative sensitivity analysis at level of unmeasured confounding Γ for $\bar{H}_\tau$ even in the presence of effect heterogeneity. That is, under the conditions of Proposition 2,

$$\lim_{I\to\infty} E(\varphi_S(\tau,\alpha,\Gamma) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \leq \alpha$$

when treatment assignment satisfies (2) at Γ and the sample average treatment effect, $\bar{\Delta}$, truly equals τ. We call the resulting procedure the *studentized sensitivity analysis*. In the next section, we assess the implications of Proposition 2 for the validity of the permutational t-based sensitivity analysis based on the random measure $\hat{F}_{\tau,\Gamma}(x)$.

We once again break the proof of Proposition 2 into a series of lemmas, which are presented in the appendix. There, we first prove that conditional upon $\mathcal{F}_I$ and $\mathcal{Z}_I$, $\hat{F}_{\tau,\Gamma}(a/I^{1/2})$ converges in probability to $\Phi(a/\nu_\Gamma)$ for all points a by slightly modifying an argument first employed by Hoeffding (1952) for permutation distributions; see also Lehmann and Romano (2005, Theorem 15.2.3) and Chung and Romano (2013, Theorem 5.1). The argument requires that for independent and identically distributed vectors $\tilde{V}_\Gamma$ and $\tilde{V}'_\Gamma$, $I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y)$ and $I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}'_\Gamma, Y)$ are *iid* and converge jointly to a bivariate normal, each with mean zero and variance ν_Γ^2. This follows from (13), (6), and the Cramér-Wold device. We then illustrate that $IS^2_{A_{\tau,\Gamma}}$ converges in probability to ν_Γ^2, which follows from (13) and (6). Finally, recalling that $\hat{G}_{\tau,\Gamma}(\cdot)$ differs from $\hat{F}_{\tau,\Gamma}(\cdot)$ only by studentization, we utilize Slutsky's theorem for randomization distributions (Chung and Romano, 2013, Theorem 5.2) to show that $\hat{G}_{\tau,\Gamma}(t)$ converges in probability to $\Phi(t)$ for all t.

5 Implications for the permutational t-test

5.1 Potential for improper size at $\Gamma > 1$

From (10) $\bar{A}_{\tau,\Gamma}$, the random variable used to facilitate the permutational t-based sensitivity analysis under H_τ, has variance

$$\text{var}(\bar{A}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = 4\theta_\Gamma(1-\theta_\Gamma)I^{-2}\sum_{i=1}^{I}(\Delta_i-\tau)^2 + \eta_i^2 + 2(2\pi_i-1)\eta_i(\Delta_i-\tau).$$

Meanwhile $\bar{D}_{\tau,\Gamma}$, the random variable whose distribution we seek to bound when testing $\bar{H}_\tau$, has variance

$$\text{var}(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) = I^{-2} \sum_{i=1}^{I} \pi_i(1-\pi_i) \left\{2\eta_i - (2\theta_\Gamma - 1)(|\Delta_i - \tau + \eta_i| - |\Delta_i - \tau - \eta_i|)\right\}^2.$$

If (2) holds at $\Gamma = 1$ and we correctly conduct inference at $\Gamma = 1 \Leftrightarrow \theta_\Gamma = 1/2$, then $\text{var}(\bar{A}_{\tau,1} \mid \mathcal{F}_I, \mathcal{Z}_I) = I^{-2}\sum_{i=1}^{I}(\Delta_i - \tau)^2 + \eta_i^2$ as $\pi_i = 1/2$ for all individuals, while under the same conditions $\text{var}(\bar{D}_{\tau,1} \mid \mathcal{F}_I, \mathcal{Z}_I) = I^{-2}\sum_{i=1}^{I}\eta_i^2 \leq \text{var}(\bar{A}_{\tau,1} \mid \mathcal{F}_I, \mathcal{Z}_I)$. In combination with (11), the permutational t-test thus yields asymptotically conservative inference in the presence of effect heterogeneity at $\Gamma = 1$ (i.e., in the context of a paired randomized experiment). Meanwhile, the bias expression for $S^2_{D_{\tau,1}}$ given in Lemma 3 when $\Gamma = 1$ takes the form $E(S^2_{D_{\tau,1}} \mid \mathcal{F}_I, \mathcal{Z}_I) = \text{var}(\bar{D}_{\tau,1} \mid \mathcal{F}_I, \mathcal{Z}_I) + (I(I-1))^{-1}\sum_{i=1}^{I}(\Delta_i - \bar{\Delta})^2$ regardless of the value of τ. Hence, when τ is set to the true sample average treatment effect $\bar{\Delta}$, the Neyman-style sensitivity analysis, the studentized sensitivity analysis, and the classical permutational t-based sensitivity analysis all have the same asymptotic performance under $\bar{H}_{\bar{\Delta}}$ at $\Gamma = 1$. At $\Gamma > 1$, there exist allocations of potential outcomes for which $\text{var}(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) > \text{var}(\bar{A}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$. This possibility, along with asymptotic normality of both $\bar{D}_{\tau,\Gamma}$ and $\bar{A}_{\tau,\Gamma}$, underpins the following result concerning the conventional sensitivity analysis based on the permutational t-test in the presence of effect heterogeneity.

Proposition 3. *Consider conducting a sensitivity analysis at Γ for $\bar{H}_\tau$ through the permutational t-test, $\varphi_F(\tau, \alpha, \Gamma)$. Then, there exist allocations of potential outcomes satisfying the conditions of Proposition 2 such that, if (2) holds at Γ*

$$\lim_{I\to\infty} E(\varphi_F(\tau, \alpha, \Gamma) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) > \alpha.$$

That is, the conventional sensitivity analysis through the permutational t-test test fails to control the Type I error rate under effect heterogeneity if (2) holds at $\Gamma > 1$ for certain patterns of effect heterogeneity in the composite null $\bar{H}_\tau$.

As a numerical illustration of Proposition 3, let I be even and consider the following allocation for Δ_i, η_i, and π_i:

$$\{\Delta_i, \eta_i, \pi_i\} = \begin{cases} \{5, 5, 4/5\} & i = 1, ..., I/2 \\ \{0, 20, 4/5\} & i = I/2+1, ..., I \end{cases} \tag{14}$$

Here $\bar{\Delta} = 2.5$, (2) holds at $\Gamma = 4$, but the effects are heterogeneous. The random variable $\bar{D}_{2.5,4}$ has expectation 0 and variance $151.84/I$, while $\bar{A}_{2.5,4}$ has expectation 0 and variance $125.6/I$. Asymptotically, the classical sensitivity analysis utilizing the permutational t will reject $\bar{H}_{2.5}$ while attempting to maintain the size at α if $\bar{D}_{2.5,4} \geq (125.6/I)^{1/2}\Phi^{-1}(1-\alpha)$, which occurs with probability $1 - \Phi\{(125.6/151.84)^{1/2}\Phi^{-1}(1-\alpha)\} > \alpha$ if $\alpha < 0.5$. For example, at $\alpha = 0.05$, the permutational t-based sensitivity analysis has an asymptotic Type I error rate of 0.067.

5.2 Proper asymptotic size at $\Gamma + \epsilon$

How severe is the potential anti-conservativeness of the permutational t-test in a sensitivity analysis? As described in §2.3, a sensitivity analysis typically proceeds by iteratively increasing the value of Γ being tested until we transition from rejecting the null hypothesis to failing to reject the null hypothesis. In large samples, the behavior of a sensitivity analysis is bias-dominated (Rosenbaum, 2004). As (9) displays, the permutational t-test correctly bounds the expectation of $\bar{D}_{\tau,\Gamma}$, and the potential for size greater than α stems only from discrepancies in the variance in those instances when the upper bound on the expectation is tight. There is hope, then, that the changepoint Γ returned by the classical permutational t-test may not be grossly unrepresentative of that of returned by the asymptotically valid studentized procedure. As we now formalize, asymptotically the changepoint Γ for a sensitivity analysis conducted by means of the permutational t-test is arbitrarily close to that of the asymptotically valid procedures in the presence of effect heterogeneity.

