A model of multiple hypothesis testing*

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Abstract

Multiple hypothesis testing practices vary widely, without consensus on which are appropriate when. This paper provides an economic foundation for these practices designed to capture leading examples, such as regulatory approval on the basis of clinical trials. In studies of multiple treatments or sub-populations, adjustments may be appropriate depending on scale economies in the research production function, with control of classical notions of compound errors emerging in some but not all cases. In studies with multiple outcomes, indexing is appropriate and adjustments to test levels may be appropriate if the intended audience is heterogeneous. Data on actual costs in the drug approval process suggest both that some adjustment is warranted in that setting and that standard procedures may be overly conservative.

Keywords: Bonferroni, family-wise error rate, false discovery rate, multiple sub-groups, multiple treatments, multiple outcomes, research costs

JEL Codes: C12

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1 Introduction

Hypothesis testing plays a prominent role in evidence-based decision-making. Typically, researchers report results from more than one test, and there has recently been increasing interest in and debate over whether the testing procedures they employ should reflect this in some way—that is, whether some form of multiple hypothesis testing (MHT) adjustment should be applied. As a concrete example, consider pharmaceutical companies reporting the results of clinical trials to regulators when seeking approval to market new drugs: the U.S. regulator (the Food and Drug Administration, FDA) recently released guidelines calling for MHT adjustments on the grounds that omitting them could “increase the chance of false conclusions regarding the effects of the drug” (Food and Drug Administration, 2022). Analogous concerns arise in many other settings. As a result, a number of procedures for MHT adjustment have been proposed, and their statistical properties are well-understood (see e.g. Romano et al., 2010, for an overview).

What is less clear is whether and when these procedures are economically desirable. That is, under what conditions does MHT adjustment lead to better decision-making from the point of view of the actor designing the process? The answer is far from obvious. It is certainly true, for example, that without MHT adjustments, the chance of making at least one type I error increases with the number of tests. But this is analogous to the truism that the more decisions one makes, the more likely one is to make at least one mistake. It is indisputable, but sheds no light on the pertinent questions, which are whether and how the rule for making individual decisions should change with the total number being made. This paper provides a framework for analyzing such questions. We aim for a framework that provides insight into generally relevant principles, and can feasibly be applied to real-world settings of interest—though naturally one should not expect the answers this yields to be the same in every circumstance.

We focus, in particular, on whether and when MHT adjustments arise as a solution to incentive misalignment between a researcher and a mechanism designer. Our interest in this case reflects two primary considerations. The first is substantive: incentives are clearly an issue in real-world cases of interest (e.g. in the drug approval process, which we will use
as a running example). The instinctive concern many seem to have is that without MHT adjustments, the researcher would have an undue incentive to test many hypotheses in the hopes of getting lucky. We would like to formalize and scrutinize that intuition. And the second is pragmatic: to have a theory of MHT adjustments, we must have a theory that rationalizes hypothesis testing at standard levels in the first place, which (as we discuss below) is hard to do convincingly in a non-strategic setting (e.g., Tetenov, 2012, 2016).

Specifically, we study a model in which a benevolent social planner chooses norms with respect to MHT adjustments, taking into account the way this shapes researchers’ incentives. The model embeds two core ideas. First, social welfare is (potentially) affected by the summary recommendations (in particular, hypothesis tests) contained in research studies, as well as by the production of new knowledge per se.\textsuperscript{1} Second, while this makes the research a public good, the costs of producing it are borne privately by the researcher. She decides whether or not to incur these costs and conduct a (pre-specified) experiment based on the private returns to doing so. The planner must, therefore, balance the goals of (i) motivating the production of research and (ii) limiting the possibility of harm due to mistaken conclusions. We represent these preferences with a utility function that includes both ambiguity-averse and expected-utility components (as, for example, in Gilboa and Schmeidler, 1989; Banerjee et al., 2020), which (we show) turn out to have intuitive connections with the statistical concepts of size control and power.

We first show that—when multiplicity takes the form of testing multiple treatments or estimating effects within multiple sub-populations\textsuperscript{2}—these assumptions rationalize separate hypothesis tests based on threshold-crossing protocols. If the planner assigns any positive weight to the production of knowledge per se, the class of optimal testing protocols is the class of unbiased maximin testing protocols, where maximin optimality is closely connected

\textsuperscript{1}We describe the case where hypothesis rejections lead to changes in welfare relative to the status quo, but under a straightforward reinterpretation, the framework can also accommodate situations in which “precise null” results affect welfare.

\textsuperscript{2}These forms of multiplicity are common in practice. For example, the majority of the clinical trials reviewed in Pocock et al. (2002, Table 1) tested for effects in more than one subgroup. In economics, 27 of 124 field experiments published in “top-5” journals between 2007 and 2017 feature factorial designs with more than one treatment (Muralidharan et al., 2020).
to size control and unbiasedness requires the power of protocols to exceed their size. In equilibrium, size and power are then functions of the economic fundamentals in our model. The proposed notion of optimality is a refinement of maximin optimality, which, within the class of maximin protocols, rules out underpowered tests. We then prove that the separate $t$-tests, which are ubiquitous in applied work, are maximin optimal and unbiased, and we provide an explicit characterization of the (unique) optimal critical values in terms of the researcher’s costs, which may in turn depend on the number of hypotheses and other features of the experimental design such as the sample size.

We next characterize the role of multiplicity, drawing two broad conclusions. First, it is generically optimal to adjust testing thresholds (i.e. critical values) for the number of hypotheses. A loose intuition is as follows. The worst states of the world are those in which the status quo of no treatment is best; in these states, a research study has only a downside, and it is desirable to keep the benefits from experimentation low enough that the researcher chooses not to experiment. If the hypothesis testing protocol were invariant to the number of hypotheses being tested, then for sufficiently many hypotheses, this condition would be violated: the researcher’s expected payoff from false positives alone would be high enough to warrant experimentation. Some adjustment for hypothesis count may thus be needed. We believe that this logic aligns fairly well with the lay intuition that researchers should not be allowed to test many hypotheses and then “get credit” for false discoveries. Interestingly, the same logic immediately implies that critical values should adjust for other factors that influence cost (e.g., sample size), though these have not attracted the same degree of attention.

Second (and as this suggests), the research cost function determines exactly how much adjustment is required. One can, in fact, pick a cost function such that no further adjustment is required, as the costs of doing research scale with the number of hypotheses tested in just such a way as to “build in” the needed correction. More generally, optimal testing protocols compensate for residual imbalances in researcher incentives with respect to the number of hypotheses, taking the researcher’s costs into account. As a result, the framework can explain when common criteria emerge as appropriate solutions, and when they do not, as a function of the economic environment. When research costs are fixed, for example, it is
optimal to control the average size of tests (e.g. via a Bonferroni correction), while when costs scale in exact proportion to the number of tests no MHT adjustment is required. Our model also helps to clarify confusion about the boundaries of MHT adjustment and whether researchers should adjust for multiple testing across different studies. The cost-based perspective suggests that MHT adjustments may be appropriate when there are cost complementarities across studies but not otherwise. ³

We then extend the analysis to demonstrate robustness to varying degrees of prior researcher knowledge about the treatment effects, and also to consider cases in which multiplicity takes the form of a single treatment that affects multiple outcomes. Per se, the presence of multiple outcomes does not necessarily imply that the researcher should report multiple test results; if there is a single decision-maker then it is optimal to require the researcher to report a single test based on an index of the outcomes. The economically optimal index takes one of two forms: it coincides with that implied by classical statistical reasoning (in the spirit of Anderson, 2008) when the outcomes represent distinct proxies for the same underlying concept, and is based on economic weights when the outcomes capture distinct contributions to welfare, as for example in Bhatt et al. (2024). If, on the other hand, the research results influence an audience of multiple decision-makers with heterogeneous preferences (in the spirit of Andrews and Shapiro, 2021) then there is a natural rationale for reporting multiple test results, and we show that the resulting problem is isomorphic to our main model, so that earlier results carry through.

To illustrate the quantitative implications of the model we apply it to our running example, regulatory approval by the FDA. Applying the formulae implied by the model to published data on the cost structure of clinical trials, we calculate adjusted critical values that are neither as liberal as unadjusted testing, nor as conservative as those implied by some of the procedures in current use. For example, if the appropriate critical value in the single-hypothesis case is 0.05, then in a representative experiment reporting five tests, the

³A broader principle these results suggest is that optimal MHT adjustments depend on how exactly the hypotheses interact. Our base model emphasizes interactions in the research costs, which (as we discuss below) appear to be meaningful in real-world settings. In the Appendix we also consider interactions of other kinds, through non-linearities in the researcher’s payoff and through interactions in the planner’s objective.
Figure 1: Multiple hypothesis testing adjustment in “top-5” experimental papers

Notes: The left-hand panel reports the share of experimental papers that conduct at least one MHT adjustment, including both indexing and control of compound error rates, by year of publication. (Note that almost all experimental studies have more than one hypothesis). The right-hand panel reports the frequency of each adjustment type, pooling across years. Adjustment types are not mutually exclusive. Authors’ calculations based on a review of publications in the American Economic Review (excluding Papers and Proceedings), Econometrica, the Journal of Political Economy, the Quarterly Journal of Economics, and the Review of Economic Studies.

critical value implied by our framework is 0.021, below the unadjusted value of 0.05 but above the Bonferroni critical value of 0.01. In addition we show that, because costs also scale with the sample size, optimal adjustments must be less conservative for larger samples in order to induce researchers to incur the correspondingly larger costs.

A broad conclusion is that which procedures are optimal depends on the details of the setting. This is true within the range of settings we consider here, and so seems likely to be all the more true for others which we do not. As economists, for example, it is natural to wonder what protocols would be best at economic journals, where the use of MHT adjustment is on the rise (Figure 1) without any clear consensus on when and how it should be done. We discuss in Appendix A the consequences of applying our framework to experimental research in economics, and whether it would be appropriate to do so. More broadly, we expect two principles that emerge from our analysis to be robust. First, costs must matter in any model that justifies MHT as a way of “getting researcher incentives right.” If incentives matter,
then it must be the net incentives, i.e. rewards net of costs, that matter. And second, different kinds of multiplicity may call for different solutions depending on how they map to decision-making.

Our paper draws inspiration from other work using economic models to inform the choice of statistical procedures, in which the preferences and incentives of researchers drive the analysis (e.g., Chassang et al., 2012; Tetenov, 2016; Banerjee et al., 2017; Spiess, 2018; Henry and Ottaviani, 2019; Banerjee et al., 2020; Williams, 2021; McCloskey and Michaillat, 2022; Yoder, 2022). This includes work on scientific communication specifically (e.g., Andrews and Shapiro, 2021; Frankel and Kasy, 2022), several aspects of which have been studied in more recent papers including Bates et al. (2022, 2023) and Kasy and Spiess (2023). None of these papers analyze multiple hypothesis testing, however. The most closely related paper is the insightful work by Tetenov (2016), who shows that t-tests are maximin optimal and uniformly most powerful in the single-hypothesis case. When analyzing the multiple-hypothesis setting, we have to deal with two major technical and conceptual challenges. First, the notions of maximin optimality and the corresponding theoretical results are more complex because the effects of different treatments may have opposite signs. Second, as we show, within the (large) class of maximin optimal protocols, no maximin protocol uniformly dominates all others, requiring a more careful analysis and the development of new notions of (global) optimality suitable for the multiple testing context.

Our paper also relates to an extensive literature at the intersection between decision theory and hypothesis testing, dating back to Wald (1950) and Robbins (1951). Previous work has motivated notions of compound error control in single-agent non-strategic environments; see in particular Kline et al. (2022, 2024) for recent examples in economics based on a Bayesian interpretation of the false discovery rate (FDR), as well as Storey (2003), Lehmann and Romano (2005b), Efron (2008b), and Hirano and Porter (2020) for further examples. We complement this literature (as well as the statistical literature discussed below) by developing a model that explicitly incorporates the incentives and constraints of

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4The literature on statistical treatment choice has similarly focused for the most part on non-strategic planner problems. See Manski (2004) and Tetew (2012) as well as Hirano and Porter (2009); Kitagawa and Tetew (2018); Athey and Wager (2021) for recent contributions.
the researchers. Relative to the decision-theoretic approach, this has two main advantages. First, it lets us characterize *when* MHT adjustments are appropriate—and also when they are *not*—as a function of measurable features of the research process. Second, it allows us to justify and discriminate between different notions of compound error (e.g. average error rate or the family-wise error rate (FWER)) in the same framework based on these same economic fundamentals.

Finally, we aim to provide guidance for navigating the extensive statistical literature on MHT through the lens of an economic model. This literature focuses on the design of algorithmic procedures for controlling particular notions of compound error; see Efron (2008a) and Romano et al. (2010) for overviews. Few statistical optimality results exist (e.g., Spjotvoll, 1972; Lehmann et al., 2005; Romano et al., 2011), however, and none of the prior work studies MHT procedures as a way of addressing incentive problems. We draw on these references for inspiration, but maximize a different (social planner’s) objective and subject to incentive compatibility constraints. We also draw on List et al. (2019)’s helpful distinction between different types of multiplicity, and show how these lead to different optimal testing procedures.

2 Model

We study MHT in a game between a social planner who chooses statistical procedures and a representative experimental researcher with private incentives. In our running example, we can think of the planner as a regulator (e.g. the FDA) who defines testing protocols for studies submitted in support of applications for the approval of new drugs, and the researcher as a pharmaceutical company running a pre-specified clinical trial of a new drug hoping to obtain such approval. Multiple testing issues arise whenever research informs multiple decisions. Our main focus is on settings with multiple *treatments* (e.g. multiple drugs) or different *subpopulations* (e.g. multiple demographic groups for which a drug may be approved). We extend our results to multiple *outcomes* (e.g. multiple endpoints) in Section 5.2.

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5See, e.g., Holm (1979); Westfall and Young (1993); Benjamini and Hochberg (1995); Benjamini and Liu (1999); Storey (2002); Storey et al. (2004); Lehmann and Romano (2005a); Lee and Shaikh (2014); Romano and Wolf (2016); List et al. (2019, 2021) among many others.
To say something coherent about MHT, a framework must of course be able to rationalize conventional (single) hypothesis testing in the first place. This is known to be a challenging problem, requiring non-trivial restrictions on the research process (see e.g. Section 1 in Tetenov, 2016, for a discussion). In particular, it requires strong asymmetries to match the inherently asymmetric nature of null hypothesis testing. For example, Tetenov (2012) shows that justifying testing at conventional levels in a single agent model with minimax regret requires extreme degrees of asymmetry: statistical tests at the 5% (1%) level correspond to decision-makers placing 102 (970) times more weight on type I than type II regret. In our game, the asymmetry necessary for rationalizing hypothesis testing will arise naturally from the planner’s desire to prevent the implementation of treatments that may hurt (groups of) individuals.

2.1 A game between a researcher and a social planner

We consider a two-stage game between the planner and the researcher. In the first stage, the planner prescribes and commits to a hypothesis testing protocol, restricting how the researcher can report findings. In the second stage, given this protocol, the researcher decides whether or not to run one of several possible experiments by comparing the private benefits of experimentation to the private costs.

Hypothesis testing protocols take as input the data from the experiment and output multiple binary findings indicating whether the treatments were found to be effective. These findings, in turn, affect social welfare. As we discuss below, we will assume that the findings correspond to the planner’s decisions of whether to implement the corresponding treatments. Because the hypothesis testing protocol is chosen ex-ante by the planner, this is equivalent to the planner pre-committing to data-dependent binary decisions and the researcher truthfully reporting these decisions given the observed data.

2.1.1 The researcher’s problem

The researcher takes the hypothesis testing protocol as given. She decides both whether to experiment and how to design the experiment. We first describe the experiment and hypothesis testing protocol and then introduce the researcher’s optimization problem.

**Experiments.** Let $\mathcal{J}$ denote the finite set of all combinations of non-exclusive treatments
that can be tested in an experiment, with \( \emptyset \in \mathcal{J} \) denoting no experimentation. The parameter vector \( \theta \in \Theta \), where \( \Theta \) is a compact parameter space, captures the effects corresponding to all possible combinations of treatments in \( \mathcal{J} \).

An experiment consists of a set of treatments \( J \in \mathcal{J} \) and a design \( \Sigma \in \mathcal{S}(J) \), which are chosen by the researcher. Here \( \mathcal{S}(J) \) is the set of all possible designs given \( J \). If the researcher experiments, she draws a vector of statistics \( X \) from a distribution \( F_{\theta,J,\Sigma} \), indexed by \( J, \theta, \) and \( \Sigma \). The design \( \Sigma \) summarizes all the relevant features of the distribution of \( X \) the researcher can choose ex-ante, such as the sample size of an experiment. In summary, the researcher can choose \( J \) and \( \Sigma \) but not \( \theta \).

Before running the experiment, the researcher pre-specifies and reports \( J \) and \( \Sigma \), which become common knowledge. That is, while the researcher can choose \( J \) and \( \Sigma \) ex-ante based on prior knowledge and private incentives as we discuss below, \( J \) and \( \Sigma \) do not depend on the realized statistics \( X \). We thus abstract from issues of \( p \)-hacking and selective reporting. This case is relevant for considering decision-making at the FDA, for example, which requires pre-registration.\(^6\)

**Remark 1 (Mutually exclusive treatments).** We focus on settings where the treatments are not mutually exclusive. This is relevant in the regulatory approval process context when there are multiple subgroups and the pharmaceutical companies receive separate approvals for each group. That said, our framework also accommodates settings with mutually exclusive treatments. First, if the researcher can report multiple findings and each treatment will be implemented with an (exogenous) probability and these probabilities sum to one, the results in Sections 3 and 4 apply due to the linearity of the planner’s objective defined below. Second, if the researcher is only allowed to report one finding, the resulting model is isomorphic to the one discussed at the end of Appendix B.3.\(^6\)

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\(^6\)Specifically, the summary of the Final Rule for Clinical Trials Registration and Results Information Submission (42 CFR Part 11) on ClinicalTrials.gov states that “[r]egistration is required for studies that meet the definition of an ‘applicable clinical trial’ (ACT) and either were initiated after September 27, 2007, or initiated on or before that date and were still ongoing as of December 26, 2007” (NIH National Library of Medicine, nd). Registration must specify, among other things, the intervention(s), primary outcomes, and intended enrollment and study design. See What constitutes clinical trial registration information? (2024). 42 CFR §11.28.
**Hypothesis testing protocols.** Given an experiment \((J, \Sigma)\), the researcher reports results in the form of a vector of non-exclusive findings

\[
 r(X; J, \Sigma) = (r_1(X; J, \Sigma), \ldots, r_{|J|}(X; J, \Sigma))^\top \in \{0, 1\}^{|J|},
\]

where \(r_j(X; J, \Sigma) = 1\) if and only if treatment corresponding to \(J_j\) is found to be effective, with \(J_j\) denoting the \(j^{th}\) entry of \(J\). We will refer to \(r\) as a *hypothesis testing protocol*. Whenever \(J = \emptyset\), the researcher reports no findings.

**Example 1.** The researcher uses linear regression to estimate the effects of treatments \(J \in \mathcal{J}\) on an outcome \(Y\) based on an experiment with \(n\) units. Let \(D_{i,j} = 1\) if unit \(i\) received treatment \(j\) and \(D_{i,j} = 0\) otherwise. Suppose that \(J = \{1, 2\}\) and that

\[
 Y_i = \mu + \theta_1 D_{i,1} + \theta_2 D_{i,2} + \varepsilon_i, \quad \varepsilon_i \overset{i.i.d.}{\sim} \mathcal{N}(0, \eta^2),
\]

where \(\mu\) and \(\theta\) are unknown parameters and \(\eta^2\) is known. Let \(X \sim \mathcal{N}(\theta, \Sigma)\), where \(X\) is the OLS estimator of \((\theta_1, \theta_2)\) and where, specializing notation, in this example the design \(\Sigma\) denotes the covariance matrix of \(X\). An example of a testing protocol is separate (one-sided) \(t\)-testing, \(r(X; \{1, 2\}, \Sigma) = (1\{X_1/\sqrt{\Sigma_{1,1}} \geq t\}, 1\{X_2/\sqrt{\Sigma_{2,2}} \geq t\})^\top\).

**The researcher’s objective.** For each \((J, \Sigma)\), the researcher takes as given the corresponding hypothesis testing protocol \(r(\cdot; J, \Sigma)\), which is chosen by the planner in the first stage. For simplicity we assume that the researcher knows \(\theta\), but our main results continue to hold when the researcher is imperfectly informed and has a prior about \(\theta\) (see Section 5.1). Let \(B_r(\theta, J, \Sigma)\) denote the private benefits from research; we leave these generic for now, but note that this admits the possibility that private benefits differ from the planner’s objective. Upon conducting the experiment, the researcher incurs (expected) costs \(C(J, \Sigma) \geq 0\). The costs depend on the set of chosen treatments \(J\) and the design \(\Sigma\) and are sunk after the experiment is conducted. The researcher’s net benefits are thus

\[
 \beta_r(\theta, J, \Sigma) = B_r(\theta, J, \Sigma) - C(J, \Sigma), \quad B_r(\theta, \emptyset, \Sigma) = C(\emptyset, \Sigma) = 0,
\]

where we normalize the net benefits to zero if no experiment is conducted.

The researcher chooses which treatments to study and how to design the experiment so as to maximize her net benefits. Formally, the researcher’s problem is

\[
 (J^*_r, \Sigma^*_r, \theta) \in \arg \max_{J \in \mathcal{J}, \Sigma \in \mathcal{S}(J)} \beta_r(\theta, J, \Sigma),
\]
where $J^*_{r,\theta} = \emptyset$ corresponds to no experimentation. We impose a standard tie-breaking rule: when the researcher is indifferent regarding whether to experiment (i.e. when $B_r(\theta, J^*_{r,\theta}; \Sigma^*_{r,\theta}) = C(J^*_{r,\theta}; \Sigma^*_{r,\theta})$), she experiments if social welfare (defined below) is weakly positive. Let $e^*_r(\theta)$ indicate whether the researcher experiments, $e^*_r(\theta) = 1 \{J^*_{r,\theta} \neq \emptyset\}$.

2.1.2 The planner’s problem

The social planner chooses a hypothesis testing protocol $r \in \mathcal{R}$ to maximize her utility, where $\mathcal{R}$ is the class of pointwise measurable protocols. The planner’s utility will depend on the welfare effects of implementing the recommended treatments as well as (potentially) a measure of the broader value of scientific research.

