Split-Sample Strategies for Avoiding False Discoveries *

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Abstract

Preanalysis plans (PAPs) have become an important tool for limiting false discoveries in field experiments. We evaluate the properties of an alternate approach which splits the data into two samples: An exploratory sample and a confirmation sample. When hypotheses are homogeneous, we describe an improved split-sample approach that achieves 90% of the rejections of the optimal PAP without requiring preregistration or constraints on specification search in the exploratory sample. When hypotheses are heterogeneous in priors or intrinsic interest, we find that a hybrid approach which prespecifies hypotheses with high weights and priors and uses a split-sample approach to test additional hypotheses can have power gains over any pure PAP. We assess this approach using the community-driven development (CDD) application from Casey et al. (2012) and find that the use of a hybrid split-sample approach would have generated qualitatively different conclusions.

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1 Introduction

A classic tradeoff in data analysis exists between estimating large numbers of parameters and generating results that do not reproduce in new samples. In computer science and machine learning this problem is known as "overfitting;" in biostatistics it manifests itself in "large-scale multiple testing." In the past decade it has become a critical issue in empirical microeconomics with the widespread use of field experiments.[1] Researchers designing field experiments often face high fixed costs in setting up the experiment and low marginal costs in adding additional survey outcomes. Increasing sample size is expensive, and the samples in many field experiments are too small to detect anything less than a large effect. Given these constraints and the focus on positive results in economics and other social sciences (Gerber and Malhotra 2008; Yong 2012), researchers face strong incentives to test for effects on many outcomes or subgroups and then emphasize the subset of significant results. Unfortunately this behavior maximizes the chances of "false discoveries" (type I errors) that do not replicate in new samples. Although social scientists are becoming aware of the problem (Miguel et al. 2014), standards have yet to be established to address it.

Limiting false discoveries is an economic problem as well as a statistical one because it must address the information asymmetry between researchers and reviewers. Though statistical tools are available for controlling the false discovery rate, most economics papers do not rigorously test whether their p-values are more extreme than would be expected under the null hypothesis based on the number of reported results. More importantly, even procedures that control the false discovery rate (FDR) or familywise error rate (FWER) may be easily gamed by underreporting of insignificant results; the problem is that the reviewer does not know whether the reported number of tests performed reflects the true number of tests performed. This problem challenges most solutions in empirical economics; unless researchers can commit to reliably reporting the full set of enacted tests, we cannot accurately adjust the p-values of those tests for multiple inference.[2]

One method of resolving the critical information asymmetry is the use of a preanalysis plan (PAP). With a PAP, the researcher publicly documents the set of hypotheses that she intends to test prior to collecting the data. This method follows an approach used for decades in biostatistics

[1] It is also an issue in many observational studies, but it is difficult to establish when a researcher first had access to the data in an observational study, and establishing this timeline is critical to any method for limiting false discoveries.

[2] The researcher need not be dishonest; in practice, she herself may not recall all the tests that she has performed.

(Simes 1986; Horton and Smith 1999). Casey et al. (2012) established best practices and popularized the use of PAPs among empirical microeconomics using a case of a Community-Driven Development (CDD) program in Sierra Leone; in that context they prespecified a broad range of outcomes as potential tests.[3] Since that influential study, others have followed this approach, and grant funders (e.g., International Initiative for Impact Evaluation, or 3ie) are internalizing the importance of multiple inference and making a PAP a condition for funding.

Despite the track record of PAPs in other fields, serious issues arise with their application in economics. First, they discourage tests that may generate novel or unexpected findings, as including these tests reduces the power of all other tests in a properly specified PAP. Second, they restrict researchers' ability to learn from the data and build economic models informed by empirical results. Much of the analysis in economics proceeds in a sequential fashion. Conditional on one test rejecting, a researcher may conduct several more tests to understand the mechanisms underlying the rejection or test for further effects on other outcomes. Specifying an analysis plan that captures all possible paths by which an analysis may proceed becomes combinatorially impractical for all but the simplest cases (Coffman and Niederle 2015; Olken 2015). This issue is increasingly important as the field moves from experiments that evaluate a specific program or treatment to experiments that inform us about the mechanisms underlying an observed treatment effect or discriminate between different economic models (Card et al. 2011; Ludwig et al. 2011).

This paper develops a split-sample approach to avoiding false discoveries which may be used as either a complement or an alternative to a preanalysis plan. This approach withholds a fraction of the data from the researcher in a "confirmation sample." Researchers conduct exploratory analysis in the fraction of the data not withheld – the "exploratory sample" – and then register a simple analysis plan documenting the subset of hypotheses that they wish to validate in the confirmation sample. In the exploratory sample, researchers can analyze the sample in an unconstrained way, without need for an algorithm or documentation of the tests that are considered.

One advantage of this approach is that anticipation is unnecessary. In concurrent but indepen-

[3]While Casey et al. (2012) is arguably the highest profile example of a PAP in empirical microeconomics, it is not the earliest. For example, Neumark (2001) applied a prespecified research design to observational data from the Current Population Survey (CPS). To add credibility he submitted the research design to a journal prior to the publication of the relevant CPS data.

dent work, Fafchamps and Labonne (2017) propose a balanced version of this approach (i.e., one with equally sized exploratory and confirmation samples) and find that it performs well, relative to a PAP, when researchers identify many hypotheses that they were unable to anticipate or when expected *t*-statistics are very large.[4] However, the split sample's flexibility comes at a cost: the approach loses power relative to a full-sample PAP on hypotheses which were anticipated. For a balanced approach, this loss in power can be large.

We assess the potential of split-sample methods under two objective functions. First, we consider a researcher who maximizes rejections over a set of *ex ante* identical hypotheses. We demonstrate that a researcher with this objective function constructing a PAP would choose to include every hypothesis in the PAP. To develop the potential of the split-sample method, we propose and analyze a series of refinements – allocating a majority of the data to the confirmation sample, using one-sided tests in the confirmation sample, choosing thresholds for passing hypotheses to the confirmation sample, and optimally allocating type I error to hypotheses – that enable it to approach the power of a perfectly anticipatory PAP. In some ways, the optimal split-sample approach in this case looks similar to a PAP; only a small fraction of the data (15%) is allocated to the exploratory sample, and pass-on rules are generous (with optimal thresholds of $t > 0.2$), so that most hypotheses get tested in the confirmation sample. Thus, we conclude that if researchers have access to a large set of homogeneous hypotheses, then split-sample methods can be used at relatively low costs in terms of statistical power. The primary gains come from avoiding the need to prespecify and providing insurance against failure to perfectly anticipate every hypothesis of interest.

We contrast this benchmark case against an objective function where researchers have heterogeneous priors over the likelihood that different hypotheses reject, or different utility from rejecting particular hypotheses. In this case, split-sample methods can be used as a complement to a well-designed PAP. We demonstrate that when hypotheses are heterogeneous, the optimal PAP may not be exhaustive, as researchers would prefer to exclude hypotheses that have low probabilities of rejecting or that generate little value when rejecting. This objective function formalizes the intuition that low-prior but high-weight "surprises" may be excluded from a PAP.

We demonstrate that researchers with heterogeneity in priors or utility from rejecting specific

[4] As an additional contribution, Fafchamps and Labonne (2017) develop normative recommendations for integrating the balanced split-sample procedure into the submission and publication process.

hypotheses will optimally adopt a hybrid approach. The hybrid approach prespecifies hypotheses with high utility weights and high priors and then uses split-sample methods to identify additional hypotheses. We consider a broad set of candidate beliefs and pass-on rules to demonstrate that the optimal hybrid approach features large power gains over the optimal pure PAP, and we generate heuristics to guide researchers. Specifically, we recommend that researchers prespecify hypotheses with high weights and priors, that they utilize 35% of the data for the exploratory sample for the remaining hypotheses, and that they use an approximate threshold of $t > 1.6$ as a guideline for passing hypotheses on to the confirmation sample. As before, the researcher can use any analysis methods to identify hypotheses in the exploratory sample, including unrestricted data mining. If the researcher can apply prior knowledge, logical consistency, or economic theory to further restrict the set of passed-on hypotheses, these procedures will be even more powerful (assuming the applied knowledge is in fact related to the data generating process). This approach strongly recalls the recommendations of Olken (2015), who suggests prespecifying a few primary hypotheses – presumably those with high researcher priors or interest – and conducting secondary analysis on the remaining hypotheses. In this context, the split-sample method controls false discoveries even among the secondary hypotheses, addressing concerns over how to interpret this class of evidence.

As an application, we reconsider the CDD intervention studied by Casey et al. (2012). This application has several advantages: on top of being the seminal application which popularized PAPs among microeconomists, a number of features of the data collected as part of this intervention allow for a straightforward specification of hypotheses that researchers would have very likely identified to test using split-sample methods. We contrast a hybrid approach that searches over these hypotheses with the results identified in the pure PAP suggested by Casey et al. (2012). We conclude that a hybrid approach would have led to important differences in the qualitative and quantitative understanding of the effects of the CDD program.

The paper proceeds as follows. First, we consider the problem of a researcher with homogeneous hypotheses who wishes to reject as many hypotheses as possible. We discuss the optimal PAP in this context and then discuss the optimal split-sample strategy in the same context, solving for optimal exploratory sample shares, type I error allocation, and rules for testing hypotheses. We then compare power under the two approaches. Next, we consider the problem of a researcher with heterogeneous hypotheses. Here, we analytically identify optimal PAP and hybrid behavior in a

simplified problem, before conducting a large set of simulations to qualitatively assess the power gains from a hybrid approach and develop heuristics to guide construction of hybrid plans. Finally, we consider the CDD application in Casey et al. and assess the effects of the CDD program on public goods hardware and institution building under a variety of PAPs and hybrid plans. We conclude with recommendations for applied researchers.

2 Background

To structure the discussion, consider the case of a researcher who conducts a field experiment which assigns treatment, T, to a random fraction of the sample. For each participant i, she collects data on a set of H outcome variables, $\{Y_{i1}, Y_{i2}, ..., Y_{iH}\}$. These outcome variables generate H hypotheses, where the underlying relationship is

$$Y_{ih} = \beta_h T_i + \varepsilon_{ih}$$

The researcher wishes to test the null hypothesis $\mathbf{H}_h^0 : \beta_h = 0$ against the two-sided alternative $\mathbf{H}_h^A : \beta_h \neq 0$. Using the sample data, we can estimate the average treatment effect $\hat{\beta}_h$ and an accompanying standard error s.e.$(\hat{\beta}_h)$. These are used to form a *t*-statistic under the null hypothesis, $\hat{t}_h = \frac{\hat{\beta}_h - 0}{\text{s.e.}(\hat{\beta}_h)}$. Using the *t*-distribution with $N-1$ degrees of freedom, the researcher can find a critical value of $\text{t}_{\alpha/2}$.[5] If the estimated $\hat{t}_h$ falls above $\text{t}_{\alpha/2}$ or below $-\text{t}_{\alpha/2}$, we reject the null hypothesis $\mathbf{H}_h^0$ at the α significance level. As scientific convention, we often take $\alpha = 0.05$.

In most field experiments the implementation of the treatment is expensive, but measuring an additional outcome variable has low marginal cost. The set $\mathcal{H}$ enumerates all potential outcome variables Y_h where $h \in \mathcal{H}$ is associated with a hypothesis as described above, and $\mathcal{H}$ is often large.[6] From the set of potential hypotheses, the researcher selects a subset of hypotheses to test. This selection depends on the researcher's objective function.

We denote the benchmark objective function as the *Agnostic Evaluation Problem*. In this problem the researcher maximizes the expected total number of statistically significant treatment ef-

[5] Let $t \sim t_{N-1}(0,1)$ be distributed according to the centered *t*-distribution with $N-1$ degrees of freedom and standard deviation of 1. The probability of t falling anywhere above the critical value $\text{t}_{\alpha/2}$ or below $\text{t}_{\alpha/2}$ is α.

[6] In practice $\mathcal{H}$ may also include hypotheses related to treatment effect heterogeneity or alternative treatments. This possibility does not affect any of our results.

fects. The researcher selects a subset of hypotheses to test, $\mathcal{H}' \subseteq \mathcal{H}$, that solves[7]

$$\max_{\mathcal{H}' \in 2^{\mathcal{H}}} \mathbb{E}\left[\sum_{h \in \mathcal{H}'} \mathbb{I}\{|\hat{t}_h| > \mathrm{t}_{\alpha/2}\}\right] \tag{1}$$

This problem, which is analogous to maximizing statistical power, represents the case where the researcher wants to know which of the outcomes may be related to treatment. It is similar to the clinical trials case for which preanalysis plans were originally developed; in that case regulators want to know all of the relevant effects of a drug. It may also be representative of some policy evaluations; a leader thinking about implementing a complicated policy wants to know which of many outcomes she can expect to affect.

There is no constraint to the maximization problem above, so the maximizing subset, $\mathcal{H}^*$, is the subset of hypotheses with a positive probability of rejection. Since even true hypotheses reject at rate α for tests of the correct size, the maximizing subset is $\mathcal{H}^* = \mathcal{H}$, and the researcher tests for effects on every possible outcome. This solution naturally opens the door to false discoveries, and limiting these false discoveries is a critical issue in most empirical disciplines (Sterling 1959).

2.1 False Discovery Problem

The fundamental problem with testing every hypothesis in $\mathcal{H}$ is that in any hypothesis test there is a chance that the sample statistic falls in the rejection region, even if the null hypothesis is true. This false discovery problem leads to costly but ultimately futile future research as well as potentially dangerous policy. More broadly, it erodes the trust that the public has in the results that researchers find. Thus it is important to minimize the false rejection of true hypotheses, or the type-I error rate.[8]

Returning to the researcher's decision in Equation (1), in the worst-case scenario all the null hypotheses in $\mathcal{H}$ are true. Even though the study contains no false hypotheses, it still rejects $\alpha \cdot |\mathcal{H}|$ of the hypotheses in expectation. As an example, suppose 100 hypotheses are tested at

[7] Here $\mathbb{I}\{\cdot\}$ is the indicator function, equal to 1 if the condition $\{\cdot\}$ is true, and equal to 0 otherwise

[8] This paper is not the first to discuss the false discovery problem in the context of randomized experiments in economics or the general social sciences. For example, see Anderson (2008) and Fafchamps and Labonne (2017) for related discussions of these issues and techniques for controlling the type-I error rate.

a significance level of 0.05. Even if all 100 null hypotheses are true, we expect the study to (incorrectly) reject five of the null hypotheses, generating five significant findings.

To address this issue, multiplicity adjustments work to control the overall type-I error rate of the study. This error rate is either the probability that the study makes at least one incorrect rejection – the familywise error rate (FWER) – or the expected proportion of rejections that are incorrect – the false discovery rate (FDR). The simplest adjustment is the Bonferroni correction, which controls FWER. With the Bonferroni correction, we divide α by the number of hypotheses tested, in this case, $|\mathcal{H}'|$.[9] The researcher's problem becomes

$$\max_{\mathcal{H}' \in 2^{\mathcal{H}}} \mathbb{E}\left[\sum_{h \in \mathcal{H}'} \mathbb{I}\{|\hat{t}_h| > \mathrm{t}_{\alpha/2|\mathcal{H}'|}\}\right] = \sum_{h \in \mathcal{H}'} \mathbb{P}_{F_h}\left(|\hat{t}_h| > \mathrm{t}_{\alpha/2|\mathcal{H}'|}\right) \tag{2}$$

where $\mathrm{t}_{\alpha/2|\mathcal{H}'|}$ is the critical value above which a standard *t*-statistic has a probability of $\frac{\alpha}{2|\mathcal{H}'|}$ of falling and F_h is the researcher's prior over the coefficient corresponding to hypothesis h. For the moment we assume uninformative (uniform) priors, but we consider richer priors in Section 4.

The critical value $\mathrm{t}_{\alpha/2|\mathcal{H}'|}$ increases with $|\mathcal{H}'|$; for example, $\mathrm{t}_{\alpha/2|\mathcal{H}'|} = 3.49$ if $|\mathcal{H}'| = 100$. The more hypotheses the researcher tests, the higher the critical value becomes, and the lower the probability of rejecting a given hypothesis becomes. Honest disclosure of $|\mathcal{H}'|$ thus goes against the researcher's incentives. Instead, to increase rejections, she should test every hypothesis in $\mathcal{H}$ but report a subset, $\mathcal{H}_r$, that contains only hypotheses with large *t*-statistics. In many cases $|\mathcal{H}_r| << |\mathcal{H}|$, and the multiplicity adjustment for each test becomes much less severe. Multiplicity adjustments are thus only effective when researchers can credibly communicate the number of hypotheses they have tested.

Historically, biostatistics has taken a strong interest in controlling false discoveries. This interest arises from the large financial incentives and potential welfare impacts related to false discoveries in clinical trials and the massive number of hypotheses tested in many genomics studies. It has thus become standard practice in the medical literature that clinical trials should register analysis

[9]More sophisticated adjustments exist that minimize the power reduction associated with additional tests. Nevertheless, it is inherent in the control of FWER, or the probability of making any type I error (i.e., false rejection), that adding more tests requires more stringent adjustment of *p*-values. Otherwise, the probability of making at least one error rises. The only case in which FWER would not rise would be the case in which the new test is perfectly correlated with one or more of the existing tests. In this case the new test does not represent new information.

plans prior to enrolling patients (De Angelis et al. 2004). Recently, empirical microeconomics has begun to adopt this model for field experiments in the form of preanalysis plans.

2.2 Preanalysis Plans

One way to credibly communicate the number of hypotheses tested is to file a preanalysis plan. A generic preanalysis plan describes in detail the analyses that a researcher intends to perform. An effective PAP requires that the researcher upload it to a public site, such as the AEA RCT Registry, prior to collecting her data. With a publicly registered PAP, the researcher "ties her hands" with respect to the analysis, thus preventing "cherry picking" of results or "p-hacking." Formally, readers can be confident that the reported set of tested hypotheses, $\mathcal{H}_r$, represents the true set of tested hypotheses, $\mathcal{H}'$.

In addition to specifying the hypotheses to be tested, an effective PAP must specify some form of multiplicity adjustment for statistical tests (assuming it tests more than one hypothesis). Without any multiplicity adjustment, the researcher's optimal strategy is to include as many hypotheses as possible, even those that may be very unlikely or of little interest, since the option value of including any given hypothesis test in the PAP is weakly positive. The gating factors on the PAP thus become the researcher's creativity and value of time.

Multiplicity adjustments formalize the implicit tradeoff that motivates PAPs to begin with. Each additional test has option value in that it may reject and be of interest, but it also carries an explicit cost in that it reduces the power of other included tests. A researcher solving the multiplicity-adjusted Agnostic Evaluation Problem, Equation (2), will nevertheless find that the optimal PAP tests *all* hypotheses in $\mathcal{H}$, so $\mathcal{H}' = \mathcal{H}$ (see Appendix A1 for proof). Intuitively, if a researcher weights all hypotheses equally and believes that all are equally likely reject, then she has no way to discriminate between hypotheses at the PAP stage, and the loss of power on existing hypotheses from adding another is dominated by the chance that that additional hypothesis rejects.

2.3 Split-Sample Methods

We discuss split-sample analyses as an alternative mechanism for controlling false discoveries. In a split-sample analysis, a researcher conducts analyses on a fraction of the entire sample –

the exploratory sample – and then validates her findings using the remainder of the data – the confirmation sample. These methods date back at least eight decades (Larson 1931; Stone 1974; Snee 1977), and they play a fundamental role in machine learning methods, where the out-of-sample performance of predictors is tested in "hold-out samples" to constrain overfitting.

