LEARNING THROUGH NOTICING: THEORY AND EVIDENCE FROM A FIELD EXPERIMENT*

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We consider a model of technological learning under which people "learn through noticing": they choose which input dimensions to attend to and subsequently learn about from available data. Using this model, we show how people with a great deal of experience may persistently be off the production frontier because they fail to notice important features of the data they possess. We also develop predictions on when these learning failures are likely to occur, as well as on the types of interventions that can help people learn. We test the model's predictions in a field experiment with seaweed farmers. The survey data reveal that these farmers do not attend to pod size, a particular input dimension. Experimental trials suggest that farmers are particularly far from optimizing this dimension. Furthermore, consistent with the model, we find that simply having access to the experimental data does not induce learning. Instead, behavioral changes occur only after the farmers are presented with summaries that highlight previously unattended-to relationships in the data. *JEL* Codes: D03, D83, O13, O14, O30, Q16.

I. Introduction

Many production functions are not known ex ante. Instead, they are learned, from personal experiences (Arrow 1962; Gittins 1979; Foster and Rosenzweig 1995; Jovanovic and Nyarko 1996) and from those of others (Banerjee 1992; Bikhchandani, Hirshleifer, and Welch 1992; Besley and Case 1993, 1994; Munshi 2004; Conley and Udry 2010). Although diverse, existing learning models share a common assumption: the key input for learning is informative data.

Many examples, however, defy this assumption. For many years, doctors had the data that they needed to prevent operating room infections, but the importance of a sterile operating room was not recognized until the germ theory of disease was accepted

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(Gawande 2004; Nuland 2004). Indian textile manufacturers failed to adopt key management practices, such as having an uncluttered factory floor, despite exposure to natural variation that pointed to their importance (Bloom et al. 2013). Even experienced teachers do not adopt the best teaching practices (Allen et al. 2011). These examples imply that learning is not just about the data that you possess, but what you notice in those data. In fact, we may not learn from the data that we ourselves generate.

In this article, we use Schwartzstein's (2014) model of selective attention to build a model of learning through noticing and test its predictions in a field experiment with seaweed farmers.[1] The model highlights an important constraint on learning. A farmer planting a crop faces a slew of potential features that might affect production—crop spacing, the time of planting, the amount and timing of the water employed, the pressure of the soil on the seedlings, and so on. He cannot possibly attend to everything (Kahneman 1973): his attention is limited (or effortful), while the number of potentially important variables is large. Since he can only learn about the dimensions that he notices (or attends to), this choice becomes a key input into the learning process.

In the model, the farmer allocates attention in a "Savage rational" way: he optimally chooses what to attend to as a Bayesian, given his prior beliefs and the costs of paying attention. Consistent with the foregoing examples, even with substantial experience—and even with lots of readily available data—a farmer may not approach the productivity frontier. Despite the (subjective) optimality assumption, an interesting feedback loop arises: a farmer who initally believes that a truly important dimension is unlikely to matter will not attend to it, and consequently will not learn whether it does matter. This failure to learn stems from not focusing on aspects of the data that could contradict a false belief. When the technology is incongruent with

1. Although the model presented in this paper is specific to issues related to technological use, Schwartzstein (2014) presents a general model of belief formation when agents are selectively attentive. The approach of modeling economic agents as responding only to a subset of available information dates back to at least Simon (1955). Nelson and Winter (1982) consider how bounded rationality (or "evolutionary processes") affect technological change. For more recent approaches to modeling limited attention in economic settings, see Sims (2003), Bordalo, Gennaioli, and Shleifer (2012, 2013), Koszegi and Szeidl (2013), and Gabaix (2013).

the farmer's priors, the losses from such suboptimization can be arbitrarily large.

Although other models of incomplete learning also accommodate the idea that people can persistently be far from the productivity frontier, our model predicts where these failures should occur. Learning failures should be concentrated on dimensions where the farmers report ignorance, that is, where they cannot answer key questions about what they precisely do (or have done) along those dimensions. Most other theories of mistoptimization (arising from overconfidence, false beliefs, unawareness, etc.) have little to say about this "failure to notice" variable.

To test the model's predictions, we conducted a field experiment with seaweed farmers in Indonesia. Seaweed is farmed by attaching strands (pods) to lines submerged in the ocean. As in the model, a large number of dimensions affect yield. To look for failures to notice, we directly asked farmers about their own production techniques. Farmers are quite knowledgeable and have clear opinions about many dimensions: almost all farmers had an opinion about the length of their typical line, the typical distance between their lines, the optimal distance between their knots (pods), the optimal distance between lines, and the optimal cycle length. On the other hand, most do not recognize a role for one key dimension: about 85% do not know the size of their pods and will not venture a guess about what the optimal size might be.[2]

To test whether this apparent failure to notice translates into a learning failure, we conducted experiments on the farmers' own plots, varying both pod size and pod (or knot) spacing. On pod spacing, which almost all farmers had an opinion about, our findings suggest that they were close to the optimum. In contrast, on pod size—which few farmers had an opinion about—our findings suggest they were far from it.

Further support for the model comes from examining farmers' response to the trial. The model suggests that simply participating in the trials may not change the farmers' behavior with

2. Many other dimensions might be important. For example, the strength of the tide, the time of day, the temperature, the tightness with which pods are attached, the strain of pods used, and so on could matter. In our analysis, we largely focus on two or three dimensions for parsimony, but actual demands on attention are much greater.

respect to pod size.[3] Intuitively, the farmers' own inattentive behavior generates an experiment of sorts every season—random variation in pod sizing—and the trial presents farmers with similar data to what they already had access to but did not learn from. Consistent with this idea, we find little change in pod size following participation in the trial. However, the model suggests an alternative way to induce learning: to provide a summary of the data that explicitly highlights neglected relationships. Consistent with this prediction, we find that farmers changed their production methods after we presented them with the trial data on yield broken down by pod size from their own plots.

Beyond the analysis of seaweed farming, we extend the model to illustrate how greater experience with related technologies can predictably *increase* the likelihood of learning failures and make precise a notion of the "complexity" of the current technology that can also induce failures. The potential costs of experience and the role of complexity are consistent with folk wisdom and evidence on technology adoption and use (e.g., Rogers 2003), but to our knowledge have largely remained theoretically unexplored.

The model also has broader implications for understanding which types of interventions can help individuals learn. In most learning models, providing more data induces learning. Our model and experimental results, in contrast, suggest that simply providing more data can have little effect on behavior if data availability is not a first-order problem. Our findings shed light on why some demonstrations or agricultural extension activities are ineffective or only moderately effective in the long run (e.g., Kilby 1962; Leibenstein 1966; Duflo, Kremer, and Robinson 2008b). At the opposite extreme, our model and experimental results show how one can induce learning without providing new data by simply providing summaries highlighting previously unattended-to relationships in the agents' own data. This result aligns with growing evidence that interventions that encourage agents to attend more closely to available data can profitably affect behavior in diverse contexts ranging from car

3. Strictly speaking, this is a prediction of the model under the assumption that merely being asked to participate does not, by itself, significantly alter farmers' beliefs about the importance of pod size, which we had prior reason to believe would be true (as we discuss later). It also relies on an assumption that it is not significantly easier for the farmers to learn relationships from the raw trial data than from the data they are typically exposed to, which also seems plausible given the experimental design.

manufacturing (Liker 2004), to teaching (Allen et al. 2011), to shopkeeping (Beaman, Magruder, and Robinson 2014).

The article proceeds as follows. Section II presents the baseline model of learning through noticing and develops the empirical predictions. Section III describes the experiment, and Section IV provides its results. Section V explores other potential applications and extends the model to develop comparative static predictions on the prevelance of failures to notice and resulting failures to learn. Section VI concludes.

II. Model

II.A. Setup

We present a stylized model of learning through noticing which builds on Schwartzstein (2014). We present it for the case of a farmer to make it concrete, and reinterpret it for other contexts, such as management or education, in Section V. A farmer plants the same crop for two periods, $t \in \{1, 2\}$. He works on a continuous sets of plots indexed by $l \in [0, 1]$. To capture the idea that the constraint on learning comes from having many things to pay attention to, his production technology has many dimensions that might matter. Specifically, for each of his plots l, the farmer chooses an N-dimensional vector of inputs, $\mathbf{x} = (x_1, x_2, \ldots x_N)$, where each x_j is drawn from a finite set X_j, and where we denote the set of possible $\mathbf{x}$ by X.

Given the input bundle $\mathbf{x} \in X$, the dollar value of net yield (total yield net of the costs of inputs) for the farmer from a plot l at time t is:

$$y_{lt} = f(\mathbf{x}|\theta) + \varepsilon_{lt} = \sum_{j=1}^{N} f_j(x_j|\theta_j) + \varepsilon_{lt}, \tag{1}$$

where $\theta = (\theta_1, \ldots, \theta_N)$ is some fixed parameter of the technology (described later) and $\varepsilon_{lt} \sim \mathcal{N}(0, \sigma_\varepsilon^2)$ is a mean zero shock that is independent across plots of land and time.[4]

4. Note that l and t appear symmetrically in equation (1). For the purpose of forming beliefs about the underlying technology (θ), it does not matter whether the farmer learns through many observations across plots at a fixed time, or through many observations over time on a fixed plot. Equation (1) also reflects a simplifying assumption that the payoff function is separable across input dimensions, allowing us to separately analyze the farmer's decisions across dimensions.

1. Beliefs. Since the model focuses on how inattention affects learning, we assume that the farmer initially does not know the parameters of the production function: he must learn $\theta = (\theta_1, \ldots, \theta_N)$ through experience.

