Exact conditional randomization tests
for causal effects under interference*

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Abstract

Many important causal questions involve interactions between units, also known as interference, such as interactions between individuals in households, students in schools, and firms in markets. Standard methods often break down in this setting. Permuting individual-level treatment assignments, for example, does not generally permute the treatment exposures of interest, such as spillovers, which depend on both the treatment assignment and the interference structure. One approach is to restrict the randomization test to a subset of units and assignments such that permuting the treatment assignment vector also permutes the treatment exposures, thus emulating the classical Fisher randomization test under no interference. Existing tests, however, can only leverage limited information in the structure of interference, which can lead to meaningful loss in power and introduce computational challenges. In this paper, we introduce the concept of a conditioning mechanism, which provides a framework for constructing valid and powerful randomization tests under general forms of interference. We describe our framework in the context of two-stage randomized designs and apply this approach to an analysis of a randomized evaluation of an intervention targeting student absenteeism in the School District of Philadelphia. We show meaningful improvements over existing methods, both in terms of computation and statistical power.

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1 Introduction

Classical approaches to causal inference typically assume that units do not interact with each other, known as the no interference assumption (Cox 1958). Many important causal questions, however, are inherently about interference between units (Sobel 2006; Hudgens and Halloran 2008). Standard approaches often break down in these settings. Randomization tests, for example, use randomization as the basis for inference (Fisher 1935) to test sharp null hypotheses of no effect. In the presence of interference, however, the same null hypotheses are usually not sharp. Recognizing this problem, Aronow (2012) and Athey et al. (2017) proposed conditional randomization tests restricted to a subset of units for whom the specified null hypothesis is sharp, often called focal units. While these approaches have some advantages over methods that rely more heavily on modeling (Bowers et al. 2013; Toulis and Kao 2013), they can be highly sensitive to the choice of focal units and provide limited guidance for complex settings.

In this paper, we develop a framework for constructing valid and powerful randomization tests under interference. Our key innovation is to introduce the concept of conditioning mechanism as the theoretical basis for more flexible conditional randomization tests. We show that recent proposals for randomization tests in the presence of interference are special cases of our broader framework. Moreover, existing methods map to specific conditioning mechanisms that, in general, fail to leverage the problem structure effectively. One issue with current methods, for example, is that they often include units whose outcomes provide no information for the null hypothesis of interest. This can lead to a meaningful loss in power compared to alternative conditioning mechanisms that utilize the observed data more effectively. Another issue is that current methods are hard to implement beyond simple settings. The main strength of our framework is therefore the ability to leverage structure from the problem to construct simple and powerful tests.
To highlight our framework in practice we focus on two-stage randomized designs, which are increasingly common designs for assessing causal effects related to interference (Hudgens and Halloran, 2008). We show how to apply our framework in this setting by suggesting concrete conditioning mechanisms for various hypotheses, analyzing data from a two-stage randomized evaluation of an intervention targeting student absenteeism in the School District of Philadelphia. In this setting, our method yields a permutation test on the exposures of interest, which is trivial to implement in practice. In contrast, alternative methods cannot be implemented as permutation tests, instead requiring complicated adjustments, as we show below. Finally, for the absenteeism study, our test is much more powerful than the adjusted alternative methods, with a roughly one-third increase in statistical power.

2 General Results for Randomization Testing

2.1 Classical randomization tests

Consider $N$ units indexed by $i = 1, \ldots, N$, and a binary treatment assignment vector $Z \in \{0, 1\}^N$, where the $i$-th component, $Z_i$, is the treatment assignment of unit $i$. The assignment vector is sampled with probability $\text{pr}(Z)$. Denote by $Y_i(Z)$ the scalar potential outcome of unit $i$ under assignment vector $Z$. Under the stable unit treatment value assumption (Rubin, 1980), the potential outcome of unit $i$ depends only on its own assignment. Each unit therefore has two potential outcomes, namely $Y_i(1)$ and $Y_i(0)$, which correspond to outcomes when unit $i$ receives treatment or control, respectively. Classically, the initial goal is to test the sharp null hypothesis of zero treatment effect for all units,

$$H_0 : Y_i(1) = Y_i(0), i = 1, \ldots, N.$$ (1)

We can assess this null hypothesis via a Fisher randomization test (Fisher, 1935). Let
$T(Z \mid Y)$ denote the test statistic; for example, $T(Z \mid Y) = \text{Ave}(Y_i \mid Z_i = 1) - \text{Ave}(Y_i \mid Z_i = 0)$ is the usual difference in means between treated and control units, where Ave denotes finite-sample average. Let $T^{\text{obs}} = T(Z^{\text{obs}} \mid Y^{\text{obs}})$ denote the observed value of the test statistic, where $Z^{\text{obs}} \sim \text{pr}(Z^{\text{obs}})$ is the observed assignment vector in the experiment, and $Y^{\text{obs}} = Y(Z^{\text{obs}})$ is the corresponding observed outcome vector. Finally, calculate the p-value

$$p\text{val}(Z^{\text{obs}}) = E_Z[\mathbb{I}\{T(Z \mid Y^{\text{obs}}) > T^{\text{obs}}\}],$$

where $\mathbb{I}(\cdot)$ is the indicator function, and $E_Z$ is the expectation with respect to the distribution $\text{pr}(Z)$. This test is valid at any level $\alpha$; that is, $\text{pr}\{p\text{val}(Z^{\text{obs}}) \leq \alpha\} \leq \alpha$, for all $\alpha \in [0, 1]$.

The key property that ensures validity of the p-value defined above is that, under $H_0$, the value of the test statistic can be imputed for every possible counterfactual assignment vector $Z'$, using only outcomes $Y^{\text{obs}}$ observed under $Z^{\text{obs}}$. This property allows us to construct the correct sampling distribution of the test statistic, and is formally stated in the following definition, which will be useful when we extend the classical randomization test to settings with interference.

**Definition 1 (Imputable test statistic).** A test statistic $T(Z \mid Y)$ is imputable with respect to a null hypothesis $H_0$ if for all $Z, Z'$ for which $\text{pr}(Z) > 0$ and $\text{pr}(Z') > 0$, it holds that

$$T\{Z' \mid Y(Z')\} = T\{Z' \mid Y(Z)\}. \quad (2)$$

The key property of an imputable test statistic is that we can simulate its sampling distribution under the null hypothesis $H_0$, even though we only observe one vector of outcomes, namely $Y^{\text{obs}}$. In the classical setting, Equation (2) follows trivially from the stable unit treatment value assumption and the sharp null hypothesis in Equation (1), which together imply that $Y(Z') = Y(Z)$, for any possible $Z, Z'$. Thus, in the classical setting all potential
outcomes are imputable, and, by extension, any test statistic is imputable.

2.2 Randomization tests via conditioning mechanisms

We now demonstrate that we can obtain valid tests without requiring the stable unit treatment value assumption or a sharp null hypothesis. To do so, we introduce the concept of a conditioning event, $C$, which is a random variable that is realized in the experiment; we leave this concept abstract for now and give concrete examples below. The key idea is to choose an event space and some conditional distribution $m(C \mid Z)$ on that space, such that, conditional on $C$, a test statistic $T(Z \mid Y, C)$ is imputable with respect to the null hypothesis. We refer to $m(C \mid Z)$ as the conditioning mechanism; $m(C \mid Z)$ and the design $pr(Z)$ together induce a joint distribution, $pr(Z, C; m) = m(C \mid Z)pr(Z)$. With these concepts, we can now state our first main result.