We define a split-sample method as encompassing three key components: a sample split, a procedure for passing tests, and an analysis plan. To facilitate exposition, consider the following "balanced" split-sample method:[10]

1. Draw a random sample of share $s = 0.5$ of the data. Label this sample as the exploratory sample. Label the remaining data as the confirmation sample.

2. Run as many tests in the exploratory sample as are of interest. Let t_h^e represent the *t*-statistic for hypothesis h in the exploratory sample. Record the *H* tests that reject at the $\alpha = 0.05$ level; for *t*-statistics with high degrees of freedom this implies all tests with $|t_h^e| > \tau = 1.96$.

3. File a brief analysis plan specifying the *H* tests passed to the confirmation sample, along with a multiplicity adjustment for those *H* tests. Applying the Bonferroni procedure to control FWER implies a critical value of $\alpha = 0.05/H$ for each test. Let $t_{\frac{0.025}{H}}$ represent the *t* critical value corresponding to a two-sided *t*-test with size $\alpha = 0.05/H$.

4. Execute the analysis plan in the confirmation sample. Let t_h^c represent the *t*-statistic for hypothesis h in the confirmation sample. Reject all hypotheses in the analysis plan with $|t_h^c| > t_{\frac{0.025}{H}}$ in the confirmation sample.

Two key problems arise with the application of split-sample methods in the context of hypothesis testing. First, there is a credibility issue: How can the researcher credibly remain blind to the confirmation sample if she herself splits the data? This issue is addressable in many field experiments through the common practice of subcontracting of data collection. The data collection contract can specify that the researcher only receives the exploratory sample initially, and then receives the confirmation sample after filing the analysis plan containing the hypotheses she wishes

[10]The procedure is balanced in that it explicitly assigns the same share of data to the exploratory and confirmation samples and implicitly assigns the same amount of type I error to the exploratory stage (for a single hypothesis) and the confirmation stage (across all hypotheses).

to validate. It may also be easily addressable in larger survey data for which only a subsample is made readily available to researchers, such as census data.

The second issue is statistical power. In concurrent work, Fafchamps and Labonne (2017) independently propose this balanced split-sample procedure and analytically assess its power relative to a PAP with a set up similar to that described in Section 2.2. They find that a balanced split-sample procedure outperforms several "unbalanced" split-sample procedures and demonstrate that if the researcher can identify enough additional hypotheses to test by working with the exploratory sample, then a balanced split-sample method could match or exceed the power of a full-sample PAP.[11] This result highlights an important advantage to using a split sample: the lack of preregistration allows researchers to identify additional hypotheses after looking at part of the data, while still controlling false discoveries.

This advantage comes at a cost however. The balanced split-sample procedure exhibits substantial power losses relative to a full-sample PAP, particularly for moderate effect sizes. For example, suppose that the researcher considers testing all hypotheses in $\mathcal{H}$ using either a PAP or a balanced split-sample method. If $|\mathcal{H}| = 20$ and $\mathbb{E}[t_h] = 2.5$, which is close to the median *t*-statistic in a sample of well-published field experiments described below, then the power of a full-sample PAP is approximately 2.2 times higher than the power of the balanced split-sample method. Appendix Figure A1 confirms this power differential for a wider range of *t*-statistics. A researcher anticipating effect sizes of these magnitudes would need to discover many new hypotheses in the data to justify these power losses.

Power will be a primary concern for researchers considering the split-sample method, particularly if they can anticipate many of the hypotheses that they will want to test. For the proceeding discussion, we assume an anticipation rate of 100%.[12] While the relative power of the split-sample method increases for lower anticipation rates, at anticipation rates near 100% the balanced split-sample method will be unattractive to most researchers absent significant methodological improvements.

[11] Fafchamps and Labonne (2017) also independently suggest that there may be opportunities to combine PAP methods with split-sample methods, but do not develop or evaluate this suggestion. We formally develop this concept, demonstrate its utility, and develop a set of heuristics for effective use of this procedure in Section 4.

[12] The anticipation rate is closely related to the ψ parameter that Fafchamps and Labonne (2017) define; in their case ψ represents "the likelihood that variables for which the null-hypothesis is non-true are included in the PAP."

3 Split Sample Improvements

We briefly describe several methodological improvements that significantly boost the power of the split-sample method. One of these improvements – one-sided tests – leverages information from the exploratory sample to optimize tests in the confirmation sample. The other two improvements – reducing the exploratory sample share, s, and varying the threshold, τ, for passing on tests – incorporate the nonlinear relationship between sample size and statistical power. We also experiment with optimally allocating type I error in the confirmation sample, but find that this optimization has a minimal impact on total rejections.

3.1 One-sided Tests

Incorporating information from the exploratory sample is fundamental to improving the performance of tests in the confirmation sample. The most obvious piece of information that the researcher may learn in the exploratory sample is the direction of the effect in question. Incorporating this information facilitates a one-sided test in the confirmation sample, which improves power by a substantial margin. For example, Appendix Figure A2 plots, by $\mathbb{E}[t_h]$, the expected full-sample *t*-statistic for hypothesis h, the power of the split-sample method to reject false hypothesis h when using one-sided and two-sided tests. This figure assumes that the researcher tests h with 19 other null hypotheses ($|\mathcal{H}| = 20$). Appendix Figure A2 reveals that if $\mathbb{E}[t_h] = 2.5$, then the split sample power when using one-sided tests in the confirmation sample is approximately 34% higher than the split sample power when using two-sided tests.

The use of one-sided tests typically raises practical and philosophical questions. How can we verify that the researcher specified the test's direction *ex ante*, rather than after observing the sign of the coefficient? Are we prepared to ignore highly significant effects that go in the unexpected direction? In the split sample case, however, most of these issues do not apply. Since the researcher files a simple analysis plan prior to accessing the confirmation sample, we know that she specified the direction of the test prior to observing the estimate. Since the researcher only passes on tests that have a reasonable chance of validating, the chance of finding a highly significant coefficient that goes in the opposite direction in the confirmation sample is extremely small.[13]

[13] Such a finding would call into question whether the sample split were truly random.

3.2 Exploratory Sample Share and Thresholds for Passing Tests

The balanced split-sample approach allocates half the data to the exploratory sample and passes on tests that achieve the conventional significance threshold of $\alpha = 0.05$. This approach has the appeal of symmetry; exploratory and confirmation samples are of equal size, and the coefficient and test statistic distributions in the two samples are identical. However, there is a fundamental asymmetry between the researcher's goals in the exploratory sample and her goals in the confirmation sample. In the exploratory sample she hopes to learn about parameter values underlying hypotheses, while in the confirmation sample she hopes to reject the hypotheses that she has passed. It is thus not obvious that the exploratory and confirmation samples should be of equal size, or that the threshold for passing a hypothesis to the confirmation sample should be set at the conventional significance level. Furthermore, these two choices influence each other. Once the exploratory and confirmation samples are of unequal size, the test statistic distributions in the two samples differ, and it becomes implausible that the optimal threshold for passing a hypothesis corresponds to the test statistic achieving the $\alpha = 0.05$ significance level in the exploratory sample.

In Appendix A2 we consider a simplified version of the research problem that allows us to characterize the optimal exploratory share, s and pass-on thresholds, τ, analytically. In this context we assume that we test H hypotheses, one of which is false, and the remaining $H - 1$ of which are true. Figure 1 plots the optimal exploratory sample share for different thresholds for passing on a hypothesis (expressed as the observed exploratory sample *t*-statistic for that hypothesis). The figure assumes a case in which the researcher tests one false hypothesis with $\mathbb{E}[t_h] = 2.5$ (in the full sample) and 19 other null hypotheses. Intuitively, we expect that as the *t*-threshold τ increases, the first stage becomes a more difficult hurdle to pass, and so optimally one would allocate more of the data to passing that hurdle. Figure 1 confirms that intuition; as the threshold for passing a hypothesis increases, the optimal exploratory sample share increases as well.[14]

In this simple example, overall power is maximized when the researcher passes all hypotheses to the confirmation sample (i.e. sets a threshold of $\tau = 0$) and allocates 10% of the data to the exploratory sample.[15] This result does not vary strongly with $\mathbb{E}[t_h]$. In general, weaker thresholds

[14] In Appendix A2 we verify analytically that optimal exploratory shares are increasing in the threshold, τ, in this environment

[15] Despite passing all hypotheses to the confirmation sample, it is still optimal to allocate positive observations to

with smaller exploratory sample shares achieve better performance. For example, using a threshold of $\tau = 1$ and an exploratory sample share of $s = 0.26$ increases power by 39% in our sample case relative to a threshold of $\tau = 2$ and an exploratory sample share of $s = 0.50$.[16]

3.3 Type I Error Allocation

In addition to revealing the likely sign of a coefficient β_h, the exploratory sample also reveals information about the magnitude of β_h. Using this information researchers could calculate the probability that hypothesis h will reject in the confirmation sample and weigh that gain against the implied power loss for other hypothesis tests; implicitly they do this when choosing not to pass on hypotheses with *t*-statistics less than τ in absolute value. FWER control, however, does not require that all tests have the same size. It only requires that the total probability of making any type I error be maintained at $\alpha = 0.05$ or less. Researchers could thus choose to apportion type I error differentially between hypotheses – one hypothesis might receive $\alpha = 0.03$ type I error, facing a lower *t* critical value in the confirmation sample, while another might receive $\alpha = 0.01$ type I error, facing a higher *t* critical value in the confirmation sample.

In Appendix A3 we solve the Agnostic Evaluation Problem under the assumption that the researcher knows the full data generating process for *t*-statistics after reviewing the exploratory data. The distributions of *t*-statistics depend only on the corresponding coefficient values, so the researcher substitutes coefficient estimates from the exploratory sample for the unknown β_h. Blindly substituting estimated values for true values, however, generates a regression-to-the-mean problem, as large values of $\hat{\beta}_h$ tend to be large both because the true β_h is non-zero and because there has been a shock in the same direction as the coefficient. We thus apply an Empirical Bayes estimator

the exploratory sample, because the researcher needs some information to execute one-sided tests in the confirmation sample. When setting a threshold of $\tau = 0$ and applying two-sided tests, the optimal allocation of data to the exploratory sample is 0%, and the researcher has reproduced the PAP method.

[16]This set of conclusions diverges sharply from Fafchamps and Labonne (2017), who also compare the balanced split sample to several candidate variations in sample share and pass-on thresholds. Specifically, they confirm that an (s, τ) combination of (0.5, 1.96) is more powerful than (s, τ) combinations of (0.5, 1.65), (0.5, 1.28), (0.5, 1.04), (0.3, 1.96), and (0.7, 1.96), supporting the balanced split sample approach. Our differing conclusions come from additionally considering parameter combinations with low s and low τ simultaneously, motivated by the discussion above.

to shrink the coefficient estimates towards zero.

Solving this problem and applying the Empirical Bayes estimator to the exploratory sample coefficients does improve power in many cases. The power gains, however, are minimal. For example, when comparing a split-sample method that uses optimized type I error allocation to one that uses a lenient threshold for passing hypotheses to the confirmation sample ($\tau = 0.2$), we find that average power is identical (within 0.1%) across all combinations of parameter values that we consider and only 1.8% higher across more empirically relevant combinations of parameter values (e.g., large numbers of hypotheses tested, modest effect sizes, and small numbers of false hypotheses).[17] We expect that most applied researchers will not find the added complexity of optimizing type I error allocation to be worth the modest power gains.

3.4 Statistical Power Simulations

When combining our split-sample improvements with FWER or FDR control procedures more sophisticated than the Bonferroni correction, it is impractical to analytically calculate power. We describe a series of Monte Carlo simulations that establish the power of our improved split-sample methods relative to a full-sample PAP under a variety of scenarios. Power depends on some parameters that the researcher has direct control over (number of tests, sample split, and the threshold for passing tests), some that she has limited control over (sample size), and others that she has no control over (share of hypotheses that are false, effect sizes, and inter-test correlation structure).

Effect size and sample size are fundamental to statistical power. These two factors interact to generate the sampling distribution of the test statistic, which determines power. The question of what *t*-statistics a researcher might expect to estimate thus informs her expected power. To limit the parameter space of interest we conducted a literature review of field experiments with the goal of determining the empirical distribution of published *t*-statistics.

[17] We report these numbers for an exploratory sample share of $s = 0.15$, which, on average, outperforms other exploratory sample shares in our simulations.

3.4.1 Empirical Distribution of *t*-statistics

Our sample consists of papers on field experiments published from 2013 to 2015 in a set of ten general-interest economics journals.[18] These criteria generate a sample of 61 papers. Using this sample we recorded the *t*-statistic for each paper's featured result. The median *t*-statistic is 2.6, the 10th percentile *t*-statistic is 1.7, and the 90th percentile *t*-statistic is 7.0. Due to the likelihood of publication bias and *p*-hacking (Franco et al. 2014), we interpret this distribution as an overestimate of the *ex ante t*-statistic distribution that a researcher should expect when beginning a typical field experiment. Nevertheless, the results imply that most researchers should (at best) expect statistical power that corresponds to mean effect sizes of 0.2 to 0.3 in our power simulations, and we focus our discussion on effect sizes in this range.[19]

3.5 Simulating Split Sample Performance

To assess the performance of a full-sample preanalysis plan against split-sample methods across a range of potential studies, we set up the following simulation environment. First, there are H hypotheses, of which H_1 are false. False hypotheses have a normalized mean effect size of μ, where the data-generating process (DGP) for hypothesis h draws a coefficient β_h from a normal distribution with mean μ and standard deviation $\mu/2$.[20] The remaining $H - H_1$ true hypotheses have a DGP with $\beta_h = 0$. Let the $H \times 1$ column vector $\boldsymbol{\beta}$ represent the H coefficients. To test for robustness in different environments we vary H, H_1, and μ across simulations (see Table 1).

To gauge the performance of the PAP, we draw an $H \times 1$ column vector of coefficient estimates,

[18] We defined a paper as involving field experiments if it mentioned "field experiment" in its abstract or listed JEL Code C93. The ten surveyed journals are the *American Economic Journal: Applied Economics*, *American Economic Journal: Economic Policy*, *American Economic Review*, *Econometrica*, *Economic Journal*, *Journal of the European Economic Association*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*, and *Review of Economics and Statistics.*

[19] Our simulations assume $N = 500$. For this sample size, an effect size of 0.2 generates an expected full-sample *t*-statistic of 2.2, and an effect size of 0.3 generates an expected full-sample *t*-statistic of 3.4. Larger samples can of course provide equivalent power at smaller effect sizes, but the empirical distribution of *t*-statistics already incorporates the influences of both typical effect sizes and typical sample sizes on statistical power.

[20] To avoid generating "false" coefficients that are arbitrarily close to zero we truncate the distribution at 0.1μ. In practice this implies a mean coefficient magnitude of 1.06μ.

$\hat{\boldsymbol{\beta}}$, from a normal distribution centered at $\boldsymbol{\beta}$ with a standard deviation equal to the standard error of a difference in means estimator ($2\sigma/\sqrt{N}$). We form an $H \times 1$ vector of *t*-statistics from $\hat{\boldsymbol{\beta}}$. We then test which hypotheses reject, allocating $\alpha = 0.05$ FWER across all H hypotheses and performing the Holm sharpening procedure (uncorrelated tests) or a FWER procedure that constructs multidimensional rejection regions (correlated tests). We sum the number of rejections, store that number, and repeat 500 times. We report the mean numbers of rejections across these 500 iterations.

To assess the split-sample procedure, we begin by simulating the coefficient estimates in the exploratory and confirmation samples. Using the vector of simulated coefficients from before, $\boldsymbol{\beta}$, we choose the share of the data going to the exploratory sample, s, and draw two vectors: $\hat{\boldsymbol{\beta}}^e(s)$, a vector of estimated $\hat{\beta}$s from the exploratory sample, and $\hat{\boldsymbol{\beta}}^c(s)$, a vector of $\hat{\beta}$s which would be estimated in the confirmation stage.[21] Both of these vectors are centered at $\boldsymbol{\beta}$, but their sampling variances differ unless $s = 0.5$. We construct *t*-statistics for each coefficient in $\hat{\boldsymbol{\beta}}^e(s)$ and $\hat{\boldsymbol{\beta}}^c(s)$ and perform two sets of split-sample analyses. First, we apply simple threshold rules, where all hypotheses with $|t_h^e| > \tau$ are passed to the confirmation stage and receive equal type I error at that stage. We contrast this approach with an "optimized" approach that allocates type I error to hypothesis tests in the confirmation sample using the Empirical Bayes method described in Section A3. With both approaches we apply one-sided tests at the confirmation stage.

Figure 2 reports the relative performance of the split-sample method against the performance of the full-sample preanalysis plan. As is apparent, the PAP always outperforms the optimized split-sample approach on this objective function. However, using one-sided tests with the optimized FWER allocation comes close in terms of power. Across different values of H, H_1, and μ the optimized split-sample method achieves an average of 92% of the power of the PAP when employing an exploratory sample fraction of $s = 0.15$. If researchers are concerned that they may fail to correctly anticipate all potential hypotheses of interest, or if the value of time prevents a perfectly crafted PAP, this approach may be attractive.

Figure 2 also reports the performance of two simpler threshold-based rules that pass all hypotheses with $|t_h^e| > \tau = 0.2$ or $|t_h^e| > \tau = 1$ to the confirmation sample and the balanced split-

[21] To reduce noise in the simulations, we form the full-sample coefficient estimate, $\hat{\boldsymbol{\beta}}$, as a weighted sum of $\hat{\boldsymbol{\beta}}^e(s)$ and $\hat{\boldsymbol{\beta}}^c(s)$. This simulates a real-world environment in which the full sample is simply the union of the exploratory and confirmation samples.

sample method. Consistent with our analytic results in Section 3.2, we find that combining a small exploratory sample fraction (approximately 15%) with a permissive threshold that passes virtually all hypothesis tests maximizes power.[22] The power difference between the optimized split-sample approach and a simpler approach that passes hypotheses with $|t_h^e| > \tau = 0.2$ is visually indistinguishable, and both approaches outperform the balanced split-sample method by a sizable margin. The approach that passes hypotheses with $|t_h^e| > \tau = 1$ falls between the optimized approach and the balanced split-sample method in power.

In additional simulations we evaluate the performance of simple threshold-based rules when test statistics are correlated or when we control the false discovery rate (FDR) rather than FWER. Correlated test statistics require less stringent adjustments to control FWER at a given level because they decrease the effective number of independent tests.[23] As expected, we find that introducing correlation between test statistics improves power for all procedures, but the optimal choices of threshold and exploratory sample share are not meaningfully changed.[24]

False discovery rate control is a popular alternative to FWER control; the FDR represents the proportion of rejections that are type I errors (i.e., false discoveries). FWER control restricts the probability of making *any* type I error, but FDR control trades off a small number of false rejections for large numbers of correct rejections. FDR control has become prominent in the biostatistics literature, and in our simulations it yields, as expected, greater power than FWER control. The optimal choices of threshold and exploratory sample share, however, are not meaningfully changed.[25]

[22] Appendix Figure A3 reports power when all hypotheses with $|t_h^e| > \tau$ are passed on to the confirmation sample, for different values of τ and s (the exploratory sample share). This figure assumes a mean effect size of $\mu = 0.3$, which corresponds to an expected full-sample *t*-statistic of 3.4. Smaller or larger values of μ also generate contour plots with qualitatively similar patterns.

[23] In an extreme case, if all test statistics are perfectly correlated, then the researcher is actually performing only one test, and no multiplicity adjustment is necessary.

[24] To run these simulations we generate positively correlated test statistics. Most FWER control procedures that incorporate dependence between test statistics, such as the free step-down resampling method or the step-wise method in Romano and Wolf (2005), rely on resampling to determine the correlation structure. Resampling is undesirable in our simulations for both coding and computational reasons, so we instead developed a rejection-region FWER control method in the spirit of Romano and Wolf (2005) that leverages the known correlation structure of our DGP.

