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A scientific approach to entrepreneurial decision-making: Large-scale replication and extension

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Abstract

Research Summary: This article runs a large-scale replication of Camuffo and colleagues in 2020, involving 759 firms in four randomized control trials. The larger sample generates novel and more precise insights about the teachability and implications of a scientific approach in entrepreneurship. We observe a positive impact on idea termination and results that are consistent with a nonlinear effect on radical pivots, with treated firms running few over no or repeated pivots. We provide a theoretical interpretation of the empirical results: the scientific approach enhances entrepreneurs' efficiency in searching for viable ideas and raises their methodic doubt because, like scientists, they realize that there may be alternative scenarios from the ones that they theorize.

Managerial Summary: The findings of this article, based on four randomized control trials involving 759 firms, offer new insights into how entrepreneurial practices can benefit from a scientific approach to decision-making. Key outcomes include an increase in the termination of ideas and a nuanced influence on the tendency to make strategy changes. Specifically, firms that adopted a scientific approach made a few strategic shifts, as opposed to either not changing or constantly changing their strategy. We suggest that this is due to the scientific approach helping entrepreneurs be more efficient when searching for valuable ideas, as well as being more careful in selecting those ideas.

KEYWORDS

decision-making, entrepreneurship, randomized control trial, scientific approach, start-up

1 | INTRODUCTION

How should entrepreneurs make decisions under uncertainty? Entrepreneurship research and practice suggests that entrepreneurs often do not rely on solid routines or methods to make these decisions. For instance, a comprehensive large-scale survey by Bennett and Chatterji (2023) shows that entrepreneurs often fail to take even the lowest-cost steps to discover the true value of their ideas. This indicates the importance of studying structured methods that can lead entrepreneurs to improve the quality of their strategic decisions.

Against this background, a recent stream of research has advanced the idea of a scientific approach to decision-making (Camuffo et al., 2020; Felin & Zenger, 2017; Zellweger & Zenger, 2023). This approach claims that, once entrepreneurs have identified a problem that they believe is worth investigating (Baer et al., 2013; Nickerson & Argyres, 2018; Nickerson & Zenger, 2004), they benefit from developing a theory of the problem and from articulating hypotheses that logically flow from that theory. These predictions should then be tested rigorously. This approach is called "scientific" because it resembles the exploratory approach used by scientists in their research. It leverages multiple streams of strategy research, including those that emphasize the importance of problem framing, discovery, and formulation (Nickerson & Zenger, 2004); the role of mental representations in navigating uncertain environments (Csaszar & Levinthal, 2016; Eisenhardt & Bingham, 2017; Felin et al., 2020; Levinthal, 2017; Ott & Eisenhardt, 2020); and the importance of experimentation and testing to mitigate uncertainty (Ghosh et al., 2020; Kohavi & Thomke, 2017; Koning et al., 2022; Murray & Tripsas, 2004; Ott et al., 2017). The main thrust of this approach is that it improves decisions because it ensures that they are based on a more accurate understanding of the contingencies that will lead to higher performance.

Despite the promise of this approach, empirical evidence is limited. A lively recent debate in the entrepreneurship literature (Sergeeva et al., 2021; Zellweger & Zenger, 2022) highlights two relevant issues. First, the validity of the scientific approach, that is, whether entrepreneurs using such an approach "*act differently, and whether the outcomes that result are superior*" (Zellweger & Zenger, 2022, p. 698), is not yet clear. Second, there is not sufficient evidence about whether a scientific approach can be effectively taught to entrepreneurs.

2 | STUDY MOTIVATION

To date, only one small-scale study has been published in this area (Camuffo et al., 2020). It reported that entrepreneurs taught to use a scientific approach to decision-making were more likely to terminate their projects and to pivot radically. But the study's small scale and specific institutional setting create natural boundaries to the credibility and generalizability of these results. A large-scale replication in other settings is therefore critical to establish the validity of this approach.

Our research design combines four randomized control trials (RCTs), including Camuffo et al.'s (2020), which was carried out in Milan in 2016. The three additional RCTs apply the same intervention (with minor adaptations due to operational and contextual constraints) across different contexts and time windows (Milan in 2017, Turin in 2018, London in 2019). Overall, we analyze data for 759 firms over time (11,463 data points). Notable studies have implemented similar large-scale RCTs across contexts (Allcott, 2015; Banerjee et al., 2015; Bowers et al., 2017; Davis et al., 2023).

