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Does a Theory-of-Value Add Value? Evidence from a Randomized Control Trial with Tanzanian Entrepreneurs

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Abstract. Entrepreneurs making decisions under uncertainty are encouraged to evaluate their initial ideas through hypothesis testing, but entrepreneurial approaches vary in their emphasis on ex-ante theory development prior to collecting evidence. In this paper, we examine whether and how entrepreneurs benefit from adopting an evidence-based approach or a theory-and-evidence-based approach to decision making. We conducted a field experiment with Tanzanian agribusiness entrepreneurs by randomly assigning entrepreneurs to two different training conditions. We find that entrepreneurs in the theory-and-evidence–based condition have higher economic performance during the observation period following the intervention. We conjecture this result stems from differences in the types of changes enacted: entrepreneurs in the theory-and-evidence–based training make more coordinated changes that encompass both core and operational elements of their business models.

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Keywords: entrepreneurship • experimentation • scientific approach • strategy formation • theory-based view • developing contexts

Introduction

Developing novel products, services, or businesses requires decision making under uncertainty wherein entrepreneurs confront an incomplete understanding of customer preferences and the competitive landscape. To mitigate such uncertainty, entrepreneurs often rely on experimentation to shape their business ideas and strategies (Murray and Tripsas 2004, Kerr et al. 2014, Gans et al. 2019). The scholarly and practitioner literature provides two distinct approaches to such decision making that emphasize experimentation. An *evidence-based* approach to decision making—popularized as the lean startup methodology (Ries 2011, Blank 2013)—emphasizes gathering evidence through interactions with customers and prototypes to assess the validity of the entrepreneur's business model and pivoting based on the results of frequent experiments (Contigiani and Levinthal 2019, Leatherbee and Katila 2020, Blank and Eckhardt 2024). A *theory-and-evidence–based* approach—exemplified by the scientific approach to entrepreneurship (Camuffo et al. 2020)—emphasizes developing a unique "theory-of-value" to formally specify a set of attributes and

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logical conjectures of the business idea, conduct experiments regarding the theory's assumptions and linkages, and evaluate evidence to inform pivots (Felin and Zenger 2017, Felin et al. 2020, Ehrig and Schmidt 2022, Valentine et al. 2024). Both approaches have witnessed an uptick in their practical applications and have been embedded in entrepreneurial training programs. However, two important gaps remain.

First, despite efforts at gathering robust evidence on the value of each approach individually (Camuffo et al. 2020, Leatherbee and Katila 2020, Koning et al. 2022, Novelli and Spina 2024), we lack studies that directly compare the two approaches. Also, studies of the theory-and-evidence–based approach (Camuffo et al. 2020, Novelli and Spina 2024) do not formally disentangle the value-add of explicating a formal theory-of-value from the value-add of experimentation. This is important to do, inasmuch as both approaches recommend that entrepreneurs gather and evaluate evidence through experiments prior to full-scale commitment of resources but differ on their prescriptions for development of a unique theory-of-value prior to experimentation. This difference may have critical implications for how entrepreneurs engage in learning to overcome uncertainty and make key strategic decisions. Thus, an open question remains: *What is the value of developing a theory-of-value to guide experimentation?*

Second, the adoption of either approach and efforts to identify their effects for entrepreneurs are largely concentrated in developed economy and nonagrarian settings. Salient here is the inattention to developing economy settings, despite entrepreneurship being recognized as an important engine of economic growth (Agarwal et al. 2007, Mair et al. 2012) and efforts by government agencies and nonprofit foundations to promote it in developing economies (Schneider 1997, Hughes and Lonie 2007, Mair et al. 2012). The thrust of these efforts, however, have been on developing market infrastructure by addressing institutional voids or business capabilities by providing basic managerial practices and business operations training (Dutt et al. 2016, McKenzie and Woodruff 2017). Arguably, the need for evidence-based or theory-and-evidence–based entrepreneurial training programs may be just as, if not more acute, in developing economies and their predominantly agricultural sector settings. But, with very few exceptions (Carlson and Hager 2022), there is a dearth of evidence on whether the implementation and benefits of either approach is generalizable to these contexts.

To discern the effects of an entrepreneur having a theory-of-value and to empirically adjudicate across evidence-based and theory-and-evidence–based approaches, we designed a field experiment within an entrepreneurship training program offered to entrepreneurs in Tanzania with the support of two nonprofit foundations interested in improving agricultural entrepreneurship in developing economies. We randomly assigned 151 entrepreneurs to two different training conditions. Consistent with the above description, the evidence-based approach emphasized quick validation of business model assumptions and hypotheses through customer interviews, surveys, and pilot tests of a minimum viable product (MVP). The theory-and-evidence–based approach encouraged entrepreneurs to develop a theory-of-value that explains the intuition for the business idea using a logic of cause and effect and emphasizes linkages across key attributes before engaging in hypothesis testing and prototype development. For both conditions, the training took place over six half-day in-person sessions on a biweekly basis for over three months. We gathered extensive data through surveys and interviews of the entrepreneurs over 15 months: This period spanned pre- and posttraining across six distinct data points and provided detailed information on their decision-making activities and outcomes. We focused on three key outcomes: the decision to terminate the business, firm performance (e.g., revenue and profit), and changes entrepreneurs made to their business models over time. In addition, we explored patterns in the data to identify potential underlying mechanisms related to *how* entrepreneurial decisions differ under the two conditions.

Our preregistered research design allowed us to isolate the role of theory building, the primary difference between the two training conditions. If all that matters is learning through experimentation—an element common to both trainings—we should see no differences across the two conditions in terms of termination rates or performance outcomes. Our results are consistent with this assertion in that they show no significant difference across the two approaches in terms of project termination rates. Thus, both approaches seem to reveal valuable information on the viability of the idea in terms of willingness to move forward. However, our evidence suggests that the two approaches lead to statistically significant and economically meaningful differences in terms of economic performance. At the end of our observation window, projects developed by entrepreneurs in the theory-and-evidence–based condition, on average, outperform those in the evidence-based condition in terms of revenues and profits, indicating that developing a theory-of-value seems to add value. Digging deeper into how a theory-of-value adds value, our analysis suggests a potential mechanism related to the type of changes entrepreneurs make to their business models over time. Although entrepreneurs in both training conditions introduced frequent changes (pivots) to their business models, relative to the control group, entrepreneurs in the theory-and-evidence–based training made more coordinated changes that involved both *core* and *operational* elements of the business model, indicating

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that they may have developed an understanding of interlinkages between core and operational decisions.

Our study contributes to the literature by addressing both gaps that motivated the study. Our theoretical contribution is in showcasing the value of having a theory-of-value to guide experimentation and by identifying a plausible underlying mechanism. Our paper suggests that relative to an evidence-based approach, a theory-and-evidence–based approach nudges entrepreneurs to create a more holistic representation of the strategic problem they encounter, which informs their experimentation efforts and enables them to develop their projects in a more integrated way. These differences have direct implications for entrepreneurial outcomes.

A second important contribution is in extending the application of entrepreneurial approaches beyond primarily Western (developed) settings. Our study uses a context—the Tanzanian agribusiness sector—which is often underrepresented in the strategy literature despite the economic importance of agriculture within developing country settings (George et al. 2016a, Foo et al. 2020). We join an emerging yet underdeveloped stream of research that seeks to understand the effects of offering strategy-making trainings to entrepreneurs in developing countries (Carlson and Hager 2022, Dimitriadis and Koning 2022, Bruton et al. 2024) as opposed to business incubation through market infrastructure or basic business capability development (Mair et al. 2012, Dutt et al. 2016, McKenzie and Woodruff 2017). We find that despite the incurring of upfront cognitive costs, the applicability of the theory-and evidence–based approach transcends context and can be used fruitfully by developing world entrepreneurs. Thus, our results should also be of interest to practitioners and policymakers, because they highlight the importance of such training to help entrepreneurs develop their business projects, in addition to trainings typically centered on managerial and business practices (Dutt et al. 2016, McKenzie and Woodruff 2017).

Conceptual Background

Two approaches emphasizing experimentation have been developed in the last two decades to help entrepreneurs make better decisions and mitigate uncertainty when bringing their projects to fruition. Each approach creates greater formality in evidence gathering by emphasizing experimentation. The evidence-based approach encourages entrepreneurs to experiment through prototypes that represent underlying beliefs of value propositions, offer them to customers and pivot based on the feedback received (Ries 2011, Blank 2013, Contigiani and Levinthal 2019, Leatherbee and Katila 2020, Shepherd and Gruber 2021). The theory-and-evidence–based approach additionally encourages entrepreneurs to formalize underlying beliefs (Valentine et al. 2024) by developing a unique theory-of-value that identifies key attributes of the decision problem and their logical links and then conduct experiments aimed at testing conjectures about these relationships and refine the theory-of-value (Felin and Zenger 2009, Camuffo et al. 2020, Ehrig and Schmidt 2022). Both approaches are similar as each recommends entrepreneurs test their beliefs against key audiences and iterate across three distinct stages of action to reduce uncertainty prior to the commitment decision (Zellweger and Zenger 2023). These stages include (a) forming beliefs about the business idea, (b) taking action to test those beliefs, and (c) updating beliefs based on new information emerging from experimentation. Although both approaches emphasize the importance of hypothesis testing and gathering evidence, they privilege different processes, summarized in Table 1. We next provide the conceptual distinctions in these processes and illustrate them with a hypothetical example of an entrepreneur from Dar es Salaam who observes that urban-dwelling Tanzanians spend 20%–30% of their monthly income on charcoal for cooking fuel, despite charcoal's negative effects on respiratory health and the environment. The entrepreneur has the idea of offering liquid petroleum gas (LPG) as an alternative fuel source to Tanzanians living in urban areas.[1]

Evidence-Based Approach to Strategic Decision Making Under Uncertainty

An evidence-based approach to decision making utilizes experiments to test falsifiable hypotheses. In the belief formation stage, the entrepreneur translates assumptions about the business project into falsifiable hypotheses that are to be rapidly tested to learn about individual elements of the business model (Murray and Tripsas 2004, Leatherbee and Katila 2020). As exemplified by the popular Lean Startup method (Ries 2011, Blank 2013, Shepherd and Gruber 2021), the belief formation stage consists of using a business model canvas (BMC, or its Lean version) to articulate beliefs about the business idea (Osterwalder and Pigneur 2010, Ries 2011, Maurya 2012). These beliefs are the basis of hypotheses that may be developed for every aspect of the business model. The approach prioritizes customer discovery and validation over technology/solution design and validation (Blank 2013). It additionally recommends entrepreneurs rank-order hypotheses in terms of information available and importance of the hypothesis for the project under consideration (Ries 2011).

In the example of LPG as an alternative fuel source to charcoal, during the belief formation stage, the entrepreneur completes a lean BMC to articulate the business idea via the nine key boxes (e.g., customer, value proposition; see Figure 1(a) for a template). At this stage, each box of the BMC represents an assumption

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Table 1. Comparison of *Evidence-Based* and *Theory-and-Evidence–Based* Approaches

Approach stage	Evidence-based	Theory-and-evidence-based
Belief formation	The entrepreneur's assumptions or intuition form the basis of the business idea and can be articulated into component parts through standardized canvases and tools (e.g., Business Model Canvas or Lean Canvas templates). Hypotheses are formulated from these assumptions and can focus on every aspect of the proposed business model.	The entrepreneur begins from an observed problem or phenomenon, from which the entrepreneur develops a "theory-of-value" that forms a holistic representation of how and why the business idea creates value. This theory-of-value articulates causal relationships and logical links between attributes (e.g., unsolved problems or needs and possible solutions) and develops conjectures linking attributes with future states of the world associated with success. Hypotheses are formulated from the theory-of-value to test the accuracy of its key assumptions (i.e., key attributes and their logical links) and alternative explanations.
Belief testing	Emphasis on quick and iterative experimentation with readily available samples (e.g., early adopters). Testing occurs empirically via interaction with prospective customers and the creation of low-cost prototypes (e.g., minimum viable products) that enable rapid inexpensive feedback.	Emphasis on targeted experimentation, with tests focused on specific parameters of the theory-of-value–attributes and logical links–and with samples representative of target customers. Testing occurs empirically via interaction with prospective customers and the creation of low-cost prototypes.
Belief updating	The entrepreneur uses test results as the main source of information for subsequent decisions (e.g., pursue; pivot; terminate).	The entrepreneur assesses evidence from tests using theory-informed metrics and thresholds. The theory-of-value serves as a roadmap to make inferences from experiments and adjust beliefs accordingly (e.g., adjust elements of the theory-of-value or identify counter-theories and conduct new tests; terminate; continue with the validated theory-of-value).

