Selective Trials: A Principal-Agent Approach to Randomized Controlled Experiments*

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Abstract

We study the design of randomized controlled experiments when outcomes are significantly affected by unobserved effort decisions taken by experimental subjects. While standard randomized controlled trials (RCTs) are internally consistent, the unobservability of effort compromises external validity. We approach trial design as a principal-agent problem and show that natural extensions of RCTs—which we call selective trials—can help improve external validity. In particular, selective trials can disentangle the effects of treatment, effort, and the interaction of treatment and effort. Moreover, they can help identify when treatment effects are affected by erroneous beliefs and inappropriate effort expenditure.

KEYWORDS: randomized controlled trials, selective trials, blind trials, incentivized trials, marginal treatment effects, mechanism design, selection, heterogeneous beliefs, compliance.

JEL: C81, C93, D82, O12.

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1 Introduction

This paper studies the design of experimental trials when outcomes depend significantly on unobserved effort decisions taken by subjects (agents).[1] Even in an ideal setting where the experimenter (principal) can randomly and independently assign an arbitrarily large number of agents to the treatment and control groups, unobserved effort limits the informativeness of randomized controlled trials (RCTs). For example, if a technology's measured returns are low, it is difficult to distinguish whether this is because true returns *are low* or because most agents *believe they are low* and therefore expend no effort using the technology. Moreover, to the extent that effort responds to beliefs, and beliefs respond to information, this makes it difficult to predict the returns to the technology on the same population as it becomes better informed. In other words, unobserved effort is a source of heterogeneity in treatment effects, and is a significant challenge to the external validity of experimental trials.[2]

We propose simple extensions of RCTs—which we call selective trials—that improve the external validity of trial results without sacrificing robustness or internal validity. These experimental designs can be used to determine the extent to which inappropriate effort or erroneous beliefs affect treatment effects. We provide a systematic analysis of trial design using a principal-agent framework with both adverse selection—an agent's type is unobserved—and moral hazard—an agent's effort is unobserved. However, unlike the standard principal-agent framework, our principal's goal is to maximize information about a technology's returns (in the sense of Blackwell) rather than profits. The principal seeks to achieve this objective through single-agent mechanisms that assign agents to treatments of varying sophistication based on the message they send.

These mechanisms improve on RCTs for two reasons. First, they let agents express

[1] Throughout the paper we call experimental subjects agents, and call the experimenter the principal. Following usual conventions, we refer to the principal as she and an agent as he.

[2] Unobserved effort is an issue whether a trial is open—agents know their treatment status—or blind—agents' treatment status is obscured by giving the control group a placebo. See Duflo et al. (2008b) for a more detailed description of RCTs and the external validity issues frequently associated with them.

preferences over their treatment by probabilistically selecting themselves in or out of the treatment group at a cost—hence the name *selective trials*.[3] This makes implicit, unobserved selection an explicit part of the experimental design. Second, these mechanisms allow for treatments of varying richness: in open trials, treatment corresponds to access to the new technology; in blind trials, treatment corresponds to an undisclosed allotment of the technology and information over the probability of having been allotted the technology; and in incentivized trials, treatment corresponds to access to the technology as well as an incentive (or insurance) contract based on outcomes.

Our results fall into two broad categories. Given a type of treatment (open, blind or incentivized), our first set of results characterize maximally informative mechanisms and examine the sampling patterns that such mechanisms induce. We show that a mechanism is maximally informative if and only if it identifies an agent's preferences over all possible treatment assignments and, given preferences, still assigns each agent to the treatment or control group with positive probability. Thus, our designs encapsulate the data generated by a standard randomized controlled trial. These designs can be implemented in a number of intuitive ways, such as a menu of lotteries or utilizing the design of Becker et al. (1964), referred to as the BDM mechanism.

While our main focus is on identification, and thus infinite samples, selective trials may impose additional costs on experimenters in finite samples. In particular, sampling patterns do not matter when arbitrarily large samples are available, but affect statistical power in finite samples. In any mechanism that identifies agents' preferences in a strictly incentive compatible way, agents with a higher value for the technology must be assigned to the treatment group with higher probability, which can reduce statistical power. However, these sampling costs can be reduced by diminishing incentives for truthfully reporting preferences. This allows the experimenter to strike a balance between sampling costs and the precision

[3]For simplicity, we focus on monetary costs, but the mechanisms can be based on non-monetary costs. For example, agents could choose between lines with different wait times to place themselves into the treatment group with different probabilities.

of the preference data that is obtained. As we detail later, these results contribute to recent discussions over the usefulness of charging subjects for access to treatment in RCTs (see, for instance, Cohen and Dupas (2010), Dupas (2009), or Ashraf et al. (2010)).

Our second class of results characterizes what can be inferred from selective trials, and highlights how they contribute to the ongoing discussion on the external validity of field experiments (Deaton, 2010; Imbens, 2010).[4] By eliciting agents' value for the technology, open selective trials recover the distribution of returns as a function of willingness to pay. As a result, open trials provide a simple and robust way to recover the marginal treatment effects (MTEs) introduced by Heckman and Vytlacil (2005). Identifying MTEs is valuable because they can be used to extrapolate the treatment effect of policies affecting the accessibility of goods, such as subsidies. However, MTEs are typically not sufficient to make projections about interventions that alter beliefs and effort expenditure, such as informational campaigns.

Selective trials go beyond MTEs and identify deep parameters by letting agents express preferences over richer treatments. Specifically, we consider blind trials where treatment status is hidden from agents by giving the control group a placebo. This allows us to vary the information an agent has over his treatment status. As a result we can identify the pure effect of treatment, as well as the agents' real and perceived returns to effort.[5] As blind trials

[4]In addition, selective trials may alleviate subversions of experimental protocol discussed in Deaton (2010). That is, explicitly allowing the agents to select themselves in and out of treatment may reduce the number of agents in the control group who obtain the treatment by other means, as well as the number of agents in the treatment group that refuse to be treated. Furthermore, the principal may use the information revealed by agents' preferences to increase monitoring of agents who expressed a high value for treatment but were assigned to the control group. Malani (2008) proposes a related solution—a trial design in which agents may select the nature of their control treatment, thus reducing incentives to subvert the experimental protocol.

[5]Although uncommon in economics, blind trials are quite common in medicine. For a brief review of RCTs in medicine see Stolberg et al. (2004). Jadad and Enkin (2007) provides a more comprehensive review. Selective trials nest preference trials, which have been used in medicine to improve the ehtics of randomized controlled trials and facilitate informed consent. A preference trial randomizes whether subjects are offered the treatment, and conditional on being offered treatment, subjects can select whether to join the treatment or control group. Our work shows that eliciting preferences is not incompatible with randomization, and that preferences carry information that facilitates inference from treatment effects. For more on preference trials, see Zelen (1979); Flood et al. (1996); Silverman and Altman (1996); King et al. (2005); Jadad and Enkin (2007); Tilbrook (2008).

are rarely used in economics—often for want of a convincing, ethical placebo—we extend the analysis to incentivized trials in which agents are informed of their treatment status, but receive different transfers conditional on observable outcomes. Under mild assumptions, this produces information similar to that produced by selective blind trials.

This paper contributes mainly to the literature on treatment effects. Most of this literature, based on a statistical framework quite different from our principal-agent approach, has focused on much simpler effort decisions and the ex post analysis of data. Agents are usually viewed as either taking treatment or not (with some exceptions: see, among others, Jin and Rubin (2008) for a recent example), and more importantly, this decision is assumed to be observable, or sufficiently correlated with exogenous observable variables (Imbens and Angrist, 1994; Angrist et al., 1996; Heckman and Vytlacil, 2005). In contrast, we consider effort decisions which are unobservable and high dimensional. Additionally, most previous approaches, even those which rely—as we do—on decision theory, focus on modeling data from an RCT after it has been run (Philipson and Desimone, 1997; Philipson and Hedges, 1998).[6] We take an ex ante perspective and propose designs for experimental trials that can help understand how beliefs and effort affect treatment effects.

Exploiting our principal-agent approach to trial design will require addressing a number of challenges. A first limit to our approach is that large samples may be needed to fully exploit the additional data we elicit. This problem is inherent to any non-parametric estimation of treatment effects conditional on a large set of explanatory variables and has been studied extensively (e.g. Pagan and Ullah, 1999). An other challenge is how to implement the designs we suggest in order to extract reliable preference data from subjects. Mechanisms that are equivalent in theory, due to the assumption of rationality, may have very different properties in practice. One may choose to elicit coarser information on preferences in order to use

[6]These studies use information correlated with agents' decisions to receive (or not receive) treatment to refine understanding of the treatment effect. This approach, as well as ours, is closely related to the classic Roy (1951) selection model in which selection into treatment reveals information about an agent's type (Heckman and Honoré, 1990; Heckman et al., 1997).

simpler designs. We believe that such practical concerns are best resolved through a mix of laboratory and field experiments in well-understood environments. It is encouraging that many elements of selective trials have already been used successfully in field studies (see, for example, Karlan and Zinman, 2009; Ashraf et al., 2010; Cohen and Dupas, 2010; Berry et al., 2011). A final set of challenges is more theoretical, and deals with extensions of our mechanisms that would elicit richer information, such as time-varying preferences, or beliefs about other participants. While these challenges are beyond the scope of this paper, we provide a short discussion of the main issues and suggest ways to approach them in the final section.

The paper is organized as follows. Section 2 uses a simple example to illustrate the main points of the paper. Section 3 defines the general framework. Section 4 investigates open selective trials. Section 5 turns to blind selective trials and shows how they can be used to identify true and perceived returns to effort. Section 6 extends the analysis to incentivized trials and shows that under reasonable assumptions they can be as informative as blind selective trials, without placebos. Section 7 concludes with a discussion of the limitations of, and future directions for, our approach to designing randomized controlled experiments.

2 An Example

To illustrate the basic insights underlying selective trials, and the potential usefulness of the data they generate, this section adopts a particularly simple structure of how agents' beliefs map to effort and how this, in turn, maps to outcomes. We underline that, for the sake of simplicity, this example is special in a number of ways that make inference very stark. Section 3 and beyond study inference in a much more general model that incorporates many important aspects of actual experiments. Among other things, the general framework allows for arbitrary heterogeneity in preferences and returns across agents.

To fix ideas, we discuss the example in terms of an experiment evaluating the health

effects of a water treatment product.[7]

2.1 A Simple Model

There are infinitely many agents indexed by $i \in \mathbb{N}$. Each agent has a treatment status $\tau_i \in \{0, 1\}$. If agent i is in the treatment group, $\tau_i = 1$, and he is given the water treatment product. Otherwise $\tau_i = 0$ and the agent is in the control group.

Agent i obtains a final outcome $y_i \in \{0, 1\}$, which can be measured by the principal. In our example $y_i = 1$ indicates that the agent has remained healthy. The probability that an agent remains healthy depends on both treatment and effort:

$$\text{Prob}(y_i = 1|e_i, \tau_i) = q_0 + Re_i\tau_i \tag{1}$$

where $e_i \in [0, 1]$ is agent i's decision of whether or not to expend effort using the product, $R \in [R_L, R_H]$ is the component of the technology's return that is common to all agents and q_0 is the unknown baseline likelihood of staying healthy over the study period, which will be controlled for using randomization. Agents have different types t which characterize their beliefs over returns R. We denote by $R_t = \mathbb{E}_t R$ the returns expected by an agent of type t. The distribution F_{R_t} of expectations R_t in the population, need not be known to the principal or the agents.[8]

We assume throughout that effort is private and cannot be monitored by the principal.

[7]It should be noted that while our main focus is on the use of RCTs in medical, public health and development contexts, our analysis applies to most environments involving decentralized experimentation. For instance, if a firm wants to try a new way to organize production, specific plant managers will have to decide how much effort to expend implementing it. The firm's CEO is in the same position as the principal in our framework, and must guess the effort exerted by his managers when evaluating returns to the new production scheme. Similarly, if a school board wants to experiment with a new program, individual teachers and administrators will have to decide how much effort to expend on implementing the program.

[8]This example focuses on heterogenous beliefs as a source of heterogenous behavior and heterogenous returns for illustrative purposes. In this setting, convincingly identifying true returns to treatment has a large effect on behavior, and would be particularly valuable. Moreover, the example allows effort to affect outcomes only in the treatment group. The general framework, described in Section 3, allows for: general, idiosyncratic, returns; effort in both the treatment and control group; and effort along an arbitrary number of dimensions (which can accommodate dynamic effort provision).