Welfare. Welfare depends on whether the researcher experiments and on her findings if she does. To define welfare, we introduce the selector function $\delta$, with each entry indicating whether a specific combinations of treatments was found to be effective,

$$\delta(r(X; J, \Sigma); J) \in \{0, 1\}^{2^{|J|}-1}, \text{ where } \sum_{k=1}^{2^{|J|}-1} \delta_k(r(x; J, \Sigma); J) \in \{0, 1\}. \quad (5)$$

If there are no findings ($\sum_k \delta_k(r(X; J, \Sigma); J) = 0$), the status quo prevails. Here $\delta(\cdot; J)$ only takes into account combinations of the treatments in the set $J$, and ignores treatments that are not studied in the experiment. For $j = 1, \ldots, 2^{|J|}-1$, let $u_j(\theta; J)$ denote the effect on welfare that would result from implementing the combination of treatments $\delta_j(r(X; J, \Sigma); J)$.

We interpret $u_j(\theta; J)$ as incorporating the planner’s ex-ante knowledge about whether there are interaction effects between treatments.

Given $r(X; J, \Sigma)$, the overall welfare is $u(\theta; J)\top \delta(r(X; J, \Sigma); J)$. We normalize $u(\theta; \emptyset) = 0$ for all $\theta \in \Theta$. That is, welfare is equal to zero under the status quo when no experimentation occurs. For a given experiment $(J, \Sigma)$, the expected welfare is

$$v_r(\theta, J, \Sigma) = \int \delta(r(x; J, \Sigma); J)\top u(\theta; J) dF_{\theta,J,\Sigma}(x). \quad (6)$$

If the researcher does not experiment, $v_r(\theta, \emptyset, \Sigma) = 0$ since $u(\theta; \emptyset) = 0$ for all $\theta \in \Theta$.

We illustrate the above notation in the following example.

Example 2 (Example 1 continued). Suppose that $\mathcal{J} = \{\emptyset, \{1\}, \{2\}, \{1, 2\}\}$ and denote by

\footnote{Note that here and elsewhere we use the subscript $j$ to index either treatments or combinations of treatments, depending on the circumstance.}
θ₁ and θ₂, the welfare from implementing treatment 1 and 2, respectively. In this case, \(u(\theta; \emptyset) = 0\), \(u(\theta; \{1\}) = \theta₁\), and \(u(\theta; \{2\}) = \theta₂\). If there are no interaction effects, so that the welfare from implementing both treatments is equal to the sum from implementing each of them separately, then

\[ u(\theta; \{1, 2\}) = (\theta₁, \theta₂, \theta₁ + \theta₂). \tag{7} \]

**The planner’s objective.** We consider a planner who wishes to increase welfare while also limiting the possibility of harm due to mistaken conclusions, and to encourage the creation of new knowledge. Specifically, the planner chooses \(r\) to maximize,

\[ U(r; \lambda, \pi) = \min_{\theta \in \Theta} v_r(\theta, J^{*}_{r, \theta}, \Sigma^{*}_{r, \theta}) + \lambda \int \epsilon^{*}_{r}(\theta) \pi(\theta) d\theta, \tag{8} \]

where \(\lambda \geq 0\) and \(\pi(\theta) \geq 0\) for all \(\theta \in \Theta\). The first component, which depends on which treatments are actually implemented, captures the desire to raise welfare while limiting harm using a standard ambiguity-averse (maximin) formulation. The second component, which depends on whether or not the researcher experiments, captures any intrinsic or longer-term subjective expected value of scientific research. This may depend on the true value \(\theta\), captured by an arbitrary positive function \(\pi(\theta)\), as we will discuss further below. The parameter \(\lambda\) allows us to consider the special case in which the planner’s objective criterion is purely maximin (\(\lambda = 0\)) as well as the more general case in which she also cares about incentivizing research per se (\(\lambda > 0\)).

The weighting of ambiguity-averse and expected-utility components in Equation (8) echoes a long tradition in economic theory (e.g. Gilboa and Schmeidler, 1989; Banerjee et al., 2020); see also Remark 2. In the regulatory approval example, this structure is motivated by regulators such as the FDA being tasked by legislators with several distinct objectives (e.g. U.S. Food and Drug Administration, ndb). Each component of Equation (8) relates to a distinct objective. The first captures the desire to avoid implementing harmful treatments, as for example under the “do no harm” principle (since, we show, our framework naturally rationalizes one-sided hypothesis testing). The second captures the longer-term value of scientific research, which need not be directly related to the immediate regulatory decision being made. As the international guidelines for clinical trials state, for example, “the rationale and design of confirmatory trials nearly always rests on earlier clinical work carried
out in a series of exploratory studies” (Lewis, 1999). More broadly, the results of one study may lead to new conceptual insights or scientific hypotheses which are valuable independent of any immediate clinical application. The relative importance of these two objectives is generally not specified, however, which will motivate our focus below on protocols that are optimal for all $\lambda \geq 0$. We will show below that, under a further assumption on $\pi$, the two components of the planner’s objective function have intuitive connections to the statistical concepts of size control and power.

**Remark 2 (Connection of planner utility to alternative objectives).** In Appendix B.4 we discuss connections between the objective function (8) and two alternative notions of optimality, motivated by the fact that there are (as we show) no uniformly most powerful testing protocols. First, we discuss the connection of (8) to optimality criteria that focus on local-to-zero parameter values. Second, we show that the second component in (8), $\int c^*_\pi(\theta)\pi(\theta)d\theta$, approximates the expected utility from implementing the treatments when the planner has a subjective prior over strictly positive treatment effects. This implies that (8) has an (approximate) decision-theoretic foundation under ambiguity aversion (see Gilboa and Schmeidler, 1989) and can equivalently be interpreted as a version of the influential $\varepsilon$-contamination model of Huber (1992), as discussed in Banerjee et al. (2020).

3 Optimal hypothesis testing protocols

In this section, we characterize optimal hypothesis testing protocols without imposing functional form restrictions on the researcher’s payoff or the planner’s utility.

3.1 Null space, alternative space, and notions of optimality

For given set of treatments $J$, define the (global) null space, the set of parameters for which the welfare effect of implementing any combination of treatments is negative, as

$$\Theta_0(J) = \left\{ \theta : u_j(\theta; J) < 0 \text{ for all } j \in \{1, \ldots, 2^{|J|} - 1\} \right\} \subseteq \Theta.$$  \hfill (9)

Define the (global) alternative space, the set of parameters for which welfare is always positive, as

$$\Theta_1(J) = \left\{ \theta : u_j(\theta; J) \geq 0 \text{ for all } j \in \{1, \ldots, 2^{|J|} - 1\} \right\} \subseteq \Theta.$$  \hfill (10)
A graphical illustration of $\Theta_0(J)$ and $\Theta_1(J)$ is provided in Figure 2.\(^8\) We assume that $\bigcap_{J \in \mathcal{J} \setminus \emptyset} \Theta_0(J) \neq \emptyset$ and $\bigcap_{J \in \mathcal{J} \setminus \emptyset} \Theta_1(J) \neq \emptyset$ (and therefore $\Theta_0(J), \Theta_1(J) \neq \emptyset$ for all $J \neq \emptyset$). Similarly, define

$$\Theta_0^*(r) = \left\{ \theta : u_j(\theta; J_{r,\theta}^*) < 0 \text{ for all } j \in \{1, \ldots, 2^{|J_{r,\theta}^*|} - 1\} \right\},$$

$$\Theta_1^*(r) = \left\{ \theta : u_j(\theta; J_{r,\theta}^*) \geq 0 \text{ for all } j \in \{1, \ldots, 2^{|J_{r,\theta}^*|} - 1\} \right\},$$

as the null and alternative space given the treatments chosen by the researcher. If the researcher chooses not to experiment, that is, whenever $J_{r,\theta}^* = \emptyset$, these definitions imply that $\Theta_0^*(r) = \Theta_0(\emptyset) = \emptyset$ and $\Theta_1^*(r) = \Theta_1(\emptyset) = \emptyset$. Finally, denote by $\bar{\Theta}_1$ the set of parameters for which welfare is weakly positive for each choice of treatments $J$, $\bar{\Theta}_1 = \bigcap_{J \in \mathcal{J} \setminus \emptyset} \Theta_1(J)$.

We consider two notions of optimality: maxmin optimality (corresponding to the case where $\lambda = 0$) and global optimality (corresponding to the more general case where $\lambda \geq 0$). Each notion of optimality takes into account the researcher’s best response. Accordingly, we say that $r^*$ is maxmin optimal if

$$r^* \in \arg \max_{r \in \mathcal{R}} \min_{\theta \in \Theta} v_r(\theta, J_{r,\theta}^*, \Sigma_{r,\theta}^*)$$

and we say that a protocol is globally optimal if

$$r^*_{\lambda, \pi} \in \arg \max_{r \in \mathcal{R}} \left\{ \min_{\theta \in \Theta} v_r(\theta, J_{r,\theta}^*, \Sigma_{r,\theta}^*) + \lambda \int_{\Theta} e_r^*(\theta') \pi(\theta') d\theta' \right\}. \quad (11)$$

We say that $r^*$ is a weakly unbiased testing protocol if

$$\beta_{r, \theta}^*(\theta) \geq 0 \text{ for all } \theta \in \bar{\Theta}_1, \text{ where } \beta_{r, \theta}^*(\theta) = \max_{J \in \mathcal{J} \setminus \emptyset, \Sigma \in \mathcal{S}(J)} \beta_r(\theta, J, \Sigma). \quad (12)$$

Here $\beta_{r, \theta}^*(\theta)$ is the largest net benefit the researcher can achieve while conducting an experiment. We say that $r^*$ is strongly unbiased if

$$\beta_r(\theta, J, \Sigma) \geq 0 \text{ for all } J \in \mathcal{J}, \theta \in \Theta_1(J), \Sigma \in \mathcal{S}(J).$$

A strongly unbiased protocol is also weakly unbiased. We use the “unbiased” terminology because, as we show below, these definitions have a close connection to the definition of unbiased tests in the hypothesis testing literature, where a test is called unbiased if its power exceeds its size (Lehmann and Romano, 2005b).

\(^8\)Our results extend directly to the case where zero is included in the null space, so that $\Theta_0(J) = \{ \theta : u_j(\theta; J) \leq 0 \text{ for all } j \in \{1, \ldots, 2^{|J|} - 1\} \}$ and $\Theta_1(J) = \{ \theta : u_j(\theta; J) > 0 \text{ for all } j \in \{1, \ldots, 2^{|J|} - 1\} \}$, provided that $\beta_r(\cdot)$ and $u_j(\cdot)$ are continuous.
Notes: Graphical illustration of the null space $\Theta_0(J) = \{\theta \in \Theta : \theta_1 < 0 \text{ and } \theta_2 < 0\}$ and the alternative space $\Theta_1(J) = \{\theta \in \Theta : \theta_1 \geq 0 \text{ and } \theta_2 \geq 0\}$ for $J = \{1, 2\}$, with additive welfare as in (7). See Remark 3 for a discussion of the two orthants where the coefficients have different signs.

3.2 Maximin optimality and size control ($\lambda = 0$)

We start by characterizing the solution to the planner’s problem when $\lambda = 0$, i.e. when the planner solves a standard maxmin problem. We show that there is a close connection between maximin optimality and size control.

The following proposition provides a characterization of maximin testing protocols $r^*$. It generalizes Proposition 1 in Tetenov (2016) (discussed in Appendix C) to a setting in which $|J| > 1$ and where researchers can choose the experimental design and which hypotheses to test.

**Proposition 1** (Characterization of maximin protocols). *A protocol $r^*$ is maximin optimal if and only if*

\[
B_{r^*}(\theta, J^*_{r^*, \theta}, \Sigma^*_{r^*, \theta}) \leq C(J^*_{r^*, \theta}, \Sigma^*_{r^*, \theta}) \quad \forall \theta \in \Theta_0^*(r^*)
\]

\[
v_{r^*}(\theta, J^*_{r^*, \theta}, \Sigma^*_{r^*, \theta}) \geq 0 \quad \forall \theta \in \Theta \setminus \Theta_0^*(r^*)
\]

(13)

*Proof.* See Appendix D.1.

Proposition 1 shows that maximin optimality is equivalent to two conditions. First, as in the case with $|J| = 1$ hypotheses, maximin testing protocols deter experimentation
over the null space $\Theta_0^*(r^*)$, where each configuration of treatments reduces welfare. This captures a notion of size control. Second, welfare must be non-negative for $\theta \in \Theta \setminus \Theta_0^*(r^*)$. This condition requires that if some treatments reduce welfare there must be others that compensate for them; it always holds when $|J| = 1$ but is non-trivial in the MHT case.

Proposition 1 provides a necessary and sufficient condition for maximin optimality in terms of parameter spaces that are themselves functions of the set of treatments $J_{r^*,\theta}^*$, chosen by the researcher in response to $r^*$. The following corollary provides a sufficient condition for maximin optimality based on model primitives, which does not depend on $J_{r^*,\theta}^*$. This condition may be easier to check in practice and implies desirable robustness properties (Proposition 4).

**Corollary 1** (Sufficient condition for maximin optimality). A protocol $r^*$ is maximin optimal if, for all $J \in J \setminus \emptyset$ and $\Sigma \in \mathcal{S}(J)$,

\[ B_{r^*}(\theta, J, \Sigma) \leq C(J, \Sigma) \quad \forall \theta \in \Theta_0(J) \]

\[ v_{r^*}(\theta, J, \Sigma) \times 1\{\beta_{r^*}(\theta, J, \Sigma) \geq 0\} \geq 0 \quad \forall \theta \in \Theta \setminus \Theta_0(J). \]

\[ (14) \]

**Proof.** The result follows because (14) implies (13). \qed

The conditions in Equation (14) mimic those in Proposition 1, but are required to hold for all potential choices of $J$ and $\Sigma$. Note that in the second condition in Equation (14) we need only weakly positive welfare whenever the researcher finds it profitable to run an experiment, since otherwise the researcher will not run any experiment and hence welfare is zero. We illustrate the implications of maximin optimality below.

**Example 3** (Average size control with linear research benefits). Suppose that $J = \{1, 2\}$ and that welfare is additive as in Equation (7). Then the null space for $J = \{1, 2\}$ is $\Theta_0(J = \{1, 2\}) = \{\theta \in \Theta : \theta_1 < 0 \text{ and } \theta_2 < 0\}$ and the alternative space is $\Theta_1(J = \{1, 2\}) = \{\theta \in \Theta : \theta_1 \geq 0 \text{ and } \theta_2 \geq 0\}$. Figure 2 provides a graphical illustration. Suppose further that the researcher’s payoff is

\[ B_r(\theta; J, \Sigma) = b \sum_{j \in J} P(r_j(X; J, \Sigma) = 1|\theta) \]

for some constant $b > 0$. The first condition in Equation (14) implies that

\[ P(r_1^*(X; J, \Sigma) = 1|\theta_1, \theta_2) + P(r_2^*(X; J, \Sigma) = 1|\theta_1, \theta_2) \leq C(J, \Sigma)/b, \quad \forall \theta_1 < 0, \theta_2 < 0. \]

\[ (15) \]
This is a size control requirement (i.e. a restriction on the probability of reporting false discoveries).

**Example 4** (Weak FWER control with nonlinear research benefits). Consider the setup in Example 3 but suppose instead that the researcher’s payoff is

$$B_r(\theta; J, \Sigma) = bP \left( \sum_{j \in J} r_j(X; J, \Sigma) \geq 1 | \theta \right)$$

for some constant $b > 0$. The first condition in Equation (14) implies that

$$P(\max_j r^*_j(X; J, \Sigma) = 1 | \theta_1, \theta_2) \leq C(J, \Sigma)/b, \quad \forall \theta_1 < 0, \theta_2 < 0. \quad (16)$$

That is, it implies weak control of the FWER (which coincides with the positive FDR for $\theta \in \Theta_0(J)$). See Appendix B.3 for additional details.

Together these examples highlight two important implications of our model. First, max-min optimality provides an economic justification for size control with multiple hypotheses. Second, the notion of size control and compound error rate is directly linked to economic fundamentals. It depends on the researcher’s private costs and benefits and on how these scale with the number of hypotheses.

**Remark 3** (Null space). The definition of the null space $\Theta_0(J)$ is closely connected to the global null hypothesis in the literature. $\Theta_0(J)$ is a subset of the strong null space $\tilde{\Theta}_0(J) = \{ \theta : u_j(\theta, J) \leq 0 \text{ for some } j \}$. We note that $\tilde{\Theta}_0(J)$ plays an important role in the second condition of Corollary 1. Since $v_r(\tilde{\theta}, J, \Sigma) \geq 0$ for all $\tilde{\theta}$ in the positive orthant by definition, this condition is equivalent to assuming that welfare is positive for $\tilde{\theta} \in \tilde{\Theta}_0(J) \setminus \Theta_0(J)$.

**Remark 4** (Deterrence of experimentation under the null). Proposition 1 implies that experiments testing welfare-reducing treatments never occur in equilibrium, which might seem unrealistic. This result is a consequence of the simplifying assumption that the researcher has perfect information about $\theta$. Section 5.1 extends our analysis to settings where the researchers has a prior about $\theta$, characterizing testing protocols that are maximin optimal with respect not just to point mass priors (equivalent to the problem in the main model) but with respect to arbitrary priors. In this scenario optimal protocols deter testing by a researcher who is certain that treatment is welfare-reducing, but may not deter testing by one who believes that treatment is very likely to be welfare-increasing but possibly harmful.
3.3 Global optimality and power ($\lambda \geq 0$)

We now consider the general case where $\lambda \geq 0$ and show that when $\lambda > 0$, the planner’s desire to promote research implies a notion of power in our model.

We impose the following assumption on the planner’s weights.

**Assumption 1** (Weights over $\bar{\Theta}_1$). Suppose that $\pi \in \Pi$, where $\Pi$ is the class of functions $\pi$ satisfying $\pi(\theta) \geq 0$ for all $\theta \in \Theta$ and $\int_{\Theta} \pi(\theta)d\theta = \int_{\bar{\Theta}_1} \pi(\theta)d\theta > 0$.

Assumption 1 restricts the support of the planner’s weights to the alternative space $\bar{\Theta}_1$. In other words, the planner desires to promote experimentation if she expects that treatments will generate a positive welfare effect and are therefore worth exploring. She derives no benefit (but also no harm), on the other hand, from exploration of parts of the parameter space in which some treatment effects are negative.

Using classical hypothesis testing terminology, Assumption 1 allows for arbitrary alternative hypotheses over the positive orthant, including those in Section 4 of Romano et al. (2011) and Chapter 9.2 of Lehmann and Romano (2005b). Economically speaking, one can think of this assumption as ensuring that the components of the the planner’s utility function (8) cleanly separate the two motives we wish to capture: avoiding harm, and pursuing benefit. Doing so has the benefit that the components of utility will then map directly into the classical statistical concepts of size and power. As we discuss in more detail in Remark 5, Assumption 1 is necessary to justify testing protocols that control size (are maximin optimal), and we thus see Assumption 1 as natural when size control (through maximin optimality in our context) is a desideratum.

The following proposition provides a characterization of globally optimal protocols.

**Proposition 2** (Globally optimal protocols). Let Assumption 1 hold. Define

$$\mathcal{R}^*(\pi, \lambda) = \left\{ r : r \text{ is maximin optimal and } \int_{\Theta} \mathbf{1}\{\lambda \beta^*_r(\theta) \geq 0\} \pi(\theta)d\theta = \int_{\bar{\Theta}_1} \pi(\theta)d\theta \right\},$$

with $\beta^*_r(\theta)$ defined in Equation (12). Assume that $\mathcal{R}^*(\pi, \lambda) \neq \emptyset$. Then

$$\mathcal{R}^*(\pi, \lambda) = \arg \max_{r \in \mathcal{R}} \left\{ \min_{\theta \in \Theta} v_r(\theta, J^*_r, \Sigma^*_{r,\theta}) + \lambda \int_{\Theta} e^*_r(\theta)\pi(\theta)d\theta \right\}.$$

**Proof.** See Appendix D.2. \qed
Proposition 2 characterizes the set of globally optimal hypothesis testing protocols. The set \( \mathcal{R}^*(\pi, \lambda) \) can be viewed as a generalization and refinement of the set of maximin protocols for \( \lambda > 0 \); when \( \lambda = 0 \) it coincides with the set of maximin protocols.

As discussed in Section 2, rationalizing hypothesis testing (let alone multiple testing) is difficult in practice. Proposition 2 shows that one can write down a coherent economic objective function that rationalizes the standard statistical practice of choosing protocols that both control size and have non-trivial power. Specifically, optimal protocols must guarantee size control (encoded in the maximin optimality requirement) and also guarantee sufficient power against alternatives in the support of \( \pi \in \Pi \) (encoded in the statement that the probability that \( \lambda \beta^*_r(\theta) \geq 0 \) must equal to one when \( \pi \) is a prior density).

To gain further intuition, note that a sufficient condition for global optimality for any \( \pi \in \Pi \) is that, for each \( J \in \mathcal{J}, \Sigma \in \mathcal{S}(J) \),

\[
\begin{align*}
(i) & \quad \beta_r(\theta, J, \Sigma) \leq 0 \quad \forall \theta \in \Theta_0(J), \\
(ii) & \quad \beta_r(\theta, J, \Sigma) \geq 0 \quad \forall \theta \in \Theta_1(J), \\
and \ (iii) & \quad \text{welfare is positive when } \theta \in \Theta \setminus \Theta_0(J) \text{ whenever the researcher finds it beneficial to experiment.}
\end{align*}
\]

The following proposition provides a formal statement in terms of unbiasedness.

**Proposition 3** (Maximin and unbiased protocols are globally optimal for any \( \pi \in \Pi, \lambda \geq 0 \)).
Suppose that Assumption 1 holds and that a maximin optimal and weakly unbiased protocol exists. Then

\[
(i) \quad \text{A protocol is globally optimal for any } \pi \in \Pi, \lambda \geq 0 \text{ if and only if it is maximin optimal and weakly unbiased.}
\]

\[
(ii) \quad \text{A protocol is globally optimal for any } \pi \in \Pi, \lambda \geq 0 \text{ if it satisfies Equation (14) and is strongly unbiased.}
\]

Proof. See Appendix D.3.

Proposition 3(i) provides a necessary and sufficient condition for global optimality. It has two important implications. First, a testing protocol is globally optimal if and only if it is maximin optimal and its power (measured by \( \beta^*_r(\theta) \) for \( \theta \in \Theta_1(J) \)) exceeds size (measured by \( \beta^*_r(\theta) \) for \( \theta \in \Theta_0(J) \)). Second, it is possible to construct globally optimal protocols that do not depend on \( \lambda \) and \( \pi \) (i.e. protocols that are globally optimal for every
λ ≥ 0, π ∈ Π). This robustness property is important in practice: it may be challenging to quantify the trade-off between worst-case welfare and the benefits of research, or to choose priors over multi-dimensional alternative spaces. As we discuss in more detail in Appendix B.4, the robustness of this approach is a major advantage relative to alternative notions of optimality. Proposition 3(ii), meanwhile, provides a sufficient condition for global optimality based on Corollary 1 that may be easier to check in practice than the necessary and sufficient condition in Proposition 3(i).