(belief) to be tested. For example, one key belief to be tested is whether charcoal is indeed the primary fuel used by urban-dwelling Tanzanians (existing alternatives), and another is whether households are aware of charcoal's negative effects on health (problem statement). In terms of customer segments, the entrepreneur is interested in discovering who is the target customer (e.g., families, singles, women) and the pain points of using charcoal as cooking fuel. Each of these beliefs embedded in the BMC are transformed into falsifiable hypotheses that include the specific action associated with testing the belief and an expected measurable outcome.

In the belief testing stage, hypotheses are probed or formally tested through data collected to verify or falsify them. The emphasis here is on quick and iterative experimentation that can be conducted with readily available samples of customers, known as "early adopters" (Eisenmann et al. 2012). Entrepreneurs are encouraged to start conducting experiments as soon as possible to validate the customer need, the market segment and size, the appropriateness and feasibility of the product, service, or technology (problem-solution fit), and more generally, all the hypotheses related to the business model. This testing occurs empirically through interactions with prospective customers. For example, entrepreneurs can use customer interviews and A/B tests to efficiently gather insights about customer needs and learn about alternative solutions, useful in the early stages of a startup with limited resources (Koning et al. 2022). Low-cost prototypes also provide rapid inexpensive feedback (Thomke 2003). Once corroborated or falsified, hypotheses become validated learning, that is, knowledge on which entrepreneurs can begin further experimentation.

Returning to our example, the entrepreneur in the testing stage engages with potential customers via interviews. Such interviews enable the entrepreneur to gather feedback. For instance, the entrepreneur may rely on interviews to assess pain points related to each customer segment's typical cooking experience. Here, the entrepreneur also ensures a minimum number of interviews (e.g., at least 10) with individuals who closely match her expected early adopters (e.g., families, singles, women) of the solution. From these interviews, the entrepreneur evaluates individual hypotheses related to the customer segment and problem boxes of the BMC. For instance, the entrepreneur learns from these interviews that families and singles have different priorities and concerns while cooking. Specifically, families seek to cook meals faster and prefer to minimize clean-up after cooking, whereas singles primarily look for cheap and easy-to-prepare meals.

In the belief updating stage, test results provide the information needed to discard or change false

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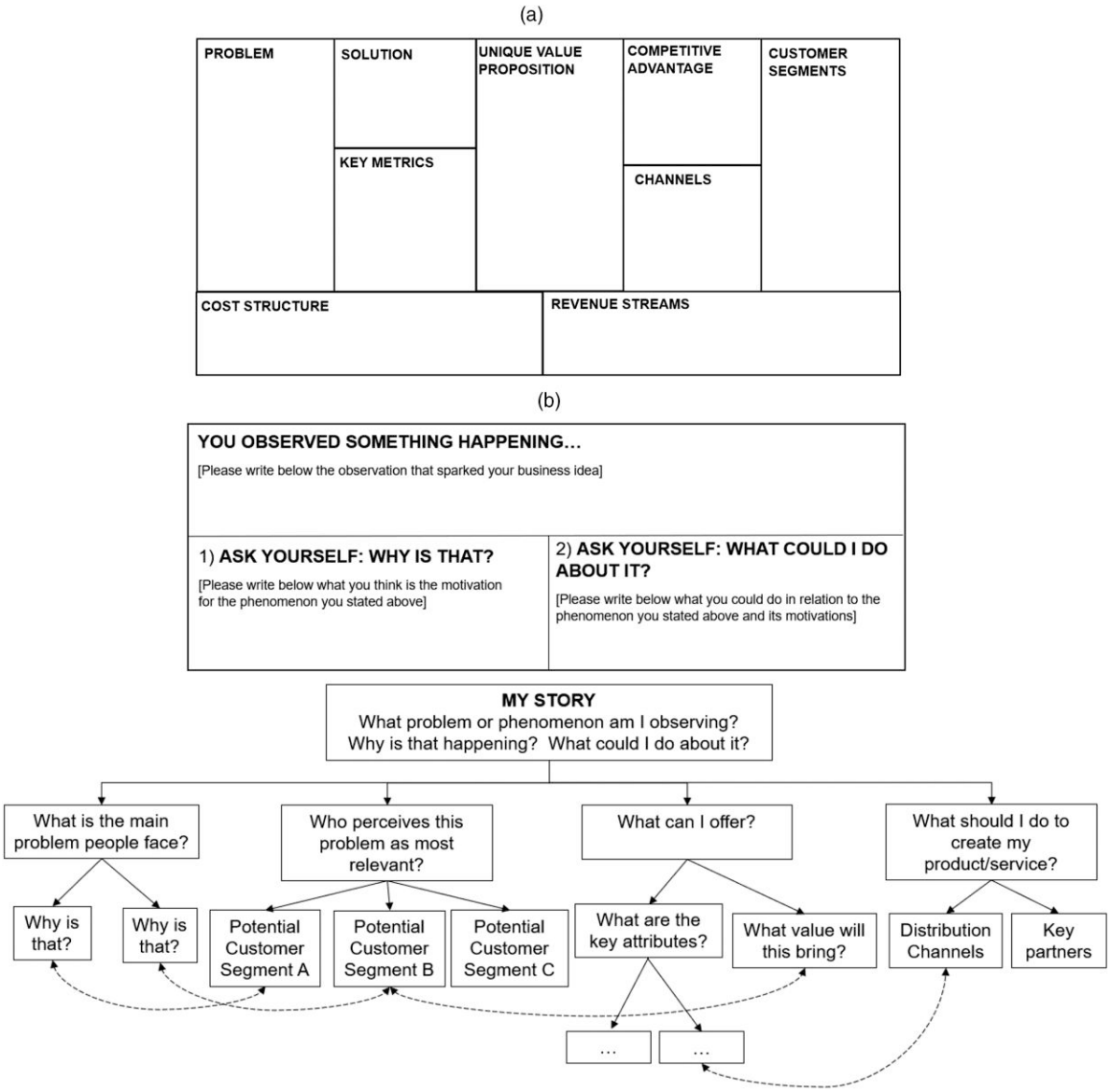
Figure 1. Templates Used in Training



Notes. (a) Lean business model canvas template (adapted from The Business Model Canvas (BusinessModelGeneration.com) and is licensed under the Creative Commons Attribution-Share Alike 3.0 Un-ported License). (b) The "story tree" template.

assumptions or confirm validated ones. The process concludes with the entrepreneur making a decision to pursue the business model, change one or more aspects of the business model, or terminate the project. The stronger the evidence against the tested hypotheses, the more likely entrepreneurs will terminate or modify their projects. The more consistent the evidence, the more likely entrepreneurs will continue pursuing their ideas.

In the example, the entrepreneur updates beliefs based on whether the data confirm or refute the BMC assumptions. For instance, singles are removed as viable early targets because an LPG-related solution does not solve their problem (ease of preparation), as discovered during belief-testing. Such formation-testing-updating is conducted for other elements of the BMC as well (e.g., channels, solution, etc.). In later iterations, for example, the entrepreneur offers an LPG canister with a meter as a low-cost MVP to test (say 10) urban families' reactions to using LPG for cooking. The entrepreneur tracks usage over a month through surveys that provide regular feedback and data from the meters to understand whether this solution solves the early adopters' problem (e.g., *do families report that they spend less time on household cooking and cleaning?*). Based on such an evidence-based approach, the entrepreneur learns whether the problem-solution nexus in the business idea can work and is worthy of pursuit through a full commitment of resources.

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Theory-and-Evidence-Based Approach to Strategic Decision Making Under Uncertainty

A theory-and-evidence–based approach to decision making emphasizes the importance of having a unique theory-of-value that informs each stage of the decision-making process. In the belief formation stage, the entrepreneur starts from an observed problem or phenomenon and provides both an explanation for why this phenomenon occurs and a logical link to a possible solution (Felin and Zenger 2009, Ehrig and Schmidt 2022). The entrepreneurs' "theory-of-value" thus articulates the business idea's key attributes, logical connections, and alternative explanations to form a holistic representation of how the business idea creates value (Felin and Zenger 2017, Ehrig and Schmidt 2022). In doing so, the entrepreneur develops a conceptual understanding of how the identified problem is connected to potential solutions, what value those solutions could bring to the market, and how different scenarios might lead to success (Ehrig and Schmidt 2022). The development of a theory-of-value can be visualized in the form of a "story tree" (Figure 1(b)), in which the entrepreneur's observed problem or phenomenon and its explanation is broken into subcomponents, and logical connections (e.g., causal links) between these subcomponents are generated. In explicating both the subcomponents and logical connections, the entrepreneur can identify multiple explanations as well as multiple solutions that may be possible, as grounded in the entrepreneur's observations.

In the LPG example, the entrepreneur forms three potential beliefs for why households rely on charcoal rather than LPG: Households (a) do not know that LPG is a cleaner alternative or (b) find LPG to be too expensive to use; or (c) cannot access LPG because there is no distribution system or infrastructure in place for purchasing, refilling, and disposing of LPG canisters. Moreover, each explanation implies different potential solutions that address the core issue (i.e., the theory-of-value). If the affordability explanation holds, the entrepreneur conjectures that for the customer segment with limited income, the focus needs to be on developing a solution that addresses cost concerns. If lack of infrastructure is the likely explanation, the entrepreneur conjectures that rural or suburban residents experience this challenge more than urban dwellers, so the solution has to focus on effectively bringing LPG outside of urban areas. As this example illustrates, the entrepreneur is articulating logical links between the observed phenomenon and its underlying potential explanation, as well as logical links to a possible solution. Testing logical links between key attributes of the theory-of-value often has implications for other elements of the entrepreneurs' business model. For instance, operations, channels to reach customers and associated costs will likely differ depending on whether the relevant population will be urban versus rural residents. As a result, the entrepreneur has to create coherence among the various aspects of initial theory-of-value, and the hypotheses are formulated to embody these linkages.

In the belief testing stage, entrepreneurs conduct targeted experiments that are focused on key attributes of their theory-of-value *and* their logical links. The theory-of-value and its subsequent iterations guides the entrepreneur in selecting worthwhile experiments, determining appropriate samples, and identifying key aspects to focus on during these tests (Felin and Zenger 2017). The emphasis here is on parameters for which uncertainty is high, and on using representative samples consistent with the theory-of-value. As with evidence-based approaches, testing can occur empirically through interactions with customers and low-cost prototypes.

Returning to our example, the Tanzanian entrepreneur focuses on testing a key attribute of her theory-of-value with a representative sample of individuals. In doing so, the entrepreneur also sets a threshold level that indicates when they consider each hypothesis supported.[2] When testing hypotheses to assess which explanation for the observed reliance on charcoal is most likely, a solution focused on affordability involves initial tests that survey low-income households to assess their willingness and ability to pay, whereas a solution focused on infrastructural concerns digs deeper into transportation pain-points via customer interviews. Moreover, the entrepreneur also simultaneously designs corresponding tests for developing the infrastructural ecosystem by surveying potential partners who could help resolve transportation challenges, with questions designed to uncover threshold levels of transportation costs that make the business model viable.

In the belief updating stage, entrepreneurs assess evidence against the theory-of-value using theory-informed metrics and thresholds to assess the evidence collected. The theory-of-value is a roadmap to make inferences from experiments and adjust beliefs accordingly (Camuffo et al. 2020). Importantly, entrepreneurs may learn at this stage that their initial beliefs were partially or entirely wrong. If this occurs, entrepreneurs integrate the newly acquired knowledge into their initial theory-of-value, adjusting it based on insights gained from experiments or identifying counter-theories (Ehrig and Schmidt 2022). The entrepreneur continues to iterate, updating and refining the theory-of-value and testing corresponding hypotheses, until the now-validated theory-of-value is deemed worthy of pursuit, or alternatively, the business idea is terminated.[3]

In the example above, the belief testing stage reveals that the bottleneck for LPG adoption is not the cost per se, but the ability to pay for acquiring a full cylinder of gas upfront. The Tanzanian entrepreneur accordingly updates beliefs on the affordability reason resulting in a new testable hypothesis that many Tanzanian

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households are deterred from switching to LPG for cooking because of the large upfront cost to refill gas cylinders and the additional cost of renting appropriate transport methods. The corresponding value proposition then changes to providing a convenient pay-as-you-go cooking gas solution to address the financial burden associated with upfront costs and cylinder refills. In later iterations, and after verifying through the MVP test whether the pay-as-you-go model increases its use in this population, the entrepreneur then revisits core features of her offerings to decide whether developing a fully-fledged solution incorporating more advanced (and expensive) technology such as smart meters of LPG usage is worthwhile.