We follow Bettis et al.'s (2016) recommendation that replication studies should move "in stages," altering the original design and context incrementally to understand how these changes affect the original detected impact. In line with this guidance, RCT2 was conducted in the same context as the original study (Milan, Italy) but in a different year (2017). RCT3 was conducted in Turin, Italy (2018), to reach a potentially different population of "tech" entrepreneurs (Turin is a technological hub). RCT4 was conducted in London, United Kingdom (2019) and extended to a broader population that did not include only early-stage start-ups.

3 | PREVIEW OF RESULTS AND CONTRIBUTION

Our results are generally consistent with Camuffo et al. (2020), which points to the robustness across contexts of the effects associated with the use of a scientific approach. Moreover, the large scale of our study allows us to detect effects non-detectable or negligible in smaller samples. Overall, we offer the following novel findings.

First, differently from Camuffo et al. (2020), we find a positive and precisely estimated effect of the intervention on idea termination across all three new RCTs; Camuffo et al.'s (2020) limited sample size did not allow them to detect this effect consistently.

Second, whereas Camuffo et al. (2020) found the effect of the intervention on the number of radical pivots to be positive and very precise, we instead find evidence in line with the existence of a nonlinear effect of the intervention on the number of radical pivots. The treatment increases the likelihood to pivot radically once or twice, but decreases the likelihood to not pivot radically at all or to do so more than twice. In Camuffo et al.'s (2020) small sample, only a few firms pivoted radically more than once, preventing the authors from observing the nonlinear effect of the treatment on pivots. In this larger sample we find stronger evidence that a scientific approach to decision-making leads firms to pivot in a more "focused" way (once or twice) and avoid radically pivoting indefinitely. In supplementary analyses, we also show that the pivoting pattern of scientific entrepreneurs correlates with higher performance.

Third, compared to Camuffo et al. (2020), our larger sample improves the precision of the effect estimates of the intervention on performance.

Fourth, the larger scale of this study enables us to go beyond the intention-to-treat estimates in Camuffo et al. (2020). We ran an instrumental variable analysis that estimated the effect of the actual adoption of the scientific approach (measured by an index of scientific intensity) instrumented by the intervention. While this estimation rests on an exclusion restriction, that is, the instrument affects the dependent variables only through the index, it increases confidence that the effects we detect reflect treatment compliance (adoption of the scientific approach) rather than a generic effect of the treatment. We also provide evidence of the robustness of our results using models that account for the joint determination of the key variables.

Fifth, we show consistency in terms of sign and magnitude of the estimates of the effect of the intervention across all the RCTs. Of course, their degree of precision varies due to the randomness across contexts and the scale of each RCT. However, the similarity of the estimates across RCTs and the stronger precision of the aggregated results in the combined sample point to consistent effects of the scientific approach across contexts.

Overall, this article offers two contributions. First, it responds to the call for systematic replication to consolidate the credibility of preliminary findings in entrepreneurship and strategy research and increase their generalizability (Bettis et al., 2016). A large-scale replication enables us to: (1) address issues of sensitivity to scale (Angrist & Pischke, 2010; King et al., 2021; Levitt & List, 2009; List et al., 2017); (2) understand variation in treatment effects across different geographical, industrial, and institutional contexts (Hsu et al., 2017; Milkman et al., 2021; Patel & Fiet, 2010); and (3) reveal novel insights otherwise undetectable with smaller experiments (Camerer et al., 2016; Crawford et al., 2022; Tsang & Kwan, 1999).

Second, our results contribute to the emerging research program on the scientific approach to entrepreneurial decision-making (Camuffo et al., 2020; Zellweger & Zenger, 2023, 2022). The evidence we provide is consistent with the thesis that entrepreneurs can be taught to reason in a scientific way and that this type of training positively affects their performance in terms of termination of non-promising projects, focused pivoting, and higher performance. These results resonate with extant entrepreneurship research stressing the superiority of decision-making approaches that combine cognition with action to learn under uncertainty (McDonald & Eisenhardt, 2020; Ott et al., 2017).