**Theorem 2.1.** Let $H_0$ be a null hypothesis and $T(Z \mid Y, C)$ a test statistic, such that $T$ is imputable with respect to $H_0$ under some conditioning mechanism $m(C \mid Z)$; that is, under $H_0$, it holds that

$$T\{Z' \mid Y(Z'), C\} = T\{Z' \mid Y(Z), C\},$$

for all $Z, Z', C$, for which $pr(Z, C; m) > 0$ and $pr(Z', C; m) > 0$. Consider the procedure where we first draw $C \sim m(C \mid Z^{\text{obs}})$, and then compute the conditional p-value,

$$pval(Z^{\text{obs}}; C) = E_Z[\mathbb{I}\{T(Z \mid Y^{\text{obs}}, C) > T^{\text{obs}}\} \mid C],$$

where $T^{\text{obs}} = T(Z^{\text{obs}} \mid Y^{\text{obs}}, C)$, and the expectation is with respect to $pr(Z \mid C) = pr(Z, C; m)/pr(C)$. This procedure is valid at any level, that is, $pr\{pval(Z^{\text{obs}}; C) \leq \alpha \mid C\} \leq \alpha$, for any $\alpha \in [0, 1]$.

Equation (3) is the critical property that the test statistic is imputable, and directly
generalizes the condition in Equation (2). As before, the key implication of Equation (3) is that we can simulate from the null distribution of $T\{Z \mid Y(Z), C\}$, given any possible conditioning event $C$. Theorem 2.1 allows us to extend conditional randomization testing to much more complicated settings, including testing under interference.

Before turning to these settings, we briefly demonstrate that classical examples of randomization testing are special cases of Theorem 2.1.

**Example 1** (Classical Fisher randomization test). Let the conditioning event space be any probability space such that $T(Z \mid Y, C) \equiv T(Z \mid Y)$ and $C \perp Z$. Then, the procedure in Theorem 2.1 reduces to the classical Fisher randomization test described in Section 2.1.

**Example 2** (Correcting for covariate imbalance). Hennessy et al. (2016) propose a conditional test that adjusts for covariate imbalance, which they quantify via a function $B(Z, X)$, where $X$ denotes a covariate vector. For instance, $B$ may be the vector of covariate means in each treatment arm, $B(Z, X) = \{\text{Ave}(X_i \mid Z_i = 1), \text{Ave}(X_i \mid Z_i = 0)\}$. Let $\text{pr}(Z) = \text{Unif}\{(0,1)^N\}$ be a Bernoulli randomization design, and consider the conditioning mechanism defined as $\text{pr}(Z, C) = \mathbb{I}\{B(Z, X) = C\}\text{pr}(Z)$. Let $T(Z \mid Y, C) \equiv T(Z \mid Y)$ be independent of $C$, and let $H_0$ be as in Equation (1). Then, the procedure of Theorem 2.1 corresponds exactly to the procedure of Hennessy et al. (2016).

### 3 Randomization Tests for General Exposure Contrasts

#### 3.1 General exposure contrasts

We now turn to constructing valid randomization tests in the presence of interference. Following Manski (2013) and Aronow and Samii (2013), we consider an exposure mapping $h_i(Z) : \{0,1\}^N \rightarrow \mathcal{H}$, where $\mathcal{H}$ is an arbitrary set of possible treatment exposures equipped with an equality relationship. Given an exposure mapping, a natural assumption that gen-
eralizes the classical stable unit treatment value assumption is

\[ Y_i(Z) = Y_i(Z'), i = 1, \ldots, N, \text{ for all } Z, Z', \text{ for which } h_i(Z) = h_i(Z'). \]  

(5)

This assumption states that potential outcomes are only functions of the exposure, rather than the entire assignment vector. In the most restrictive case of no interference, the exposure mapping is \( h_i(Z) = Z_i \); in the most general case without any restrictions on interference, the exposure mapping is \( h_i(Z) = Z \). An example of an intermediate case is if \( h_i(Z) = \sum_{j \in N_i} Z_j \), where \( N_i \) is the set of unit \( i \)'s neighbors in some network between units, and the exposure mapping of \( i \) is therefore the number of \( i \)'s treated neighbors (Toulis and Kao, 2013). In these examples, we implicitly defined \( \mathcal{H} = \{0, 1\} \), \( \mathcal{H} = \{0, 1\}^N \), and \( \mathcal{H} = \mathbb{N} \), respectively.

We can now formulate hypothesis tests on contrasts between treatment exposures. Let \( \{a, b\} \subseteq \mathcal{H} \) be two exposures of interest. The null hypothesis on the contrast between exposures \( a \) and \( b \) is

\[ H_0 : Y_i(Z) = Y_i(Z'), i = 1, \ldots, N, \text{ for all } Z, Z' \text{ for which } h_i(Z), h_i(Z') \in \{a, b\}. \]  

(6)

The classical sharp null in Equation (1) is a special case of Equation (6), with \( \mathcal{H} = \{a, b\} = \{0, 1\}. \) Under the no interference setting of Equation (1), we can easily permute the vector of unit exposures \( \{a, b\} \) by permuting the treatment assignment vector because the null hypothesis contains all possible exposures. In most interference settings, however, the null hypothesis in Equation (6) is not sharp because it only considers a subset of possible exposures. As a result, observing \( Y(Z_{\text{obs}}) \) gives only limited information about counterfactual outcomes \( Y(Z'), Z' \neq Z_{\text{obs}} \). And since \( h_i \) may have arbitrary form, we cannot permute unit exposures by naively permuting the treatment assignment vector.
3.2 Constructing valid tests for general exposure contrasts

Testing contrast hypothesis in Equation (6) is challenging because only a subset of units are exposed to exposures $a$ or $b$, and only for a subset of assignment vectors. We therefore construct conditioning events in terms of both units and treatment assignment vectors. Specifically, let $\mathcal{C} = \{(U, Z) : U \subseteq \mathbb{U}, Z \subseteq \mathbb{Z}\}$ be the space of conditioning events, where $\mathbb{U}$ denotes the power set of units, and $\mathbb{Z}$ denotes the power set of assignment vectors. For some conditioning event $\mathcal{C} = (U, Z) \in \mathcal{C}$, the conditioning mechanism can be decomposed, without loss of generality, as

$$m(\mathcal{C} | Z) = f(\mathcal{U} | Z)g(\mathcal{Z} | \mathcal{U}, Z), \quad (7)$$

where $f$ and $g$ are distributions over $\mathbb{U}$ and $\mathbb{Z}$, respectively. Given conditioning event $\mathcal{C} = (U, Z)$, we consider test statistics, $T(Z | Y, \mathcal{C})$, that depend only on outcomes of units in $U$; following terminology in Athey et al. (2017), we call $U$ the set of focal units. For example, we can set $T(Z | Y, \mathcal{C})$ to be the difference in means between focal units exposed to $a$ and units exposed to $b$:

$$T(Z | Y, \mathcal{C}) = \text{Ave}\{Y_i | i \in U, h_i(Z) = a\} - \text{Ave}\{Y_i | i \in U, h_i(Z) = b\}. \quad (8)$$