Proposition 4. *Suppose (2) holds at level* Γ *and that the sample average treatment effect equals* τ*. Consider conducting a sensitivity analysis at level of unmeasured confounding* $\Gamma + \epsilon$ *for any* $\epsilon > 0$ *by means of* $\varphi_F(\tau, \alpha, \Gamma + \epsilon)$*, the permutational t-based sensitivity analysis valid for the constant effect hypothesis* H_τ*. Then, under the assumptions of Proposition 3,*

$$\lim_{I \to \infty} E(\varphi_F(\tau, \alpha, \Gamma + \epsilon) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) = 0$$

That is, if (2) holds at Γ*, the permutational t-test asymptotically commits a Type I error with probability 0 when conducting a sensitivity analysis at* $\Gamma+\epsilon$*, despite potentially having size greater than* α *when the sensitivity analysis is conducted at* Γ*.*

To illustrate Proposition 4, we return to the example given in in (14). Suppose we conduct a sensitivity analysis using the permutational t-test at $\Gamma = 4.01$. The random variable $\bar{D}_{2.5,4.01}$ has expectation -0.01 and variance $151.86/I$, while $\bar{A}_{2.5,4.01}$ has expectation 0 and variance $125.42/I$. If we conduct a sensitivity analysis at $\Gamma = 4.01$ the permutational t-test rejects the null asymptotically if $\bar{D}_{2.5,4.01} \geq \Phi^{-1}(1-\alpha)(125.42/I)^{1/2}$, or equivalently if $(\bar{D}_{2.5,4.01} + 0.01)/(151.86/I)^{1/2} \geq \{\Phi^{-1}(1-\alpha)(125.42/I)^{1/2} + 0.01\}/(151.86/I)^{1/2}$. The left-hand side converges in distribution to a standard normal, while the right-hand side simplifies to $\{0.01I^{1/2} + 125.42^{1/2}\Phi^{-1}(1-\alpha)\}/151.86^{1/2}$, which goes to ∞ as I increases. Hence, the test rejects with probability 0 in the limit. In summary, while $\varphi_F(2.5, \alpha, 4)$ does not asymptotically control the Type I error rate for this example as illustrated in the previous section, $\varphi_F(2.5, \alpha, 4.01)$ does asymptotically, as it would replacing $\Gamma = 4.01$ with $\Gamma = 4 + \epsilon$ for any $\epsilon > 0$.

5.3 Persistence of a permutational paradox

In a recent paper, Ding (2017) described the occurrence of a paradox in the analysis of randomized experiments ($\Gamma = 1$), wherein tests based on $\varphi_F(\tau, \alpha, 1)$ have lower power than tests based on $\varphi_N(\tau, \alpha, 1)$ when $\bar{\Delta} >> \tau$. The paradox lies in that $\varphi_N(\tau, \alpha, 1)$ is designed as a test for Neyman's weak null that the sample average treatment effect takes on the value τ, while $\varphi_F(\tau, \alpha, 1)$ is a test

for the null that the treatment effect is constant at τ (a subset of Neyman's null). As presented in the discusssion of the paper by Aronow and Offer-Westort (2017), the paradox is explained by recognizing that $\varphi_N(\tau, \alpha, 1)$ exploits local information when constructing the variance utilized to perform inference. In short, $\varphi_N(\tau, \alpha, 1)$ uses a variance with residuals centered at the observed difference in means $\bar{Y}$, while $\varphi_F(\tau, \alpha, 1)$ utilizes a reference distribution whose variance centers residuals at the hypothesized value τ. If $\bar{\Delta} >> \tau$, then the variance utilized by $\varphi_F(\tau, \alpha, 1)$ may be substantially inflated, resulting in the decrease in power.

We now illustrate that, for the same reasons, this paradox can persist in a sensitivity analysis when comparing the power of $\varphi_N(\tau, \alpha, \Gamma)$ and $\varphi_S(\tau, \alpha, \Gamma)$ to that of $\varphi_F(\tau, \alpha, \Gamma)$ for $\Gamma > 1$. We turn attention once again to the allocation of potential outcomes in (14). The sample average treatment effect is truly 2.5, yet suppose we instead test the null hypothesis that $\bar{\Delta} = 0$ through $\varphi_N(0, \alpha, 4)$, $\varphi_S(0, \alpha, 4)$ and $\varphi_F(0, \alpha, 4)$. Then, $\bar{D}_{0,4}$ has expectation 1.6 and variance $129.28/I$, while $\bar{A}_{0,4}$ has expectation 0 and variance $153.6/I$, indicating that the variance of the reference distribution utilized by the permutational t is larger than necessary. The variance estimator $S^2_{\bar{D}_{0,4}}$, utilized by the studentized procedure, has expectation $129.28/I + 2.56/(I-1) \approx 131.84/I$, coming much closer to the true variance of $\bar{D}_{0,4}$. $S^2_{\bar{D}_{0,4}}$ is formed by finding the sample variance of the observations $Y_i - 0.6|Y_i|$ and then divides by I, while the distribution utilized by the permutational t is $4(0.8)(0.2)I^{-2}\sum_{i=1}^{I}(Y_i - 0.6|Y_i|)^2$, hence not centering the terms $Y_i - 0.6|Y_i|$. As $E(I^{-1}\sum_{i=1}^{I} Y_i - 0.6|Y_i| \mid \mathcal{F}_I, \mathcal{Z}_I) = E(\bar{D}_{0,4} \mid \mathcal{F}_I, \mathcal{Z}_I) = 1.6$, the variance utilized by the permutational t-test is unduly inflated. By virtue of asymptotic normality of the reference distributions, this provides higher large sample power for $\varphi_N(0, \alpha, 4)$ and $\varphi_S(0, \alpha, 4)$ than for $\varphi_F(0, \alpha, 4)$ despite the fact that, logically, falsehood of Neyman's null at τ implies falsehood of the constant effects model at τ. In §6.3, we present a simulation study wherein this phenomenon occurs for sensitivity analyses conducted in the absence of hidden bias. In §7, we illustrate through the analysis of our motivating example that this phenomenon can also impact the width of confidence intervals created by inverting any of these tests.

5.4 The benefits of the studentized sensitivity analysis

The calculations in §§5.1 - 5.3 compare the asymptotic distribution utilized by the conventional permutational t-test, $\varphi_F(\tau, \alpha, \Gamma)$, to that of the Neyman-style sensitivity analysis $\varphi_N(\tau, \alpha, \Gamma)$ and, equivalently, to that of the studentized sensitivity analysis $\varphi_S(\tau, \alpha, \Gamma)$. In small samples, we now illustrate through a simulation study that the Neyman-style large sample sensitivity analysis can itself become anti-conservative when the true randomization distribution for $\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}}$ exhibits departures from normality. We additionally highlight the ability of the studentized sensitivity analysis to capture departures from normality in small samples, leading us to recommend the studentized procedure over its large sample approximation in practice. We once again proceed with the allocation of $\{\Delta_i, \eta_i, \pi_i\}$ in (14), and set $I = 100$. We then generate 10,000 realizations from the resulting biased randomization distribution (2) with this allocation of biased probabilities. Within each realization, we test the null hypothesis $\bar{\Delta} = 2.5$ against the alternative that $\bar{\Delta} > 2.5$ with desired level $\alpha = 0.05$ and allowable bias $\Gamma = 4$ using $\varphi_N(2.5, 0.05, 4)$, $\varphi_S(2.5, 0.05, 4)$, and $\varphi_F(2.5, 0.05, 4)$. For $\varphi_S(\cdot)$ and $\varphi_F(\cdot)$, we replace $\hat{F}^{-1}_{2.5,4}(0.95)$ and $\hat{G}^{-1}_{2.5,4}(0.95)$ with Monte-Carlo estimates based on 10,000 randomizations.

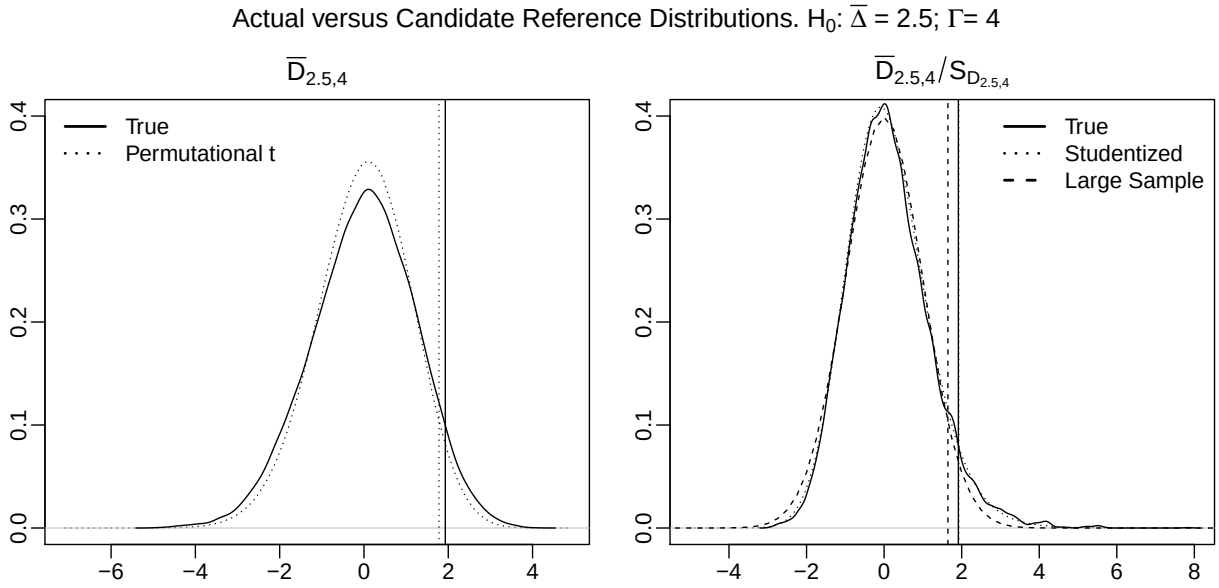

Figure 2: Bounding the randomization distribution in the presence of hidden bias in the simulation study of §5.4. The sample average treatment effect in this simulation equals $\bar{\Delta} = 2.5$, and the smallest Γ for which (2) holds equals 4. (Left) The left-hand side shows the true distribution of $\bar{D}_{2.5,4}$ with a solid line, while the dotted line shows the attempted bounding distribution utilized by the permutational t-test. (Right) The right-hand side shows the true distribution of $\bar{D}_{2.5,4}/S_{D_{2.5,4}}$ (solid), along with the bounding distributions from the studentized sensitivity analysis (dotted) and from the large sample normal approximation (dashed). In both figures, the vertical lines correspond to the 0.95 quantile of the displayed distributions.