[25] To run these simulations we apply the adaptive step-up FDR control procedure from Benjamini et al. (2006) rather than the Holm FWER control sharpening procedure.

4 Hybrid Approaches

Our methodological improvements dramatically increase the power of the split-sample method. Nevertheless, even an enhanced split-sample approach still falls short of the power of a full-sample preanalysis plan. Furthermore, the optimal exploratory sample share is often low – e.g., 15% – and the thresholds for passing hypotheses to the confirmation stage are generally lenient. These results imply that researchers are not learning much from the small exploratory sample and that there is minimal screening of hypotheses. In essence, the optimal split-sample approach attempts to preserve most of the power of a full-sample PAP while retaining the ability to discover potential hypotheses using a small sample of exploratory data.

In this section we consider hybrid approaches that combine the power of the full-sample PAP with the flexibility of a split-sample approach. To motivate the hybrid approach, consider a richer (less agnostic) version of the multiplicity-adjusted Agnostic Evaluation Problem,

$$\max_{\mathcal{H}' \in 2^{\mathcal{H}}} \sum_{h \in \mathcal{H}'} u_h \mathbb{P}_{F_h} \left(|\hat{t}_h| > \mathrm{t}_{\alpha/2|\mathcal{H}'|} \right)$$

where u_h represents the utility that the researcher gets from rejecting hypothesis h. F_h represents the researcher's prior over the coefficient corresponding to hypothesis h, and we now assume that F_h may vary across hypotheses. Since researchers are unlikely to develop extremely detailed priors over $|\mathcal{H}|$ coefficients, we simplify the problem to one in which priors are over a Bernoulli distribution representing whether hypothesis h is false, with the coefficient β_h fixed at a given effect size b if hypothesis h is false. Without loss of generality assume $b > 0$. This generates a simplified maximization problem,

$$\max_{\mathcal{H}' \in 2^{\mathcal{H}}} \sum_{h \in \mathcal{H}'} u_h p_h \mathbb{P} \left(\hat{t}_h > \mathrm{t}_{\alpha/2|\mathcal{H}'|} \mid \beta_h = b \right)$$

where p_h represents the researcher's prior that hypothesis h is false. For expositional ease the objective function assumes that the researcher only values correct rejections, but allowing the researcher to also value false rejections does not change any of our conclusions.[26]

[26] Since we constrain the probability of *any* type I error to fall below α, the contribution of false rejections to the objective function is trivial in magnitude unless all hypotheses are true, in which case the only possible rejections are false rejections.

With heterogeneous utility weights or priors, the optimal "pure" PAP – i.e., a PAP that is not part of a hybrid approach – may now test fewer than $|\mathcal{H}|$ hypotheses. To see this, consider the gains and losses from adding hypothesis $h \notin \mathcal{H}'$ to a PAP testing the set of hypotheses $\mathcal{H}'$.

$$\text{Gain from adding } h\text{:} \quad u_h p_h \mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/(2|\mathcal{H}'|+1)} \mid \beta_h = b\right) \tag{3}$$

$$\text{Loss from adding } h\text{:} \quad \sum_{j \in \mathcal{H}'} u_j p_j \left[\mathbb{P}\left(\hat{t}_j > \mathrm{t}_{\alpha/2|\mathcal{H}'|} \mid \beta_h = b\right) - \mathbb{P}\left(\hat{t}_j > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_h = b\right)\right] \tag{4}$$

If u_h or p_h is close to zero – that is, if the researcher gets little utility from rejecting hypothesis h or believes there is little chance that hypothesis h is false – then Equation (3) reveals that there is little gain to including h in the PAP. In that case the loss in power to reject other hypotheses, represented by Equation (4), dominates the gain, and the researcher is better off excluding h from her PAP. More generally, if u_h and p_h are small relative to the utility weights and priors on other hypotheses, the researcher should tend towards excluding h.

These results imply that a PAP writer should carefully consider which tests she values most or believes are most likely to reject. It is not clear that the median PAP writer has fully internalized this tradeoff. In a sample of PAPs that we collected in 2014 from the AEA RCT Registry, for example, the median PAP contained 128 tests. Nevertheless, limiting a PAP to hypotheses with high utility weights or priors forecloses the possibility of novel or unexpected discoveries since, by definition, an unexpected discovery is one with a low prior. A solution suggested by Olken (2015) suggests pre-specifying only a few primary hypotheses and using a secondary analysis, which foregoes the control of false discoveries, to identify these potential surprises. Of course, foregoing the control of false discoveries may weaken the credibility of these novel results.

To address these concerns, we consider hybrid procedures that combine smaller PAPs with split-sample methods. Hypotheses with high priors or weights go in the PAP, where they can leverage the full sample for maximum power. The remaining hypotheses can be tested in the exploratory sample and, at the researcher's discretion, passed on to the confirmation sample. This setup controls the number of tests, and thus preserves power for the most important hypotheses, while still retaining flexibility to explore other hypotheses and controlling false discoveries for all tested hypotheses.

In constructing a hybrid plan the researcher faces the question of which hypotheses to place in the PAP and which to test in the split sample. The researcher's objective remains maximizing

(weighted) total rejections. We can represent her problem as

$$\begin{aligned} &\max_{\mathcal{H}_p \in 2^{\mathcal{H}}} \sum_{h \in \mathcal{H}_p} u_h p_h \mathbb{E}_{\mathcal{H}'} \left[\mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/2|\mathcal{H}'|} \mid \beta_h = b, \mathcal{H}' \right) \right] \\ &+ \sum_{h \notin \mathcal{H}_p} u_h p_h \mathbb{P}\left(\hat{t}^e_h > \tau \mid \beta_h = b \right) \mathbb{E}_{\mathcal{H}'} \left[\mathbb{P}\left(\hat{t}^c_h > \mathrm{t}_{\alpha/|\mathcal{H}'|} \mid \beta_h = b, \mathcal{H}' \right) \right] \end{aligned} \tag{5}$$

where $\mathcal{H}_p$ represents the set of hypotheses placed in the PAP of the hybrid plan and $\mathcal{H}'$ represents the total set of hypotheses tested (i.e., the union of $\mathcal{H}_p$ and the set of hypotheses carried to the confirmation stage; in a pure PAP, $\mathcal{H}_p = \mathcal{H}'$). The first line in Equation (5) represents the expected number of weighted rejections in the PAP portion of the hybrid plan, and the second line represents the expected number of weighted rejections in the split-sample portion of the hybrid plan.

The analytic solution to this problem is complicated by the fact that $\mathcal{H}'$ is itself a random variable since it depends on how many hypotheses cross to the confirmation stage. Nevertheless, we can derive comparative statics to guide the researcher in constructing a hybrid plan. First, define

$$\begin{aligned} &\zeta_j(\mathcal{H}_p, \mathcal{H}') = \mathbb{I}\{j \in \mathcal{H}_p\} \big(\mathbb{P}\left(\hat{t}_j > \mathrm{t}_{\alpha/2|\mathcal{H}'|} \mid \beta_j = b, \mathcal{H}' \right) - \mathbb{P}\left(\hat{t}_j > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_j = b, \mathcal{H}' \right) \big) \\ &+ \mathbb{I}\{j \notin \mathcal{H}_p\} \mathbb{P}\left(\hat{t}^e_j > \tau \mid \beta_j = b \right) \big(\mathbb{P}\left(\hat{t}^c_j > \mathrm{t}_{\alpha/|\mathcal{H}'|} \mid \beta_j = b, \mathcal{H}' \right) - \mathbb{P}\left(\hat{t}^c_j > \mathrm{t}_{\alpha/(|\mathcal{H}'|+1)} \mid \beta_j = b, \mathcal{H}' \right) \big). \end{aligned}$$

ζ_j represents the loss in power for rejecting hypothesis j when the researcher adds one more test. For comparative statics purposes, notice that $\zeta_j(\mathcal{H}_p, \mathcal{H}') > 0$ and that it does not depend on u_h or p_h.

With a hybrid plan, the researcher must consider which hypotheses are suitable under three constraints. First, the net benefits of adding a hypothesis to the prespecified portion of the hybrid plan, compared to excluding the hypothesis altogether, are given by:

$$\begin{aligned} &\text{Gain from adding } h \text{ to prespecified portion:} \quad u_h p_h \mathbb{E}_{\mathcal{H}'} \left[\mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}' \right) \right] \\ &\text{Loss from adding } h \text{ to prespecified portion:} \quad \mathbb{E}_{\mathcal{H}'} \big[\sum_{j \neq h} u_j p_j \zeta_j(\mathcal{H}_p, \mathcal{H}') \big] \end{aligned}$$

As before, this suggests that a researcher using a hybrid plan will be willing to include hypotheses that have $u_h p_h$ above a critical threshold. With a hybrid plan, a researcher also has the option of testing a hypothesis under the split-sample portion of the plan. The net benefits of adding a hypothesis to the split-sample portion of the hybrid plan, compared to excluding the hypothesis

altogether, are given by:

Gain from adding h to split-sample portion: (6)

$$u_h p_h \mathbb{E}_{\mathcal{H}'}\left[\mathbb{P}\left(\hat{t}_h^e > \tau \mid \beta_h = b\right)\mathbb{P}\left(\hat{t}_h^c > \mathrm{t}_{\alpha/(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right)\right]$$

Loss from adding h to split-sample portion: (7)

$$\left[p_h\big(\mathbb{P}\left(|\hat{t}_h^e| > \tau \mid \beta_h = b\right) - \mathbb{P}\left(|t| > \tau\right)\big) + \mathbb{P}\left(|t| > \tau\right)\right] \cdot \mathbb{E}_{\mathcal{H}'}\Big[\sum_{j \neq h} u_j p_j \zeta_j(\mathcal{H}_p, \mathcal{H}')\Big]$$

The relevant parameters determining whether a hypothesis is worth testing in the split sample are similar, but asymmetric unlike in the prespecified case considered earlier. Once again, u_h only appears in Equation (6), so hypotheses with higher u_h will be tested in the split sample. However, p_h now appears in both expressions. Dividing through by p_h, it is clear that p_h disappears from the benefits side of the equation. On the loss side of the equation, it remains in one place; $\mathbb{P}\left(|t| > \tau\right)$ is now divided by p_h. In other words, the role of p_h for a hypothesis tested in the split sample is to change the relative probability that the additional hypothesis is a false hypothesis, with associated $u_h p_h$ benefits, or a true hypothesis, with no benefits at all. This means that the loss side of the expression is decreasing in p_h, and hypotheses with high p_h will be more likely to be tested in the split sample, rather than omitted, particularly when the split sample specifies a low threshold for passing tests, τ.[27]

These two inequalities are sufficient for learning about the optimality of hybrid plans:

Proposition 1. *Suppose a researcher seeks to maximize the objective function $\sum_h u_h p_h R_h$, where R_h is an indicator for rejecting hypothesis h. If there is an interior solution to the problem of the pure PAP, and if available hypotheses are sufficiently dense in $u_h p_h$ to guarantee the existence of a marginal hypothesis, then there exist hybrid plans where at least one hypothesis is tested by split-sample search which are strictly more powerful than any pure PAP.*

Proofs are in Appendix A4.

In principle there exist hypotheses with (u_h, p_h) which the researcher would be willing to test in the split sample but not in the prespecified part of the hybrid, hypotheses which the researcher

[27]The intuition is that for low values of τ, any hypothesis is likely to pass to the confirmation stage, so the expected loss is approximately identical regardless of whether the hypothesis is true or false. In that case the researcher only wants to test hypotheses that are reasonably likely to be false.

would be willing to test in the prespecified portion of the hybrid but not in the split-sample portion of the hybrid,[28] hypotheses which the researcher would be not willing to test in either part of the procedure, and hypotheses which the researcher would be willing to test in both parts.

For the last set of hypotheses, where either approach leads to positive net benefits, we must consider which hypotheses the researcher should prespecify. The net benefits of moving a hypothesis from the split-sample portion of a hybrid plan to the prespecified portion are given by:

Gain from adding h to PAP portion: (8)

$$u_h p_h \mathbb{E}_{\mathcal{H}'}\left[\mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right) - \mathbb{P}\left(\hat{t}_h^e > \tau \mid \beta_h = b\right)\mathbb{P}\left(\hat{t}_h^c > \mathrm{t}_{\alpha/(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right)\right]$$

Loss from adding h to PAP portion: (9)

$$\left(1 - p_h\big(\mathbb{P}\left(|\hat{t}_h^e| > \tau \mid \beta_h = b\right) - \mathbb{P}\left(|t| > \tau\right)\big) - \mathbb{P}\left(|t| > \tau\right)\right) \cdot \mathbb{E}_{\mathcal{H}'}\Big[\sum_{j \neq h} u_j p_j \zeta_j(\mathcal{H}_p, \mathcal{H}')\Big]$$

Differencing Equations (8) and (9) and differentiating with respect to u_h reveals that as u_h increases, the net benefits from moving a hypothesis to the prespecified portion increase by

$$\frac{\partial}{\partial u_h} = p_h \mathbb{E}_{\mathcal{H}'}\left[\mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right) - \mathbb{P}\left(\hat{t}_h^e > \tau \mid \beta_h = b\right)\mathbb{P}\left(\hat{t}_h^c > \mathrm{t}_{\alpha/(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right)\right]$$

Since we saw in Section 3 that split-sample strategies have lower power than PAPs, this derivative is positive and it suggests that, *ceteris paribus*, hypotheses with higher utility weights will be more likely to be included in the prespecified portion. In turn, if we differentiate the net benefits with respect to p_h, we see

$$\begin{aligned}\frac{\partial}{\partial p_h} &= u_h \mathbb{E}_{\mathcal{H}'}\left[\mathbb{P}\left(\hat{t}_h > \mathrm{t}_{\alpha/2(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right) - \mathbb{P}\left(\hat{t}_h^e > \tau \mid \beta_h = b\right)\mathbb{P}\left(\hat{t}_h^c > \mathrm{t}_{\alpha/(|\mathcal{H}'|+1)} \mid \beta_h = b, \mathcal{H}'\right)\right] \\ &+ \left(\mathbb{P}\left(|\hat{t}_h^e| > \tau \mid \beta_h = b\right) - \mathbb{P}\left(|t| > \tau\right)\right)\mathbb{E}_{\mathcal{H}'}\Big[\sum_{j \neq h} u_j p_j \zeta_j(\mathcal{H}_p, \mathcal{H}')\Big]\end{aligned}$$

Since the probability of a noncentral *t*-statistic exceeding τ (in absolute value) is larger than the probability of a central *t*-statistic exceeding τ (in absolute value), this derivative must be positive and larger than the partial derivative with respect to u_h. Thus, we see that, *ceteris paribus*, higher

[28] One can derive that p_h separates these two groups. If p_h is larger than the relative power in the second stage of the split sample to the prespecified portion multiplied by the conditional probability that a hypothesis entering the second stage has the right sign and is false, then hypotheses that would yield positive net benefits under any approach either should only be tested under prespecification or else could be tested under either approach; otherwise, hypotheses should only be tested under the split sample or under either approach

values of p_h lead to prespecification within a hybrid plan, and the effect of an increase in p_h on prespecification is larger than a comparable increase in u_h. Intuitively, there is more power to reject hypotheses included in the prespecified portion of the hybrid plan, so researchers will want to include hypotheses with a large expected rejection value ($u_h p_h$). On top of this, the penalty for including a hypothesis in the split sample (relative to prespecifying it) is smaller when p_h is low, as the hypothesis is less likely to pass to the confirmation stage and inflate the multiple inference correction. Thus, an optimal hybrid approach has "high-value" hypotheses (high $u_h p_h$) prespecified, as well as hypotheses with lower utility weights but high priors. Hypotheses that are potentially interesting (high u_h) but have lower priors (low p_h) are tested in the split sample.

These results confirm our intuition that, in an optimal hybrid plan, hypotheses that the researcher cares more about or believes are more likely to reject belong in the PAP portion, while hypotheses that the researcher cares less about or believes are unlikely to reject belong in the split-sample portion (or should not be considered at all). We next construct a large set of hybrid plans and simulate power to determine guidelines for constructing hybrid plans.

4.1 Simulating Hybrid Plan Performance

To assess the performance of hybrid plans, we continue with the previous simulation environment. We now introduce researcher priors over the probability that hypotheses are false. There are H hypotheses, H_1 of which the researcher believes are false with probability p. False hypotheses again have a normalized mean effect size of μ, where the data-generating process draws a coefficient β_h from a normal distribution with mean μ and standard deviation $\mu/2$. The remaining $H - H_1$ hypotheses the researcher believes are true with probability q ($1 - q \leq p$). We assume that the researcher's priors are on average correct; i.e., the true DGP draws false (true) hypotheses with probability p (q) among the believed-false (believed-true) hypotheses. Higher values of p and q imply more accurate researcher priors; in the case in which the researcher is entirely uninformed, $p = 1 - q$. For the moment we assume the researcher values all rejections equally, so $u_h = 1$.

To test for robustness in different environments, we vary H, H_1, p, q, and μ across simulations. We simulate the performance of PAPs as in Section 3.5. To assess the hybrid procedure, we first declare the PAP portion of the plan. We allow this PAP portion to vary in integer size from a single

hypothesis to $H-1$ hypotheses. Using the results from Equations (8) and (9) we always place believed-false hypotheses in the PAP portion prior to placing any believed-true hypotheses in it.

For hypotheses in $\mathcal{H}_p$, the PAP portion of a hybrid plan, we form the full-sample *t*-statistics from the corresponding elements of $\hat{\boldsymbol{\beta}}$, the coefficient estimates that we simulated for non-hybrid PAPs. For the split-sample portion, we choose the share of the data going to the exploratory sample, s, and draw two $(H-|\mathcal{H}_p|)\times 1$ vectors of coefficient estimates: $\hat{\boldsymbol{\beta}}^{e}_{hyb}(s)$, a vector of estimated $\hat{\beta}$s from the exploratory sample, and $\hat{\boldsymbol{\beta}}^{c}_{hyb}(s)$, a vector of $\hat{\beta}$s from the confirmation sample. Both of these vectors have expectations equal to the corresponding elements of $\boldsymbol{\beta}$, but their sampling variances differ unless $s=0.5$. We construct *t*-statistics for each coefficient in $\hat{\boldsymbol{\beta}}^{e}(s)$ and $\hat{\boldsymbol{\beta}}^{c}(s)$ and perform split-sample analyses analogous to those in Section 3.5. Note that FWER or FDR control now occurs on the total set of hypotheses tested, or the union of $\mathcal{H}_p$ and the set of hypotheses carried to the confirmation stage. Thus the researcher cannot multiplicity adjust the PAP portion of the hybrid plan until she files the split-sample analysis plan.

Table 1 reports the different parameter values used in the hybrid simulations. We simulate 2,112 combinations of parameter values in total; in the discussion we also focus on "more empirically relevant" parameter values, which we define as $H\geq 50$, $\mu\leq 0.3$, and $H_1/H\leq 0.2$ based on the results from our surveys of field experiments (see Section 3.4.1) and PAPs. We begin by comparing the power of an optimally constructed hybrid plan to the power of a hybrid plan that only includes believed-false hypotheses in its PAP portion. We refer to the latter plan as a "believed-false hybrid plan." Across all parameter combinations, the optimal hybrid plan is equivalent to the believed-false hybrid plan 44% of the time.[29] In the remaining 56% of cases, the optimal hybrid plan has a PAP portion that is a strict superset of believed-false hypotheses.[30] It is almost never the case,

[29]In 9% of cases, the "optimal" hybrid plan prespecifies a strict subset of the believed-false hypotheses, typically omitting a single believed-false hypothesis. This occurs because the objective function in these cases is very flat with respect to hybrid plan size, so the difference in power between a believed-false hybrid plan and a slightly smaller hybrid plan is within simulation error. We verify this by running a duplicate set of simulations that constrain the optimal hybrid plan to include all believed-false hypotheses. Despite the constrained version optimizing over a smaller set of potential plans, which mechanically reduces power given simulation error, the median (mean) power difference is only 0.6% (1.0%) across all cases in which the "optimal" hybrid plan is smaller than the believed-false hybrid plan.