Research Question and Context

The above two approaches have been developed by two distinct sets of scholars and practitioners, largely in parallel. Given the practical importance of each approach and their in-built focus on the importance of gathering evidence, there has also been significant effort expended at testing these approaches with empirical evidence, often with rigorous attention to experimental design.[4] However, important critiques have been raised as scholars seek to understand and compare the two approaches. Evidence-based approaches have been criticized for being overly focused on quick and frequent hypothesis testing and not devoting enough attention to generating a holistic understanding of the causal logic behind business experiments (Felin et al. 2020). An over-emphasis on rapid and frequent trial-and-error processes may lead entrepreneurs to inadvertently reject good ideas (Ladd 2016, Chen et al. 2024) or end up with lower-quality outcomes (Hartmann et al. 2022). Underlying these critiques is the concern that evidence-based approaches may lead entrepreneurs to focus on easily validated ideas rather than on path-breaking ones (Felin et al. 2020).

The theory-and-evidence–based approach is also not without drawbacks. Creating and relying on a theory-of-value comes at a cost. Constructing a theory-of-value entails high cognitive costs, requiring entrepreneurs to "slow down" and devote effortful attention and concentration early on (Kahneman 2011). Such cost-related concerns imply that the approach may be ill suited for entrepreneurs with limited experience or resources (Novelli and Spina 2024). Also, it may be challenging for entrepreneurs to change their theories-of-value once they create specific mental models, potentially leading to inertia and resistance to change (Barr et al. 1992, Benner and Tripsas 2012, Zuzul and Tripsas 2020). Upfront theorizing may result in entrepreneurs' overlooking "unknown unknowns" (Ehrig and Foss 2022) or overweight the importance of learning from prediction errors rather than actively shaping their future through action (Sergeeva et al. 2022). Underlying these critiques is the concern that a theory-and-evidence–based approach creates higher ex ante costs and potentially leads to overly restrictive models of reality. Further, it is unclear whether and how a focus on theory adds value.

Moreover, although scholars have made fruitful efforts to validate each approach, we lack evidence directly comparing the two. Such comparison has both theoretical and practical value. Theoretically, it contributes to "strategies-as-theories" research (Gavetti et al. 2005, Gary and Wood 2011, Eisenhardt and Bingham 2017, Carroll and Sørensen 2021) and the theory-based view of the firm by examining whether generating unique theories-of-value affects firm performance. Additionally, understanding whether having a theory-of-value matters allows us to assess its potential complementarity with entrepreneurial experimentation, another burgeoning literature stream (Kerr et al. 2014, Lindholm-Dahlstrand et al. 2019, Koning et al. 2022, Bennett and Chatterji 2023). These point to a need to disentangle the relative value of "theory" versus "experimentation" or to identify synergies between these two elements (Rindova and Kotha 2001, Murray and Tripsas 2004). Practically, examining the effects of a theory-of-value and adjudicating across evidence-based and theory-and-evidence–based approaches has important implications for how decision makers should allocate their time and resources. If there is little value-add from theorizing prior to experimentation, then entrepreneurs may conserve valuable time and cognitive effort. If, however, theorizing prior to experimentation does add value, then the benefits may justify these upfront costs. Such adjudication is critical to the design of training programs offered by accelerators/incubators, universities, nonprofits, and public agencies (Valerio et al. 2014, Cohen et al. 2019, Hallen et al. 2023).

Importantly, the empirical evidence on either approach is largely in the context of developed, high-income country contexts. It is unclear whether the approaches can and ought to generalize to low income and emerging economy contexts. Indeed, entrepreneurship studies of low-income and emerging economies remain limited, notwithstanding calls for more research (George et al. 2016a, b). Increasing financial inclusion and reducing global poverty has long been among the United Nations challenge goals (United Nations 2015, 2024). Moreover, recognizing entrepreneurship as a key engine, public agencies and nonprofit foundations devote significant resources to bolster the requisite institutional and infrastructural foundations and provide basic training to aspiring entrepreneurs on basic managerial practices and business capability development (Schneider 1997, Hughes and Lonie 2007, Mair et al. 2012, Dutt et al. 2016, McKenzie and Woodruff 2017). These efforts could be complemented by systematic training programs such as the evidence-based and theory-and-evidence–based approaches to further empower entrepreneurship as an

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engine for economic growth in developing countries. To do so, we need studies that shed light on the value-add of theory versus experimentation in contexts where proper allocation of scarce time and resources is important in terms of individual and social outcomes (Quinn and Woodruff 2019, Dimitriadis and Koning 2022).

Accordingly, we ask the following research question: *What, if any, is the value of developing a theory-of-value to guide experimentation?* We also develop a randomized control trial in Tanzania, as explicated in greater detail below.

Research Design

Setting

We embedded our field experiment within an entrepreneurship training program conducted with 151 small agribusiness entrepreneurs located across three regions in Tanzania (Morogoro, Pwani, and Dar Es Salaam). Tanzania represents one of the largest countries in East Africa and is one of the fastest growing economies in sub-Saharan Africa (The World Bank Group 2022). We chose agribusiness as it is a critical sector in developing economies. Globally, the agriculture sector employs more than one billion people, making it one of the world's largest sectors (Fitz-Koch et al. 2018, FAO 2022). In Tanzania, the agriculture sector employs 60% of its labor force and accounts for 28% of gross domestic product (GDP) (FAO 2022, The World Bank Group 2022).

Experimental Design

Our randomized field experiment compares evidence-based and theory-and-evidence–based approaches. To isolate the role of a theory-of-value that guides experimentation, we created two training conditions, described in detail below. Consistent with best practices, we preregistered the field experiment (Banerjee and Duflo 2011).[5] The coauthor team represented a cross-institutional partnership that leveraged synergies between expertise in experimental design and knowledge of the local context to design and deliver an entrepreneurship training program to agribusiness entrepreneurs.

Entrepreneurs admitted to the training attended six half-day in-person sessions held every other week from October to December 2021. All entrepreneurs received training consisting of the same number of sessions, same locations over time, and with same instructors across all sessions. The instruction was in Swahili, and training materials (e.g., slide decks) were in English, consistent with entrepreneurs' and instructors' familiarity with both languages. In total, the training lasted 24 hours and was offered free of charge to entrepreneurs. Entrepreneurs were randomly divided into smaller classrooms of about 15 participants each, with five trained instructors teaching in both an evidence-based and a theory-and-evidence–based condition.[6]

Following the training, entrepreneurs attended three events (two in-person and one online) featuring local speakers from the agribusiness sector. The last event also served as a graduation ceremony for entrepreneurs who completed the program and provided data. These events, although not part of our core intervention (i.e., no treatment was delivered during those events), served as important incentives for entrepreneurs to remain engaged over the study period. The program and data collection ended in July 2022.

The training program was designed to help entrepreneurs evaluate business ideas. Sessions 1–3 focused on problem validation (i.e., is there a commercial case for the business idea?). Sessions 4 and 5 focused on solution validation (i.e., what is the best way to solve the problem?). Session 6 recapped the course and included a final pitch session. The curricula of both training conditions had the same length and structure, including common topics taught in the same order. Entrepreneurs in both conditions learned how to validate a business idea with tools widely used in entrepreneurship courses (e.g., Lean Business Model Canvas, Customer Personas, Customer Journey), and all entrepreneurs learned how to interview/survey potential customers and build an MVP. Entrepreneurs across both conditions also received a training journal with preprinted templates of these tools.

Mirroring the Conceptual Background and Table 1, the two experimental conditions differed on the use of a theory-of-value to guide experimentation. The evidence-based condition emphasized quick validation rounds of business model assumptions and hypothesis testing through customer interviews, surveys, and MVP pilots. The theory-and-evidence–based condition taught entrepreneurs to ground all their decision-making processes, including experimentation, on a unique theory-of-value for their business ideas. All sessions included concrete examples rooted in the Tanzanian context and used the LPG case study to help participants understand and apply the specific approach. The difference between experimental conditions is illustrated by Figure 2, which visually depicts the decision-making process for each approach. Entrepreneurs were instructed to iterate within each approach to first validate the problem, and then to validate the solution. The Online Appendix, Section A, provides differences between the two conditions across the three stages highlighted in Table 1, including details on content and their application to for the LPG case study. Throughout the training, the crucial difference across the two experimental conditions is the role of a theory-of-value. For the theory-and-evidence–based condition, a theory-of-value serves as the backbone of all decision making; for the evidence-based condition, a theory-of-value is absent.

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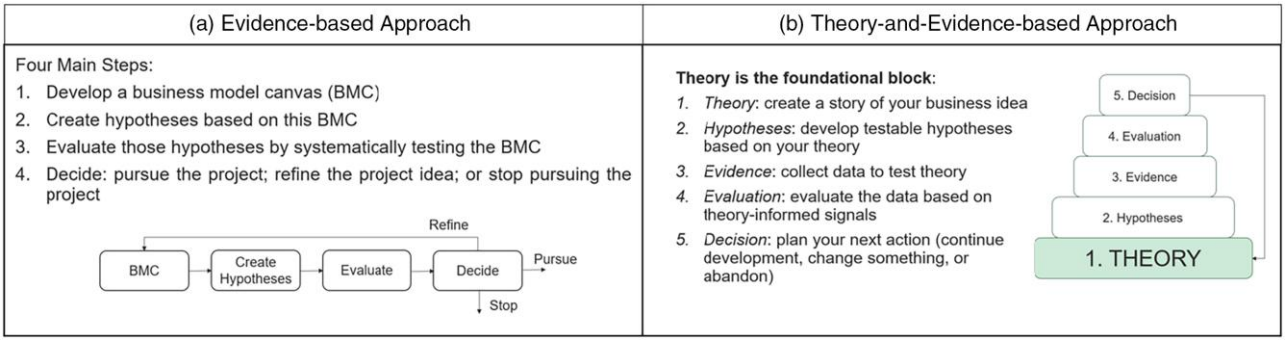
Figure 2. (Color online) Comparison of Systematic Approaches to Decision Making



Recruitment Process and Randomization

An open call for application to the training program was issued in April 2021, targeting two types of entrepreneurs: early-stage entrepreneurs developing a novel business idea (referred to as *startups*) and entrepreneurs in established companies developing a new project (referred to as *companies*). For the sake of clarity, we refer to both venture types as "projects." The call remained open for three months. Applicants completed an application survey and a phone interview. The process resulted in 202 applicants from Tanzania (130 startups and 72 companies). When filling in the application survey, a filtering question asked respondents to indicate whether the individual responding to the survey would also be attending the training program, if admitted. Respondents could only continue with the survey if they responded yes to that question. We removed 37 applicants located outside our targeted regions of Morogoro, Dar-es-Salaam, and Pwani, reducing the applicant pool to 165. We asked each applicant to state their preferred training location as either Morogoro or Dar es Salaam. Because travel between these two cities is costly and lengthy, we offered parallel training sessions in both locations to maximize attendance and reduce attrition. We offered 90 spots in Morogoro and 60 spots in Dar-es-Salaam, allocating entrepreneurs to these locations according to their stated preference. We then randomly allocated entrepreneurs within locations to different treatment conditions and assigned the remaining applicants to a control condition that received no training.[7]

After randomization, we took two steps to increase the internal validity of our study. First, we minimized the risk for contamination across treatment conditions by allocating entrepreneurs who declared knowing each other (and thus were likely to exchange training material and discuss content outside of classes) to the same treatment condition.[8] Second, we made three manual adjustments to the randomization, none of which had a significant effect on the research design.[9] The final allocation included 76 projects in the evidence-based condition (33 in Dar-es-Salaam, 43 in Morogoro), and 75 projects in the theory-and-evidence–based condition (31 in Dar, 44 in Morogoro). Finally, to maximize interaction between participant entrepreneurs and instructors delivering the treatment and among participants, we allocated entrepreneurs to small classes of about 15 participants each. Our sample thus included four classes in Dar-Es-Salaam (two classes per experimental condition) and six classes in Morogoro (three per experimental condition).