Figure 2 shows the true distributions of $\bar{D}_{2.5,4}$ (left) and $\bar{D}_{2.5,4}/S_{D_{2.5,4}}$ (right), along with the distributions utilized by a sensitivity analysis for the null that $\bar{\Delta} = 2.5$ at $\Gamma = 4$. For the sensitivity analysis to control the size at α, the bounding distribution used by each procedure would need to stochastically dominate the true distribution, such that the quantiles generated by each procedure should fall to the right of those of the true distribution. The left-hand side of Figure 2 illustrates that for the permutational t-test the opposite holds: the quantiles of the true distribution fall to the right of those of the candidate worst-case distribution. Correspondingly, the estimated Type I error rate for the permutational t-based sensitivity analysis exceeded 0.05, and was in fact 0.0702. That is, the permutational t-test at $\Gamma = 4$ fails to control the Type I error rate. The right-hand side of Figure 2 illustrates that the Neyman-style large sample test, while valid in sufficiently large samples by Proposition 1, also fails to bound the true distribution of $\bar{D}_{2.5,4}/S_{D_{2.5,4}}$ in this finite sample simulation. The large sample test simply uses a normal approximation to the distribution of $\bar{D}_{2.5,4}/S_{D_{2.5,4}}$, while the figure illustrates that the true distribution exhibits skewness. The Type I error rate is even worse than that of the permutational t, estimated at 0.0798. The studentized sensitivity analysis, asymptotically valid

by Proposition 2, is able to capture the skewness in the true distribution of $\bar{D}_{2.5,4}/S_{D_{2.5,4}}$. Figure 2 shows that the estimated 95^{th} percentile for the studentized test is virtually identical to that of the true distribution. This yields a Type I error rate of 0.053, coming much closer to the desired level $\alpha = 0.05$.

6 Sensitivity analysis in the absence of hidden bias

6.1 A favorable reality unknown to the practitioner

In the absence of hidden bias (i.e., at $\Gamma = 1$), tests based on $\varphi_N(\cdot)$, $\varphi_F(\cdot)$, and $\varphi_S(\cdot)$ all provide asymptotically valid tests for the null hypothesis $\bar{H}_\tau$ as described in §5.1. Under these circumstances, we now assess whether or not the performance of $\varphi_N(\cdot)$ and $\varphi_S(\cdot)$, valid asymptotically under effect heterogeneity even when (2) holds at $\Gamma > 1$, lags behind that of $\varphi_F(\cdot)$. We address this question in the context of a scenario which is favorable to the researcher as advocated in Rosenbaum (2004) and Rosenbaum (2010, §14): the true sample average treatment effect, $\bar{\Delta}$, is larger than the hypothesized value τ, yet unbeknownst to the researcher there is no unmeasured confounding ($\Gamma = 1$). The practitioner is, as would be the case in reality, blind to this fact, and hence would like to not only perform inference under the assumption of no unmeasured confounding, but also to assess the robustness of the study's findings to unmeasured confounding through a sensitivity analysis. We assume throughout §6.1 that the paired differences Y_i are drawn independently and identically distributed from a distribution $\Upsilon(\cdot)$ with $E(Y_i^2) < \infty$. Let $E(Y_i) = \bar{\Delta}$ and $\text{var}(Y_i) = \sigma^2$. Calculations are then presented with respect to this superpopulation formulation for various choices of $\bar{\Delta}$, σ^2, and $\Upsilon(\cdot)$.

6.2 Design sensitivity

In a sensitivity analysis, bias dominates variance in large samples. In fact, under mild regularity conditions there is a number $\tilde{\Gamma}$, called the design sensitivity, such that the power of a sensitivity analysis tends to 1 if $\Gamma < \tilde{\Gamma}$ and tends to 0 if $\Gamma > \tilde{\Gamma}$ as $I \to \infty$ in the favorable situation of no bias in treatment assignment (Rosenbaum, 2004). The design sensitivity is closely related to the Bahadur efficiency of a sensitivity analysis (Rosenbaum, 2015), and can be used as a means of appraising competing design strategies, comparing different test statistics, and assessing the impact of different assumptions within a sensitivity analysis. Larger values for $\tilde{\Gamma}$ indicate reduced sensitivity of inferences to unmeasured confounding in large samples.

For any value of Γ, $\varphi_N(\cdot)$, $\varphi_F(\cdot)$, and $\varphi_S(\cdot)$ all utilize the random variable $\bar{D}_{\tau,\Gamma} = I^{-1}\sum_{I=1}^{I}(Y_i - \tau) - (2\theta_\Gamma - 1)|Y_i - \tau|$. Under the favorable situation being considered this random variable has expectation $\bar{\Delta} - \tau - (2\theta_\Gamma - 1)E|Y_i - \tau|$, yet in a sensitivity analysis we replace this true expectation with 0, the worst-case expectation for $\bar{D}_{\tau,\Gamma}$ as proven in Lemma 2. While the variances of the worst-case null distributions differ between these procedures, this does not affect the design sensitivity as, once again, the sensitivity analysis becomes bias dominated. Under mild conditions, for all three procedures the design sensitivity is simply the value of Γ that solves $\bar{\Delta} - \tau - (2\theta_\Gamma - 1)E|Y_i - \tau| = 0$, which gives rise to the following result.

Proposition 5. *Suppose that the paired differences Y_i are iid draws from a distribution $\Upsilon(\cdot)$ with mean $E(Y_i) = \bar{\Delta} > \tau$ and finite variance $var(Y_i) = \sigma^2 < \infty$. Then, $\varphi_F(\tau, \alpha, \Gamma)$, $\varphi_N(\tau, \alpha, \Gamma)$, and $\varphi_S(\tau, \alpha, \Gamma)$ have the same design sensitivity for testing the null hypothesis $\bar{H}_\tau$, given by*

$$\tilde{\Gamma} = \frac{E|Y_i - \tau| + |\bar{\Delta} - \tau|}{E|Y_i - \tau| - |\bar{\Delta} - \tau|}. \tag{15}$$

Proof. The result for $\varphi_F(\cdot)$ is an immediate consequence of Corollary 1 of Rosenbaum (2013). For $\varphi_N(\cdot)$ (and, equivalently, $\varphi_S(\cdot)$), under the assumption of *iid* draws from a distribution with finite variance $IS^2_{D_{\tau,\Gamma}}$ has a limit in probability. Therefore, Proposition 2 of Rosenbaum (2013) holds, with Corollary 1 of Rosenbaum (2013) once again furnishing the formula for the design sensitivity. □

6.3 The simulated power of a sensitivity analysis

While a sensitivity analysis is bias-dominated in large samples, in small and moderate samples discrepancies in variance play a larger role. Through a simulation study, we assess the power of a sensitivity analysis conducted through $\varphi_F(\cdot)$, $\varphi_N(\cdot)$, and $\varphi_S(\cdot)$ in finite samples under the favorable setting described in §6.1 In each simulation, I observed paired differences are drawn independently from a normal distribution with mean $\bar{\Delta} = 1/2$ and variance $\sigma^2 = 1/2$ under $\Gamma = 1$, no unmeasured confounding. Applying the formula (15) yields that for all three procedures, $\tilde{\Gamma} = 6$. For each iteration, we test the null that $\bar{\Delta} = 0$ with a greater than alternative for a range of values for Γ. We conduct this simulation study for $I = 50, 100, 500$, and 5000 to compare the relative performance of the tests in this favorable setting for different sample sizes. For each combination of I and Γ, we repeat the above process of drawing I pairs and testing the null that $\bar{\Delta} = 0$ at that value of Γ 10,000 times.