[30]We report these figures for an exploratory share of 35%. Across all exploratory shares that we simulated, the optimal hybrid plan is equivalent to the believed-false hybrid plan 39% of the time.

however, that the optimal hybrid plan contains all hypotheses and becomes a conventional PAP.

Although the optimally sized hybrid plan is weakly larger than the believed-false hybrid plan, we focus on the power of the latter plan type for two reasons. First, constructing an optimally sized hybrid plan requires the researcher to know features of the DGP, such as effect size, p, and q, before seeing any of the data. We thus expect it will be impractical for a researcher to construct an optimally sized hybrid plan in most cases. Second, the power difference between optimally sized hybrid plans and believed-false hybrid plans is minimal in almost all cases. For example, in the median (mean) case across all parameter combinations, the believed-false hybrid plan achieves 99% (98%) of the power of an optimally sized hybrid plan. Even when restricting to parameter combinations that are more empirically relevant, $H \geq 50$, $\mu \leq 0.3$, and $H_1/H \leq 0.2$, the believed-false hybrid plan still achieves 99% (96%) of the power of an optimally sized hybrid plan. When accounting for simulation error, the true power difference between believed-false plans and optimal plans is even smaller.[31]

Focusing on believed-false plans, we establish rules of thumb for optimal exploratory share, s, and pass-on threshold, τ. Table 2 reports power for a believed-false hybrid plan under ten different values of exploratory share, s: 0.10, 0.15, 0.20, 0.25, 0.30, 0.35, 0.40, 0.45, 0.50, and 0.75. This hybrid plan passes hypotheses for confirmation if they achieve a *t*-statistic of $\tau = 1.6$ or greater in absolute value in the exploratory sample, but the patterns in Table 2 are broadly similar for other thresholds in the range of 1.4 to 1.8 (see Appendix Table A1). In this table, and the remaining tables and figures in this section (except Panels E and F of Figure 4), we normalize power against the power of an exhaustive PAP that specifies all hypotheses; a value of 1 indicates that the two strategies have identical power. Column (1) reports average power over all parameter combinations, while Columns (2) through (5) report average power over parameter combinations that are more empirically relevant. Column (3) restricts $q \geq 0.90$, and Columns (4) and (5) restrict $q \geq 0.96$, as we find that q is an important determinant of absolute and relative power. The last column restricts H, H_1/H, and μ to their most extreme values (100, 0.10, and 0.2 respectively).

[31]These figures overstate the power of an optimally sized plan because the optimally sized plan is the plan that performed best out of *all* possible plan sizes. It thus represents the maximum of a series of 1,000-iteration simulations, whereas the believed-false plan is the maximum of a single 1,000-iteration simulation. Based on simulation error alone we thus expect the optimally sized plan to outperform the believed-false hybrid plan by an average of one to three percent, even if there is no true power difference between the plans.

In all five columns the optimal exploratory share value is in the range of 0.30 to 0.40. More importantly, the objective function appears fairly flat in this range, so we recommend $s = 0.35$ as a reasonable rule of thumb for exploratory share.

Figure 3 plots average power for believed-false hybrid plans as a function of the pass-on threshold τ (with $s = 0.35$). The solid line plots average power across all parameter combinations, the dashed line plots average power over parameter combinations that are more empirically relevant, and the dotted line further restricts the simulation set to cases in which $q \geq 0.90$. In all three cases the pass-on threshold that performs best on average appears to be in the range of 1.4 to 2.0, so we recommend $\tau = 1.6$ as a reasonable rule of thumb for the pass-on threshold.[32] Versions of Appendix Table A1 and Figure 3 that control FDR instead of FWER generate qualitatively similar results – exploratory shares in the range of 0.30 to 0.40 and pass-on thresholds in the range of 1.4 to 2.0 are generally optimal (see Appendix Table A2).

Table 3 reports standardized coefficients from regressions of the optimal pass-on threshold on features of the study and DGP: q, μ, p, H, and H_1/H. Column (1) uses the full set of parameter combinations for the estimation sample. Column (2) restricts the estimation sample to more empirically relevant parameter combinations, and Column (3) further restricts the sample to cases in which $q \geq 0.90$. The two features that affect the optimal pass-on threshold most strongly are q, which enters positively, and effect size μ, which enters negatively. These results suggest that a researcher should lean towards a higher pass-on threshold when she is more confident that the believed-true hypotheses are truly null – the standard of evidence for passing on a test is higher when one's prior is that the test will not reject. She should lean towards a lower pass-on threshold when she believes that *t*-statistics for false hypotheses are likely to be large in magnitude – the stricter multiplicity adjustment from running more tests is of little consequence if the *t*-statistics are very large. Higher values of p also suggest higher pass-on thresholds, though the standardized

[32]Since the researcher specifies priors in our simulations, she could alternatively pass on hypotheses based on the posterior probability that a hypothesis is false after observing the exploratory data. In our simulations posterior-based pass-on rules perform similarly to fixed τ thresholds, which is unsurprising since the only heterogeneity in priors arises across believed-false and believed-true hypotheses (so there is a one-to-one mapping of exploratory *t*-statistics to posteriors within the believed-true hypotheses). A researcher with richer heterogeneity in priors might benefit from using posterior-based pass-on rules, but we suspect that most researchers will not have such detailed priors.

effect is less pronounced than changes in q, and the raw effect is much less pronounced.[33]

Figure 4 plots the distribution of the relative power of a believed-false hybrid plan (compared to an exhaustive PAP or a believed-false PAP). Panel A plots the distribution across the full set of parameter combinations, while Panel B plots the distribution across only more empirically relevant parameter combinations. Across the full set of parameter combinations, the believed-false hybrid plan is typically more powerful than an exhaustive PAP. Nevertheless, in 43% of cases the exhaustive PAP is more powerful.[34] Across more empirically relevant parameter combinations, the believed-false hybrid plan is more powerful than an exhaustive PAP in 64% of cases. Panels C and D are identical to Panels A and B, but the researcher now values rejection of believed-false hypotheses twice as much as rejection of other hypotheses (i.e., $u_h = 2$ for believed-false hypotheses). This simulates the intuitive case in which the researcher has heterogeneous preferences over hypotheses and places ones that she values more in the PAP portion of the hybrid plan. Across all parameter combinations the believed-false hybrid now outperforms the exhaustive PAP in 76% of cases, and among more empirically relevant parameter combinations the believed-false hybrid is more powerful in 84% of cases. Finally, Panels E and F are identical to Panels A and B, but the power of the believed-false hybrid plan is now rescaled relative to the power of a believed-false PAP (i.e., a PAP that only included believed-false hypotheses). These panels reveal that the hybrid approach dominates an approach that only tests believed-false hypotheses in the vast majority of cases – 91% of the time across the full set of parameter combinations, and 86% of the time across the most empirically relevant parameter combinations.

To guide researchers in choosing when to use a hybrid plan, Table 4 reports standardized coefficients from regressions of the relative power of a believed-false hybrid plan on features of the study and DGP: q, μ, p, H, and H_1/H. As in Table 3, Column (1) uses the full set of parameter combinations for the estimation sample, Column (2) restricts the estimation sample to more empirically relevant parameter combinations, and Column (3) further restricts the sample to cases in

[33] Recall from Table 1 that the standard deviation of p is almost five times higher than the standard deviation of q. In practice, we expect that p could plausibly range from near zero to near one (i.e., the believed-false hypotheses could all be false or all true), whereas q is likely to remain close to one (i.e., it is rare that every single hypothesis in a study with many hypotheses is false).

[34] This finding is consistent with Proposition 1, above, as in these cases the optimal PAP may not have an interior solution.

which $q \geq 0.90$. The two features that affect the power of a believed-false hybrid plan (relative to an exhaustive PAP) most strongly are q, which enters positively, and effect size μ, which enters negatively. These results imply that a researcher has the greatest incentive to adopt a hybrid plan over an exhaustive PAP when she expects few believed-true hypotheses to reject or when she believes that *t*-statistics for false hypotheses are likely to be modest in magnitude. The intuition is straightforward. When effect sizes are small, conserving power for believed-false hypotheses becomes important, and the exhaustive PAP specifies too many tests. Alternatively, when very few believed-true hypotheses are false, it is suboptimal to dilute the power of believed-false hypothesis tests by testing every believed-true hypothesis. In both cases the hybrid plan conserves power for believed-false hypotheses by passing on only a subset of believed-true hypotheses to the confirmation stage.

5 Application: GoBiFo Revisited

Casey et al. (2012) document the impacts of GoBiFo, a community-driven development (CDD) intervention in Sierra Leone. In the application we reanalyze the effects of GoBiFo using various candidate PAPs and hybrid approaches that the authors could have constructed. We summarize the Casey et al. (2012) discussion of the institutional features of GoBiFo here before discussing several features of the evaluation that make it an appealing choice of an application.

CDD programs are an important outlet for international donor funding, and GoBiFo had a variety of features common to CDD-type programs in the developing world. First, it provided block grants, training, and business start-up capital based on community proposals with a goal of enhancing public goods access. These grants were substantial relative to living standards: financial outlays were $4,667 per village, or about $100 per household. To receive these grants, village development committees (VDCs) were required to submit a development proposal to the ward development committee (WDC), the next higher level of government bureaucracy, for review, endorsement, and transmission to the relevant District Council for approval. 43% of grants were used for local public goods (such as community centers, sports fields, primary school repairs and sanitation); 40% applied to agriculture and livestock or fishing management (such as seed multiplication, communal farming, or goat herding); and the remaining 17% went towards skills training

and small business development initiatives. Casey et al. (2012) describe these facets of the GoBiFo intervention as the "hardware" of the intervention.

On top of block grants to create new public goods, GoBiFo had several features meant to build democratic institutions, which may be particularly relevant in the traditional authority context of Sierra Leone. In particular, GoBiFo both established VDCs, which would play a role in coordinating local governance, and instituted participation requirements for historically marginalized groups, such as women and youth. These participation requirements included, for example, that VDC bank accounts included at least one female signatory and that public works proposals document evidence of the inclusiveness of women and youth in the proposal generation requirements. Inclusiveness and democratization were monitored by GoBiFo staff at substantial cost – monitoring and facilitating this institution building cost about the same amount as the actual development grants given out. Casey et al. (2012) describe this facet of GoBiFo as the "software" effects of the CDD program.

Casey et al. (2012) introduce a PAP with twelve hypotheses, listed in Table 5, which also includes the t-statistics and FWER-adjusted p-values reported in the paper. The first three of these hypotheses relate to the "hardware" of public good provision in the village. In all three cases, Casey et al. find strong evidence that the "hardware" of public good provision changed. Examining the underlying variation, these hypotheses confirm that GoBiFo was successfully implemented, and led to an outlay of funds and investment in public goods. The remaining nine hypotheses relate to the "software" of the program, examining a range of outcomes, including participation in collective action, trust of leaders, participation in local governance, and reductions in crime and conflict in the community. Casey et al. (2012) find no evidence that GoBiFo affects any of these outcomes, at least after adjusting p-values for the number of hypotheses tested (twelve). Ultimately, they conclude that the program was implemented as planned, and led to some expenditures and a change in the public goods environment, but that there is no evidence that it changed the social institutions governing these villages.

The evaluation of GoBiFo makes for a natural test application of hybrid methods for several reasons. First, as the one of the seminal papers introducing preanalysis plans to economists, we observe a carefully thought out and well-regarded PAP which has become a template for PAPs in the literature. Thus, we can compare any effects identified through the hybrid approach against

those that careful researchers might expect to find through a well-crafted PAP. Second, the design of the GoBiFo data collection is such that there is a natural delimitation of a relatively small set of empirical tests which would have been likely to be conducted by a researcher using a split-sample approach. More specifically, the public dataset has a few types of variables: village identifiers, which can be used to implement panel data regressions; treatment status and time indicators; eight village-level covariates; and 334 candidate dependent variables. Among the candidate dependent variables, 183 constitute the "core" dependent variables, which the authors use to construct twelve indices, each based around a particular hypothesis about the intervention (following O'Brien (1984); Kling et al. (2007)). These twelve indices constitute the variables for the main analysis in the paper.

The twelve hypotheses in Casey et al. (2012) are each average treatment effects across the whole sample, estimated by comparing endline treatment and control outcomes. Thus, the primary results in the PAP and the initial presentation are estimated by

$$y_v = \beta T_v + \gamma X_v + \varepsilon_v$$

where X_v are covariates and T_v is an indicator for treatment status[35].

Our goal in analyzing these data is to determine what a researcher employing a hybrid split-sample method would have been likely to discover. In order to avoid specification search, in our split-sample procedure we propose two extensions to the Casey et al. (2012) analysis that a researcher armed with an exploratory sample would have been likely to discover. First, for 62 of the 183 core variables, there was baseline data collection prior to the GoBiFo intervention. Eleven of the twelve hypothesis indices contain at least one of these panel variables.[36] We create indices

[35]In our estimates, we use the full vector X_v of controls rather than just the stratifying covariates as in Casey et al. (2012). We made this choice to keep the covariate environment constant across heterogeneity specifications, discussed below. It does not qualitatively affect any conclusions in this paper.

[36]Broadly, there are three classes of dependent variables. There are economic dependent variables (e.g., asset ownership), subjective expectation dependent variables (e.g., trust in village authorities), and dependent variables that describe community responses to a series of "structured community activities" (SCAs). These SCAs are a measurement tool introduced by the authors for which they asked the village to make a variety of public goods decisions and measured both the outcome and the deliberation process leading to those decisions. In general, the panel data include the economic dependent variables and many of the subjective expectations variables. The SCA variables are necessarily excluded from the panel, as are some of the more specific subjective expectations.

from the panel variables and consider village fixed effects regressions for variables where the panel can be utilized.[37] In these specifications, we estimate

$$y_{vt} = \beta T_{vt} + \delta_t + \alpha_v + e_{vt}$$

Second, the eight village-level covariates can be used for heterogeneity analyses. In fact, several of these covariates seem motivated by theory on public good provision. For example, heterogeneity based on village size is a classic idea in the public goods literature (Olson 1965; Ostrom 2009), and heterogeneity in public goods provision based on ethnolinguistic fractionalization is suggested by Alesina et al. (1999), Easterly and Levine (1997), and Miguel and Gugerty (2005). Other covariates have less clear relationships to theory, such as an indicator for whether a sampling ward is in the Bombali district (where sampled villages were larger, but also potentially different culturally). However, the fact that the number of potential heterogeneity dimensions is limited to eight allows us to search them exhaustively without making value judgements on which heterogeneity dimensions would or would not have been discovered by researchers. Panel C of Table 5 lists the eight covariates.[38]

We consider heterogeneous treatment effects specifications using each of these eight variables, estimating equations of the form

$$y_v = \beta T_v + \xi T_v * X_v + \gamma X_v + \omega_v$$

and

$$y_{vt} = \beta T_{vt} + \xi T_{vt} * X_v + \alpha_v + \delta_t + w_v$$

With average treatment effects, eight dimensions of heterogeneity, twelve hypotheses based on endline treatment-control comparisons, and eleven hypotheses based on panel comparisons, this generates 207 total candidate hypotheses. For convenience we refer to endline treatment-control

[37] This approach differs from the "include panel data" version of the specifications included in Table 3 of Casey et al. (2012). That approach presents estimates from a combined approach that averages estimates from endline treatment-control comparisons (for variables with no available panel data) with those from a difference-in-difference specification (for variables with available panel data). The difference-in-difference specification groups all treatment villages rather than including village fixed effects, which potentially reduces precision.

[38] Of these dimensions, Casey et al. (2012) explicitly stratified randomization on village size, distance to the road, and district, which suggests heterogeneity analyses on these dimensions in particular

comparisons and panel comparisons as separate "hypotheses" as they constitute separate statistical tests, even though in some cases they test the same underlying hypothesis.

Given this set of candidate hypotheses, we compare performance with a number of options for prespecification. In each case we suppose that we had prespecified a subset of hypotheses, and then assume that the authors used the split-sample method to identify and test for remaining hypotheses. We further suppose that the authors had used a random 35% of the data as an exploratory sample.[39] Since the sample split is random, we bootstrap the sample split 500 times. In each case, we test all of the 207 hypotheses that are not prespecified in the exploratory sample, and pass on all hypotheses which have a *t*-statistic of at least 1.6 in the exploratory sample. We then adjust our standard errors based on either a FWER adjustment with Holm sharpening or FDR control and determine the number of rejections.

We consider several potential prespecifications for standalone PAPs or hybrid plans. First, as a benchmark, we consider the actual PAP specified by Casey et al. (2012) ($H_1 = 12$). Second, we consider a more restricted PAP where the authors would have prespecified just the three "hardware" hypotheses ($H_1 = 3$). This PAP may be motivated if the authors had held the very plausible (and *ex post* validated) priors that the "hardware" hypotheses were more likely to be influenced by treatment. Third, we consider PAPs that prespecify all 23 ATE hypotheses (twelve endline and eleven panel; $H_1 = 23$). Fourth, for each heterogeneity dimension we consider PAPs that prespecify all 23 ATE hypotheses and the candidate heterogeneity dimension for all 23 hypotheses ($H_1 = 46$). Such a PAP could be motivated by either greater researcher interest in testing for differential effects in one heterogeneity dimension relative to others or a greater prior that heterogeneous effects would exist in one particular dimension (perhaps motivated by theory or prior work). Finally, we consider a PAP that prespecifies all 207 hypotheses and an additional, fully exhaustive PAP that specifies all 207 hypotheses and tests for endline treatment-control differences on each of the 183 core dependent variables as well as heterogeneous interactions with each of them.

[39] We block sample the sample-split randomization at the village level.

5.1 Power Results

To analyze the different approaches to prespecification, we first consider the total number of rejections under different approaches and then consider which specific hypotheses reject. Table 6 lists the total number of hypothesis indices rejected from each of these approaches. For PAP approaches, the data are deterministic – hypotheses either would have rejected in this dataset, or they would not have.[40] For hybrid approaches, the data are probabilistic, as rejections depend on the realization of the random sample split. Using the twelve-hypothesis PAP specified by the authors, we replicate the three "hardware" rejections. Hybrid approaches that take the authors' prespecifications and then use the split-sample procedure to search through the rest of the 207 hypotheses identify 5.3 rejections (5% FWER), 5.9 rejections (10% FWER), or 8.5 rejections (5% FDR). Clearly, these are large gains. If the authors had prespecified only the three hardware hypotheses, hybrid approaches see a further power gain of 0.2 to 0.3 rejections under each treatment of type I error (FWER or FDR control), compared to hybrid plans that prespecify the twelve hypotheses.

We then consider the candidate rejections if the authors had included the panel specifications in their PAP. Including the panel specification of ATEs adds an additional rejection under any error treatment. Later, we see that this exclusion affects the qualitative conclusions regarding the overall effects of GoBiFo. Hybrid performance is unaffected by including or excluding the panel hypotheses from the prespecified portion.