In other words, we assume that all observable dimensions of effort are already controlled for, and focus on those dimensions that are not observable. For example, with a water treatment product, an experimenter may be able to determine whether or not the agent has treated water in his home, but it may be much more difficult to determine if the agent drinks treated water when away from home.[9]

Given effort e_i, agent i's expected utility is given by

$$\mathbb{E}_t[y_i|e_i] - ce_i, \tag{2}$$

where $c \in (R_L, R_H)$ is the agents' cost of effort. In our example, this may be the cost of remembering to use the product, the social cost of refusing untreated water, or disliking the taste of treated water. In addition, we assume each agent has quasilinear preferences with respect to money. An agent's willingness to pay for treatment is $V_t = \max\{R_t - c, 0\}$, which we assume is less than some value $V_{\max}$ for all agents.

We focus initially on open trials where agents know their treatment status before making effort decisions, and contrast two ways of running trials: a standard RCT, where agents are randomly assigned to the treatment group with probability π, and a selective open trial which lets agents express preferences over treatment by selecting their probability of treatment.

A possible implementation of selective trials uses the BDM mechanism:

- Each agent sends a message $m_i \in [0, V_{\max}]$ indicating his willingness to pay for treatment.
- A price p_i to obtain treatment is independently drawn for each agent from a distribution with convex support, and c.d.f. F_p such that $0 < F_p(0) < F_p(V_{\max}) < 1$.
- If $m_i \geq p_i$, the agent obtains the treatment at price p, otherwise, the agent is in the control group and no transfers are made.

[9]Still, as Duflo et al. (2010) shows, innovative monitoring technologies may be quite effective. To the extent that monitoring is possible, it should be done.

Note that a higher message m increases an agent's probability of treatment, $F_p(m)$, as well as his expected payment: $\int_{p \leq m} p \, \mathrm{d}F_p$. Since F_p has convex support, it is dominant for an agent of type t to send message $m = V_t$.

2.2 The Limits of RCTs and the Value of Self-Selection

Inference from Randomized Controlled Trials. We begin by considering the information produced by an RCT. If agent i is in the treatment group, he chooses to expend effort ($e = 1$) if and only if $R_t \geq c$. Hence, the average treatment effect identified by an RCT is[10]

$$\begin{aligned} \Delta^{RCT} &= \mathbb{E}[y|\tau = 1] - \mathbb{E}[y|\tau = 0] \\ &= \mathbb{E}[q_0 + R \times \mathbf{1}_{R_t \geq c}|\tau = 1] - \mathbb{E}[q_0|\tau = 0] \\ &= R \times \text{Prob}\,(R_t > c) = R \times (1 - F_{R_t}(c)). \end{aligned}$$

When the distribution of agents' expectations F_{R_t} is known, then an RCT will identify R. However, in most cases F_{R_t} is not known, and the average treatment effect Δ^{RCT} provides a garbled signal of the underlying returns R. If the outcomes of agents in the treatment group are not particularly good compared to agents in the control group, the principal does not know if this is because the water treatment product is not particularly useful, or because the agents did not expend sufficient effort using the treatment.

Inference from Open Selective Trials. We now turn to selective trials and show they are more informative than RCTs.

The selective trial described above elicits agents' willingness to pay and, conditional on a given willingness to pay V, generates non-empty treatment and control groups. Since it is dominant for agents to truthfully reveal their value, an agent with value V_t has probability

[10] In the medical literature, R is referred to as the *efficacy* of a treatment, and Δ^{RCT}, which identifies the average treatment effect, is referred to as the *effectiveness* of the treatment. While effectiveness varies with the beliefs and effort decisions of agents in the experimental population, efficacy does not.

$F_p(V_t)$ of being in the treatment group and probability $1 - F_p(V_t)$ of being in the control group. Both of these quantities are strictly positive since $0 < F_p(0) < F_p(V_{\max}) < 1$.[11]

The selective trial described above provides us with the set of local instruments needed by Heckman and Vytlacil (2005) to estimate marginal treatment effects (MTEs). That is, for any willingness to pay V, we are able to estimate,

$$\begin{aligned}\Delta^{MTE}(V) &\equiv \mathbb{E}[y|\tau = 1, V_t = V] - \mathbb{E}[y|\tau = 0, V_t = V] \\ &= \mathbb{E}[y|\tau = 1, m_t = V] - \mathbb{E}[y|\tau = 0, m_t = V]\end{aligned}$$

which can be used to perform policy simulations in which the distribution of types is constant but access to the technology is changed—for example, subsidies. Moreover, MTEs can be integrated to recover the average treatment effect identified by an RCT.

In the current environment, because willingness to pay is a good signal of future use, MTEs can be used to identify the true returns R. Specifically, all agents with value $V_t > 0$ have expectations R_t such that $R_t - c > 0$, and expend effort $e = 1$ using the technology.[12] Hence, it follows that

$$\begin{aligned}\Delta^{MTE}(V > 0) &= \mathbb{E}\left[q_0 + R \times e_t \,|\tau = 1, V_t > 0\right] - \mathbb{E}[q_0|\tau = 0, V_t > 0] \\ &= R.\end{aligned}$$

A selective trial identifies the average treatment effect, MTEs, and true returns R. Hence, it is more informative than an RCT, which only identifies the average treatment effect.

The true returns R and the distribution of valuations V_t have several policy uses. First,

[11] Note also that agents with higher value are treated with higher probability. This matters for the precision of estimates in actual experiments, where sample size is not infinite. We return to this point in Section 4.

[12] In this environment the same result can be obtained by charging a price p for a probability of treatment π such that $F_{R_t}\left(\frac{p}{\pi} - c\right) > 0$, and evaluating treatment effects only for those willing to pay. The idea that higher prices will select individuals who value the technology more and may use it more intensely can be traced back to the seminal selection model of Roy (1951). See Oster (1995) for a discussion of related ideas in the context of non-profit organizations.

knowing R allows us to simulate the treatment effect for a population where everyone expends the appropriate amount of effort. Second, these variables allow us to estimate the returns to increasing usage within a given population. Finally, the data provided by the selective trial can be used to inform agents and disrupt learning traps more effectively than data from an RCT. For example, imagine that the true returns to the technology are high, but most agents believe they are low. In that case, an RCT will measure low returns to the treatment and will not convince agents that they should be expending more effort. In contrast, the data generated by a selective trial would identify that true returns are high, lead agents to update their beliefs, and efficiently adopt the water treatment product.[13]

2.3 Richer Treatments

In the previous subsection, a selective trial identified true returns because willingness to pay was a good predictor of future usage. However, as our continuing example shows, this will not always be the case. Thus, MTEs are generally not sufficient to infer true returns, and whether beliefs are affecting measured treatment effects. However, more sophisticated selective trials, such as blind selective trials and incentivized selective trials, can be used to recover true returns.

We modify the example so that the returns R to the technology include both baseline returns and returns to effort: $R = (R_b, R_e) \in \mathbb{R}^2$. In the context of a water treatment product, R_b could be the baseline returns to using the water treatment product only when it is convenient to do so, and R_e the additional returns to using it more thoroughly (for example, bringing treated water when away from home). Success rates given effort and

[13]For empirical work on the effect of appropriate information on behavior, see Thornton (2008), Dupas (2011) or Nguyen (2009). For theoretical work on failures of social learning, see the classic models of Banerjee (1992) or Bikhchandani et al. (1992).

treatment status are:

$$\begin{aligned} \text{Prob}(y = 1|\tau = 0, e) &= q_0 \\ \text{Prob}(y = 1|\tau = 1, e) &= q_0 + R_b + eR_e. \end{aligned}$$

An agent of type t has expectation $(R_{b,t}, R_{e,t})$ over returns $R = (R_b, R_e)$, and expends effort if and only if $R_{e,t} \geq c$. Therefore, an agent's willingness to pay for treatment is given by $V_t = R_{b,t} + \max\{R_{e,t} - c, 0\}$.

Inference from Open Selective Trials. We have already shown that open selective trials can identify treatment effects conditional on willingness to pay. However, in the current environment, willingness to pay is no longer a good signal of effort. Indeed, there are now two reasons why an agent might value the treatment: he believes that a thorough use of the product has high returns ($R_{e,t}$ is high)—the channel emphasized in Section 2.2—or he believes that a casual use of the water treatment product is sufficient to obtain high returns and that thorough use brings little additional return ($R_{b,t}$ is high, but $R_{e,t}$ is low). Hence, agents who are willing to pay because they think baseline returns are high need not be the agents who will actually expend effort. Formally, a selective trial still identifies MTEs,

$$\Delta^{MTE}(V) = R_b + R_e \text{Prob}(R_{e,t} \geq c | R_{b,t} + \max\{R_{e,t} - c, 0\} = V),$$

but these are generally not sufficient to recover R_b and R_e.[14] As a result, MTEs are insufficient to simulate the returns of a population of agents that all expended appropriate effort, or more generally, the returns to increasing the effort of agents. Nor do MTEs provide the information needed for agents to infer true returns.

[14]For instance, it is not possible to distinguish a situation in which returns to effort are equal to R_e and a proportion ηV of agents with value V expends effort, from a situation in which returns to effort are $2R_e$ and a proportion $\frac{\eta}{2} V$ of agents with value V expends effort.

Blind Selective Trials. In a blind trial, the agent does not know his treatment status $\tau \in \{0, 1\}$ at the time of effort, but rather knows his probability $\phi \in [0, 1]$ of having been assigned to the treatment group. Open trials are blind trials where ϕ is either 0 or 1.

Given a probability ϕ of being treated, the agent expends effort if and only if $\phi R_{e,t} - c > 0$. The agent's expected value for being treated with probability ϕ is

$$V_t(\phi) = \phi R_{b,t} + \max\{\phi R_{e,t} - c, 0\}.$$

We depart from standard blind trials in a simple but fundamental way: while standard blind trials keep ϕ fixed and do not infer anything from the specific value of ϕ used, we allow ϕ to vary and use both willingness to pay, and outcomes at different values of ϕ, for inference.[15]

As with open trials, willingness to pay can be elicited using a BDM-type mechanism. Since willingness to pay $V_t(\phi)$ now depends on ϕ, the mechanism in Section 2.1 is implemented after the agent is asked to send a message $m(\phi)$ for each possible value of ϕ. A value of ϕ is then drawn from a c.d.f. F_ϕ, with support $[0, 1]$ and mass points at 0 and 1. Transfer p is independently drawn from a c.d.f. F_p, as before. If $m(\phi) \geq p$, the agent pays p and is allotted the treatment with probability ϕ ; otherwise, the agent is in the control group and no transfers are made.

A first advantage of blind trials is that, unlike open trials, an agent's actual treatment status τ and his belief ϕ over his treatment status can be different. This allows for a robust identification of baseline returns R_b. If an agent is assigned a probability of treatment $\phi > 0$ low enough that $\phi R_H < c$, he will not expend any effort. Still, a proportion $\phi > 0$ of these agents do receive treatment while a proportion $1 - \phi > 0$ do not. Hence we can identify R_b by measuring the effect of treatment for agents known not to exert effort:

$$R_b = \mathbb{E}\left[y \middle| \phi < \frac{c}{R_H}, \tau = 1\right] - \mathbb{E}\left[y \middle| \phi < \frac{c}{R_H}, \tau = 0\right].$$

[15] A similar insight comes from Malani (2006), which examines variation in outcomes associated with variations in the probability of treatment across blinded experiments to identify placebo effects.

A second advantage of blind trials is that the agents' value mapping $V_t(\phi)$ allows identification of which agents expend effort when treated for sure. The amount that an agent with belief $\phi = 1/2$ is willing to pay to learn his treatment status is $\theta_t \equiv \frac{1}{2}[V_t(\phi{=}1) + V_t(\phi{=}0)] - V_t(\phi{=}1/2)$. If the agent does not intend to exert effort conditional on treatment, he will not value information and θ_t will be equal to 0. Inversely, if the agent does intend to exert effort, information is valuable since it allows him to tailor his behavior to treatment status, and $\theta_t > 0$.[16] In the current example, provided that a positive measure of agents satisfy $\theta_t > 0$, we can identify R_e using either of the following expressions:

$$\begin{aligned} R_e &= \mathbb{E}[y|\phi{=}1, \theta_t{>}0, \tau{=}1] - \mathbb{E}[y|\phi{=}1, \theta_t{=}0, \tau{=}1] \\ &= \mathbb{E}[y|\phi{=}1, \theta_t{>}0, \tau{=}1] - \mathbb{E}\left[y \middle| \phi{<}\frac{c}{R_H}, \theta_t{>}0, \tau{=}1\right]. \end{aligned}$$

Incentivized Selective Trials. We now show that incentivized trials can provide the principal with information similar to that produced by blind trials. This is useful as in many areas of economic interest, blind trials are not practical due to the lack of suitable, or ethical, placebos.