Figure 3 presents the results, showing, for each I, the power of the three procedures being compared as Γ increases. For all values of I, and at all points Γ, note the consistency of the power orderings: $\varphi_F(\cdot)$ is less powerful than $\varphi_S(\cdot)$, which is itself less powerful than $\varphi_N(\cdot)$. That the permutational t-based sensitivity analysis is less powerful than both the studentized sensitivity analysis and the Neyman-style large sample sensitivity analysis is reflective of the phenomenon described in §5.3: the procedure based on the permutational t-test does not exploit local information, while the new procedures do by utilizing the estimated variance $S^2_{\bar{D}_{0,\Gamma}}$. As I increases, we see that for all three procedures, the power profiles approach a step function at the common design sensitivity, $\tilde{\Gamma} = 6$, reflective of Proposition 5.

The gap between the power of $\varphi_S(\cdot)$ and that of $\varphi_N(\cdot)$, particularly in small samples, stems from $\varphi_N(\cdot)$ employing a normal distribution regardless of the true distribution of $\bar{D}_{0,\Gamma}$ and despite using $S^2_{\bar{D}_{0,\Gamma}}$ in place of $\text{var}(\bar{D}_{0,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$. This increase in power comes at the cost of being anti-conservative in finite samples when the null is true, even in the case presented here where Y_i are themselves actually normally distributed. For example, at $I = 30$ if we test the null that $\bar{\Delta} = 1/2$ instead of $\bar{\Delta} = 0$, at $\Gamma = 1$ and $\alpha = 0.05$, the permutational t-test and the studentized test both have Type I error rate of exactly $\alpha = 0.05$ (Lehmann and Romano, 2005, Theorem 5.8.1), while through the Type I error rate for the large sample procedure is $1 - T_{29}(\Phi^{-1}(0.95)) = 0.055$, where $T_{29}(p)$ is the p-quantile of a t distribution with 29 degrees of freedom.

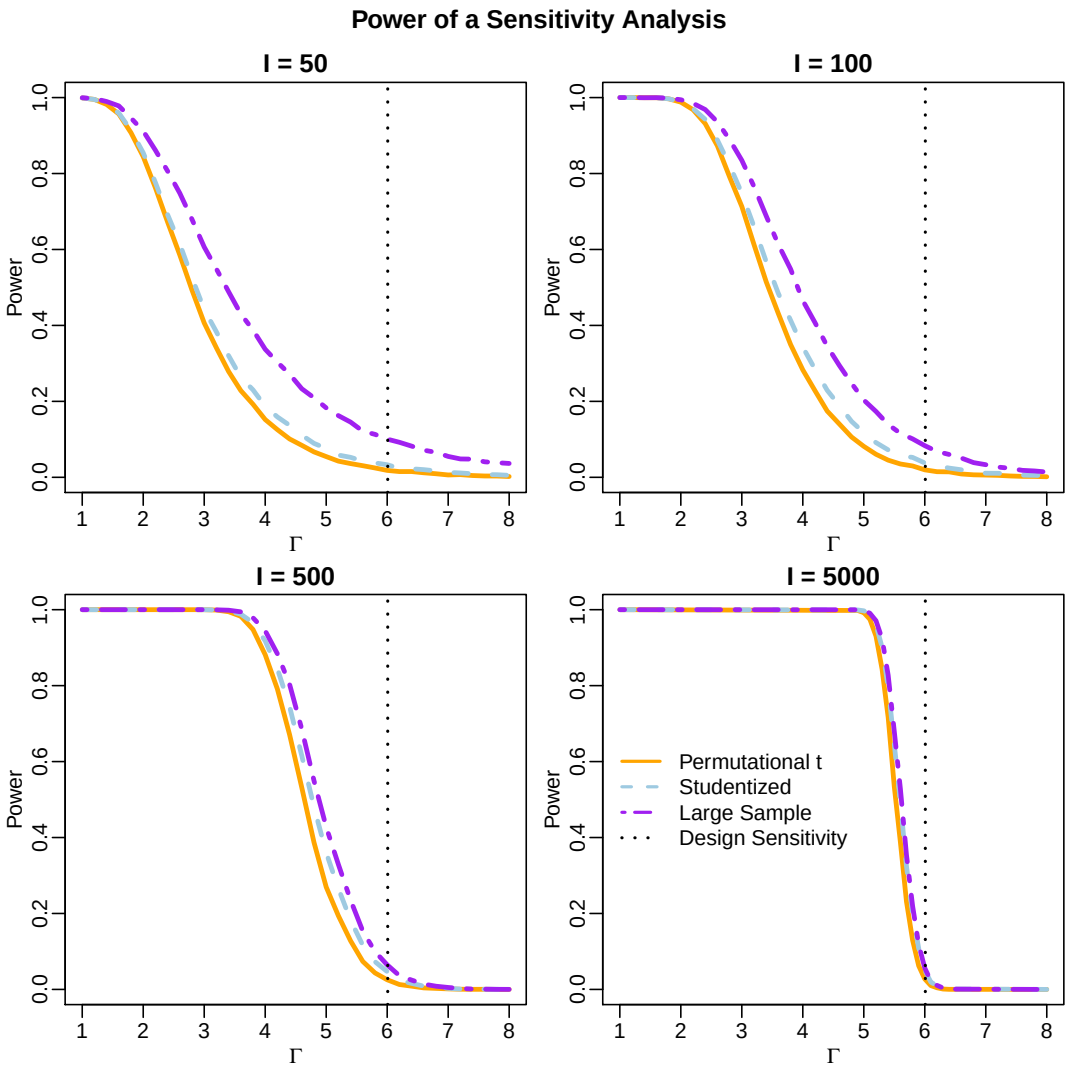

Figure 3: The power of a sensitivity analysis in a favorable situation. The four plots show the power of the test against the null $\bar{\Delta} = 0$ in the absence of unmeasured confounding as a function of Γ for $I = 50, 100, 500$, and 5000 pairs.

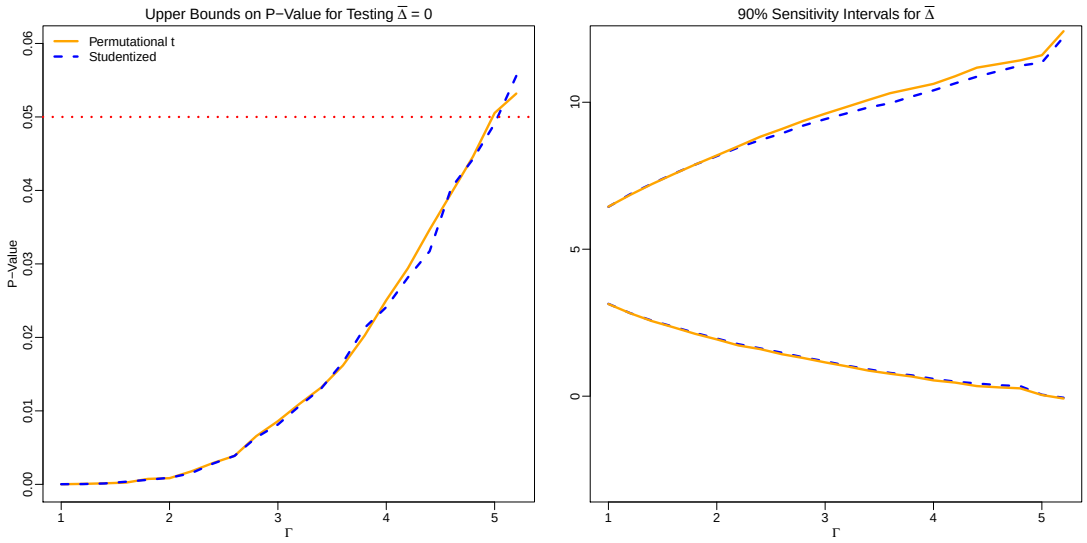

Figure 4: (Left) Upper bounds on the p-value as a function of Γ for the permutational t-based sensitivity analysis (solid) and the studentized sensitivity analysis (dashed). (Right) 90% sensitivity intervals for $\bar{\Delta}$ created by inverting both testing procedures, again as a function of Γ.