The next several rows of Table 6 consider PAPs that prespecify all 23 hypotheses for ATEs and all 23 hypotheses for one candidate dimension of heterogeneity. Three dimensions of heterogeneity lead to increases in power when prespecified in a PAP or hybrid approach: village size, the presence of an extractive chief, and location in a Bombali district. The largest power increase occurs when prespecifying heterogeneity with respect to village size, which ties to past theory. Had the authors prespecified that they anticipated heterogeneity by village size, they would have identified six to eight rejections, depending on the type I error treatment, even without a hybrid approach. If they had prespecified heterogeneity by village size and used a hybrid approach to identify meaningful

[40] One could compare power under plausible realizations of the dataset by bootstrapping the entire dataset. Replications of the main comparison tables are available from the authors. In general, bootstrapped versions of the dataset show a broader range of potential rejections under all approaches, but comparisons between approaches are qualitatively unaffected.

heterogeneity on other dimensions, they would have identified 7.1 to 11.0 rejections, depending on the type I error treatment. Gains are largest, in this context, when applying 5% FDR control. In fact, the 11.0 rejections in the hybrid plan with this prespecification represent the most rejections we see across any simulated analysis plan. In contrast, if the authors had prespecified that they anticipated heterogeneity with respect to distance to the road, whether villagers had owned slaves, average years of education, ethno-linguistic fractionalization, or past war exposure, the PAP would not find additional rejections compared to a PAP that fully specified all ATE specifications. If the authors had prespecified one of these dimensions, the hybrid approach would also add less power than it would to a PAP which did not prespecify those heterogeneity dimensions.

Finally, we consider a PAP that prespecifies every ATE hypothesis and every heterogeneity dimension. This PAP would need to be fully anticipatory, and it would have rejected eight hypotheses under 5% FDR, nine hypotheses under 10% FWER, and ten hypotheses under 5% FDR. Across all considered specifications, this performance yields more total rejections than any other PAP or hybrid plan except for the hybrid that prespecifies the village size dimension (in FDR). This result is consistent with our simulations. If we think that the original Casey et al. PAP contained the "high prior" outcomes, then p, the probability that a believed-false hypothesis rejects, is 0.25. In contrast, there are about eleven total rejections in the highest-powered FDR hybrid, three of which are prespecified. If these are all correct rejections, then the number believed-true hypotheses that were in fact false is at least eight. Given that there are 195 tested hypotheses that were excluded from the PAP, this suggests that q, the probability that a believed true hypothesis is true is 0.96 in this application. If we run a GoBiFo-tailored set of simulations, with $p = 0.25$, $q = 0.96$, $H = 200$, $H_1 = 12$, and effect sizes of the magnitudes in GoBiFo, a "believed false" hybrid has 95% of the power of an exhaustive PAP and more than double the power of a "believed-false" PAP, consistent with the magnitudes we find here. The relatively small gap in likelihood of a hypothesis being false between "believed true" and "believed false" hypotheses in this context prevents the hybrid from realizing some of the potential power gains seen in other parameterizations.

Even the exhaustive PAP is not truly exhaustive – the researcher only considers summary indices and interactions of these indices with heterogeneity dimensions. The full dataset includes many dependent variables, and another potential PAP of interest would include every dependent variable, including the individual variables that comprise summary indices and their interactions

with heterogeneity dimensions. Ultimately, such a PAP would contain 1,854 hypotheses. The final row of Table 6 examines how many of the 207 index hypotheses reject when conducting all 1,854 tests. It demonstrates the importance of multiple inference concerns – if every possible candidate hypothesis had been prespecified, there would have been only five indices that were ultimately rejected (under 5% or 10% FWER), or nine under FDR. These figures represent declines of 10% to 44% relative to a PAP that "only" prespecifies 207 hypotheses.

The results above treat all rejections equally. We cannot guess which sets of heterogeneity hypotheses researchers trying to learn from GoBiFo would have valued most, though it seems reasonable to guess that the ATE hypotheses would have been of relatively high interest. We now explore the performance of the exhaustive PAP if one of the dimensions of heterogeneity also received higher weights in the researcher's objective function, and vary the relative weight from one to five. Letting R_h represent an indicator for hypothesis h rejecting, we examine

$$\sum_h u_h R_h \quad \text{where} \quad u_h = \begin{cases} u, \ h \in \text{ATE } or \ h \in \text{dimension } k \\ 1, \ \text{else} \end{cases}$$

To illustrate the potential role of weights in analysis choices, we focus on two heterogeneity dimensions where at least one approach found additional rejections. We contrast three potential approaches to controlling false discoveries: an exhaustive PAP that includes all 207 hypotheses, a restricted PAP that includes just the 46 hypotheses which receive a higher weight (the ATE and the relevant heterogeneity dimension), and a hybrid approach that prespecifies the restricted PAP and performs a split-sample analysis on other variables.

Figure 5 draws these comparisons. In all cases, we normalize the sum of weighted rejections in the exhaustive PAP to one, regardless of the weight. Panels A and B of Figure 5 plot the sum of weighted rejections when tests for heterogeneity with respect to number of village households receive higher weight, controlling FWER (Panel A) or FDR (Panel B) respectively. In both cases the hybrid approach rejects more weighted hypotheses than the restricted PAP approach, and both approaches gain relative to the exhaustive PAP as the weights increase. If the researcher weights rejections in the number of households dimension more than other heterogeneity dimensions, the hybrid approach generates, relative to the exhaustive PAP, fewer rejections when controlling FWER (Panel A) and more rejections when controlling FDR (Panel B). With FWER control, the difference

becomes small as the weights increase. While the restricted PAP never matches the performance of the hybrid approach, it outperforms the exhaustive PAP under FDR for relative weights larger than three. Intuitively, the larger number of hypotheses in the exhaustive PAP reduces power to reject each individual hypothesis, which is costly for the higher-weighted hypotheses.

A similar pattern emerges in Panels C and D of Figure 5, where the highly-weighted dimension is the presence of an extractive chief. In this case, the hybrid always outperforms the restricted PAP, but it also outperforms the exhaustive PAP for weights larger than 1.75 (when controlling FWER) or 1.9 (when controlling FDR). Similarly, the restricted PAP outperforms the exhaustive PAP for weights larger than 2.6 (FWER control) or 4.0 (FDR control).

5.2 Qualitative findings under different approaches

While the number of rejections is useful for assessing statistical power, it cannot characterize the paper that could have been written under each analysis plan. For example, we know from Casey et al. (2012) that analysis of GoBiFo using a pure PAP to prespecify the twelve endline treatment-control comparisons concluded that GoBiFo affected the three hardware hypotheses, but that there were no statistically significant effects on any of the software hypotheses.

Tables 7 and 8 report all hypotheses that reject in at least 10% of trial runs under at least one specification while controlling FWER and FDR at 5%, respectively, along with the full-sample t-statistics for each hypothesis.[41] The different panels in these tables group hypotheses for average treatment effects or by specific heterogeneity dimensions. In each table, Column (1) identifies which hypotheses reject under the twelve-hypothesis Casey et al. PAP, Column (2) identifies the probability of rejection under a hybrid approach that prespecifies the Casey et al. PAP, and Columns (3) and (4) present analogous results for a "hardware-only" PAP that prespecifies the first three hypotheses. In Columns (5) through (7) we consider PAP and hybrid approaches that include all 23 ATE hypotheses and 23 hypotheses for one heterogeneity dimension. For Columns (5) and (6), we focus on rejection rates when one dimension of heterogeneity is prespecified, either as part

[41] Heterogeneity with respect to distance to the road, education levels, slaveholding, war exposure, and ethnolinguistic fractionalization do not appear in these tables as they do not reject at least 10% of the time under any of the procedures considered. In the analogous tables that bootstrap the dataset, we do see some weaker evidence that some of these heterogeneity dimensions may have been relevant under alternate potential sample realizations.

of a stand-alone PAP (Column (5)) or as part of a hybrid approach (Column (6)). For each panel the rejection probabilities in Columns (5) and (6) thus refer to the particular prespecification in which the relevant heterogeneity dimension was prespecified, while Column (7) reports rejection probabilities when the hypothesis is not in the prespecified heterogeneity dimension but instead is tested in the split-sample portion.[42] Finally, Column (8) indicates the rejections that would occur had all 207 main hypotheses been prespecified.

Panels A of Tables 7 and 8 present ATE hypotheses that reject at least 10% of the time in one or more specifications. The three "hardware" hypotheses rejected in Casey et al. (2012) reject in any of the approaches considered. This occurs because the study was overpowered for these hypotheses – with full-sample t-statistics of 5.3, 8.0, and 12.6, the results can survive far more stringent multiple-inference corrections.

An additional "software" ATE, the hypothesis that "GoBiFo increases public participation in local governance," has a full-sample t-statistic of 4.4 in the panel specification and rejects when prespecified. This rejection is qualitatively important – the fact that GoBiFo appears to change participation in local governance on average is an indication that GoBiFo did indeed have a measurable impact on institution-building, which may provide support for the large fraction of expenditures dedicated to these software components. Note that the panel specification was important for this result; the raw endline treatment-control comparison for this hypothesis does not reject (as Casey et al. found). This contrast highlights the importance of perfect anticipation when hybrid strategies are not available, as the authors did not prespecify the fixed effects specification. The hybrid approach also rejects this hypothesis when prespecified, and more notably it rejects the hypothesis the majority of the time when not prespecified. For example, had Casey et al. used a hybrid approach that prespecified the same twelve hypotheses, and explored simple village fixed effects specifications in the split-sample portion, they would have had a 66% chance of rejecting this software hypothesis under FWER control and an 88% chance of rejecting it under FDR control. Thus, under a hybrid approach, the ATE conclusions would likely have been qualitatively different.

Panels B of Tables 7 and 8 indicate that GoBiFo exhibited treatment effect heterogeneity with respect to village size, primarily for "software" hypotheses. This dimension is particularly interesting as it is a classic feature in the public goods literature since its introduction by Olson (1965)

[42]For completeness we average over all potential alternate heterogeneity dimensions in Column (7).

– collective action becomes more difficult in larger groups. Furthermore, the authors explicitly stratified on this dimension, guaranteeing that imbalance in treatment could not yield false heterogeneity results. Under 5% FWER control (Table 7), if the authors prespecified heterogeneity with respect to village size they would have identified that in larger villages GoBiFo was less effective at improving local project infrastructure and less effective at increasing access to information about local governance. Under 5% FDR control (Table 8), if the authors prespecified heterogeneity with respect to village size they would have found the same reduced effects on project infrastructure and information, and also that in larger villages GoBiFo was less successful in increasing public participation in local governance, and – perhaps surprisingly – that these decreases occurred even though GoBiFo motivated more collective action and contributions to public goods. Column (5) indicates that this last effect would have been found under a PAP that only specified ATEs and the village-size dimension and a hybrid that prespecified the same, but not under a PAP that exhaustively specified all candidate dimensions of heterogeneity. Moreover, if the hybrid approach were utilized with FDR control, the authors would have had a 52% chance of concluding that in larger villages GoBiFo was also less effective at increasing inclusion and participation in community planning and infrastructure if it were prespecified. This finding would not have been identified under any of the PAPs considered.

Under the hybrid approach, the authors would have also likely concluded that village size was a meaningful dimension of heterogeneity if they did not prespecify it. Under FWER control, there is a 14-20% chance of concluding that larger villages were less effective at improving local project infrastructure (depending on how many other hypotheses were prespecified) and a 19-24% chance of concluding that larger villages were less effective at increasing access to information about local governance. These chances become much larger under FDR control. Using a hybrid that did not specify heterogeneity with respect to village size with the FDR correction, the authors would have had a 51-58% chance of identifying that larger villages were less effective at improving local project infrastructure, a 50-57% chance of identifying that larger villages were less effective at increasing access to information, and a 48-59% chance of identifying that larger villages were less effective at increasing participation in local governance, along with smaller chances on the other two hypotheses that could have rejected if prespecified. Broadly, we conclude that GoBiFo had meaningful treatment heterogeneity with respect to village size, and a researcher interested in this

dimension would have been able to identify about 4.5 differences under the hybrid approach (if prespecified and using the FDR correction) and about 1.6 differences under the hybrid approach if they had failed to prespecify this dimension. Thus, we conclude that using a hybrid approach on the GoBiFo evaluation would have led to qualitatively different conclusions about the effects of GoBiFo on institution building. Moreover, it would have expanded upon Olson's insights on the importance of the number of stakeholders by establishing that the number of stakeholders also matters for the effectiveness of institution building around public good provision.

Panels C of Tables 7 and 8 document that there also appears to be meaningful heterogeneity with respect to the baseline presence of an extractive chief on "software" outcomes. The presence of an extractive chief is a signal on the local institutional environment. Theoretically, we might anticipate an extractive chief to have two possible effects on GoBiFo provision. On the one hand, an extractive chief may be more successful in diverting GoBiFo to his own purposes, weakening institution building and negatively affecting both hardware and software outcomes (e.g., Ostrom 1996; Dayton-Johnson 2003). On the other hand, baseline levels of institutional quality and public good provision may be lower under an extractive regime, which may amplify the impacts of GoBiFo. Quantitatively, we find that had the authors prespecified ATEs and heterogeneity with respect to an extractive chief, they would have identified that GoBiFo was more effective in increasing trust when an extractive chief was present (under both FWER and FDR control). Had they used a hybrid they would have found this result under either FWER or FDR control if they prespecified it, and even when not prespecified they would have found it with 18-27% probability under FWER control or with 69-74% probability under FDR control. Moreover, if they used a hybrid approach and prespecified this dimension, they would also have identified with 92% probability (under FDR control) that when there is an extractive chief, GoBiFo is less effective at improving project infrastructure. This latter result could only have been found under a hybrid approach.

Panels D of Tables 7 and 8 document that GoBiFo was also differentially effective in Bombali villages, at least on "hardware" outcomes. There may be contextual reasons to anticipate these effects, or one may have anticipated these effects due to differences in the sampling frame (sampled Bombali villages are larger than non-Bombali villages). Once again, the benefits of the hybrid approach are larger under FDR control, as are the number of rejections. However, absent a theoretical lens to interpret this heterogeneity, we simply note that the ultimate conclusions of an analysis

on GoBiFo would have documented important heterogeneity in its effectiveness. This is perhaps unsurprising given the implementation heterogeneity inherent to CDD programs, but nevertheless of clear interest to program implementers.

Taking these results together, we conclude that GoBiFo affected all three hardware hypotheses, as Casey et al. (2012) found. However, it also led to the software effect of increasing local participation in public governance. Furthermore, there was important heterogeneity related to GoBiFo: it was more effective in producing software outcomes in smaller villages and in villages with an extractive chief at baseline. Discovering any of the software results in a PAP depends crucially on which PAP was written. When using a hybrid approach, in contrast, at least some of the software effects would have been found with very high probability. Our analysis of weighted rejections also found that while the exhaustive PAP is attractive in terms of overall numbers of rejections, it is not always the most effective in terms of rejections of any specific hypothesis.

6 Conclusions and Recommendations

Preanalysis plans have emerged as a powerful tool to control false discoveries, and, in doing so, encourage scientific replicability. PAPs are not costless, however – researchers must correctly anticipate the full menu of tests to be run without access to any data. This paper develops a split-sample approach that allows researchers to learn from a portion of the data before finalizing an analysis plan. We suggest several optimized ways to learn from a portion of the data, including optimal sample splits, switching to one-sided tests, and thresholds for passing hypotheses. When researchers can write down a preanalysis plan which includes all false hypotheses (so that they need not learn from the data), and when the researchers' objective function is to maximize the expected number of rejections, preanalysis plans outperform the split-sample approach. However, the optimized split-sample approach provides about 90% of the power of the pure PAP, so that they may be preferred in cases where the researcher is unable to fully anticipate all tests that are to be run in advance, either because she will learn about some tests to run from patterns in the data or because the cost of her time is too high to fully develop a perfectly anticipatory PAP.

When hypotheses appear heterogeneous to a researcher *ex ante*, either in terms of prior beliefs or intrinsic interest, a hybrid approach that prespecifies some hypotheses and searches through the

data for the remaining ones typically outperforms a pure PAP. Simulations suggest that these gains are quantitatively meaningful over parameter values which are empirically relevant. We test this approach by reanalyzing GoBiFo, a CDD-program examined in the seminal PAP developed by Casey et al. (2012). In contrast to the results identified in the PAP, we find that GoBiFo had meaningful implications on building institutions, both on average and particularly in smaller villages.

We conclude with general recommendations for applied researchers constructing analysis plans. The first decision a researcher faces is whether to hold back a fraction of the data to preserve the option of split-sample analyses. If the researcher is confident that her anticipation rate approaches 100% and has uniform priors and utility weights across hypotheses, then it suffices to write an exhaustive pure PAP. Otherwise, holding back a fraction of the data is optimal (subject to cost constraints). As a rule of thumb we suggest an exploratory sample share of $s \approx 0.35$.

Conditional on holding back a fraction of the data, the researcher must decide how to construct a hybrid plan. We suggest separating hypotheses into "believed-false" and "believed-true" groups, with the former having relatively high values of $u_h p_h$ and the latter having relatively low values of $u_h p_h$. The researcher can then prespecify the "believed-false" hypotheses while considering the "believed-true" hypotheses in the split sample. In the split-sample analysis, as a rule of thumb we suggest a threshold for passing tests to the confirmation stage of $\tau \approx 1.6$. Researchers with heterogenous priors or utility weights, however, may (and ideally should) exercise discretion in passing tests to the confirmation stage; more interesting hypotheses (higher u_h) could face a somewhat lower threshold and less interesting hypotheses (lower u_h) a somewhat higher threshold. Furthermore, when applying sharpened FWER or FDR control (as we always recommend), we emphasize that there is no cost to passing hypotheses that are guaranteed to reject, so it is safe to pass any hypothesis that achieves very high *t*-statistics (e.g., 5 or more) in the exploratory sample.

In some cases, cost or logistical constraints may prevent the implementation of split-sample analyses. Absent those considerations, however, a split-sample component can enhance the range of potential discoveries for many PAPs.