The farmer begins with some prior beliefs about θ, where we denote a generic value by $\tilde{\theta}$. We assume that although many dimensions could be relevant for production, only a few of them actually are. To capture this, we assume that the farmer attaches prior weight $\pi_j \in (0, 1)$ to input j being relevant and the remaining weight, $(1-\pi_j)$, to input j being irrelevant. Specifically, the farmer's prior holds that:

$$(2) \quad f_j(x_j|\tilde{\theta}_j) = \begin{cases} 0 & \text{if input } j \text{ is irrelevant} \\ \tilde{\theta}_j(x_j) \sim \mathcal{N}(0, \nu^2), & \text{if input } j \text{ is relevant.} \\ \quad \text{i.i.d across } x_j \in X_j & \end{cases}$$

We assume $\nu^2 > 0$ and that the prior uncertainty is independent across dimensions, meaning that knowledge of how to set input j does not help the farmer set input j'. Under these assumptions, when input j is irrelevant, it does not matter how x_j is set. When input j is relevant, with probability 1 some particular level of x_j produces the greatest output. But the farmer does not initially know which one because the exact θ_j is unknown and must be learned.

2. Costly Attention. To this standard learning problem, we add limited attention. The farmer makes a zero-one decision of whether to attend to a given input. Let $\mathbf{a_t} \in \{0, 1\}^N$ denote a vector that encodes which dimensions the farmer attends to in period t, where $a_{jt} = 1$ if and only if he attends to dimension j in period t. For each input dimension he attends to, he faces a cost e, reflecting the shadow cost of mental energy and time.

Inattention operates in two ways. First, when the farmer does not attend to input j, a "default action" is taken, which we capture simply as a uniform distribution over the possible values of that input: X_j. Thus, the farmer's actions are random in the absence of attention. When a farmer attends to an input, he chooses its level.

Second, if the farmer does not attend to an input, he also does not notice the set level(s). Thus, in period 2, instead of knowing the values x_{jl1} which were actually chosen, he only remembers:

(3)
$$\hat{x}_{jl1} = \begin{cases} x_{jl1} & \text{if } a_{j1} = 1 \\ \emptyset & \text{if } a_{j1} = 0. \end{cases}$$

Notationally, we write the full second-period history as $h = (y_{l1}, \mathbf{x}_{l1})_{l \in [0,1]}$, which is what the farmer would recall if he were perfectly attentive, and $\hat{h} = (y_{l1}, \widehat{\mathbf{x}}_{l1})_{l \in [0,1]}$ as the recalled history.[5]

In our seaweed application, if the farmer does not attend to pod size, he both creates random variation by virtue of not focusing on this dimension when cutting raw seaweed into pods and does not know the know the specific pod sizes that were set, making it harder for him to learn a relationship between pod size and output.

3. The Farmer's Problem. The farmer is risk-neutral and maximizes the expected undiscounted sum of net payoffs—yield minus attentional costs—across the two periods. To simplify the presentation by allowing us to drop the l subscript, we restrict the farmer to strategies that are symmetric across plots in a given period, but allow him to mix over input choices. When it does not cause confusion, we abuse notation and let x_{jt} both denote the farmer's choice along dimension j at time t if he attends to that input, and the default uniform distribution over possible input levels when he does not attend.

This simple model captures a basic learning problem. In the first period, the farmer makes choices that trade off the future benefits of experimentation against maximizing current expected payoffs. In the second period, the farmer simply chooses to maximize current expected payoffs.[6]

5. Under the interpretation that y measures the dollar value of yield net of input costs, we are implicitly assuming that the farmer knows the total input costs even if he does not attend to certain input choices. By analogy, we may know our total monthly spending without knowing our spending by category, for example food or clothing.

6. The model's assumption of two periods but many plots implies that experimentation will occur across plots in a fixed time period, rather than over time. Thus, we can avoid tangential issues that arise in settings with multiperiod experimentation, such as considering the degree to which agents are sophisticated in updating given missing information (Schwartzstein 2014). That there are a continuum of plots also simplifies matters by allowing us to abstract from noise in the learning

II.B. Results

We first ask when the farmer will end up at the productivity frontier. For a technology (θ) and given cost of attention (e), let x_j^* denote a (statically) optimal input choice along dimension j. More precisely, x_j^* maximizes $f_j(x_j|\theta_j)$ whenever dimension j is worth attending to, that is, whenever the payoff from setting the optimal input along dimension j and incurring the attentional cost to do so, $\max_{x_j} f_j(x_j|\theta_j) - e$, exceeds the payoff from not attending to the input and thus randomly setting it, $\frac{1}{|X_j|}\sum_{x_j'} f_j(x_j'|\theta_j)$. Whenever dimension j is not worth attending to, x_j^* equals the default uniform distribution over X_j (with an obvious abuse of notation). To make the problem interesting, we assume that there is at least one dimension that is worth paying attention to, because otherwise the farmer will necessarily end up at the frontier.

To more broadly understand when the farmer ends up at the productivity frontier, we focus on second-period choices.

PROPOSITION 1.

(i) When there are no costs of attention ($e = 0$), the farmer learns to optimize every dimension: in the second (terminal) period he chooses $x_{j2} = x_j^*$ for all j.

(ii) With costs of attention ($e > 0$),

- (a) The farmer may not learn to optimize certain dimensions: for every technology θ, there exists a collection of prior weights $\pi_i > 0$, $i = 1\ldots,N$, such that in the second period he chooses $x_{j2} \neq x_j^*$ for some input j.
- (b) Losses from not optimizing are unboundedly large: for every constant $K \in \mathbb{R}^+$, there exists a technology θ and collection of prior weights $\pi_i > 0$, $i = 1\ldots,N$, such that in the second period the farmer chooses $x_{j2} \neq x_j^*$ for some j and, by doing so, loses at least K.

(iii) The farmer does not learn to optimize a dimension only if he did not attend to it in the first period: in the second period he chooses $x_{j2} \neq x_j^*$ only if $a_{j1} = 0$.

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process; in principle the farmer can perfectly learn the technology in a single period. This will allow us to focus on how inattention, rather than incomplete information, contributes to incomplete learning.

Proof. See Online Appendix A for all proofs.

The first part of Proposition 1 replicates the standard result of learning by doing models that, with no costs of attention, learning is primarily a function of experience and enough of it guarantees optimal technology use (e.g., Arrow 1962; Nelson and Phelps 1966; Schultz 1975). This intuition is invoked (at least implicitly) in arguments that learning by doing is not an important determinant of adoption for old technologies in which individuals have abundant experience (Foster and Rosenzweig 2010).

The second part of the proposition in contrast shows that, with costs of attention, failures to notice can lead to failures to learn, and these failures can lead to big payoff losses. The basic idea is simple: inattention is self-confirming (Schwartzstein 2014). If the farmer initially falsely believes an input is not worth attending to, he will not notice the information that proves him wrong and will continue not attending to the input even if it is very important.[7] As the proof of Proposition 1 makes clear, the farmer fails to learn to optimize dimension j when the dimension matters, but he places sufficiently low prior weight π_j on it mattering. Resulting errors can be arbitrarily large: the constant K need not be in any way proportional to the cost of attention, so even very small attentional costs can produce large losses. The final part of the proposition says that optimization failures come directly from noticing failures in our model. This makes the model testable by placing an empirically verifiable restriction that a farmer fails to optimize on a dimension j only if $\hat{x}_{jl1} = \emptyset$ on that dimension.[8]

Proposition 1 yields the following testable predictions:

PREDICTION P1. Agents may fail to attend to some dimensions.

PREDICTION P2. Agents may persistently choose suboptimal input levels along some dimensions.

7. The logic is similar to why individuals can maintain incorrect beliefs about the payoff consequences to actions that have rarely been tried in bandit problems (Gittins 1979) and in self-confirming equilibria (Fudenberg and Levine 1993), and why these incorrect beliefs in turn support suboptimal decisions.

8. Similarly, under the assumption that farmers do not measure and consequently randomly select inputs along dimensions they do not pay attention to, identifying dimensions along which farmers persistently use a wide variety of input levels will also predict failures to notice and resulting failures to learn. Likewise, identifying dimensions along which farmers cannot recall the empirical relationship between the input level and the payoff will predict such failures.

PREDICTION P3. Agents only persistently choose suboptimal input levels along dimensions they do not attend to, which can be identified by measuring recall.

To develop further predictions, remember that inattention has two consequences: a failure to precisely set input levels and a failure to notice and recall what they are. These two effects work in different directions. Failing to remember clearly impedes learning. However, failing to precisely set input levels can help learning: when the farmer does not attend to an input, he inadvertently experiments and generates valuable data. Because he does not notice these data, he does not capitalize on them. But this distinction has an interesting implication.