**Theorem 3.1.** Let $H_0$ be a null hypothesis as in Equation (6), $m(\mathcal{C} | Z)$ be a conditioning mechanism as in Equation (7), $\mathcal{C} = (U, Z)$, and $T$ be a test statistic defined only on focal units, as in Equation (8). Then, $T$ is imputable under $H_0$ if $m(\mathcal{C} | Z) > 0$ implies that $Z \in \mathbb{Z}$, and for every $i \in U, Z' \in \mathbb{Z}$, that

$$h_i(Z') \in \{a, b\}, \text{ if } h_i(Z) \in \{a, b\}, \text{ or}$$

$$h_i(Z') = h_i(Z), \text{ if } h_i(Z) \notin \{a, b\}. \quad (9)$$

or

$$h_i(Z') = h_i(Z), \text{ if } h_i(Z) \notin \{a, b\}. \quad (10)$$

8
If $T$ is imputable the randomization test for $H_0$ as described in Theorem 2.1 is valid at any level $\alpha$.

Building on Theorem 3.1 we can construct a family of valid conditional randomization tests by enumerating the assignment vectors for which conditions (9) and (10) hold. As an example, for any choice of $f(U \mid Z)$ we could define $g(Z \mid U, Z)$ as follows:

$$g(Z \mid U, Z) = 1, \text{ only if } Z = \{Z' \in Z : \text{Equations (9) and (10) are satisfied for } Z'\}. \quad (11)$$

Since $g$ is degenerate and chooses $Z$ deterministically, the conditioning mechanism $m(C \mid Z)$ is indexed solely by the conditional distribution, $f$, of focal units; we denote these conditioning mechanisms $m[f]$. Thus, our methodology provides a large space of conditioning mechanisms, which yield valid conditional randomization tests by construction. We can then search this space to select conditioning mechanisms with desired characteristics, such as high statistical power. For example, we could choose $f$ to maximize the expected number of effective focal units, $|\text{eff}(U)|$, where $\text{eff}(U) = \{i \in U : h_i(Z_{\text{obs}}) = a \text{ or } b\}$, that is, the units in $U$ whose outcomes are informative about $H_0$. Similarly, we could ensure that the number of possible randomizations is also large, and instead maximize the quantity $|\text{eff}(U)| \cdot |Z|$. Many choices are possible and should be tailored to the specific application.

4 Application: Interference in two-stage randomized trials

4.1 Two-stage randomized trials

We now turn to the use of conditional randomization tests in two-stage randomized trials, which are important designs for assessing spillovers between units (Hudgens and Halloran).
Specifically, we consider the setting of Basse and Feller (2017), in which \( N \) units reside in \( K \) households indexed by \( j = 1, \ldots, K \). In the first stage of the two-stage randomized trial, \( K_1 \) households are assigned to treatment, completely at random. In the second stage, one individual in each treated household is assigned to treatment, completely at random. As before, \( Z_i \in \{0,1\} \) is the assignment of unit \( i \), and \( Z = (Z_1, \ldots, Z_N) \) is the entire assignment vector. There is a residence index \( R_{ij} \), such that \( R_{ij} = 1 \) if unit \( i \) resides in household \( j \), and is 0 otherwise. Let \( [i] = \sum_j jR_{ij} \) denote the household wherein unit \( i \) resides. Finally, let \( W = (W_1, \ldots, W_K) \) denote the assignment vector on the household level, so that \( W_j = \sum_i Z_i R_{ij} \).

The stable unit treatment assumption is not realistic in this context, so we make two assumptions on the interference structure that will imply a specific exposure mapping. First, we make the partial interference assumption (Sobel, 2006): units can interact within, but not between, households. Second, we make the stratified interference assumption (Hudgens and Halloran, 2008): unit \( i \)'s potential outcomes only depend on the number of units treated in the household, here 0 or 1, rather than the precise identity of the treated unit. Manski (2013) refers to this as the “anonymous interactions” assumption. See Hudgens and Halloran (2008) for additional discussion on both assumptions.

These two assumptions can be expressed by the exposure mapping \( h_i(Z) = (Z_i, W_{[i]}) \). Since the potential outcome of unit \( i \) depends only on \( h_i(Z) \) by Assumption \( 5 \), for brevity we will use \( Y_{i}(Z_i, W_{[i]}) \) to denote the value of outcome \( Y_i(Z) \). Thus, unit \( i \)'s potential outcome can take only three values:

\[
Y_i(Z) \in \{Y_i(0,0), Y_i(0,1), Y_i(1,1)\};
\]

that is, \( Y_i(0,0) \) if unit \( i \) is a control unit in a control household; \( Y_i(0,1) \) if unit \( i \) is a control unit in a treated household; and \( Y_i(1,1) \) if unit \( i \) is a treated unit in a treated household. The
fourth combination, $Y_i(0, 1)$ is not possible because when unit $i$ is treated, household $[i]$ is also treated. Thus, the space of exposures is $\mathcal{H} = \{a, b, c\}$, with $a = (0, 0), b = (0, 1), c = (1, 1)$.

### 4.2 A valid test for spillovers in two-stage designs

We now focus on testing the null hypothesis of no spillover effect:

$$H_0^s: Y_i(0, 0) = Y_i(0, 1), \text{ for all } i = 1, \ldots, N.$$  \hfill (12)

In the supplement, we give comparable analysis and results for the null hypothesis of no primary effect, $H_0^p: Y_i(0, 0) = Y_i(1, 1)$, for every unit $i$. Equation (12) is a special case of the exposure contrast as defined in Equation (6), with $a = (0, 0)$ and $b = (0, 1)$. As in Section 3.2, we set the test statistic to be the difference in means for the two exposures. The challenge is to find a conditioning mechanism that guarantees validity while preserving power.

We impose two main constraints on our choice of focal units. First, we choose focal units exposed only to $\{a = (0, 0), b = (0, 1)\}$, since units exposed to $c = (1, 1)$ do not contribute to the difference-in-exposure-means test statistic. Equivalently, we want to exclude units assigned to $Z_i = 1$ from being considered as focals. Second, we choose a single non-treated unit at random from each household as focal. In the supplement, we show that choosing one focal unit per household leads to a randomization test that is equivalent to a permutation test on the exposures of interest, $a = (0, 0)$ and $b = (0, 1)$. We now state our testing procedure formally.

**Proposition 1.** Consider the following testing procedure:

1. In control households ($W_j = 0$), choose one unit at random. In treated households ($W_j = 1$), choose one unit at random among the non-treated units ($Z_i = 0$).
2. Compute the distribution of the test statistic in Equation (8) induced by all \( \binom{K}{K_1} \) permutations of exposures on the chosen units, using \( a = (0,0) \) and \( b = (0,1) \) as the contrasted exposures.

3. Compute the p-value.

Steps 1-3 outline a procedure that is valid for testing the null hypothesis of no spillover effect, \( H_0^s \).

The procedure in Proposition 1 is essentially: first, choose a set of focal units by choosing one unit from each household, at random, excluding units for which \( Z_i = 1 \); second, perform a classical Fisher randomization test on these units, which directly permutes their exposures. We show in the supplement that this procedure is an application of Theorem 3.1 with

\[
f(U \mid Z) = \text{Unif}\{U \subseteq U : Z_i I(i \in U) = 0, \text{ for every } i, \sum_i I(i \in U) R_{ij} = 1, \text{ for every } j\}. \tag{13}
\]

The first constraint in Equation (13) ensures that we only select focal units, \( i \in U \), that are not assigned to treatment; the second constraint restricts the focal set to one unit per household, as discussed earlier.