Quantitative and Qualitative Descriptions of Entrepreneurs

Quantitative Summary Statistics. Table 2 compares entrepreneurs in the evidence-based and theory-and-evidence–based conditions. We observe unbalances only in terms of the self-reported probability of introducing changes, and control for it in all models. Sixty-two percent of entrepreneurs in the treated samples are male. The average entrepreneur is 32 years old, with an average of 3 years of managerial and entrepreneurial experience, 5.7 years of total work experience, and 3 years of work experience in the agricultural sector. On average, entrepreneurs in our sample are highly educated (79% report having tertiary education) and likely to have already attended a business support training (63% of the sample). Projects belong to the agricultural sector, with a striking prevalence of projects related to farming (74%) and processing or logistics/marketing (~33% each). Projects in our sample are mostly for-profit (93%) and had been developed for a median of 18 months. Most projects are at presales/sales stages rather than prototypes (59%). Fifty-seven percent of entrepreneurs declared their intention to work full time on their projects. This sample thus refers to a fairly experienced population of entrepreneurs and to projects with some degree of development (e.g., the entrepreneur selling buns and scones already had some initial sales and had purchased a new oven).

We retained applicants not selected into the training as a separate *control* group. This group consisted of the 37 entrepreneurs located outside the three pilot regions, plus the 14 entrepreneurs randomly excluded from the

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Table 2. Balance Checks

	Evidence-based	Theory-and-evidence-based	Raw difference	Total
Respondents' age	31.421	32.853	1.432	32.132
	(0.870)	(1.074)		(0.690)
Respondent is founder/owner (%)	0.895	0.947	0.052	0.931
	[0.035]	[0.026]		[0.018]
Gender (% male)	0.684	0.560	−0.124	0.623
	(0.054)	(0.058)		(0.040)
Working full time (%)	0.566	0.573	0.008	0.570
	(0.057)	(0.057)		(0.040)
Work experience (years)	4.908	6.507	1.599	5.702
	(0.664)	(0.873)		(0.550)
Work experience in agriculture (years)	2.974	3.320	0.346	3.146
	(0.386)	(0.438)		(0.291)
Managerial experience (years)	3.217	3.453	0.236	3.334
	(0.438)	(0.495)		(0.329)
Entrepreneurial experience (years)	3.671	4.027	0.356	3.848
	(0.448)	(0.490)		(0.331)
Tertiary education (%)	0.776	0.813	0.037	0.795
	(0.048)	(0.045)		(0.033)
Business degree (%)	0.118	0.067	−0.052	0.093
	(0.037)	(0.029)		(0.024)
Firm type (= 1 if startup)	0.658	0.640	−0.018	0.649
	(0.055)	(0.056)		(0.039)
For-profit business (%)	0.921	0.947	0.026	0.934
	(0.031)	(0.026)		(0.020)
Perceived probability of termination	25.171	28.547	3.376	26.848
	(3.539)	(4.177)		(2.729)
Perceived probability of major changes (0–100)	57.763	48.547	−9.216*	53.185
	(3.955)	(3.792)		(2.757)
Hours worked (from interview)	34.895	30.400	−4.495	32.662
	(2.838)	(2.795)		(1.994)
Total revenue in 2020 (Baseline value US$–winsorized 99th)	663.909	1,088.170	424.261	1,020.723
	(122.364)	(358.552)		(179.600)
Total costs in 2020 (Baseline value US$–winsorized 99th)	1,110.566	1,573.447	462.882	1,271.408
	(254.512)	(391.876)		(191.173)
Total profit in 2020 (US$–winsorized 1st–99th)	−106.017	92.901	198.919	118.378
	(253.173)	(301.380)		(183.509)
Number of owners (from interview)	1.895	2.040	0.145	1.967
	(0.147)	(0.271)		(0.153)
Number of salaried employees (from interview)	1.487	3.107	1.620	2.291
	(0.326)	(1.342)		(0.687)
Number of other employees (from interview)	3.039	2.293	−0.746	2.669
	(0.818)	(0.497)		(0.479)
Idea stage (= 1 if sales or presales)	0.592	0.587	−0.005	0.589
	(0.057)	(0.057)		(0.040)
Months worked on the project	29.855	24.640	−5.215	27.265
	(4.669)	(4.080)		(3.100)
Other business courses attended (%)	0.632	0.627	−0.005	0.629
	(0.056)	(0.056)		(0.039)
N	76	75		151

Notes. Means reported with standard errors in parentheses. Revenues and profit figures do not include outlier projects (see Online Appendix Table B1 for these details).
*$p < 0.10$.

training. This group did not attend any training but agreed to provide data for this study in exchange for access to the posttraining events. Given its nonrandomness, we do not include results from the control group in the main analysis. However, we use their data to descriptively discuss results of the control group as a comparative benchmark in the main text and report intention-to-treat (ITT) analysis in the Online Appendix. Table B1 in the Online Appendix shows that in addition to location, this group differs systematically from the training conditions in terms of gender, education levels, possessing a business degree, work effort in terms of hours per week, and number of owners. The control group includes mostly male entrepreneurs

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(78%), has a higher average level of education (92% have tertiary education and 18% have a business degree), and a larger founding team size (2.7 people on average). Unobservable traits connected to the location of entrepreneurs' activities could also be unbalanced.

Qualitative Descriptions. A word cloud of the most frequent words used by entrepreneurs to describe their business ideas during baseline interviews indicated that the bulk of entrepreneurial activities related to agribusiness (Online Appendix, Section E, Figure E1). As expected, a common theme across entrepreneurial ideas is the production and sales of locally sourced agricultural products, with variance across ideas in the type of product envisioned, production methods and distribution systems. For example, one entrepreneur wanted to scale the idea of selling buns and scones enriched by nutritious, orange-fleshed potatoes in various settings (e.g., street retail food stores, kiosks, and mini-supermarkets), whereas other entrepreneurs wanted to provide locally sourced vegetables as alternatives to imports. Another theme is creating new products and services. For instance, one entrepreneur engaged in new product development for transforming locally sourced mangoes into packaged products (e.g., dried mangoes, mango chips), and another entrepreneur created soy-based baby formulas based on observed unmet needs of a family member whose baby had severe allergies to all animal milk. Among entrepreneurs focused on providing services, one entrepreneur pursued the idea of providing flower decorations at events. Yet another entrepreneur observed an increasing trend in the Tanzanian population in noncommunicable diseases (e.g., diabetes, hypertension) and sought to increase awareness of their linkages with food/diet choices to enhance public health. Moreover, and as indicated by the quantitative statistics above, entrepreneurs also varied in their stage of business development.

Data Collection

We collected information about entrepreneurs' projects and decision making across six datapoints or periods between April 2021 (baseline datapoint) and July 2022 (last data collection point).[10] For each period, the data collection consisted of entrepreneurs completing a questionnaire (online or offline) and then being phone-interviewed by a team of about 15 local research assistants (RAs) who were Swahili native-speakers.[11] The questionnaire asked for information about current project status, performance, and entrepreneurs' traits. Phone interviews focused on entrepreneurs' decision-making activities. Table 3 reports key dates and response rates for all six periods. Across the 151 treated entrepreneurs, we collected 756 questionnaires (83% response rate) and 719 interviews (79% response rate). Response rates are not statistically different between experimental conditions, except for interview data in period 2 ($t = -1.87$; $p = 0.06$), for which respondents in the theory-and-evidence–based condition have a higher response rate (83% response rate in the theory-and-evidence–based condition and 70% in the evidence-based condition). We collected at least five questionnaires for 116 entrepreneurs (77% of our initial sample; evidence based = 72%; theory-and-evidence based = 81%) and at least five interviews for 109 entrepreneurs (72%; evidence based = 67%; theory-and-evidence based = 77%).

Attrition and Noncompliance

As common in field experiments (Ghanem et al. 2024, Molina-Millán and Macours 2025), we experienced cases of attrition and noncompliance. Specifically, 16 entrepreneurs (8 in the evidence-based condition; 8 in

Table 3. Data Collection and Response Rates

	0	1	2	3	4	5	
	Prior to training (baseline)	During training	One month after training	Two months after training	Four months after training	Six months after training	
	Apr–Aug 2021	Oct–Nov 2021	End of Dec 2021–Jan 2022	Feb 2022	Apr 2022	Jun–Jul 2022	Total no./%
No. of Survey Responses	151	113	121	125	124	122	756
No. of Interview Responses	151	98	115	117	119	119	719
% of Survey on Initial Sample	100%	75%	80%	83%	82%	81%	83%
Evidence-based	100%	78%	75%	79%	80%	78%	82%
Theory-and-evidence-based	100%	72%	85%	87%	84%	84%	85%
t-test	—	0.79	−1.59	−1.26	−0.59	−0.99	−1.52
% of Interviews on Initial Sample	100%	65%	76%	77%	79%	79%	79%
Evidence-based	100%	68%	70%	75%	75%	75%	77%
Theory-and-evidence-based	100%	61%	83%	80%	83%	83%	82%
t-test	—	0.91	−1.87*	−0.73	−1.15	−1.15	−1.62

Notes. Training consisted of six sessions that ran biweekly starting in October 2021. Three posttraining events took place on February 5, April 23, and June 25 of 2022 to facilitate data collection in periods 3–5.

*$p < 0.10$.

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the theory-and-evidence–based condition) never replied to any data collection round after the baseline. We consider these entrepreneurs as "full" attritors (10% rate). Table C1 in the Online Appendix reports results for selective attrition, showing minor systematic differences between attritors across the two conditions and that respondents' subsamples are still balanced. This suggests a random attrition pattern that allows us to assume that analyses excluding attritors preserve internal validity. To alleviate issues related to attrition and selection bias, we run boundary nonparametric analyses to ensure robustness of results (Horowitz and Manski 2000, Kling et al. 2007). Excluding full attritors, we also experienced noncompliance from some participants who did not attend any sessions but still replied to at least one data collection round after the baseline (8 evidence based, 11 theory-and-evidence based, with no statistically significant difference in the proportion of noncompliers between the two training groups; Pearson $\chi^2 = 0.6042$; $p = 0.437$). Compared with compliers, noncompliers on average had fewer for-profit businesses, fewer entrepreneurs with some business education, and exhibited greater pessimism regarding business survival probability (Table C2).

Dependent Variables

Our analysis focuses on two distinct categories of dependent variables: performance outcomes and decision making. Our first performance outcome variable is *Termination,* computed as a dummy variable equal to one if the entrepreneur indicated they terminated their project at any given point in time within our observation window.[12] We also collected data on *Revenues, Costs,* and *Profits,* reported in figures converted to USD from originally recorded Tanzanian shillings (TZS) for the cumulative sales, costs, and profits made by entrepreneurs in our observation period. *Revenues* and *Costs* amounts are related to the business project under development during the course: For startup projects, this equals the sales (costs) of the overall business; for established companies, we asked for the overall sales (costs) of their business and the share pertaining to the project developed in the training program.[13] *Profits* is computed as the difference between revenues and costs (inclusive of owners' and personnel wages). Figures and regression results exclude four outlier projects (three of which were in the theory-and-evidence–based condition) that reported exceptionally large sales (60 times the average of nonoutliers). The Online Appendix, Sections F1 and F2, includes details on the exclusion of those four projects, as well as models including those firms. After excluding these outliers, we winsorize *Revenues* at the 99th percentile and *Profits* at the 1st and 99th percentile of each period distribution, including data from the nonrandom control group. We also report results with nonwinsorized outcomes and with alternative winsorizing procedures in the Online Appendix, Section F3.[14] Additionally, we compute *periodic revenues* and *periodic profits* by dividing the total amounts between each data collection period by the number of months that passed between those periods (see the Online Appendix, Section F8).

To track decision making, we computed several dummy variables that measure whether entrepreneurs made changes to the business models of their projects at any given point in time within our observation window. These variables are based on data collected by RAs during phone interviews with entrepreneurs. RAs asked entrepreneurs whether they made any changes to the business model since the last interview[15] and additionally coded which of the nine elements (i.e., problem, solution, unique value proposition, key metrics, competitive advantage, customer segments, channels, revenue stream and cost structure) of the BMC were changed (Please see the Online Appendix, Section D, for details on this coding process). Using this information, we created both a cross-sectional (across all periods) and a panel (period-specific) version of the following variables. *Business Model Changes* is equal to one if an entrepreneur introduced any kind of change to their business models, regardless of which element. *No Changes* is equal to one if entrepreneurs made no changes to the business model. We further distinguish between core and operational changes in the business models.[16] *Core Changes* is a dummy variable set to one if the entrepreneur made changes to the problem, solution, unique value proposition, competitive advantage, and customer segments elements of the BMC. We consider these elements core to a venture because they reflect the key value proposition being offered and its rational justification against a given market. Making changes to these elements thus entails fundamentally altering the nature of the business and its raison d'être. *Operational Changes* is a dummy variable set to one if the changes occurred in the four remaining elements (channels, key metrics, revenue stream, and cost structure). We consider these elements operational because they can be changed without fundamentally altering the business idea. We also create a dummy variable *Both Changes* set equal to one if the entrepreneur introduced changes in both categories.[17] Because the above three variables are not mutually exclusive, we created a categorical variable with four mutually exclusive categories that reflect the different types of changes (e.g., no changes; only core changes; only operational changes; both changes).