Example 5 (Unbiased protocols). Under the setup of Example 3, the alternative space is Θ₁(J = {1, 2}) = {θ ∈ Θ : θ₁ ≥ 0 and θ₂ ≥ 0}. A necessary condition for the protocol r* to be strongly unbiased is

\[ P(r^*_1(X; J, Σ) = 1|θ₁, θ₂) + P(r^*_2(X; J, Σ) = 1|θ₁, θ₂) ≥ C(\{1, 2\}, Σ)/b, \forall θ₁ ≥ 0, θ₂ ≥ 0. \]  

Equation (17) (and Proposition 2) shows that global optimality requires power to (weakly) exceed the cost-to-benefit ratio of the experiment whenever θ ≥ 0.

Remark 5 (Assumption 1 and size control). Restrictions on Π such as those in Assumption 1 are necessary to justify hypothesis testing protocols that control size (are maximin optimal). Suppose instead that Π contains all possible positive weights integrating to one (i.e. prior distributions) on Θ. Then there exists a λ ≥ 0 and π ∈ Π such that no maximin protocol is a solution to problem (11). To see this, fix a prior π = 1{θ = (−1, −1, . . . , −1)} under which the planner knows that any (combination of) treatments is harmful. Then for sufficiently large λ, assuming the researcher’s payoff is increasing in the number of findings, the protocol r(X; J, Σ) = (1, . . . , 1) dominates any maximin protocol, since when λ is large the planner would prefer to experiment even when the resulting welfare effects are negative. That is, there exist combinations of π and λ such that the planner chooses protocols that do not control size, and instead incentivizes experimentation regardless of the value of θ. Restricting Π guarantees that optimal protocols control size (are maximin optimal) for all λ and motivates sufficiently powerful protocols only when θ is expected to have positive effects.

3.4 Robustness to uncertainty about the researcher’s payoff

So far, we have assumed that the planner knows the researcher’s payoff. The following proposition shows that maximin optimality for protocols satisfying Equation (14) is preserved
if the planner knows only an upper bound on the researcher’s payoff. Global optimality is preserved under additional restrictions on Π.

**Proposition 4** (Robustness to unknown researcher payoffs). Let the conditions in Proposition 2 hold. Then

(i) Any maximin protocol \( r^* \) satisfying the conditions in Corollary 1 under payoff function \( \beta_{r^*}(\theta, J, \Sigma) \) is also maximin optimal for any \( \beta'_{r^*}(\theta, J, \Sigma) \) such that \( \beta'_{r^*}(\theta, J, \Sigma) \leq \beta_{r^*}(\theta, J, \Sigma) \) for all \( \theta \in \Theta, J \in J, \Sigma \in S(J) \).

(ii) Any maximin protocol \( r^* \) satisfying the conditions in Corollary 1 under payoff function \( \beta_{r^*}(\theta, J, \Sigma) \) is globally optimal for any \( \beta'_{r^*}(\theta, J, \Sigma) \) such that \( \beta'_{r^*}(\theta, J, \Sigma) \leq \beta_{r^*}(\theta, J, \Sigma) \) for all \( \theta \in \Theta, J \in J, \Sigma \in S(J) \) and any positive function \( \pi \in \tilde{\Pi} \), where \( \tilde{\Pi} = \{ \pi : \int_{\Theta_{1}(r^*)} \pi(\theta)d\theta = \int_{\Theta} \pi(\theta)d\theta \}, \Theta_{1}(r^*) = \{ \theta : \beta'_{r^*}(\theta, J^*_r, \Sigma^*_{r, \theta}) \geq 0 \} \).

**Proof of Proposition 4.** See Appendix D.4.

Proposition 4(i) demonstrates an important robustness property of our maximin optimality results in settings where the researcher’s payoff function is unknown. Proposition 4(ii) states that protocols that are maximin optimal and unbiased with respect to the upper bound \( \beta_{r}(\theta, J, \Sigma) \) are also globally optimal for some weights \( \pi \in \Pi \). That is, global optimality is preserved when considering a (weakly) smaller class of alternatives. For example, for the optimal separate \( t \)-tests in Section 4, the set of weights \( \tilde{\Pi} \) is a subset of the set of strictly positive treatment effects.

Proposition 4 is particularly useful when applied to settings in which the planner’s uncertainty about the researcher’s payoff hinges on the costs \( C(J, \Sigma) \).

**Corollary 2** (Unknown cost function). Let the conditions in Proposition 2 hold. Consider \( \beta_{r}(\theta, J, \Sigma) = B_{r}(\theta, J, \Sigma) - C(J, \Sigma) \) and \( \beta'_{r}(\theta, J, \Sigma) = B_{r}(\theta, J, \Sigma) - C'(J, \Sigma) \), for some \( C'(J, \Sigma) \geq C(J, \Sigma) \). Then any maximin and unbiased protocol \( r^* \) under \( \beta_{r} \) is also maximin under \( \beta'_{r} \) and globally optimal for any \( \pi \in \tilde{\Pi} \), with \( \tilde{\Pi} \) defined in Proposition 4.

**Proof.** The proof follows directly from Proposition 4.
Corollary 2 states that in settings with uncertainty over the true cost function \( C'(J, \Sigma) \), the planner may use sensible upper bounds \( C(J, \Sigma) \geq C'(J, \Sigma) \). This result is important in empirical applications such as the ones we consider in Section 6.

4 Optimal protocols under linearity and normality

Which (if any) specific hypothesis testing protocols are maximin and globally optimal? The answer to this question depends on the functional form of the researcher’s benefits, the functional form of welfare, and the distribution of \( X \). In this section, we show that separate \( t \)-tests are optimal in settings with a linear researcher benefit (Assumption 2), a linear welfare (Assumption 3), and a normally distributed vector of statistics \( X \) (Assumption 4).

4.1 Assumptions

Linearity assumptions. Let \( \omega \) denote a vector of weights and define \( \bar{\omega}(J) = \sum_{j=1}^{\vert J \vert} \omega_{j} \) for \( J \in J \). As we discuss below, these weights will let us capture factors that affect the importance of the different treatments symmetrically from the point of view of both the researcher and the planner, though of course they nest the case in which all treatments are equally important (\( \omega_{j} = 1 \ \forall j \)). We consider the following assumption on the researcher’s benefits.

**Assumption 2** (Linear benefits). The researcher’s benefit is (recall here that the entry \( r_{j}(X; J, \Sigma) \) corresponds to treatment \( J_{j} \))

\[
B_{r}(\theta; J, \Sigma) = b \int \sum_{j=1}^{\vert J \vert} \omega_{j} r_{j}(x; J, \Sigma) dF_{\theta, J, \Sigma}(x),
\]

where \( \omega_{j} > 0 \) for all \( j \), \( b > 0 \), and \( b\bar{\omega}(J) > C(J, \Sigma) > 0 \) for all \( (J, \Sigma) \).

The condition \( b\bar{\omega}(J) > C(J, \Sigma) \) ensures that the experiment \( (J, \Sigma) \) is a relevant option; otherwise the researcher would never conduct this experiment, regardless of \( r \).

We consider the following linearity assumption on welfare.

**Assumption 3** (Linear welfare). For all \( r \in \mathcal{R} \),

\[
\delta(r(x; J, \Sigma); J)^{\top}u(\theta; J) = \sum_{j=1}^{\vert J \vert} \omega_{j} r_{j}(x; J, \Sigma) u(\theta; J_{j}),
\]

where \( u(\theta; J_{j}) \) is the welfare from implementing treatment \( J_{j} \).
Before stating the formal results, we provide an interpretation of Assumptions 2 and 3 in the context of our leading example, the drug approval process with multiple subgroups and multiple treatments.

With *multiple subgroups*, we interpret \( r_j(X; J, \Sigma) \) as indicating whether the drug was found to be effective for subgroup \( J_j \), which is of size \( \omega_{J_j} \). The component \( b\omega_{J_j} \) denotes the expected profits from selling the drug to subgroup \( J_j \), where \( b \) denotes the average per-sale profit. Assumption 2 then states that researchers care about the sum of the expected profits they can earn by selling the drug to each of the subpopulations for which its use is approved. Our specification of welfare in Assumption 3, meanwhile, corresponds to the utilitarian welfare from approving the drug, where \( u(\theta; J_j) \) denotes the per unit treatment effect on members of subgroup \( J_j \). The economic import of the assumption is that there are no spillovers between different subgroups.

With *multiple treatments* the interpretation is similar, but here each treatment denotes a different drug in the same market. \( \omega_{J_j} \) denotes the expected number of individuals that would purchase and use drug \( J_j \) if approved, and \( u(\theta; J_j) \) is the effect of drug \( J_j \) on those individuals. As before, \( b \) denotes the average per-sale profit. The economic import of Assumption 3 is that the sets of people who would use the different drugs are disjoint (or that the drugs do not exhibit interaction effects).

**Normality assumption.** We focus on the leading case where \( X \) is normally distributed, which is motivated by asymptotic approximations based on central limit theorems. We also assume that each entry of \( X \) has the same variance, an assumption that we discuss in detail below. Let \( \theta_J \) denote the subvector of \( \theta \) corresponding to treatments \( J \).

**Assumption 4** (Normality and homogeneous variances). For each experiment \((J, \Sigma)\), let \( X \sim \mathcal{N}(\theta_J, \Sigma) \) and suppose that \( u(\theta, j) = \theta_j \), so that \( X_j \) is an unbiased normally distributed signal of the welfare effect of treatment \( j \). The class of designs \( \mathcal{S}(J) \) is such that, specializing notation, \( \Sigma_{i,i} = \Sigma_{j,j} \in (\underline{\gamma}, \overline{\gamma}) \) for all \( i, j \in \{1, \ldots, |J|\} \) and some constants \( 0 < \underline{\gamma} < \overline{\gamma} < \infty \).

Assumption 4 imposes that the vector of statistics \( X \) is normally distributed, centered around the vector of welfare effects \( \theta_J \). The researcher can choose the covariance matrix, but we restrict the class of designs she can chose from to designs in which the variances (the
diagonal entries of $\Sigma$) are positive and equal to each other. This implies that the researcher can choose the overall sample size of the experiment, for example, but is constrained in allocating sample across experimental arms.

Appendix B.1 develops two distinct microfoundations for the equal-variance requirement in Assumption 4. First, if the researcher can choose any positive definite $\Sigma$ strategically, it is optimal for the planner to choose a protocol $r$ that forces the researcher to use a design with a variance-equalizing allocation, i.e. under which $\Sigma_{i,i} = \Sigma_{j,j}$ for $i \neq j$. In addition to supporting Assumption 4, this result provides guidance on the optimal design of experiments given the incentives in our economic model. Second, we examine a case in which the diagonal elements of $\Sigma$ are heterogeneous but the researcher does not know their values ex ante. In this case we show that, if the researcher’s prior is such that, in expectation, the variances are equal conditional on the standardized treatment effects, then the optimal protocol coincides with the one we will derive here under Assumption 4. This might describe, for example, a situation in which the researcher designs an experiment that she believes ex ante to be equally powered to detect standardized effects on each treatment, even though ex post some estimates turn out to be more precise than others.

The following three examples illustrate the scope of Assumption 4.

**Example 6** (Homoskedastic design). Consider the model in Equation (2) and the design in Example 1. Suppose that $n/3$ units receive treatment $j = 1$, $n/3$ receive treatment $j = 2$, and the remaining $n/3$ units are assigned to the control group. Then Assumption 4 holds with $X \sim \mathcal{N}(\theta_{\{1,2\}}, \Sigma)$, $\Sigma_{1,1} = \Sigma_{2,2} = 2\eta^2/(3n)$.

**Example 7** (Heteroskedastic design). Suppose we have $n$ units $i \in \{1, \ldots, n\}$, each corresponding to a (fixed) type $X_i \in \{0, 1\}$. Let $D_i \in \{0, 1\}$ denote a binary treatment. The outcome $Y_i$ is given by the following model,

$$Y_i = \tau_1 X_i D_i + \tau_2 (1 - X_i) D_i + \varepsilon_i, \quad \varepsilon_i \overset{i.i.d.}{\sim} \mathcal{N}(0, \sigma_1^2 X_i + (1 - X_i)\sigma_2^2),$$

where $Y_i$ is normalized to have mean zero in the absence of treatments. The researcher obtains OLS estimates $\hat{\tau}_1$ and $\hat{\tau}_2$. Let $\sum_{i=1}^n D_i X_i = n \frac{\sigma_1^2}{\sigma_1^2 + \sigma_2^2}$, so that the design equalizes the variances of the estimators across different groups. Then Assumption 4 holds with $X = (\hat{\tau}_1, \hat{\tau}_2)$ and $\theta = (\tau_1, \tau_2)$.
**Example 8** (Design violating Assumption 4). Consider the setup in Example 7 but with $\sum_{i=1}^{n} D_i X_i = n/2$. The researcher obtain OLS estimates $\hat{\tau}_1$ and $\hat{\tau}_2$ and reports $X = (\hat{\tau}_1, \hat{\tau}_2, \Sigma_{1,1} = \Sigma_{2,2} = \frac{n}{n})$. This design does not satisfy Assumption 4 because $X_j$ in this case is not centered at the welfare effects $(\tau_1, \tau_2)$. However, as we show in Appendix B.1, the optimal protocol under Assumption 4 remains optimal for this design if $\sigma_j$ is ex-ante unknown to the researcher, who has a prior such that $\mathbb{E}[\sigma_j | \theta] = \sigma$ for all $j$ and some $\sigma > 0$. This captures, for example, scenarios where researchers have priors over treatment effects measured in standard deviation units, which is a common measure of efficacy in some clinical trials (Faraone, 2008).

### 4.2 Optimality of t-tests

The following proposition shows that separate $t$-tests are maximin optimal under Assumptions 2, 3, and 4 and globally optimal if, in addition, Assumption 1 holds.

**Proposition 5** (Maximin and global optimality of separate $t$-tests). Let Assumptions 2, 3, and 4 hold. Then, the testing protocol $r^t(X; J, \Sigma) = (r^t_1(X; J, \Sigma), \ldots, r^t_{|J|}(X; J, \Sigma))^\top$, where

$$r^t_j(X; J, \Sigma) = 1 \left\{ \frac{X_j}{\sqrt{\Sigma_{j,j}}} \geq t(J, \Sigma) \right\}, \quad \forall j \in J$$

satisfies the conditions in Corollary 1 (and therefore is maximin optimal) if and only if $t(J, \Sigma) \geq \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b \omega(J)} \right)$. If, in addition, Assumption 1 holds, then $r^t$ is globally optimal for any $\lambda \geq 0, \pi \in \Pi$ if and only if $t(J, \Sigma) = \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b \omega(J)} \right)$.

**Proof.** See Appendix D.5.

Proposition 5 shows that separate one-sided $t$-tests with critical values $t(J, \Sigma) \geq \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b \omega(J)} \right)$ are maximin optimal. Throughout the rest of our discussion we will write $t$ in lieu of $t(J, \Sigma)$ whenever clear from the context. The key technical step in the proof is to show that under

9Here, for simplicity, we ignore the error from estimating $\sigma_1$ and $\sigma_2$ in finite samples. This reasoning is motivated by asymptotic approximations based on consistent estimators of $\sigma_1$ and $\sigma_2$.

10This scenario occurs, for example, whenever researchers have priors of the form $\tau_j = \zeta_j \sigma_j$, where $\zeta_j$ is a constant and $\sigma_j \sim \mathcal{P}$ is unknown and drawn from a given prior $\mathcal{P}$ so that the effect size scales in terms of standard deviation by a constant $\zeta_j$, and, in addition, $\mathbb{E}[\sigma_j] = \sigma$. 

25
this protocol welfare is non-negative even when parameters have different signs (Equation (13)). Proposition 5 also shows that standard separate one-sided $t$-tests are globally optimal for any prior $\pi \in \Pi$ and any weight $\lambda \geq 0$. While $t$-tests with thresholds larger than $\Phi^{-1}(1 - \frac{C(J, \Sigma)}{\bar{\omega}(J)})$ are maximin optimal, such tests are not globally optimal. Only $t$-tests with threshold $t = \Phi^{-1}(1 - \frac{C(J, \Sigma)}{\bar{\omega}(J)})$ are maximin and globally optimal. This demonstrates that global optimality is a refinement of maximin optimality that restricts attention to protocols with sufficient power.

Proposition 5 also shows that whether and to what extent the level of these separate tests should depend on the number of hypotheses being tested depends on the structure of the research production function $C(J, \Sigma)$ and on $\bar{\omega}(J)$, and in particular on how they vary with $J$. For example, suppose that $\omega_j = 1$ for all $j$ (each treatment is equally important) so that $\bar{\omega}(J) = |J|$. If $C(J, \Sigma) = \alpha$ for some constant $\alpha$ then a Bonferroni correction is optimal. This corresponds to a stylized case in which the costs of experimentation are fixed regardless of the number of treatments tested ($J$) or the precision of the estimates ($\Sigma$). If, on the other hand, $C(J, \Sigma) = \alpha|J|$ then the optimal size of the test is $\alpha$ irrespective of $|J|$. This might correspond, for example, to a case in which there are no fixed costs and testing each additional treatment requires the same increment to the sample size. The former case arguably captures the lay intuition that if the researcher can test many hypotheses in the hopes of securing some private benefit, then the planner should require a hypothesis testing protocol that discourages this. In the latter case it is still true that the researcher obtains a higher expected reward from taking on projects that test more hypotheses, ceteris paribus, but the appropriate correction for this is already “built in” to the costs of conducting research, so that no further correction is required.

While our original motivation for obtaining the result in Proposition 5 was to study the consequences of multiplicity ($|J|$), it follows immediately that, because the costs $C(J, \Sigma)$ may depend on the design $\Sigma$ in addition to $J$, the optimal critical values may as well. For example,
if the researcher can choose the number of treatments to test and also the sample size \( \bar{n} \) to use per treatment, then her cost structure might take the form \( C(J, \Sigma) = |J|(c_{|J|} + c_{\bar{n}}\bar{n}) \). In this case (normalizing \( b = 1 \) and again assuming for simplicity that \( \bar{\omega}(J) = |J| \)) we obtain optimal thresholds \( t = \Phi^{-1}(1 - c_{|J|} - c_{\bar{n}}\bar{n}) \) which are increasing in the (per-treatment) sample size \( n \) as well as in the fixed cost per treatment. Intuitively, to the extent that large-sample experiments are more costly to the researcher to run, the planner need worry less about discouraging the researcher from running such experiments when doing so would not be socially optimal. This point follows from exactly the same economic logic that rationalizes adjusting testing thresholds with respect to \( J \), but has not (to our knowledge) come up in past discussions of MHT adjustment. In that sense it illustrates the value of working out the economic logic underlying MHT adjustment carefully, to make sure we have fully grasped the consequences of any implicit assumptions.

The practical value of Proposition 5 lies in the fact that it connects optimal testing protocols to measurable properties of the cost function \( C(J, \Sigma) \). We illustrate this in Section 6.1 where we develop the application to clinical trials, using publicly available data on moments of their cost structure to derive specific testing thresholds. Appendix A provides a second illustration, applying the framework to experimental program evaluation research in economics using unique data from the Jameel Poverty Action Lab (J-PAL). Readers primarily interested in implications for practice may wish to skip ahead to these exercises.

**Remark 6 (One-sided vs. two-sided tests).** Under the assumptions in this section, separate one-sided \( t \)-tests are globally optimal. This is because the status quo remains in place if the researcher does not report any findings. This may be one reason that one-sided tests have been seen as appropriate in the drug approval context.\(^{12}\) When there is uncertainty about the planner’s action when no findings are reported, on the other hand, our model can justify two-sided hypothesis testing. We report this result in Appendix B.2 and for expositional

\(^{12}\)For example, former FDA advisor Lloyd Fisher writes that “For drugs that may be tested against placebos, with two positive trials required (as in the United States), it is argued that from both a regulatory and pharmaceutical industry perspective, one-sided tests at the 0.05 significance level are appropriate. In situations where only one trial against a placebo may be done (for example, survival trials), one-sided tests at the 0.025 level are appropriate in many cases.”(Fisher, 1991)
convenience continue to develop the one-sided case here.

Remark 7 (Average size control and FWER control). When the researcher’s payoff is linear (Assumption 2) and \( \omega_j = 1 \) for all \( j \), an implication of Propositions 2 and 5 is that maximin protocols control average size, 

\[
b/|J| \sum_{j \in J} P(r_j^*(X; J, \Sigma) = 1|\theta = 0) \leq C(J, \Sigma)/|J|.
\]

Many of the popular MHT corrections reviewed in the introduction do not directly target average size control and, thus, will generally not be optimal in our model. This explains why classical Bonferroni corrections are optimal in our model when \( C(J, \Sigma) \) is constant (see Section 6.1), while common refinements of Bonferroni such as Holm (1979)’s method are not. By construction, Bonferroni satisfies average size control, whereas common refinements do not. The optimality of Bonferroni (and average size control) is driven by the linearity of the experimenter’s payoff function. Bonferroni corrections may not be optimal with nonlinear payoff functions, but are maximin optimal for all payoff functions dominated by a linear payoff function (see Proposition 4). We discuss the impact of non-linearities for the design of optimal protocols in Appendix B.3.

5 Extensions

Here we consider two empirically relevant extensions of our main results. In Section 5.1, we analyze a variant of our model where the researcher is imperfectly informed and does not know \( \theta \) exactly. In Section 5.2, we apply our results to settings with multiple outcomes. Appendix B presents several additional extensions.

5.1 Imperfectly informed researchers

So far, we have assumed that the researcher is perfectly informed and knows \( \theta \). Here, we show that our main results continue to hold in settings where the researcher has imperfect information in the form of a prior about \( \theta \).\(^{13}\) Denote this prior by \( \pi' \in \Pi' \), where \( \Pi' \) is the class of all distributions over \( \Theta \).\(^{14}\) The prior \( \pi' \) captures knowledge about \( \theta \) that is available to the researcher but not to the planner.

\(^{13}\)In the single-hypothesis testing case, Tetenov (2016) gives results under imperfect information. However, these results rely on the Neyman-Pearson lemma, which is not applicable to multiple tests.