Specifically, suppose that in period 2 a farmer could have access to summary statistics about his own data from period 1, for example because an extension officer calculates and presents the farmer with such information. What effect would this have? One way to model this is to suppose that, for each input j, the farmer is told the level $\tilde{x}_j^* \in X_j$ that achieved the greatest yield in period 1, as well as the corresponding sample average $\bar{y}_j(\tilde{x}_j^*)$.[9] Since the cost of tracking the input relative to yield on measure 1 of plots of land is e, it is natural to assume that the cost of attending to a single statistic $(\bar{y}_j(\tilde{x}_j^*), \tilde{x}_j^*)$ is much lower, which for simplicity we will take to equal 0. To emphasize the effect of receiving a summary about data the farmer would generate on his own, suppose that receiving it comes as a complete surprise: when choosing how to farm and what to attend to in the first period, he does not anticipate that he will later be provided with this summary. However, suppose that he understands how the summary is constructed, that is, he correctly interprets the

9. Specifically, letting $L_j(x_j) = \{l \in [0,1] : x_{jl1} = x_j\}$ denote the set of plots of land where the farmer uses input level x_j in the first period, the sample average payoff conditional on using that input equals:

$$\bar{y}_j(x_j) = \frac{1}{|L(x_j)|}\int_{l \in L(x_j)} y_{l1} dl = \frac{1}{|L(x_j)|}\int_{l \in L(x_j)} f_j(x_j|\theta_j) + \sum_{k \neq j} f_k(x_{kl1}|\theta_k) + \varepsilon_{l1} dl$$
$$= f_j(x_j|\theta_j) + \sum_{k \neq j} f_k(\sigma_{k1}|\theta_k),$$

where $\sigma_{k1} \in \Delta(X_k)$ denotes the distribution over input levels in X_k implied by the farmer's first period strategy. $\tilde{x}_j^*$ is then defined as a maximizer of $\bar{y}_j(\cdot)$, where any ties are arbitrarily broken.

summary given his prior over θ and the objective likelihood function of the summary.

PROPOSITION 2. If the farmer has access to summary information $((\bar{y}_j(\tilde{x}_j^*), \tilde{x}_j^*)_{j=1}^N)$ prior to making second-period decisions, then he learns to optimize every dimension j: he chooses $x_{j2} = x_j^*$ for all j.

The intuition behind Proposition 2 is simple: together with $\hat{h}$, $(\bar{y}_j(\tilde{x}_j^*), \tilde{x}_j^*)$ is a sufficient statistic relative to the farmer's second-period decision along dimension j; that is, he will make the same choice on that dimension whether he knows all of h or just $\hat{h}$ and $(\bar{y}_j(\tilde{x}_j^*), \tilde{x}_j^*)$.

Proposition 2 demonstrates that in the model, learning failures do not result from a lack of readily available data (or from a doctrinaire prior that does not allow for learning), but from a failure to attend to important details of the data.[10] Summarizing features of the data that the farmer had seen but failed to notice can thus be useful.

The value of summaries relies on the farmer facing costs of paying attention and not attending to some dimensions on his own, as Proposition 1 demonstrates that the farmer will otherwise learn to optimize independent of receiving a summary. In fact, combining the two propositions yields the simple corollary that the farmer will react to the summary only along dimensions he does not attend to in its absence. The model may thus rationalize certain extension or consulting activities that do not help agents collect new data, but rather record and highlight relationships in these data. For the purpose of the main empirical exercise, we have the following predictions:

PREDICTION P4. Agents may fail to optimize along neglected dimensions, even though they are generating the data that would allow them to optimize.

10. The stark finding of Proposition 2—that any failure to learn stems solely from failing to extract information from available data, rather than from a lack of exposure—relies on the richness of the environment, for example, that there is a continuum of plots. The key point is more general: given the data that he generates, the inattentive farmer could benefit from processing those data differently when he did not attend to important input dimensions when forming beliefs.

Prediction P5. Summaries generated from the agents' own data can change their behavior.

II.C. Comparison with Alternative Approaches

We briefly contrast our model with alternative approaches. First, a model in which an agent exogenously fails to optimize along certain dimensions would be simpler, but would problematically be consistent with every kind of learning failure. In contrast, our model allows for testable predictions about when an agent will fail to optimize, for example, when he cannot answer questions about what he does. Second, a more extreme form of not noticing—unawareness—could also produce failures to learn, where unawareness means that an agent does not even realize that a dimension exists. Conversely, our model predicts that an agent may persistently fail to optimize along important dimensions he is aware of when a technology is prior incongruent—failures to learn come from not appreciating the importance of variables instead of neglecting their existence entirely. For example, in contrast to models of unawareness, doctors were dismissive of practices like hand-washing long after it was hypothesized that there could be empirical relationships between doctors washing their hands and outcomes like maternal deaths (Nuland 2004). The germ theory of disease was important for getting doctors to appreciate such relationships. Third, the predictions of our model are distinct from those of "bandit models" (Gittins 1979), models of self-confirming equilibria (Fudenberg and Levine 1993), or "local" models of learning by doing (Conley and Udry 2010). Although such models also allow for persistent failures to optimize, in those models a lack of data explains learning failures. When the binding constraint is instead a failure to notice, the main bottleneck is not one of data collection but one of data processing.

Finally, models of "rational inattention" (e.g., Sims 2003; Gabaix 2013) also model attentional costs, but further assume a form of rational expectations in which agents know what is worth attending to, rather than having to learn what to attend to through experience. This assumption implies that the size of learning failures resulting from inattention is bound by attentional costs. In contrast, Proposition 1 shows that potential losses from not noticing are unboundedly large in our model: our model can shed light on big deviations from optimality.

II.D. Applying the Model

The analysis suggests an exercise with the following steps: (i) find a multidimensional setting with experienced agents, (ii) predict or measure what agents attend to, (iii) assess whether agents are optimizing, (iv) assess whether agents could achieve a higher payoff given data available to them, and (v) conduct an "attentional intervention."

For (i), we prefer a setting in which the first-order reason behind any incomplete learning is compellingly a failure to notice rather than a lack of available information. Situations with experienced farmers, using mostly old technologies (e.g., fertilizer) fits this description; situations in which a new technology has just been introduced (e.g., hybrid seeds in the 1920s) may be more fruitfully analyzed through the lens of more traditional models of technology adoption and use.

For (ii), we would like to collect data that can be used to predict or measure which dimensions of the production process agents do or do not pay attention to. Such data can include survey responses detailing agents' beliefs about how they have set inputs along various production dimensions in the past (allowing agents to reveal their knowledge on those inputs), their beliefs about what constitutes best practices, and data on how they actually set inputs.

For (iii) and (iv), we want to collect or generate data that can be used to analyze whether agents are optimizing given data available to them, and whether they do a poorer job optimizing dimensions they appear not to pay attention to. To perform this exercise, Proposition 2 suggests that it may suffice to analyze data that the agent generates herself.

For (v), we want to perform an intervention that involves presenting information in a way that helps agents learn relationships that they would not learn on their own, and examine whether the intervention affects agents' behavior and resulting payoffs. From Proposition 2, such an intervention can involve providing a summary of how different input choices along unattended-to dimensions (identified by (ii)) have empirically influenced the payoff-relevant output, given data agents in principle have available to them.

The seaweed experiment, detailed below, follows the steps laid out above and tests Predictions P1–P5. The model generates testable predictions beyond Predictions P1–P5 that can be explored by following additional exercises. Section V details some of these predictions and exercises.

III. The Seaweed Experiment

III.A. Setting

Our experiment takes place with seaweed farmers in the Nusa Penida district in Indonesia. Seaweed farming exhibits key features of an ideal setting discussed in Section II.D: it involves experienced agents who have had many opportunities to learn—they have grown seaweed since the early 1980s, with many crop cycles in each year—but where the production technology involves many dimensions.

Most farmers in the area that we study follow what is called “the bottom method”: in each plot, a farmer drives wooden stakes in the shallow bottom of the ocean, and then attaches lines across the stakes. He takes raw seaweed from the last harvest and cuts it into pods.[11] A farmer then plants these pods at a given interval on the lines. After planting, farmers tend their crops (remove debris, etc.). About 35 to 40 days later, they harvest the seaweed, dry it, and then sell it to local buyers.

While seemingly straightforward, this process requires decisions on many different dimensions along the way, ranging from whether to use the bottom method or other methods, how long to wait before harvesting, and even where and how to dry the seaweed. We focus primarily on three dimensions. We explore the farmers’ decisions on the distance between lines and distance between pods, which influence how much sunlight and nutrients the pods have access to, as well as the degree to which they are exposed to waves. Additionally, we look at the size of the pods the farmers plant, which may influence the growth of the pods for numerous reasons; for example, bigger seedlings may result in higher yields in still water, but may be more likely to break (or be lost completely) in ocean locations that face significant waves.

Seaweed farming shares essential features with farming other crop types, where the many different decisions over

11. Most farmers initally grew a variety called spinosum, but some have moved to a different strain called cottonii due to buyer advice as well as government and nongovernmental extension programs.

inputs add up to determine yields, making it plausible that insights from our study could generalize. Prior to our study, there were already indications that some farmers may not have been optimizing, as their methods differed from local extension officers' recommended practices.

III.B. Experimental Design

From June 2007 to December 2007, we administered a survey that we use to elicit which dimensions of the seaweed production process the farmers pay attention to (see Appendix Figure A.I for the project timeline). From a census of about 2,706 farmers located in 7 villages (24 hamlets) commissioned by us in 2006, we drew a random sample of 500 farmers for the baseline survey, stratified by hamlet. Out of these, 489 were located and participated in the baseline survey (see Appendix Figure A.II). The baseline survey consisted of two parts: (i) a questionnaire that covered demographics, income, and farming methods; and (ii) a "show and tell" where the enumerators visited the farmers' plots to measure and document actual farming methods (see Section III.C for the data description).