Independent Variables and Additional Variables of Interest

Because our goal is to compare entrepreneurs across treatment conditions, our main independent variable

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Theory-and-Evidence is a dummy taking a value of one when the project was assigned to the theory-and-evidence–based condition and zero for the evidence-based condition. To assess the effectiveness of our experimental design, we trained RAs to evaluate the responses by entrepreneurs to questions about the development of their projects. For each of the two variables related to theory and hypothesis development, we used five items that were ranked on a Likert (1–5) scale and took the average of these items to create each of the two variables, which could range in value from a theoretical minimum of one and a maximum of five (Cronbach alphas for the theory and hypothesis scores are respectively of 0.90 and 0.91). We use these scores as manipulation checks to verify that entrepreneurs in the theory-and-evidence–based condition show higher theory-related scores (see the Online Appendix, Section D, for details on the items and coding process). Across all specifications, we add controls related to variables unbalanced after randomization based on *t*-tests (Table 2). We also add a dummy controlling for the type of project (startup or established firm), and one for instructors. These dummies allow us to control for unobservable factors related to the type of project developed during training and potential instructor-related fixed effects.

Empirical Strategy

We compute ITT effects comparing entrepreneurs in the evidence-based and theory-and-evidence–based experimental conditions regardless of compliance. We employ linear models for continuous dependent variables and probit models for binary dependent variables. In the latter case, we report average marginal effects to ease interpretation. In panel regressions, standard errors are clustered at the entrepreneur level, and period dummies are added to the specifications. In cross-sectional regressions, we report robust standard errors. We also report randomization inference *p*-values as an additional robustness check for the statistical significance of our results (Heß 2017). We report results with and without the control variables. In the relevant sections of the Online Appendix, we report several robustness checks including among others: cross-sectional models with standard errors clustered by classroom; models with interactions between the treatment indicator and training location; analyses on the subpopulation of "panel" respondents; and additional nonparametric boundary analyses to alleviate attrition concerns.

Results

Manipulation Checks: Entrepreneurs' Theory and Hypothesis Scores and Evidence Collection Efforts

For the training goals to manifest in implementation, the theory-and-evidence–based approach should have higher theory scores relative to the evidence-based approach, as this is the primary difference between them. They should, however, have similar hypothesis and evidence collection scores as both approaches equally emphasize hypothesis testing and evidence gathering (experimentation). Figures E2a, E2b, and E6 in the Online Appendix, Section E, provide information on these manipulation checks.

Figure E2a in the Online Appendix shows the predicted theory score over time from a linear regression that interacted the theory-and evidence–based treatment indicator with the period indicators and included control variables noted above. Consistent with training goals, entrepreneurs in the theory-and-evidence–based condition show an increase in the theory score from the baseline (Period 0) compared with those in the evidence-based condition. Starting from Period 1, we see a sharp increase in the theory score for Period 1 (during training) and Period 2 (the first data collection round after training ends) in the theory-and evidence–based approach, but such a trend is not observed in the evidence-based approach. Table E1 in the Online Appendix provides additional details on the regression model underlying Figures E2a and E2b, showing that the theory-and-evidence–based condition is associated with a significant increase of the theory score in Periods 2 and 3. Thus, the training goals seem to manifest in observed scores across the two treatment conditions in terms of how entrepreneurs approach their project based on their use of theory, providing support that the manipulations worked as intended. We also note the decay over time for both conditions after Period 3. This is consistent with entrepreneurs who graduated from the training program resolving uncertainty regarding the focal project or going back to business as usual.

Figure E2b in the Online Appendix reports the predicted *hypothesis* score for the two treatment conditions. The hypothesis score measures the extent to which the respondent has identified specific hypotheses, as well as the degree of alignment between these hypotheses and their theory-of-value and/or business model. Over time, this score also captures the extent to which new hypotheses entrepreneurs may develop are aligned with aspects of the theory-of-value and/or business model that need changing as a result of experimentation (see the Online Appendix, Section D1, for more details). Relative to the base Period 0, entrepreneurs in both periods show an increase in hypothesis scores between periods 0 and 2 (from the baseline to the end of training, similar to Figure E2a), but there are no significant differences between the two treatment conditions. Consistent with this observation, regression results reported in Table E1 in the Online Appendix show that the theory-and-evidence–based condition is not associated with a statistically significant increase of the hypothesis score across all data collection periods.

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Figure E6 in the Online Appendix shows the share of entrepreneurs who reported collecting evidence to test their hypotheses in each period. The trend is consistent with Figure E2b in the Online Appendix: All treated entrepreneurs (95%) relied on evidence collection from the onset and increased their evidence collection in Periods 1 and 2. Moreover, the training's content encouraged entrepreneurs in the evidence-based condition to collect information more frequently and entrepreneurs in the theory-and-evidence–based condition to spend more time evaluating the evidence collected against their theory-of-value before collecting new data. The trends in Figure E6 in the Online Appendix seem consistent with this manipulation because a higher share of the evidence-based entrepreneurs continued to collect evidence in Periods 3 and 4, relative to the theory-and-evidence–based entrepreneurs.

Overall, these patterns suggest that the theory-and-evidence–based condition effectively nudged entrepreneurs to develop more structured theories-of-value. Both conditions are similar in terms of hypothesis testing and evidence collection. The Online Appendix, Section E, provides results comparing the two treatment conditions to the control group: The control group had lower theory and hypothesis scores than for the two treated conditions (Figures E3 and E4), and fewer entrepreneurs collected evidence over time (Figure E7).[18]

Outcomes: Project Termination and Performance Outcomes

We now turn to the test of the null for no significant differences between the two experimental conditions. For *Termination* we find no statistical or economically significant differences (Table 4, Models 1 and 2). Specifically, eight (10%) and seven (9%) entrepreneurs decided to terminate their projects in the evidence-based and theory-and-evidence–based conditions, respectively. None of the entrepreneurs in the control group terminated their project. Cross-sectional regressions conservatively consider projects by attritors as active because we have no information to establish whether they terminated their project. Nine entrepreneurs who terminated their original project decided to restart their entrepreneurial journey with an entirely new project (five in the evidence-based condition and four in the theory-and-evidence–based condition).

For economic outcomes, recall that both treatments are balanced in terms of stage of business projects, revenues and profits reported prior to the training (Table 2). We begin with an analysis of the panel trend of cumulative amounts. Figure 3 shows the trend, by experimental condition, of cumulative *Revenues*, *Costs*, and *Profits*. Tables 4 and 5 report econometric results from (a) a panel linear model with period dummies, (b) a difference-in-difference model relative to the baseline, and (c) cross-sectional results using the last available datapoint in the panel for each entrepreneur.

Table 4. Performance Metrics Regressions for Differences Across Treatment Conditions: Termination and Revenues

	Termination dummy		Cumulative revenues (99th winsorized)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	LPM	LPM	Pooled OLS	Pooled OLS	DiD	Cross section	Cross section
Theory-and-evidence	−0.01	0	1,793.36	2,014.06^	1,548.13	2,231.04	2,383.79^
	(0.05)	(0.05)	(1,124.68)	(1,166.10)	(976.43)	(1,361.40)	(1,353.32)
RI *p*-value	[0.363]	[0.363]	[0.136]	[0.100]	[0.148]	[0.104]	[0.076]
Lower bound	—	—	494.85	543.39	84.70	47.88	162.53
Upper bound	—	—	3,272.39***	3,347.56***	3,417.76***	5,198.50***	5,363.11***
N	151		709			147	
Standard error	Robust	Robust	Firm	Firm	Firm	Robust	Robust
Period dummies	No	No	Yes	Yes	Yes	No	No
Firm dummies	No	No	No	No	Yes	No	No
Controls	No	Yes	No	Yes	No	No	Yes
No. of firms	151				147		

Notes. Models 1 and 2 report the results of a linear probability model with robust standard errors on the probability of terminating the project. Attritors are considered as having not terminated their projects. Models 3 and 4 report the results of a pooled OLS model with period dummies, without and with controls, respectively. Model 5 reports a difference-in-difference model with fixed-effects relative to the baseline. In these models, data for attritors is not imputed. Models 6 and 7 report cross-sectional results using the last available datapoint in the panel for each entrepreneur, without and with controls respectively. 79% of participants reported data in the last period of observation. For Models 3–7, four outlier projects have been excluded, and the dependent variable (cumulative revenues) has been winsorized at the 99th percentile of the within-period distribution. Standard errors, clustered at the entrepreneur (firm) level for panel models or robust standard errors for cross-sectional models, are reported in parentheses. Randomization inference *p*-values (250 repetitions) are reported in squared parentheses. To alleviate attrition concerns, we report lower and upper nonparametric bounds of treatment effects ($N = 882$; six periods for 147 unique firms). Controls include probability of major changes at the baseline, project type, instructor dummies.

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\wedge}p < 0.1$.

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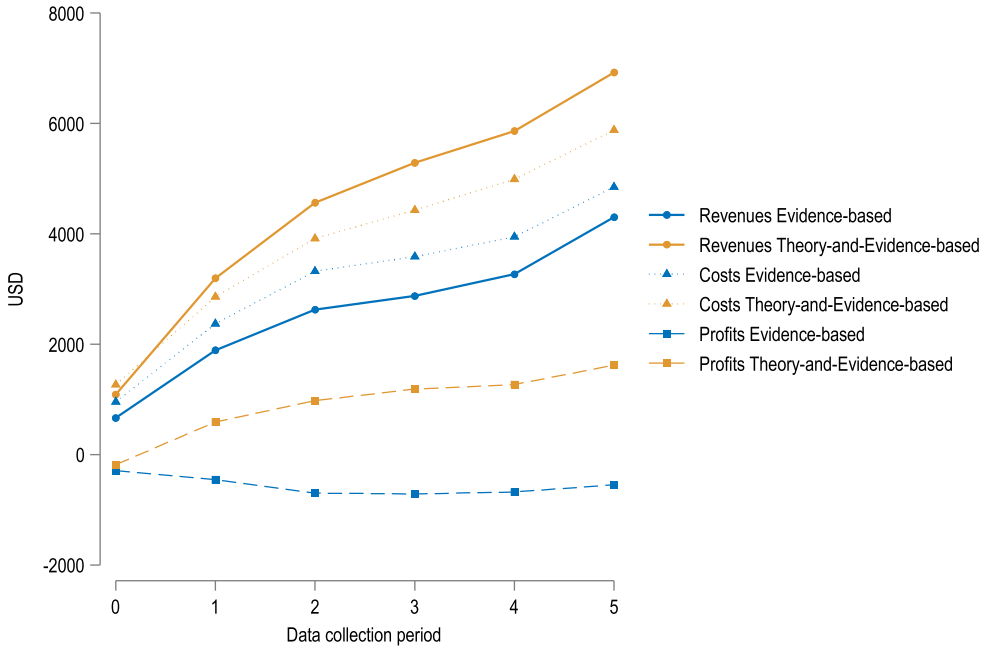
Figure 3. (Color online) Cumulative Revenues, Costs, and Profits Across Treatment Conditions (US$)



Notes. Four outlier projects are excluded. Cumulative revenues and costs are winsorized at the 99th percentile of the within-period distribution. Cumulative profits are winsorized at the 1st and 99th percentile of the within-period distribution. The two measures are used in Table 4, Models 3–5, and Table 5, Models 1–3.