\(^{14}\)The assumption that \( \Pi' \) is unrestricted is made for simplicity. For our theoretical results, we only need that the class of priors \( \Pi' \) contains at least one element that is supported on the null space \( \Theta_0 \).
We assume that the vector of statistics $X$ is drawn from a normal distribution conditional on $\theta$, where $\theta$ itself is drawn from the prior $\pi'$ with $\int_{\Theta} \pi' (\theta) d\theta = 1$:

$$X \mid \theta \sim \mathcal{N} (\theta, \Sigma), \quad \theta \sim \pi', \quad \pi' \in \Pi',$$

where $\Sigma$ is positive definite and assumed to be known after being chosen by the researcher.

The researcher acts as a Bayesian decision-maker and chooses $(J^*_r, \pi'_r, \Sigma^*_r, \pi'_r) \in \arg \max_{J \in \mathcal{J}, \Sigma \in \mathcal{S}(J)} \int \beta_r (\theta, J, \Sigma) d\pi' (\theta)$.

The researcher’s prior is correctly specified, and welfare is given by

$$\bar{v}_r (\pi', J, \Sigma) = \int v_r (\theta, J, \Sigma) d\pi' (\theta).$$

Under imperfect information, we define maximin protocols with respect to the prior $\pi'$.

**Definition 1 ($\Pi'$-maximin optimal).** We say that $r^*$ is $\Pi'$-maximin optimal if and only if

$$r^* \in \arg \max_{r \in \mathbb{R}} \inf_{\pi' \in \Pi'} \bar{v}_r (\pi', J^*_r, \Sigma^*_r, \pi'_r).$$

Definition 1 generalizes the notion of maximin optimality in Section 3, which is stated in terms of the parameter $\theta$. When $\Pi'$ contains only point mass distributions, the two notions of maximin optimality are equivalent.

The next proposition shows that one-sided $t$-tests with appropriately chosen critical values are maximin optimal under imperfect information.

**Proposition 6 (Maximin optimality).** Let Assumptions 2, 3, and 4 hold. Then the protocol

$$r^*_j (X; J, \Sigma) = 1 \left\{ \frac{X_j}{\sqrt{\Sigma_{j,j}}} \geq \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b \omega (J)} \right) \right\}, \quad \forall j \in J,$$

is $\Pi'$-maximin optimal.

**Proof.** See Appendix D.6.

Proposition 6 shows that the maximin optimality of $t$-tests continues to hold under imperfect information. Intuitively, maximin optimality is preserved here because the worst-case prior is a point mass prior over $\theta$, so that the same reasoning as under perfect information applies. This follows from classical results on linear programs (see Appendix D.6). Global optimality of $r^t$ with $t = \Phi^{-1} (1 - C(J, \Sigma))$ follows as a direct corollary of Propositions 2 and 6. We omit the details for brevity.
5.2 Multiple outcomes

We turn next to the case in which the vector of statistics $X$ represent distinct outcomes that are (potentially) affected by a single treatment. Here it is not obvious that there is more than one decision to be made, and hence that the researcher should be asked to report more than one result. If a single regulator decides whether to approve a single treatment based on its effects on multiple outcomes, for example, then the question is how to optimally aggregate information on these effects.

Another possibility, however, is that the planner must design the process of scientific communication with multiple audiences in mind. To take an extreme but illustrative case, suppose that each outcome matters only to one decision-maker, and not to others. The hypothesis testing protocols the planner specifies will then determine for which outcomes the researcher reports findings, and hence which decision-makers implement the treatment. This interpretation is in the spirit of Andrews and Shapiro (2021), who emphasize the role of audience heterogeneity in the process of scientific communication. It also turns out to yield results that are isomorphic to those obtained earlier. We therefore study it first in Section 5.2.1 before turning to the issue of aggregation in Section 5.2.2. To streamline exposition we assume in both cases that the set of outcomes and the design are not chosen strategically by the researcher, so that $\Sigma$ is fixed.

5.2.1 Multiple policymakers

Consider an experiment with an audience of $G$ different policy-makers, each of which cares about a different one of $G$ different outcomes $Y = (Y_1, \ldots, Y_G)^\top$, associated with statistics $X = (X_1, \ldots, X_G) \sim \mathcal{N}(\theta, \Sigma)$. Define $u(\theta) = (u(\theta, 1), \ldots, u(\theta, G))$, where $u(\theta, g)$ is the effect of the treatment on $Y_g$.

In this simplified setting, the researcher chooses only whether or not to experiment, maximizing her net benefit $\beta_r(\theta, G, \Sigma)$. If she experiments, she reports $G$ different tests, one for each outcome, such that the testing protocol takes the following form

$$r(X; G, \Sigma) = (r_1(X; G, \Sigma), \ldots, r_G(X; G, \Sigma))^\top \in \{0, 1\}^G,$$

where $r_g(X; G, \Sigma)$ is the finding corresponding to outcome $g$.

The planner is aware of the different policymakers who may read and implement the
study’s findings but does not know for certain which will do so. She thus faces two sources of uncertainty with respect to both the welfare effects of the treatment and the audience for evidence of those effects. Suppose that each policymaker is equally likely to consider implementing the policy (i.e. implements the policy with probability $1/G$) and, for simplicity, that all outcomes are equally important. The expected welfare effect of an experiment with $G$ outcomes is then

$$v_r(\theta, G, \Sigma) = \frac{1}{G} \sum_{g=1}^{G} P(r_g(X; G) = 1|\theta)u(\theta, g).$$

(21)

This problem is isomorphic to the one with multiple treatments or subpopulations when the researcher does not choose $(J, \Sigma)$. For example, under the assumptions in Section 4.2 with $\beta_r(\theta, G, \Sigma) = b \int \sum_{g=1}^{G} r_g(x; G, \Sigma)dF_{\theta, \Sigma}(x) - C(G, \Sigma)$, Proposition 5 implies that

$$r_g(X; G, \Sigma) = 1 \left\{ \frac{X_g}{\sqrt{\sum_{g,g} w^\top \Sigma w}} > \Phi^{-1} \left( 1 - \frac{C(G, \Sigma)}{bG} \right) \right\}, \quad \forall g \in \{1, \ldots, G\},$$

i.e. separate $t$-testing, is maximin and globally optimal.$^{15}$

5.2.2 Optimal indexing with a single policymaker

Now suppose instead that there is single policymaker who decides whether to implement the treatment based on its effect on $G$ different outcomes. If the researcher experiments, she reports findings $r(X; G, \Sigma) \in \{0, 1\}$. Her payoff from experimentation is $\beta_r(\theta, G, \Sigma) = \int r(x; G, \Sigma)dF_{\theta, \Sigma}(x) - C(G, \Sigma)$. Welfare is given by

$$v_r(\theta, G, \Sigma) = \begin{cases} P(r(X; G, \Sigma) = 1|\theta)u(\theta) & \text{if } \beta_r(\theta, G, \Sigma) > 0, \\ \max \{P(r_j(X; G, \Sigma) = 1|\theta)u(\theta), 0\} & \text{if } \beta_r(\theta, G, \Sigma) = 0, \\ 0 & \text{if } \beta_r(\theta, G, \Sigma) < 0, \end{cases}$$

(22)

where $u(\theta)$ is defined below.

Because there is only a single decision to be made, one-sided $t$-tests based on a weighted average of the outcome-specific statistics,

$$r^t(X; G, \Sigma) = 1 \left\{ \frac{X^\top w}{\sqrt{w^\top \Sigma w}} > \Phi^{-1}(1 - C(G, \Sigma)) \right\},$$

(23)

will be maximin and globally optimal for suitable choices of weights $w$ if $X \sim \mathcal{N}(\theta, \Sigma)$.

$^{15}$As an alternative to the approach above, one could examine a worst-case approach with respect to the identity of the implementing policymaker. This model leads to very conservative hypothesis testing protocols, as we discussed in a previous version of the paper (Viviano et al., 2024).
The choice of the optimal weights depends on the interpretation of the outcomes $Y$, the corresponding statistics $X$, and the parameter space $\Theta$. We distinguish two cases.

**Multiple measurements.** Suppose that each entry of $X$ is a distinct measure of a common underlying parameter, so that $\Theta = \{\theta : \theta_1 = \cdots = \theta_G, \theta_1 \in [-M, M]\}$, and assume that $u(\theta) = \theta_1$. In other words, there is effectively only a single underlying parameter that affects welfare, but all components of $X$ are relevant to decision-making because all are independently informative about that parameter. In this case, the protocol (23) with variance-minimizing weights, $w = \arg\min_{v: v^\top v = 1} v^\top \Sigma v$, is maximin and globally optimal.\(^{16}\) This choice of weights coincides with classical notions of uniformly most powerful tests in the statistical literature on single hypothesis testing (Van der Vaart, 2000) and is similar to the recommendations, for example, in Anderson (2008). Thus, with multiple measurements, the recommendations from the economic model with incentives coincide with those based on the classical statistical approach.

**Economically distinct outcomes.** Now suppose that each entry of $X$ measures impacts on distinct arguments in the policymaker’s welfare function, so that $\Theta = [-M, M]_G$ (i.e. there are no cross-parameter restrictions). Welfare is equal to $u(\theta) = w^*^\top \theta$, for some known welfare weights $w^*$. In this case, the protocol (23) with $w = w^*$ is maximin and globally optimal (provided that $w^*^\top \Sigma w^* > 0$).\(^{17}\) That is, if a weighted average of the underlying parameters is what determines the welfare consequences of implementing the treatment, then an analogous weighted average of the individual statistics is the appropriate test statistic.

While welfare-weighting is not (yet) standard practice, there are recent examples such as Bhatt et al. (2024) in which researchers have constructed indices to reflect the preferences or objectives of stakeholders.\(^{18}\) In the context of clinical trials, Ho et al. (2015) illustrate

\(^{16}\)Maximin optimality follows verbatim from the proof of Proposition 1. Global optimality follows from the fact that for $\theta \geq 0$, the researcher’s payoff is weakly positive.

\(^{17}\)This follows because the rejection probability is increasing in $\sum_y w^*_y \theta_y$ and equal to the costs at $\theta = 0$.

\(^{18}\)Specifically, Bhatt et al. (2024) calculate an index of crime-related outcomes in which these are weighted by estimates of their social cost, citing an earlier version of this paper to motivate their approach. In another example, researchers working with the NGO GiveDirectly have elicited preferences over outcomes from the recipients of cash transfers to construct the weights over those outcomes in their subsequent analysis (Personal communication, Miriam Laker. 27 March 2024.)
Notes: This figure summarizes the consequences of multiplicity for inference in our model, focusing for expositional convenience on a case in which $|J|$ is a sufficient statistics for the costs of conducting research, i.e. abstracting from the impact of other design choices.

how patient preference information can be elicited to quantitatively assess the trade-offs between different effects of a treatment, and current FDA guidelines support such practices (U.S. Food and Drug Administration, 2024).

6 Empirical illustration and broader applicability

This section discusses the scope for applying and implementing the framework’s implications (summarized in Figure 3). Section 6.1 considers our running example, the regulatory approval process, while Section 6.2 considers processes of scientific discovery more broadly.

6.1 Empirical illustration

Proposition 5 showed that the implications of multiple treatments for MHT adjustment hinge on how research costs $C(J, \Sigma)$ vary with the set of chosen treatments $J$ and the experimental
design $\Sigma$. In particular, denote the level of the separate $t$-tests in Proposition 5 as
\[ \alpha(J, \Sigma) := \frac{C(J, \Sigma)}{b\bar{\omega}(J)}. \]
This is the probability that any given hypothesis will be rejected when $\theta = 0$ and the researcher conducts an experiment with treatments $J$ and design $\Sigma$. In the FDA approval context with multiple subgroups, we can think of $b$ as the expected profit per customer, and $\bar{\omega}(J)$ as the total number of customers that would buy the drug if it were approved for every subgroup. The social planner will therefore want to elicit information about $C(J, \Sigma)$, $b$, and $\bar{\omega}(J)$ to compute the proper adjustment factor. To build intuition, it will be helpful to impose the simplifying assumption $\bar{\omega}(J) = |J|$, which holds for example if each subgroup $j$ receives equal weight $\omega_j = 1$.

**Adjustment factor in general form.** Let $\bar{C}$ denote the cost of a benchmark experiment with a single treatment.\(^{19}\) Without loss of generality we can write
\[ \alpha(J, \Sigma) = \bar{\alpha} \times \frac{C(J, \Sigma)}{|J|\bar{C}} \]
where $\bar{\alpha} = \bar{C}/b$ denotes the size of the hypothesis test in the benchmark experiment. This formulation shows that the appropriate size for tests in a study with $|J|$ hypotheses can be calculated as the product of two quantities. The first is the size of the optimal test in the benchmark, single-hypothesis case. The second is the MHT *correction factor* $[C(J, \Sigma)/\bar{C} \times 1/|J|]$, which captures how the cost per test varies as the number of hypotheses tested grows—taking into account the fact that this may also involve variation in the design $\Sigma$. Unless the overall costs are fixed ($C(J, \Sigma) = \bar{C}$) this correction factor will differ from the standard Bonferroni correction factor $1/|J|$. Notice also that if costs are strictly proportional to the number of hypotheses ($C(J, \Sigma) = \bar{C} \times |J|$) then no adjustment is required; standard inference without adjustment for MHT is optimal.

**Choice of $\bar{\alpha}$.** We consider different specifications of $\bar{\alpha}$, the test size for a benchmark study with a single treatment. There are competing benchmarks one might consider: FDA guidelines currently recommend a size of 2.5% for one-sided single hypothesis tests ([Food and Drug Administration](nda) 2022), but Tetenov (2016), using data on the costs and expected profits from Phase III trials,\(^{19}\) This benchmark experiment could, for instance, be an experiment with the minimum sample size for a Phase III trial according to FDA guidance (see [U.S. Food and Drug Administration (nda)](nda)).
proposes a value of 15%. Therefore, and given the dispersion in the cost of trials for different drugs (Grabowski et al., 2002), we provide here results for a range of values between 2.5% and 15%.

**Modeling costs.** In principle the regulator could evaluate the MHT adjustment term in (24) separately for different categories (e.g., therapeutic classes) or even using data on each study individually. They might require pharmaceutical companies to declare the fixed and variable costs of a study when pre-registering it (information about which is often contained in contracts between the companies and the hospital or contract research organization organizing a trial). Here we wish to illustrate the potential consequences of doing so using existing, published estimates of moments of the cost structure of clinical trials. This requires that we model the cost function. We consider a simple formulation with both fixed and variable costs:

\[
C(J, \Sigma) = c_f + c_v \sum_{j \in J} n_j, \tag{25}
\]

where \(c_f\) denotes a fixed cost of running a clinical trial, \(n_j\) denotes the number of participants in each subgroup, and \(c_v\) the variable cost per participant. It will be convenient to rewrite this expression as

\[
C(J, \Sigma) = c_f + c_v |J| \bar{m} \bar{n}, \quad \bar{n} = \frac{1}{|J|} \sum_{j \in J} n_j
\]

where \(\bar{n}\) is the average sample size across subgroups in the trial in question and \(\bar{m}\) is the average overall sample size across all trials divided by the average number of subgroups across trials (which we denote as \(\bar{J}\)). With an abuse of notation, we can then write the optimal level as a function \(\alpha(|J|, \frac{\bar{n}}{\bar{m}})\) of the number of treatment arms and the ratio of the average subgroup sample size to the benchmark.

**Cost calibration.** Sertkaya et al. (2016) use data on the costs of 31,000 clinical trials of pharmaceutical drugs conducted in the United States between 2004 and 2012 to estimate that the average fixed costs of a Phase 3 trial were 46% of the average total cost, with the rest varying either directly with the number of subjects enrolled or with the number of sites at which they were enrolled.\(^{20}\) It follows that \(c_f/(c_f + c_v \bar{m} \bar{J}) = 0.46\), where \(\bar{m}\bar{J}\) is (by definition) the average overall sample size in a study. Using \(\bar{J} = 3\) based on the tabulations

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\(^{20}\)According to Sertkaya et al. (2016, Table 2), average variable costs (i.e., the per-patient and per-site...
in Pocock et al. (2002) yields an MHT correction factor of \( \frac{\bar{n} + 2.56/|J|}{3.56} \).\(^{21}\)

Inserting this into (24), we thus arrive at

\[
\alpha(|J|, \frac{\bar{n}}{\bar{m}}) = \alpha \times \left[ \frac{1}{3.56} \left( \frac{1 + 2.56/|J|}{3.56} \right) \text{ Multiplicity adjustment} + \frac{1}{3.56} \left( \frac{\bar{n}}{\bar{m}} - 1 \right) \text{ Sample size per arm} \right]. \tag{26}
\]

The correction factor here has two terms. The first is a “pure” correction for multiple hypothesis testing, accounting for the influence of the number of treatment arms \(|J|\). The second corrects for the effects of sample size on study cost: studies with sample sizes larger than the benchmark (\(\bar{n} > \bar{m}\)) are more expensive to run and thus require less strict testing thresholds.

Table 1 illustrates the implications quantitatively. It tabulates the test size implied by (26) for a range of values of \(|J|\) (rows), \(\alpha\) (columns), and \(\bar{n}/\bar{m}\) (column groups). For example, for studies with a benchmark sample size \((\bar{n} = \bar{m})\) and assuming \(\bar{\alpha} = 0.15\) as in Tetenov (2016), those with \(|J| = 2\) would use size 0.096, those with \(|J| = 3\) would use 0.078, and so on, asymptoting to 0.043 (up to rounding) at \(|J| = \infty\). If instead we set \(\bar{\alpha} = 0.025\), as recommended by FDA, then studies with \(|J| = 2\) would use 0.016, those with \(|J| = 3\) would use 0.013, and so on, asymptoting to 0.007 at \(|J| = \infty\). Note that these values are both more conservative than unadjusted thresholds, and also less conservative than would typically be implied by, for example, procedures suggested by the FDA (since the size of the methods suggested by the FDA converges to zero as \(|J| \to \infty\) with independent tests).

The table also illustrates how the adjustment factor depends on the (relative) average per-treatment sample size \(\bar{n}/\bar{m}\). The final three columns vary this while holding \(\bar{\alpha}\) fixed at 0.025.\(^{22}\) When \(\bar{n}\) is smaller than \(\bar{m}\) we require more stringent size control, while for larger \(\bar{n}\) we require less stringent size control. For example, with \(|J| = 2\), studies with half the benchmark sample size would use 0.012, while studies with double the benchmark sample costs were USD 10,826,880, and average total costs were USD 19,890,000, so that the fraction of fixed costs is \((19,890,000 − 10,826,880)/19,890,000 \approx 0.46\).

\(^{21}\)We use the median estimate multiplied by the probability of reporting more than one subgroup. The critical values are not particularly sensitive to \(\bar{J}\); if for example we fix \(\bar{\alpha}(1) = 0.025\) and double \(\bar{J}\) from 3 to 6 this decreases \(\alpha(2)\) from 0.016 to 0.015, \(\alpha(3)\) from 0.013 to 0.011, and \(\alpha(\infty)\) from 0.007 to 0.004.

\(^{22}\)As per Corollary 2, using a smaller cost/benefit ratio ensures optimality when the true ratio is unknown.
Table 1: Critical values as functions of hypothesis count and sample size

| $|J|$ | $\bar{\alpha}$ | 100% | 50% | 150% | 200% |
|----|----|-----|-----|-----|-----|-----|
|    | 0.025 | 0.05 | 0.1 | 0.15 | 0.021 | 0.0285 | 0.032 |
| 1  | 0.016 | 0.032 | 0.064 | 0.096 | 0.012 | 0.019 | 0.023 |
| 2  | 0.013 | 0.026 | 0.052 | 0.078 | 0.009 | 0.0165 | 0.020 |
| 3  | 0.012 | 0.023 | 0.046 | 0.069 | 0.008 | 0.0155 | 0.019 |
| 4  | 0.011 | 0.021 | 0.042 | 0.064 | 0.007 | 0.0145 | 0.018 |
| 5  | 0.010 | 0.020 | 0.040 | 0.060 | 0.006 | 0.0135 | 0.017 |
| 6  | 0.010 | 0.019 | 0.038 | 0.058 | 0.006 | 0.0135 | 0.017 |
| 7  | 0.009 | 0.019 | 0.037 | 0.056 | 0.006 | 0.0134 | 0.016 |
| 8  | 0.009 | 0.018 | 0.036 | 0.054 | 0.006 | 0.0134 | 0.016 |
| 9  | 0.007 | 0.014 | 0.028 | 0.042 | 0.004 | 0.0132 | 0.014 |
| $\infty$ | 0.007 | 0.014 | 0.028 | 0.042 | 0.004 | 0.0132 | 0.014 |

Notes: This table tabulates optimal critical values obtained from (26) for different numbers of hypotheses ($|J|$) and (relative) sample sizes ($\bar{n}/\bar{m}$), all given an assumed critical value for the benchmark case of a single-hypothesis experiment ($\bar{\alpha}$).

size would use 0.023. The same comparative statics reasoning would apply if the FDA were to make individualized decisions based on trial-specific costs, which could be obtained, for example, from privately-owned contract data (Sertkaya et al., 2016).

6.2 Broad takeaways

Questions about multiple testing arise in many processes of scientific communication whose characteristics differ from those we posit here. Academic publishing is an obvious one. MHT use is on the rise at economic journals, for example, but without clear consensus on the when and how it should be done (recall Figure 1). One can make a case for applying our framework more or less as-is to certain kinds of economic research—a point we illustrate in Appendix A, where we work out the quantitative consequences of applying it to experimental program evaluation research using a unique dataset on the costs of projects submitted to J-PAL. But it may also be prudent to step back and consider general principles that have emerged from the analysis that seem likely to apply even when our specific assumptions do not.

First, if MHT adjustments are a means of “getting research incentives right,” then they must depend on the costs of doing research. It must be the net incentives, i.e., rewards net
of costs, that matter. This perspective is useful as it grounds optimal testing procedures in measurable quantities, as we saw above in the applications to clinical trials and policy experiments in economics. It may also help to clarify confusion about the boundaries of MHT adjustment—the concern, for example, that if it is appropriate to apply MHT adjustment to all the hypotheses tested within one study, then it seems no less appropriate to apply it to all the hypotheses tested within one researcher’s lifetime. A cost-based perspective avoids such “reductio ad absurda” by delineating clear boundaries. Two hypotheses interact for the purposes of determining testing protocols if they also interact in the research cost function. MHT adjustment might thus be inappropriate within a single paper that reports results from two separate experiments, but might be appropriate across two papers that report distinct results from the same experiment.