4.3 Comparison with existing methods

Our proposed procedure builds on several existing methods. First, the method of Aronow (2012), which outlines some ideas that we discuss here, develops a test for the null hypothesis of no spillover effect. Second, Athey et al. (2017) extend this approach to a broader class of hypotheses. In the supplement, we show that their proposed approach is equivalent to choosing a set of focal units independent of \( Z \); that is, \( f(U \mid Z) \equiv f(U) \). Thus, the analyst has less control over the choice of conditioning mechanism, which may reduce power or lead to an impractical test.
Importantly, in the two-stage design we consider, the methods of Aronow (2012) and Athey et al. (2017) are identical; see supplement for additional details. Specifically, their approaches suggest choosing one focal unit at random from each household:

\[
  f(U \mid Z) \equiv f(U) = \text{Unif}\{U \subseteq U : \sum_i \mathbb{I}(i \in U)R_{ij} = 1, \text{ for every } j\}. \tag{14}
\]

In general, our proposed design in Equation (13) will be easier to implement and will be more powerful than the design in Equation (14). First, we can implement our design via a simple permutation test, as described in Proposition 1. This is not always possible for the design in Equation (14), however, where the analyst has to calculate the support of \( g \) in Equation (11), fully and exactly, and then take uniform draws over that set for the randomization distribution. This calculation is exponentially hard and introduces meaningful implementation barriers. There are also no existing theoretical guarantees for when the test of Athey et al. (2017) can be implemented as a simple permutation test.

Second, unlike in our proposed design, the design in Equation (14) may include treated units as focal units. Since treated units are not part of the effective focal set for testing the null hypothesis of no spillover effect, including them in the test is wasteful. In particular, our proposed design will always have at least as many effective focal units as the design in Equation (14), and at least as many assignment vectors in the randomization test, which increases power. To quantify this comparison, consider the setting in which all households have \( n \) units. We show in the supplement that for the choice of \( f(U \mid Z) = f(U) \) in Equation (14), the number of effective focal units is a random variable with distribution

\[
  |\text{eff}(U)| \sim K - K_1 + \text{Binomial}(K_1, 1/n), \quad \text{where } K \text{ is the number of all households, and } K_1 \text{ is the number of treated households. So, } E[|\text{eff}(U)|] = K - K_1(1 - 1/n). \]

By contrast, the choice of \( f(U \mid Z) \) in Equation (13) leads to a number of effective focal units that is always equal to \( K \), the number of all households. For instance, in the experiment we describe
next, there are 3,169 households with \( n = 2 \) units. Restricting to this subset, the design in Equation (14) has an average of 2,123 effective focal units, a reduction of one-third from our proposed design.

Finally, we briefly mention Rigdon and Hudgens (2015), who propose a promising method for calculating exact confidence intervals in two-stage randomized designs with binary outcomes. Unfortunately, this method is not applicable to our setting since we have a continuous outcome; nor is the proposed approximation well-suited for exact tests of a given null hypothesis.

### 4.4 Application to a school attendance experiment

We illustrate our approach using a randomized trial of an intervention designed to increase student attendance in the School District of Philadelphia (Rogers and Feller, 2017). Following the setup in Basse and Feller (2017), we focus on a subset of this experiment with \( N = 8,654 \) students in \( K = 3,876 \) multi-student households, of which \( K_1 = 2,568 \) were treated. For this subset, the district sent targeted attendance information to the parents about only one randomly chosen student in that household. The outcome of interest is the number of days absent during the remainder of the school year. Following Rosenbaum et al. (2002), we focus on regression-adjusted outcomes, adjusting for a vector of pre-treatment covariates, including demographics and prior year attendance. Additional details on the analysis are included in the supplement, including results for the primary effect.

To assess spillovers, we sample 100 sets \( U^{(l)}, l = 1, \ldots, 100 \), for both ours and Athey et al. (2017)’s choice of function \( f(U \mid Z) \). For each set, we compute \( p \)-values for the null hypothesis of no spillover effect \( H_0^s \) in Equation (12) and report whether it rejects with \( p < 0.05 \). Overall, the test using Athey et al. (2017)’s method rejects the null of no effect for 66% of focal sets; the test using our method rejects the null of no effect for 92% of focal sets.
We also obtain confidence intervals and Hodges-Lehmann point estimates by inverting a sequence of randomization tests under an additive treatment effect model, \( Y_i(1,0) = Y_i(0,0) + \tau \) for all \( i = 1, \ldots, N \). For each focal set, obtaining these quantities is straightforward given \( U \) via standard methods (Rosenbaum et al., 2002). Aggregating information across focal sets, however, remains an open problem; we discuss this briefly in Section 5. For simplicity, we summarize the results by presenting medians across focal sets. For our proposed approach, the median value of the Hodges-Lehmann point estimates is \( \hat{\tau}^{(\text{cond})} \approx -1 \) day, with associated 95% confidence interval \([-1.70, -0.34]\). For the method of Athey et al. (2017), the median point estimate is \( \hat{\tau}^{(\text{rand})} \approx -1.1 \) days, with associated 95% confidence interval \([-1.84, -0.28]\). Across focal sets, the average width of the confidence intervals obtained via Athey et al. (2017)’s method is 1.60, compared to 1.42 with our approach, a reduction of 11%.

Overall, results from both approaches are in line with those obtained by Basse and Feller (2017) via unbiased estimators. These confirm the presence of a meaningful within-household spillover effect that is nearly as large as the primary effect. Substantively, this suggests that intra-household dynamics play a critical role in reducing student absenteeism and should be an important consideration in designing future interventions.

5 Discussion

While we demonstrate this approach with two-stage designs, constructing appropriate conditional mechanisms can be challenging in more complex settings. A more general procedure requires understanding the interference structure, and then optimizing for test power subject to that structure. Even when construction of conditioning mechanisms is possible, our framework for conditional randomization tests generally produces a distribution of p-values across random choices for the conditioning event. While this does not affect the validity of
the test, it leaves open problems such as interpretation and sensitivity of the test results; see, for example, Geyer and Meeden (2005). At the same time, the distribution itself likely contains useful information to improve the power of the test. In ongoing research, we are working to leverage methods from the multiple testing literature to address this problem.

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A Proof of theorems and statements

A.1 Proof of validity of classical Fisher test in Section 2.1

We reproduce the proof of Hennessy et al. (2016) with slight modifications. This proof will provide an introduction to the proof of the validity of the conditional test that follows.

\textit{proof – Reproduced from Hennessy et al. (2016).} We need to show that:

\[ \text{pr}\{p \leq \alpha \mid H_0\} \leq \alpha, \text{ for all } \alpha \in [0,1], \]

where the probability is with respect to \(\text{pr}(Z_{\text{obs}})\), and \(p = p\text{val}(Z_{\text{obs}})\) is defined as:

\[ p = \text{pr}\{T(Z \mid Y_{\text{obs}}) \geq T(Z_{\text{obs}} \mid Y_{\text{obs}})\}. \]

We proceed in steps:

\underline{Step 1:} Let \(U\) be a random variable with the same distribution as \(T(Z \mid Y_{\text{obs}})\) as induced by \(\text{pr}(Z)\) and let \(F_U\) be its cumulative distribution function. We can then write:

\[ p = 1 - F_U\{T(Z_{\text{obs}} \mid Y_{\text{obs}})\}. \]

\underline{Step 2:} By definition, under \(H_0\) we have that \(Y(Z) = Y(Z_{\text{obs}})\) for all \(Z\), and so \(T(Z \mid Y_{\text{obs}}) = T\{Z \mid Y(Z)\}\). It follows that, under \(H_0\), \(U\) has the same distribution as \(T(Z \mid Y_{\text{obs}})\).

\underline{Step 3:} The randomness in \(T(Z_{\text{obs}} \mid Y_{\text{obs}})\) is induced by the randomness in \(Z_{\text{obs}}\). In the testing procedure, \(Z_{\text{obs}} \sim \text{pr}(Z_{\text{obs}})\). Combining with Step 2, we see that the distribution of \(T(Z_{\text{obs}} \mid Y_{\text{obs}})\) induced by \(\text{pr}(Z_{\text{obs}})\) is the same as that of \(U\) under \(H_0\). We thus have:

\[ p = 1 - F_U(U). \]
By the probability integral transform theorem, \( p \) is uniform, and so \( pr(p \leq \alpha | H_0) \leq \alpha. \)

## A.2 Proof of Theorem 2.1

The proof of Theorem 2.1 follows that of the classical Fisher test, with some important modifications.