In an incentivized selective trial, the agent obtains a treatment status $\tau \in \{0, 1\}$, makes a fixed transfer p (which can be positive or negative), and receives a bonus (or penalty) w in the event that $y = 1$. Note that if $p > 0$ and $w > 0$, then the agent is being assigned an incentive contract. If instead $p < 0$ and $w < 0$, the agent is assigned an insurance contract.

Given a bonus level w, the agent expends effort if and only if $(1 + w)R_{e,t} - c > 0$. In turn, the agent's willingness to pay for treatment given bonus w is

$$V_t(w) = (1 + w)R_{b,t} + \max\{(1 + w)R_{e,t} - c, 0\}.$$

[16] This result holds very generally (see Proposition 5), although inference is typically less stark. The result is easy to verify in the current example. If the agent exerts effort conditional on being treated for sure (i.e. $R_{e,t} > c$), then $\theta_t = \frac{1}{2}[R_{b,t} + R_{e,t} - c] - \frac{1}{2}R_{b,t} - \max\left\{\frac{1}{2}R_{e,t} - c, 0\right\} \geq \min\left\{\frac{R_{e,t}-c}{2}, \frac{c}{2}\right\} > 0$.

As before, the mapping $w \mapsto V_t(w)$ can be elicited using a variant of the BDM mechanism (described in Appendix B). Incentivized trials allow us to evaluate baseline returns in a straightforward manner. When offered a full insurance contract $w = -1$, the agent will expend effort $e = 0$ so that

$$R_b = \mathbb{E}[y|w=-1, \tau=1] - \mathbb{E}[y|w=-1, \tau=0].$$

In turn, notice that for any type t with $R_{e,t} > 0$, there exists a value w_t such that whenever $w > w_t$, the agent expends effort $e = 1$. Value w_t is identified from mapping $w \mapsto V_t(w)$ because

$$\left.\frac{\partial V_t}{\partial w}\right|_{w>w_t} = R_{e,t} + R_{b,t} > R_{b,t} = \left.\frac{\partial V_t}{\partial w}\right|_{w<w_t}.$$

Additionally, this last expression allows us to identify the agent's subjective beliefs over baseline returns and returns to effort $(R_{b,t}, R_{e,t})$. For some value $\overline{w}$ sufficiently high that it induces some agents to expend effort, returns to effort can be identified by either of the following expressions

$$\begin{aligned} R_e &= \mathbb{E}[y|w=\overline{w}, \overline{w} - w_t > 0, \tau=1] - \mathbb{E}[y|w=\overline{w}, \overline{w} - w_t < 0, \tau=1] \\ &= \mathbb{E}[y|w=\overline{w}, \overline{w} - w_t > 0, \tau=1] - \mathbb{E}[y|w=-1, \overline{w} - w_t > 0, \tau=1]. \end{aligned}$$

Just like blind trials, incentivized trials identify true returns $R = (R_b, R_e)$.

Altogether, this section suggests that while unobserved effort is an issue for the external validity of standard randomized controlled trials, appropriate ex ante trial design—rather than ex post data treatment—may help in alleviating these concerns.

The rest of the paper explores how these results extend in a much more general and realistic environment that allows for:

- arbitrary heterogeneity among agents, including heterogeneous preferences, beliefs and

returns;

- multidimensional effort in both the treatment and control group. This can accommodate complex technologies, dynamic effort provision, or attempts by agents in the control group to obtain substitute treatments.

The sections which follow provide systematic results in this general framework about which mechanisms are the most informative, what sampling patterns they produce, and what can be inferred from the data they generate.

3 A General Framework

We now generalize the framework used in our example. Once again, there are infinitely many agents, indexed by $i \in \mathbb{N}$. Returns to the technology are described by parameter $R \in \mathcal{R} \subset \mathbb{R}^{\kappa}$.

Types. Each agent i has a type $t \in T$, which includes a belief over returns R, as well as factors that might affect behavior and outcomes, such as idiosyncratic costs of effort, idiosyncratic returns, and beliefs over such factors. We assume that agents are exchangeable, so that their types are i.i.d. draws from some distribution $\chi \in \Delta(T)$, which is itself a random variable. A profile of types is given by $\mathbf{t} \in T^{\mathbb{N}}$. For concision we omit publicly observable traits, but it is straightforward to allow for them.

Outcomes and Success Rates. Agent i obtains an outcome $y_i \in \{0, 1\}$.[17] An agent's true and perceived likelihoods of success (that is, $\text{Prob}(y = 1)$) depend on his type, the aggregate returns to the technology and the agent's effort choice $e \in E$, where E is a compact subset

[17] As Appendix A shows, binary outcomes simplify notation, but are not essential to our results.

of $\mathbb{R}^{\kappa'}$. Success rates are denoted by

$$\begin{aligned} q(R,t,\tau_i,e_i) &= \text{Prob}(y{=}1|R,t,\tau_i,e_i) \\ q_t(\tau_i,e_i) &= \int_R q(R,t,\tau_i,e_i)\mathrm{d}t(R) \end{aligned}$$

where $q(R,t,\tau,e)$ is the true success rate of an agent of type t (this allows for idiosyncratic returns) while $q_t(\tau,e)$ is the probability of success perceived by an agent of type t. We assume that q and q_t are continuous with respect to effort e. Note that as e can be multidimensional, the model is consistent with dynamic effort expenditure and agent learning about returns to treatment, or his treatment status, as in Philipson and Desimone (1997) or Chan and Hamilton (2006).[18]

Preferences. Given effort e_i, treatment status τ_i, monetary transfer p_i, and final outcome y_i, agent i's utility is $u(y_i,t_i) - c(e_i,t_i) - p_i$.

Note that p_i can be negative and all transfers can be shifted by a fixed amount (for instance a show-up fee). This may be used to improve participation, or deal with agents' cash constraints. In addition, one could elicit preferences over goods other than money.[19]

Assignment Mechanisms. We distinguish three ways to assign treatment:

1. *Open selective trials* are mechanisms $G_o = (M_o, \mu_o)$ where M_o is a set of messages and $\mu_o : M_o \to \Delta(\{0,1\} \times \mathbb{R})$ maps individual messages to a probability distribution over treatment status $\tau_i \in \{0,1\}$ and transfers $p_i \in \mathbb{R}$.

[18] In most settings, this effort decision is multidimensional. For instance, in the case of fertilizer, it is not enough for agents to just expend effort spreading fertilizer. As Duflo et al. (2008a) highlight, effort is needed to choose the appropriate seeds to go with the fertilizer, learn how much and when to water the crops, and to learn how much fertilizer gives the highest returns at the lowest cost. In this case it is natural to think of effort as a vector, where the first component corresponds to choosing the amount of fertilizer, the second to picking the right seeds, the third to properly applying it, and so on.

To accommodate dynamic effort expenditure, different dimensions of the effort vector may indicate contingent effort expenditure depending on realized observables, such as the state of crops, or how they seem to respond to previous fertilizer use.

[19] Appendix A allows for a broader set of trade-offs and agents with non-quasilinear preferences.

2. *Blind selective trials* are mechanisms $G_b = (M_b, \mu_b)$ where M_b is a set of messages and $\mu_b : M_b \to \Delta([0,1] \times \mathbb{R})$ maps messages to a probability distribution over uncertain treatment status ϕ_i (where $\phi_i = \text{Prob}(\tau_i = 1)$) and transfers p_i.

3. *Incentivized selective trials* are mechanisms $G_w = (M_w, \mu_w)$ where M_w is a set of messages and $\mu_w : M_w \to \Delta(\{0,1\} \times \mathbb{R} \times \mathbb{R})$ maps messages to a probability distribution over treatment status τ_i, a fixed transfer p_i from the agent to the principal, and a bonus w_i transferred from the principal to the agent conditional on $y_i = 1$.

Note that these are single agent mechanisms. Agent i's final assignment depends only on his message, and not on messages sent by others. We denote by $\pi(m) \equiv \text{Prob}(\tau = 1|m)$ the likelihood of being given the treatment when sending message m. We focus largely on mechanisms G such that χ-almost surely, every agent i has a dominant message $m_G(t_i)$. In all these designs agents can probabilistically select their assignment using messages, hence the name *selective trials*.

Informativeness of Mechanisms. We evaluate mechanisms according to their informativeness in the sense of Blackwell. We say that a mechanism G is at least as informative as a mechanism G' (denoted by $G' \preceq G$) if the data generated by G' can be simulated using only data generated by G.

Specifically, denote by a_i the assignment given to agent i by whichever mechanism is chosen. The principal observes data $\mathbf{d}_G = (m_i, a_i, y_i)_{i \in \mathbb{N}}$. Denote by $\mathcal{D}_G$ the set of possible data sequences generated by mechanism G. Mechanism G is at least as informative as mechanism G', denoted by $G' \preceq G$, if and only if there exists a fixed data manipulation procedure $h : \mathcal{D}_G \to \Delta(\mathcal{D}_{G'})$ such that for all $\mathbf{t} \in T^{\mathbb{N}}, R \in \mathcal{R}$, $h(\mathbf{d}_G(\mathbf{t}, R)) \sim \mathbf{d}_{G'}(\mathbf{t}, R)$.

This notion of informativeness is easier to work with in environments with infinite samples, as this focuses on issues of identification rather than issues of statistical power. However, this definition also applies in the case of finitely many agents.[20]

[20]With infinite samples, sampling patterns do not matter. Thus, there is a large equivalence class of most

Although our framework is quite general, we intentionally limit our approach in three ways. First, we assume agents are rational, that is, they play undominated strategies, regardless of the complexity of the assignment mechanism. Second, we examine only single-agent mechanisms. Third, despite the fact that effort expenditure may be dynamic, we restrict attention to mechanisms that elicit preferences only once. Note, however, that the timing of this elicitation may be freely chosen by the principal. Specifically, messages could be elicited before agents have any exposure to the technology, or after they have assessed it. Section 7 discusses the limitations of assuming rationality and examining only single-agent mechanisms, and the difficulties of eliciting preferences more than once.

4 Open Selective Trials

In open selective trials an agent is assigned a treatment status τ and a transfer p based on message m. Given this assignment (τ, p), the indirect utility of an agent with type t is $V_t(\tau) - p$ where,

$$V_t(\tau) = \max_{e \in E} \; q_t(\tau, e)u(y{=}1, t) + [1 - q_t(\tau, e)]u(y{=}0, t) - c(e, t).$$

We normalize the value of being in the control group $V_t(\tau{=}0)$ to zero for every type. Hence $V_t \equiv V_t(\tau{=}1)$ denotes the agent's willingness to pay for treatment. For simplicity we assume that there exists a known value $V_{\max} \in \mathbb{R} > 0$ such that for all $t \in T$, $V_t \in (-V_{\max}, V_{\max})$ and that the distribution over values induced by the distribution of types χ admits a density. The optimal effort for type t given treatment status τ is denoted by $e^*(\tau, t)$. [21]

informative mechanisms. When samples are finite, these mechanisms remain undominated in the sense of Blackwell, but need no longer be equivalent.

[21] At this stage, whether optimal effort is unique or not does not matter. We explicitly assume a unique optimal effort level in Sections 5 and 6 to apply a convenient version of the Envelope Theorem.

4.1 Information Production in Open Selective Trials

Our first result highlights the fact that selective trials are natural extensions of RCTs. An RCT is a mechanism $G_0 = (\emptyset, \pi_0)$. As $M = \emptyset$, no messages are sent, all agents are assigned to the treatment group with the same probability $\pi_0 \in (0, 1)$, and there are no transfers.

Fact 1 (full support sampling)**.** *Consider a mechanism $G = (M, \mu)$. If there exists $\xi > 0$ such that for all $m \in M$, $\pi(m) \in (\xi, 1 - \xi)$, then $G_0 \preceq G$.*

Recalling that $\pi(m) \equiv \text{Prob}(\tau = 1|m)$, Fact 1 shows that if every type has a positive probability of being in the treatment or control group, then it is as informative as an RCT. This holds for any $\xi > 0$ because the sample size is infinite. We succinctly discuss sampling issues that arise in actual experiments with finite samples in Section 4.2.