Related to this second point, we contribute beyond Camuffo et al. (2020) by providing a more precise theoretically-based interpretation of empirical results. Through abductive reasoning (Behfar & Okhuysen, 2018; King et al., 2021; Pillai et al., 2020), we conceptualize two plausible mechanisms associated with a scientific approach: *efficient search* and *methodic doubt*. The mechanism of *efficient search* refers to the higher efficiency of scientific entrepreneurs in scanning the space of possible solutions to their business problems. This is achieved thanks to their heightened ability to prioritize ideas that are more likely to be successful. The mechanism of *methodic doubt*, meanwhile, raises their awareness of contingencies that could make ideas less likely to succeed. Entrepreneurial ideas are considered valuable only in the presence of thorough reasoning, compelling arguments, and consistent evidence. We discuss why our empirical results—higher termination and focused pivoting for "scientific" entrepreneurs—are consistent with a framework in which the latter mechanism dominates the former, raising terminations, and the prioritization of better ideas implied by the efficient search leads to more focused pivots (pivot once or twice rather than no or many pivots).

In the next section, we provide the background of the scientific approach to entrepreneurial decision-making and report the key results of Camuffo et al.'s (2020) study. We then describe the research design, data, and method used in our empirical investigation and report the analyses and findings of the experimental studies. Following our theoretically-based interpretation of the empirical findings, the final section discusses the theoretical and practical implication of our replication study.

4 | THEORETICAL BACKGROUND

4.1 | A scientific approach to entrepreneurial decision-making

To navigate uncertainty throughout the entrepreneurial process, entrepreneurship research has recently developed a systematic approach that broadly addresses the idea that entrepreneurs should behave like scientists (Agarwal et al., 2023; Camuffo et al., 2020; Zellweger & Zenger, 2023). This approach encourages entrepreneurs to develop theories that help them to envision a future state space, comprising selected attributes and the causal relationships among them, to which they assign a belief (Camuffo et al., 2023; Ehrig & Schmidt, 2022; Felin & Zenger, 2009, 2017).[1] These theories are then used as a guide to determine whether and when to collect evidence that can validate or update such beliefs as well as determine the design of the experiments to be conducted. Beliefs on a theory can vary in their strength and, for simplicity, they can be thought of as subjective probabilities.

The scientific approach to entrepreneurial decision-making is tightly coupled with the scholarly work on problem framing, discovery, and formulation (Baer et al., 2013; Cummings & Nickerson, 2024; Nickerson et al., 2007; Nickerson & Argyres, 2018; Nutt, 1998; Park & Baer, 2022). Belief formation through theorizing is the first step in the entrepreneur-as-scientist approach, and it involves engaging in deliberate cognitive efforts to frame a problem (Park & Baer, 2022) and carefully formulate it as a conceptual structure (Baer et al., 2013; Felin & Zenger, 2009; Zellweger & Zenger, 2022). Theories are then translated into testable hypotheses (Leatherbee & Katila, 2020), which are tested through experiments. These experiments elicit signals that provide evidence about the hypotheses and their assumptions (Camuffo et al., 2020). This information is then incorporated, following a Bayesian updating process, into updated beliefs (Zellweger & Zenger, 2023), leading entrepreneurs to make more informed decisions about whether to continue with the current plan, pivot to a different business model, or terminate the venture (Camuffo et al., 2020; Ries, 2011).

4.2 | Mechanisms

The positive effects of the adoption of the scientific approach to entrepreneurial decision-making are driven by two mechanisms: efficient search and methodic doubt. The former mechanism refers to the increased efficiency of scientific entrepreneurs in their quest for solutions to

[1]We define theories as systems of ideas or concepts intended to explain, predict, or hypothesize the existence of a phenomenon and which are based on general principles independent of the phenomenon whose existence is explained, predicted, or hypothesized. They comprise two components: a future state space and beliefs. The future state space derives from a causal structure that captures the number of, type of, and causal relationships among the conceptual elements of the theory. Beliefs represent a restriction on the future state space and capture the "plausibility" of the theory, that is, the subjective evaluation of the extent to which the theory is likely to envisage possible and valuable future states (Camuffo et al., 2023).

a specific business problem (Felin et al., 2020; Felin & Zenger, 2017). The development of theories helps entrepreneurs clearly articulate the characteristics of the problem being targeted. Once the key dimensions of the problem are clear, the characteristics of the potential solutions become less ambiguous. Thus, theory-based problem formulation changes the way in which solutions are sought, from a "searching through" to a "searching for" process (Felin et al., 2023, p. 9). It enables decision-makers to specify, ex ante, what they are looking for, and streamline the costly process of cycling through all options (Lippman & Rumelt, 2003; Rivkin, 2000). Solutions to a well-formulated problem can be more efficiently tested because experiments conducted under this condition deliver less noisy signals about the validity of the hypothesized solution or about variations to the solution that could deliver more value.