**Theorem** (2.1). Let \( H_0 \) be a null hypothesis and \( T(Z \mid Y, C) \) a test statistic, such that \( T \) is imputable with respect to \( H_0 \) under some conditioning mechanism \( m(C \mid Z) \); that is, under \( H_0 \), it holds that

\[
T\{Z' \mid Y(Z'), C\} = T\{Z' \mid Y(Z), C\},
\]

for all \( Z, Z', C \), for which \( pr(Z, C; m) > 0 \) and \( pr(Z', C; m) > 0 \). Consider the procedure where we first draw \( C \sim m(C \mid Z^{\text{obs}}) \), and then compute the conditional p-value,

\[
pval(Z^{\text{obs}}; C) = E_Z[\mathbb{I}\{T(Z \mid Y^{\text{obs}}, C) > T^{\text{obs}}\} \mid C],
\]

where \( T^{\text{obs}} = T(Z^{\text{obs}} \mid Y^{\text{obs}}, C) \), and the expectation is with respect to \( pr(Z \mid C) = pr(Z, C; m)/pr(C) \).

This procedure is valid at any level, that is, \( pr\{pval(Z^{\text{obs}}; C) \leq \alpha \mid C\} \leq \alpha \), for any \( \alpha \in [0, 1] \).

**Proof.** We need to show that:

\[
pr(p \leq \alpha \mid H_0, C) \leq \alpha, \text{ for all } C : pr(C \mid Z^{\text{obs}}) > 0.
\]

where the probability is with respect to \( pr(Z^{\text{obs}} \mid C) \), and \( p \) is defined as:

\[
p = pr\{T(Z \mid Y^{\text{obs}}, C) \geq T\{Z^{\text{obs}} \mid Y^{\text{obs}}, C\} \mid C\}
\]

We proceed in steps:
Step 1: Fix $C$. Let $U$ be a random variable with the same distribution as $T(Z \mid Y^{obs}, C)$ as induced by $\text{pr}(Z \mid C)$ and let $F_U$ be its cumulative distribution function. We can then write:

$$p = 1 - F_U\{T(Z^{obs} \mid Y^{obs}, C)\}.$$

Step 2: In the procedure, we have $Z^{obs} \sim \text{pr}(Z^{obs})$ and $C \sim \text{pr}(C \mid Z^{obs})$, implying that $\text{pr}(Z^{obs}, C) > 0$. So, by imputatability of the test statistic in Equation (3) under $H_0$, we have that

$$T(Z \mid Y(Z), C) = T(Z \mid Y^{obs}, C).$$

for all $Z \sim \text{pr}(Z \mid C)$, since this guarantees $\text{pr}(Z, C) > 0$. This means that under $H_0$, $U$ has the same distribution as $T(Z \mid Y^{obs}, C)$.

Step 3: The randomness in $T(Z^{obs} \mid Y^{obs}, C)$ is induced by the randomness in $Z^{obs}$ conditional on $C$. Combining with Step 2, we see that the distribution of $T(Z^{obs} \mid Y^{obs}, C)$ induced by $\text{pr}(Z^{obs} \mid C)$ is the same as that of $U$ under $H_0$. We thus have:

$$p = 1 - F_U(U).$$

By the probability integral transform theorem, $p$ is uniform and so $\text{pr}(p \leq \alpha \mid H_0, C) \leq \alpha$. 

\[\square\]
A.3 Proof of Theorem 3.1

For reader’s convenience we repeat the definitions of the contrast null hypothesis, conditioning mechanism, and test statistic, which are used by Theorem 3.1:

\[
H_0: Y_i(Z) = Y_i(Z'), i = 1, \ldots, N, \text{ for all } Z, Z' \text{ for which } h_i(Z), h_i(Z') \in \{a, b\}.
\]

\[
m(C | Z) = f(U | Z)g(Z | U, Z),
\]

\[
T(Z | Y, C) = \text{Ave}\{Y_i | i \in U, h_i(Z) = a\} - \text{Ave}\{Y_i | i \in U, h_i(Z) = b\},
\]

where \( C = (U, Z) \), and \( U, Z \) are any subsets of units and assignment vectors, respectively.

The main challenge is to prove that the conditions of the theorem do indeed ensure that the test statistics in Equation (8) is imputable under \( H_0 \).

**Theorem (3.1).** Let \( H_0 \) be a null hypothesis as in Equation (6), \( m(C | Z) \) be a conditioning mechanism as in Equation (7), and \( T \) be a test statistic defined only on focal units, as in Equation (8). Then, \( T \) is imputable under \( H_0 \) if \( m(C | Z) > 0 \) implies that \( Z \in Z \), and for every \( i \in U, Z' \in Z \), that

\[
h_i(Z') \in \{a, b\}, \text{ if } h_i(Z) \in \{a, b\},
\]

\[
h_i(Z') = h_i(Z), \text{ if } h_i(Z) \notin \{a, b\}.
\]

If \( T \) is imputable the randomization test for \( H_0 \) as described in Theorem 2.1 is valid at any level \( \alpha \).

**Proof.** For a conditioning event \( C = (U, Z) \), assume that \( m(C|Z) > 0 \) implies that \( Z \in Z \) and that:

\[
\forall i \in U, \forall Z' \in Z, \begin{cases} h_i(Z') \in \{a, b\} \text{ if } h_i(Z) \in \{a, b\}, \\ h_i(Z') = h_i(Z) \text{ if } h_i(Z) \notin \{a, b\}. \end{cases}
\]
Now let $Z, Z', C$ be such that $\Pr(Z, C; m) > 0$ and $\Pr(Z', C; m) > 0$. This implies that $m(C \mid Z) > 0$ and $m(C \mid Z') > 0$. So, by the assumption above, it follows that $Z \in \mathcal{Z}$ and $Z' \in \mathcal{Z}$.

Now take $i \in \mathcal{U}$. If $h_i(Z') \notin \{a, b\}$, then by assumption $h_i(Z) = h_i(Z')$ since $Z, Z' \in \mathcal{Z}$. And so by Equation (5) of the paper, we have that $Y_i(Z') = Y_i(Z)$. If instead $h_i(Z') \in \{a, b\}$, then $h_i(Z) \in \{a, b\}$ and so under the null hypothesis, $Y_i(Z') = Y_i(Z)$. Therefore, we proved that $Y_{i\mathcal{U}}(Z') = Y_{i\mathcal{U}}(Z)$, where $Y_{i\mathcal{U}}(Z)$ denotes the subvector of outcomes of units in $\mathcal{U}$ under assignment vector $Z$. Since the test statistic, $T(Z \mid Y, C)$, is defined only on $Y_{i\mathcal{U}}$, the subvector of outcomes of units in $\mathcal{U}$, it follows that $T(Z' \mid Y(Z'), C) = T(Z' \mid Y(Z), C)$.

A.4 Proof of Proposition 1

**Proposition.** Consider the following testing procedure:

1. In control households ($W_j = 0$), choose one unit at random. In treated households ($W_j = 1$), choose one unit at random among the non-treated units ($Z_i = 0$).

2. Compute the distribution of the test statistic in Equation (8) induced by all $\binom{K}{K_1}$ permutations of exposures on focal units, using $a = (0, 0)$ and $b = (0, 1)$ as the contrasted exposures.