As Plott and Zeiler (2005) and others have shown, information elicited in non-incentive compatible ways can be unreliable. Moreover, as Kremer and Miguel (2007) and others have noted, reported beliefs about a technology's return are often uncorrelated with use. Therefore, we focus on *strictly incentive compatible* assignment mechanisms—assignment mechanisms such that χ-almost every agent has a strictly preferred message.[22]

Our next result shows that an open selective trial is a most informative trial if and only if it identifies each agent's value V_t, and, conditional on any expressed valuation, assigns a positive mass of agents to both the treatment and control group.

Proposition 1 (most informative mechanisms)**.** *Any strictly incentive compatible mechanism G identifies at most value V_t (i.e. $V_t = V_{t'} \Rightarrow m_G(t) = m_G(t')$).*

Whenever G identifies values V_t (i.e. $m_G(t) = m_G(t') \Rightarrow V_t = V_{t'}$) and satisfies full support ($0 < \inf_m \pi(m)$ and $\sup_m \pi(m) < 1$), then $G' \preceq G$ for any strictly incentive compatible

[22]Note that the mechanisms we consider can accommodate surveys. Consider the mechanism $G = (T, \pi_0)$ with message space $M = T$ where the likelihood of treatment is constant and equal to π_0 and no transfers are made. This is essentially an RCT supplemented with a rich survey. Since assignment does not depend on the message, truthful revelation of one's type is weakly dominant. Unfortunately, any other message is also weakly dominant. Hence, data generated by such a mechanism is likely to be unreliable, especially if figuring out one's preferences is costly.

mechanism G'.

It follows that open selective trials can identify at most the distribution of returns conditional on the agents' valuations, which can be used to construct marginal treatment effects (MTEs). It is important to note that these mechanisms identify MTEs independently of the experimenters's beliefs. Hence, to the extent that elicited values are reliable, these mechanisms identify MTEs with a degree of robustness comparable to that with which RCTs identify average treatment effects.[23]

Implementing Most Informative Trials. Here we exhibit two straightforward implementations of most informative selective trials. The first is the BDM mechanism described in Section 2.1, with the expanded message space $M = [-V_{\max}, V_{\max}]$. Once again the principal draws a price $p_i \in [-V_{\max}, V_{\max}]$ independently for each agent from a common c.d.f. F_p with support $[-V_{\max}, V_{\max}]$. If $m_i \geq p_i$, then the agent is assigned $(\tau = 1, p_i)$; otherwise, he is assigned $(\tau = 0, 0)$.

Fact 2 (BDM Implementation)**.** *Whenever F_p has full support over $[-V_{\max}, V_{\max}]$, an agent with value V_t sends optimal message $m_{BDM} = V_t$ and the BDM mechanism is a most informative mechanism.*

A second implementation is a menu of lotteries. Consider mechanism G^*, where $M = \left(-\frac{1}{2}, \frac{1}{2}\right)$, any agent sending message m is assigned to the treatment group with probability $\pi(m) = \frac{1}{2} + m$ and must make a transfer $p(m) = V_{\max} m^2$. One can think of agents as having a baseline probability of being in the treatment group equal to $\frac{1}{2}$ and deciding by how much they want to deviate from this baseline. An agent with value V_t chooses message m to maximize

$$\pi(m)V_t - p(m) = V_t\left(\frac{1}{2} + m\right) - V_{\max} m^2. \tag{3}$$

[23]Note that selective trials also identify higher order moments of the outcome distribution conditional on treatment status and valuation, which may be useful to researchers.

This problem is concave in m, and first order conditions yield an optimal message $V_t/2V_{\max}$ which identifies V_t. In addition, every agent is assigned to the treatment and control group with positive probability. Thus G^* is a most informative mechanism.

Note that G^* gives agents higher expected utility than an RCT which assigns agents to the treatment and control group with probability $\frac{1}{2}$. More generally, for any RCT, a selective trial that assigns price $p = 0$ for a probability of treatment π equal to that of the RCT must increase the agents' expected utility. Thus, selective trials may help decrease the number of agents who refuse randomization. This is potentially useful since refusal rates are often significant (Jadad and Enkin (2007) find rates approaching 50 percent in medical trials), which reduces the external validity of treatment effects (Malani, 2008).

4.2 The Cost of Running Selective Trials

In equilibrium, the menu of lotteries G^* yields sampling profile $\pi(V) = \frac{1}{2}\left(1 + \frac{V}{V_{\max}}\right)$, which is strictly increasing in value V. In the BDM mechanism the sampling profile, $\pi_{BDM}(V) = F_p(V)$, is also increasing in V. This property holds for any mechanism.

Proposition 2 (monotonicity)**.** *Consider a strictly incentive compatible mechanism G. If agents t and t' with values $V_t > V_{t'}$ send messages $m_G(t) \neq m_G(t')$, then it must be that $\pi(m_G(t)) > \pi(m_G(t'))$.*

Thus, in any selective trial, agents with high values are over-sampled—they have a higher likelihood of being in the treatment group—and those with low values are under-sampled. In contrast, RCTs have a flat sampling profile. While sampling patterns do not matter when there is an arbitrarily large number of agents, they can significantly affect statistical power when samples are finite.

This issue is related to the recent debate in development economics on charging for treatment in RCTs.[24] If, as in Ashraf et al. (2010), willingness to pay is correlated with

[24]This literature is motivated by questions of efficiency, and is mostly interested in whether charging for

product usage, then eliciting willingness to pay might be quite useful in understanding true returns. If, instead, as in the case of Cohen and Dupas (2010), most agents have low values, and willingness to pay is a poor predictor of actual use, undersampling agents with low values may significantly reduce statistical power. Furthermore, in such a setting, willingness to pay provides little information about intended use.[25]

We make two contributions to this debate. First, we note that when trade-offs between money and treatment are uninformative, selective trials can and should be based on more informative trade-offs. For instance, if most of the heterogeneity in willingness to pay is driven by wealth and credit constraints, then eliciting willingness to wait, or willingness to perform a tedious task (like sitting through multiple information sessions) may be a better indicator of future usage than willingness to pay. If this is the case, selective trials can and should be designed around such tradeoffs. As we discuss in Section 7, this requires some knowledge of the agents and their environment.

Second, we show that carefully designed selective trials can reduce the costs of oversampling by reducing the slope of the sampling profile.

Proposition 3 (sampling rates and incentives)**.** *For any mechanism $G = (M, \mu)$ and $\underline{\rho} < \overline{\rho}$ in $(0, 1)$, there exists a mechanism $G' = (M, \mu')$ such that $G \preceq G'$, and for all $m \in M$, $\pi'(m) \in [\underline{\rho}, \overline{\rho}]$.*

The following must also hold. Denoting the expected utility of type t sending message m in mechanism G' (including transfers) by $U(t|m, G')$, then

$$\max_{m_1, m_2 \in M} \quad |U(t|m_1, G') - U(t|m_2, G')| \leq 2(\overline{\rho} - \underline{\rho}) V_{\max}.$$

Proposition 3 implies that it is always possible to reduce the slope of a mechanisms'

usage improves how well treatment is matched with those who need and use it. This paper takes a slightly different perspective, and is interested in how controlling for willingness to pay improves inference from experimental trials.

[25] As Dupas (2010) shows, this can also hinder social learning.

sampling profile without affecting identification. Unfortunately, reducing the slope of the sampling profile also reduces incentives for truth-telling. We illustrate this with mechanisms $(G^*_\lambda)_{\lambda\in(0,1)}$ which generalize G^* as follows: $M = \left(-\frac{1}{2}, \frac{1}{2}\right)$, $\pi(m) = \frac{1}{2} + \lambda m$ and $p(m) = \lambda V_{\max} m^2$. As the slope of the sampling profile λ goes to zero, each agent will be sampled with probability approaching $\frac{1}{2}$ and will pay an amount approaching zero, irrespective of the message he sends. For any $\lambda > 0$, $m = V_t/2V_{\max}$ is still a dominant strategy for an agent of type t. However, if an agent with value V_t instead sends message $V/2V_{\max}$ with $V \neq V_t$, his expected loss is

$$U(t|m = V_t/2V_{\max}) - U(t|m = V/2V_{\max}) = \frac{\lambda}{4V_{\max}}(V_t - V)^2,$$

which vanishes as the slope of the sampling profile λ goes to 0.

Importantly, although there is a trade-off between oversampling high values and the noisiness of the information that one can elicit, the slope of the sampling profile is a free parameter which the principal can and should optimize over.

Altogether, this section has shown that open selective trials provide a simple way to identify MTEs and, more generally, the distribution of returns conditional on willingness to pay. In addition, while selective trials systematically oversample high value agents, this issue is negligible when sample size is large or agents are very responsive to incentives. However, as Section 2 highlights, willingness to pay need not be a good predictor of actual effort and MTEs may not allow identification of deep parameters of interest. The following sections explore richer treatments which can better identify the role of effort.

5 Blind Selective Trials

5.1 Framework and Basic Results

In blind trials the agent is assigned a probability of being in the treatment group, $\phi \in [0,1]$, which is disclosed to the agent, and an actual treatment status, $\tau \in \{0,1\}$, which is known only to the principal. Thus, the pair (τ, ϕ) can be thought of as a full description of an agent's overall treatment. Blind selective trials nest both open selective trials (where $\phi \in \{0,1\}$) and standard blind trials, where ϕ is fixed.

Assignment Mechanisms. As noted in Section 3, selective blind trials are mechanisms $G = (M, \mu)$ where $\mu : M \to \Delta([0,1] \times \mathbb{R})$. Given a message m, μ assigns the agent a likelihood of being treated $\phi \in [0,1]$ which is known to the agent, and a transfer $p \in \mathbb{R}$. An actual (unrevealed) treatment status $\tau \in \{0,1\}$ is drawn according to ϕ. We denote by $\mu(\phi|m)$ the distribution over ϕ given message m.

Utility and Effort. An agent of type t's value for uncertain treatment status ϕ is:

$$V_t(\phi) = \max_{e \in E} \Big(\phi q_t(\tau{=}1, e) + (1{-}\phi) q_t(\tau{=}0, e)\Big)\Big(u(y{=}1, t) - u(y{=}0, t)\Big) + u(y{=}0, t) - c(e, t).^{26} \tag{4}$$

The corresponding effort decision is $e^*(\phi, t)$, which we assume is unique.[27] Consistent with earlier notation we maintain $V_t(\phi{=}0) = 0$. Note that $V_t(\phi{=}1) = V_t$ is the agent's value for treatment in an open trial. Throughout the section, we keep ϕ as an argument of $V_t(\phi)$ and denote the value of $V_t(\phi)$ at φ by $V_t(\phi = \varphi)$. Thus, $V_t(\phi)$ denotes the entire mapping: $\varphi \mapsto V_t(\phi{=}\varphi)$.

[26] Note that $V_t(\phi)$ must be convex. This follow from the fact that any mean preserving spread in belief ϕ is equivalent to the arrival of a signal about treatment status. As more information is necessarily useful in this setting, this implies that $V_t(\phi)$ is convex.

[27] Using the results of Milgrom and Segal (2002) this allows us to apply the usual Envelope Theorem to $V_t(\phi)$ in Proposition 6. Note that this also implies that $e^*(\phi, t)$ is continuous in ϕ.

Proposition 4 (most informative mechanisms)**.** *Any strictly incentive compatible blind mechanism G identifies at most mapping $V_t(\phi)$ (that is, $V_t(\phi) = V_{t'}(\phi) \Rightarrow m_G(t) = m_G(t')$).*

If G identifies $V_t(\phi)$ (that is, $m_G(t) = m_G(t') \Rightarrow V_t(\phi) = V_{t'}(\phi)$) and satisfies $\inf_{\phi,m} \mu(\phi|m) > 0$ then $G' \preceq G$ for any strictly incentive compatible mechanism G'.

A simple generalization of the BDM mechanism is a most informative blind trial. Pick distributions, F_ϕ over $[0,1]$, and $F_{p|\phi}$ over $[-V_{\max}, V_{\max}]$ with densities bounded away from 0. The blind BDM Mechanism (bBDM) has message space $M = [-V_{\max}, V_{\max}]^{[0,1]}$, so that a message m corresponds to a value function $V_t(\phi)$. Given message m, the principal draws values $\phi = \varphi$ and p according to distributions F_ϕ and $F_{p|\phi}$. If $m_i(\varphi) \geq p$, the agent is assigned (φ, p). Otherwise, the agent is assigned $(0,0)$. It is straightforward to show that $m_{bBDM}(t) = V_t(\phi)$. Additionally, bBDM satisfies the full sampling constraint $\inf_{\phi,m} \mu(\phi|m) > 0$.