Results show that entrepreneurs in the theory-and-evidence–based condition, on average, achieved higher revenues and profits over time compared with those in the evidence-based condition. Figure 3 reveals that both revenues and costs increase in both conditions. For entrepreneurs in the theory-and-evidence–based condition, revenues (solid line) increase more than costs (dotted line). For entrepreneurs in the evidence-based condition, costs (dotted line) remain higher than their revenues (solid line) across all periods. Hence,

Table 5. Performance Metrics Regressions for Differences Across Treatment Conditions: Profits

	Cumulative profits (1st–99th winsorized)				
	(1)	(2)	(3)	(4)	(5)
	Pooled OLS	Pooled OLS	DiD	Cross section	Cross section
Theory-and-evidence	1,391.45*	1,376.96**	1,494.29**	1,824.93*	1,769.87*
	(547.05)	(525.64)	(547.79)	(755.12)	(734.11)
RI *p*-value	[0.000]	[0.004]	[0.008]	[0.004]	[0.004]
Lower bound	715.63	660.93	729.09	825.95	825.95
Upper bound	2,230.89***	2,194.11***	2,547.41***	3,509.51***	3,532.68***
N		709		147	
Standard error	Firm	Firm	Firm	Robust	Robust
Period dummies	Yes	Yes	Yes	No	No
Firm dummies	No	No	Yes	No	No
Controls	No	Yes	No	No	Yes
No. of firms			147		

Notes. Models 1 and 2 report the results of a pooled OLS model with period dummies, without and with controls, respectively. Model 3 reports a difference-in-difference model with fixed-effects relative to the baseline. In these models, data for attritors is not imputed. Models 4 and 5 report cross-sectional results using the last available datapoint in the panel for each entrepreneur, without and with controls respectively. 79% of participants reported data in the last period of observation. Four outlier projects have been excluded from all models. The dependent variable (cumulative profits) has been winsorized at the 1st and 99th percentile of the within-period distribution. Standard errors, clustered at the entrepreneur (firm) level for panel models or robust standard errors for cross-sectional models, are reported in parentheses. Randomization inference *p*-values (250 repetitions) are reported in squared parentheses. To alleviate attrition concerns, we report lower and upper nonparametric bounds of treatment effects (N = 882; six periods for 147 unique firms). Controls include probability of major changes at the baseline, project type, instructor dummies.

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$; ^$p < 0.1$.

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entrepreneurs in the evidence-based condition accrue average negative profits, whereas those in the theory-and-evidence–based condition accrue average positive profits (dashed lines). Table F15 in the Online Appendix displays the approximated monthly revenues, costs, and profits across theory-and-evidence– and evidence-based groups throughout our observation window. Complementing the graphical evidence shown in Figure 3, Table F15 shows theory-and-evidence–based projects have, on average, slightly higher costs than evidence-based projects and that these high costs are compensated by higher revenues. On the contrary, evidence-based projects' average monthly costs are higher than revenues, which in turn leads to average negative profits. Regression results in Table 4 show the difference in revenues across the two conditions is economically meaningful. The difference in differences (DiD) estimation reported in Model 5 shows, on average, projects developed by entrepreneurs in the theory-and-evidence–based treatment experienced an increase in revenues of $1,548.13 (Randomization Inference (RI) $p = 0.148$). Pooled ordinary least squares (OLS) results and cross-sectional models related to the last available revenue figure display comparable results both in terms of effect size and statistical significance[19] (Table 4). The differences across the two conditions for cumulative profits are greater, more statistically significant, and robust to the inclusion or exclusion of control variables. Specifically, Table 5 shows a difference of $1,494.29 (RI $p = 0.008$) in the DiD specification. Notably, the average annual income per capita in Tanzania is around $1,100 (The World Bank Group 2021), so the effect found at the business level over the 15-month timespan of our observation window appears to be substantial. However, it is worth recalling that the *Profit* figures are computed subtracting total *Costs* from the *Revenues* amount and that *Costs* include wages, indicating that projects developed by entrepreneurs in the theory-and-evidence–based condition generate, on average, profits that enable them to pay salary of their founders and employees and would allow them, for example, to hire an additional individual. At the same time, negative profits recorded for the average project developed by entrepreneurs in the evidence-based condition implies that founders and employees are still able to earn wages. Tables 4 and 5 report results of nonparametric bounds estimation (Section F4 in the Online Appendix explains the procedure used for computing bounds), alleviating attrition concerns and reinforcing the robustness of the estimated effects.

Our results are robust to several alternative specifications: we replicate results using alternative winsorizing procedures and without winsorizing (Section F3 in the Online Appendix), with alternative bounds (Section F4 in the Online Appendix), and considering different subpopulations (Section F5 in the Online Appendix). Moreover, we examine whether there are heterogeneous effects within groups through a cross-sectional simultaneous quantile regression on the final cumulative revenue and profit (Section F6 in the Online Appendix). The results reveal that effects for the theory-and-evidence–based condition become larger at the 75th and 95th percentiles, indicating a stronger effect of the treatment on projects at the right tail of the performance distributions (Tables F9 and F10 in the Online Appendix). When looking at results by project type (Section F7 in the Online Appendix), we find positive improvements for both startups and established companies, with results for startups exhibiting higher statistical significance. Section F8 in the Online Appendix explores performance effects using periodic measures. Section H in the Online Appendix shows that the statistically significant differences in performance are robust to the inclusion of the (nonrandom) control group.

Decisions: Business Model Changes

Although the above analyses examine differential effects of the theory-and-evidence–based and evidence-based conditions, our preregistered methodology also stated that we would "examine potential mechanisms behind these outcomes by collecting variables related to how and when decisions are made." In this section, we turn to these mechanisms by examining the nature of pivots. As noted in the variable description section, we distinguish between "major" or "core" changes made to the business model of entrepreneurs' projects and "operational" changes. We start by testing the null for no significant differences between the two experimental conditions. Across the entire observation period, we find that the share of entrepreneurs introducing *Business Model Changes* is similar between the two conditions. Specifically, 57 entrepreneurs (76%) in the theory-and-evidence–based condition introduced at least one change versus the 55 (72%) in the evidence-based condition. As a comparison, only 33% of entrepreneurs in the control group did so. Table 6 reports the average marginal effects from a Probit regression with the full set of controls included. In Model 1, the results on the dummy (recording whether at least one change to the business model was introduced) indicate no significant differences.

Our data enable us to examine business model changes in terms of the types of changes introduced. Specifically, we distinguish between the four mutually exclusive categories of *No Changes*, *Operational Only*, *Core Only*, and *Both Changes* to the business model. Figure 4 shows cross-sectional results reflecting entrepreneurs who made changes regardless of period. Recall that in this analysis, *Both Changes* indicates that entrepreneurs have introduced at least one core change and one operational change to their business model, simultaneously or not. We do not find sizeable differences between treatment conditions in the *No Changes* category. However, among the changes made, we find

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Table 6. Regression: Business Model Changes (Cross Section)

	(1)	(2)				(3)	(4)	(5)
	BM changes	No changes	Operational changes only	Core changes only	Both changes	Operational changes	Core changes	Both changes
Theory-and-evidence	0.05	−0.05	−0.03	−0.13*	0.21**	0.17*	0.08	0.20**
	(0.07)	(0.07)	(0.04)	(0.07)	(0.08)	(0.07)	(0.07)	(0.07)
RI *p*-value	[0.516]					[0.028]	[0.316]	[0.000]
N	151	151				151	151	151
Standard error	Robust	Robust				Robust	Robust	Robust
Controls	Yes	Yes				Yes	Yes	Yes
Model	Probit	Multinomial probit				Probit	Probit	probit

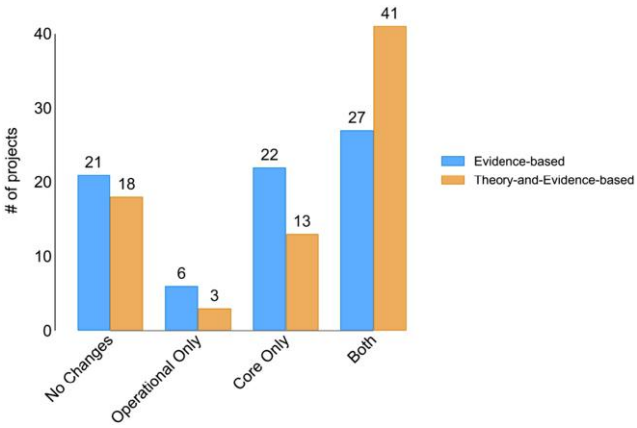
Notes. Average marginal effects reported from probit (Models 1 and 3–5) and multinomial probit (Model 2) regressions, controlling for probability of major changes at the baseline, firm type, and instructor dummies. The DVs for Model 2 are based on the mutually exclusive categorical variable. Model 2 excludes instructor dummies due to failed convergence: Results are fully replicated with a multinomial logit model and instructor dummies (Online Appendix, Section G). Results are robust to alternative specifications, including specifications without any control variables albeit with slightly lower statistical significance (Online Appendix, Section G). Attritors are considered to have not introduced any change. Robust standard errors reported in parentheses, with randomization inference *p*-values in squared brackets.

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $\hat{}p < 0.1$.

that more entrepreneurs in the evidence-based condition introduced singular (operational only or core only) changes compared with entrepreneurs in the theory-and-evidence–based condition, who instead were more likely to introduce both type of changes (the *Both Changes* category).

Figure 4's patterns are supported by regression analysis. In Table 6, Model 2 presents the average marginal effects from a multinomial probit regression. Entrepreneurs in the theory-and-evidence condition are 24 percentage points (pp) more likely to have introduced *Both Changes* and 13 pp less likely to have introduced *Core Only* changes. Because these findings obscure the overall number of changes made, we also provide the aggregated cross-sectional results for *Core, Operational,* and *Both Changes* in Models 3–5, respectively, noting that these are no longer mutually exclusive. Results show that the entrepreneurs in the theory-and-evidence–based condition were significantly more likely to introduce *Operational Changes* (17-pp increase, Model 3); entrepreneurs in both conditions were not significantly different with regard to making *Core Changes* (Model 4), and entrepreneurs in the theory-and-evidence–based condition were significantly more likely to make *Both Changes* (20-pp increase, Model 5).

Figure 4. (Color online) Changes to the Business Model (Cross Section)



Notes. Number of entrepreneurs for each type of change in the cross section, given the mutually exclusive categorization. Entrepreneurs in the "No Changes" category never introduced any changes. Those in the "Operational Only" and "Core Only" categories introduced changes related only to operational or core elements of their business models, respectively. Entrepreneurs in the "Both" category introduced changes to both types of elements of their business models. The corresponding categorical variable is used in Table 6, Model 2.

Figure 5 illustrates types of changes made to each project by data collection period. Figure 5(a) shows that in every period, a higher number of entrepreneurs made *No Changes* to their projects in the evidence-based condition relative to the theory-and-evidence–based condition. In Figure 5(b), the *Operational Only Changes* trend illustrates that, except for Period 1, entrepreneurs in the theory-and-evidence–based condition made more operational only changes to their projects relative to entrepreneurs in the evidence-based, particularly in the last three periods of data collection. Figure 5(c), the *Core Only* changes trend reveals that entrepreneurs in the evidence-based condition made the greatest number of core only changes to their projects in Period 1, while entrepreneurs in the theory-and-evidence–based condition made the greatest number core only changes to their projects in Period 2, one month after training had concluded and coinciding with the peak in the theory score as shown in Figure E2a in the Online Appendix. In Periods 3–5, entrepreneurs in both conditions made a similar number of *Core Only* changes. Finally, Figure 5(d) shows more entrepreneurs in the theory-and-evidence–based condition introduced *Both Changes* consistently over time relative to entrepreneurs in the evidence-based condition. These results are corroborated

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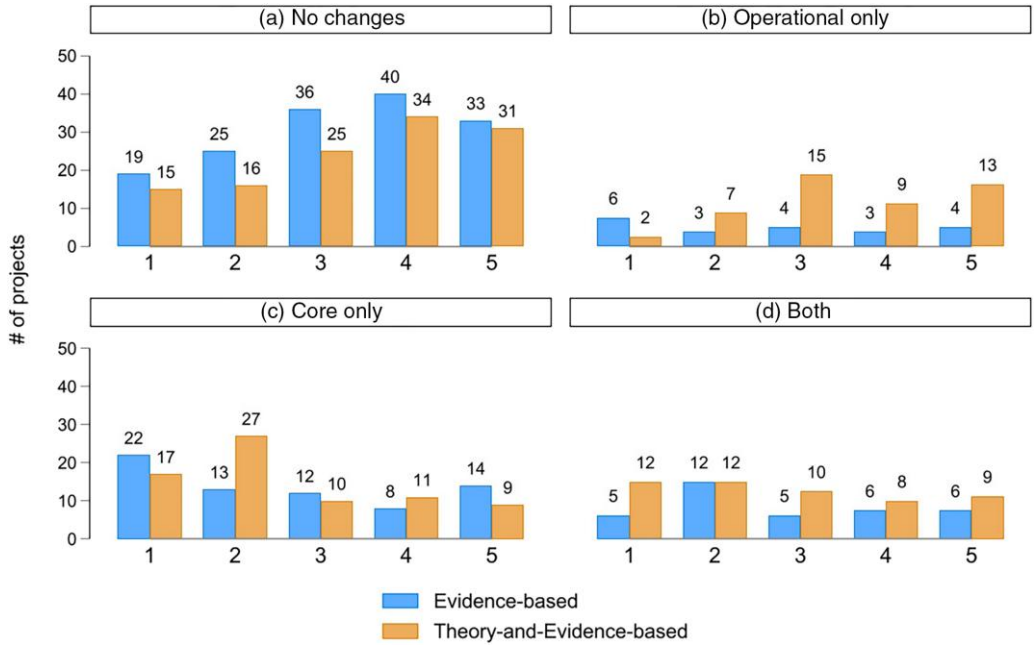
Figure 5. (Color online) Changes to the Business Model (Panel)



Notes. Number of entrepreneurs for each type of change in each data collection period (mutually exclusive categorization). The corresponding categorical variable is employed in Table 7.

by a multinomial probit model with period dummies (Table 7). Relative to those in the evidence-based condition, entrepreneurs in the theory-and-evidence–based condition are on average 14 pp less likely to have not made any changes within a period (Model 5), 8 pp more likely to have made *Operational Only* changes (Model 6), and 6 pp more likely to have introduced *Both Changes* together (Model 8). Results of Model 8 exhibit slightly higher statistical significance when control variables are included. There are no significant differences for *Core Only* changes (Model 7).