Second, different types of multiplicity should typically be treated differently. Multiple treatments and multiple sub-populations imply multiple decisions—which treatments should be given, and to which subpopulations? This in turn means that multiple tests are needed, and the MHT question becomes pertinent. Multiple outcomes, on the other hand, do not necessarily imply that the researcher must conduct multiple tests. They can (and often are) aggregated into summary statistics instead. This distinction aligns with our reading of the historical narrative about MHT practices in the literature: the multiple-treatment case—genetic association testing in particular—has often been cited as the leading motivation for new MHT procedures proposed (see Dudoit et al., 2003; Efron, 2008a, for reviews), while the (superficially similar) case of multiple outcomes case seems to have been grouped in with the others subsequently and less intentionally.

What of non-experimental research? Our focus has been on the experimental case, where it is reasonable to imagine requiring researchers to commit in advance to the tests they will run. Optimal protocols for observational studies would likely need to account for additional factors. Observational work is often iterative in complex ways, and issues such as p-hacking may consequently loom large. (Interestingly, and in contrast to experimental work in economics, fewer than 5% of top-5 non-experimental empirical papers published in 2020 even mention multiple testing as a potential issue.) That said, the central role of the research cost function is a point of commonality across these problems.
Finally, any discussion of frequentist hypothesis-testing would be incomplete without acknowledging that one could also move away from this paradigm (which we have presumed) entirely towards a Bayesian alternative. In this regard, proposals to control the FDR are interesting. Several papers have pointed out a Bayesian rationale for doing so: controlling the (positive) FDR can be interpreted as rejecting hypotheses with a sufficiently low posterior probability (e.g., Storey, 2003; Gu and Koenker, 2020; Kline et al., 2022). In fact, close examination shows that these arguments apply even in the case of a single hypothesis. The essential idea is to balance the costs of false positives and false negatives, rather than prioritizing size control at any (power) cost. In this sense we interpret these arguments less as support for a particular solution to the MHT problem, and more as a reminder of the decision-theoretic merit of Bayesian approaches more generally.

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Conflict of Interest
Niehaus is co-chair of the Science for Progress Initiative at J-PAL, which provided the data used in Appendix A. The role is uncompensated. He is not an officer, director or board member of J-PAL.

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Online Appendix

A Application to economic experiments

In this appendix we consider the application of the framework to academic economic research. Not all academic research fits within our framework, tailored as it is to the regulatory approval process. Research intended primarily to increase generalizable, conceptual knowledge, for example, without reference to any particular policy decision, does not map naturally. And data on costs are not available systematically for all economic research. We focus on program evaluation experiments. These (arguably) map well to our framework, in that they are typically designed at least in part to inform an immediate policy decision about whether to retain or scale up a policy intervention, and we have access to unique data on the costs of conducting them that we obtained for this purpose.

Data. The data cover (essentially) all funding proposals for evaluations submitted to the Abdul Latif Jameel Poverty Action Lab (J-PAL) from 2009 to 2021. J-PAL is the leading funder and facilitator of experimental economic research in low-income countries, and funds projects that are typically designed to inform policy in those countries. In this sense the characteristics of these projects align fairly closely with the assumptions in our framework. The data contain the reported total financial cost of each project (including the amount requested from sources other than J-PAL, and with personnel costs broken out separately) as well as the overall sample size and the number of experimental arms in the study. This allows us to examine how costs vary with the design of the experiment and with hypothesis multiplicity with respect to the number of treatments. Financial costs are not the only
costs incurred, of course, but are likely to be highly correlated with other relevant ones that a researcher deciding whether to undertake a project would consider: ceteris paribus, larger budgets will tend to mean more researcher effort raising funds and managing teams of research assistants, for example.

**Modeling costs.** To see how testing adjustments would vary as a function of average sample size $\bar{n}$ and the number of treatment arms $|J|$ we follow steps (and use notation) similar to those in Section 6.1. Specifically, we again assume equal weights $\omega_j = 1$ and let $\bar{C}$ and $\bar{\alpha}$ denote the cost and the testing threshold, respectively, for a benchmark experiment with average sample size $\bar{m}$, so that (24) again gives the optimal size of a test. For this application we posit a Cobb-Douglas form

$$C(J, \Sigma) = k |J|^\beta (\bar{n})^\iota$$

for the cost function, which will fit the cost data reasonably well, and yields

$$\alpha(|J|, \bar{n}) = \bar{\alpha} \times |J|^{\beta-1} \times \left( \frac{\bar{n}}{\bar{m}} \right)^\iota$$

where the later two factors adjust for hypothesis count and sample size, respectively.

**Empirical analysis.** We take this to the data by taking logarithms of (27) and estimating the resulting linear model via OLS. An open question is how to model potential variation in $k$, the fixed cost factor. The correct interpretation of the regression results is that they identify values of the parameters $\beta$ and $\iota$ that would be appropriate for a planner to substitute into (28) while setting $\bar{\alpha}$ to the appropriate test size for an experiment testing one hypothesis, with sample size $\bar{m}$, and with the same fixed cost factor $k$. It is therefore useful to consider specifications that include further controls that may capture variation in $k$ across classes of projects. We include (incrementally) indicators for project type (pilot, full study, or add-on funding), indicators for the J-PAL initiative in question, and the (log of) personnel costs as potential proxies for $k$. Across all specifications we report results from tests of the hypotheses that $\beta$ is zero and one, respectively. The first condition holds if costs are invariant with respect to the number of arms, in which case average size control is indicated (Proposition 5). The second holds if costs are proportionate to the number of arms, in which case no MHT adjustment is indicated. Tests of the nulls that $\iota$ is zero and one assess corresponding conditions with respect to sample size.
The data reject both of these hypotheses (see Table 2). Regardless of the specification, costs are significantly and substantially less than proportional to the number of treatments tested, with estimated elasticities ranging from 0.13 to 0.22. But they are also not invariant to scale: projects with more arms cost significantly more, as these estimates are all significantly different from zero at the 5% level.\footnote{To avoid a reductio ad absurdum we do not apply MHT adjustments to our tests of the hypothesis that MHT adjustments are required.} Interpreted through the lens of our model, these patterns provide both a prima facie justification for applying MHT adjustment to studies of this sort, and also imply that simply controlling the average size of tests in these studies (e.g. via a Bonferroni correction) would be too conservative.

In Table 3 we make this point concrete, calculating the specific adjustments implied (and, for comparability, using the same format as Table 1). Adjustments vary significantly as the number of treatment arms increases, but are less conservative than Bonferroni corrections. Interestingly, the adjustments vary only moderately as a function of the sample size, as illustrated in the last three columns of Table 3.

**Heterogeneity analysis.** The specification underlying Table 2 allows for variation in the fixed costs, but there may also be variation across categories of experiment in the slope coefficients $\beta$ and $\iota$. As a final exercise we explore this possibility. We focus in particular on the distinction between experiments in high- as opposed to low-income countries. While the latter are J-PAL’s main focus and make up 80% of the data, the sample also includes a smaller and more recent subset of projects set in high-income countries (primarily the United States) which may differ from those in low income countries. Table 4 presents results estimated for these two subsamples separately.

The results for low income countries (Columns 1–4) are similar to those in Table 2, as one would expect. In high income countries, however, studies with more arms cost \textit{less} on average in several specifications (Columns 5–7), presumably reflecting heterogeneity in project types. As we control for more proxies for fixed costs $k$ this relationship becomes smaller and statistically insignificant. When in particular we control for personnel costs the estimated relationship between total costs and experimental arms is—unlike that in low-income countries—very close to zero. This is consistent with the idea that projects
Table 2: Multiple hypothesis testing adjustment in economics

| Dependent variable: log (total project cost) | (1)   | (2)   | (3)   | (4)   |
|---------------------------------------------|-------|-------|-------|-------|
| log (Treatment Arms) $[\beta]$              | 0.216 | 0.140 | 0.222 | 0.132 |
|                                             | (0.078) | (0.070) | (0.072) | (0.054) |
| log (Number of surveys per arm) $[\iota]$  | 0.253 | 0.107 | 0.136 | 0.075 |
|                                             | (0.024) | (0.023) | (0.024) | (0.020) |
| log (Personnel Costs)                       |       |       |       | 0.531 |
|                                             |       |       |       | (0.030) |
| Proposal Type FE                            | No    | Yes   | Yes   | Yes   |
| Initiative FE                               | No    | No    | Yes   | Yes   |
| $p$-value, $H_0: \beta = 0$                 | 0.005 | 0.045 | 0.002 | 0.014 |
| $p$-value, $H_0: \beta = 1$                 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value, $H_0: \iota = 0$                 | 0.000 | 0.000 | 0.000 | 0.000 |
| $p$-value, $H_0: \iota = 1$                 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations                                | 730   | 728   | 692   | 617   |
| Adjusted $R^2$                               | 0.129 | 0.334 | 0.410 | 0.656 |

Note: The dependent variable in all specifications is the (log of) the total cost of the proposed project. Proposal type fixed effects include indicators for full projects, pilot projects, and add-on funding to existing projects. Initiative fixed effects include an indicator for each J-PAL initiative that received funding applications. Heteroskedasticity-robust standard errors in parenthesis.

in low-income countries tend to incur substantial variable costs—collecting original survey data in rural areas, for example—while those in high-income countries tend to incur primarily fixed costs (as for example if they are able to take advantage of pre-existing administrative data sources to measure impacts). While only suggestive, this result illustrates how the cost structures of experimental research may vary by context, implying in turn differences in the appropriate MHT adjustments.24

24Our data do not let us observe and draw out sharper distinctions between different projects (e.g., whether an experiment is a field or online experiment). A planner who could observe such information would generally wish to condition testing protocols on it. In some cases researchers might be able to credibly disclose the cost structure of studies in a similar class to theirs, and pre-specify test procedures based on these. If so one
Table 3: Critical values as functions of hypothesis count and sample size in economics

| $|J|$ | $\bar{n}/\bar{m}$ | $J$ | 100% | 50% | 150% | 200% |
|-----|--------|-----|------|-----|------|------|
|     | $\bar{\alpha}$ | 0.025 | 0.05 | 0.1 | 0.15 | 0.047 | 0.051 | 0.052 |
| 1   | 0.025 | 0.050 | 0.100 | 0.150 | 0.026 | 0.028 | 0.028 |
| 2   | 0.013 | 0.027 | 0.054 | 0.081 | 0.018 | 0.019 | 0.020 |
| 3   | 0.009 | 0.019 | 0.038 | 0.057 | 0.014 | 0.015 | 0.015 |
| 4   | 0.007 | 0.015 | 0.030 | 0.045 | 0.011 | 0.012 | 0.013 |
| 5   | 0.006 | 0.012 | 0.024 | 0.037 | 0.010 | 0.010 | 0.011 |
| 6   | 0.005 | 0.010 | 0.021 | 0.031 | 0.008 | 0.009 | 0.009 |
| 7   | 0.004 | 0.009 | 0.018 | 0.027 | 0.007 | 0.008 | 0.008 |
| 8   | 0.004 | 0.008 | 0.016 | 0.024 | 0.007 | 0.007 | 0.007 |
| 9   | 0.003 | 0.007 | 0.014 | 0.022 | 0.007 | 0.007 | 0.007 |

Notes: This table tabulates optimal critical values obtained from the last column in Table 2 (using J-PAL data) for different numbers of hypotheses ($|J|$) and (relative) sample sizes ($\bar{n}/\bar{m}$), all given an assumed critical value for the benchmark case of a single-hypothesis experiment ($\bar{\alpha}$).

B Additional extensions

Throughout the remainder of the appendix, we will often suppress the dependence of the testing protocols on $J$ and $\Sigma$ and simply write $r(X)$. All proofs are in Appendix E.

B.1 Heterogeneous variances

In this section, we study settings with heterogeneous variances. We first consider a setting in which the researcher can choose any positive definite $\Sigma$ strategically and replace Assumption 4 with the following assumption.

**Assumption 5 (Normality with unrestricted $\Sigma$).** Suppose that for each $J \in \mathcal{J}$, $X(J) \sim \mathcal{N}(\theta_J, \Sigma)$, where $\Sigma$ is known, and $u(\theta, j) = \theta_j$ for all $j$. Let $\mathcal{S}(J)$ be equal to the space of positive definite matrices in $\mathbb{R}^{|J| \times |J|}$.

Under Assumption 5, $\Sigma$ is chosen ex-ante by the researcher but observed ex-post by the planner. As a result, the planner can de-facto mandate any choice of $\Sigma \in \mathcal{S}(J)$ by only rewarding designs that maximize her utility.

would expect this to lead to widespread disclosure via the unraveling effects that are standard in disclosure games.
Table 4: Experimental research costs: heterogeneity analysis

|                     | Main sample (low income countries) | High income countries |
|---------------------|-----------------------------------|-----------------------|
|                     | (1) (2) (3) (4)                   | (5) (6) (7) (8)       |
| log(Treatment Arms) | 0.380*** (0.084)                 | −0.309* (0.165)       |
|                     | 0.280*** (0.077)                 | −0.260* (0.142)       |
|                     | 0.312*** (0.071)                 | −0.209 (0.146)        |
|                     | 0.163*** (0.056)                 | 0.003 (0.115)         |
| log(Number of surveys per arm) | 0.295*** (0.027) | 0.193*** (0.053) |
|                     | 0.153*** (0.027)                 | 0.037 (0.045)         |
|                     | 0.155*** (0.026)                 | 0.083 (0.054)         |
|                     | 0.081*** (0.020)                 | 0.068 (0.041)         |
| log(Personnel Costs)| 0.531*** (0.027)                 | 0.530*** (0.089)     |

Proposal Type FEs
|             | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
|-------------|----|-----|-----|-----|----|-----|-----|-----|
| p-value, $H_0 : \beta = 0$ | 0.000 | 0.000 | 0.000 | 0.012 | 0.060 | 0.067 | 0.151 | 0.977 |
| p-value, $H_0 : \beta = 1$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 610 | 608 | 577 | 518 | 117 | 115 | 101 | 85 |
| Adjusted $R^2$ | 0.173 | 0.362 | 0.400 | 0.678 | 0.104 | 0.332 | 0.323 | 0.530 |

Note: The dependent variable in all specifications is the (log of) the total cost of the proposed project. Proposal type fixed effects include indicators for full projects, pilot projects, and add-on funding to existing projects. Initiative fixed effects include an indicator for each J-PAL initiative that received funding applications. “High income countries” are as according to the World Bank classification. Heteroskedasticity-robust standard errors in parenthesis.

We will show below that designs with sample allocations such that each estimator has the same variance are optimal in our setting. We will refer to such designs as designs with variance-equalizing allocation.

**Definition 2** (Design with variance-equalizing allocation). Under Assumption 5, we refer to $\Sigma(J)$ as a design with a variance-equalizing allocation if $\Sigma_{i,i}(J) = \Sigma_{j,j}(J)$ for all $i, j \in \{1, \ldots, |J|\}$.

In experiments, a variance-equalizing allocation consists of assigning a number of units to each treatment arm proportional to the corresponding outcome variance.

The following proposition characterizes a maximin and globally optimal hypothesis testing protocol under Assumption 5.
Proposition 7 (Maximin optimality of separate t-tests). Let Assumptions 2, 3, and 5 hold. Then, the testing protocol $r^N(X)$, where

$$r^N_j(X; J, \Sigma) = \begin{cases} 1 \left\{ \frac{X_j}{\sqrt{\Sigma_{j,j}}} \geq t^*(J, \Sigma) \right\}, & \forall j \in J \quad \text{if } \Sigma \text{ satisfies the variance-equalizing allocation} \\ 0, & \text{otherwise} \end{cases}$$

is maximin and globally optimal if $t^*(J, \Sigma) = \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b_\omega(J)} \right)$.

Proposition 7 shows that separate one-sided t-tests with critical values $t = \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b_\omega(J)} \right)$ are maximin and globally optimal if the experimental design satisfies the variance-equalizing allocation. The planner “forces” the researcher to choose designs with a variance-equalizing allocation by not rewarding any results from experiments with other designs. Proposition 7 provides guidance both on which testing protocol to implement and on which design to incentivize. It illustrates the benefits of variance-equalizing allocations.

Proposition 7 derives the optimal experimental design when the researcher knows and can choose $\Sigma$. In practice, however, the researcher may only have partial ex-ante knowledge about $\Sigma$, so that it may not be feasible to enforce variance-equalizing allocations. To formalize this scenario, for a given $(J, \Sigma)$, $X \sim N(\theta J, \Sigma)$, where $\theta_j = \tau_j/\sigma_j$ for some positive $\sigma_j > 0$ and treatment effect $\tau_j$ for all $j$. Here we interpret $\theta_j$ as a ratio between the treatment effect $\tau_j$ and the standard error $\sigma_j$ (possibly rescaled by the square-root of the sample size). In this setting, the diagonal entries of the covariance matrix are the same by construction. Note that the researcher can still choose the overall sample size (see Example 8).

Suppose that the researcher only knows $\theta$, i.e the effect measured in standard deviations, but not necessarily of $(\tau, \sigma)$ separately. To encode such uncertainty about $(\tau, \sigma)$, we assume that the researcher has a prior $(\tau, \sigma) \sim P_\theta$, which depends on $\theta$. Here $u(\theta, \{j\}) = E[\tau_j|\theta]$ is the expected treatment effect $\tau_j$ given $\theta$ under the prior $P_\theta$.

We formalize this framework in the following assumption.

Assumption 6 (Normality with partial ex-ante knowledge). Suppose that for all $j$, $\theta_j = \tau_j/\sigma_j$ for some $\sigma_j > 0$, with $|\omega_j \theta_j| < M < \infty$. Let $(\tau, \sigma) \sim P_\theta$ and $u(\theta, \{j\}) = E[\tau_j|\theta]$. For an experiment with treatments $J$, let $X|(\tau, \sigma) \sim N(\theta J, \Sigma)$. The class of designs $S(J)$ is such that $\Sigma_{i,i} = \Sigma_{j,j} \in (\underline{\gamma}, \bar{\gamma})$ for all $i, j \in \{1, \ldots, |J|\}$, for some constants $0 < \underline{\gamma} < \bar{\gamma} < \infty$. 

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Note that here the diagonal entries of $\Sigma$ are the same not because the variances are the same, but because we think of $X_j$ as the estimated effect divided by its standard deviation (possibly multiplied by a constant that can depend on the overall sample size as in Example 8). Therefore, the condition that $\Sigma$ has the same diagonal entries holds by definition.

The following proposition characterizes the difference between the worst-case welfare of the separate $t$-tests in Proposition 5 and the maximin optimal solution. We write $t$ in lieu of $t(J, \Sigma)$ to alleviate the notation.

**Proposition 8.** Let Assumptions 2, 3, and 6 hold. Then the following hold:

(i) For $r^t$ defined in Equation (20) with $t = \Phi^{-1}\left(1 - \frac{C(J, \Sigma)}{b_0(J)}\right)$, we have that

$$\min_{\theta} v_r(\theta; J^*_t, \theta, \Sigma^*_r, \theta) - \max_{r} \min_{\theta} v_r(\theta; J^*_t, \theta, \Sigma^*_r, \theta) \geq -M \max_{J \in \mathcal{J}} \min_{\sigma' \in \mathbb{R}^+} \sum_{j \in J} \left| \sigma' - \mathbb{E}[\sigma_j | \theta] \right|.$$

(ii) Suppose in addition that $\mathbb{E}[\sigma_j | \theta] = \sigma'$ for all $j$ and some scalar $\sigma' > 0$. Then, $r^t$ defined in Equation (20) with $t = \Phi^{-1}\left(1 - \frac{C(J, \Sigma)}{b_0(J)}\right)$ is maximin and globally optimal.

Proposition 8(i) shows that $t$-tests are optimal up to an error of order equal to the distance between the median standard deviation $\sigma$ and the expected standard deviation $\mathbb{E}[\sigma_j | \theta]$. Proposition 8(ii) also shows that whenever $\mathbb{E}[\sigma_j | \theta]$ is the same (in expectation) for all $j$, separate $t$-testing is maximin and globally optimal. This is the case, for example, when researchers have priors over standardized treatment effects.

**B.2 Two-sided tests**

The model in Section 3 naturally justifies one-sided hypothesis testing. To move away from one-sided hypothesis testing we need to allow policymakers to take actions different from the baseline treatment of “do nothing” when no recommendations is made. We therefore consider a model where a policymaker, who does not coincide with the planner, may implement treatments if no recommendation is made. This justifies two-sided testing.

Consider a model where the researcher reports a vector of recommendations, $r(X)$, and the sign of each statistic $j$, $\text{sgn}(X_j)$, for which $r_j(X) = 1$. That is, for each treatment, the researcher reports either a positive recommendation ($r_j(X) = 1$ and $\text{sgn}(X_j) = 1$), a negative recommendation ($r_j(X) = 1$ and $\text{sgn}(X_j) \in \{-1, 0\}$), or no recommendation ($r_j(X) = 0$).
The key feature of the model is that if \( r_j(X) = 0 \), the policymaker may or may not implement treatment \( j \). Define the indicator \( p \), where \( p = 1 \) if the policymaker implements treatments with \( r_j(X) = 0 \) and \( p = 0 \) otherwise. The planner does not know \( p \), and we consider a worst case approach with respect to \( p \), consistent with the maximin approach we consider in the main text. If \( r_j(X) = 1 \), the policymaker implements treatment \( j \) if \( \text{sgn}(X_j) = 1 \) and does not implement the treatment if \( \text{sgn}(X_j) \in \{-1, 0\} \).

Welfare conditional on experimentation is \( \sum_{j \in J} r_j(X)1\{\text{sgn}(X_j) = 1\} \theta_j + p \sum_{j \in J}(1 - r_j(X)) \theta_j \), which can be rewritten (up to constant terms) as

\[
\beta^\text{two}_r(\theta, J, \Sigma, p) = (1 - p) \sum_{j \in J} \omega_jr_j(X, J, \Sigma)1\{\text{sgn}(X_j) = 1\} - p \sum_{j \in J} \omega_jr_j(X, J, \Sigma)1\{\text{sgn}(X_j) \in \{-1, 0\}\} \theta_j p - C(J, \Sigma).
\]

We consider maximin testing protocols with respect to \( \theta \) and \( p \). The following proposition shows that standard separate two-sided \( t \)-tests are maximin optimal in this modified model.