Blind selective trials have two distinct advantages over open selective trials. First, blind selective trials decorrelate an agent's belief ϕ and treatment status τ. As detailed in the next subsection, this will allow the principal to identify whether empirical success rates are being driven by the agent's behavior or by the treatment itself. Second, by identifying the value function $V_t(\phi)$, blind selective trials provide useful information about an agent's intended behavior and his perceived success rate.

5.2 The Value of Decorrelating Beliefs and Treatment Status

Changes in success rates due to treatment come from two sources: the effect of the treatment itself, and the effect of behavioral changes induced by anticipation of treatment. In an open trial, changes in behavior are perfectly correlated with changes in treatment status. As a result, the effect of treatment and the effect of behavioral changes induced by the expectation of treatment are hard to distinguish. In contrast, blind trials allow us to disentangle these two effects by distinguishing an agent's actual treatment status τ and his (correct) belief ϕ that he is being treated.

We can disentangle these effects by considering $\mathbb{E}[y|V_t(\phi), \phi=\varphi, \tau]$, the measured success rate conditional on the value function $V_t(\phi)$, belief $\phi = \varphi$ and treatment status τ, which is identified by selective blind trials. This allows identification of MTEs conditioned on the entire value function, $\Delta^{MTE}(V_t(\phi))$, as well as the pure treatment and behavioral effects $\Delta^T(V_t(\phi))$ and $\Delta^B(V_t(\phi))$:

$$\begin{aligned}\Delta^T(V_t(\phi)) &= \lim_{\substack{\varphi\to 0\\ \varphi>0}} \mathbb{E}[y|V_t(\phi), \phi=\varphi, \tau=1] - \mathbb{E}[y|V_t(\phi), \phi=\varphi, \tau=0]\\ \Delta^B(V_t(\phi)) &= \lim_{\substack{\varphi\to 1\\ \varphi<1}} \mathbb{E}[y|V_t(\phi), \phi=\varphi, \tau=0] - \mathbb{E}[y|V_t(\phi), \phi=0, \tau=0].\end{aligned}$$

Indeed, as φ approaches zero, an agent's effort converges to $e^*(\tau = 0, t)$, the effort he would expend if he knew he was not treated.[28] Hence, Δ^T identifies the returns to treatment keeping the agent's behavior at its default level $e^*(\tau=0, t)$. Similarly, as φ approaches one, the agent's effort converges to $e^*(\tau=1, t)$, the effort associated with sure treatment. Thus, Δ^B is the effect of behavior change alone. Finally,

$$\Delta^I \equiv \Delta^{MTE} - \Delta^T - \Delta^B \tag{5}$$

measures the aggregate treatment effect (conditional on value $V_t(\phi)$), net of the effect of treatment and behavior alone. That is, Δ^I measures the interaction effect between behavior and treatment. If Δ^I is positive, then treatment and effort changes are complementary in producing successful outcomes. If, instead, Δ^I is negative, this suggests that there is a negative interaction between treatment and the perceived optimal effort of agents.[29]

[28] We use a continuity argument because $\phi = 0$ implies $\tau = 0$, hence, there is no treatment group. This is essentially an identification at infinity argument, as in Heckman (1990) or Heckman and Honoré (1990), which entails well-known practical difficulties.

[29] These quantities can also be measured unconditionally across the entire agent population, or conditioned only on the value for sure treatment, V_t. Moreover, Δ^T can be estimated using a standard blind RCT with a sufficiently low value of ϕ.

Note that selective blind trials can allow for double-blind designs in which the principal has varying beliefs over the likelihood that an agent is being treated. Varying the beliefs of the principal may help identify the treatment effect due to variations in the principal's behavior. Treating this question properly requires a

Being able to identify Δ^T and Δ^B has important practical implications. Consider, for example, a cholesterol-reducing drug. If agents react to anticipated treatment by eating more fatty foods, then the aggregate effect of treatment could be quite small even if the effect of the drug alone is significant. In this environment, Δ^T is the treatment effect purified of changes in behavior, that is, the effect of the drug on people who do not change their diet.

When interpreting Δ^B and Δ^I, it is important to keep in mind that these are the direct and interaction effects at the agents' *perceived* optimal effort level $e^*(\tau=1,t)$. Consequently, if Δ^I and Δ^B are small, this may be because effort does not improve the success rate of treatment, or because the agent is expending little effort. In order to distinguish these two possibilities, we need additional information on the effort of agents. As the following subsection shows, this is what $V_t(\phi)$ provides.

5.3 The Value of Eliciting Preferences $V_t(\phi)$

As highlighted in Section 2.3, the mapping $V_t(\phi)$ can tell us whether and by how much treatment changes an agent's effort. Recalling that $V_t(\phi=0) = 0$, knowledge of mapping $V_t(\phi)$ provides the following simple test.

Proposition 5 (a test of "intention to change behavior")**.**
If $e^*(\phi=0,t) = e^*(\phi=1,t)$*, then for all* φ*,* $V_t(\phi=\varphi) = \varphi V_t(\phi=1)$*.*
If $e^*(\phi=0,t) \neq e^*(\phi=1,t)$*, then for all* $\varphi \in (0,1)$*,* $V_t(\phi=\varphi) < \varphi V_t(\phi=1)$*.*

When effort changes with treatment status, the agent gets additional surplus from tailoring his behavior to τ. The difference $\varphi V_t(\phi=1) - V_t(\varphi)$ is thus the agent's willingness to pay to learn his actual treatment status, which will be zero if effort is independent of treatment.[30] Recalling that $q_t(\tau,e)$ is the perceived success rate of an agent with type t, the

better understanding of the principal's incentive problem, which we abstract away from in this paper.

[30]When $\varphi = 1/2$ this coincides with test statistic θ_t defined in Section 2.3.

Note that in a richer decision theoretic framework, agents may have preferences for early revelation of uncertainty even though their actions do not depend on information (Kreps and Porteus, 1978). In such a framework, an agent's value for information would be a noisy (but still informative) signal of intent to change behavior.

value function $V_t(\phi)$ also allows us to estimate an agent's perceived returns to effort.

Proposition 6 (identifying perceived returns to effort)**.** *For any value* φ*,*

$$\left.\frac{\partial V_t(\phi)}{\partial \phi}\right|_{\varphi} = [q_t(\tau{=}1, e^*(\varphi, t)) - q_t(\tau{=}0, e^*(\varphi, t))] \times [u(y{=}1, t) - u(y{=}0, t)].$$

In particular, we can compute the agent's perceived increase in treatment effects when moving from default effort (induced by $\varphi = 0$) to perceived optimal effort given treatment (induced by $\varphi = 1$):

$$\left.\frac{\partial V_t(\phi)}{\partial \phi}\right|_{1} \Big/ \left.\frac{\partial V_t(\phi)}{\partial \phi}\right|_{0} = \frac{q_t(\tau{=}1, e^*(\varphi{=}1, t)) - q_t(\tau{=}0, e^*(\varphi{=}1, t))}{q_t(\tau{=}1, e^*(\varphi{=}0, t)) - q_t(\tau{=}0, e^*(\varphi{=}0, t))}.^{31} \tag{6}$$

This data helps evaluate whether under-provision of effort is to blame for poor treatment effects. Returning to the example in Section 2, imagine a trial of a water treatment product known to the principal to be effective only if agents use it whenever they drink water. If measured returns to the treatment are low, there are two competing explanations: 1) the treatment is not effective in the agents' disease environment, 2) agents are not expending appropriate effort using the product. Agents' perceived returns can help distinguish these explanations. If perceived returns to effort are high, then the agent is likely to be expending significant effort, and it becomes more likely that the treatment is not effective in a particular disease environment. If, instead, perceived returns are low, it becomes more likely that the treatment has an effect that is unmeasured due to agents' lack of effort.

Preference data $V_t(\phi)$ may also provide some insight into the nature of placebo effects. Under a sufficiently broad definition of behavior (including unconscious or involuntary behavior), behavioral treatment effects Δ^B are largely undistinguishable from placebo effects (Malani, 2006). However, because indirect preferences identify whether or not agents intend

[31] Identifying these derivatives does not require large samples, but requires the precise elicitation of preferences. This relies heavily on the rationality of agents. In practice, it may be preferable to use simpler mechanisms that elicit $V_t(\phi)$ for only a few values of ϕ, and construct discrete approximations of the appropriate derivatives. As $V_t(\phi)$ is convex in ϕ, a few points are sufficient to obtain correct bounds on derivatives.

to change their behavior (Proposition 5), this data provides some indication of whether behavioral effects Δ^B are driven by conscious or unconscious changes in behavior. For instance, if agents do not intend to change their behavior, and yet exhibit positive behavioral effects ($\Delta^B > 0$), this suggests that unconscious or involuntary changes in behavior are driving behavioral treatment effects.

6 Incentivized Selective Trials

We now show how quantities similar to those identified by blind selective trials can be identified without a placebo. This can be accomplished using an incentivized selective trial, which allows agents to express preferences over contracts.[32] A fully worked-out numerical example illustrating inference from incentivized trials is given in Appendix D.

6.1 Framework and Basic Results

Assignment Mechanisms. As noted in Section 3, an incentivized trial is a mechanism $G = (M, \mu)$, where $\mu : M \to \Delta(\{0,1\} \times \mathbb{R} \times \mathbb{R})$. Given a message m, μ is used to draw a treatment status τ, a fixed transfer p from the agent, as well as a bonus w transferred to the agent in the event of success. Note that both p and w may be negative in the case of insurance. The pair (τ, w) can be thought of as an aggregate treatment.

Utility and Effort. The agents' indirect preferences over contracts (τ, w), denoted by $V_t(\tau, w)$, are given by

$$V_t(\tau, w) = \max_{e \in E} q_t(\tau, e)[u(y{=}1, t) + w] + [1 - q_t(\tau, e)]u(y{=}0, t) - c(e, t). \tag{7}$$

[32] For field experiments using explicit incentives see, for instance, Gertler (2004); Schultz (2004); Volpp et al. (2006, 2008); Thornton (2008); and Kremer et al. (2009).

We denote by $e^*(\tau, w, t)$ the induced effort level, and maintain the normalization $V_t(\tau = 0, w = 0) = 0$.

Insurance. A specific value of w that will be useful is $w_{0,t} \equiv -[u(y = 1, t) - u(y = 0, t)]$, the utility difference between failure ($y = 0$) and success ($y = 1$). The negative bonus $w_{0,t}$ essentially provides the agent with perfect insurance over the outcome y. When fully insured, the agent will expend the effort that minimizes the cost of his effort regardless of his treatment status. Note that this level of effort differs from the default behavior of untreated agents in an open trial, as agents in open trials may still be exerting some effort to improve their outcomes.

We proceed by assuming that $w_{0,t}$ is known to the principal. At the end of the section we show that under some conditions, $w_{0,t}$ can be inferred from elicited preferences $V_t(\tau, w)$.

6.2 What can be Inferred from Incentivized Trials?

It is straightforward to extend Propositions 1 and 4, which characterize most informative mechanisms. That is, G is a most informative incentivized trial if it identifies mapping $V_t(\tau, w)$ and, given any message, puts positive density on all possible treatments (τ, w). As before, the BDM mechanism can be adapted to identify $V_t(\tau, w)$—Appendix B provides a detailed description. Note that the information produced by incentivized trials nests that produced by open trials. In particular, $V_t(\tau = 1, w = 0) = V_t$.

As in the case of blind selective trials, incentivized selective trials allow us to decorrelate treatment and effort, as well as infer an agent's perception of how effort affects outcomes. Incentivized selective trials recover the empirical success rate $\mathbb{E}[y|V(\tau, w), \tau, w]$ as a function of preferences, treatment and incentives. This will be independent of reward w if effort does not matter for outcomes or if incentives do not affect effort expenditure.

Isolating returns to treatment and returns to effort. A contract with transfer $w_{0,t} \equiv -[u(y=1,t)-u(y=0,t)]$ provides the agent with perfect insurance. The optimal effort given full insurance minimizes the cost of effort and remains the same regardless of treatment status. We refer to this effort choice as *no effort*. Given $w_{0,t}$, we can identify two quantities similar to those discussed in Section 5.2:

$$\begin{aligned}\text{Returns to Treatment} \mid \text{No Effort} &= \mathbb{E}[y|V_t(\tau,w),\tau=1,w=w_{0,t}] - \mathbb{E}[y|V_t(\tau,w),\tau=0,w=w_{0,t}] \\ \text{Returns to Effort} \mid \text{Treatment} &= \mathbb{E}[y|V_t(\tau,w),\tau=1,w=0] - \mathbb{E}[y|V_t(\tau,w),\tau=1,w=w_{0,t}]\end{aligned}$$

Note that here, returns are measured using no effort as a baseline, rather than the default effort level $e^*(\tau=0,w=0,t)$ exerted by agents in the control group of an open trial.