7 Studentized sensitivity analysis in the study of genetic damage from alcohol

We return now to the motivating example from §1.3 to compare the studentized sensitivity analysis to the sensitivity analysis based on a permutational t-test. From several perspectives, the results, and the corresponding message, remain virtually unchanged. At $\alpha = 0.05$, the studentized sensitivity analysis continues to reject the null until $\Gamma_S = 5.02$, fractionally below the changepoint reported by the permutational t-test of $\Gamma_F = 5.04$. Figure 4 shows a more detailed comparison of the two methods. The figure on the left-hand side shows a comparison between the worst-case p-values reported by the two methods for a range of values for Γ. There, we see that that two methods report worst-case p-values at each point Γ which are virtually identical to each other. The plot on the right-hand side shows 90% confidence intervals (also referred to in this context as sensitivity intervals) obtained by, at a particular value of Γ, inverting the two procedures. The confidence intervals returned by the studentized procedure are consistently narrower than those returned by the permutational t (though to a negligible degree). This again results from the discussion in §5.3, as the extremes of the confidence interval represent ranges of values for the parameter $\bar{\Delta}$ farther from the observed average of the paired differences. The results of this analysis highlight that, at least in this data example, the analysis using the permutational t-test did not materially inflate the perceived robustness of the study's findings to unmeasured confounding. As the calculations of §5.2 indicate, this is very likely to be a consistent conclusion of such comparisons across sensitivity analyses in paired observational studies with even modest sample sizes.

8 Concluding remarks

The subtleties of the model of constant treatment effects, and the differences in the implications of additivity in randomized experiments versus observational studies, may be lost on practitioners employing sensitivity analyses. As such, practitioners may well have in mind the null of no average treatment effect when performing a sensitivity analysis by means of the permutational t-test. Applied sensitivity analyses typically report the minimal value of Γ for which, at a given level α, the hypothesis test fails to reject the null. The sensitivity analysis based on the permutational t-test, $\varphi_F(\tau, \alpha, \Gamma)$, fails to control the Type I error rate when (2) holds at Γ for certain allocations of potential outcomes whose treatment effects average to τ; however as Proposition 4 demonstrates, simply conducting a sensitivity analysis at $\Gamma + \epsilon$ for any $\epsilon > 0$ eliminates the problem asymptotically. From a practical perspective, this suggests that the reported changepoint values of Γ for which studies can no longer reject the null hypothesis when utilizing the permutational t, and with it the perceived robustness of a study's findings to unmeasured confounding, will likely not be substantively larger than what is justified. The discrepancies in the changepoint Γs reported in §7 of 5.04 (permutational t) versus 5.02 (studentized) certainly align with this narrative.

As our exposition has evidenced, in the presence of heterogeneous effects the studentized sensitivity analysis is to be preferred over the conventional permutational t-based sensitivity analysis for several reasons. It maintains the desired level asymptotically, is able to capture departures from normality in finite samples, and has the potential to yield more powerful tests than than the permutational t-test when the true average treatment effect is far from the hypothesized value τ. A worthy caveat is in order. If the constant treatment effects model holds at τ, and (2) holds at Γ, then $\varphi_F(\tau, \alpha, \Gamma)$ is an exact test, in that it controls the Type I error at α for all sample sizes I. Under the same model, $\varphi_S(\tau, \alpha, \Gamma)$ only does so in the limit as $I \to \infty$. The lack of exactness under the constant effects model stems from the studentized statistic $\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}}$ not being arrangement-increasing, while $\bar{D}_{\tau,\Gamma}$ is (Rosenbaum, 2002b, Chapter 2). The distribution $\hat{G}_{\tau,\Gamma}(t)$ in (12) thus need not stochastically dominate $\bar{D}_{\tau,\Gamma}/S_{D_{\tau,\Gamma}}$ under a constant effects model for all I, while $\hat{F}_{\tau,\Gamma}(x)$ in (11) does stochastically dominate $\bar{D}_{\tau,\Gamma}$ for any I. In a randomized experiment ($\Gamma = 1$), this issue does not occur, and the studentized test also yields an exact test for the constant effects model. This led Loh et al. (2017) to recommend a procedure akin to $\varphi_S(\tau, \alpha, 1)$ in randomized experiments for testing both Neyman's weak null at τ and the null of a constant effect at τ, eliminating the paradoxical phenomenon observed in §5.3 while maintaining the benefits in power which gave rise to said phenomenon. To create a single sensitivity analysis which is asymptotically valid for Neyman's null while exact if the model of constant effects holds, one path forward is through the test $\varphi_{F\wedge S}(\tau, \alpha, \Gamma) = \varphi_S(\tau, \alpha, \Gamma) \wedge \varphi_F(\tau, \alpha, \Gamma)$, i.e. rejecting only if both procedures reject. This would, unfortunately, deprive us of the improvements in power from exploiting local information described in §5.3. Improving the procedure $\varphi_{F\wedge S}(\cdot)$ in observational studies would be a worthwhile avenue for future research.

Even under the assumption of additivity, the permutational t-test tends to have worse power in a sensitivity analysis than other choices of test statistics, such as the signed rank test, certain u-statistics, and other members of the class of m-statistics (Rosenbaum, 2007, 2011), leading it to not be favored for sensitivity analyses in practice. The extent to which studentized versions of

those tests provide valid sensitivity analyses for the sample average treatment effect remains an open question. The machinery underpinning the results presented herein should prove beneficial in addressing that question.

This work serves to further dispel the notion that the sensitivity analyses based on the model of Rosenbaum (2002b, §4.2) is only useful for testing sharp null hypotheses. Sensitivity analyses for composite nulls on binary outcomes have been developed in Rosenbaum (2002a) and Fogarty et al. (2017), while in the case of continuous outcomes Rosenbaum (2003) presents an exact sensitivity analysis for the Walsh averages. By providing a sensitivity analysis for the sample average treatment effect while accommodating effect heterogeneity, we hope to further enable and encourage researchers to conduct sensitivity analyses when inferring treatment effects in observational studies.

A Additional proofs

A.1 Proposition 1

Proof of a detail in Lemma 2

We now show that $\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1-\theta_\Gamma)|\Delta_i - \tau - |\eta_i|| \geq |\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau)$. We do this in three cases depending upon the values for $sign(\Delta_i - \tau + |\eta_i|)$ and $sign(\Delta_i - \tau - |\eta_i|)$

Case 1 ($\Delta_i - \tau + |\eta_i| \geq 0$, $\Delta_i - \tau - |\eta_i| \geq 0$). Here $\Delta_i - \tau \geq |\eta_i|$. Recalling that $0 \leq 2\theta_\Gamma - 1 \leq 1$,

$$\begin{aligned}\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1-\theta_\Gamma)|\Delta_i - \tau - |\eta_i|| &= (2\theta_\Gamma - 1)|\eta_i| + \Delta_i - \tau \\ &\geq |\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau).\end{aligned}$$

Case 2 ($\Delta_i - \tau + |\eta_i| \geq 0$, $\Delta_i - \tau - |\eta_i| < 0$). Here we have that the result holds with equality, as $\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1-\theta_\Gamma)|\Delta_i - \tau - |\eta_i|| = |\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau)$.

Case 3 ($\Delta_i - \tau + |\eta_i| < 0$, $\Delta_i - \tau - |\eta_i| < 0$). In this case $-(\Delta_i - \tau) \geq |\eta_i|$. Noting $-(\Delta_i - \tau) = -2\theta_\Gamma(\Delta_i - \tau) + (2\theta_\Gamma - 1)(\Delta_i - \tau)$ and $2\theta_\Gamma, (2\theta_\Gamma - 1) \geq 0$,

$$\begin{aligned}\theta_\Gamma|\Delta_i - \tau + |\eta_i|| + (1-\theta_\Gamma)|\Delta_i - \tau - |\eta_i|| &= (1 - 2\theta_\Gamma)|\eta_i| - (\Delta_i - \tau) \\ &\geq |\eta_i| + (2\theta_\Gamma - 1)(\Delta_i - \tau).\end{aligned}$$

The inequality thus always holds.

Proof of Lemma 3

For any constant τ, and any constant $\Gamma \geq 1$

$$\begin{aligned}&E(S^2_{D_{\tau,\Gamma}} \mid \mathcal{F}_I, \mathcal{Z}_I) - \text{var}(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \\ &= \frac{1}{I(I-1)} \sum_{i=1}^{I} \left(E(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) - E(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \right)^2 \geq 0.\end{aligned}$$

Proof. For ease of exposition, we suppress conditioning on $\mathcal{F}_I$ and $\mathcal{Z}_I$ at certain points in the derivation below.

$$\begin{aligned}
E(S^2_{D_{\tau,\Gamma}} \mid \mathcal{F}_I, \mathcal{Z}_I) &= \frac{1}{I(I-1)} \left(\sum_{i=1}^{I} E(D^2_{i,\tau,\Gamma}) - I^{-1} \sum_{k,\ell=1}^{I} E(D_{k,\tau,\Gamma} D_{\ell,\tau,\Gamma}) \right) \\
&= I^{-2} \left(\sum_{i=1}^{I} E(D^2_{i,\tau,\Gamma}) - \frac{1}{I-1} \sum_{k \neq \ell} E(D_{k,\tau,\Gamma}) E(D_{\ell,\tau,\Gamma}) \right) \\
&= I^{-2} \left(\sum_{i=1}^{I} \left(\mathrm{var}(D_{i,\tau,\Gamma}) + E(D_{i,\tau,\Gamma})^2 \right) - \frac{1}{I-1} \sum_{k \neq \ell} E(D_{k,\tau,\Gamma}) E(D_{\ell,\tau,\Gamma}) \right) \\
&= \mathrm{var}(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) + \frac{1}{I(I-1)} \sum_{i=1}^{I} \left(E(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) - E(\bar{D}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \right)^2,
\end{aligned}$$

proving the result. □

Remark 1. The result of Lemma 3 applies beyond the collection of random variables $\{D_{i,\tau,\Gamma}\}$. Take any collection of I independent random variables $\{X_i\}$ with $E(X_i) = \mu_i$ and $\mathrm{var}(X_i) = \sigma_i^2$, and consider their random average $\bar{X}$. Then, $E(S_X^2) - \mathrm{var}(\bar{X}) = ((I-1)I)^{-1} \sum_{i=1}^{I} (\mu_i - \bar{\mu})^2 \geq 0$.