3. Compute the p-value.

Steps 1-3 outline a procedure that is valid for testing the null hypothesis of no spillover effect, $H_0^s$.

**Proof.** Define

$$\mathcal{U}(Z) = \{ \mathcal{U} \in \mathcal{U} : Z_i \mathbb{1}(i \in \mathcal{U}) = 0, i = 1, \ldots, N, \text{ and } \sum_i \mathbb{1}(i \in \mathcal{U}) R_{ij} = 1, \text{ for every household } j \}.$$
In words, \( \mathbb{U}(Z) \) is the set of all subset of units for which: no unit in the subset is treated under \( Z \), and each household has exactly one unit in the subset. Step 1 of the procedure in Proposition 1 chooses focals according to conditioning mechanism:

\[
m(C|Z) = f(U | Z)g(Z | U, Z),
\]

where we define

\[
f(U | Z) = \text{Unif}\{\mathbb{U}(Z)\},
\]

\[
g(Z | U, Z) = \mathbb{I}[Z = \{Z': h_i(Z') \in \{(0,0), (0,1)\} \text{ for all } i \in \mathcal{U}\}].
\]

That is, \( f(U | Z) \) is uniform on \( \mathbb{U}(Z) \) and \( g \) is degenerate on the set of assignments for which all units in \( \mathcal{U} \) are either in control or exposed to spillovers.

Fix a conditioning event \( C = (\mathcal{U}, Z) \).

**Step 1:** Let \( H(Z) \in \{0,1\}^K \) denote the exposure of focal units under \( Z \), where we use 0 for control and 1 for spillovers. Also, let \( W(Z) \in \{0,1\}^K \) denote the household assignment under assignment vector \( Z \). Since there is one focal per household and household assignment determines the exposure of a focal, \( H \) and \( W \) are equal almost surely:

\[
H(Z) = W(Z), \text{ for all } Z, \text{ and so } H = W, \text{ almost surely.}
\]

**Step 2:** For any \( Z, Z' \in \mathcal{Z} \), it holds that

\[
g(Z | U, Z) = g(Z | U, Z').
\]

This also follows from definition of \( g \) in Equation (11) since \( g(Z | U, Z) \equiv g(Z | U) \) does not depend on \( Z \) given a fixed \( U \); note that \( U \) depends on \( Z \) itself, but still \( g \) does not depend
on $Z$ if $U$ is given.

**Step 3**: For any $w \in \{0, 1\}^K$, it holds that:

$$\sum_{Z: W(Z) = w} f(U|Z) pr(Z|W = w) = \text{const.}$$

To see this, first note that $pr(Z|W) = \prod_{k: W_k = 1} 1/n_k$, where $n_k$ is the number of units in the household. Furthermore, $\sum_{Z: W(Z) = w} f(U|Z) = \prod_{k: W_k = 0} 1/n_k$. Therefore, $\sum_{Z: W(Z) = w} f(U|Z) pr(Z|W = w) = \prod_k 1/n_k = \text{const} —$ actually this is equal to the marginal probability of the focal set, $pr(U)$.

**Step 4**: We prove that the conditioning mechanism yields a randomization distribution that is uniform in its support. Fix a conditioning event $C = (U, Z)$. Then,

$$pr(H|C) = pr(W|C) \ [\text{from Step } 1]$$

$$\propto pr(C|W) pr(W)$$

$$\propto \sum_Z pr(C, Z|W) pr(W)$$

$$\propto \sum_{Z: W(Z) = W} pr(C|Z) pr(Z|W) pr(W)$$

$$\propto \sum_{Z: W(Z) = W} f(U|Z) g(Z | U, Z) pr(Z|W) pr(W) \ [\text{by definition of mechanism}]$$

$$\propto g(Z | U) pr(W) \sum_{Z: W(Z) = W} f(U|Z) pr(Z|W) \ [\text{from Step } 2]$$

$$\propto pr(W) \ [\text{from Step } 3]$$

$$= \binom{N}{N_1}^{-1}. \quad (15)$$
**Step 5:** From the definition of the test statistic:

\[ T(Z \mid Y, C) = T(Z' \mid Y, C) \text{ if } H(Z) = H(Z'). \]

Therefore, we can write \( T(Z \mid Y, C) \equiv T(H \mid Y, C) \), where \( H \equiv H(Z) \) for brevity.

**Step 6:** From Step 4 the conditional distribution of the focal's exposure under the particular conditioning mechanism is a permutation of their exposures under \( Z_{\text{obs}} \), as prescribed by the testing procedure of Proposition 1. This is sufficient for validity since the test statistic is in fact a function of \( H \), by Step 5.

### B Additional discussion of alternative methods

#### B.1 Equivalence of tests from Athey et al. (2017) and Aronow (2012) for two-stage designs

The tests described by [Athey et al. (2017)](2017) and [Aronow (2012)](2012) coincide for testing spillover effects, \( H^s_0 \), in our two-stage randomized setting. We will show that the method of [Aronow (2012)](2012) is equivalent to our procedure, with \( f(U \mid Z) = f(U) \). Briefly, the method of [Aronow (2012)](2012) can be summarized as follows:

1. Draw a set of units \( U \subset U \), uniformly at random – identical to Step 1 of [Athey et al. (2017)](2017).

2. Compute the p-value by using the conditional randomization distribution \( P(Z \mid U, Z_{U} = Z_{U}^{\text{obs}}) \), where \( Z_{U} \) is the subvector of \( Z \) that is restricted to the units in \( U \).
The conditional randomization distribution is therefore equal to:

\[
P(Z | U, Z_U = Z_U^{\text{obs}}) \propto P(U, Z_U = Z_U^{\text{obs}} | Z) \pr(Z) \propto P(Z_U = Z_U^{\text{obs}} | U, Z) P(U | Z) \pr(Z)
\]

\[
= \mathbb{I}(Z_U = Z_U^{\text{obs}}) P(U) \pr(Z).
\]

Now, consider a conditioning event \( C = (U, Z) \) from a mechanism \( m_f(C | Z) = f(U)g(Z | U, Z) \), where according to Equation (11) in the main paper is degenerate on the set:

\[
Z = \{ Z' : h_i(Z') = (1, 1) \text{ if } h_i(Z^{\text{obs}}) = (1, 1) \text{ and } h_i(Z') \in \{(0, 0), (0, 1)\} \text{ otherwise, for all } i \in U \}
\]

(16)

Under this definition and the setting of spillover effects, for every unit \( i \in U \) in the focal set and every assignment vector \( Z' \in Z \) in the test, we will have either \( Z'_i = 0 \) if \( Z_i^{\text{obs}} = 0 \) or \( Z'_i = 1 \) if \( Z_i^{\text{obs}} = 1 \). Thus, if \( Z' \in Z \) it follows that \( Z'_U = Z_U^{\text{obs}} \). Suppose the reverse is true, that is, \( Z'_U = Z_U^{\text{obs}} \). Consider unit \( i \) in the focal set for which \( Z_i^{\text{obs}} = 1 \). Then, \( Z'_i = 1 \) as well, and so \( h_i(Z') = h_i(Z^{\text{obs}}) \) for such units. Consider unit \( i \) in the focal set for which \( Z_i^{\text{obs}} = 0 \). Then, \( Z'_i = 0 \) as well, and so \( h_i(Z') = \in \{(0, 0), (0, 1)\} \), by definition of exposures. Thus, if \( Z'_U = Z_U^{\text{obs}} \) it follows that \( Z' \in Z \). Therefore, the two statements are equivalent, and the conditioning mechanism with \( f(U | Z) = f(U) \) will yield the same test as in \cite{Athey2017} and \cite{Aronow2012}.