From the list of farmers who participated in the baseline survey, we randomly selected 117 to participate in an experimental trial to determine the optimal pod size for one of their plots (stratified by hamlet), as well as the optimal distance between pods for a subset of those farmers. This trial allows us to analyze whether farmers are optimizing, and whether they do a poorer job optimizing dimensions they seem not to pay attention to. The trials occurred between July 2007 and March 2008, shortly after the baseline was conducted for each farmer. Each farmer was told that, with his assistance, enumerators would vary the seaweed production methods across 10 lines within one of his plots, and that he would be presented with the results afterward. All of the farmers we approached participated in the trials and were compensated for doing so in two ways. First, we provided the necessary inputs for planting the lines and guaranteed a given income from each line so that the farmers would at least break even. Second, we provided a small gift (worth $1) to each farmer to account for his time.

Participating farmers were randomly assigned into one of two treatments: sort (65 farmers) and weight (52 farmers). The sort treatment was built around the idea that each farmer had substantial variation in pod size within his own plot (see Figure I

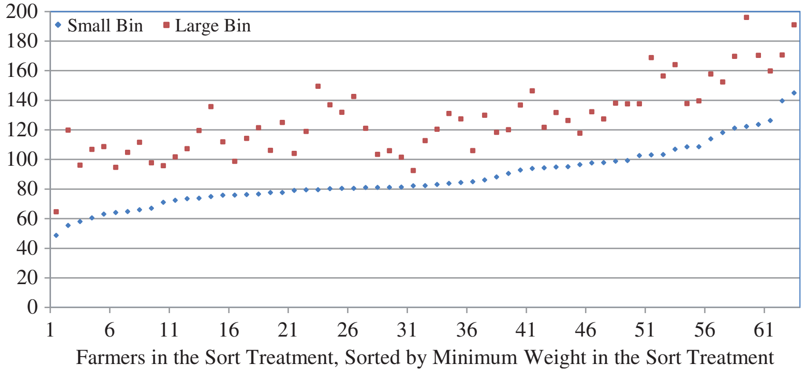


FIGURE I

Within-Farmer Variation in Baseline Pod Sizes

This figure documents the within-farmer variation in baseline pod sizes. For each farmer, it shows the average size within the small and large bins in the sort treatment, where farmers are sorted according to the average size in their small bin.

for the distribution of sizes within farmers' own plots in this treatment). Given this variation, we wanted to understand whether a farmer could achieve higher yields by systematically using a specific size within the support of those he already used. Each farmer was asked to cut pods as he usually would and then the pods were sorted into three groups. Working with the farmer, the enumerators attached the pods into the lines by group (three lines with small pods, four lines with medium pods, and three lines with large pods). The lines were then planted in the farmer's plot.

Despite the wide range of pod sizes used within a given farmer's plot, it is still possible that he could do better by moving to a size outside that range. The weight treatment was designed to address this issue by testing a broader set of initial pod sizes. To generate variation, the pod weights were initially set at 40 g to 140 g (in intervals of 20 g) for the first few trials. However, to better reflect the ranges of weights used by the farmers, the weights were changed to 90—180 g for spinosum and 90—210 g for cottonii (both in intervals of 30 g).[12] The pods of different sizes

12. We ensured that the actual weights of the pods that were planted were within 10 g of the target weight, so it is best to think of these as bins around these averages. This adds some noise to the weight categories, biasing us against finding different effects by weight.

were randomly distributed across about 10 lines of each farmer's plot, with the enumerator recording the placement of each pod. The farmers were present for the trials and saw where each pod was planted on the lines.

In the weight treatment, we also tested whether farmers optimized distance between pods. We compared two distances, 15 cm and 20 cm, since the average distance between pods at baseline was around 15 cm and past technical assistance programs had suggested larger spacing.

All farmers were told to normally maintain their plots. The enumerators returned to reweigh the seedlings twice while in the sea: once around day 14 and again around day 28. Around day 35, the seaweed was harvested and weighed for the last time. Farmers had access to all of the raw data generated by the trials: They saw or helped with planting, weighing, harvesting, and recording the results.

From April to May 2008, we conducted the first follow-up surveys, which were designed to test whether farmers changed any of their methods as a result of trial participation. These changes would have happened in the cycle after the trial: farmers would have had time to incorporate anything they learned on their own from the trial into the next cycle. Surveys were conducted with a subsample of 232 farmers, which included all of the farmers who participated in the trials, as well as an additional set of farmers who were randomly selected from the baseline as a control group; 231 farmers completed the survey.

From May to June 2008, the enumerators gave each farmer a summary table that provided information on his returns from different methods and highlighted which method yielded the highest return on his plot.[13] The enumerators talked through the results with each farmer, describing the average pod size that he typically used and the difference between that size and his optimal one. (Note that the optimal pod size for the sort trials was defined as the average size of pods in the optimal group—small, medium, or large.) Each farmer in the weight treatment was also told whether his optimal distance between pods was 15 cm or 20 cm. Appendix Figure A.III provides examples of summary tables.

About two months after we gave the results to the farmers (July–August 2008), we conducted a second follow-up survey to

13. We worked with a local nongovernmental organization (NGO) to design a simple, easy-to-understand table to summarize the trial results.

learn if they changed their methods as a result of having received their trial results, allowing us to examine whether presenting farmers with summaries generated from data they had already seen affects their behavior. Out of the original 232 farmers, 221 were found.

III.C. Data, Sample Statistics, and Randomization Check

1. Data. Baseline survey: The baseline consisted of two parts. First, we presented each farmer with a questionnaire to collect detailed information on demographics (e.g., household size, education), income, farming experience, and current farming practices—labor costs, capital inputs, technologies employed, difference in methods based on seasonality and plot location, crop yields, and so on—as well as beliefs on optimal farming methods. We additionally collected data on both "learning" and "experimentation." The learning questions focused on issues such as where the farmer gains his knowledge on production methods and where he goes to learn new techniques. The experimentation questions focused on issues like whether the farmer ever experiments with different techniques and, if so, with which sorts of techniques, and whether he changes his production methods all at once or via a step-wise process.

Second, we conducted a "show and tell" to document each farmer's actual production methods. During the show and tell, we collected information on the types of lines used, the sizes of a random sample of pods, the distances between a random sample of seedlings, the distances between seaweed lines, and so forth.

Experimental trial results: we compiled data from each of the experimental trials, documenting plot locations, the placement of pods within a plot, and pod sizes at each of the weighings. Thus, we can compute the yield and the return from each pod.

Follow-up surveys: the two follow-up surveys (one conducted after the trial and the other after providing the summaries) collected information on farmers' self-reported changes in farming techniques. The surveys also measured actual changes in techniques using the show and tell module.

2. Baseline Sample Statistics and Randomization Check. Table I presents baseline demographic characteristics and baseline seaweed farming practices. Panel A illustrates that most farmers (83%) were literate. Panel B documents that, on average,

TABLE I

Baseline Demographic Characteristics and Farming Practices

	(1) Mean	(2) Std. dev.	(3) N
Panel A: demographic characteristics			
Monthly per capita expenditures (Indonesian Rph)	369,542	348,367	487
Age of HH head (years)	43.08	11.87	474
Number of assets	8.09	3.23	487
HH head is literate	0.83	0.38	480
Panel B: seaweed farming practices			
Years farming seaweed	18.36	7.15	475
Learned to farm seaweed from parents	0.50	0.50	487
Has a loan from person to whom sells seaweed	0.28	0.45	353
Number of days in previous cycle	36.74	7.75	487
Mean distance between lines at baseline (enumerator measured; cm)	15.47	1.96	486
Mean distance between pods at baseline (enumerator measured; cm)	15.20	1.47	486
Mean pod size at baseline (enumerator measured; grams)	105.74	28.72	487
Grows cottonii strand	0.34	0.47	487

Notes. This table provides sample statistics on the farmers' demographic characteristics and seaweed farming practices from the baseline survey.

the farmers had been farming seaweed for about 18 years, with about half reporting that they learned how to farm from their parents. Panel B also shows that, at baseline, the mean enumerator measured pod size was about 106 g, while both the average distance between pods and between lines was about 15 cm.

In Online Appendix Table I, we provide a randomization check across the control and both treatment groups. We test for differences across the groups on 10 baseline demographic and farming variables. As illustrated in columns (4) through (6), only 3 out of the 30 comparisons we consider are significant at the 10% level, which is consistent with chance.

IV. Experimental Results

IV.A. *Results from the Baseline Survey and the Experimental Trial*

The theoretical framework suggests that some farmers will not keep track of input dimensions that influence returns. Table II presents baseline survey responses for 489 farmers.

TABLE II
Baseline Survey Responses on Process and Practices

	(1) % Unable to provide answer	(2) Perceived mean
Panel A: self-reported current production methods		
Typical pod size (g)	86	118.11
		[57.01]
Typical length of line (cm)	2	5.05
		[1.04]
Typical distance between lines (cm)	1	16.49
		[3.14]
Panel B: self-reported optimal production methods		
Optimal pod size (g)	87	148.26
		[248.45]
Optimal distance between knots (cm)	2	15.97
		[2.84]
Optimal distance between lines (cm)	2	16.39
		[3.01]
Optimal cycle length (days)	1	37.43
		[7.14]

Notes. This table provides sample statistics on 489 farmers' responses from the baseline survey. Standard deviations are in brackets. Column (2) provides mean responses conditional on answering.

Panels A and B document self-reported current and optimal methods, respectively. Column (1) presents the percentage of farmers who were unable to provide an answer, and column (2) provides the means and standard deviations of self reports conditional on answering.

Result 1. Farmers Only Attend to Certain Dimensions of the Production Function.