**Proposition 9** (Maximin optimality of two-sided tests). Consider the model described in this section and the testing protocol \( \tilde{r}(X) = (\tilde{r}_1(X), \ldots, \tilde{r}_{|J|}(X))^\top \), where

\[
\tilde{r}_j(X) = 1 \left\{ \frac{|X_j|}{\sqrt{\Sigma_{jj}}} \geq \Phi^{-1} \left( 1 - \frac{C(J, \Sigma)}{b\bar{\omega}(J)} \right) \right\}, \quad \forall j \in J.
\]

Let Assumptions 2, 3, and 4 hold. Then, \( \tilde{r} \) is maximin optimal, i.e.,

\[
\tilde{r} \in \arg\max_{r \in R} \min_{\theta \in \Theta} \min_{p \in \{0, 1\}} \beta^\text{two}_r(\theta, J^*, \Sigma^*, \theta, p), \text{ where } (J^*, \Sigma^*, \theta^*, p) \in \arg\min_{J \in \mathcal{J}, \Sigma \in \mathcal{S}(J)} \beta^\text{two}_r(\theta, J, \Sigma, p).
\]

The critical value of the optimal two-sided \( t \)-tests is the same as for the one-sided \( t \)-tests in Proposition 5. This is because the planner is maximin with respect to \( p \). Using similar arguments as in the main text, one can show that two-sided \( t \)-tests are also globally optimal.

**B.3 Interactions between treatments**

The results in Section 3 apply to a broad class of researcher benefit and welfare functions, including settings with interactions between treatments. The presence of interactions (in particular in the researcher’s benefits) may justify notions of size control that are different
than average size control. In this section, we outline the conclusions that one can draw when there are additional interactions.

Following Example 4, we start by replacing the linear benefits function in Assumption 2 with a threshold function, where studies yield a (constant) positive payoff if and only if they produce sufficiently many findings:

$$\beta_r(\theta, J, \Sigma) = b \int 1\{\sum_{j \in J} r_j(x) \geq 1\} dF_{\theta}(x) - C(J, \Sigma) \quad (30)$$

With a threshold crossing payoff function, the incremental value of a finding to the researcher depends on the number of other findings. For this class of protocols, a necessary condition for maximin optimality is that

$$1 - \mathbb{P}(\sum_{j \in J} r_j(X) = 0|\theta) \leq C(J, \Sigma)/b, \quad \forall \theta \in \Theta_0(J). \quad (31)$$

This leads to more complicated optimal hypothesis testing protocols that depend on the joint distribution of $X$.

Suppose first that $r_j(X) \perp r_{j'}(X)$ with $j \neq j'$, which is satisfied in settings where $X$ has independent entries and $r_j$ only depends on $X_j$. An empirical example is when studying treatment effects on independent subgroups of individuals, and considering testing protocols that only depend on the effect on a given subgroup. Under independence, optimal protocols satisfy $\mathbb{P}(r_j(X) = 1) = p^*$ for some $p^*$ and impose that $1 - (1 - p^*)^{|J|} \leq C(J, \Sigma)/b$ so that $p^* \leq 1 - (1 - C(J, \Sigma)/b)^{1/|J|}$. When $C(J, \Sigma)/b = \alpha$, which is equivalent to assuming that the costs are constant in the number of findings, we can show that the right-hand side is of order $1/|J|$ as $|J| \to \infty$. Thus, asymptotically, fixed-cost research production functions again rationalize Bonferroni-type corrections.

Absent restrictions on the correlation structure, one can construct maximin optimal protocols by invoking Proposition 4, bounding $1 - \mathbb{P}(\sum_{j \in J} r_j(X) = 0|\theta)$ as

$$1 - \mathbb{P}(\sum_{j \in J} r_j(X) = 0|\theta) \leq \sum_{j \in J} \mathbb{P}(r_j(X) = 1|\theta),$$

and imposing maximin optimality with respect to this upper bound. This type of correction again amounts to a Bonferroni-type correction with a constant cost function.

Finally, suppose in addition that (combinations of treatments) are exclusive so that $\sum_{j \in J} r_j(X) \leq 1$ and $X$ satisfy Assumption 4 with in addition $\Sigma$ being a diagonal matrix (i.e.
X_j are effects estimated on independent subgroups). This introduces an additional form of interaction because only one entry of r can be positive (e.g., encoding any form of interaction effects in the welfare function). An example of maximin optimal protocol is the one where \( r_j(X) = 1\{j \in \arg \max_j X_{j'} \} \times 1\{\max_j X_{j'} \geq t\} \) where the threshold t must satisfy the restriction in Equation (31), such that \( \mathbb{P}(\max_j X_{j'} \geq t|\theta) \leq C(J, \Sigma)/b \) for all \( \theta \in \Theta_0(J) \). This directly relates to weak FWER control.

### B.4 Comparisons with other notions of optimality

In this section, we compare the proposed notion of optimality to other notions of optimality. We consider a simplified version of the model in the main text where the researcher only decides whether or not run an experiment with a given set of treatments \( \bar{J} \) and design \( \Sigma \).

**Assumption 7.** Suppose that \( J \in \mathcal{J} = \{\emptyset, \bar{J}\} \) for some \( \bar{J} \), with \( |\bar{J}| > 1 \), with \( \mathcal{S}(\bar{J}) = \{\Sigma\} \) for some positive definite \( \Sigma \).

Assumption 7 simplifies the subsequent analysis, while allowing us to highlight the main insights. We keep the notation \( J_{r,\theta}^* \) and \( \Sigma_{r,\theta}^* \), as in Equation (4), where \( J_{r,\theta}^* \) and \( \Sigma_{r,\theta}^* \) now reflect the researcher’s optimal choices over the restricted sets \( \mathcal{J} \) and \( \mathcal{S}(\bar{J}) \) in Assumption 7.

#### B.4.1 Uniformly most powerful testing protocols

A natural approach for choosing optimal hypothesis testing protocols would be to set \( \lambda = 0 \) and choose maximin protocols that uniformly dominate all other maximin protocols, as Tetenov (2016) does in the \( |\bar{J}| = 1 \) case. This corresponds to looking for uniformly most powerful (UMP) tests in the terminology of classical hypothesis testing. Unfortunately, this approach is not applicable in our context since there is no dominant maximin protocol.

**Proposition 10** (No dominant maximin protocol). *Let Assumptions 2, 3, and 7 hold. Suppose that \( 0 < C(\bar{J}, \Sigma) < \bar{\omega}(\bar{J}) \). Then there exists a compact parameter space \( \Theta \), a distribution \( \{F_\theta, \theta \in \Theta\} \), and weights \( \omega > 0 \) such that no maximin protocol (weakly) dominates all other maximin protocols. Moreover, there exists such a distribution \( F_\theta \) satisfying Assumption 4.*

In the terminology of classical hypothesis testing, Proposition 10 states that there are settings in which no UMP tests exist with multiple treatments. This is an important differ-
ence between the MHT case we study and the single hypothesis case considered in Tetenov (2016), where UMP protocols exist.

Note that Proposition 10 does not imply, however, that there are no maximin protocols that are strictly dominated. For example, t-tests with $t = \infty$ are maximin optimal (Proposition 5) but dominated by protocol (20) with $t = \Phi^{-1}(1 - C(J, \Sigma)/b\bar{\omega}(J))$.

### B.4.2 Local power

Here we compare maximin protocols in terms of their power over a local (to $\theta = 0$) alternative space. We consider a notion of local power with the property that locally most powerful protocols are also admissible when $\lambda = 0$. This notion of local power is inspired by the corresponding notions in Section 4 of Romano et al. (2011) and Chapter 9.2 of Lehmann and Romano (2005b). We show that any globally most powerful protocol is also locally most powerful (and thus admissible if $\lambda = 0$) under linearity and normality.

We start by defining a suitable local alternative space.

**Definition 3 ($\epsilon$-alternatives).** For $\epsilon > 0$, define the local alternative space (with an abuse of notation) as

$$\Theta_1(\epsilon) := \left\{ \theta : \theta_j = \epsilon \text{ for some } j, \theta_{j'} \in [0, \epsilon] \text{ for all other } j' \right\}.$$  

The set of $\epsilon$-alternatives $\Theta_1(\epsilon)$ is the set of parameters for which, for some policy decision, welfare is strictly positive by $\epsilon$.

Based on Definition 3, we introduce the following notion of local power.

**Definition 4 (Locally more powerful).** A testing protocol $r$ is locally more powerful than $r'$ if

$$\liminf_{\epsilon \downarrow 0} \left\{ \frac{1}{\epsilon} \inf_{\theta \in \Theta_1(\epsilon)} v_r(\theta, J^*_r, \Sigma^*_r) - \frac{1}{\epsilon} \inf_{\theta' \in \Theta_1(\epsilon)} v_{r'}(\theta', J^*_r, \Sigma^*_r) \right\} \geq 0.$$  

(32)

Definition 4 introduces a partial ordering of protocols based on their worst-case performance under $\epsilon$-alternatives. It considers parameter values in an alternative space that contains the origin as $\epsilon \to 0$. The rescaling by the location parameter $\epsilon$ avoids trivial solutions. Under the notion of local power in Definition 4, the planner prioritizes power for detecting small effects.

The next proposition characterizes locally most powerful protocols. For simplicity, we focus on settings with equal weights $\omega_j = 1$ for all $j$. The results extend straightforwardly to settings with general weights.
Proposition 11 (Separate size control is locally most powerful). Let Assumptions 2, 3, 4, and 7 hold with \( \omega_j = 1 \) for all \( j \). Then \( r^* \in \mathcal{R} \) is maximin optimal and locally most powerful if and only if it satisfies Equation (13) and

\[
P(r^*_j(X; J, \Sigma) = 1|\theta = 0) = \frac{C(J, \Sigma)}{b\omega(J)} \quad \forall j \in \{1, \ldots, J\}.
\] (33)

The following corollary shows that separate \( t \)-tests are also locally optimal.

Corollary 3 (Separate \( t \)-tests are locally most powerful). Let Assumption 1 and the conditions in Proposition 11 hold. Then for any \( \lambda > 0 \), the globally optimal \( t \)-test with \( t = \Phi^{-1}(1 - \frac{C(J, \Sigma)}{b\omega(J)}) \) in Proposition 5 is also maximin optimal and locally most powerful.

The proof follows directly from Propositions 5 and 11. More generally, any globally most powerful test (weakly maximin and unbiased) is locally most powerful when at \( \theta = 0 \) all rejection probabilities are the same. Therefore, our notion of global optimality in the main text can be viewed as a refinement of notions of most powerful tests inspired by the literature on hypothesis testing.

B.4.3 Globally optimal protocols, subjective utility, and weighted average power

In the main text, we consider the planner utility \( U(r; \lambda, \pi) \) defined in (8). Here we compare \( U(r; \lambda, \pi) \) to the following alternative planner utility function:

\[
U'(r; \lambda, w) = \min_{\theta \in \Theta} v_r(\theta, J^*_r, \Sigma^*_r) + \lambda \int v^*_r(\theta, J^*_r, \Sigma^*_r)w(\theta)d\theta,
\] (34)

Unlike \( U \), which captures the intrinsic value of knowledge generation by integrating over whether an experiment is conducted, \( U' \) directly integrates over social welfare \( v_r \) with weights \( w \). The utility function \( U' \) is equivalent to the one considered in Gilboa and Schmeidler (1989) and Banerjee et al. (2020). It has a decision-theoretic interpretation Gilboa and Schmeidler (1989) and is closely related to Huber’s \( \varepsilon \)-contamination model (see Banerjee et al., 2020).

It is natural to ask whether there are maximin protocols that maximize the second component of \( U' \) for any \( w \). Such a result would guarantee that for any choice of \( w \) (and \( \lambda \)), we can always find a maximizer of \( U' \), as was the case with the planner utility \( U \) in the main text. We show that such a protocol does not exist. For simplicity, we state the result for \( \omega_j = 1 \) for all \( j \).
Proposition 12 (Optimal protocols depend on weights). Suppose that Assumptions 2, 3, 4, and 7 hold with $\omega_j = 1$ for all $j$. For any two maximin protocols $r^*$ and $r'$ with $r^* \neq r'$, there exists a set of weights $w(\theta) : \int_{\Theta_1} w(\theta) = \int_{\Theta} w(\theta)$ such that
\[
\int_{\Theta_1} (v_{r^*}(\theta, J^*_r, \Sigma^*_r) - v_{r'}(\theta, J'^*_r, \Sigma'^*_r)) w(\theta) d\theta < 0.
\]

Proposition 12 highlights an important limitation of maximizing weighted average welfare. The optimal hypothesis protocol depends on the weights $w$. This sensitivity to the choice of weights is undesirable in practice because choosing suitable weights over the high-dimensional alternative space $\Theta_1$ is typically very difficult and somewhat arbitrary. By contrast, the results in Section 4.2 show that the proposed model yields optimal protocols that satisfy unbiasedness—a standard requirement for statistical tests—and do not depend on $\pi$ or $\lambda$.

Nevertheless, it is possible to show that $U$ is approximately equal to $U'$ for a suitable choice of the parameter spaces $\Theta'_1 \subset \Theta_1$. The key insight here is that the weight $\pi$ can take arbitrary positive values on $\Theta_1$ and therefore can approximately match the welfare effects multiplied by the weights $w$ (over the positive orthant $\Theta_1$). This implies that any protocol that maximizes $U$ for all $(\lambda, \pi)$, such as the one in Proposition 5, also maximizes $U'$ for any $w$ supported on $\Theta'_1 \subset \Theta_1$ up-to a possibly small error. We formalize this intuition in the following proposition. To state the result, let $v_{r=1}(\theta) = v_{r=1}(\theta, \bar{J}, \Sigma)$, where $r = 1$ is a protocol with $r_j(X) = 1$ almost surely for all $j$.

Proposition 13. Let Assumption 7 hold. Let $\pi(\theta) = v_{r=1}(\theta) w(\theta)$ and $w(\theta)$ be a weight (i.e., $w(\theta) \geq 0$) with $\int_{\Theta_1} w(\theta) d\theta = \int_{\Theta} w(\theta) d\theta = 1$ for some $\Theta'_1 \subset \Theta_1$. Then
\[
\left| U'(r; \lambda, w) - U(r; \lambda, \pi) \right| \leq \lambda \max_{\theta \in \Theta'_1} |v_{r=1}(\theta) - v_r(\theta, \bar{J}, \Sigma)|
\]

Proposition 13 shows that we can bound the difference between $U$ and $U'$ by the difference in social welfare when $r = 1$. To gain further insights, suppose that $v_1(\theta, \bar{J}, \Sigma)$ is linear (Assumption 3) and $\omega_j = 1$ for all $j$, so that we can write
\[
v_{r=1}(\theta) - v_r(\theta, \bar{J}, \Sigma) = \sum_{j \in \bar{J}} \theta_j \left( 1 - \mathbb{E}[r_j(X)|\theta] \right).
\]
It follows that for any consistent testing protocol, the difference between $U'$ and $U$ converges to zero for any alternative space $\bar{\Theta}'_1$ that contains strictly positive effects.\(^{25}\) As a result, the globally optimal separate $t$-tests in Proposition 5 are also approximately optimal under $U'$. We illustrate this in the following corollary.

**Corollary 4.** Suppose that Assumptions 1, 2, 3, 4, and 7 hold. Consider $r^t$ defined in Proposition 5 with $t = \Phi^{-1}\left(1 - \frac{C(j; \Sigma)}{\omega(j)}\right)$. Then for any $w(\theta)$ satisfying the conditions in Proposition 13 with $\bar{\Theta}'_1 = \left\{ \theta : u(\theta, j) > \mu, \forall j \in \bar{J} \right\}$ for a positive constant $\mu$ and any $\lambda \geq 0$,

$$\max_r U'(r; \lambda, w) - U'(r^t; \lambda, w) \leq \lambda \sum_{j \in \bar{J}} \Phi\left(t - \frac{\mu}{\Sigma_{j,j}}\right)$$

**C  Review of single hypothesis case in Tetenov (2016)**

Tetenov (2016) considers a game between an informed agent and a regulator, where the agent decides whether to run a pre-specified experiment. We explain his results using the terminology of our framework. Define the *null space* of parameter values as the set of parameters such that implementing the (single) treatment being studied would reduce welfare, $\Theta_0 := \{ \theta : u(\theta) < 0 \}$. Similarly, define the *alternative space* of parameter values as the set of parameters such that the treatment increases welfare $\Theta_1 := \{ \theta : u(\theta) \geq 0 \}$. Welfare is $v_r(\theta) = u(\theta)$ if $\int r(x) dF_\theta(x) \geq C$, where $C$ is the cost of running the experiment, and zero otherwise. That is, welfare is non-zero if the expected payoff from experimenting, $\int r(x) dF_\theta(x)$, is larger than the cost of experimentation.

To justify single hypothesis testing, Tetenov (2016) focuses on maximin optimal testing protocols, i.e. testing protocols that maximize worst-case welfare, $r^* \in \arg \max_{r \in \mathcal{R}} \min_{\theta \in \Theta} v_r(\theta)$. Proposition 1 in Tetenov (2016) shows that a testing protocol is maximin optimal if and only if $\int r^*(x) dF_\theta(x) \leq C$ for all $\theta \in \Theta_0$. The model thus rationalizes standard size control.

To select among the many alternative maximin testing protocols, Tetenov (2016) provides admissibility results under an additional monotone likelihood ratio property. He shows that admissible testing protocols satisfy the following condition $\int r^*(x) dF_\theta(x) = C$, with the

\(^{25}\)A testing protocol is consistent if $E[r_j(X)|\theta] \to 1, \theta \in \hat{\Theta}_1$, where the rate of convergence depends on the sample size via $\Sigma$.\]
testing protocol taking the form of threshold crossing protocols, \( r(X) = 1\{X \geq t^*\} \). This result provides a formal justification for standard (one-sided) tests.

### D Proofs of results in main text

To simplify the exposition, we will sometimes write \( C \) instead of \( C(J, \Sigma) \) and \( P(r_j(X) | \theta) \) instead of \( P(r_j(X) = 1 | \theta) \) whenever it does not cause any confusion. Without loss of generality, unless otherwise specified, we normalize \( b \) instead of \( P \).

Because \( \Theta \) is a compact space, without loss of generality, we will assume that \( \{|\theta_j| \leq M \text{ for some finite constant } M < \infty \text{ and all } j\} \). We will use that \( \Theta_0(J) \neq \emptyset \) for all \( J \neq \emptyset \) and \( \Theta_1 \neq \emptyset \) (as discussed in the main text). Finally, we will write \( t \) in lieu of \( t(J, \Sigma) \) to alleviate the notation.

#### D.1 Proof of Proposition 1

The proof proceeds in two steps. We first provide an equivalent condition for maximin optimality and then use this condition to prove the result.

**Step 1: Equivalent condition for maximin optimality.** Define the worst-case \( \theta \) as a function of the protocol \( r \) as \( \theta^*(r) = \min_{\theta \in \Theta} v_r(\theta) \). Observe that since \( \Theta_0(J) \neq \emptyset \) for all \( J \neq \emptyset \), we have that \( v_r(\theta, J, \Sigma) \leq 0 \) for any \( r \in \mathcal{R}, J \in \mathcal{J}, \theta \in \Theta_0(J), \Sigma \in \mathcal{S}(J) \). It follows that a protocol is maximin optimal if and only if \( v_r(\theta^*(r), J^*_r, \Sigma^*_r) = 0 \).

To see this, note first that \( v_r(\theta^*(r), J^*_r, \Sigma^*_r) = 0 \) since the planner can always choose \( \bar{r}(X; J, \Sigma) = (0, 0, \ldots, 0) \) for all \( (X, J, \Sigma) \) and obtain \( v_r(\theta, J^*_r, \Sigma^*_r) = 0 \) for all \( \theta \in \Theta \). We now show that \( v_r(\theta^*(r), J^*_r, \Sigma^*_r) \leq 0 \) by contradiction. Suppose not. Then it must be that \( J^*_r \neq \emptyset \) and because \( \bigcap_{J \in \mathcal{J} \setminus \emptyset} \Theta_0(J) \neq \emptyset \) by assumption, we can pick \( \theta' \in \bigcap_{J \in \mathcal{J} \setminus \emptyset} \Theta_0(J) \), with \( J^*_r \) corresponding to the researcher’s best action. Then because of how we picked \( \theta' \), \( v_r(\theta', J^*_r, \Sigma^*_r) < 0 \leq \min_{\theta \in \Theta} v_r(\theta, J^*_r, \Sigma^*_r) \), which leads to a contradiction.

**Step 2: Proof of main result.** To prove the “if” direction, we only need to show that under \( r^* \), \( v_r(\theta^*(r^*), J^*_r, \Sigma^*_r) \geq 0 \) (which, by the argument in Step 1, implies that \( v_r(\theta^*(r^*), J^*_r, \Sigma^*_r) = 0 \)). Equation (13) implies that \( v_r(\theta, J^*_r, \Sigma^*_r) = 0 \) for all \( \theta \in \Theta_0^0(r^*) \), so that \( v_r(\theta^*(r^*), J^*_r, \Sigma^*_r) = 0 \) if \( \theta^*(r^*) \in \Theta_0^0(r^*) \). If, instead,
\[ \theta^*(r^*) \in \Theta \setminus \Theta_0^*(r^*) \], we have that \[ v_r(\theta^*(r^*), J^*_{r,\theta^*(r^*)}, \Sigma^*_{r,\theta^*(r^*)}) \geq 0 \] by construction of \[ r^* \].

We now prove the “only if” direction, showing that such conditions are necessary for any given \( r \in R \) to be maximin optimal. Consider the case where \( \beta_r(\theta; J^*_{r,\theta^*}, \Sigma^*_{r,\theta^*}) > 0 \) for some \( \theta \in \Theta \). Then we have \( v_r(\theta^*(r); J^*_{r,\theta^*(r)}, \Sigma^*_{r,\theta^*(r)}) \leq v_r(\theta; J^*_{r,\theta^*}, \Sigma^*_{r,\theta^*}) < 0 \) for some \( \theta \in \Theta \). Suppose instead that \( v_r(\theta; J^*_{r,\theta^*}, \Sigma^*_{r,\theta^*}) < 0 \), for some \( \bar{\theta} \in \Theta \). Then similarly \( v_r(\theta^*(r); J^*_{r,\theta^*(r)}, \Sigma^*_{r,\theta^*(r)}) \leq v_r(\bar{\theta}; J^*_{r,\bar{\theta}}, \Sigma^*_{r,\bar{\theta}}) < 0 \), completing the proof.