Lemma 5. *For each i, for any constant τ*

$$16\theta_\Gamma(1-\theta_\Gamma)^3 \eta_i^2 \leq var(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 16\theta_\Gamma^3(1-\theta_\Gamma)\eta_i^2 \tag{16}$$

$$E(U^4_{i,\tau,\Gamma}) \leq 128\theta_\Gamma^4 \left((\Delta_i - \tau)^4 + \eta_i^4 \right) \tag{17}$$

Further, if treatment assignment satisfies (2) at Γ,

$$16\theta_\Gamma(1-\theta_\Gamma)^3 \eta_i^2 \leq var(D_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 4\theta_\Gamma^2 \eta_i^2 \tag{18}$$

$$E(D^4_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 128\theta_\Gamma^4 \left((\Delta_i - \tau)^4 + \eta_i^4 \right) \tag{19}$$

Proof. Let $U_i = U_{i,\tau,\Gamma}$, and let the analogous hold for D_i. To prove (16), observe $\mathrm{var}(U_i \mid \mathcal{F}_I, \mathcal{Z}_I) = \theta_\Gamma(1-\theta_\Gamma)(2\eta_i - (2\theta_\Gamma - 1)(|\Delta_i - \tau + \eta_i| - |\Delta_i - \tau - \eta_i|))^2$, which is at least $16\theta_\Gamma(1-\theta_\Gamma)^3\eta_i^2$ and at most $16\theta_\Gamma^3(1-\theta_\Gamma)\eta_i^2$. The proof of (18) simply replaces $\theta_\Gamma(1-\theta_\Gamma)$ with $1/4$ in the upper bound.

Proving (17) simply requires multiple applications of the fact that $(a+b)^2 \leq 2a^2 + 2b^2$ for scalars a and b. Without loss of generality assume that $\eta_i \geq -\eta_i$

$$\begin{aligned}
E(U_i^4 \mid \mathcal{F}_I, \mathcal{Z}_I) &= \theta_\Gamma(\Delta_i - \tau + \eta_i - (2\theta_\Gamma - 1)|\Delta_i - \tau + \eta_i|)^4 \\
&\quad + (1-\theta_\Gamma)(\Delta_i - \tau - \eta_i - (2\theta_\Gamma - 1)|\Delta_i - \tau - \eta_i|)^4 \\
&\leq \theta_\Gamma(2\theta_\Gamma(\Delta_i - \tau + \eta_i))^4 + (1-\theta_\Gamma)(2\theta_\Gamma(\Delta_i - \tau - \eta_i))^4 \\
&\leq 128\theta_\Gamma^4 \left((\Delta_i - \tau)^4 + \eta_i^4 \right)
\end{aligned}$$

The proof of (19) is analogous. □

Lemma 6. *Both $I^{1/2}\bar{D}_{\tau,\Gamma}$ and $I^{1/2}\bar{U}_{\tau,\Gamma}$ are asymptotically normal. Further, let*

$$k^*_{\tau,\Gamma}(\alpha) = \Phi^{-1}(1-\alpha)\left\{I^{-2}\sum_{i=1}^{I} var(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)\right\}^{1/2}. \tag{20}$$

Then, if (2) holds at Γ and the the true sample average treatment effect equals τ,

$$\lim_{I\to\infty} pr(\bar{D}_{\tau,\Gamma} \geq k^*_{\tau,\Gamma}(\alpha) \mid \mathcal{F}_I, \mathcal{Z}_I) \leq \alpha. \tag{21}$$

Proof. We prove asymptotic normality of $I^{1/2}\bar{U}_{\tau,\Gamma}$, and with it (21) by reference to Lemma 1; the proof for $I^{1/2}\bar{D}_{\tau,\Gamma}$ is analogous. Let $U_i = U_{i,\tau,\Gamma}$, and let the analogous hold for D_i. The U_i are conditionally independent given $\mathcal{F}_I$ and $\mathcal{Z}_I$. Further, by Lemma 2 and sharpness of U_i as a stochastic upper bound we have that $E\left(I^{-1}\sum_{i=1}^{I} U_i \mid \mathcal{F}_I, \mathcal{Z}_I\right) \leq 0$. To illustrate asymptotic normality of $I^{1/2}\bar{U}$, it suffices to show that Lyapunov's condition holds for $\delta = 2$, i.e. that (suppressing conditioning on $\mathcal{F}_I$ and $\mathcal{Z}_I$ henceforth)

$$\sum_{i=1}^{I} E|U_i - E(U_i)|^4 / \left(\sum_{i=1}^{I} \text{var}(U_i)\right)^2 \to 0$$

By (16), $I^{-1}\sum_{i=1}^{I} \text{var}(U_i) \geq 16\theta_\Gamma(1-\theta_\Gamma)^3 I^{-1}\sum_{i=1}^{I}\eta_i^2$, which is greater than $16\theta_\Gamma(1-\theta_\Gamma)^3 C$ for some $C > 0$ as $I \to \infty$ by (5). Applying Jensen's inequality and utilizing (17) and (6), we have that $I^{-2}\sum_{i=1}^{I} E|U_i - E(U_i)|^4 \to 0$. Hence,

$$\begin{aligned}\sum_{i=1}^{I} E|U_i - E(U_i)|^4 / \left(\sum_{i=1}^{I} \text{var}(U_i)\right)^2 &= I^{-2}\sum_{i=1}^{I} E|U_i - E(U_i)|^4 / \left(I^{-1}\sum_{i=1}^{I} \text{var}(U_i)\right)^2 \\ &\leq I^{-2}\sum_{i=1}^{I} E|U_i - E(U_i)|^4 / (16\theta_\Gamma(1-\theta_\Gamma)^3 C)^2 \to 0.\end{aligned}$$

This, along with (8), proves the result. □

Lemma 7. *Suppose that treatment assignment satisfies (2) at Γ and that the sample average treatment effect equals τ. If conditions (5) and (6) hold, then for all $\epsilon > 0$, as $I \to \infty$*

$$pr\left(-\epsilon + \bar{D}_{\tau,\Gamma} \geq 0 \mid \mathcal{F}_I, \mathcal{Z}_I\right) \to 0. \tag{22}$$

$$pr\left\{\epsilon + IS^2_{D_{\tau,\Gamma}} \leq I^{-1}\sum_{i=1}^{I} var(U_{i,\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \mid \mathcal{F}_I, \mathcal{Z}_I\right\} \to 0 \tag{23}$$

Proof. Let $D_i = D_{i,\tau,\Gamma}$, $U_i = U_{i,\tau,\Gamma}$ and suppress conditioning on $\mathcal{F}_I$ and $\mathcal{Z}_I$. We begin by demonstrating (22). By Lemma 2, $\text{pr}\left(-\epsilon + \bar{D} \geq 0\right) \leq \text{pr}\left(-\epsilon + \bar{D} - E(\bar{D}) \geq 0\right)$. The variance

of $\text{var}(D_i)$ is, by (18), less than $4\theta_\Gamma^2\eta_i^2$. Therefore, using (6),

$$\text{var}\left(\bar{D}\right) \leq 4\theta_\Gamma^2 I^{-2}\sum_{i=1}^{I}\eta_i^2 \to 0$$

as $I \to \infty$. Chebyshev's inequality then yields (22).