Consistent with Prediction P1, a vast majority of farmers were inattentive to some input dimensions, particularly pod size. Following equation (3), we measure farmers' attention by eliciting self reports on current practices. Eighty-six percent could not provide an answer for their current pod size at baseline (Table II, Panel A, column (1)), while 87% of farmers did not even want to hazard a guess about what the optimal pod size should be (Panel B).[14] Since many farmers failed to notice key facts about

14. The enumerators reported to us that many farmers could not answer these questions, even when probed.

their pod sizes, it is perhaps unsurprising that a broad range of sizes is observed both within (see Figure I) and across (see Figure II) farmers.[15]

On the other hand, farmers were more attentive to other input dimensions. They appeared attentive to both the distance between knots that secure the pods to a line and the distance between lines. Unlike pod size, most farmers (98–99%) provided an answer for their distance between lines (Panel A); similarly many had an opinion about the optimal distance between both the knots and lines (Panel B). Given that the farmers appeared relatively more attentive to the distance between knots and lines than to pod size, we might expect that the actual distances employed would exhibit less variance than the pod sizes. Indeed, the coefficient of variation for the distance between lines (0.13) and pods (0.10) is much smaller than that for pod size (0.27), indicating that the practices for these inputs were relatively less variable across farmers.

Results 2 and 3. Farmers Are Not Optimizing, and Seem to Do a Relatively Poor Job Optimizing Unattended-to Dimensions.

Predictions P2 and P3 hold that farmers may be off the productivity frontier and that learning failures will be concentrated along dimensions they do not attend to. Given the foregoing results, this suggests that farmers will be especially far from optimizing pod size. For the 117 farmers that participated in the experimental trials, we have data on optimal pod size. Table III summarizes the predicted percentage income gain from switching to trial recommendations.[16] Panel A reports this estimate for farmers in the sort treatment, providing information on both the predicted gain a farmer could achieve by changing the size of each pod from his baseline average to his best performing size and the predicted gain by changing the size of each pod from his worst performing size to his best among sizes used at baseline.

15. Online Appendix Figures IA and IB separately present the across-farmer distribution of baseline pod sizes for cottonii and spinosum growers, respectively.

16. We do not have follow-up data on income or yields; we compute predicted changes to income based on results from the trials. To do so, we make several strong assumptions. First, we assume that past seaweed prices are consistent with the future ones, which may be unrealistic as the prices may fall if all farmers increase their yields. Second, we assume that farmers do not change other methods (have fewer cycles, harvest earlier, etc.) if their yields change. Thus, this evidence should be viewed more as suggestive, rather than causal.

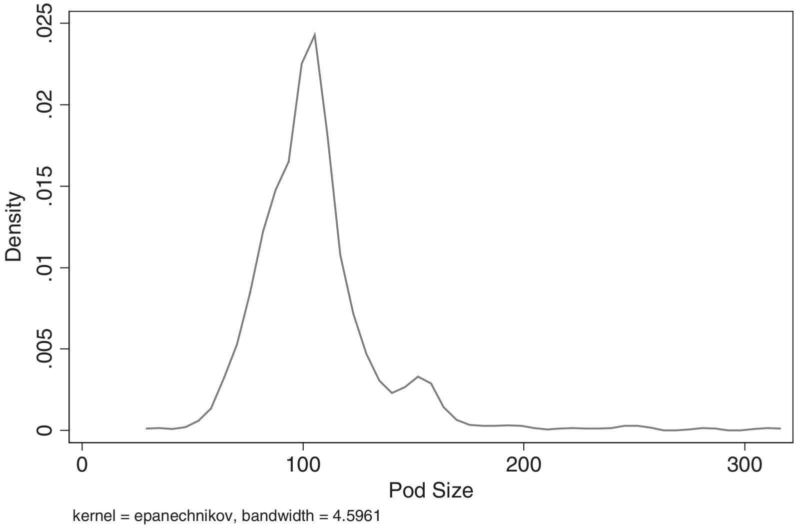


FIGURE II

Distribution of Baseline Pod Sizes (in Grams)

TABLE III

ESTIMATED PERCENT INCOME GAIN FROM SWITCHING TO TRIAL RECOMMENDATIONS

	(1) Median	(2) 95% confidence interval
Panel A: sort treatment group		
Gain to moving from average to recommendation	7.06	[2.92, 14.19]
Gain to moving from worst to recommendation	23.3	[19.00, 28.18]
p-value from F-test of equality of coefficients	.01	
Panel B: weight treatment group		
Gain to moving from average to recommendation	37.87	[23.60, 58.86]
p-value from F-test of equality of coefficients	0	

Notes. In the sort treatment, the F-tests come from separate farmer-level regressions of yield on the three size dummies (small, medium, large), where the null is that the coefficients on the dummies are equal. The F-tests in the weight treatment are constructed analogously.

Panel A also provides information on p-values from F-tests of the null that yield does not vary in pod sizes used at baseline for farmers in the sort treatment. Panel B then presents the predicted income gain a farmer could achieve by changing the size of each pod from his baseline average to his best performing size in the weight treatment, as well as information on p-values from F-tests of the null that yield does not vary in pod sizes used in this treatment. We provide the estimated median across farmers in column (1), and provide the confidence interval of the estimate in column (2).[17]

On net, the results indicate that farmers are potentially forgoing large income gains by not noticing and optimizing pod size.[18] In the sort treatment, the median estimated percentage income gain by moving from the average to the best performing size is 7.06%, whereas the median estimated gain by moving from the worst to the best is 23.3% (Panel A). In the weight treatment, the estimated gain by moving from the baseline average to the best size is 37.87% (Panel B). The potential gains are comparable to estimates of the gains to switching from the lower-yielding spinosum to the higher-yielding cottonii strain, where many farmers were induced to switch strains due to a combination of buyer advice and extension services.[19] The gains are also large when compared to Indonesia's transfer programs: Alatas et al. (2012) find that the unconditional cash transfer program is equivalent to 3.5–13% of the poor's yearly consumption, while the rice subsidy program is equivalent to 7.4%.

Given the wide heterogeneity in returns, illustrated in Online Appendix Figure II, many individual farmers could even potentially increase their incomes by much more than what is typical across the farmers. Most strikingly, the gains from the sort treatment suggest that farmers would have done much better by systematically using a specific size within the support of those they already used. This fact indicates that it is unlikely that farmers' failure to optimize purely reflects a failure of experimentation and is consistent with Prediction P4 that farmers fail to optimize given their own data when they do not attend to important dimensions.[20]

17. Given the wide heterogeneity in the results, the median is likely a better measure of what is typical than the mean.

18. In the sort treatment, about half the farmers were told that their most productive bin was their largest bin, while about 30% were told that it was their smallest. In the weight treatment, about 55% were told that they should increase their initial sizes.

19. See, for example, http://www.fao.org/docrep/x5819e/x5819e06.htm, table 6.

20. However, comparing the gains-from-switching estimates across the sort and weight treatments indicates that farmers may have done even better by

Turning to the precision of these estimates, each trial had around 300 pods per farmer, so we have a reasonable number of observations to calculate these returns. In the sort treatment, we estimated a regression of yield on size dummies (small, medium, large) for each farmer separately, where the median p-value from F-tests of the null that pod size does not matter among sizes used at baseline—that is, that the coefficients on the dummies are all equal—is .01 across farmers (Table III, Panel A). Figure III presents the distribution of these p-values across farmers. Although there is some variability across farmers in the precision with which we can reject the null that pod size does not matter among sizes used at baseline, p-values bunch in the range [0, .01]. The story is even clearer in the weight treatment, where, for every farmer, we can reject the null that pod size does not matter at a .01 significance level (Table III, Panel B). In fact, for every farmer, the p-value from the F-test is estimated at 0 up to four decimal places.

Farmers appear to perform better in setting their distance between pods—a dimension they seemed to notice at baseline. Results from the weight treatment indicate that for 80% of farmers, the optimal distance between pods was 15 cm. Given that most farmers were at 15 cm to begin with, these data suggest that very few farmers would do better by changing to 20 cm.[21]

Overall, the findings suggest that many farmers failed to notice pod size and were not optimizing size, while many farmers noticed distance between pods and may have been optimizing distance (at least within the support of distances that we tested). These results—consistent with Predictions P2 and P3—suggest that inattention contributes to a failure to optimize and hinders learning by doing. We next analyze responses to the trial to further examine Prediction P4 and to test Prediction P5.

IV.B. *Results Following the Experimental Trial*

The model suggests that farmers should respond more to participating in the trial plus receiving the summary than to

moving to a size outside of the support of those already used, which can be interpreted as suggesting that a lack of experimentation also contributes to a failure to optimize.

21. Also, given the apparent heterogeneity in the optimal size across farmers (Online Appendix Figure III), this suggests that there is more heterogeneity across farmers in the optimal size than the optimal distance between pods.