The latter mechanism refers to the fact that the scientific approach "instills" in entrepreneurs a *methodic doubt* about the problems they formulate and the solutions they evaluate. The scientific approach fosters critical thinking, contrasting decision-makers' tendencies to unawareness (Karni & Vierø, 2017), overconfidence (Åstebro et al., 2014), and the neglect of opportunity costs (Bennett & Chatterji, 2023; Chen et al., 2018). Entrepreneurs-as-scientists "*acknowledge that opportunities are hard to uncover as entrepreneurs have legitimate doubts that opportunities they see are in fact valuable*" (Zellweger & Zenger, 2023, p. 381). In other words, the scientific approach makes entrepreneurs more likely to identify contingencies that might reduce the probability that their idea will be successful. In doing so, it raises the threshold entrepreneurs set to consider ideas as valuable, meaning that they do so only in the presence of thorough reasoning, compelling arguments, or consistent evidence (Kerr et al., 2014; Koning et al., 2022).

4.3 | Existing evidence

The empirical evidence about the effects of a scientific approach to entrepreneurial decision-making is limited to the work of Camuffo et al. (2020). Their study exposed a small sample of 116 entrepreneurs to a training program that taught founders to "behave as scientists" when making decisions.

Camuffo et al.'s (2020) main proposition is that entrepreneurs who adopt a scientific approach are more likely to terminate their projects ("exit," in their terminology) and to pivot radically. Termination is interpreted as a positive outcome because it prevents entrepreneurs from pursuing projects that are not valuable, saving crucial resources that could be used in better endeavors. Investing in entrepreneurial projects with no potential—a type I decision error or false positive—entails significant opportunity costs at the individual and collective level.

The logic explaining why the adoption of the scientific approach leads to termination is based on the idea that it makes entrepreneurs more likely to reach a fair assessment of the value of their projects and recognize those with a low probability of success, avoiding biases of overconfidence and judgment variability that often plague their decisions (Åstebro et al., 2014; Camerer & Lovallo, 1999; Kahneman et al., 2011). In addition to termination, Camuffo et al. (2020) focused on radical pivoting, which they defined as strategic changes to the original business model (Kirtley & O'Mahony, 2023) in terms of value proposition or customer segment. They showed that entrepreneurs who use a scientific approach radically pivot more. The authors suggest that this happens because the approach makes entrepreneurs more efficient in redirecting search to portions of the space of possibilities that are more promising and valuable. Finally, Camuffo et al. (2020) estimate the impact of the scientific approach on performance, showing that it leads to increased revenues.

Despite the novelty of Camuffo et al.'s (2020) study and its value from a research and practice perspective, its small scale limits the generalizability of its findings. Furthermore, the small sample constrained the authors' choice of empirical specification and estimation methods. Moreover, Camuffo et al. (2020) do not provide detailed insights about the mechanisms through which a scientific approach to decision-making generates higher termination, pivots, and superior performance.

5 | RESEARCH DESIGN, DATA, AND MEASURES

5.1 | Experimental design

We focus on entrepreneurs who have identified a problem and investigate how exposure to a scientific approach helps them to better formulate and address this problem. The dataset we analyze includes the data from the original study by Camuffo et al. (2020) (henceforth, RCT1) as well as three additional datasets deriving from three RCTs that replicate the original study's design (henceforth RCT2, RCT3, and RCT4). RCT1 was conducted in Milan, Italy (2016). As discussed earlier, we selected the contexts of the additional studies following Bettis et al. (2016), who posit that "quasi-replications" that test the impact of specific research designs in different contexts and populations "*hold especially strong promise for the field of strategic management*, *because* (*they*) *inform us about how well results hold up in multiple settings*" (p. 2196). In line with this guidance, we conducted RCT2 in the same context as the original study (Milan, Italy) but in a different year (2017). We conducted RCT3 in Turin, Italy (2018), to tailor the intervention to a different population of entrepreneurs. While Milan offers entrepreneurs a relevant economic ecosystem, Turin is a technological hub, which also extends the study to a more varied pool of industries. We conducted RCT4 in London, United Kingdom (2019), with the goal of targeting a broader pool of potential applicants not restricted to early-stage startups. Thus, overall, the four RCTs cover a set of varied entrepreneurial types, ranging from entrepreneurs in non-high-tech sectors (RCT1 and RCT2), to high-tech entrepreneurs (RCT3) and more established small firms (RCT4).