We now prove (23). Recall that $IS_D^2 = (I-1)^{-1}\sum_{i=1}^{I} D_i^2 - I/(I-1)(\bar{D})^2$. By Lemma 4,

$$\begin{aligned}
&\text{pr}\left\{\epsilon + IS_D^2 \leq I^{-1}\sum_{i=1}^{I}\text{var}(U_i)\right\} \\
&\leq \text{pr}\left\{\epsilon + IS_D^2 \leq (I-1)^{-1}\sum_{i=1}^{I}\text{var}(D_i) + (I-1)^{-1}\sum_{i=1}^{I}(E(D_i)-E(\bar{D}))^2\right\} \\
&= \text{pr}\left\{\epsilon + (I-1)^{-1}\left(\sum_{i=1}^{I}D_i^2 - \sum_{i=1}^{I}E(D_i^2)\right) - I(I-1)^{-1}(\bar{D}^2 - E(\bar{D})^2) \leq 0\right\}
\end{aligned}$$

The proof of (22) along with (7) yields that $I/(I-1)(\bar{D}^2 - E(\bar{D})^2)$ converges in probability to 0. We now show that $(I-1)^{-1}\left(\sum_{i=1}^{I}D_i^2 - \sum_{i=1}^{I}E(D_i^2)\right)$ also converges in probability to 0. Using (19),

$$\begin{aligned}
\text{var}\left\{(I-1)^{-1}\sum_{i=1}^{I}D_i^2\right\} &\leq (I-1)^{-2}\sum_{i=1}^{I}E(D_i^4 \mid \mathcal{F}_I, \mathcal{Z}_I) \\
&\leq 128\theta_\Gamma^4(I-1)^{-2}\sum_{i=1}^{I}((\Delta_i - \tau)^4 + \eta_i^4),
\end{aligned}$$

which converges to 0 as $I \to \infty$ through (6). Applying Chebyshev's inequality yields the desired convergence in probability, which in turn yields (23). □

Proof of Proposition 1

Define $\kappa_{\tau,\Gamma}(\alpha) = S_{D_{\tau,\Gamma}}\Phi^{-1}(1-\alpha)$ with $0 < \alpha \leq 0.5$. By (23), taking $\epsilon \downarrow 0$,

$$\lim_{I\to\infty}\text{pr}(k_{\tau,\Gamma}(\alpha) \geq k^*_{\tau,\Gamma}(\alpha) \mid \mathcal{F}_I, \mathcal{Z}_I) = 1.$$

This, in combination with (21), yields the conclusion of the proposition.

A.2 Proposition 2

Lemma 8. *Take a vector $\tilde{V}_\Gamma$ distributed as in §2.3 with $\tilde{V}_{i,\Gamma} = \pm 1$ and $pr(\tilde{V}_{i,\Gamma} = 1 \mid \mathcal{F}_I, \mathcal{Z}_I) = \theta_\Gamma$. Let $\tilde{V}'_\Gamma$ be an iid copy of $\tilde{V}_\Gamma$. Then, under (6) and (13), $I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y)$ and $I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}'_\Gamma, Y)$*

are iid and converge jointly to a bivariate normal, each with mean zero and variance $\nu_\Gamma^2 = 4\theta_\Gamma(1-\theta_\Gamma)\nu^2$.

Proof. Recall that $A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y) = \tilde{V}_{i,\Gamma}|Y_i - \tau| - (2\theta_\Gamma - 1)|Y_i - \tau|$, and that $E(A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y) \mid \mathcal{F}_I, \mathcal{Z}_I) = 0$. Let $A_i = A_{i,\tau,\Gamma}(\tilde{V}_\Gamma, Y)$, and let $A_i' = A_{i,\tau,\Gamma}(\tilde{V}_\Gamma', Y)$. Since uncorrelatedness implies independence for the normal, to show independence of the limiting distributions for $\bar{A}$ and $\bar{A}'$ it suffices to show that $\mathrm{cov}(\bar{A}, \bar{A}' \mid \mathcal{F}_I, \mathcal{Z}_I) = 0$.

$$\begin{aligned}
\mathrm{cov}(\bar{A}, \bar{A}' \mid \mathcal{F}_I, \mathcal{Z}_I) &= E\left\{\mathrm{cov}(\bar{A}, \bar{A}' \mid Y, \mathcal{F}_I, \mathcal{Z}_I) \mid \mathcal{F}_I, \mathcal{Z}_I\right\} \\
&\quad + \mathrm{cov}\left\{E(\bar{A} \mid Y, \mathcal{F}_I, \mathcal{Z}_I), E(\bar{A}' \mid Y, \mathcal{F}_I, \mathcal{Z}_I) \mid \mathcal{F}_I, \mathcal{Z}_I\right\} \\
&= I^{-2} E\left\{\sum_{i=1}^{I} (Y_i - \tau)^2 \mathrm{cov}(\tilde{V}_{i,\Gamma}, \tilde{V}_{i,\Gamma}' \mid \mathcal{F}_I, \mathcal{Z}_I) \mid \mathcal{F}_I, \mathcal{Z}_I\right\} + 0 \\
&= 0
\end{aligned}$$

By the Cramér-Wold device, to show bivariate asymptotic normality it suffices to show that $I^{1/2}(w_1\bar{A} + w_2\bar{A}')$ converge to a normal with mean zero and variance $(w_1^2 + w_2^2)\nu_\Gamma^2$ for any vector of constants (w_1, w_2). Fixing (w_1, w_2), we now show this to be the case through Lyapunov's condition. We have $E(w_1A_i + w_2A_i \mid \mathcal{F}_I, \mathcal{Z}_I) = 0$, that $E(A_i^4 \mid \mathcal{F}_I, \mathcal{Z}_I) = \pi_i(\Delta_i - \tau + \eta_i)^4 + (1 - \pi_i)(\Delta_i - \tau - \eta_i)^4 \leq 8(\Delta_i - \tau)^4 + 8\eta_i^4$, and that $E((w_1A_i + w_2A_i')^4 \mid \mathcal{F}_I, \mathcal{Z}_I) \leq 8(w_1^4 + w_2^4)E(A_i^4 \mid \mathcal{F}_I, \mathcal{Z}_I)$. Combining this with (6), we have that $I^{-2}\sum_{i=1}^{I} E((w_1A_i + w_2A_i')^4 \mid \mathcal{F}_I, \mathcal{Z}_I) \to 0$. By (13), we have that $I^{-1}\sum_{i=1}^{I} \mathrm{var}(w_1A_i + w_2A_i' \mid \mathcal{F}_I, \mathcal{Z}_I) \to (w_1^2 + w_2^2)v_\Gamma^2 > 0$. Hence,

$$\begin{aligned}
&\sum_{i=1}^{I} E((w_1A_i + w_2A_i')^4 \mid \mathcal{F}_I, \mathcal{Z}_I) \Big/ \left(\sum_{i=1}^{I} \mathrm{var}(w_1A_i + w_2A_i' \mid \mathcal{F}_I, \mathcal{Z}_I)\right)^2 \\
&= I^{-2}\sum_{i=1}^{I} E((w_1A_i + w_2A_i')^4 \mid \mathcal{F}_I, \mathcal{Z}_I) \Big/ \left(I^{-1}(w_1^2 + w_2^2)\sum_{i=1}^{I} \mathrm{var}(A_i \mid \mathcal{F}_I, \mathcal{Z}_I)\right)^2 \to 0.
\end{aligned}$$

Lyapunov's condition is satisfied at $\delta = 2$, thus proving the result. □

Lemma 9. *Under the assumptions of Proposition 2, for any point* a

$$\hat{F}_{\tau,\Gamma}(a/I^{1/2}) \xrightarrow{p} \Phi\left(a/\nu_\Gamma\right),$$

Proof. Observe that

$$\begin{aligned}
E(\hat{F}_{\tau,\Gamma}(a/I^{1/2}) \mid \mathcal{F}_I, \mathcal{Z}_I) &= E(E(\chi\{I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y) \leq a\} \mid Y, \mathcal{F}_I, \mathcal{Z}_I) \mid \mathcal{F}_I, \mathcal{Z}_I) \\
&= E(\chi\{I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y) \leq a\} \mid \mathcal{F}_I, \mathcal{Z}_I) \\
&= \mathrm{pr}(I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y) \leq a)
\end{aligned}$$

By Lemma 8, $I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y)$ converges in distribution to a normal with mean 0 and variance ν_Γ^2. Hence, $E(\hat{F}_{\tau,\Gamma}(a/I^{1/2}) \mid \mathcal{F}_I, \mathcal{Z}_I) \to \Phi(a/\nu_\Gamma)$. Through Chebyshev's inequality, to illustrate the

desired convergence in probability it suffices to show that $E(\hat{F}^2_{\tau,\Gamma}(a/I^{1/2}) \mid \mathcal{F}_I, \mathcal{Z}_I) \to \Phi^2(a/\nu_\Gamma)$, which is equivalent to $\text{var}(\hat{F}_{\tau,\Gamma}(a/I^{1/2}) \mid \mathcal{F}_I, \mathcal{Z}_I) \to 0$.