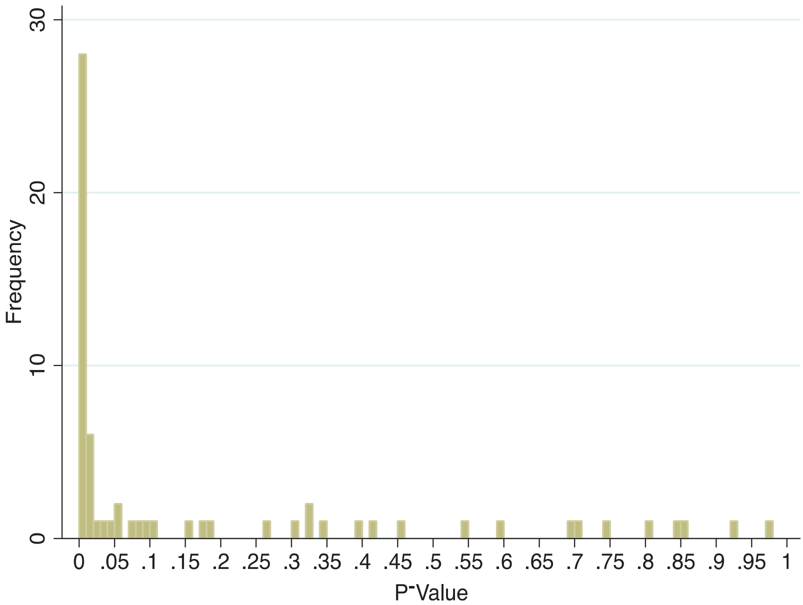


FIGURE III

Distribution of p-Values from Farmer-Level Tests of Whether Pod Size Matters in the Sort Treatment

participating in the trial by itself. In fact, consistent with Prediction P4, we may not expect trial participation by itself to have much of an effect on future behavior: farmers' own behavior generated an experiment of sorts every season—their failure to notice size created random variation in pod sizing—and the trial presents farmers with data that is similar to what they already had access to, but incompletely learned from. Following Prediction P5, farmers should be more likely to respond when they are also presented with a summary of the trials' findings, as the summary is easier to process.[22]

22. The prediction that farmers will respond more to the trial plus the summary than to the trial by itself implicitly relies in part on an assumption that simply being asked to participate in the trial does not significantly draw farmers' attention to pod size—that is, being asked to participate does not lead farmers to significantly update the probability they place on pod size mattering, $\pi_{size.}$ While similar assumptions may not hold in other contexts (e.g., Zwane et al. 2011), it appears reasonable in this context. Few farmers in other parts of Indonesia previously took up NGO advice on pod size. Indeed, this was one of the reasons we became interested in

To explore these hypotheses, we estimate the following model for each farmer i in hamlet v:

$$Y_{ivt} = \beta_0 + \beta_1 F1_t + \beta_2 F2_t + \beta_3 Trial_{iv} + \beta_4 Trial_{iv} \cdot F1_t + \beta_5 Trial_{iv} \cdot F2_t + \alpha_v + \eta_{ivt}, \tag{4}$$

where Y_{ivt} is a production choice at time t, $F1_t$ is an indicator variable that denotes the first follow-up after the experimental trial, $F2_t$ is an indicator variable that denotes the second follow-up after the summary findings were presented to farmers, and $Trial_{iv}$ is an indicator variable that denotes trial participation. We also include a hamlet fixed effect, α_v, as the randomization was stratified along this dimension.[23] There are two key parameters of interest: β_4 provides the effect of participating in the trial prior to obtaining the summary of the findings, and β_5 provides the effect after the summary is provided.

Table IV presents the regression results. In columns (1) and (2), the outcome of interest is the self-reported measure of whether the farmer has made any changes in his production techniques. Column (1) reports the coefficient estimates from equation (4), and column (2) reports the estimates from a model that additionally includes farmer fixed effects. Columns (3) and (4) replicate the analysis in the first two columns, but with the enumerator measured pod size as the outcome. We estimate all models using OLS and all standard errors are clustered by farmer.

RESULTS 4 AND 5. Farmers Do Not Respond to Simply Participating in the Experimental Trials, but React to the Summaries.

Consistent with Predictions P4 and P5, we find that simply participating in the experimental trials had little effect on farmers' subsequent production decisions, while observing the summaries was effective. We do not find a significant effect of trial

running the current experiment in the first place. In fact, very few farmers at baseline (roughly 10%) indicated that they would change their farming methods in response to an NGO or government recommendation or in response to advice from a friend (Online Appendix Table II), whereas far more farmers (roughly 40%) indicated that they would change their practices in response to results from other plots. These results suggest a hesitation among these farmers to take advice at face value.

23. The inclusion of a hamlet fixed effect does not significantly influence the results.

TABLE IV

EFFECT OF TRIAL PARTICIPATION ON SELF-REPORTED TECHNIQUES AND MEASURED POD SIZE

	(1)	(2)	(3)	(4)
	Changed farming techniques		Pod size (g)	
Trial participation	−0.084		−2.184	
	(0.051)		(3.610)	
After trial	−0.146	−0.148	−11.333	−11.661
	(0.048)***	(0.057)**	(3.003)***	(3.578)***
After summary data	−0.145	−0.150	−13.587	−13.859
	(0.050)***	(0.061)**	(2.896)***	(3.496)***
Trial participation * after trial	0.072	0.079	−2.051	−1.550
	(0.060)	(0.071)	(4.411)	(5.306)
Trial participation * after summary data	0.162	0.171	6.951	7.316
	(0.069)**	(0.084)**	(4.095)*	(4.982)
Hamlet fixed effects	X		X	
Farmer fixed effects		X		X
Observations	684	684	684	684
Test for equality of coefficients on trial participation* after trial and trial participation * after summary				
F-statistic	2.10	1.51	8.79	5.95
p-value	.148	.221	.003	.015
Mean of dependent variable for the control group				
After trial	0.10	0.10	97.68	97.68
After summary data	0.11	0.11	95.39	95.39

Notes. This table provides the coefficient estimates of the effect of treatment on farming methods after the trial (follow-up 1) and after observing the summary data (follow-up 2), conditional on baseline farming methods. The trial participation dummy indicates that the farmer belongs in either the sort or weight treatment group. Changed farming techniques includes self-reported changes in pod sizes and distances, while pod size is enumerator measured. All regressions are estimated using OLS and standard errors are clustered at the farmer level. Statistical significance is denoted by: *** $p < .01$, ** $p < .05$, * $p < .10$.

participation on self-reported changes to farming techniques, prior to when farmers received the summarized results (Table IV, column (1)). However, after receiving the summaries, about 16% more farmers reported changing a technique, which is about one and a half times the mean of the dependent variable (column (1)). Adding farmer fixed effects does not significantly alter the coefficient (column (2)). Note, however, that a test comparing β_4 and β_5 fails to reject the null hypothesis of equality at conventional levels (p-value = .1483 in column (1) and p-value = .2209 in column (2)). It is possible that some of the results from this self-reported measure may be driven by farmers wanting to please the enumerators after participating in the trial, though this is unlikely as the control group also received regular visits from enumerators to both survey and measure their farming practices. We next turn to the enumerator-measured results, which are less likely to suffer from this type of bias.

We do not find a significant effect of trial participation on enumerator-measured pod sizes, prior to when farmers received the summarized results (columns (3) and (4)).[24] After receiving the summaries, however, members of the treatment group increased their pod sizes by about 7 g (on average) relative to the control. This is significant at the 10% level in the basic specification (column (3)) and the positive sign is consistent with the average trial recommendation. The coefficient estimate remains roughly the same (7.3 g) when including farmer fixed effects, but the significance level falls below conventional levels (p-value = .14) due to an increase in the standard error (column (4)). Nevertheless, we reject the null that the coefficients are equal ($\beta_4 = \beta_5$) with p-values of .0033 (column (3)) and .0154 (column (4)). While farmers did not appear to attend to pod size prior to the trials, providing summary information on their optimal size seems to have changed their behavior.[25]

24. There is a statistically significant negative coefficient on "After Trial" and "After Summary Data" in columns (3) and (4), suggesting that on average, control farmers used larger pods at baseline. Common shocks to productivity could be responsible for such a trend. For example, since pods are cut from raw seaweed from the previous harvest, it is possible that common shocks led to high yields in the harvest before the baseline, which in turn led to bigger pods even if farmers did not attend to—nor precisely measure—them.

25. In Online Appendix Table III, we disaggregate the results by whether farmers were told to increase or decrease their pod size in the follow-ups. To do so, we interact the interaction of the treatment and follow-up variables with an indicator

We next separately explore the impact of participating in the sort and weight treatments. Specifically, we modify the basic model to include separate dummy variables for participating in the sort and weight treatments and interact these variables with the indicators for follow-up status. Table V presents the results. In columns (1) and (2) the outcome of interest is the self-reported measure of whether the farmer has made any changes in his production techniques, in columns (3) and (4) it is enumerator-measured pod size, and in columns (5) and (6) it is enumerator-measured distance between pods (recall that we only experimented with this distance in the weight treatment). The results in columns (1)–(4) are similar to what we found in the model that did not distinguish between the treatments: simple participation in the trial had little effect on farmers' decisions, whereas participation plus receiving the summary affected both self-reported production techniques (columns (1) and (2)) and enumerator-measured pod sizes (columns (3) and (4)), though the last effect is statistically significant only in the sort treatment.[26]

Finally, we find no effect of either simple trial participation or receiving the summaries on enumerator-measured distance between pods (columns (5) and (6)). This is consistent with the model: unlike pod size, farmers appear to have previously noticed distance, had beliefs on the optimal distance, and tended to be at the optimum (at least within the support of distances tested in the trial). As a result, we would not expect large changes in distance from either participating in the trial or receiving its results. Note, however, that although this result is consistent with the model, the insignificant result on distance could also be driven by the smaller sample size.

variable denoting whether the farmer was told to increase the pod size. We observe that farmers who were told to increase pod size did so both after the first and second follow-up. However, it is possible that this is simply capturing the fact that if farmers are randomizing with respect to pod size, those who "should go bigger" are those who had abnormally low draws of their pod sizes at baseline. Thus, in expectation, those farmers would in fact go bigger the next period even if they continue to randomize, making these results difficult to interpret.

26. We also tested whether treatment effects differed by years of seaweed farming (experience) or education, and we do not observe differences. However, one possibility is that we do not have sufficient variation in these variables or sufficient power size to adequately test for such heterogeneous treatment effects.