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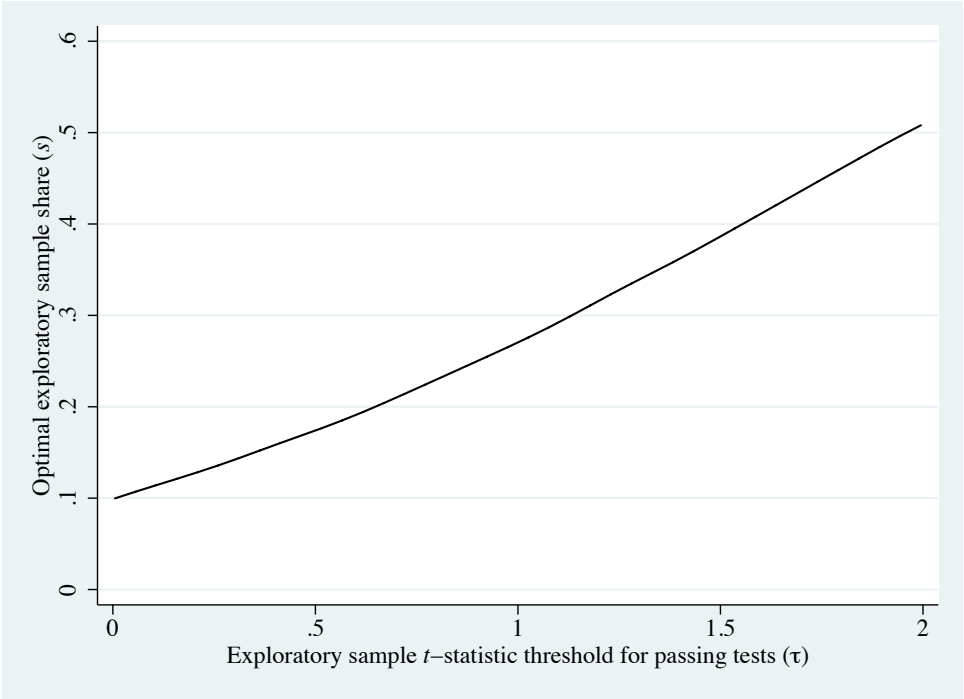
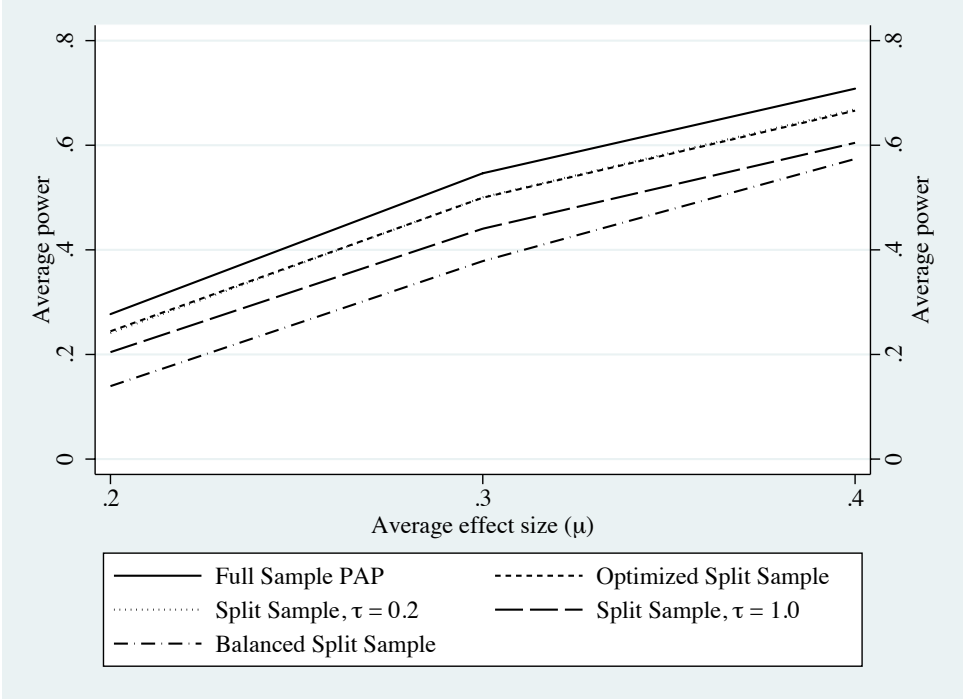
Figures and Tables

Figure 1: Optimal Exploratory Sample Share by Pass-on Threshold



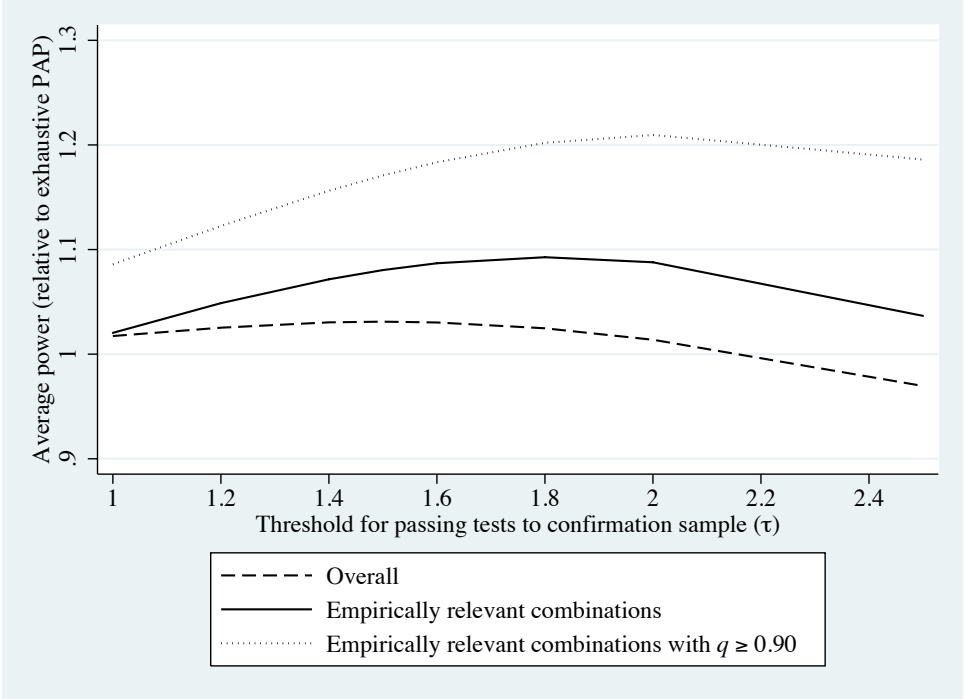
Notes: $H = 20$ hypotheses tested or explored.

Figure 2: PAP and Split-Sample Power



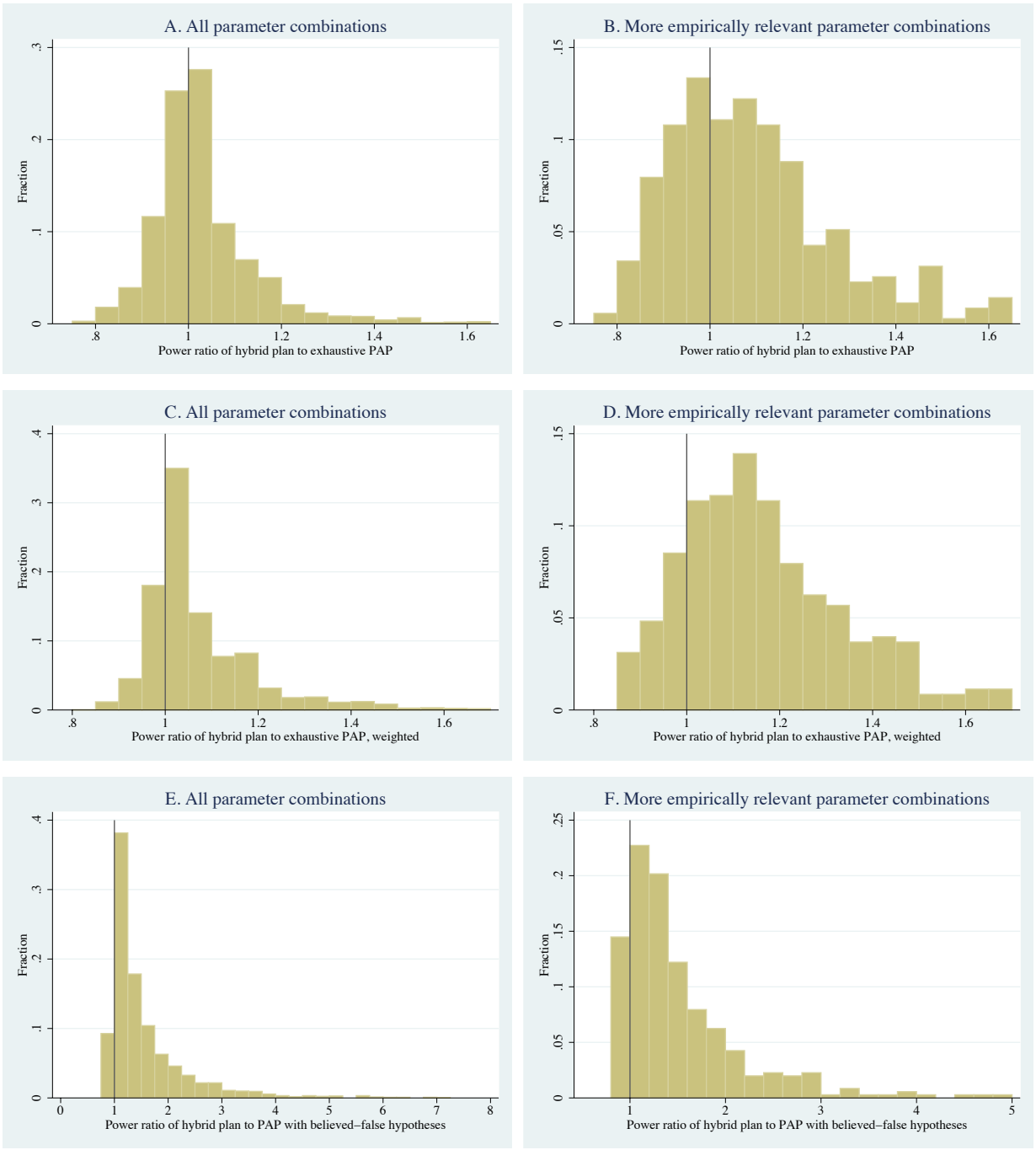
Notes: Exploratory share $s = 0.15$. See Table 1 for values of total hypotheses, average effect size, and share false.

Figure 3: Average Hybrid Power by Pass-on Threshold



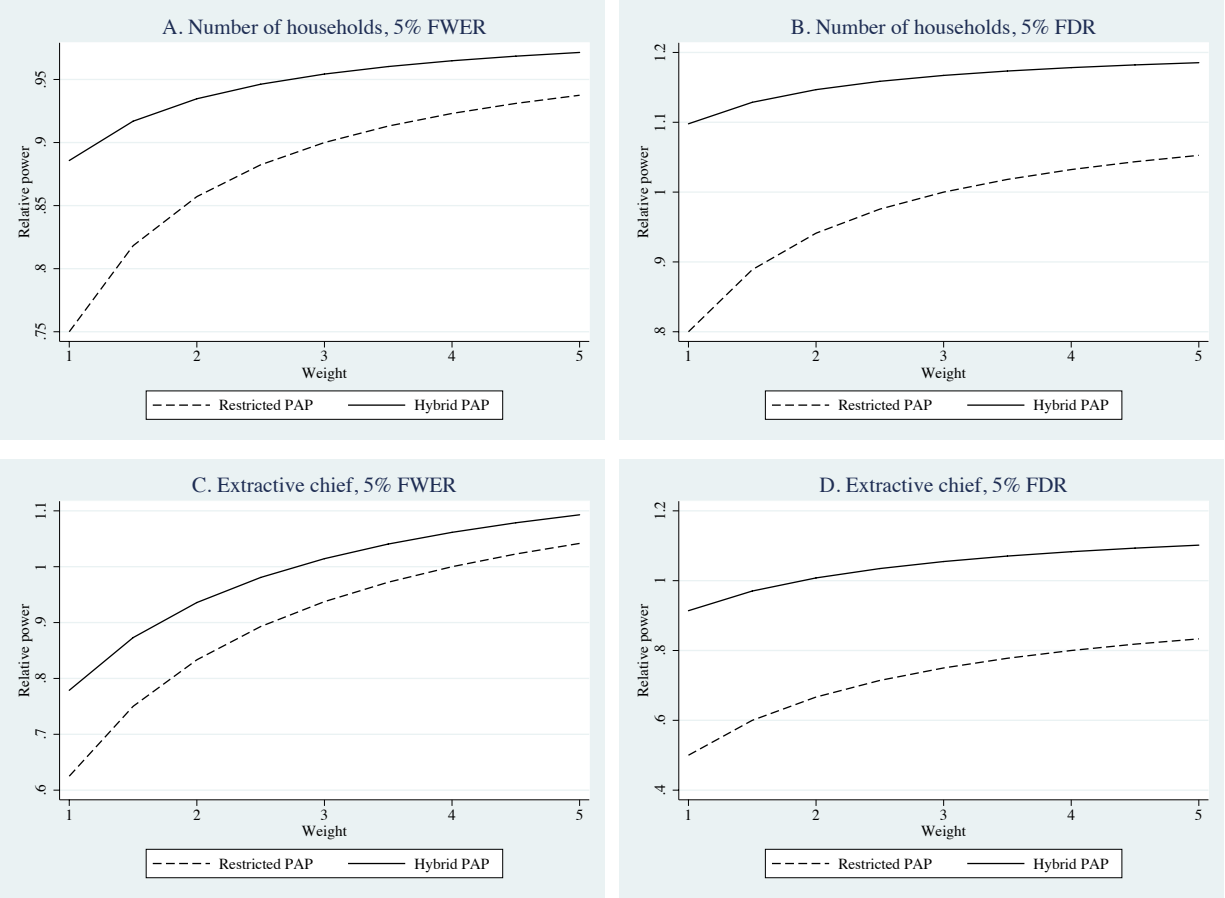
Notes: Exploratory share $s = 0.35$.

Figure 4: Distribution of Relative Power of Hybrid Plan



Notes: Threshold for passing tests to confirmation sample is fixed at $\tau = 1.6$; exploratory sample share is fixed at $s = 0.35$.

Figure 5: Weighted Rejections in GoBiFo



Notes: Power is normalized relative to an exhaustive PAP.

Table 1: Simulation Parameter Values

Parameter	Values	Mean	Std Dev
P(false | believed false) (p)	0.25, 0.50, 0.80, 1.00	0.64	0.33
P(true | believed true) (q)	0.80, 0.82, 0.84, …, 1.00	0.90	0.07
Average effect size (μ)	0.2, 0.3, 0.4, 0.5	0.35	0.13
Total hypotheses (H)	10, 20, 50, 100	45	40.4
Share believed false (H_1/H)	0.10, 0.20, 0.50	0.27	0.21

Table 2: Average Power by Exploratory Sample Share for Hybrid Plan with Threshold $\tau = 1.6$

Exploratory sample share	(1)	(2)	(3)	(4)	(5)
0.10	0.93	0.97	1.10	1.26	1.29
0.15	0.97	1.02	1.13	1.28	1.32
0.20	0.99	1.05	1.15	1.28	1.32
0.25	1.01	1.07	1.17	1.29	1.35
0.30	1.02	1.08	1.18	1.29	**1.36**
0.35	1.03	**1.09**	**1.18**	**1.30**	1.35
0.40	**1.03**	1.08	1.18	1.29	1.32
0.45	1.03	1.07	1.17	1.29	1.32
0.50	1.02	1.05	1.16	1.29	1.29
0.75	0.93	0.90	1.05	1.24	1.15
Parameter restrictions					
Total hypotheses (H)		≥ 50	≥ 50	≥ 50	$= 100$
Share believed false (H_1/H)		≤ 0.20	≤ 0.20	≤ 0.20	$= 0.10$
Avg effect size (μ)		≤ 0.3	≤ 0.3	≤ 0.3	$= 0.2$
P(true | believed true) (q)			≥ 0.90	≥ 0.96	≥ 0.96
Combinations	21,120	3,520	1,920	720	30

Notes: Each cell reports, for a given exploratory sample share, the average power ratio of a hybrid plan to an exhaustive PAP. The hybrid plan has a fixed threshold for passing tests to the confirmation sample of $\tau = 1.6$. Column maximums are in bold.

Table 3: Determinants of Optimal Threshold for Passing Tests to Confirmation Sample

Dependent variable:	Optimal threshold for passing tests (τ)		
	(1)	(2)	(3)
P(true | believed true) (q)	0.66	0.76	0.75
	(60.5)	(35.3)	(25.7)
Avg effect size (μ)	-0.37	-0.34	-0.32
	(-34.5)	(-15.6)	(-10.8)
P(false | believed false) (p)	0.26	0.36	0.40
	(23.8)	(16.8)	(13.5)
Total hypotheses (H)	0.33	0.11	0.11
	(30.3)	(5.3)	(3.8)
Share believed false (H_1/H)	0.06	0.05	0.03
	(5.3)	(2.4)	(1.2)
Parameter restrictions			
Total hypotheses (H)		≥ 50	≥ 50
Share believed false (H_1/H)		≤ 0.20	≤ 0.20
Avg effect size (μ)		≤ 0.3	≤ 0.3
P(true | believed true) (q)			≥ 0.90
R^2	0.75	0.84	0.84
N (combinations)	2,112	352	192

Notes: In all regressions the dependent variable is the optimal threshold for passing tests to the confirmation sample. Parentheses contain t-statistics. The exploratory sample share is set to $s = 0.35$ in all cases.

Table 4: Determinants of Hybrid Plan Power (Relative to Exhaustive PAP)

Dependent variable:	Relative hybrid plan power		
	(1)	(2)	(3)
P(true | believed true) (q)	0.54	0.74	0.64
	(38.2)	(34.6)	(23.6)
Avg effect size (μ)	-0.44	-0.42	-0.59
	(-31.4)	(-19.5)	(-21.8)
P(false | believed false) (p)	0.24	0.32	0.31
	(17.2)	(15.2)	(11.2)
Total hypotheses (H)	0.10	0.02	0.04
	(7.4)	(1.1)	(1.5)
Share believed false (H_1/H)	0.13	0.13	0.05
	(9.1)	(6.2)	(1.8)
Parameter restrictions			
Total hypotheses (H)		≥ 50	≥ 50
Share believed false (H_1/H)		≤ 0.20	≤ 0.20
Avg effect size (μ)		≤ 0.3	≤ 0.3
P(true | believed true) (q)			≥ 0.90
R^2	0.58	0.84	0.86
N (combinations)	2,112	352	192

Notes: In all regressions the dependent variable is the power ratio of a hybrid plan relative to an exhaustive PAP. Parentheses contain t-statistics. The exploratory sample share is set to $s = 0.35$ in all cases.

Table 5: GoBiFo Hypotheses and Baseline Covariates

	t-stat	FWER *p*-val
A. "Hardware" Hypotheses		
1 GoBiFo project implementation	12.8	0.000
2 Participation in GoBiFo improves local project infrastructure	5.2	0.000
3 Participation in GoBiFo improves general economic welfare	8.0	0.000
B. "Software" Hypotheses		
4 Participation in GoBiFo increases collective action and contributions to	0.3	0.980
5 GoBiFo increases inclusion and participation in community planning and implementation, especially for poor and vulnerable groups; GoBiFo norms spill over into other types of community decisions, making them more	0.1	0.980
6 GoBiFo changes local systems of authority, including the roles and public perceptions of traditional leaders (chiefs) versus local elected government	1.5	0.664
7 Participation in GoBiFo increases trust	0.9	0.913
8 Participation in GoBiFo builds and strengthens community networks	0.8	0.913
9 Participation in GoBiFo increases access to information about local	1.0	0.913
10 GoBiFo increases public participation in local governance	2.0	0.315
11 By increasing trust, GoBiFo reduces crime and conflict in the community	0.2	0.980
12 GoBiFo changes political and social attitudes, making individuals more liberal toward women, more accepting of other ethnic groups and	1.0	0.913
C. Candidate Heterogeneity Dimensions		
1 Number of households*		
2 Distance to the road*		
3 Ethnolinguistic fractionalization		
4 Baseline presence of an extractive chief		
5 War exposure		
6 Anyone in the village owns/has owned a slave		
7 Average education		
8 Bombali district*		

Notes: * indicates dimension of explicit stratification. Values of *t*-statistics and FWER *p*-values are taken from Table 2 of Casey et al. (2012).

Table 6: Rejections in GoBiFo under different analysis plans

		5% FWER	10% FWER	5% FDR
Prespecification	Method	(1)	(2)	(3)
As written in Casey et al. (2012)	PAP	3	3	3
	Hybrid	5.3	5.9	8.4
		(1.0)	(1.1)	(1.6)
Hardware only	PAP	3	3	3
	Hybrid	5.5	6.1	8.7
		(1.1)	(1.2)	(1.7)
All ATE	PAP	4	4	4
	Hybrid	5.5	6.0	8.3
		(0.9)	(1.0)	(1.5)
Number of households + all ATE	PAP	6	7	8
	Hybrid	7.1	7.4	11.0
		(0.6)	(0.8)	(1.2)
Extractive chief + all ATE	PAP	5	5	5
	Hybrid	6.2	6.5	9.1
		(0.8)	(0.9)	(1.5)
Bombali district + all ATE	PAP	6	6	7
	Hybrid	6.5	6.7	9.2
		(0.6)	(0.8)	(1.1)
Distance to road + all ATE	PAP	4	4	4
	Hybrid	5.4	5.8	7.7
		(0.8)	(1.0)	(1.4)
Anyone owned slaves + all ATE	PAP	4	4	4
	Hybrid	5.4	5.8	7.8
		(0.8)	(1.0)	(1.4)
Years of education + all ATE	PAP	4	4	4
	Hybrid	5.4	5.8	7.8
		(0.8)	(1.0)	(1.4)
War exposure + all ATE	PAP	4	4	4
	Hybrid	5.4	5.8	7.8
		(0.8)	(1.0)	(1.4)
Ethnolinguistic fractionalization + all ATE	PAP	4	4	4
	Hybrid	5.4	5.8	7.8
		(0.9)	(1.0)	(1.4)
All heterogeneity in PAP	PAP	8	9	10
All heterogeneity and all candidate dependent variables in PAP*	PAP	5	5	9

Notes: Each cell presents the total rejections for a given prespecification plan and multiplicity adjustment. Columns represent multiplicity adjustments while rows represent alternative prespecifications. Parentheses contain standard deviations of results across random sample splits. * indicates that only rejections of hypothesis indices are reported.