To replicate RCT1, we organized the three new RCTs as two-arm field experiments in which participant firms were randomly assigned to a "treatment" and a "control" group. Both groups underwent an intervention (a training program), but only firms in the treatment group were trained to adopt the scientific approach. We observed and compared over time the pre/post-intervention differences in behaviors, decisions, and performance between firms in the treatment and control groups. Taken together, the four RCTs involved 759 firms and 11,463 data points.[2] Sections 1 and 2 in the Online Appendix report results of the randomization checks and the management of attrition.

5.2 | Recruitment of participants

As in Camuffo et al. (2020), we advertised each program at a national level over multiple online and offline channels, including social media posts, newsletters, magazines, and events. One of the reasons why these programs were appealing to potential applicants is that they were

[2]All RCTs are pre-registered at the AEA registry and subjected to the relevant IRB/Ethics Committees for approval. Detailed information about each RCT is available upon request.

advertised and delivered under the brands of some of the host countries' top business schools. In addition, the instructors were experienced mentors, which reinforced the perception of the value of the programs. We conducted each advertising campaign over several weeks. Regardless of the media used, the campaign promoted the program as a management training program (to avoid self-selection based on interest in a specific topic) offered free of charge to firms operating in any industry. Potential application was not restricted in any way, so as to comply with standard anti-discrimination ethical guidelines for RCTs. In the application process, participants were required to provide information about their business, team, and decision-making practices via an online survey and a brief telephone interview. We did not admit to the program applicants who failed to complete the survey or the interview.

5.3 | Sample composition and participants' characteristics

The previous sections have already provided some information about the characteristics of the firms involved in the four analyzed RCTs in terms of industry and stage of development. We provide more details about the participants in Section 3 of the Online Appendix (Tables A7 and A8), which provides descriptive statistics and pairwise correlations about meaningful covariates for the samples of the three new RCTs. On average, entrepreneurs involved in these new RCTs held at least a Bachelor Degree and were in their 30s; 65% of them were male, and 36% had a STEM background (positively correlated with male gender). Their average entrepreneurial experience was 2.46 years, while they had 4.51 years of industry experience and 3.66 years of managerial experience. Table A9 of the Online Appendix provides an industry breakdown of the participants for each RCT and the total sample. We used the NACE Rev. 2, the official statistical classification of economic activities in the European Community, to classify firms into sectors. More than 70% of participants' firms were in the service sector with a strong presence of ICT services (20.82%); professional, scientific, and technical activities (15.15%); food services (18.05%); and manufacturing (11.59%). The three new RCTs covered a broader range of sectors than Camuffo et al. (2020). For instance, there were no firms in the art sector in the three RCTs conducted in Italy, while there were 23 firms in this sector in the RCT run in the UK. The new RCTs also covered other sectors under-represented in RCT1, such as agriculture; forestry and fishing; education; human health and social work activities; and professional, scientific, and technical activities, improving the cross-sector representativeness of the full sample of firms.

5.4 | Intervention details

The intervention followed Camuffo et al. (2020). In each RCT, we assigned firms to either a treatment or a control group through simple randomization. We also broke down the treatment and control groups into smaller classes/learning groups and randomly assigned each subgroup to an experienced instructor. The baseline survey administered to participants prior to the intervention provided a wide array of observable characteristics, which we used to test whether the composition of the two groups was balanced. As the four RCTs were conducted asynchronously, the research team was able to introduce additional relevant dimensions to the baseline survey over time. As a result, the list of observables is larger for later RCTs.

Treatment and control groups attended the same number of training sessions, covering the same topics related to strategy and entrepreneurship. The sessions were highly experiential, and the small learning-group size ensured that instructors provided feedback to each participant. In every RCT, each instructor taught both a treatment and a control group at different times of the day or different days of the week. This choice allowed us to control for cross-instructor differences (e.g., teaching styles) in our analyses (by introducing instructor fixed effects) that might affect the absorption of the content taught to participants. Since instructors were not blind to the treatment, we directly supervised the delivery of each session to ensure high teaching standards and that the content was in line with the experimental design described above. We prevented cross-condition contamination by ensuring that participants in the experimental and control groups did not meet and potentially share key elements of the treatment. For example, we offered the training sessions of the two groups on different days of the week or on the same day of the week but at different times of the day. To further prevent contamination, the research team kept all communication to the two groups of decision-makers attending the program discrete and separate. For the same reason, the research team checked if applicants to the program had any acquaintance with other applicants and allocated those that did to the same experimental group.