TABLE V

EFFECT OF TRIAL PARTICIPATION IN SORT VERSUS WEIGHT TREATMENTS

	(1)	(2)	(3)	(4)	(5)	(6)
	Changed farming techniques		Pod size (g)		Distance between pods (cm)	
Sort * after trial	0.089	0.100	3.944	4.657		
	(0.065)	(0.077)	(4.461)	(5.310)		
Weight * after trial	0.052	0.053	−9.257	−8.929	0.289	0.304
	(0.075)	(0.089)	(6.610)	(7.882)	(0.328)	(0.387)
Sort * after summary data	0.141	0.153	10.908	11.768		
	(0.075)*	(0.091)*	(4.418)**	(5.286)**		
Weight * after summary data	0.187	0.192	2.185	2.093	0.226	0.172
	(0.095)*	(0.114)*	(5.819)	(7.002)	(0.303)	(0.362)
Hamlet fixed effects	X		X		X	
Farmer fixed effects		X		X		X
Observations	684	684	684	684	499	499
Mean of dependent variable for the control group						
After trial	0.10	0.10	97.68	97.68	15.39	15.39
After summary data	0.11	0.11	95.39	95.39	15.27	15.27

Notes. This table provides the coefficient estimates of the effect of the different treatments on farming methods after the trial (follow-up 1) and after observing the summary data (follow-up 2), conditional on baseline farming methods. Changed farming techniques includes self-reported changes in pod sizes and distances, while pod size and distance between pods are enumerator measured. All regressions are estimated using OLS and standard errors are clustered at the farmer level. Statistical significance is denoted by: *** $p < .01$, ** $p < .05$, * $p < .10$.

IV.C. Alternative Explanations

Although the experimental findings track the theory, other explanations are possible. First, perhaps we missed important costs and the farmers' pod size strategies are optimal. For example, perhaps carefully cutting pods to a particular size is costly in terms of labor effort. However, if farmers believed that they were at the optimum, there would be no reason for them to react to the treatment. Their reactions suggest that they felt that they were not opimizing based on the trial data.

A second explanation is that maybe the farmers are simply innumerate or face computational constraints. They could not learn from participating in the trial—or even from the natural data variation—because they could not perfom the neccessary calculations.[27] However, 83% of farmers are literate. The fact that they perform well on distance also suggests that numeracy is not the main constraint or it would be problematic along this dimension as well.

Third, perhaps the variation in size is too small to detect (at least without a measuring technology, e.g., a scale). This also seems unlikely: pod size variation is large, where the average range of sizes used by a given farmer in the sort treatment is 39 g, or roughly a third of the average size. This is roughly equivalent to the size of 8 nickels or 19 grapes. Such variation is likely detectable by sight or feel, especially considering the acuity required for many agricultural tasks, like applying fertilizers or pesticides.[28] Variation in yield across the range of sizes typically used is similarly large, as indicated by the large implied percentage income changes presented in Table III. Converting these numbers into percentage gains in grams, the median percentage gain to moving from the worst to the recommended size is over 30% in the sort treatment, for example. Finally, we saw that farmers do not react to the data generated by the experimental

27. Recent experimental evidence suggests that computational constraints can impede learning in laboratory games, providing another rationale for why summaries can be effective (Fudenberg and Peysakhovich 2013).

28. Indeed, research on perception indicates that people can detect much smaller differences. For example, laboratory evidence on weight perception suggests that people can detect changes that amount to at least 2% of an initial weight. As an illustration, people can accurately detect the difference between 100 g and 102 g and the difference between 200 g and 204 g (Teghtsoonian 1971).

trials, even though they were present for (and helped with) the weighing and recording of results.[29]

Perhaps the biggest stumbling block for these (and other) explanations is the link between failures to learn and self-reported knowledge. The theory makes a clear prediction: failures to learn will be centered on dimensions that individuals lack knowledge of, as in the case of pod size. Although other explanations may explain learning failures—even from experiments that people participate in—it is less clear why those failures would be linked to what is noticed. Limited attention appears to be the most plausible explanation of this key fact.

V. Extensions

V.A. Other Applications

Though the model was cast as one of farming, it applies more generally to tasks where agents learn which input choices x maximize a payoff-relevant output y. In this section, we sketch a few other applications. The goal is not to argue definitively that the model explains a particular set of stylized facts, but to highlight facts consistent with the model and suggest how future experiments or data collection exercises could test our interpretation. Note that a prerequisite to applying a model of noticing is to be more precise about the possible granular decisions that go into production. The use of fertilizer cannot be summarized by "dollars spent on fertilizer." Rather, the individual choices that need to be made, such as when it is applied, how to space applications, and so on, need to be specified. A model of noticing only matters when there are details to notice.

1. Management. To maximize product quality (y), the managerial process involves many nuanced choices (x): exactly how to monitor worker effort, what to look for to determine whether a machine needs preventive maintenance, whether to worry about

29. While farmers knew they would be presented with the summary information—which would have attenuated any incentive to attend to the relationship between pod size and yield in the raw trial data—they also knew there would be a lag in receiving the summary, meaning that they still had some incentive to attend if they thought pod size mattered, as this could allow them to improve their practices in the interim.

the cleanliness of the shop floor, and so on. Bloom et al. (2013) examine managerial choices for a set of Indian textile plants. They give free consulting advice to a randomly selected subset. The advice is interesting because it does not suggest any new physical technology, but simply helps managers think about their production processes differently. In the simplest economic frameworks, where firms optimize given available technologies (at least given sufficient experience), we would expect this type of advice to have little to no effect. Instead, Bloom et al. (2013) find a 17% increase in productivity in the treated plants.

Why does consulting help so much? This puzzle is magnified considering that much of the advice appears aimed at getting firms to act on readily available data. One piece of advice, for example, was to record defect rates by design. Another was to clean up trash from the shop floor, where floor cleanliness naturally varies over time and the relationship between cleanliness and output could have been gleaned from the plant's own data. Our interpretation is that managers could have acted on such relationships, had only they *known to look*. The managers were stuck in noticing traps.[30,31]

A simple twist on this experiment, suggested by Proposition 1, would allow for a test of this interpretation. In seaweed, the tell-tale sign was that farmers did not even know their pod size. One could similarly ask managers questions about the features suggested in the consultation, prior to providing the advice. For example, does the manager know how often the factory floor is cleaned? Our model suggests that optimization failures are concentrated on dimensions where the answer is "I don't know." This approach would also allow the analyst to predict heterogeneity of

30. The well known Toyota problem-solving system also appears to be based on the idea that managers often do not notice important relationships in existing data (Liker 2004). The "father" of the Toyota system, Taiichi Ohno, required trainees to stand in a circle drawn on the floor of a plant and observe highly routinized jobs (e.g., install a headlamp) over and over again until they could devise improvements. This is arguably all about attention: the "Ohno circles" do not encourage workers to gather new data, but to extract more information from already available data.

31. A question is why consultants could not simply tell managers what to pay attention to. Indeed, Bloom et al. (2013) find that giving recommendations is not as effective as also having implementation phases, in which some of the recommended practices are also demonstrated. Trust may explain this result if people fear deception or believe that the consultants do not understand the complexities of production. Theory suggests a role for simple communication (to influence π_j), but only when receivers trust the advice enough so that it substantially moves their beliefs.

treatment effects of the consulting advice, since plant managers may not have the same priors and thereby fail to notice different features.

2. Education. A teacher who aims to maximize student achievement (y) needs to consider many variables (x): the intensity with which he covers various subjects, the ways in which he tests students, how he interacts with students, and so on. Here also, recent studies show that even experienced teachers are not on the educational production frontier. In an evocative study by Allen et al. (2011), teachers review video recordings of their own classes, with an expert pointing out various details. This simple intervention showed large effects on measured student achievement in the year following completion of the intervention. The magnitude of the effect is equivalent to moving a student from the 50th to the 59th percentile in test scores.

Our interpretation is that this intervention pointed out relationships in the teachers' own data that they failed to notice.[32] For example, a teacher might have neglected how the precise way he handles students' questions affects their engagement. However, in the process of reviewing video tapes, experts may have also communicated new information (such as "other teachers do this"). A way to extend the intervention to test whether inattention underlies the original failure to optimize would be to first survey teachers about their own class behaviors. One could then videotape teachers both pre- and post-intervention, code their practices on various dimensions, and see whether the intervention improves their teaching on the dimensions that they appear not to attend to as measured by the pre-intervention surveys.

3. Extension. Agricultural extension services demonstrate profitable technologies on farmers' own plots to encourage adoption. Duflo, Kremer, and Robinson (2008b) demonstrate how ineffective extension services can be. They find that farmers who observed a fertilizer trial on their own plot or a neighbor's plot

32. Allen et al. themselves describe a major part of the intervention as having teachers "observe [video clips of] his or her behavior and student reactions and to respond to consultant prompts by noting the connection between the two" (2011, p. 1035).

initially showed modest increases in fertilizer use, but this did not last.[33]

In our framework, a problem of extension is that farmers may not know what to notice while watching demonstrations. We illustrate this with a simple extension of the model in Online Appendix B. The result is that farmers can (correctly) believe from the demonstration that the technology is profitable but falsely believe that they learned how to use it. Indeed, they may not notice the dimensions necessary for proper use. The result can be a pattern of adoption and decay, like that found in the Duflo, Kremer, and Robinson (2008b) study.[34] Farmers give up on using fertilizer because it does not produce the yields they thought it would. The model suggests a way of testing this mechanism: by eliciting farmers' beliefs about the demonstrator's actions along various input dimensions. The model predicts a greater decay effect when farmers do not accurately recall what the demonstrator did along essential dimensions.