Identifying Perceived Returns to Effort. Indirect preferences over contracts $V_t(\tau,w)$ also provide information on perceived returns to effort. Recall that $q_t(\tau,e)$ denotes the agent's perceived likelihood of success given treatment status τ and effort e.

Proposition 7 (identifying perceived success rates)**.**

$$\forall \tau, w, \quad \frac{\partial V_t(\tau,w)}{\partial w} = q_t(\tau, e^*(\tau,w,t)).$$

Given knowledge of $w_{0,t}$, this allows us to compute subjective returns to treatment and perceived appropriate effort:

$$\begin{aligned}\text{Perceived Returns to Treatment} &= q_t(\tau=1,w=w_{0,t}|V_t(\tau,w)) - q_t(\tau=0,w=w_{0,t}|V_t(\tau,w)) \\ \text{Perceived Returns to Effort} &= q_t(\tau=1,w=0|V_t(\tau,w)) - q_t(\tau=1,w=w_{0,t}|V_t(\tau,w)).\end{aligned}$$

Note that if perceived returns to effort are low, this can indicate that an agent plans on expending little or no effort using the technology. The principal can use this information in deciding which agents' usage to monitor more closely.

The monetary equivalent of the cost of effort agents incur to obtain the perceived return to effort above can be obtained by rearranging (7):

$$c(e^*(\tau, w=0, t)) - c(e^*(\tau, w=w_{0,t}, t)) = -w_{0,t} \times q_t(\tau, e^*(\tau, w=0, t)) - [V_t(\tau, w=0) - V_t(\tau, w=w_{0,t})].$$

Note that all parameters on the right hand side are identified from data, except perhaps $w_{0,t}$. Identifying the costs incurred by agents can greatly improve inference. In particular it allows us to distinguish—among agents who believe that appropriate effort has high returns—those who believe that only a small amount of effort is sufficient to obtain high returns, from those who believe that a significant amount of effort is necessary to obtain high returns.

Identifying the full insurance contract. One drawback of incentivized trials is that they rely on identifying the full insurance contract $w_{0,t}$, which may depend on the agent's type. However, $w_{0,t}$ can be identified under additional assumptions.

Fact 3. *Assume that outcome $y = 1$ yields strictly greater utility than $y = 0$, i.e. $u(y = 1, t) > u(y=0, t)$, and agents perceive treatment to be beneficial:*

$$\forall e_0 \in E, \exists e_1 \in E \;\; s.t. \;\; c(e_1, t) \leq c(e_0, t) \quad and \quad q_t(\tau=0, e_0) < q_t(\tau=1, e_1).$$

$$Then, \qquad w_{0,t} = \max\{w \mid V_t(\tau=1, w) = V_t(\tau=0, w)\}.$$

In words, when treatment facilitates success, the full insurance transfer $w_{0,t}$ is the highest transfer such that the agent does not value obtaining the treatment. Note that our assumptions rule out cases where the agent believes treatment reduces the likelihood of success, as well as environments where the agent values treatment only for reasons other than its impact on the principal's outcome of interest. Whenever the assumptions of Fact 3 do not hold, $w_{0,t}$ must be calibrated from alternative data, for example, the expected amount of wages lost when sick. This is a delicate task, and estimates of $w_{0,t}$ are likely to be noisy.

The corresponding insurance contract would not induce no effort, but rather a small, and slightly uncertain, level of effort. Hence, whenever insurance contract $w_{0,t}$ is estimated with noise, we also obtain noisy estimates of treatment effects.

7 Discussion

This paper studies inference and external validity when experimental subjects take unobserved decisions which can affect outcomes. In particular, as effort expenditure is driven by beliefs and beliefs can respond to information, the returns measured by an RCT may not be representative of the returns a better informed population would obtain. To address this issue we take a principal-agent approach to trial design where the principal maximizes the informativeness of data. This leads us to study selective trials, which improve on RCTs by letting agents express preferences over treatments of varying richness. We show that selective trials can identify whether agents' beliefs are reducing measured treatment effects, as well as separate the returns from treatment, effort, and their interaction.

More generally, this paper advocates a mechanism design approach to randomized controlled trials, which we believe can help build bridges between reduced form methods—largely concerned with robustness and internal validity—and structural methods—which use models to identify deep parameters necessary to evaluate external validity. While we believe that this research agenda can yield many useful applications, successfully implementing its insights requires overcoming a number of practical hurdles. In the remainder of this section we discuss some of these implementation challenges and directions for future work.

7.1 Implementation Issues.

In theory, the selective trials described in this paper are robust and require no specific knowledge on the part of the principal. However, our results are obtained under three important sets of assumptions that may be challenged in practice.

Behavioral Assumptions. The correct elicitation of preferences, which is key to our analysis, relies strongly on the assumption that agents are rational. However, as people often fail to play dominant strategies, BDM-like mechanisms only provide a noisy signal of the agents' underlying valuations (Keller et al., 1993; Bohm et al., 1997). This suggests that running even relatively simple open selective trials, let alone full-fledged blind or incentivized selective trials, is likely to be challenging.

Agents may also be subject to behavioral biases that are not taken into account by our framework.[33] A specific concern is that the act of making choices may change agents' preferences. For example, it is possible that an agent who expresses a strong desire for treatment, but does not get it, may attempt to obtain treatment by other means, but would not do so if his valuation was never elicited.[34] Another concern is that agents may try to infer the value of treatment from the principal's choice of experimental design. For example, similar to Milgrom and Roberts (1986), if treatment is only available at a high cost, agents may infer that the technology is more valuable.[35] In these environments, a careful principal should take into account how experimental design influences behavior before drawing inferences.

Ultimately, we believe the best way to address these concerns is through careful and extensive experimentation, blending both laboratory and field work. As laboratory experiments allow the observation of underlying fundamentals, they are essential to understand which implementations of selective trials produce more reliable data, and what the relevant biases may be. In turn, field experiments—in simple environments where actual behavior is observable, and trustworthy surveys may be conducted—are needed to check that the insights gathered from the laboratory apply in more realistic settings. We anticipate that appropriate

[33] For instance, loss aversion, ambiguity aversion or even social preferences may play a significant role. A different bias might come from the psychological cost of parting from *any* amount money (Cohen and Dupas, 2010; Kremer and Miguel, 2007).

[34] One way to test for this is to construct a second control group that is never asked to express preferences.

[35] In such a setting, one would want to consider the design itself as part of the experimental treatment and compare agents whose preferences are the same, but have been elicited using different mechanisms.

implementations should give agents multiple opportunities to learn how to play the relevant mechanisms before they actually express preferences over treatment (Plott and Zeiler, 2005). Additionally, it may be preferable to use mechanisms that elicit coarse information about preferences, but impose a smaller cognitive burden on agents.[36]

Finally, even if our behavioral assumptions are wrong, the data generated still enriches that obtained through an RCT. Although this invalidates the interpretation of the data put forth in this paper, it does not preclude a more standard analysis focusing on average treatment effects, or a more sophisticated analysis taking into account relevant biases.

Sample Size. Large samples are likely to be necessary in order to get the full value of the additional data that our mechanisms elicit. Note that the difficulty is not with the data collection process (the correct elicitation of preferences relies only on rationality). Rather, sample size restricts our ability to compute meaningful estimates of treatment effects conditional on preferences. The issue is inherent to any non-parametric estimation of treatment effects conditional on a rich set of explanatory variables, and existing methodologies apply (see for instance Pagan and Ullah (1999)). Given sufficiently large samples, a kernel regression may be practical. In small samples, it may be necessary to bin agents with similar preferences. Alternatively, it may be informative to estimate parametric relationships between treatment effects and preference data.[37]

Cash Constraints. Eliciting preferences using monetary trade-offs is impractical in the presence of severe cash constraints. When the constraint in only on the side of the agent, a possible (but expensive) solution is to give agents a show-up fee which they can use to express preferences.

[36] In the case of open trials, one may elicit the agent's preferences over only a few lotteries—see Appendix B for a discussion. In the case of blind trials, one may elicit $V_t(\phi)$ at a few values of ϕ and exploit the fact that $V_t(\phi)$ is convex to fit simple functional forms.

[37] Note that controlling for preferences may reduce the heterogeneity of treatment effects within each bin. This may alleviate statistical power concerns.

More fundamentally, monetary trade-offs may be uninformative of intended behavior in environments where there is sizable heterogeneity in the marginal value of income. For example, Cohen and Dupas (2010) find that willingness to pay for bednets in Kenya is a poor predictor of their actual use.[38] In that setting, other trade-offs—such as willingness to wait, willingness to perform tedious tasks, or willingness to return at a later time—may be more informative of agents' intended behavior. The choice of the relevant trade-off is an important degree of freedom that should be guided by local knowledge.

In general, it is clear that implementing the ideas advocated in this paper entails complex experimental designs and the details of individual experiment may need to be fine tuned with careful, context-dependent, pilot projects. However, we are encouraged by recent field experiments showing that complex designs can be successfully implemented (see Ashraf et al., 2010; Karlan and Zinman, 2009; and particularly Berry et al., 2011, which implements a BDM mechanism in the field). Thus, despite the significant caveats detailed in this section, we are hopeful that our approach will prove useful in guiding future field work.

7.2 Theoretical Extensions

Our approach also suggests directions for further theoretical work. We expect these extensions to be amenable to analysis, but they are sufficiently interesting in their own right to deserve an independent treatment. We outline two of these extensions, specifying both the challenges they pose and their potential value added.

Extension to Dynamic Mechanisms. While our framework can accommodate learning and dynamic effort expenditure by agents, we focus on mechanisms that elicit agents' preferences only once. This is a significant restriction as identifying whether, and how, agents are changing their behavior over time is an important input in the analysis of treatment

[38]Note that this is not always the case. Ashraf et al. (2010) document the opposite finding for water treatment products in Zambia.

effects (Philipson and Desimone, 1997; Philipson and Hedges, 1998; Scharfstein et al., 1999; Chan and Hamilton, 2006). However, the timing of elicitation is a free design variable. In particular it may occur before or after an agent has been exposed to the technology.

For concreteness, consider a technology that requires sustained effort to yield returns, for example, anti-depressants with delayed effects, technologies exhibiting significant learning-by-doing, and so on. Eliciting how preferences change over time would improve inference by helping to distinguish agents exhibiting consistent motivation throughout the trial from agents whose motivation drops in the middle. The difficulty is that eliciting preferences in the future necessarily changes anticipations of treatment status, and in turn changes current effort expenditure. In particular, if an agent is promised treatment in future periods to induce a particular effort level today, then it becomes impossible to elicit preferences in the future without breaking this promise.[39]

Extension to multi-agent mechanisms. The mechanisms considered in this paper are all single-agent mechanisms—an agent's assignment depends only on the message he sends and not on the messages sent by others. This allows us to identify the agent's preferences and his beliefs over his own returns to treatment and to effort. Considering multi-agent mechanisms, in which assignment depends on the messages sent by others, can allow us to identify the agent's beliefs about others' values, others' success rates, and so on.

The information elicited by multiple-agent mechanisms may be useful if there are externalities between agents, as in Miguel and Kremer (2004), or to investigate social learning. For example, if we observe that most agents have low value for the technology but believe that others have high value for the technology, this suggests a specific failure of social learning, and provides us with the means to correct it. Indeed, if most agents do not expend effort using the technology but believe others do, then agents will interpret others' poor outcomes

[39]In the context of labor market experiments, Abbring and Van den Berg (2003, 2005) make a similar point: if expectations of potential access to treatment change ex ante behavior (e.g. investment in human capital) then treatment effects are not identified.

as a signal that even with high effort the technology does not yield returns. Providing the agents with actual data on others' willingness to pay corrects these inference mistakes and may increase experimentation.