### D.2 Proof of Proposition 2

Recall that \( \int_{\Theta} \pi(\theta)d\theta = \int_{\Theta_1} \pi(\theta)d\theta \) and \( \pi(\theta) \geq 0 \) under Assumption 1. Notice that any maximin protocol maximizes \( \min_{\theta \in \Theta} v_r(\theta, J^*_{r,\theta^*}, \Sigma^*_{r,\theta^*}) \) by definition. Notice further that for any \( \theta \in \Theta_1 e^*_r(\theta) = 1\{\beta^*_r(\theta) \geq 0\} \) for \( \theta \in \Theta_1 \), where the weak inequality follows from the tie-breaking rule (since for \( \theta \in \Theta_1 \) welfare effects are always weakly positive). This implies that the set of protocols that maximize \( \lambda \int_{\Theta} e^*_r(\theta)\pi(\theta)d\theta \) coincides with the set of protocols satisfying \( \int_{\Theta} 1\{\lambda \beta^*_r(\theta) \geq 0\} \pi(\theta)d\theta = \int_{\Theta_1} \pi(\theta)d\theta \), so that \( \lambda \int_{\Theta} e^*_r(\theta)\pi(\theta)d\theta = \lambda \int_{\Theta_1} \pi(\theta)d\theta \), which is the largest value it can achieve (since \( \pi(\theta) \geq 0 \)). This completes the proof.

### D.3 Proof of Proposition 3

We only prove Part (i). Part (ii) is a direct implication of Part (i).

The “if” direction is a direct corollary of Proposition 2. The “only if” direction proceeds by contraposition. Take any protocol \( r \) that is not maximin. Then for \( \lambda = 0 \) this protocol is not globally optimal. Take any protocol \( r \) that is not unbiased (but could be maximin). Then because \( r \) is not unbiased we can find a set of weights \( \pi \in \Pi \) such that \( \int e^*_r(\theta)\pi(\theta)d\theta \) is not globally optimal for \( \lambda > 0 \). The proof completes because we assumed existence of unbiased maximin protocol.

### D.4 Proof of Proposition 4

In this proof, we make the dependence on the researcher’s payoff function \( \beta \) explicit and write \( (J^*_{r,\theta}(\beta), \Sigma^*_{r,\theta}(\beta)) = \arg \max_{J \in \mathcal{J}, \Sigma \in \mathcal{S}(J)} \beta_r(\theta, J, \Sigma) \).

**Proof of Part (i).** By Corollary 1, a protocol is maximin optimal for given \( \beta_r(\theta, J, \Sigma) \) if \( v_r(\theta; J^*_{r,\theta}(\beta), \Sigma^*_{r,\theta}(\beta)) \geq 0 \) for all \( \theta \in \Theta \).
Take $\theta \in \Theta_0(J)$. Then, since $\beta_\ast^r(\theta, J, \Sigma) \leq \beta_\ast^r(\theta, J, \Sigma)$ for all $\theta \in \Theta, J \in \mathcal{J}, \Sigma \in \mathcal{S}(J)$, it follows that $\beta_\ast^r(\theta, J, \Sigma) \leq 0$. In addition, we have that $1\{\beta_\ast(\theta, J, \Sigma) \geq 0\} \geq 1\{\beta_\ast^r(\theta, J, \Sigma) \geq 0\}$. Therefore, $v_\ast(\theta; J_\ast^r(\beta'), \Sigma_\ast^r(\beta')) \geq 0$ under the tie-breaking rule.

Take $\theta \not\in \Theta_0(J)$ (the subsequent argument applies to any $\theta \in \Theta_0(J)$, and any $J \in \mathcal{J} \setminus \emptyset$). There are two cases: $J_\ast^r(\beta') \neq \emptyset$ and $J_\ast^r(\beta') = \emptyset$. Consider first the case where $J_\ast^r(\beta') \neq \emptyset$. Then $r^*$ satisfying the condition in Corollary 1 implies that $v_\ast(\theta; J, \Sigma)1\{\beta_\ast(\theta, J, \Sigma) \geq 0\} \geq 0$ for all $\Sigma \in \mathcal{S}(J)$. Because this argument applies to all $J \in \mathcal{J} \setminus \emptyset$, it must be (since $1\{\beta_\ast(\theta, J_\ast^r, \Sigma_\ast^r) \geq 0\} = 1$ if $J_\ast^r \neq \emptyset$) that $v_\ast(\theta, J_\ast^r(\beta'), \Sigma_\ast^r(\beta')) \geq 0$ when $J_\ast^r(\beta') \neq \emptyset$ proving maximin optimality case in this case. Suppose instead that $J_\ast^r(\beta') = \emptyset$. Then we must have $v_\ast(\theta, J_\ast^r(\beta'), \Sigma_\ast^r(\beta')) = 0$ proving the claim.

**Proof of Part (ii).** The proof follows similarly to the proof of Proposition 2. The protocol $r^*$ described in (ii) maximizes the maximin component of the planner’s utility by construction. Moreover, it maximizes the second component of the planner’s utility because it guarantees that $\int e_\ast(\theta) d\pi'(\theta) = \int d\pi'(\theta)$ for all $\pi' \in \tilde{\Pi}$, by construction of $\tilde{\Pi}$.

**D.5 Proof of Proposition 5**

Maximin optimality of separate $t$-tests

We start by proving the “if” statement. We will prove that the conditions in Equation (14) hold for any $(J, \Sigma)$, with $J \neq \emptyset$ and $\Sigma$ satisfying Assumption 5. We will therefore suppress the dependence of $r(\cdot; J, \Sigma)$ on $(J, \Sigma)$ for notational convenience. Clearly, for any $J$ such that $|J| = 1$ Equation (14) trivially holds, since $P(r_j(X) = 1|\theta)$ is monotonically increasing in $\theta_j$ and constant in $\theta_{-j}$, where $\theta_{-j}$ denotes all elements in $\theta$ except for the $j$th element. It therefore suffices to focus on settings where $|J| > 1$. Recall that we write $\omega(J) = \sum_{j \in J} \omega_j$.

Because $P(r_j(X) = 1|\theta)$ is monotonically increasing in $\theta_j$ and constant in $\theta_{-j}$, the first condition in Equation (14) is satisfied for any $t \geq \Phi^{-1}(1 - C(J, \Sigma)/\omega(J))$. We now show that the second condition also holds.

To show this, it suffices to show that the worst-case objective function is weakly positive for any $t \geq \Phi^{-1}(1 - C(J, \Sigma)/\omega(J))$. With an abuse of notation, we denote by $\theta_j$ the treatment effect divided by $\sigma := \sqrt{\sum_{j,j} \omega_{jj}}$, which is finite and strictly positive by assumption. Note further that $C(J, \Sigma) = \omega(J)(1 - \Phi(t^*))$, $t^* = \Phi^{-1}(1 - C(J, \Sigma)/\omega(J))$ and that, for any
$t' \geq t^*$, we have $C(J, \Sigma) \geq \bar{\omega}(J)(1 - \Phi(t'))$.

Using such standardization, since $u(\theta, j) = \sigma \theta_j$ by Assumption 4 (where $\theta_j$ now denotes the treatment effect divided by $\sigma$) and $\Theta$ is compact, $\min_\theta v_{t'}(\theta, J, \Sigma)1\{\beta_{t'}(\theta, J, \Sigma) \geq 0\}$ must be bounded from below (since $\Sigma_{j, j} = \sigma^2 > \gamma > 0$). It follows that for any $t \geq t^*$, proving that $\min_\theta v_{t'}(\theta, J, \Sigma)1\{\beta_{t'}(\theta, J, \Sigma) \geq 0\}$ weakly positive is equivalent to proving weak positivity of the value of the objective function (at the optimum) in the following problem:

$$\min_{\theta \in [-M, M]^{|J|}} \sigma \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \theta_j, \quad \text{s.t.} \ \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \geq C(J, \Sigma) \geq \bar{\omega}(J)(1 - \Phi(t)).$$

(35)

Note that the value of the objective function in Equation (35) is bounded from below by the value of the following objective function (at the optimum)

$$\min_{\theta \in [-M, M]^{|J|}} \sigma \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \theta_j, \quad \text{s.t.} \ \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \geq \bar{\omega}(J)(1 - \Phi(t)), \quad (36)$$

since we relaxed the lower bound on the constraint. We focus on the case where $t$ is finite (if $t = \infty$ the planner’s utility is always zero and the result trivially holds). It therefore suffices to show that the value of the objective function (at the optimum) in Equation (36) is weakly positive to obtain the desired claim of maximin optimality.

In the following, we prove by contradiction that the objective in Equation (35) is weakly positive for any $t \geq \Phi^{-1}(1 - C(J, \Sigma)/\bar{\omega}(J))$ by first assuming that the objective of Equation (35) is strictly negative and then using a contradiction argument.

**Step 1: Preliminary observation.** Observe that welfare is negative only if the minimizer $\theta^*$ is such that for some $j$, $\theta_j^* < 0$, and for some other $j' \neq j$, $\theta_j'^* > 0$ because $\omega_j \geq 0$ for all $j$. To see this, note that to have negative welfare, there must exist some $\theta_j^* < 0$. Moreover, there must exist some $\theta_j'^* > 0$ because if $\theta_j^* < 0$ for all $j$ the constraint is violated. This shows that a necessary condition for welfare to be strictly negative is that for some $j \neq j'$, $\theta_j^* < 0$ and $\theta_j'^* > 0$.

**Step 2: Focus on interior solution.** Note that if we replace the constraint in Equation (36), $\theta_j \in [-M, M]^{|J|}$, with $\theta \in [-M', M']^{|J|}$, $M' > M$ for some large enough but finite $M'$, the solution to the corresponding optimization problem is a lower bound for Equation (36).

Now we argue that it suffices to focus on solutions $\theta^*$ in the interior of $[-M', M']^{|J|}$ for some (possibly) large but finite $M'$, as we replace $\theta_j \in [-M, M]^{|J|}$ with $\theta_j \in [-M', M']^{|J|}$.
to show negativity of the objective function. It suffices to show that $\theta_j \neq -\infty$ for all $j$ to claim that if the worst-case objective is negative, the minimizer must be in the interior of $[-M', M']^{|J|}$. To see why, observe that if $\theta^*_j = -\infty$ for at least one $j$, its contribution to the objective function is zero (since $z(1 - \Phi(t - z)) \to 0$ as $z \to -\infty$). However, it forces some of the elements of $\theta_{-j}$ to be positive via its impact on the constraint. Hence, if the minimum of the objective is strictly smaller than zero, there must exist a minimizer $\theta^*$, which is in the interior of $[-M', M']^{|J|}$ for some (possibly) large but finite $M'$. Therefore, we will replace $\theta_J \in [M, M]^{|J|}$ with $\theta_J \in [-M', M']^{|J|}$, $M' > M$ for $M'$ finite but large enough such that this constraint is not binding in the following steps.

**Step 3: Constraint qualification.** Next, we show for finite $t$ the KKT conditions are necessary for the optimality of $\theta^*_J$ in the interior of $[-M', M']^{|J|}$. Note that $t > -\infty$ by construction since $\bar{\omega}(J) > C(J, \Sigma)$ in Assumption 2. To show this we use the LICQ. Observe that the derivative of the constraint function is $-\sum_{j \in J} \omega_j \phi(t - \theta_j)$. Note that $\sum_{j \in J} \omega_j \phi(t - \theta_j) \neq 0$ for finite $t$ and any $\theta$ such that at least one $|\theta_j|$ is finite since $\omega_j > 0$ for all $j$.

**Step 4: Lagrangian.** We now study necessary conditions for the optimal solution of the problem in Equation (36). Consider the Lagrangian function

$$\sigma \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \theta_j + \nu \left[ \bar{\omega}(J)(1 - \Phi(t)) - \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \right] + \mu_{1,j}[\theta - M'] + \mu_{2,j}[-\theta - M'].$$

Now observe that by the argument in Step 2 and complementary slackness, we can focus on the cases where $\mu_{1,j} = \mu_{2,j} = 0$ for all $j$ (i.e. $\theta^*_J$ is an interior of $[-M', M']^{|J|}$ for some finite $M'$ which we can choose large enough). Taking first-order conditions of the Lagrangian and using the fact that $\omega_j > 0$ for all $j$, we obtain

$$\omega_j \phi(t - \theta_j) \theta_j + \omega_j (1 - \Phi(t - \theta_j)) = \frac{\nu}{\sigma} \omega_j \phi(t - \theta_j)$$

or, equivalently,

$$\frac{\nu}{\sigma} = \frac{1}{\phi(t - \theta_j)} \left[ \phi(t - \theta_j) \theta_j + (1 - \Phi(t - \theta_j)) \right]. \quad (37)$$

**Step 5: Contradiction argument.** We conclude the proof of the “if” direction using a contradiction argument showing that strict negativity of the objective function violates the necessary condition established as a preliminary observation in Step 1. Suppose that the
objective function is strictly negative. Then there must exists a \( j \) such that \( \theta^*_j < 0 \) and \( j' \neq j \) such that \( \theta^*_{j'} > 0 \). In addition, observe that using Equation (37), we can write

\[
0 > \theta^*_j = \frac{\nu}{\sigma} - \frac{(1 - \Phi(t - \theta^*_{j}))}{\phi(t - \theta^*_{j})} \quad 0 < \theta^*_{j'} = \frac{\nu}{\sigma} - \frac{(1 - \Phi(t - \theta^*_{j'}))}{\phi(t - \theta^*_{j'})}.
\]

Using the fact that \( t \) is finite, it follows that \( \frac{1 - \Phi(t - \theta^*_{j})}{\phi(t - \theta^*_{j})} < \frac{\nu}{\sigma} \frac{1 - \Phi(t - \theta^*_{j'})}{\phi(t - \theta^*_{j'})} \). Observe now that the expression implies \( \frac{1 - \Phi(z)}{\phi(z)} < \frac{1 - \Phi(z')}{\phi(z')} \) for some \( z < z' \). However, since \( (1 - \Phi(z))/\phi(z) \) is monotonically decreasing in \( z \) we have a contradiction.

Finally, we prove the “only if” statement. Take any \( t < \Phi^{-1}(1 - C(J, \Sigma)/\bar{\omega}(J)) \). Then by continuity of \( P(r(X) = 1|\theta) \) in \( \theta \) (which follows by Gaussianity of \( X \)), we can find a configuration of treatments \( \theta_j < 0 \) for all \( j \), such that \( \beta_r(\theta, J, \Sigma) > 0, \theta \in \Theta_0(J) \) violating the first condition in Equation (14).

**Global optimality of separate \( t \)-tests**

To establish global optimality it suffices to show that the researcher would chooses to experiment for all \( \theta \in \Theta_1 \) and any \( (J, \Sigma) \). This follows because \( B_{r^*}(0, J, \Sigma) = C(J, \Sigma) \) and \( B_{r^*}(\theta, J, \Sigma) \) is weakly increasing in each element of \( \theta \) under Assumption 2. Finally, to show that any other choice of \( t' \neq t \), \( r^{t'} \) is not globally optimal, it suffices to note that whenever \( t' < \Phi^{-1}\left(1 - \frac{C(J, \Sigma)}{\bar{\omega}(J)}\right) \) then the protocol \( r^{t'} \) is not maximin optimal, since, by continuity of the Gaussian CDF, we can take values of \( \theta \in (-\epsilon, \cdots, -\epsilon) \) for \( \epsilon \) sufficiently small so that the researcher’s best response is to experiment (and the planner’s utility is strictly negative). If instead \( t' > \Phi^{-1}\left(1 - \frac{C(J, \Sigma)}{\bar{\omega}(J)}\right) \), we can take values of \( \theta \in (\epsilon, \cdots, \epsilon) \) for some sufficiently small \( \epsilon > 0 \), so that the researcher’s best response is not to experiment, so that \( J^*_\theta = \emptyset \). The result therefore follows by Proposition 2.

**D.6 Proof of Proposition 6**

We will use the following lemma in the proof. To alleviate the notation, we keep the dependence of \( v_r(\theta; J, \Sigma) \) and \( \bar{v}_r(\pi; J, \Sigma) \) on \( (J, \Sigma) \) implicit whenever there is no ambiguity.

**Lemma 1** (Sufficient conditions for maximin optimality). \( r^* \) is \( \Pi^* \)-maximin optimal (Definition 1) if \( \inf_{\pi' \in \Pi^*} \bar{v}_{r^*}(\pi', J, \Sigma) \geq 0 \), for all \( (J, \Sigma) \).

**Proof.** Clearly, for \( J = \emptyset \) the result trivially holds. Therefore, it suffices to show that this holds also for \( J \neq \emptyset \). By construction, \( v_r(\theta) \leq 0 \) for all \( \theta \in \Theta_0(J) \) and all \( r \in \mathcal{R} \).
Because $\Theta_0(J) \neq \emptyset$ for all $r \in \mathcal{R}$, we have that $\int_{\Theta} v_r(\theta)d\pi'(\theta) \leq 0$ for all $\pi' \in \Pi'$ such that $\int_{\Theta_0(J)} \pi'(\theta)d\theta = 1$. Because $\Pi'$ is unrestricted, there exists at least one $\pi'$ such that $\int_{\Theta_0(J)} \pi'(\theta)d\theta = 1$. It follows that $\inf_{\pi' \in \Pi'} \tilde{v}_r(\pi') \leq 0$ for all $r \in \mathcal{R}$. Therefore, $r$ is maximin optimal if $\inf_{\pi' \in \Pi'} \tilde{v}_r(\pi') \geq 0$ for all $(J, \Sigma)$.

In view of Lemma 1, it suffices to show that the worst-case objective function is weakly positive for any $(J, \Sigma)$. Without loss of generality (since $\Sigma$ has non-zero and finite homogeneous component variances under Assumption 4), we normalize $\Sigma_{j,j} = 1$ for all $j$ and define $\theta_j$ as the treatment effect divided by $\sqrt{\Sigma_{j,j}}$ (which are homogeneous by assumption) so that $\theta_j \in [-M', M']$ for some finite $M'$ (because $\sqrt{\Sigma_{j,j}} > 0$ by assumption).

**Step 1: Preliminaries for maximin optimality.** By Lemma 1, to establish maximin optimality, it suffices to show that the solution to the following optimization problem is weakly positive

$$\min_{\pi' \in \Pi'} \int \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \theta_j d\pi'(\theta), \quad \text{s.t.} \quad \int \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) d\pi'(\theta) \geq \bar{\omega}(J)(1 - \Phi(t)). \quad (38)$$

Observe that since $C(J, \Sigma) > 0$, $t$ is finite.

**Step 2: Finite dimensional optimization program.** Because the optimization problem only depends on the marginal distributions of $\theta_j$, the maximization over $\pi' \in \Pi'$ can be equivalently rewritten as a minimization over marginal distributions $\pi'_1, \ldots, \pi'_J, \theta_j \sim \pi'_j$. By Theorem 1, result 1 in Gaivoronski (1986) (which essentially states that we can find an optimizer that is a point mass distribution), the value of the objective function in Equation (38) at its minimum is equal (under the given constraints) to the solution of the following program

$$\min_{\theta \in \Theta} \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \theta_j, \quad \text{s.t.} \quad \sum_{j \in J} \omega_j (1 - \Phi(t - \theta_j)) \geq \bar{\omega}(J)(1 - \Phi(t)). \quad (39)$$

At this point, the optimization program in Equation (39) is equivalent to the one in Equation (36) (up to a multiplicative factor in the objective function). Therefore, we can follow verbatim the proof of Proposition 5.

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26The conditions in the above reference are satisfied since $\Phi(t - \theta_j)$ is a continuous function in $\theta_j$, $\Theta$ is a compact space, and we can find a distribution so that the constraint holds with strict inequality. Note that the equality of the two objective functions follows directly by the definition of Lagrangian function in result 1, Theorem 1 of Gaivoronski (1986).
E Proofs of results in Online Appendix

E.1 Proof of Proposition 7

The proof follows from three observations. First, under $r^N$, the researcher will always choose $\Sigma_{j,j}$ under a variance-equalizing allocation, since otherwise her utility is weakly negative.

Second, the combination of variance-equalizing allocation and $r^N$ being optimal follows from the fact that in equilibrium the utility of the social planner evaluated at the variance-equalizing allocation and $r^N$ equals $\lambda$. This is the largest possible value it can take, since the maximin component of the planner’s utility is at most zero (which follows from arguments similar to those in the proof of Proposition 1) and the subjective utility from experimentation is at most $\lambda$.

Third, maximin and global optimality of $r^N$ under the variance-equalizing allocation follow from the same arguments as in the proof of Proposition 5.

E.2 Proof of Proposition 8

Proof of (i). By the same argument as in the proof of Proposition 1, we have that $\max_r \min_{\theta} v_r(\theta, J^*, \Sigma^*) = 0$. Take $\theta \in \Theta_0(J)$. Note that $r^*_j = 1\{X_j/\sqrt{\Sigma_{j,j}} \geq t\}$ is such that for $t = \Phi^{-1}(1 - C(J, \Sigma)/b\bar{\omega}(J))$, the researcher payoff is weakly negative for any $\theta \in \Theta_0(J)$ and all $J \in \mathcal{J}$. It follows by the tie-breaking rule that for all $J \in \mathcal{J}$, $\Sigma \in \mathcal{S}(J)$, we have that $\min_{\theta \in \Theta_0(J)} v_r(\theta, J, \Sigma) = 0$. Therefore, it suffices to focus on settings where $\theta \notin \Theta_0(J)$.

Fix an arbitrary scalar $\sigma' > 0$. Note that under Assumption 6, we have $X|(\tau, \sigma) \sim \mathcal{N}(\theta, \Sigma)$. Therefore, we can write (with $\theta$ and $\Sigma$ being non-stochastic)

$$v_r(\theta, J, \Sigma) = \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta]E[\tau_j|\theta] = \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta]E[\tau_j|\theta]$$

$$= \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta]E[\tau_j|\theta] = \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta]E[\tau_j|\theta].$$

Consider the following objective function (recalling the normalization $b = 1$)

$$\tilde{v}_r^*(\theta, J, \Sigma) := \min_{\theta \in \Theta} \sum_{j \in J} P(r^*_j(X_j, J, \Sigma) = 1|\theta) \omega_j \theta_j \sigma'$$

s.t. $\sum_{j \in J} \omega_j P(r^*_j(X, J, \Sigma) = 1|\theta) \geq C(J, \Sigma).$

(40)

The result in Proposition 7 implies that for any $\sigma'$, Equation (40) is weakly positive for any $\theta \in \Theta$, and in particular $\min_{\theta} \tilde{v}_r^*(\theta, J, \Sigma) = 0$ for all $J \in \mathcal{J}$, $\Sigma \in \mathcal{S}(J)$. This follows after a
simple change of variables where we use \( \tilde{\theta}_j = \sigma' \theta_j \) in lieu of \( \theta_j \) in Proposition 7, and since \( \sigma' \) does not depend on \( j \) and the diagonal entries of \( \Sigma \) are all the same under Assumption 6.