$$
\begin{aligned}
&E(\hat{F}^2_{\tau,\Gamma}(a/I^{1/2}) \mid \mathcal{F}_I, cZ_I) \\
&= E\left[\sum_{z,z' \in \Omega_I} \chi\{I^{1/2}\bar{A}_{\tau,\Gamma}(z_1 - z_2, Y) \le a\}\chi\{I^{1/2}\bar{A}_{\tau,\Gamma}(z_1' - z_2', Y) \le a\} \prod_{i=1}^{I} \theta_\Gamma^{z_{i1}+z'_{i1}}(1-\theta_\Gamma)^{2-z_{i1}-z'_{i1}} \mid \mathcal{F}_I, \mathcal{Z}_I\right] \\
&= \text{pr}(I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}_\Gamma, Y) \le a, I^{1/2}\bar{A}_{\tau,\Gamma}(\tilde{V}'_\Gamma, Y) \le a) \to \Phi^2(a/\nu_\Gamma)
\end{aligned}
$$

as desired, where the last line utilizes Lemma 8. □

Lemma 10.

$$E(S^2_{A_{\tau,\Gamma}} \mid \mathcal{F}_I, \mathcal{Z}_I) = var(\bar{A}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I)$$

Proof. The lemma follows by Remark 1 along with (9). □

Lemma 11. *Under the assumptions of Proposition 2,*

$$IS^2_{A_{\tau,\Gamma}} \xrightarrow{p} \nu^2_\Gamma$$

Proof. Let $S^2_A = S^2_{A_{\tau,\Gamma}}$, and let the analogous hold for A_i. Decompose $IS^2_A = (I-1)^{-1}\sum_{i=1}^{I} A_i^2 - I/(I-1)\bar{A}$. $I/(I-1)E(\bar{A} \mid \mathcal{F}_I, \mathcal{Z}_I)$ is 0 by (9), while by Lemma 8 $\bar{A}$ has limiting variance $\nu^2_\Gamma/I \to 0$. Hence, $I/(I-1)\bar{A}$ converges in probability to 0 by Chebyshev's inequality. Meanwhile, $E((I-1)^{-1}\sum_{i=1}^{I} A_i^2 \mid \mathcal{F}_I, \mathcal{Z}_I) \to \nu^2_\Gamma$ by Lemma 10 and (13). To show that the variance of this term goes to zero, observe that $A_i^4 \le 8(1 + (2\theta_\Gamma - 1)^4)|Y_i - \tau|^4$. Similar arguments to those of Lemma 7, utilizing (6), then yield that the variance goes to zero, thus yielding the result through Chebyshev's inequality. □

Proof of Proposition 2

By Lemma 9, $\hat{F}_{\tau,\Gamma}(a/I^{1/2})$, the biased randomization distribution of $I^{1/2}\bar{A}_{\tau,\Gamma}$, converges in probability to $\Phi(a/\nu_\Gamma)$ for all points a. By Lemma 11 and the continuous mapping theorem $I^{1/2}S_{A_{\tau,\Gamma}}$ converges in probability to ν_Γ. Recall that $\hat{G}_{\tau,\Gamma}(t)$ is the biased randomization distribution of the studentized statistic $I^{1/2}\bar{A}_{\tau,\Gamma}/(I^{1/2}S_{A_{\tau,\Gamma}})$. Setting $a = tI^{1/2}S_{A_{\tau,\Gamma}}$ and using Slutsky's theorem for randomization distributions (Chung and Romano, 2013, Lemma 5.2), we have that $\hat{G}_{\tau,\Gamma}(t)$ then converges in probability to $\Phi(t\nu_\Gamma/\nu_\Gamma) = \Phi(t)$ for all points t as desired.

A.3 Proposition 4

Recall that $\theta_{\Gamma+\epsilon} > \theta_\Gamma$ for any $\epsilon > 0$. Consider $D_{i,\tau,\Gamma+\epsilon} = Y_i - \tau - (2\theta_{\Gamma+\epsilon} - 1)|Y_i - \tau|$. Since (2) holds at Γ, by arguments parallel to those in Lemma 1 $\bar{D}_{\tau,\Gamma+\epsilon}$ is stochastically bounded by the

random variable $\bar{W}_{\tau,\Gamma,\epsilon}$, where

$$W_{i,\tau,\Gamma,\epsilon} = \Delta_i - \tau + \tilde{V}_{i,\Gamma}|\eta_i| - (2\theta_{\Gamma+\epsilon} - 1)\{(1 + \tilde{V}_{i,\Gamma})|\Delta_i - \tau + |\eta_i|| + (1 - \tilde{V}_{i,\Gamma})|\Delta_i - \tau - |\eta_i||\}/2.$$

Hence, $E(\bar{W}_{\tau,\Gamma} \mid \mathcal{F}_I, \mathcal{Z}_I) \geq E(\bar{D}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I)$. Further define $U_{i,\tau,\Gamma+\epsilon}$ as before, namely

$$U_{i,\tau,\Gamma+\epsilon} = \Delta_i - \tau + \tilde{V}_{i,\Gamma+\epsilon}|\eta_i| - (2\theta_{\Gamma+\epsilon} - 1)\{(1 + \tilde{V}_{i,\Gamma+\epsilon})|\Delta_i - \tau + |\eta_i|| + (1 - \tilde{V}_{i,\Gamma+\epsilon})|\Delta_i - \tau - |\eta_i||\}/2.$$

We now show that in the limit, $E(\bar{U}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I) > E(\bar{W}_{\tau,\Gamma,\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I)$.

$$\begin{aligned}
&E(\bar{U}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I) - E(\bar{W}_{\tau,\Gamma,\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I) \\
&= I^{-1} \sum_{i=1}^{I} \left[(\theta_{\Gamma+\epsilon} - \theta_\Gamma) \left\{ |\eta_i| - (2\theta_{\Gamma+\epsilon} - 1)|\Delta_i - \tau + |\eta_i|| \right\} \right. \\
&\left. + (\theta_\Gamma - \theta_{\Gamma+\epsilon}) \left\{ -|\eta_i| + (2\theta_{\Gamma+\epsilon} - 1)|\Delta_i - \tau - |\eta_i|| \right\} \right] \\
&= (\theta_{\Gamma+\epsilon} - \theta_\Gamma) I^{-1} \sum_{i=1}^{I} \left\{ 2|\eta_i| + (2\theta_{\Gamma+\epsilon} - 1) \left(|\Delta_i - \tau - |\eta_i|| - |\Delta_i - \tau + |\eta_i|| \right) \right\} \\
&\geq 4(1 - \theta_{\Gamma+\epsilon})(\theta_{\Gamma+\epsilon} - \theta_\Gamma) I^{-1} \sum_{i=1}^{I} |\eta_i|,
\end{aligned}$$

where the last line follows arguments similar to those used to prove (16). In the limit, the last line is greater than or equal to $4(1-\theta_{\Gamma+\epsilon})(\theta_{\Gamma+\epsilon}-\theta_\Gamma)C > 0$ by (5). Hence, if (2) holds at Γ but a sensitivity analysis is conducted at $\Gamma+\epsilon$, $E(\bar{D}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I)$ is strictly less than $E(\bar{U}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I)$ asymptotically, which is itself less than or equal to zero if the average treatment effect equals τ by Lemma 2.

Let $\mu_D = E(\bar{D}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I) < 0$, and $\sigma_D^2/I = \text{var}(\bar{D}_{\tau,\Gamma+\epsilon} \mid \mathcal{F}_I, \mathcal{Z}_I)$. Asymptotically, the permutational t-test rejects the null of $\bar{\Delta} = \tau$ in favor of $\bar{\Delta} > \tau$ at $\Gamma + \epsilon$ for any α if $I^{1/2}\bar{D}_{\tau,\Gamma+\epsilon} \geq \nu_{\Gamma+\epsilon}\Phi^{-1}(1-\alpha)$ by Proposition 2.

$$\begin{aligned}
&\lim_{I\to\infty} E(\varphi_F(Y, \alpha, \tau, \Gamma + \epsilon) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \\
&= \lim_{I\to\infty} \text{pr}(I^{1/2}\bar{D}_{\tau,\Gamma+\epsilon} \geq \nu_{\Gamma+\epsilon}\Phi^{-1}(1-\alpha) \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \\
&= \lim_{I\to\infty} \text{pr}(I^{1/2}(\bar{D}_{\tau,\Gamma+\epsilon} - \mu_D)/\sigma_D \geq (\Phi^{-1}(1-\alpha)\nu_{\Gamma+\epsilon} - I^{1/2}\mu_D)/\sigma_D \mid \mathcal{F}_I, \mathcal{Z}_I, \bar{H}_\tau) \\
&= \lim_{I\to\infty} 1 - \Phi\left(\frac{-I^{1/2}\mu_D + \Phi^{-1}(1-\alpha)\nu_{\Gamma+\epsilon}}{\sigma_D} \right) = 0,
\end{aligned}$$

where the last line stems from $\mu_D < 0$ and asymptotic normality of $I^{1/2}\bar{D}_{\tau,\Gamma}$ by Lemma 6.

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