Appendix

A Extensions

A.1 General Outcome Space

Most of the results extend directly to the case where y takes values in a general outcome space Y, and is distributed according to some density function $f_y(R, \tau, e, t)$. We denote by $f_{y,t}(\tau, e) \equiv \int_R f_y(R, \tau, e, t)\mathrm{d}t(R)$ the subjective distribution of returns from the perspective of an agent of type t. Values simply go from being sums of two terms to being integrals. The only change to the mechanisms we consider concern incentivized trials. Incentive contracts are now functions $w : Y \to \mathbb{R}$. We have that

$$\begin{aligned} V_t &= \max_{e\in E} \int_y u(y,t) f_{y,t}(\tau = 1, e)\mathrm{d}y - c(e,t) \\ V_t(\phi) &= \max_{e\in E} \phi \int_y u(y,t) f_{y,t}(\tau = 1, e)\mathrm{d}y + (1-\phi)\int_y u(y,t) f_{y,t}(\tau = 0, e)\mathrm{d}y - c(e,t) \\ V_t(\tau, w) &= \max_{e\in E} \int_y [u(y,t) + w(y)] f_{y,t}(\tau, e)\mathrm{d}y - c(e,t). \end{aligned}$$

Propositions 1, 2, 3, 4 and 5 extend directly with these generalized value functions. Propositions 6 and 7, which identify subjective returns differ as follows. Proposition 7, which deals with incentivized trials is the easiest to extend. Indeed, we have that

$$\forall y_0, \quad \frac{\partial V_t(\tau, w)}{\partial w(y_0)} = f_{y,t}(\tau, e^*(\tau, w, t))(y_0),$$

which is a direct extension of Proposition 7.

Proposition 6, which deals with blind trials is more difficult to extend as now we have only a one-dimensional instrument, $\phi \in [0,1]$ to identify an entire function $f_{y,t}$ rather than the single parameter q_t. We now identify

$$\frac{\partial V_t(\phi)}{\partial \phi} = \int_y u(y,t)[f_{y,t}(\tau = 1, e^*(\phi,t))(y) - f_{y,t}(\tau = 0, e^*(\phi,t))(y)]\mathrm{d}y \tag{8}$$

which corresponds to a utility weighted subjective treatment effect given subjectively appropriate effort under belief ϕ.

A.2 Eliciting Preferences under Non-Quasilinear Utility

The approach developed in this paper largely extends to the case where preferences are not quasilinear, although we must consider slightly different mechanisms. We now consider utility taking the form $u(y, e, p, t)$ where $y \in Y$, $e \in E$, $p \in P$ is now a prize (i.e. a bundle of goods which may or may not include monetary transfers), and t is the agent's type. We focus on the case where there exists an unambiguously most desirable prize $\overline{p} \in P$, and an unambiguously least desirable prize, $\underline{p} \in P$.

In the case of open trials, indirect preferences take the following form:

$$V_t(\tau, p) = \max_e \int_y u(y, e, p, t) f_{y,t}(\tau, e)\mathrm{d}y.$$

Say we want to elicit preference over $(\tau, p) \in \{0,1\} \times P$. We assume for simplicity that for all such (τ, p), $V_t(\tau = 0, \underline{p}) \leq V_t(\tau, p) \leq V_t(\tau = 1, \overline{p})$. We normalize $V_t(\tau = 0, p = \underline{p}) = 0$ and $V_t(\tau = 1, p = \overline{p}) = 1$. Consider the following generalization of the BDM mechanism: the agent sends a message $m \in \mathbb{R}^{\{0,1\}\times P}$, which corresponds to a value function; the principal randomly picks (τ, p, λ) from some continuous distribution over $\{0,1\} \times P \times [0,1]$; the agent is assigned (τ, p) if $m(\tau, p) > \lambda$ and the lottery $\lambda \times (\tau = 1, p = \overline{p}) + (1 - \lambda) \times (\tau = 0, p = \underline{p})$

otherwise. In this setting it is dominant for the agent to send message $m = V_t$. Similar mechanisms allow us to identify indirect preferences in the case of blind and incentivized trials.

Propositions 1, 3, 4 and 5 extend directly with these generalized value functions. Again, extending Propositions 6 and 7 requires some more work. Proposition 6—which identifies subjective returns to effort using blind trials—extends as is when $y \in \{0, 1\}$, and extends according to equation (8) when y takes values in a general outcome set Y. Proposition 7 extends as is when preferences are separable in prize p, that is, when $u(y, e, p, t) = u_0(y, e, t) - u_1(p, t)$. When preferences are not separable in prize p, incentivized trials allow us to identify $f_{y,t}(y)\frac{\partial u}{\partial p}\Big|_{y,p}$ for all values of y and p (when preferences are separable, the multiplicative constant can be identified from the fact that probabilities sum to 1).

B Implementation

B.1 Implementing Open Selective Trials as a Finite Menu of Lotteries

The mechanisms described in the paper all use a continuum of messages and elicit the agent's exact willingness to pay. Of course it is possible to use simpler mechanisms to elicit coarser information. This example shows how to identify which of N intervals an agent's willingness to pay belongs to.

Let the principal choose value thresholds $-V_{\max} = V_0 < V_1 < \cdots < V_N = V_{\max}$. She can elicit the interval where an agent's value lies by offering a menu of lotteries. This menu is constructed with messages $M = \{1, \cdots, N\}$ and any increasing sequence $\pi(1) < \pi(2) < \cdots < \pi(N)$ of sampling rates. Thus, message $m \in M$ corresponds to buying the lottery that delivers treatment with probability $\pi(m)$. In order to match these messages with the appropriate value interval, the principal simply sets $p(m)$, the price of lottery m, according

to:

$$\forall k > 1, \quad p(k) = p(k-1) + (\pi(k) - \pi(k-1))V_{k-1}. \tag{9}$$

Note that the sequence of prices is entirely determined by $p(1)$. Denote by $G^{\pi,p}$ the mechanism corresponding to this menu of lotteries, then:

Fact 4. *Under mechanism $G^{\pi,p}$ an agent of type t sends message k if and only if $V_t \in [V_{k-1}, V_k]$.*

This emphasizes the many degrees of freedom the principal has when implementing selective trials as menus of lotteries. The value intervals according to which agents are classified, and the rates according to which they obtain treatment are, to a large extent, free parameters. The only restriction is that sampling rates must be increasing in an agent's value (Proposition 2).

B.2 Implementing Incentivized Selective Trials

This section complements Section 6 by describing how to implement incentivized selective trials as an extension of BDM. Let the message space M be the set of (normalized) possible utility functions $V_t(\tau, w)$:

$$M = \{m \in \mathbb{R}^{\{0,1\}\times\mathbb{R}} \text{ s.t. } m(0,0) = 0\}.$$

Let $F_{\tau,w}$ be a full-support probability distribution over $\{0,1\} \times \mathbb{R}$ and let $(F_{p|\tau,w})_{(\tau,w)\in\{0,1\}\times\mathbb{R}}$ denote a set of full-support conditional probability distributions over $p \in \mathbb{R}$. The mechanism is run as follows: the agent submits a utility function m. A pair (τ, w) and a price p are independently drawn according to $F_{\tau,w}$ and $F_{p|\tau,w}$. If $p \leq m(\tau, w)$, then the agent is given allocation (τ, w) and pays p. If $p > m(\tau, w)$, the agent is assigned $(0, 0)$ and makes no transfers. Because $F_{\tau,w}$ as well as $F_{p|\tau,w}$ have full-support, it is optimal for the agent to send message $m(t) = V_t(\tau, w)$. In turn, a mechanism is a *most informative incentivized trial* if

and only if: (i) it elicits value function $V_t(\tau, w)$, and (ii), for any message m, the induced distribution over $(\tau, w) \in \{0,1\} \times \mathbb{R}$ has full support.

Note that instead of eliciting preferences over a continuous domain $\{0,1\} \times \mathbb{R}$, the same methodology can be used to elicit preferences over a finite grid. The distribution $F_{\tau,w}$ then needs to have full-support with respect to the grid of interest.

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Online Appendix – not intended for publication

C Proofs

Proof of Fact 1: The data $\mathbf{d}_G$ can be broken in two subsamples, $(d_G^{\sigma_0(i)})_{i\in\mathbb{N}}$ and $(d_G^{\sigma_1(i)})_{i\in\mathbb{N}}$, such that σ_0, σ_1 are non-decreasing mappings from $\mathbb{N}$ to $\mathbb{N}$, and for all $i \in \mathbb{N}$, $\tau_{\sigma_0(i)} = 0$ and $\tau_{\sigma_1(i)} = 1$. Since $\forall m,\ \pi(m) \in [\xi, 1-\xi]$, we have that each such subsample is infinite and we can pick σ_1 and σ_0 to be strictly increasing from $\mathbb{N}$ to $\mathbb{N}$. We define mapping h (such that $h(\mathbf{d}_G) \sim \mathbf{d}_{G_0}$) as follows.

We use the notation $h(\mathbf{d}_G) = (d_i^h)_{i\in\mathbb{N}}$, where $d_i^h = (m_i^h, p_i^h, \tau_i^h, y_i^h)$. For every $i \in \mathbb{N}$, set $m_i^h = \emptyset$, $p_i^h = 0$, and draw τ_i^h as a Bernoulli variable of parameter π_0. Finally set $y_i^h = y_{\sigma_{\tau_i^h}(i)}$. It is easy to check that indeed, $h(\mathbf{d}_G) \sim \mathbf{d}_{G_0}$. ■

Proof of Proposition 1: The proof of the first claim is very similar to that of Fact 1. Consider a mechanism $G = (M, \mu_G)$ such that every player has a strictly dominant strategy. An agent with value $V(t_i)$ chooses a message m_i to solve

$$\max_{m\in M} \quad \pi(m)V(t_i) - \mathbb{E}_\mu(p_i|m_i = m).$$

This problem is entirely defined by player i's value $V(t_i)$. Since a.e. player has a strictly optimal message, this problem has a unique solution for a.e. value.

We now construct a mapping $h : \mathcal{D} \to \Delta(\mathcal{D})$ such that the data generated by G' can be simulated from data generated by G using mapping h. For simplicity we describe the mapping h in the case where M is finite. Given $\mathbf{d}_G$, $h(\mathbf{d}_G)$ is generated as follows.

First, we break down the basic data $\mathbf{d}_G$ in $2 \times \operatorname{card} M$ subsets, according to treatment τ and the message $m_G(V)$ corresponding to the value declared by the agent. Formally, for all $m \in M$ and $\tau \in \{0, 1\}$, we define $(d_G^{\sigma_{m,\tau}(i)})_{i\in\mathbb{N}}$ the ordered subsequence such that for

all i, $m_G(V_{\sigma_{m,\tau}(i)}) = m$ and $\tau_{\sigma_{m,\tau}(i)} = \tau$. Since $0 < \inf_m \pi(m) < \sup_m \pi(m) < 1$, all these subsamples are infinite. Hence, $\sigma_{m,\tau}$ can be chosen to be strictly increasing from $\mathbb{N} \to \mathbb{N}$. We use these subsamples to simulate data $\mathbf{d}_{G'}$.

Let us denote $h(\mathbf{d}_G) = (d_i^h)_{i\in\mathbb{N}}$. For all $i \in \mathbb{N}$, $d_i^h = (m_i^h, p_i^h, \tau_i^h, y_i^h)$. We first set $m_i^h = m_{G'}(V_i)$. Then using $\mu_{G'}(m_i^h)$, we draw values τ_i^h and p_i^h. Finally we set $y_i^h = y_{\sigma_{m_i^h,\tau_i^h}(i)}$. This defines $h : \mathcal{D} \to \Delta(\mathcal{D})$. It is easy to check that $h(\mathbf{d}_G) \sim \mathbf{d}_{G'}$.[40] This concludes the proof. ■

Proof of Fact 2: The fact that the BDM mechanism elicits values is well-known. Since F_p has full support over $[-V_{\max}, V_{\max}]$, assignment to treatment also satisfies full-support and the second part of Proposition 1 implies that G_{BDM} is a most informative mechanism. ■

Proof of Proposition 2: Agents of type t and t' are such that $V_t > V_{t'}$ and $m_G(t) \neq m_G(t')$. Denote $\pi(m) = \mu_G(\tau = 1|m)$ and $p_m = \mathbb{E}_{\mu_G(\cdot|m)}p$. By optimality of the message, it must be that

$$\begin{aligned} \pi(m_G(t))V_t - p_{m_G(t)} &> \pi(m_G(t'))V_t - p_{m_G(t')} \\ \pi(m_G(t'))V_{t'} - p_{m_G(t')} &> \pi(m_G(t))V_{t'} - p_{m_G(t)}. \end{aligned}$$

Adding the two inequalities yields that $[\pi(m_G(t)) - \pi(m_G(t'))](V_t - V_{t'}) > 0$, which implies that $\pi(m_G(t)) > \pi(m_G(t'))$. ■

Proof of Proposition 3: We begin with the first assertion. Given mechanism $G = (M, \mu)$,

[40] Note that for the sake of notational simplicity, this construction ends up wasting data points by not taking consecutive elements from the subsamples. This is inconsequential here since we have infinitely many data points.

we define mechanism $G' = (M, \mu')$ as follows:

$$\forall m \in M, \quad \mu'(m) = \begin{cases} \tau = 0, p = 0 & \text{with probability} \quad \underline{\rho} \\ \mu(m) & \text{with probability} \quad \overline{\rho} - \underline{\rho} \\ \tau = 1, p = 0 & \text{with probability} \quad \overline{\rho} \end{cases}$$

Clearly mechanism G' is strategically equivalent to mechanism G. The proof that $G \preceq G'$ is omitted since it is essentially identical to that of Fact 1.