In summary, the above results indicate that entrepreneurs in the theory-and-evidence–based condition had higher performance (revenues and profits) relative to the evidence-based condition. These entrepreneurs also made relatively more changes to the operational elements of their business model, not just singularly, but also in coordination with core changes.[20]

Discussion

We join a growing body of work in strategy and entrepreneurship that suggests systematic approaches to decision making, such as the evidence-based and theory-and-evidence–based approaches, provide important benefits for entrepreneurial firms (Ott et al. 2017, Zellweger and Zenger 2023). Studies employing experimental designs,

Table 7. Regression: Business Model Changes (Panel)

	(1) No changes	(2) Operational changes	(3) Core changes	(4) Both changes	(5) No changes	(6) Operational changes	(7) Core changes	(8) Both changes
Theory-and-evidence		0.74***	0.29	0.51*	—	0.80***	0.25	0.63**
Mprobit coefficients		(0.22)	(0.21)	(0.25)	—	(0.23)	(0.21)	(0.23)
Average marginal effects	−0.14**	0.08**	0.01	0.05	−0.14**	0.09**	−0.01	0.06*
	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.03)
N		568				568		
Standard error		Firm				Firm		
Controls		No				Yes		
Model		Multinomial probit				Multinomial probit		
Period dummies		Yes				Yes		

Notes. DVs, categorical variable recording the type of change introduced in each period; Base outcome, no changes. Controls include probability of major pivoting at the baseline, firm type (startup or business), and instructor dummies. Standard errors clustered at the entrepreneur (firm) level reported in parentheses. Baseline (pretraining) period is excluded by the computation because no changes were introduced relative to other periods.

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$; ^$p < 0.1$.

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however, tend to study each type of approach individually against a control group that receives a placebo training with no emphasis on experimentation (Camuffo et al. 2020, 2024; Novelli and Spina 2024). This study offers the first systematic comparison of these two popular approaches and discusses their effects on entrepreneurial decision making and performance outcomes. We conducted research in a geographical context (Africa) and industry (agribusiness) that are comparatively underrepresented in the management literature, but where entrepreneurship is critically important (George et al. 2016a, Fitz-Koch et al. 2018, Bruton et al. 2024). Our design enables us to draw conclusions about the effectiveness of each approach, and whether adding a theory-of-value does add value to entrepreneurial experimentation.

Our results suggest three main insights: Entrepreneurs in our sample who were taught to develop a theory-of-value (the theory-and-evidence–based approach) (a) have similar termination rates to entrepreneurs who rely on experimentation without a theory-of-value (the evidence-based approach), (b) experienced higher revenues and profits throughout our observation window, and (c) were more likely to introduce *both* operational and core changes to the business models of their projects, as opposed to core only or operational only changes. Taken together, these results demonstrate that there is indeed value in developing a theory-of-value to guide experimentation and decision making. We now elaborate on the theoretical implications of these findings.

Theoretical Implications

Because both evidence-based as well as theory-and-evidence–based approaches use experimentation, entrepreneurs trained in either approach benefit by investigating potentially valuable ideas and terminating those deemed less viable, relative to entrepreneurs receiving a placebo training or no training at all. This is consistent with prior studies (Camuffo et al. 2020, Leatherbee and Katila 2020) that emphasize the value of hypothesis testing prior to commitment of resources, as well as with related work that emphasize the importance of a firm's ability to experiment and adapt as they learn under conditions of high uncertainty and rapid change (Rindova and Kotha 2001, Murray and Tripsas 2004, Eisenhardt and Bingham 2017).

However, for unleashing the potential value of the ideas deemed worthy of pursuit, the theory-and-evidence–based approach benefits entrepreneurs in terms of both realized revenues and profits, relative to the evidence-based approach. Our study thus contributes by providing evidence that adjudicates whether a theory-of-value adds value over and beyond gathering evidence through hypothesis testing. This result is consistent with prior work on managerial cognition that has shown more accurate causal mental models to be a key source of superiority in firm strategy and performance (Gary and Wood 2011) and practitioner-oriented work emphasizing strategy mapping and propositional logic to develop "strategy arguments" (Carroll and Sørensen 2021).

Our study also offers some initial insights on *how* entrepreneurial decisions differ under the two conditions. In doing so, we extend the literature on the scientific approach by identifying a potential underlying mechanism through which a theory-of-value adds value. Specifically, our data suggests that having a theory-of-value is associated with making changes to the business model in a more coordinated manner, that is, changes that involve *both* core and operational elements of the business model. Recall that we consider core changes those that entail modifications of the problem, solution, unique value proposition, competitive advantage, and customer segments elements of the BMC; operational changes involve channels, key metrics, revenue stream, and cost structure. We speculate below about the possibility that these changes are tied to the complementarities between theory and experimentation embedded in the design of the theory-and-evidence–based condition across all three stages of belief formation, testing, and validation.

In the belief formation stage, a theory-of-value explicates how the entrepreneur's beliefs about the business idea (e.g., a set of antecedents or first principles) logically relate to core and operational elements of the business model, thus enabling the identification of interdependencies between core and operational elements. Entrepreneurs are thus better able to see how the pieces of the puzzle fit together when translating the business idea into value creation. Such "vision" has been identified as critical to success because the abstract conceptualization results in a big picture set of relationships to identify a "simpler, more universal form of the problem" (Schilling 2018, p. 340). In doing so, entrepreneurs may rely on their instincts about customer desires that have not yet formed, and as famously noted by Steve Jobs, "read things that are not yet on the page" (Isaacson 2012, p. 97). The identified antecedents, their causal linkages to core and operational elements, and interdependencies within the business model thus provide structured hypotheses that are potentially falsifiable.

Having a theory-of-value may also make experimentation in the belief testing stage more informative, enabling entrepreneurs to have better designed experiments that test carefully selected, nonredundant beliefs or business model elements *in conjunction* with each other. Because each hypothesis acts as a knowledge module that plugs into a conceptual architecture, other things equal, there is reduced variation relative to testing loosely connected hypotheses. Theory-guided experiments might provide information not only about each individually tested hypothesis (e.g., a specific core/operational element), but also the whole causal

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chain (e.g., connection with assumptions, interdependency of core, and operational elements) within which the hypothesis is embedded.

Experimentation may also make the theory-of-value more effective because the belief updating stage allows entrepreneurs to evaluate the initial theory-of-value in light of experimental evidence. The experiments may support some initial insights, falsify other assumptions and causal links, generate novel insights, and identify what elements of the business model need to be adjusted, singularly or jointly with others. Because the boundary conditions of the validated learning derived from each hypothesis test are clear, entrepreneurs may develop a better sense of where learning can be reapplied. This would enable them to reformulate their theory-of-value in a holistic rather than piecemeal manner, visualizing once again how their refined set of antecedents causally link to core and operational elements, and what changes in the business model go hand in hand.

In conjunction, theory and experimentation within the iterative process of belief formation, testing and updating suggests a stronger alignment between underlying causal factors, strategy, and operations that results in higher performance outcomes. In this sense, the theory-and-evidence–based approach to entrepreneurial decision making echoes insights in strategic management that coherence between a firm's strategy and associated activities is key to value creation and capture (Porter and Siggelkow 2008). Here, we extend the literature by reinforcing the "strategy-as-theories" view (Gavetti et al. 2005, Gary and Wood 2011, Eisenhardt and Bingham 2017, Felin and Zenger 2017, Carroll and Sørensen 2021) and providing experimental evidence that formalizing a theory-of-value (Valentine et al. 2024) can help achieve that congruence and ultimately boost performance.

Contextual Implications

In replicating training approaches in the developing world and in an agribusiness context, we answer calls for research for programs that may help increase financial inclusion and reduce global poverty (George et al. 2016a, b) and provide evidence of generalizability. Similar to those in Western contexts, entrepreneurs in severely resource constrained environments benefit from expending cognitive effort and resources in articulating a theory-of-value prior to experimentation. Such generalizability is not necessarily intuitive, given significant differences in the nature of entrepreneurial ventures across Western and non-Western contexts. In Africa, for example, 90% of entrepreneurs initiate small or microenterprises and operate in the informal sector (African Development Bank Group 2013, Chow and Rubin 2013, Sriram et al. 2020). Predominantly, these entrepreneurs are necessity-driven because of limited employment opportunities. Given the critique above of higher upfront costs embedded in the theory-and-evidence–based approach, a legitimate concern is whether investing entrepreneurial resources for developing a theory-of-value is justifiable in the limited resource context typical of low-income economies (Bruton et al. 2024). Moreover, entrepreneurs may continue with an initial idea even when they should abandon it, because the Western paradigm of "entrepreneurial failure as learning" is not prevalent in non-Western contexts for cultural and financial reasons. Instead, stigmatization of failure results in loss of social standing and limited access to resources and networks (Amankwah-Amoah 2018). Despite these differences, our study establishes that training programs emphasizing experimentation empower termination of projects perceived to have low potential and that a theory-of-value enhances the capabilities of entrepreneurs to unlock the value potential of their ideas by undertaking coordinated changes in their business model. The enhanced value (in terms of revenues and profits) is substantial, given entrepreneurs in our study focused on developing new business ideas.

The insights from this study underscore that training on the theory-and-evidence–based approach helps developing country entrepreneurs improve their systematic decision making under uncertainty. Such entrepreneurial training can complement efforts to build market institutions (Schneider 1997, Hughes and Lonie 2007, Mair et al. 2012, Dutt et al. 2016) and training programs on managerial and business capability development (McKenzie and Woodruff 2014, 2017) to enable entrepreneurs to make better strategic decisions. This type of entrepreneurial training is fundamentally different from other common interventions such as training on basic business practices (e.g., record keeping and marketing; McKenzie and Woodruff 2014), psychology-based personal initiative trainings (Campos et al. 2017), and/or capital grant endowments (Karlan et al. 2015). Particularly striking here is the higher economic significance of the theory-and-evidence–based treatment effect observed in our study (relative to both the evidence-based treatment and the nonrandom control group) compared with the treatment effects of business training programs (reported to be a 5%–15% increase across the meta-analysis of studies in McKenzie (2021)). Absent a rigorous and systematic comparison (please see the future research avenues below) of entrepreneurial and business training programs, we can only speculate on reasons why. Predominantly, the differences in economic effects may stem from the fact that our study focused on new business ideas rather than established operations (which constitute most of the samples in the studies reviewed by McKenzie (2021)). As a result, it may well be that the higher economic effects stem from a lower baseline level of revenues and profits. However, the substantive effect may also be attributed to the fact that during the highly uncertain stages of new business

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ideas, developing a theory-of-value and using it to guide experimentation is a critical differentiator in creating unique value propositions.