Table 7: Rejections in GoBiFo under different analysis plans and 5% FWER

Prespecification	Analysis	Full sample *t*-statistic	PAP as written		Hardware-only PAP		All ATEs + one heterogeneity dimension			All heterogeneity dimensions	All dependent variables
Method			PAP	Hybrid	PAP	Hybrid	PAP	Hybrid		PAP	PAP
								Pre-specified	Split sample		
Hypothesis			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Average treatment effects											
GoBiFo implementation	Endline	12.57	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo improves local project infrastructure	Endline	5.27	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo improves general economic welfare	Endline	8.01	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo increases public participation in local governance	Panel	4.42	0	0.66	0	0.69	1.00	1.00		1.00	1.00
B. Heterogeneous treatment effects, by village size											
GoBiFo improves local project infrastructure	Panel	-3.79	0	0.17	0	0.20	1.00	1.00	0.14	1.00	0
GoBiFo increases collective action and contributions to public goods	Panel	2.68	0	0	0	0	0	0	0	0	0
GoBiFo increases inclusion and participation in community planning and implementation…	Panel	-2.43	0	0	0	0	0	0	0	0	0
Participation in GoBiFo increases access to information about local governance	Panel	-3.86	0	0.21	0	0.24	1.00	1.00	0.19	1.00	0
GoBiFo increases public participation in local governance	Panel	-3.17	0	0.01	0	0.01	0	0	0.00	0	0

Prespecification	Analysis	Full sample *t*-statistic	PAP as written		Hardware-only PAP		All ATEs + one heterogeneity dimension			All heterogeneity dimensions	All dependent variables
Method			PAP	Hybrid	PAP	Hybrid	PAP	Hybrid		PAP	PAP
								Pre-specified	Split sample		
Hypothesis			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C. Heterogeneous treatment effects, by presence of an extractive chief											
GoBiFo improves local project infrastructure	Panel	-2.56	0	0	0	0	0	0	0	0	0
Participation in GoBiFo increases trust	Panel	3.66	0.222	0.00	0.266	1.00	1.00	0.18	0.00	0	0
D. Heterogeneous treatment effects, by Bombali district											
GoBiFo improves local project infrastructure	Endline	-3.91	0	0.19	0	0.21	1.00	1.00	0.16	1.00	0
GoBiFo implementation	Panel	-2.97	0	0	0	0	0	0	0	0	0
GoBiFo improves general economic welfare	Panel	4.76	0	0.83	0	0.83	1.00	1.00	0.79	1.00	1.00

Notes: Each cell reports the frequency of rejections for a given hypothesis under each potential prespecification. PAPs are deterministic while hybrid results are probabilistic based on random sample splits. FWER controlled at 5%. Column (6) indicates the power if the prespecification is all ATEs plus the relevant heterogeneity dimension, while Column (7) indicates the power if the prespecification is all ATEs plus a different heterogeneity dimension (averaged across all candidate heterogeneity dimensions).

Table 8: Rejections in GoBiFo under different analysis plans and 5% FDR

Prespecification	Analysis	Full sample *t*-statistic	PAP as written		Hardware-only PAP		All ATEs + one heterogeneity dimension			All heterogeneity dimensions	All dependent variables
Method			PAP	Hybrid	PAP	Hybrid	PAP	Hybrid		PAP	PAP
								Pre-specified	Split sample		
Hypothesis			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Average treatment effects											
GoBiFo implementation	Endline	12.57	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo improves local project infrastructure	Endline	5.27	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo improves general economic welfare	Endline	8.01	1.00	1.00	1.00	1.00	1.00	1.00		1.00	1.00
GoBiFo increases public participation in local governance	Panel	4.42	0	0.88	0	0.88	1.00	1.00		1.00	1.00
B. Heterogeneous treatment effects, by village size											
GoBiFo improves local project infrastructure	Panel	-3.79	0	0.57	0	0.58	1.00	1.00	0.51	1.00	1.00
GoBiFo increases collective action and contributions to public goods	Panel	2.68	0	0.1	0	0.13	1.00	1.00	0.05	0	0
GoBiFo increases inclusion and participation in community planning and implementation…	Panel	-2.43	0	0.01	0	0.02	0	0.52	0.00	0	0
Participation in GoBiFo increases access to information about local governance	Panel	-3.86	0	0.55	0	0.57	1.00	1.00	0.50	1.00	1.00
GoBiFo increases public participation in local governance	Panel	-3.17	0	0.57	0	0.59	1.00	1.00	0.48	1.00	0

Prespecification	Analysis	Full sample *t*-statistic	PAP as written		Hardware-only PAP		All ATEs + one heterogeneity dimension			All heterogeneity dimensions	All dependent variables
Method			PAP	Hybrid	PAP	Hybrid	PAP	Hybrid		PAP	PAP
								Pre-specified	Split sample		
Hypothesis			(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
C. Heterogeneous treatment effects, by presence of an extractive chief											
GoBiFo improves local project infrastructure	Panel	-2.56	0	0.03	0	0.06	0	0.92	0.01	0	0
Participation in GoBiFo increases trust	Panel	3.66	0	0.73	0	0.74	1.00	1.00	0.69	1.00	1.00
D. Heterogeneous treatment effects, by Bombali district											
GoBiFo improves local project infrastructure	Endline	-3.91	0	0.58	0	0.60	1.00	1.00	0.56	1.00	1.00
GoBiFo implementation	Panel	-2.97	0	0.36	0	0.41	1.00	1.00	0.24	0	0
GoBiFo improves general economic welfare	Panel	4.76	0	0.91	0	0.91	1.00	1.00	0.91	1.00	1.00

Notes: Each cell reports the frequency of rejections for a given hypothesis under each potential prespecification. PAPs are deterministic while hybrid results are probabilistic based on random sample splits. FDR controlled at 5%. Column (6) indicates the power if the prespecification is all ATEs plus the relevant heterogeneity dimension, while Column (7) indicates the power if the prespecification is all ATEs plus a different heterogeneity dimension (averaged across all candidate heterogeneity dimensions).

Appendix

Not For Print Publication

A1 Optimal pure PAP

Suppose the researcher has access to H hypotheses which may be true or false, and false hypotheses are characterized by a data generating process where $\beta_h = b > 0$. If priors over the hypotheses being false and weights over rejecting individual hypotheses are uniform, we prove that the optimal PAP would include all of them.

Suppose one would prespecify $H-1$ hypotheses and was considering prespecifying one more. One would be willing to prespecify all H hypotheses if

$$\mathbb{P}(t_h > \mathrm{t}_{\frac{\alpha}{2H}} | \beta_h = b) > (H-1)\big[\mathbb{P}(t_k > \mathrm{t}_{\frac{\alpha}{2(H-1)}} | \beta_k = b) - \mathbb{P}(t_k > \mathrm{t}_{\frac{\alpha}{2H}} | \beta_k = b)\big]$$

Since $\mathbb{P}(t_h > \tau | \beta_h = b) = \mathbb{P}(t_k > \tau | \beta_k = b)\ \forall\ k, h$ with uniform hypotheses, this implies that

$$H\mathbb{P}(t_h > \mathrm{t}_{\frac{\alpha}{2H}} | \beta_h = b) - (H-1)\mathbb{P}(t_h > \mathrm{t}_{\frac{\alpha}{2(H-1)}} | \beta_h = b) > 0 \tag{10}$$

The left side equals 0 when $b = 0$. Therefore, if we show that the derivative of Equation (10) is increasing with respect to $b\ \forall\ H$, we can conclude that for $b > 0$ additional hypotheses should be included in the PAP. Letting $\mathbb{P}(t_h > \mathrm{t}_{\frac{\alpha}{2H}} | \beta_h = b) = (1 - \Phi(\mathrm{t}_{\frac{\alpha}{2H}} - b))$ and $\mathrm{t}_{\frac{\alpha}{2H}} = \Phi^{-1}(1 - \frac{\alpha}{2H})$, where Φ represents the normal CDF, the derivative of Equation (10) wrt b is given by

$$H\phi(\mathrm{t}_{\frac{\alpha}{2H}} - b) - (H-1)\phi(\mathrm{t}_{\frac{\alpha}{2(H-1)}} - b) > 0$$

where ϕ is the normal PDF. If we rearrange and take logs this condition becomes

$$\ln(H) - 0.5\ \mathrm{t}^2_{\frac{\alpha}{2H}} + b\ \mathrm{t}_{\frac{\alpha}{2H}} > \ln(H-1) - 0.5\ \mathrm{t}^2_{\frac{\alpha}{2(H-1)}} + b\ \mathrm{t}_{\frac{\alpha}{2(H-1)}} \tag{11}$$

This reveals that the value of including hypotheses is increasing in b if $\ln(H) - 0.5\ \mathrm{t}^2_{\frac{\alpha}{2H}} + b\ \mathrm{t}_{\frac{\alpha}{2H}}$ is increasing in H. To verify this, we differentiate the left side of Equation (11) with respect to H and observe that we should include hypotheses if

$$\frac{1}{H} + \frac{b(\frac{\alpha}{2H^2})}{\phi(\mathrm{t}_{\frac{\alpha}{2H}})} > \frac{(\mathrm{t}_{\frac{\alpha}{2H}})(\frac{\alpha}{2H^2})}{\phi(\mathrm{t}_{\frac{\alpha}{2H}})}$$

Now note that

$$1-\Phi(x) \leq \frac{\phi(x)}{x} \text{ since } x(1-\Phi(x)) = \int_x^\infty x\phi(u)du \leq \int_x^\infty u\phi(u)du = -\phi(u)|_x^\infty = \phi(x)$$

Thus we see that $\frac{1}{H} = \frac{\frac{\alpha}{2H^2}}{1-\Phi(\mathrm{t}_{\frac{\alpha}{2H}})} \geq \frac{(\mathrm{t}_{\frac{\alpha}{2H}})(\frac{\alpha}{2H^2})}{\phi(\mathrm{t}_{\frac{\alpha}{2H}})}$, so it is sufficient to show that

$$\frac{1}{H} + \frac{b(\frac{\alpha}{2H^2})}{\phi(\mathrm{t}_{\frac{\alpha}{2H}})} > \frac{1}{H}$$

which clearly holds for $b > 0$ and all H. Thus, a researcher facing a data generating process with uniform hypotheses and $b > 0$ will want to prespecify all hypotheses.

A2 Optimal Exploratory Share and Pass-on Thresholds

Suppose we have H hypotheses, $H-1$ of which are true, and there are N total observations. We are interested in the power on the remaining false hypothesis, for which we assume $\beta = b$. If we use a Bonferroni correction to control FWER, assign fraction s of the data in the exploratory sample, and pass on all hypotheses where $|t_h^e| > \tau$, then the power function for our false hypothesis can be represented as

$$\begin{aligned}
&\sum_{k_1=0}^{H} \binom{H}{k_1} \Big(\Phi(-\tau) + \big(1-\Phi(\tau)\big)\Big)^{k_1} \cdot \big(\Phi(\tau) - \Phi(-\tau)\big)^{H-k_1} \\
&\quad \cdot \Big[\Phi(-\tau - b\sqrt{sN/4}) \cdot \Phi(-\mathrm{t}_{\alpha/k_1+1} - b\sqrt{(1-s)N/4}) \\
&\quad + \big(1-\Phi(\tau - b\sqrt{sN/4})\big) \cdot \big(1-\Phi(\mathrm{t}_{\alpha/k_1+1} - b\sqrt{(1-s)N/4})\big)\Big]
\end{aligned}$$

where $\mathrm{t}_{\alpha/k_1+1}$ is the *t* critical value for a test of size $\frac{\alpha}{k_1+1}$. Taking the first order condition with respect to s, one can find an approximate closed form for the optimal exploratory share. Let $m(.)$ represent the Mills ratio; the optimal exploratory share is approximately a function of the expected Mills ratios in the two stages, given by

$$\sqrt{\frac{1-s^*}{s^*}} \approx \mathbb{E}_{k_1}\Big[\frac{m(\tau - b\sqrt{s^*N/4})}{m(\mathrm{t}_{\alpha/k_1+1} - b\sqrt{(1-s^*)N/4})}\Big]$$

Since $m'(.) < 0$, the optimal exploratory share is increasing in the τ threshold.

A3 Optimal Type I Error Allocation

To motivate the optimal allocation of type I error, consider a simple case in which a researcher tests two hypotheses. The conventional allocation of type I error splits it evenly between the two hypotheses, with each receiving $\alpha = 0.025$. If a researcher were confident that first hypothesis was false but skeptical that second one was false, she might test only the first one in the confirmation sample, implicitly assigning $\alpha = 0.05$ type I error to it and $\alpha = 0$ type I error to the second one. If she were only somewhat more confident about the first hypothesis rejecting than the second, however, then the optimal allocation of type I error to each test would not be binary in nature. She might, for example, allocate $\alpha = 0.04$ type I error to the first hypothesis and $\alpha = 0.01$ type I error to the second hypothesis.

To solve for the optimal allocation of type I error across tests we return to the Agnostic Evaluation Problem. Letting R_h represent an indicator for hypothesis h rejecting, the problem is

$$\max \sum_{h=1}^{H} R_h$$

If the researcher knows the sampling distributions of the test statistics, which we assume are *t*-statistics in this case, then this problem becomes

$$\max_{\alpha_h} \sum_{h=1}^{H} F_t(F_t^{-1}(1-\alpha_h) - \mathbb{E}[t_h^c]),$$

where F_t represents the t cumulative distribution function and $\mathbb{E}[t_h^c]$ represents the expectation of hypothesis *h*'s *t*-statistic in the confirmation sample. Solving this problem yields the first order condition (FOC)

$$\frac{f_t(F_t^{-1}(1-\alpha_h) - \mathbb{E}[t_h^c])}{f_t(F_t^{-1}(1-\alpha_h))} = \frac{f_t(F_t^{-1}(1-\alpha_{h'}) - \mathbb{E}[t_{h'}^c])}{f_t(F_t^{-1}(1-\alpha_{h'}))} \;\; \forall \;\; h, h',$$

where f_t represents the t probability density function. The numerator in the FOC represents the marginal return, in terms of rejection probability, to relaxing the t critical value for hypothesis h. The denominator in the FOC represents the marginal change in the size of the test, α_h, from relaxing the t critical value for hypothesis h. The FOC thus reveals that type I error should first be allocated to the test with the highest return per unit of type I error, and at the optimum the return to allocating type I error should be equal across all tests with positive type I error allocations.

In practice the researcher does not know $\mathbb{E}[t_h^c]$ – if she did there would be no need to run the experiment. The obvious solution is to use the exploratory sample coefficient estimate, $\hat{\beta}_h^e$, to estimate $\mathbb{E}[t_h^c] = \hat{\beta}_h^e / \hat{\sigma}_h^c$ (where $\hat{\sigma}_h^c$ is a consistent estimator of $\hat{\beta}_h^c$'s standard error). This estimate of $\mathbb{E}[t_h^c]$ is consistent, but in any finite sample it suffers from a regression-to-the-mean problem: Large values of $\hat{\beta}_h^e$ tend to be large both because the true β_h is non-zero and because there has been a shock in the same direction as the coefficient. Using the raw estimate $\hat{\beta}_h^e$ thus tends to over allocate type I error to hypotheses with large exploratory sample *t*-statistics and under allocate it to hypotheses with modest exploratory sample *t*-statistics. This suggests applying a shrinkage estimator to the exploratory sample coefficient estimates.

The shrinkage estimator we consider is the Empirical Bayes estimator. This estimator applies Bayes Theorem:

$$\mathbb{P}(\beta_h | \hat{\beta}_h^e) = \frac{\mathbb{P}(\hat{\beta}_h^e | \beta_h) \cdot \mathbb{P}(\beta_h)}{\mathbb{P}(\hat{\beta}_h^e)}.$$

Using the empirical distribution of the coefficients from the exploratory sample, $\hat{\beta}_j^e$, and applying the law of iterated expectations we estimate:

$$\mathbb{P}(\beta_h | \hat{\beta}_h^e) = \frac{\mathbb{P}(\hat{\beta}_h^e | \beta_h) \cdot H^{-1}}{\sum_{j=1}^{H} \mathbb{P}(\hat{\beta}_h^e | \hat{\beta}_j^e) \cdot H^{-1}} = \frac{\mathbb{P}(\hat{\beta}_h^e | \beta_h)}{\sum_{j=1}^{H} \mathbb{P}(\hat{\beta}_h^e | \hat{\beta}_j^e)}.$$

Note that we only evaluate $\mathbb{P}(\beta_h | \hat{\beta}_h^e)$ for values of β_h corresponding to points of support in the empirical distribution of $\hat{\beta}_j^e$, and the denominator is a constant that ensures the posterior probabilities sum to one.[43] To understand the estimator's operation, consider the largest $\hat{\beta}_j^e$, $\hat{\beta}_{max}^e$. The posterior for β_{max} is centered below $\hat{\beta}_{max}^e$ because $\hat{\beta}_{max}^e$ is the upper bound of the support for any posterior. Other coefficient estimates $\hat{\beta}_j^e$ "pull down" $\mathbb{E}[\beta_{max}]$, with each posterior point of support $\hat{\beta}_j^e$ receiving weight $\mathbb{P}(\hat{\beta}_{max}^e | \beta_{max} = \hat{\beta}_j^e)$. Thus the estimator "shrinks" larger coefficients towards the empirical mean of the coefficients. With the posterior distribution $\mathbb{P}(\beta_h | \hat{\beta}_h^e)$ it is trivial to estimate $\mathbb{E}[t_h^c]$ and compute the FOCs. In practice we assign type I error based on the expectation of the FOC (using the posterior distribution) rather than the FOC evaluated at the estimate of $\mathbb{E}[t_h^c]$.[44]

In summary, by solving the Agnostic Evaluation Problem and applying a shrinkage estimator, we can allocate type I error to hypotheses in incremental amounts using information from the exploratory sample. In our simulations, when expected *t*-statistics are modest or the share of

[43] To compute $\mathbb{P}(\hat{\beta}_h^e | \beta_h)$ we appeal to the Central Limit Theorem and assume a normal distribution for $\hat{\beta}_h^e$.

[44] This seems desirable since the FOC is not linear in $\mathbb{E}[t_h^c]$.

hypotheses that are false is low, this optimized type I error allocation results in modest increases in power over a rule that passes all hypotheses with *t*-statistics exceeding a threshold τ. However, the power differences are small when comparing against "loose" thresholds that pass on most tests.