4. Surgery. A final example comes from considering a surgeon who aims to maximize the postoperation health of a patient (y) through a multitude of choices (x), including her effort (e.g., how hard she concentrates, how much time she spends on the surgery), her alertness (e.g., the time of day the operation is scheduled, how many surgeries she does in a row), and how she interacts with the rest of the operating team (e.g., does she make sure everybody knows each other's name, do they discuss the case prior to operation). It is natural that she may not attend to some important factors.

The evidence suggests that introducing checklists can reduce the incidence of complications. For example, Haynes et al. (2009) find that introducing a 19-item surgical safety checklist in eight hospitals reduced the rate of death in the 30 days following noncardiac surgery from 1.5% before the checklist was introduced to 0.8% afterward, and more general inpatient complications from

33. Note that fertilizer use appears to be profitable in this setting (Duflo, Kremer, and Robinson 2008a).

34. Similar patterns of adoption then decay following demonstration trials have been found for other technologies as well, such as improved cooking stoves (Hanna, Duflo, and Greenstone 2012).

11% to 7%. Checklists surely deal with simple forgetfulness: people forget a step they know they should take. However, they may also help counteract selective inattention. Take a common checklist item: asking surgeons to ensure that all team members are introduced by name and role prior to skin incision. This can facilitate team communication, but when faced with more direct "medical" details, it is easy to imagine that surgeons undervalue and fail to notice this one. The checklist can force attention on this detail. To explore this hypothesis, it would be useful to measure surgeons' beliefs about the importance of the differing checklist items. The model suggests that benefits of the checklists in part stem from including items that surgeons may believe are less important than they turn out to be.

V.B. Predicting Learning Failures

Our approach has been to bring empirical rigor to the model by exploiting the prediction that failures to optimize should go hand in hand with measured failures to notice. This largely leaves open the question of how we might ex ante predict failures to notice and resulting failures to learn based on features of technologies or environments. In the context of seaweed farming, for example, could we have ex ante predicted that farmers would notice the distance between pods but not pod size? Here we sketch some possible ideas.

First, agents' past experiences with technologies can create blinders. Online Appendix B considers an extension of the model where the farmer sequentially uses different technologies and his beliefs about whether an input dimension is likely to be important depends on his experiences with earlier technologies. The key result is that experience has a potential cost: previous experience with a similar technology may "teach" the farmer to attend to the "wrong" things.[35] In the context of seaweed farming, for example,

35. Rogers (2003) presents an example on the introduction of tractors in northern India, taken from Carter (1994). Tractors replaced bullocks as a way to power farms and provide transportation, but these tractors typically broke down quickly. Farmers did not perform routine maintence, such as cleaning air filters and replacing oil filters. Moreover, they placed blankets over the tractors' hoods in cold weather, which can lead the engines to overheat. They did this despite a foreign consultant who advised the farmers on maintainence routines. One possibility for the farmers' persistent (mis)behavior is that they transferred knowledge from their prior experience: bullocks did not require air filters to be cleaned or oil filters to be changed, but they did need to be protected from harsh winter weather. The model

other agricultural experiences may have played a role in why farmers attend to distance between pods but not pod size: it could be that the size of a plant's seed typically does not affect yield, while the distance between seeds does.

Second, the complexity of the technology—which we equate with the the number of dimensions N—creates greater demands on noticing. Suppose technologies are drawn from some distribution where a given input j matters with independent probability $p>0$. Furthermore, suppose the agent's priors are drawn from some distribution such that the farmer believes input j is likely to be important ($\pi_j = \pi^H \in (0,1)$) with independent probability $q<1$ and likely to be unimportant ($\pi_j = \pi^L < \pi^H$) with the remaining probability. Then, the likelihood that the farmer initially believes that some important input is unlikely to matter is $[1-(pq+(1-p))^N]$, which is increasing in N and tends toward 1 as $N \to \infty$. Intuitively, the agent will miss at least one important input as the number of inputs increases.

Third, greater noise in the relationship between an input and the outcome can make the person less likely to attend to the input since any given observation is less informative about the sytematic part of the relationship. The empirical work above suggests more heterogeneity in optimal pod size than optimal distance between pods across farmers. While slightly outside the formal model that abstracts from social learning, such "noise" implies a lower value of attending to data from neighbors' plots and a greater attentional cost of learning the importance of size.[36]

Finally, the person may be more likely to attend to inputs that are naturally recorded and to relate those inputs to the outcome. Indeed, some dimensions need neither attention nor memory to recollect. For example, even at the time of harvest, the distance between pods is still easily observable. The original pod size, however, can no longer be recovered simply by looking at the lines at harvest time. The farmer would have to remember

makes the further prediction that all else equal, farmers would have been more likely to learn to properly use tractors if they did not previously use a different technology, like bullocks, as a source of power.

36. Although greater noise can also matter in standard learning models since it affects the speed with which relationships can be learned (e.g., Munshi 2004), it may matter much more when people are inattentive: greater noise can completely shut down learning by making it (subjectively) not worthwhile to attend.

the pod's size from the start of the season to relate it to the outcome.

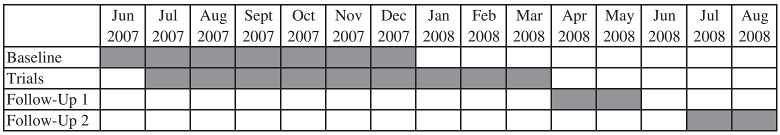
VI. Conclusion

In this article, we propose an alternative hypothesis for learning failures: they stem not only from insufficient data but from people insufficiently attending to key features of the data that they possess. This perspective has important implications for how we think about the role of experience in learning, particularly challenging the standard intuition that experience guarantees effective technology use: while experience with a technology leads to improved performance along noticed dimensions, it can have little effect along other dimensions that are important but neglected. Experience with a related technology can even have detrimental effects on learning when the input dimensions that are important across the technologies fail to overlap. The model similarly provides insights into educational interventions, suggesting they are useful not only for new technologies but also for existing technologies when there are indications that people are insufficiently attentive to key aspects of production. It also suggests ways of improving these interventions: there can be large benefits from moving away from just providing more data to helping individuals understand the relationships in the data they already have.

We test the model in the context of seaweed farming, showing that the farmers fail to optimize along input dimensions they do not notice, but that helping them "see" relationships along those dimensions affects their input choices. Looking forward, the model provides a framework for future empirical work in this area. It highlights the benefits of studies aimed at better understanding what predicts inattention to some dimensions while suggesting factors to look for, including previous experience with incongruent technologies. Similarly, research could further explore what gets people to start noticing important dimensions, for example, the conditions under which communication between asymmetrically informed parties leads to better outcomes.

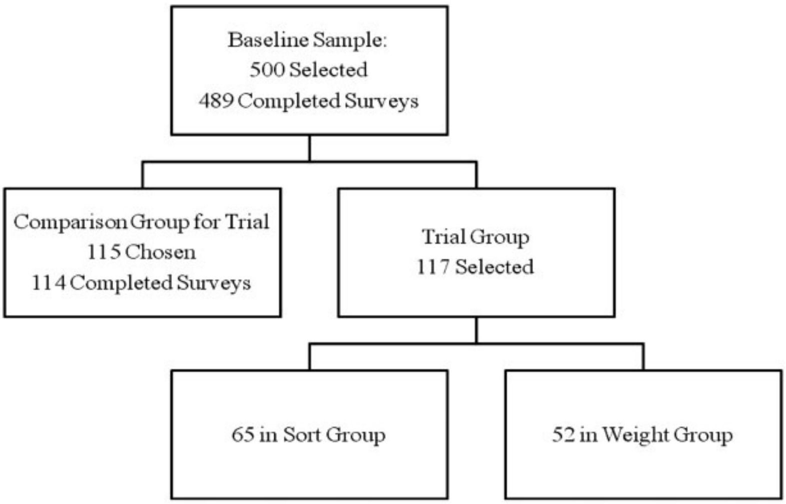
At the broadest level, the article suggests a more nuanced view of human capital. Human capital is not summarized by exposure to data or experience: embodied in individuals is information about what to notice and what to neglect.

APPENDIX: ADDITIONAL FIGURES



APPENDIX FIGURE A.I

Experimental Design



APPENDIX FIGURE A.II

Sample Design

A *Weight Example*

Pod Size	Distance	#Pods per line	Initial investment	Return per line
40	15	33	1650	4510
40	20	26	1300	3553
60	15	33	2310	1517
60	20	26	1820	1195
80	15	33	2970	1871
80	20	26	2340	1474
100	15	33	3630	1904
100	20	26	2860	1500
120	15	33	4290	597
120	20	26	3380	470
140	15	33	4950	1574
140	20	26	3900	1240

Currently

Pod Weight: 152.5	**Distance: 15**

Recommendation:

Pod Weight: 40	**Distance: 15**

APPENDIX FIGURE A.III

Examples of Summary Tables

B *Sort Example*

Line Type	Distance	Average Pod Weight (g)	Return per pod (g)	Average pods per line	Return per line (g)
Large		129.92	167.65		5716.76
Medium	14	98.34	155.51	34.1	5302.92
Small		86.18	158.82		5415.88

Recommendation	Switch to **large** pod size, with average weight **129.92**g.

Appendix Figure A.III

(Continued)

Harvard University, NBER, and BREAD
Harvard University and BREAD
Dartmouth College

Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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