Boundary Conditions, Alternative Explanations, and Future Research

We acknowledge limitations and boundary conditions of our study, some of which also present avenues for future research. First, although our study's design provides for adjudication across two training approaches that have gained steam in recent decades, resource constraints coupled with geographical location challenges precluded our ability to create a *random* control group. Although the comparison of the randomized treatment conditions to the nonrandom control group in our study accord with prior studies (Camuffo et al. 2020, Leatherbee and Katila 2020), we relegated these analyses to the Online Appendix and exercised caution in interpreting the results. Second, although we add to the growing body of evidence on evidence-based and theory-and-evidence–based entrepreneurial training approaches, particularly by examining a developing country context, our results are still subject to the concern of a lack of external validity. The specific context and the collected sample may not be generalizable to other regions, industries, and settings.

The boundary conditions of our study, coupled with the theoretical implications resulting from our additional analysis on types of business model changes, also offer opportunities for future research. We contained the current study to a faithful replication of the two training approaches in the field, and the study also occurred in the period immediately after the COVID pandemic (although there was no lockdown that affected business operations in Tanzania). Future studies could leverage larger sample sizes to undertake deeper dives into how theories are built and developed and whether theory building is more beneficial to specific subsets of entrepreneurs. Variation in business ideas, prior human capital (education or experience) or comfort/preference for theorizing may impact the effort deployed in developing a theory-of-value. Similarly, because theory and experimentation complement each other, future research may examine the use of data-driven analysis and access to enabling technologies (e.g., machine learning for prediction purposes) and whether and how these affect such complementarities. Such technology-enabled data analysis may affect precision of testing of the theorized business model and aid in simulations, with implications for what coordinated changes may result in more value-add. Larger sample sizes would also enable future studies to explore the patterns we have uncovered regarding interdependencies in business model changes to formally test hypotheses for the mechanism we theorize about as well as examine other potential mechanisms. We would also benefit from studies that systematically compare the effects of entrepreneurial versus business practices training programs (e.g., record keeping and marketing; McKenzie and Woodruff 2014) to understand whether, why, and how different types of training programs yield differences in economic performance.[21]

Although we conjectured above that coordinated changes in the business model may drive performance differences between the theory-and-evidence–based and the evidence-based group, several additional factors may be at play, either by themselves or due to an interplay with coordinated changes to the business model.[22] One plausible alternative explanation is that without a theory-of-value to guide experimentation, hypothesis testing, and evidence gathering is associated with higher costs (incurred due to more frequent experimentation rounds or pivots) in a resource-constrained context like Tanzania. Transcending context, experimentation creates added cost for entrepreneurs *during* the stages when they are enduring relatively long periods of negative profits before experiencing positive ones, and it may be the case that a theory-guided experiment yields greater benefits that offset these costs relative to nontheory guided experiments. Future replications of these comparisons may enable discernment of costs associated with *experimentation* (with or without a guiding theory-of-value) from operational costs of the business and incorporate a longer period to gauge potential long-term effects to assess whether the identified effects or differences across participants in both training conditions endure.

Another alternative explanation may be that entrepreneurs in the theory-and-evidence–based condition experience higher revenues and profits because projects based on a theory-of-value increase their motivations and effort investments. Although our DiD regression shows no significant increase in the number of working hours reported by entrepreneurs in the theory-and-evidence–based condition, future research may explicitly track changes in motivation and entrepreneurial effort before and after training and examine differences across treatment conditions.

Finally, entrepreneurs exposed in our theory-and-evidence–based condition may learn to be more persuasive, inasmuch as they develop a more convincing story of their business by virtue of tools they were exposed to (e.g., the Story Tree template in Figure 1(b)). The process of developing a theory-of-value may not only lead entrepreneurs to spend time reflecting on the underlying logic and interlinkages in their business model, but also manifest into better narratives that persuade their customers, suppliers, and complementors and help those ventures gain support from key audiences and scale faster than their counterparts. This may be especially critical in non-Western contexts like Tanzania, where businesses rely on informal community

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networks or cope with weak formal institutions (Bruton et al. 2024). Future research may examine whether and how this is the case.

Taken together, we hope that our call for replications (particularly in non-Western contexts) and extensions that shed light on sources of variation and underlying mechanisms will fuel additional scholarship that builds out the theoretical importance and practical relevance of entrepreneurial training programs that combine theory and evidence. Collectively, such studies can then inform policy and practice for designing training programs that help entrepreneurs across developed and developing economy contexts unleash the power of their ideas for personal, economic, and social gains.

Conclusion

Our study addressed the important question of whether incorporating a theory-of-value into the decision-making process helps entrepreneurs navigate uncertainty and make better strategic decisions. Using a field experiment, we compared the effectiveness of entrepreneurs trained in evidence-based versus theory-and-evidence–based approaches. In doing so, we provided an explanation for the value-add of a theory-of-value: The theory-and-evidence–based approach enabled entrepreneurs to develop a more holistic representation of the strategic problem they face, create and test hypotheses more holistically, and interpret the experimental evidence to update beliefs in a more integrated way. We hope that our study will inspire researchers to further explore the effects of theorizing on decision-making in different contexts and examine the mechanisms underlying these effects.

Endnotes

[1] The example is fictitious, but loosely inspired by the firm KopaGas (not involved in our study).

[2] To set a threshold, entrepreneurs identify their belief of the minimum value needed for a hypothesis to be considered true and adjust it based on participation in the test (e.g., increase the threshold if there is a low number of respondents to avoid false positives and have more confidence in the results). Online Appendix A provides details on defining thresholds.

[3] As with an evidence-based approach, entrepreneurs taking a theory-and-evidence–based approach may also represent their ideas using a BMC. With a theory-of-value, the entrepreneur has a guiding framework to compile the BMC, make sense of what needs changing as they gather evidence via experimentation, and understand why the change should be made when a hypothesis turns out to be false.

[4] See, for example, Leatherbee and Katila (2020) on the U.S. National Science Foundation's Innovation-Corps' lean startup training and Camuffo et al. (2024) for large scale RCT replication of the scientific approach to decision-making.

[5] We pre-registered our experimental design with the American Economic Association's RCT Registry (ID: AEARCTR-0007560; https://www.socialscienceregistry.org/trials/7560). We provide information on any deviations from the preregistration plan in the relevant sections below.

[6] Instructors were recruited within the local institution, where we relied on co-authors' experiences in academia and agricultural entrepreneurship. To "train the trainer," we conducted an intensive online training comprising eight sessions of about four hours each (32 hours in total) that covered all materials instructors taught in class. We ensured instructors absorbed and mastered the content of both approaches by running mock lecturing sessions, providing additional case studies, creating progress checkpoints, and arranging dedicated Q&A sessions.

[7] Although recruitment efforts adhered to our pre-registration plan, outcomes deviated from the intended numbers of participants and their locational characteristics. Our preregistered intent was for a sample size of 225 participants (75 in each experimental condition and control group). As discussed later, this deviation resulted in a nonrandom control group.

[8] Moving participants after the randomization lowered the risk of randomization failures. If by chance people belonging to a pair/group of friends were already allocated to the same experimental condition, we did not make any change. In total, five people were moved to the evidence-based treatment and seven people to the theory-and-evidence–based treatment. There are 11 pairs/groups in the theory-and-evidence–based treatment, and 9 in the evidence-based one. A t-test reveals no differences in the number of entrepreneurs paired between conditions ($t = 0.6311$, $p = 0.53$).

[9] These manual adjustments occurred after three entrepreneurs showed up to the wrong sessions. Specifically, one entrepreneur not admitted to training showed up to Session 1 for the evidence-based group, one entrepreneur assigned to theory-and-evidence–based training showed up to the evidence-based session, and one entrepreneur assigned to the evidence-based training showed up for the theory-and-evidence–based session. In principle, these are three cases of non-compliance. However, we allowed these swaps given that they occurred at the very beginning and because these marginal changes did not affect the balance of samples. Robustness tests of all the models using the "original" post-randomization variable for ITT results regressions reveal fully consistent results.

[10] Given higher than anticipated costs/logistical challenges, we revisited the preregistered plan of collecting 10 datapoints and prioritized collecting high-quality data for a shorter period of time over having more observations of lower quality.

[11] Both surveys and interviews were conducted in Swahili. Research assistants received intensive training on interview techniques (four sessions of about 3 hours each for a total of 12 hours), including how to conduct a phone interview and code qualitative information using predetermined scales and measures. We ensured RAs' comprehension by running mock interviews, assigning at-home tasks and creating progress checkpoints. Moreover, dedicated Q&A sessions were complemented by direct communication tools (using mobile group chats) wherein RAs and the research team could solve issues or questions in real time. Additional meetings were held throughout the program to verify the quality of data collection efforts. Because RAs were assigned entrepreneurs that belonged to only one (or the other) experimental condition, they had no exposure to any observed differences in strategies and outcomes across experiment conditions.

[12] Data on termination was gathered (a) through the following survey question at every data collection point after the baseline: "Since the first session of the [training program], have you made any changes to the business idea you've applied with?" to which respondents could select as a response "Yes, I have quit the project I applied with" and (b) in each RA interview. Online Appendix E3 clarifies how reliability of termination data were assessed.

[13] To collect data on revenues over time, we asked participants the following survey question at every data collection point: "In the year [X], what have been the Total Sales of your [business idea/entity] as a whole?" For established firms only, we added a follow-

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up question asking: "What fraction of these Total Sales could be directly related to the business idea [you'd like to develop/you are currently developing] in the course? (Please choose 0 if the business idea is not yet contributing to your Sales)." Similarly, for costs we asked: "In the year [X], how much have you spent on expenses for your [business idea/entity] as a whole (including raw materials, utilities, services for business use, wages)?" We added the following question for established firms only: "What fraction of these expenditures could be directly related to the business idea [you'd like to develop/you are currently developing] in the course? (Please choose 0 if you have not spent anything on the business idea)." To conduct checks on the survey data, we asked RAs to ask the same questions during their interviews. Specifically, RAs asked the following question concerning cumulative revenues: "Since January 1st [Year X], has the project you are currently working on (i.e., the startup or innovative project you are working on in the course) generated any sales? [If yes] How much?" For costs, they asked: "Could you please tell me approximately how much you have spent on expenses for your business idea (including raw materials, utilities, services for business use, wages) since January 1st [Year X]?" Please see additional details on the survey and interview questions related to our outcome measures in Online Appendix D.

[14] We did not preregister winsorization of the performance variables.

[15] In each interview post baseline, RAs asked: "Considering the activities that have occurred since the last call in [Month X], have you made any changes to your business model? (i.e., the business model of the startup or innovative project [the respondent is] currently working on in the course)." If the respondent indicated changes, RAs were instructed to ask: "What has changed?"

[16] This distinction was preregistered. In the pre-registration, we inadvertently omitted competitive advantage from the *Core Changes* category, although it was always our intention to include it in the analysis given that competitive advantage captures the expected advantage of the unique value proposition relative to competitors.

[17] For the cross-sectional analysis, this variable is set equal to if the entrepreneur introduced both core and operational changes to the business model at least once, regardless of the data collection period. For the panel analysis, this variable is set equal to one if the entrepreneur introduces both core and operational changes within a given period.

[18] Given the nonrandomness of the control group, these tests are relegated to the Online Appendix but nonetheless provide additional legitimacy and face validity to the manipulation checks.

[19] Models including controls have lower p-values than models without controls. This signals that regressions including instructor and project-type dummies capture a larger portion of variation, isolating a within-effect that is less noisy than the averaged one accounted by models without those dummies.

[20] Although not the subject of formal analysis in the paper, Table G1 in the Online Appendix reveals that for the theory-and-evidence–based approach, coordinated changes are associated with higher average revenues relative to only one type of change or no changes, while the evidence is less conclusive for average profits. We leave it up to future research to examine these relationships in greater detail, including the source of observed differences in average revenues versus profits (e.g., higher immediate costs of implementing coordinated changes that increase revenues, volatility in profits, etc.).

[21] Such studies would help uncover whether the two types of training provide differential economic value when controlling for potential differences attributable to contexts, sampling, design, measures, etc. For example, even among the studies on business training programs reviewed by McKenzie (2021) and McKenzie and Woodruff (2014), there are systematic differences in these attributes, and not surprisingly, the studies show different economic outcomes. Similarly, there may be systematic differences in the samples. Several studies reviewed in McKenzie (2021) have samples of participants with low education levels (e.g., fewer than 12 years of schooling) versus our sample where 78% report having tertiary education. Moreover, many of the samples in the studies examining business practices typically exclude the agriculture sector.

[22] We appreciate our editor's and reviewers' advice to address these alternative explanations.

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