A4 Proof of Proposition 1

For the proposition, it suffices to demonstrate that there is a hybrid plan that uses split-sample search for at least one hypothesis and that is superior to the optimal pure PAP. Suppose there exists an optimal pure PAP with an internal solution, that is, that $\exists\ \mathcal{H}_p \subset \mathcal{H}$ s.t. $h \in \mathcal{H}_p$ are prespecified and $h \notin \mathcal{H}_p$ are not. For each hypothesis h, define ε_h by

$$\begin{aligned}\varepsilon_h = \quad & \sum_{j \in \mathcal{H}_p} u_j p_j \Big[\mathbb{P}(\hat{t}_j > \mathrm{t}_{\frac{\alpha}{2|\mathcal{H}_p|}} | \beta_j = b) - \mathbb{P}(\hat{t}_j > \mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} | \beta_j = b)\Big] \\ & - u_h p_h \mathbb{P}(\hat{t_h} > \mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p+1|)}} | \beta_h = b)\end{aligned}$$

so that hypotheses with $\varepsilon_h < 0$ would be included in the optimal pure PAP and those with $\varepsilon_h > 0$ would not. Finally, suppose that there exists a "marginal" hypothesis, a, such that for any $\delta > 0$:

$$0 < \varepsilon_a < \delta \tag{12}$$

By definition, the researcher is, in the limit, indifferent between including or omitting hypothesis a from the pure PAP. Now consider adding hypothesis a to the split-sample portion of a hybrid plan that prespecifies $\mathcal{H}_p$. Analyzing hypothesis a in the exploratory sample (and conditionally testing it in the confirmation sample) leads to power gains over the optimal PAP if

$$\begin{aligned}& u_a p_a \mathbb{P}(\hat{t}_a^c > \mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} | \beta_a = b) \mathbb{P}(\hat{t}_a^e > \tau | \beta_a = b) \\ & > \Big[p_a \mathbb{P}(|\hat{t}_a^e| > \tau | \beta_a = b) + (1 - p_a) 2\mathbb{P}(t > \tau)\Big] \\ & \cdot \sum_{j \in \mathcal{H}_p} u_j p_j \Big[\mathbb{P}(\hat{t}_j > \mathrm{t}_{\frac{\alpha}{2|\mathcal{H}_p|}} | \beta_j = b) - \mathbb{P}(\hat{t}_j > \mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} | \beta_j = b)\Big]\end{aligned} \tag{13}$$

Or, using the definition of ε_h, if

$$\begin{aligned}& u_a p_a \mathbb{P}(\hat{t}_a^c > \mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} | \beta_a = b) \mathbb{P}(\hat{t}_a^e > \tau | \beta_a = b) \\ & > \Big[p_a \mathbb{P}(|\hat{t}_a^e| > \tau | \beta_a = b) + (1 - p_a) 2\mathbb{P}(t > \tau)\Big] \cdot \Big[u_a p_a \mathbb{P}(\hat{t}_a > \mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} | \beta_a = b) + \varepsilon_a\Big]\end{aligned}$$

Simplifying, we have

$$u_a p_a \Big[\frac{\mathbb{P}(\hat{t}_a^c > \mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} | \beta_a = b) \mathbb{P}(\hat{t}_a^e > \tau | \beta_a = b)}{p_a \mathbb{P}(|\hat{t}_a^e| > \tau | \beta_a = b) + (1-p_a) 2\mathbb{P}(t > \tau)} - \mathbb{P}(\hat{t}_a > \mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} | \beta_a = b) \Big] > \varepsilon_a \quad (14)$$

Let $\Phi(.)$, $\overline{\Phi}(.)$, and $\phi(.)$ represent the CDF, upper-tail CDF, and PDF, respectively, of the standard normal distribution. Let $\sigma_b^c = 2/\sqrt{(1-s)N}$, $\sigma_b^e = 2/\sqrt{sN}$, and $\sigma_b = 2/\sqrt{N}$. Then we can rewrite Equation (14) as

$$u_a p_a \Big[\frac{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} - b/\sigma_b^c) \cdot \overline{\Phi}(\tau - b/\sigma_b^e)}{p_a(\Phi(-\tau - b/\sigma_b^e) + \overline{\Phi}(\tau - b/\sigma_b^e)) + 2(1-p_a)\overline{\Phi}(\tau)} - \overline{\Phi}(\mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} - b/\sigma_b) \Big] > \varepsilon_a$$

Since we can bound ε_a arbitrarily close to zero, it suffices to show that

$$\frac{\overline{\Phi}(\tau - b/\sigma_b^e)}{p_a(\Phi(-\tau - b/\sigma_b^e) + \overline{\Phi}(\tau - b/\sigma_b^e)) + 2(1-p_a)\overline{\Phi}(\tau)} > \frac{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} - b/\sigma_b)}{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} - b/\sigma_b^c)}$$

Now note that the left-hand side achieves a minimum when $p_a = 1$, so we can prove the proposition by proving that

$$\frac{\overline{\Phi}(\tau - b/\sigma_b^e)}{\Phi(-\tau - b/\sigma_b^e) + \overline{\Phi}(\tau - b/\sigma_b^e)} = \frac{1}{\overline{\Phi}(\tau + b/\sigma_b^e)/\overline{\Phi}(\tau - b/\sigma_b^e) + 1} > \frac{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} - b/\sigma_b)}{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} - b/\sigma_b^c)} \quad (15)$$

Choose $s = s^*$ such that $(b/\sigma_b - b/\sigma_b^{c*}) < (\mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} - \mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}})$. Thus $\frac{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{2(|\mathcal{H}_p|+1)}} - b/\sigma_b)}{\overline{\Phi}(\mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} - b/\sigma_b^{c*})} = \eta$, where $\eta < 1$. Applying L'Hôpital's rule to the left-hand side of Equation (15) we see that

$$\begin{aligned} \lim_{\tau \to \infty} \frac{1}{\overline{\Phi}(\tau + b/\sigma_b^{e*})/\overline{\Phi}(\tau - b/\sigma_b^{e*}) + 1} &= \lim_{\tau \to \infty} \frac{1}{\phi(\tau + b/\sigma_b^{e*})/\phi(\tau - b/\sigma_b^{e*}) + 1} \\ = \lim_{\tau \to \infty} \frac{1}{e^{-2\tau b/\sigma_b^{e*}} + 1} &= 1 \end{aligned}$$

Thus we can choose $\tau = \tau^*$ such that $\eta < \frac{1}{\overline{\Phi}(\tau^* + b/\sigma_b^{e*})/\overline{\Phi}(\tau^* - b/\sigma_b^{e*}) + 1} < 1$.[45] These choices of s^* and τ^* satisfy Equation (15), so a hybrid plan that prespecifies $\mathcal{H}_p$ and considers hypothesis a in the split-sample portion using a sufficiently low value of s and a sufficiently high value of τ will outperform the optimal pure PAP in expectation.

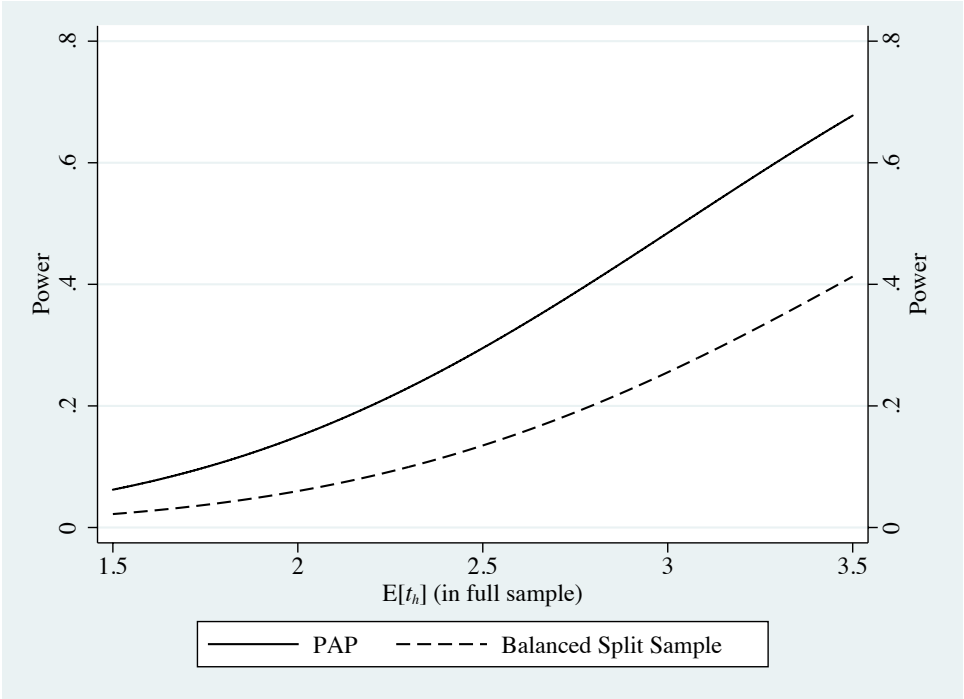
[45] For completeness, note that we may choose δ in Equation (12) to be less than $(\frac{1}{\overline{\Phi}(\tau^* + b/\sigma_b^{e*})/\overline{\Phi}(\tau^* - b/\sigma_b^{e*}) + 1} - \eta) \cdot u_a p_a \cdot \overline{\Phi}(\mathrm{t}_{\frac{\alpha}{|\mathcal{H}_p|+1}} - b/\sigma_b^{c*})$.

In practice there may not be sufficient density of $u_h p_h$ to guarantee the existence of a truly marginal hypothesis with ε_h arbitrarily close to zero, in which case the proposition might not hold. When considering sharpened FWER or FDR control, however, we can substantially relax the marginal hypothesis condition in Equation (12). The intuition is straightforward – with FWER or FDR sharpening, a hypothesis that rejects after multiplicity adjustments does not "consume" any type I error, because we can be virtually certain that the rejection is not false. Setting a high threshold, τ, for passing tests to the confirmation sample ensures that tests will reject at the confirmation stage with probability approaching one and impose no multiple testing penalty on the prespecified hypotheses. There is thus near-zero cost to considering the marginal hypothesis in the split-sample when using a high value of τ.[46] As long as there exists some probability of a large effect size, then a hybrid plan with a high τ threshold that considers the marginal hypothesis in the split-sample portion will outperform the pure PAP that omits the marginal hypothesis. For example, suppose that $N = 500$ and that the optimal pure PAP includes $|\mathcal{H}_p| = 100$ hypotheses. If there is a 1 in 100,000 chance of an effect size of 0.3 (0.8) standard deviations in a panel specification (endline comparison), then a hybrid plan that considers the marginal hypothesis, a, using $s = 0.2$ and $\tau = 7$ will outperform the optimal pure PAP as long as the average utility weight $\overline{u}_h$ for hypotheses in $\mathcal{H}_p$ is no more than ten times larger than u_a.[47]

[46] With FWER or FDR sharpening, the term that premultiplies the summation in Equation (13) changes from the probability that hypothesis a passes to the confirmation stage to the probability that hypothesis a passes to the confirmation stage and fails to reject. For large τ this probability approaches zero.

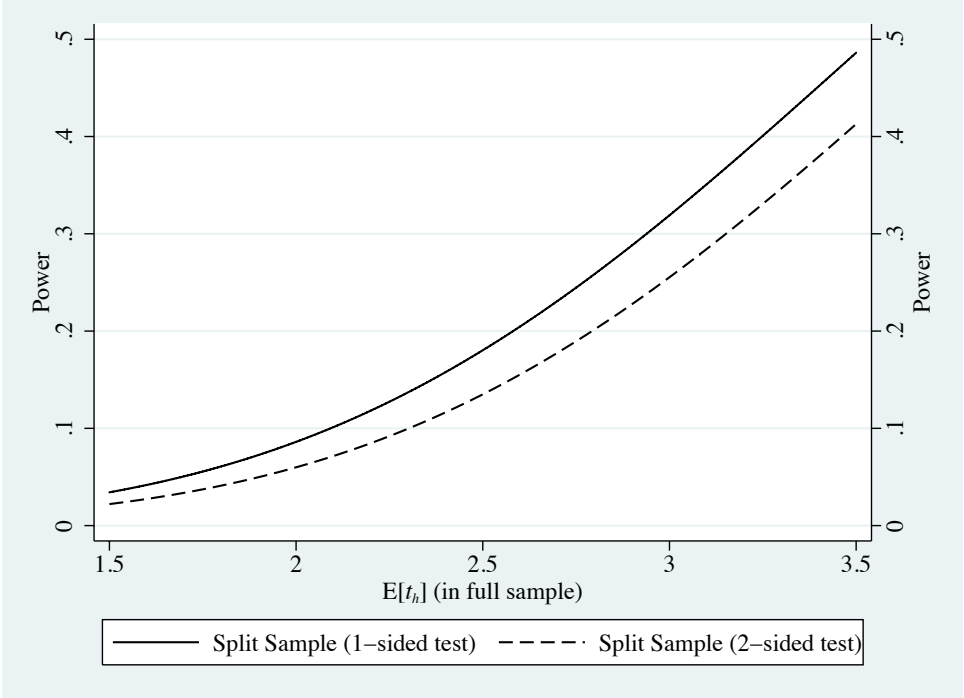
[47] For this calculation we assume that the prior distribution on β_a is weakly decreasing in b. We also make the worst-case assumptions that $p_h \approx 1$ for all hypotheses in $\mathcal{H}_p$ and that $\beta_h \approx \mathrm{t}_{\frac{\alpha}{2|\mathcal{H}_p|}}$. These assumptions are extremely conservative. Stata code to calculate bounds for different parameter values is available upon request.

Figure A1: Power of PAP and Balanced Split Sample for Hypothesis *h*



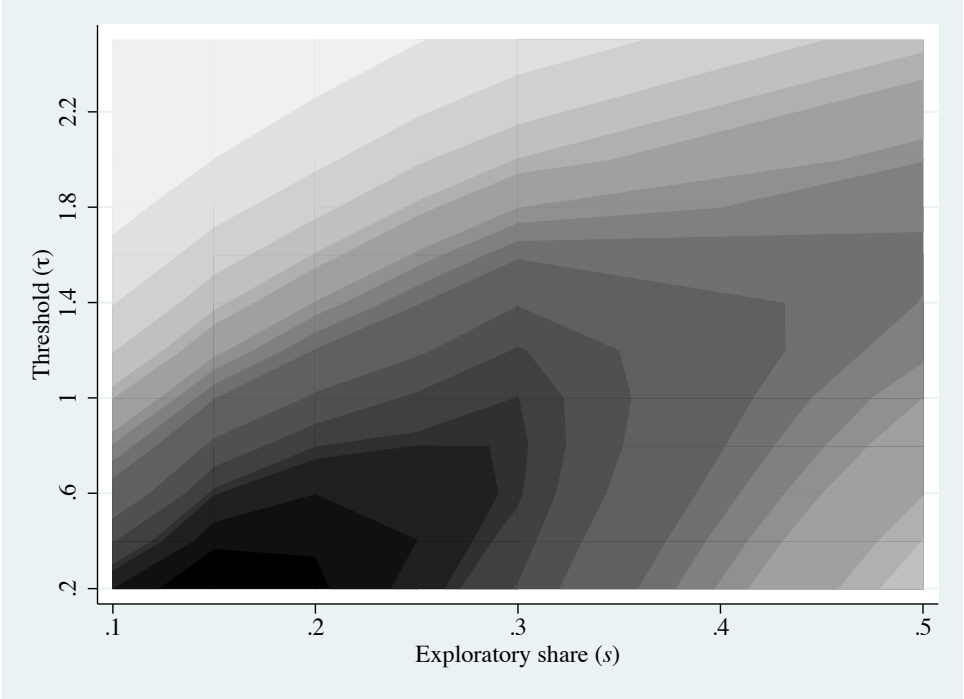
Notes: $H = 20$ hypotheses tested or explored.

Figure A2: Power of 1- and 2-Sided Split Sample for Hypothesis *h*



Notes: $H = 20$ hypotheses tested or explored; $s = 0.50$.

Figure A3: Power by exploratory share and pass-on threshold



Notes: Darker shades connote higher power; average effect size $\mu = 0.3$.

Table A1: Average Power by Exploratory Sample Share (FWER)

Exploratory sample share	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: τ = 1.4			*Panel B: τ = 1.5*		
0.20	1.006	1.054	1.142	1.000	1.053	1.148
0.25	1.021	1.070	1.154	1.017	1.070	1.160
0.30	1.027	**1.074**	**1.158**	1.029	**1.082**	**1.172**
0.35	**1.030**	1.072	1.156	**1.031**	1.080	1.171
0.40	1.028	1.061	1.149	1.030	1.072	1.165
0.45	1.021	1.044	1.139	1.025	1.058	1.157
0.50	1.010	1.021	1.123	1.017	1.037	1.144
	Panel C: τ = 1.6			*Panel D: τ = 1.8*		
0.20	0.994	1.050	1.154	0.979	1.042	1.163
0.25	1.013	1.074	1.173	1.001	1.069	1.184
0.30	1.023	1.083	1.177	1.014	1.081	1.190
0.35	1.030	**1.087**	**1.183**	1.025	**1.093**	1.202
0.40	**1.031**	1.081	1.180	**1.028**	1.093	**1.202**
0.45	1.027	1.069	1.174	1.028	1.086	1.200
0.50	1.020	1.050	1.159	1.023	1.071	1.189
Parameter restrictions						
Total hypotheses (H)		≥ 50	≥ 50		≥ 50	≥ 50
Share believed false (H_1/H)		≤ 0.20	≤ 0.20		≤ 0.20	≤ 0.20
Avg effect size (μ)		≤ 0.3	≤ 0.3		≤ 0.3	≤ 0.3
P(true | believed true) (q)			≥ 0.90			≥ 0.90
Combinations	14,784	2,464	1,344	14,784	2,464	1,344

Notes: Each cell reports, for a given exploratory sample share, the average power ratio of a hybrid plan to an exhaustive PAP, controlling FWER. The hybrid plan has a fixed threshold τ for passing tests to the confirmation sample; τ varies by panel. Column maximums are in bold.

Table A2: Average Power by Exploratory Sample Share (FDR)

Exploratory sample share	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: τ = 1.4			*Panel B: τ = 1.5*		
0.20	0.977	1.006	1.125	0.970	1.003	1.133
0.25	0.996	1.030	1.144	0.991	1.029	1.154
0.30	1.007	1.046	**1.154**	1.004	1.048	1.165
0.35	1.014	1.050	1.153	1.012	1.055	1.166
0.40	**1.016**	**1.050**	1.153	**1.016**	**1.057**	**1.169**
0.45	1.015	1.045	1.151	1.016	1.054	1.168
0.50	1.009	1.029	1.138	1.011	1.041	1.157
	Panel C: τ = 1.6			*Panel D: τ = 1.8*		
0.20	0.963	0.999	1.139	0.946	0.987	1.149
0.25	0.985	1.027	1.162	0.970	1.017	1.173
0.30	0.999	1.048	1.175	0.986	1.041	1.188
0.35	1.009	1.057	1.177	0.999	1.053	1.193
0.40	1.014	**1.062**	1.183	1.007	1.064	1.203
0.45	**1.016**	1.062	**1.183**	**1.011**	**1.067**	**1.205**
0.50	1.012	1.051	1.175	1.010	1.062	1.201
Parameter restrictions						
Total hypotheses (H)		≥ 50	≥ 50		≥ 50	≥ 50
Share believed false (H_1/H)		≤ 0.20	≤ 0.20		≤ 0.20	≤ 0.20
Avg effect size (μ)		≤ 0.3	≤ 0.3		≤ 0.3	≤ 0.3
P(true | believed true) (q)			≥ 0.90			≥ 0.90
Combinations	14,784	2,464	1,344	14,784	2,464	1,344

Notes: Each cell reports, for a given exploratory sample share, the average power ratio of a hybrid plan to an exhaustive PAP, controlling FDR. The hybrid plan has a fixed threshold τ for passing tests to the confirmation sample; τ varies by panel. Column maximums are in bold.