B.2 The method of Athey et al. (2017) as a weighted permutation

The method of Athey et al. can be cast in our framework, where \( f(U | Z) = f(U) \), i.e., the selection of focals does not depend on the observed assignment, and where randomization distribution is \( P(Z | C) \) is uniform over the set \( Z \) defined in Equation (16). We denote
by $\mathcal{U}^{\text{eff}}(Z) = \{i \in \mathcal{U} : Z_i = 0\}$. We denote by $H_i = h_i(Z)$ the exposure of unit $i$ under assignment vector $Z$.

**Step 1** It holds that $\mathcal{U}^{\text{eff}}(Z) = \mathcal{U}^{\text{eff}}(Z^{\text{obs}})$, for every $Z \in \mathcal{Z}$.

**Step 2:** Consider unit $i \in \mathcal{U}^{\text{eff}}(Z^{\text{obs}})$. We have:

$$P(H_i = (1, 0) \mid Z \in \mathcal{Z}) = P(Z_i = 0, W_{[i]} = 1 \mid Z \in \mathcal{Z})$$

$$= \frac{P(Z_i = 0 \mid W_{[i]} = 1)P(W_{[i]} = 1)}{P(Z \in \mathcal{Z})}$$

$$= \frac{(n_i - 1)/n_i \cdot \left(\frac{N}{N_i}\right)}{P(Z \in \mathcal{Z})}$$

$$\propto \frac{n_i - 1}{n_i}$$

**Step 3:** From Step 2, we have the constraint that for all $Z \in \mathcal{Z}$:

$$\sum_{i \in \mathcal{U}^{\text{eff}}(Z)} \mathbb{I}(H_i(Z) = (1, 0)) = N_1^{\text{eff}}(Z^{\text{obs}}).$$

In words, the number of exposed units is constant for all $Z \in \mathcal{Z}$.

**Step 4:** Combining all the steps, we see that $P(H \mid Z \in \mathcal{Z})$ is a weighted permutation:

1. $\sum_{i \in \mathcal{U}^{\text{eff}}} \mathbb{I}(H_i = (1, 0)) = N_1^{\text{eff}}$.

2. $\forall i \in \mathcal{U}^{\text{eff}}, P(H_i = (1, 0) \mid Z \in \mathcal{Z}(\mathcal{U}, Z^{\text{obs}})) \propto \frac{n_i - 1}{n_i}$.

This result implies that the method of [Athey et al. (2017)](2017) can be implemented as a permutation test only when the households are of equal sizes.
C Simulations and analysis details

C.1 Simulations

We compare the power of the test we proposed in the previous section, which choses the focals conditionally on $Z^{obs}$, to that of the test in Athey et al. (2017) which choses the focals randomly. Figure 1 illustrates the potential power gains, by considering the extreme case of $K = 500$ households of equal size $n = 50$ with $K_1 = 250$ treated households, and focusing on the power of the test of no primary effect $H^p_0$.

If we are interested in testing the no spillover effect hypothesis $H^s_0$, the expected difference in the number of effective focal units between our test and the test of Athey et al. (2017) decreases with $n$. In the case of the no primary effect hypothesis $H^p_0$, the difference increases with $n$. This phenomenon is illustrated in Figure 2.
Figure 2: Power of the two methods for testing the null hypotheses of no primary effect (left) and no spillover effect (right), as a function of household size $n_i$

C.2 Details of analysis

C.2.1 Covariate adjustment

In all the analyses in the paper, covariates where taken into account via the same *model-assisted* approach used by Basse and Feller (2017) (see Section 7 and Section 9.2 of their paper). Briefly, we use a holdout set to estimate the parameter of a regression, then we use those estimators parameters to obtain predicted values $\{\hat{Y}_i\}$ for the outcomes in our sample and compute the residuals $\hat{e}_i = Y_i^{obs} - \hat{Y}_i$. We then apply the conditional testing methodology to the residuals, instead of the original potential outcomes (the residuals can be thought of as "transformed outcomes"). Note that this approach is similar to that used by Rosenbaum et al. (2002).

C.2.2 Confidence intervals

We ran an additional analysis comparing the size of confidence intervals for our method and for that of Athey et al. (2017). Specifically, for each of $H_0^s$ and $H_0^p$, we drew 100 focal
Figure 3: Size of the confidence intervals obtained by the two methods, for multiple draws of focal sets, when testing the hypothesis of no primary effect (left) and the hypothesis of no spillover (right)

sets using our method, and 100 using the method of Athey et al. (2017), and computed the associated confidence intervals, obtained by inverting sequences of Fisher randomization tests (Rosenbaum et al., 2002). Figure 3 summarizes the results. We see that our method leads to smaller confidence intervals compared to the method of Athey et al. (2017), and that the difference is slightly more dramatic for the primary effect than for the spillover effect.

C.2.3 Point estimates

Point estimates are obtained using the a variant of the Hodges-Lehmann estimator (Hodges Jr and Lehmann, 1963). Specifically, for a conditioning event \( C \), we numerically solve the equation:

\[
E[T \mid C, H^P_\tau] = T^{\text{obs}}
\]

where \( H^P_\tau \) is the null hypothesis \( Y_i(1, 1) = Y_i(0, 0) \), by considering a grid of values for \( \tau \), and computing the expectation of the null distribution of \( T \) under the hypothesis \( H^P_\tau \) and keeping the value \( \hat{\tau} \) of \( \tau \) that is closest to \( T^{\text{obs}} \).
C.2.4 Analysis results for testing $H_0^P$

The median value of the Hodges-Lehmann for the primary effect is approximately equal for both choices of functions $f$. We have: $\hat{\tau}_{\text{cond}} \approx \hat{\tau}_{\text{rand}} = -1.5$ days, with associated confidence interval $[-2.2, 0.75]$ for our method, and $[-2.3, -0.8]$ for the method of [Athey et al. (2017)].

The average of length of confidence intervals obtained with our method is 1.4 days, versus 1.6 days for the method of [Athey et al. (2017)]. The fraction of focals leading to a p-value below 0.05 is 100% in our case (this is a Monte-Carlo estimate based on 100 replications), versus 92% for the method in [Athey et al. (2017)].

D Comparing the powers of tests

Our heuristics are built in a number of steps. First, we give a heuristic in the context of classical Fisher randomization tests, showing that in general, tests that are balanced and use more units are more powerful. In a second step, we show that since our test and the test in [Athey et al. (2017)] can be conceived as classical Fisher randomization tests run on the focal units (in the two-stage randomization case), the heuristics for the classical Fisher randomization test apply.