We now turn to the second assertion. Consider two messages m_1 and m_2 respectively (and optimally) sent by types with values V_1 and V_2. Let $p_m = \mathbb{E}_{\mu_G(\cdot|m)} p$. We must have that

$$\begin{aligned} \pi_{G'}(m_1)V_1 - p_{G'}(m_1) &\geq \pi_{G'}(m_2)V_1 - p_{G'}(m_2) \\ \pi_{G'}(m_2)V_2 - p_{G'}(m_2) &\geq \pi_{G'}(m_1)V_2 - p_{G'}(m_1). \end{aligned}$$

These two inequalities yield that $(\pi_{G'}(m_2) - \pi_{G'}(m_1))V_1 \leq p_{G'}(m_2) - p_{G'}(m_1) \leq (\pi_{G'}(m_2) - \pi_{G'}(m_1))V_2$, which implies that $|p_{G'}(m_2) - p_{G'}(m_1)| < (\overline{\rho} - \underline{\rho})V_{\max}$. Hence the difference in utilities between sending two messages m_1 and m_2 for an agent with value $V \in [-V_{\max}, V_{\max}]$ is $|(\pi_{G'}(m_1) - \pi_{G'}(m_2))V - p_{G'}(m_1) + p_{G'}(m_2)| \leq 2(\overline{\rho} - \underline{\rho})V_{\max}$. ■

Proof of Proposition 4: The proof of Proposition 4 is essentially identical to that of Proposition 1 and hence omitted. ■

Proof of Proposition 5: The proof is given for the general case where there might be multiple optimal effort choices. Let $V_t(\tau, e)$ denote the expected value of type t under treatment

status τ and when expending effort e. We have that

$$\begin{aligned} V_t(\phi) &= \max_{e\in E} \phi V_t(\tau=1,e) + (1-\phi)V_t(\tau=0,e) \\ &\leq \phi \max_{e\in E} V_t(\tau=1,e) + (1-\phi)\max_{e\in E} V_t(\tau=0,e). \end{aligned}$$

If $\arg\max_{e\in E} V_t(\tau=1,e) \cap \arg\max V_t(\tau=0,e) \neq \emptyset$, the inequality is an equality and, since we normalized $V_t(\phi=0)=0$ we obtain that $V_t(\varphi) = \varphi V_t(\phi=1)$. Inversely, if $\arg\max_{e\in E} V_t(\tau=1,e) \cap \arg\max V_t(\tau=0,e) = \emptyset$, the inequality is strict and $V_t(\varphi) < \varphi V_t(\phi=1)$. ■

Proof of Proposition 6: The result follows directly from applying the Envelope Theorem to equation (4). ■

Proof of Proposition 7: The result follows directly from applying the Envelope Theorem to equation (7). ■

Proof of Fact 3: Whenever $w = w_{0,t}$, the agent is perfectly insured and $V_t(\tau=1,w) = V_t(\tau=0,w)$ since access to the technology is valuable only in so far as it affects outcomes. We now show that whenever $w > w_{0,t}$, $V_t(\tau=1,w) > V_t(\tau=0,w)$. The agent's value is

$$V_t(\tau,w) = \max_{e\in E} q_t(\tau,e)[u(y=1,t) - u(y=0,t) + w] + u(y=0,t) - c(e,t).$$

Let e_0^* be the agent's optimal effort level if $\tau=0$. By assumption, there exists e_1 such that $c(e_1,t) \leq c(e_0^*,t)$ and $q_t(\tau=1,e_1) > q_t(\tau=0,e_0^*)$. Since $w > w_{0,t} = u(0,t) - u(1,t)$, it follows that the agent gets strictly higher value under configuration $(\tau=1,e_1)$ than under configuration $(\tau=0,e_0^*)$. This concludes the proof. ■

Proof of Fact 4: Indeed, $m_{G^{\pi,p}}(V) = k$ if and only if for all $k' \neq k$,

$$V\pi_k - p_k > V\pi_{k'} - p_{k'}. \tag{10}$$

For $k' < k$, this last condition is equivalent to $V \geq \max_{k'<k}\{(p_k - p_{k'})/(\pi_k - \pi_{k'})\}$, which in turn is equivalent to $V > V_{k-1}$. Similarly, for $k' > k$, equation (10) is equivalent to $V_k > V$. This concludes the proof. ■

D A Numerical Example Illustrating Inference from Incentivized Trials

This section illustrates step by step the process of inference from trial data, starting with a standard RCT, adding data from open selective trials and concluding by adding both objective and subjective data from an incentivized trial.

As regards the environment, we return to a setting where returns are two dimensional: $R = (R_b, R_e)$. As before, in the context of a water treatment product, R_b could be the baseline returns of using the water treatment product only when it is convenient to do so and R_e the returns to using it more thoroughly (for instance, bringing treated water when away from home). Success rates are given by:

$$q(\tau = 0, e) = 0 \quad \text{and} \quad q(\tau = 1, e) = R_b + eR_e,$$

where $e \in \mathbb{R}_+$ is the agent's effort expenditure. An agent with type t has beliefs $R_t = (R_{b,t}, R_{e,t})$ and maximizes $\mathbb{E}_t[y] - c(e)$ where $c(e) = \frac{e^2}{2}$. The effort expended in an incentivized trial is thus $e^*(w, t) = R_{e,t}(1 + w)$, which nests the effort decision of an open trial, $e^*(w=$

$0, t) = R_{e,t}$.

Throughout, we illustrate the inference process by considering the case where each parameter has a low and high value: $R_e, R_{e,t} \in \{1/4, 1/2\}$, $R_b \in \{0, 1/8\}$ and $R_{b,t} \in \{0, 3/32\}$. Each element of a selective trial adds data which will narrow down the set of possible values.[41]

Inference from an RCT. An RCT identifies the average treatment effect, $\widehat{\Delta} = R_b + R_e \times R_{e,t}$. For the numerical values specified above the possible outcomes are described in the following matrix

	$R_e = 1/2$		$R_e = 1/4$	
	$R_{e,t} = 1/2$	$R_{e,t} = 1/4$	$R_{e,t} = 1/2$	$R_{e,t} = 1/4$
$R_b = 1/8$	$\widehat{\Delta} = 3/8$	$\widehat{\Delta} = 1/4$	$\widehat{\Delta} = 1/4$	$\widehat{\Delta} = 3/16$
$R_b = 0$	$\widehat{\Delta} = 1/4$	$\widehat{\Delta} = 1/8$	$\widehat{\Delta} = 1/8$	$\widehat{\Delta} = 1/16$.

As illustrated by the matrix, if $\widehat{\Delta} \in \{1/16, 3/16, 3/8\}$ this identifies the returns of the technology (R_b, R_e). However, treatment effects $\widehat{\Delta} \in \{1/8, 1/4\}$ are consistent with multiple true returns.[42] In particular, when $\widehat{\Delta} = 1/4$, it may be that casual use of the water treatment product is not particularly effective ($R_b = 0$), more thorough use is not particularly effective ($R_e = 1/4$), or more thorough use is effective, but agents don't believe it is, and so do not expend much effort into using the water treatment product more thoroughly ($R_e = 1/4$, $R_{e,t} = 1/2$).

[41] For simplicity, we consider priors that put point masses on a few possible states. Unfortunately, such strong priors often result in degenerate inference problems. We computed the states to keep the inference problem well defined and better reflect the mechanics of inference from a continuous state space. This accounts for the somewhat unusual aspect of our parameter values.

[42] For example, $(R_b = 0, R_e = 1/2, R_{e,t} = 1/2), (R_b = 1/8, R_e = 1/2, R_{e,t} = 1/4)$ and $(R_b = 1/8, R_e = 1/4, R_{e,t} = 1/2)$ are all consistent with $\widehat{\Delta} = 1/4$.

Note that agents' beliefs may be self confirming. For instance, an agent who believes that effort has high returns, $R_{e,t} = 1/2$, who observes $\widehat{\Delta} = 1/4$ will continue to believe returns are high, even though this data could be generated by $R_e = 1/4$. Such self-confirming beliefs are frequent in the experimentation and social learning literatures (Rothschild, 1974; Banerjee, 1992; Bikhchandani et al., 1992).

Inference from a Selective Open Trial. By Fact 1, open selective trials identify treatment effects $\widehat{\Delta}$. Additionally, by Proposition 1, an open selective trial identifies the agent's willingness to pay for treatment $V_t = R_{b,t} + R_{e,t}^2/2$. To illustrate the value of this data, focus on the case where $\widehat{\Delta} = 1/4$. As shown above, this is consistent with three different vectors of (R_b, R_e, $R_{e,t}$). Based on this, we illustrate the six possible values of V_t in the following matrix:

	$R_b = 0, R_e = 1/2, R_{e,t} = 1/2$	$R_b = 1/8, R_e = 1/2, R_{e,t} = 1/4$	$R_b = 1/8, R_e = 1/4, R_{e,t} = 1/2$
$R_{b,t} = 3/32$	$V_t = 7/32$	$V_t = 1/8$	$V_t = 7/32$
$R_{b,t} = 0$	$V_t = 1/8$	$V_t = 1/32$	$V_t = 1/8$.

If $V_t = 1/32$ the data from selective trials indicates $R_{e,t} = 1/4 = e^*$. As the treatment effect is $\widehat{\Delta} = 1/4$ the only consistent returns are $R_b = 1/8$ and $R_e = 1/2$. If $V_t = 7/32$, there remains uncertainty since the data is consistent with both $(R_b = 0, R_e = 1/2)$ and $(R_b = 0, R_e = 1/4)$. Finally if $V_t = 1/8$, the data is consistent with any of the states (R_b, R_e, $R_{e,t}$) that produce $\widehat{\Delta} = 1/4$. That is to say that even in this limited example, data from a selective open trial (and hence, MTEs) may not help in identifying underlying returns. We now turn to how incentivized trials allow us to infer whether effort or returns to effort are low.

Inference from an Incentivized Trial. Incentivized trials yield:

$$\widehat{\Delta}(w) = R_b + R_e \times R_{e,t}(1+w) \quad \text{and} \quad V_t(\tau{=}1, w) = R_{b,t}(1+w) + \frac{[R_{e,t}(1+w)]^2}{2}.$$

As an open selective trial already identifies $V_t = V_t(w{=}0) = R_{b,t} + R_{e,t}^2/2$ and $\widehat{\Delta} = \widehat{\Delta}(w{=}0) = R_b + R_e \times R_{e,t}$, by eliciting valuations and treatment effects for a small w the principal

can also identify $\frac{\partial V_t(\tau,w)}{\partial w}\Big|_{w=0} = R_{b,t}+R_{e,t}^2$ and $\frac{\partial \widehat{\Delta}(w)}{\partial w}\Big|_{w=0} = R_e \times R_{e,t}$. With this data the principal can identify:

$$R_{e,t} = \left[2\left(\frac{\partial V_t}{\partial w}\Big|_{w=0} - V_t(w=0)\right)\right]^{1/2}$$

and thus, the rest of the unknown parameters: $R_e = \frac{\partial \widehat{\Delta}(w)}{\partial w}\Big|_{w=0} \Big/ R_{e,t}$, $R_{b,t} = \frac{\partial V_t(\tau,w)}{\partial w}\Big|_{w=0} - R_{e,t}^2$, $R_b = \widehat{\Delta} - R_e \times R_{e,t}$. The same information can be identified in a mathematically simpler, but more data intensive, way by identifying $w_{0,t}$ and the empirical quantities associated with that value.

Altogether, incentivized selective trials allow us to identify both the true returns (R_b, R_e) and the agents' beliefs $(R_{b,t}, R_{e,t})$. Thus, in this example, data from a selective incentivized trial allows an principal to determine how effective casual and thorough use of the water treatment product is without having to observe individual agents' usage. This is possible as eliciting each agents' indirect preferences over the water treatment product and bonuses associated with staying healthy allows the principal to infer the agents' beliefs about the effects of casual and more thorough usage. This, in turn, allows the principal to infer behavior and identify the deep structural parameters determining the product's effectiveness, as well as how beliefs about effectiveness lead to different outcomes.