As a result, we can write
\[
\min_{\theta \in \Theta} v_r(\theta, J^*, \Sigma^*) = \min_{\theta \in \Theta} v_r(\theta, J^*, \Sigma^*) - \min_{\theta \in \Theta, J \in S(J)} \tilde{v}_r(\theta, J, \Sigma) + \min_{\theta \in \Theta, J \in S(J)} \tilde{v}_r(\theta, J, \Sigma) - \max_{\theta \in \Theta} \max_{J, \Sigma \in S(J)} v_r(\theta, J^*, \Sigma^*) \leq 0
\]

To complete the proof of the lower bound, note that for any \( \sigma' > 0 \), because \( |E[r_j^*(X)|\theta_j]\) \leq M, by Hölder’s inequality,
\[
\min_{\theta \in \Theta} \left[ v_r(\theta, J, \Sigma) - \tilde{v}_r(\theta, J, \Sigma) \right] = \max_{\sigma' \in \Theta} \min_{\theta \in \Theta} \sum_{j \in J} \omega_j E[r_j^*(X)|\theta_j] (E[\sigma_j|\theta] - \sigma') \geq \max_{\sigma' \in \Theta} \sum_{j \in J} M |E[\sigma_j|\theta] - \sigma'|
\]

The proof completes after taking the maximum over \( J \).

**Proof of (ii).** Whenever \( E[\sigma_j|\theta] = \sigma' \) for a constant \( \sigma' > 0 \), it follows that we can write
\[
v_r(\theta, J, \Sigma) = \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta] \leq \sum_{j \in J} \omega_j E[r_j(X, J, \Sigma)|\theta] \sigma'.
\]

Therefore, one can follow verbatim the proof of Proposition 5 to establish maximin optimality because as the objective function is the same as the one under Assumption 4 up-to a fixed positive constant \( \sigma' \) (and therefore weakly positive whenever the researcher chooses to experiment). Global optimality also follows verbatim from the proof of Proposition 5.

### E.3 Proof of Proposition 9

To prove the claim, following verbatim the argument in the proof of Corollary 1, it suffices to show that \( v_r^{\text{two}}(\theta, J, \Sigma, p)1\{\beta_r(\theta, J, \Sigma, p) \geq 0\} \geq 0 \) for all \( \theta \in \Theta, p \in \{0, 1\}, J \in J, \Sigma \in S(J) \). Define \( t^* = \Phi^{-1}(1 - \frac{C(J, \Sigma)}{b_0(J)}) \) and, for notational convenience, let \( \Sigma_{i,i} = 1 \) without loss of generality (under Assumption 4). Letting \( Z \sim \mathcal{N}(0, \Sigma) \), where \( \Sigma_{j,j} = 1 \) for all \( j \),
\[
E[r_j^T(X, J, \Sigma)1\{\text{sgn}(X_j) = 1\}|\theta_j] = \left[ 1 - \frac{\Phi(t^* - \theta_j) - \Phi(-\theta_j)}{1 - \Phi(-\theta_j)} \right] (1 - \Phi(-\theta_j)) = (1 - \Phi(t^* - \theta_j)).
\]

Similarly, we can write \( E[r_j^T(X, J, \Sigma)1\{s_j(X) = -1\}|\theta_j] = \Phi(-t^* - \theta_j) \). Collecting the terms, upon experimentation (occurring only if \( \beta_{r^*_r}(\theta, J, \Sigma, p) \geq 0 \))
\[
v_r^{\text{two}}(\theta, J, \Sigma, p) = \sum_{j \in J} \omega_j \theta_j \left[ (1 - \Phi(t^* - \theta_j))(1 - p) - p\Phi(-t^* - \theta_j^*) \right].
\]
To show maximin optimality, it suffices to show (by the same argument in Proposition 5)

\[
\min_{\theta, p} \sum_{j \in J} \omega_j \theta_j \left[ (1 - \Phi(t^* - \theta_j))(1 - p) - p\Phi(-t^* - \theta_j) \right]
\]

such that \( \sum_{j \in J} \omega_j \left[ (1 - \Phi(t^* - \theta_j))(1 - p) + p\Phi(-t^* - \theta_j) \right] \geq C(J, \Sigma) \)

is weakly positive.

Consider first the case where \( p = 0 \). In this case, we can show that Equation (41) is weakly positive using the arguments in the proof of Proposition 5.

Consider now the case where \( p = 1 \). Define \( \tilde{\theta}_j = -\theta_j \), and note that \( \Phi(-t^* + \tilde{\theta}_j) = 1 - \Phi(t^* - \tilde{\theta}_j) \). Then, we can use the arguments in the proof of Proposition 5 with \( \tilde{\theta}_j \) in lieu of \( \theta_j \) to show that (41) is weakly positive. This completes the proof.

**E.4 Proof of Proposition 10**

The researcher’s net benefit is \( \sum_{j \in J} P(r_j(X)|\theta) - C \), where we suppress the dependence of \( r(X, J, \Sigma) \) and \( C(J, \Sigma) \) on \( J \) and \( \Sigma \) to alleviate the notation and, as before, normalize \( b = 1 \) without loss of generality. In addition, we set \( \omega_j = 1 \) for all \( j \), although one could directly extend our reasoning to more general \( \omega \).

Recall that under Assumption 7, \( J = \{\emptyset, \bar{J}\} \). Therefore we can write the social planner’s utility accounting for the researcher’s best response as

\[
v_r(\theta) = \begin{cases} 
v_r(\theta, \bar{J}, \Sigma), & \text{if } \beta_r(\theta, \bar{J}, \Sigma) > 0 \\
\max\{v_r(\theta, \bar{J}, \Sigma), 0\}, & \text{if } \beta_r(\theta, \bar{J}, \Sigma) = 0 \\
0, & \text{otherwise,}
\end{cases} \quad (42)
\]

where the second case follows from the tie-breaking rule. We will refer to \( \Theta_0 \) as \( \Theta_0(\bar{J}) \) suppressing its dependence on \( \bar{J} \) whenever clear from the context.

Take the parameter space (suppressing its dependence on \( \bar{J} \) for expositional convenience)

\[ \Theta = \left\{ \theta \in [-M, M]^J \text{ such that } \text{sign}(\theta_1) = \text{sign}(\theta_2) = \cdots = \text{sign}(\theta_{J-1}) \right\} \]

To prove the statement we show that there exists a (set of) maximin protocol that strictly dominates all others over an arbitrary set \( \Theta' \subseteq \Theta \), and a different (set of) maximin testing protocol that it strictly dominates all others over some arbitrary set \( \Theta'' \cap \Theta' = \emptyset, \Theta'' \subseteq \Theta \). We choose \( \Theta' = (0, \cdots, 0, t) \) for a small \( t \) and \( \Theta'' = (t, \cdots, t, 0) \) for a small \( t \). We choose \( X \sim \mathcal{N}(\theta, I) \), which
satisfies Assumption 4. Observe that by definition of $\Theta_0$, $\Theta_0 \subseteq \tilde{\Theta}_0 = \{\theta : \theta_j \leq 0 \text{ for all } j\}$, where $\tilde{\Theta}_0$ also contains those elements that lead to weakly negative welfare.

**Step 1: Construction of the function class.** Define

$$R^1 = \{ r \in R : C \geq P(r_{j\mid j}^1(X)|\theta = 0) > \frac{C}{J} \text{ and } r \text{ is maximin} \}.$$ 

We claim that $R^1 \neq \emptyset$, i.e., there exists a function $r \in R^1$. An example is a threshold crossing testing protocol of the form

$$r_j(X) = 1\{X_j > t_j\}, t_j < j = \infty, t_j = \Phi^{-1}(1 - \min\{C, 1\}). \quad (43)$$

In addition, we observe that $\sup_{r \in R^1} P\left(r_{j\mid j}^1(X)|\theta, j = 0, \theta_{j < j} = 0\right) = \min\{C, 1\}$ from the example in Equation (43). Define $\tau = \min\{C, 1\}$ for the rest of the proof.

**Step 2: Comparisons with maximin protocols.** We now claim that for $\theta = (0, 0, \cdots, 0, t)$, for $t$ approaching zero, there exists a maximin testing protocol $r^1 \in R^1$ which leads to strictly larger welfare than any maximin decisions $r^2 \in M \setminus R^1$, where $M$ denotes the set of maximin protocols. To show our claim, it suffices to compare $r^1$ to any maximin testing protocol $r^2 \not\in R^1$ with $P\left(r_{j\mid j}^2(X)|\theta = 0\right) \leq \frac{C}{J}$. To see why, observe that whenever the above probability is between $\left(\frac{C}{J}, \tau\right]$, we contradict the statement that $r^2 \not\in R^1$. When instead $P\left(r_{j\mid j}^2(X)|\theta = 0\right) > \tau$, $r^2$ is not maximin optimal, since this implies that $C < 1$, which in turn implies that by Assumption 4, the researcher would experiment under $\left(\theta_j = -t, \theta_{j < j} = -t\right)$, for some small positive $t$, leading to strictly negative welfare.

**Step 3: Comparisons of welfare.** For $\theta = (0, 0, \cdots, 0, t)$ write

$$v_{\tau, 1}(0, 0, \cdots, 0, t) = t \times P\left(r_{j\mid j}^1(X_1, X_2, \cdots, X_j)|\theta_{-j} = 0, \theta_j = t\right).$$

By continuity of $P(\cdot)$ in $t$, it follows that we can choose $t > 0$ sufficiently small so that $v_{\tau, 1}(\theta = (0, 0, \cdots, t)) - v_{\tau, 2}(\theta = (0, 0, \cdots, t)) > 0$.

**Step 4: Testing protocol $r^1$ is not dominant.** We are left to show that there exists a function $r^3 \in M \setminus R^1$ that leads to strictly larger welfare than any $r^1 \in R^1$ for some different $\theta$. Choose $\theta = (t, t, \cdots, 0)$. Let $R^2 = M \setminus R^1$. We claim that $R^2$ is non-empty. An example is $r_{j}^3(X) = 1\{X_j > t_j\}, t_j = \Phi^{-1}\left(\min\left\{\frac{C}{J-1}, 1\right\}\right), j < J, t_j = \infty$. Consider the alternative
\[ \hat{\theta} = (t, \cdots, t, 0). \]

Observe now that we have
\[
v_{r3}(\hat{\theta}) - \sup_{r^1 \in \mathcal{R}^1} v_{r1}(\hat{\theta}) = t \times \left[ \sum_{j<\bar{J}} P(r^3_j(X)|\theta = \hat{\theta}) - \sup_{r^1 \in \mathcal{R}^1} \sum_{j<\bar{J}} P(r^1_j(X)|\theta = \hat{\theta}) \right].
\]

Next, we claim that
\[
\sum_{j<\bar{J}} P(r^1_j(X)|\theta = 0) < (\bar{J} - 1)(\min\{C/(\bar{J} - 1), 1\}).
\]

We prove the claim by contradiction. Suppose that the above equation does not hold. Then it must be that (since \( P(r^1_{|\bar{J}}(X) = 1|\theta = 0) > C/\bar{J} \))
\[
\sum_{j<\bar{J}} P(r^1_j(X)|\theta = 0) + P(r^1_{|\bar{J}}(X)|\theta = 0) > (\bar{J} - 1)(\min\{C/(\bar{J} - 1), 1\}) + \frac{C}{\bar{J}}.
\]

Clearly if \( C/(\bar{J} - 1) \leq 1 \), Equation (44) is true since otherwise by Equation (45) we would contradict maximin optimality of \( r^1 \). Suppose that \( C/(\bar{J} - 1) > 1 \). Then for \( r^1 \) to be maximin optimal we must have that \( (\bar{J} - 1) + C/\bar{J} \leq C \). However, it is easy to show that this implies that \( C/\bar{J} \geq 1 \) which leads to a contradiction.

Finally, using continuity, we obtain that for \( t \) small enough any \( r^1 \in \mathcal{R}^1 \) is dominated by \( r^3 \), completing the proof.

### E.5 Proof of Proposition 11

As in the proof of Proposition 10, we write the researcher’s net benefit as \( \sum_{j \in J} P(r_j(X)|\theta) - C \). Maximin optimality directly follows from Proposition 1. Thus, we focus on local power.

The proof proceeds as follows. We first find a lower bound on the worst-case power of \( r^* \). We then argue that any other maximin testing protocol attains power below this lower bound (and therefore has lower power than \( r^* \)).

Define \( v_r(\theta) \) as in Equation (42) under Assumption 7. As in the statement of the proposition, we will be assuming that \( \omega_j = 1 \) for all \( j \) in Assumption 2, although our reasoning can extend to general \( \omega > 0 \).

**Step 1: Lower bound on worst-case power.** We claim that \( \lim_{\epsilon \downarrow 0} \frac{1}{\epsilon} \inf_{\theta \in \Theta_1(\epsilon)} v_{r^*}(\theta) \geq \frac{C}{\bar{J}} \). We now show why. Denote by \( \theta(\epsilon) \in \Theta_1(\epsilon) \) the parameter under the local alternative. Observe that the welfare under the local alternative reads as follows
\[
(A) = \inf_{\theta(\epsilon)} \sum_{j \in J} \theta(\epsilon) P(r^*_j(X)|\theta = \theta(\epsilon)), \text{ such that } \theta_j(\epsilon) = \epsilon \text{ for some } j, \quad \theta_j(\epsilon) \in [0, \epsilon] \quad \forall j.
\]
We write

\[(A) \geq \inf_{w \in [0,1]^J, \sum_j w_j \geq \varepsilon, \theta(\varepsilon) \in \Theta_1(\varepsilon)} \sum_{j \in J} w_j P(r_j^*(X) | \theta = \theta(\varepsilon)) \]

\[
= \inf_{w \in [0,1]^J, \sum_j w_j \geq \varepsilon, \theta' \in [0,1]^J : \sum_j \theta_j \geq \varepsilon} \sum_{j \in J} w_j P(r_j^*(X) | \theta = \theta') := g(\varepsilon).
\]

Define \(W(\varepsilon_1, \varepsilon_2) = \{ (w, \theta) \in [0,1]^J \times [0,\varepsilon_2]^J : \sum_j w_j \geq \varepsilon_1, \sum_j \theta_j \geq \varepsilon_2 \}\) and write

\[
\frac{1}{\varepsilon} g(\varepsilon) = \inf_{(w, \theta') \in W(1, \varepsilon)} \sum_{j \in J} w_j P(r_j^*(X) | \theta = \theta') = \inf_{(w, \theta') \in W(1,1)} \sum_{j \in J} w_j P(r_j^*(X) | \theta = \varepsilon \theta').
\]

Observe that \(W(1, 1)\) is a compact space. In addition \(P(r_j^*(X) | \theta = \varepsilon \theta')\) is continuous in \(\varepsilon\) for any \(\theta' \in \Theta\) by Assumption 4. As a result, \(g(\varepsilon) / \varepsilon\) is a continuous function in \(\varepsilon\). Therefore,

\[
\lim_{\varepsilon \to 0} \frac{g(\varepsilon)}{\varepsilon} = \inf_{(w, \theta') \in W(1,1)} \sum_{j \in J} w_j P(r_j^*(X) | \theta = \varepsilon \theta' \times 0) = \inf_{(w, \theta') \in W(1,1)} \sum_{j \in J} w_j \frac{C}{J} = \frac{C}{J}.
\]

This completes the proof of our claim.

**Step 2: Alternative set of maximin protocols.** We now claim that any maximin protocol \(r'\), which does not satisfy Equation (33), must satisfy for some \(j \in \{1, \ldots, J\},

\[
P(r_j'(X) = 1 | \theta = 0) < \frac{C}{J}.
\]

(46)

We prove the claim by contradiction. Consider a maximin protocol \(r'\) such that for all \(j\) Equation (46) is violated. Then if \(r'\) is maximin optimal and satisfies Equation (46) with equality for all \(j\), there must be an \(r^*\) defined as in the proposition statement equal to \(r'\), which leads to a contradiction. Therefore it must be that if \(r'\) does not satisfy Equation (46) for some \(j\), \(r'\) is such that for some \(j\) Equation (46) is satisfied with reversed strict inequality and for all \(j\) is satisfied with reversed weak inequality. In such a case, it follows that \(\sum_j P(r_j'(X) = 1 | \theta = 0) > C\). This violates maximin optimality, as we can take \(\theta = (-t, -t, \ldots, -t) \in \Theta_0\) for some small \(t\). Then by Assumption 5 (namely, by continuity of \(F_\theta\)), we have for \(t\) small enough, \(\sum_j P(r_j'(X) = 1 | \theta_k = -t, \forall k) > C\). Therefore, for \(t\) small enough, the protocol \(r'\) does not satisfy the conditions in Proposition 1.

**Step 3: Power comparison.** Observe now that \(\inf_{\theta \in \Theta_1(\varepsilon)} \sum_{j \in J} \theta P(r_j(X) | \theta) \leq \varepsilon P(r_j(X) = 1 | \theta_j = \varepsilon, \theta_{-j} = 0), \) since the vector \((\theta_j = \varepsilon, \theta_{-j} = 0) \in \Theta_1(\varepsilon)\). Using Assumption 5 we have

\[
\lim_{\varepsilon \to 0} P(r_j(X) = 1 | \theta_j = \varepsilon, \theta_{-j} = 0) = P(r_j(X) = 1 | \theta = 0) < \frac{C}{J}.
\]
This completes the proof of the if statement.

Step 4: “Only if” statement. The “only if” statement follows from the fact that if \( r^* \) does not satisfy the condition in the proposition, then we can find a different function \( r'' \) which leads to larger power than \( r^* \) by the same argument as after Equation (46). As a result, in this case \( r^* \) violates the condition of local optimality.

E.6 Proof of Proposition 12

Under Proposition 1 and continuity of \( X \) (Assumption 4) every maximin protocol must satisfy (recalling the normalization \( b = 1 \)) 

\[
\sum_{j \in J} P(r_j(X) = 1|\theta = 0) \leq C(J)
\]

We can write the weighted welfare under \( r \) as 

\[
\int_{\Theta} w(\theta) \sum_{j \in J} P(r_j(X) = 1|\theta) \theta_j d\theta.
\]

We now discuss two cases: (i) \( P(r|\bar{J}|(X) = 1|\theta = 0) = C(\bar{J}, \Sigma) \), and (ii) \( P(r|\bar{J}|(X) = 1|\theta = 0) < C(\bar{J}, \Sigma) \). Define \( v_r(\theta) \) as in Equation (42).

Case (i): Suppose first that \( P(r|\bar{J}|(X) = 1|\theta = 0) = C(\bar{J}, \Sigma) \), which implies that \( P(r_1(X) = 1|\theta = 0) = 0 \). Then choose \( w(\theta) = 1\{ (\theta_1, \cdots, \theta_j) = (\epsilon, 0, \ldots, 0) \} \) for some small \( \epsilon > 0 \). Take \( r' \) such that \( r'_1(X) = 1\{ X_1/\sqrt{\Sigma_{1,1}} \geq \Phi^{-1}(1 - C(\bar{J}, \Sigma)) \} \), \( r'_j(X) = 0, \forall j > 1 \). It is easy to show that \( r'(X) \) is maximin under Assumptions 2 and 4. Then it follows that 

\[
\int_{\theta \in \Theta_1} (v_r(\theta) - v_{r'}(\theta)) w(\theta) d\theta = \epsilon \left( P(r_1(X) = 1|\theta = (\epsilon, 0, \ldots, 0)) - P(r'_1(X) = 1|\theta = (\epsilon, 0, \ldots, 0)) \right).
\]

By continuity, it follows that, as \( \epsilon \downarrow 0 \),

\[
P(r_1(X) = 1|\theta = (\epsilon, 0, \ldots, 0)) \to 0, \quad P(r'_1(X) = 1|\theta = (\epsilon, 0, \ldots, 0)) \to C(\bar{J}, \Sigma) > 0.
\]

Hence, by continuity, we can take \( \epsilon > 0 \) small enough so that

\[
\int_{\theta \in \Theta_1} w(\theta) \left( v_r(\theta) - v_{r'}(\theta) \right) < 0.
\]

Case (ii): Suppose now that \( P(r|\bar{J}|(X) = 1|\theta = 0) < C(\bar{J}, \Sigma) \). Then, we can take

\[
r'_1(X) = 1\{ X_1/\sqrt{\Sigma_{1,1}} \geq \Phi^{-1}(1 - C(\bar{J}, \Sigma)) \}, \quad r'_j(X) = 0, \quad \forall j < \bar{J},
\]

and \( w(\theta) = 1\{ (\theta_j) = (0, \cdots, 0, \epsilon) \} \). The argument now follows verbatim as in the previous case, with the first entry replacing the last entry.
Under Assumption 7, we can write $e^*_r(\theta) = 1\{\beta_r(\theta, \bar{J}, \Sigma) \geq 0\}$. We can write
\[
|U'(r; \lambda, w) - U(r; \lambda, \pi)| = \lambda \int v_r(\theta, \bar{J}, \Sigma)e^*_r(\theta)w(\theta)d\theta - \int e^*_r(\theta)\pi(\theta)d\theta \\
= \lambda \int v_r(\theta, \bar{J}, \Sigma)e^*_r(\theta)w(\theta)d\theta - \int e^*_r(\theta)v_{r=1}(\theta)w(\theta)d\theta \\
= \lambda \int (v_r(\theta, \bar{J}, \Sigma) - v_{r=1}(\theta))e^*_r(\theta)w(\theta)d\theta.
\]
Note that $\int |e^*_r(\theta)w(\theta)|d\theta \leq \int |w(\theta)|d\theta = 1$. Therefore, using Holder’s inequality,
\[
\left| \int (v_r(\theta, \bar{J}, \Sigma) - v_{r=1}(\theta))e^*_r(\theta)w(\theta)d\theta \right| \leq \max_{\theta \in \bar{\Theta}_1'} |v_{r=1}(\theta) - v_r(\theta, \bar{J}, \Sigma)|
\]
where the maximum is over $\bar{\Theta}_1'$ since $w(\theta)$ has support only over $\bar{\Theta}_1'$. The proof completes.

Define $r^* = \arg\max_r U'(r; \lambda, w)$. Because $r^t$ is maximin and unbiased and by definition of $\bar{\Theta}_1$, since the maximin component is always weakly negative (see proof of Proposition 1), we have
\[
U'(r^*; \lambda, w) \leq \lambda \int v_{r=1}(\theta)w(\theta)d\theta \quad \text{and} \quad U(r^t; \lambda, \pi) = \lambda \int \pi(\theta)d\theta.
\]
for any $\pi$ satisfying Assumption 1. Therefore, for any $\pi$ defined in Proposition 13, we have
\[
U'(r^*; \lambda, w) - U'(r^t; \lambda, w) = U'(r^*; \lambda, w) - U(r^t; \lambda, \pi) + U(r^t; \lambda, \pi) - U'(r^t; \lambda, w) \leq U(r^t; \lambda, \pi) - U'(r^t; \lambda, w).
\]
The proof now follows directly from Proposition 13.