D.1 Model, p-values and power

Model

Consider a classical Fisher randomization test (no interference), with complete randomization where $N_1$ out of $N$ units are treated. Let $p = \frac{N_1}{N}$. Suppose that that the true effect is constant additive $\tau$, and that we test for the null of no effect $H_0$. In order to give concrete analytical heuristics, we consider a model for the potential outcomes and focus on asymptotics.
(see also Lehmann and Romano (2006) for this approach):

$$Y_i(Z_i) \sim \tau Z_i + \mathcal{N}(\mu, \sigma^2)$$

**P-values**

As mentioned, we will focus our argument on asymptotic heuristics. Denote by $V = Var(T|Y_{obs}, H_0)$ the randomization variance of the test statistics conditional on $Y_{obs}$, and assuming $H_0$ is true. We have, for large $N$:

$$V = \frac{1}{N} \left[ \frac{\sigma^2}{p(1-p)} + \tau^2 \right]$$

Denote by $V_{obs}$ the variance of the test statistic $V_{obs} = Var(T)$. We have, for large $N$:

$$V_{obs} = V - \frac{\tau^2}{N}$$

and so by applying the appropriate CLT’s, we have:

$$\frac{T}{\sqrt{V}} \approx \mathcal{N}(0, 1) \quad \text{and} \quad \frac{T_{obs} - \tau}{\sqrt{V_{obs}}} \approx \mathcal{N}(0, 1)$$

Note the application of the CLT is heuristic here, and some regularity conditions are required. We can then obtain an approximation of the distribution of a (one-sided) p-value for large $N$:

$$pval = P(T \geq T_{obs}) \approx 1 - \Phi \left( \frac{T_{obs}}{\sqrt{V}} \right)$$
using the asymptotics from above. We can then verify that:

\[
\frac{T_{\text{obs}}}{\sqrt{V}} = \frac{T_{\text{obs}} - \tau}{\sqrt{V_{\text{obs}}}} \cdot \sqrt{1 - C} + \sqrt{N} \sqrt{C} \\
\approx W \sqrt{1 - C} + \sqrt{N} \sqrt{C}
\]

where \( W \sim \mathcal{N}(0, 1) \) and \( C = \frac{\tau^2}{\sigma^2(1-p) + \tau^2} \) and so:

\[pval = 1 - \Phi(W \cdot \sqrt{1 - C} + \sqrt{N} \sqrt{C}) \quad (17)\]

**Power**

We can use the approximation of Equation 17 to deal with the power. For \( \alpha \in [0, 1] \), the power of the test at level \( \alpha \) will be:

\[\beta_\alpha = P(pval \leq \alpha)\]

but we verify that:

\[pval \leq \alpha \Leftrightarrow W \geq \frac{\Phi^{-1}(1 - \alpha) - \sqrt{N} \sqrt{C}}{\sqrt{1 - C}}\]

and so the power of the test will be approximately:

\[\beta_\alpha = 1 - \Phi\left(\frac{\Phi^{-1}(1 - \alpha) - \sqrt{N} \sqrt{C}}{\sqrt{1 - C}}\right) \quad (18)\]

**D.2 Comparing classical tests**

We are interested in comparing tests with different proportions \( p \) of treated units, and with different numbers \( N \) of units. We will denote these quantities by \( N^{(1)} \) and \( N^{(2)} \) for the
number of units, and \( p^{(1)} \) and \( p^{(2)} \) for the proportions. Let \( \beta^{(1)} \) and \( \beta^{(2)} \) be the associated powers. Finally, notice that:

\[
\beta^{(1)} \leq \beta^{(2)} \iff \Phi^{-1}(1 - \alpha) - \frac{\sqrt{N^{(1)}} \sqrt{C(1)}}{\sqrt{1 - C(1)}} \geq \Phi^{-1}(1 - \alpha) - \frac{\sqrt{N^{(2)}} \sqrt{C(2)}}{\sqrt{1 - C(2)}}
\]

\[\iff \gamma^{(1)} \geq \gamma^{(2)}\]

where \( \gamma^{(1)} = \frac{\Phi^{-1}(1 - \alpha) - \sqrt{N^{(1)}} \sqrt{C(1)}}{\sqrt{1 - C(1)}} \).

### D.2.1 Different fraction of treated units

Suppose that both tests have the same number of units \( N^{(1)} = N^{(2)} = N \), but different fractions of treated units \( p^{(1)} \neq p^{(2)} \). We have:

\[
\gamma^{(1)} - \gamma^{(2)} = \sqrt{N} \left( \frac{\sqrt{C(2)}}{1 - \sqrt{C(2)}} - \frac{\sqrt{C(1)}}{1 - \sqrt{C(1)}} \right) + \left( \Phi^{-1}(1 - \alpha) - \frac{\Phi^{-1}(1 - \alpha)}{\sqrt{1 - C(2)}} \right)
\]

\[
\rightarrow \sqrt{N} \left( \frac{\sqrt{C(2)}}{1 - \sqrt{C(2)}} - \frac{\sqrt{C(1)}}{1 - \sqrt{C(1)}} \right)
\]

and so for large \( N \),

\[
\gamma^{(1)} - \gamma^{(2)} \geq 0 \iff \frac{\sqrt{C(2)}}{1 - \sqrt{C(2)}} - \frac{\sqrt{C(1)}}{1 - \sqrt{C(1)}} \geq 0
\]

\[\iff p^{(1)}(1 - p^{(1)}) \leq p^{(2)}(1 - p^{(2)})\]

\[\iff |p^{(1)} - \frac{1}{2}| \geq |p^{(2)} - \frac{1}{2}|\]

so in conclusion:

\[
\beta^{(1)} \leq \beta^{(2)} \iff |p^{(1)} - \frac{1}{2}| \geq |p^{(2)} - \frac{1}{2}|
\]

which, in words, means that the balanced test has more power (asymptotically).
D.2.2 Different number of units

Suppose that \( N^{(1)} \neq N^{(2)} \) but that the fractions of treated units in each test is identical. That is, \( p^{(1)} = p^{(2)} = p \). The immediate consequence is that \( C^{(1)} = C^{(2)} = C \), and so:

\[
\gamma^{(1)} - \gamma^{(2)} = \frac{\sqrt{C}}{1 - \sqrt{C}} \left( \sqrt{N^{(2)}} - \sqrt{N^{(1)}} \right)
\]

and so:

\[
\beta^{(1)} \leq \beta^{(2)} \iff N^{(1)} \leq N^{(2)}
\]

which in words means that the test with more units has more power (asymptotically).

D.3 Comparing the power of our test with that of Athey et al. (2017)

If we restrict our attention to the special case where all households have equal size \( n_i = n \), then both our method and the method of Athey et al. (2017) can be seen as classical Fisher randomization tests applied on a set of "effective" focal units, where the set of "effective focals" is always at least as large with our method as in the method of Athey et al. (2017), and is always balanced if the initial assignment \( pr(Z) \) is balanced. We can then leverage the result of Section D.2 to argue heuristically that for classical Fisher randomization tests, larger and more balanced is generally better (again, this is a heuristic since the result was established asymptotically, for a certain model of potential outcomes.. we believe that it is a reasonable argument, however), and so we expect our method to lead to more powerful test. This has been confirmed in the simulations of Section C.1 and in the analysis.
E Testing the null hypothesis of no primary effect

The paper focused on testing the null hypothesis of no spillover effects $H_0^S$. In this section, we briefly give equivalent results for testing the null hypothesis of no primary effect $H_0^P$. We omit the proofs, since they follow exactly the same outlines as the proof for $H_0^S$.

A simple choice of $f$ function for testing the null hypothesis of no primary effect is:

$$f(U|Z) = \text{Unif}(\mathbb{U}^{(P)}(Z))$$

where:

$$\mathbb{U}^{(P)}(Z) = \{U \in \mathbb{U} : Z_i = 1 \Rightarrow i \in U, \ \forall i \in U \quad \text{and} \quad \sum_i \mathbb{I}(i \in U)R_{ij} = 1, \text{for every household } j\}$$

If applied to Theorem 2, this choice of $f$ leads to the following procedure, which mirrors that of Proposition 1:

1. In control households ($W_j = 0$), choose one unit at random. In treated households ($W_j = 1$), choose the treated unit as focal.

2. Compute the distribution of the test statistic Equation (8) induced by all $\binom{K}{K_1}$ permutations of exposures on focal units, using $a = (0,0)$ and $b = (1,1)$ as the contrasted exposures.

3. Compute the p-value.

This procedure is valid conditionally and marginally for testing $H_0^P$. 