5.5 | Training content and differences between treatment and control

The guiding principle across all three training programs was to teach the same frameworks and techniques to entrepreneurs in the treatment and control groups but to teach each group a distinct approach to those frameworks and techniques. Control entrepreneurs were taught to apply them in a traditional way, while treated entrepreneurs were taught to use them to develop theory and hypotheses, to test the latter, and to evaluate the results.[3] Specifically, both treated and control groups were taught frameworks that they could use to support decision-making, such as the business model canvas (BMC) or the balanced scorecard; both groups were also exposed to evidence-gathering techniques such as qualitative interviews, surveys, and A/B testing. Both groups were taught to apply these frameworks and techniques to their specific contexts and were given feedback from their peers and instructor.

The key difference between the two groups was that the treatment group was taught to apply the frameworks and techniques in accordance with a scientific approach to decision-making, that is, by developing a theory of the problem and logically deriving hypotheses from it that they could then test through rigorous experiment or equivalent methods, revising their beliefs based on the gathered evidence. The control group, instead, was free to apply these frameworks and techniques in the way they found most appropriate.

5.5.1 | An example

An example clarifies the difference between the two groups. One of the first sessions of the training program focused on entrepreneurs' business models. As part of that session,

[3]The content for RCT2, 3, and 4 included material that was comparable to that used in Camuffo et al. (2020), with the exception of some modifications required to adapt the program to the local constraints and to incorporate the learning from the previous programs. For example, the number and time of the sessions was adapted to comply with the availability of facilities, and the selection of frameworks was adapted to the type of participants. All of this applied equally to treatment and control groups. Table A10a,b in the Online Appendix provides more detail regarding the content taught to participants in RCT2, 3, and 4 and the differences between treated and control groups' training.

entrepreneurs were taught the BMC, a tool widely used in business education that concisely and visually represents a company's business model. It is composed of nine elements that describe a firm's customer value proposition, customer segments, channels, customer relationships, revenue streams, key resources, key partners, key activities, and cost structure. The control group was exposed to the basic content of the BMC and was taught to use this tool to develop a general overview of their business and discuss its implications with peers. This is the typical way in which the BMC is taught in MBAs and Executive programs.

The treatment group, too, was exposed to the basic content of the BMC and asked to apply it to their business and to discuss it with their peers. However, different from the control group, participants in the treatment group were explicitly nudged to articulate a theory about why the elements included in the BMC would contribute to value creation, how they were logically connected, what the underlying conceptual causal structure was, and why these elements, as a whole, would generate value for customers. Additionally, treated participants were asked to explicitly formulate testable hypotheses based on their BMC.

In subsequent sessions, participants in both groups were taught techniques to collect data to better inform their decisions. For instance, they were taught about qualitative interviews, surveys, and experiments; the strengths and weaknesses of each of these methodologies; and the conditions under which they can be applied effectively. Participants in both groups were invited to think about which techniques they could use in their businesses and discuss with their implementation with their peers and instructor. The control group was free to choose how to apply these techniques and was given general feedback about how the technique was applied. The treatment group was instead explicitly invited to use these techniques to test the hypotheses formulated in the previous sessions and was given feedback based on whether the proposed design was consistent with the hypotheses they set out to test.[4]

All participants received genuine and valuable feedback. For example, if a participant proposed to administer a survey to a very small sample of target customers, the instructor would recommend increasing the sample size irrespective of whether they belonged to the treatment or the control group. If a participant formulated a survey question in a way that could be improved in terms of clarity, they would be offered suggestions regarding how to improve it, irrespective of whether they were in the treatment or in the control group.

5.6 | Data collection

Large teams of research assistants systematically collected data on the RCT participants through telephone interviews conducted over the span of several months. We hired research assistants

[4]The following example illustrates how treated entrepreneurs articulated the theories underlying their ideas. An entrepreneur participated to the UK RCT (RCT4) and joined the training program with an idea centered around selling consumer electronics in a customer-friendly way. As a result of his participation in the treatment group, he formulated the problem he was facing by articulating a theory grounded in the belief that "*customers are not technologically savvy and all they care about is solving their specific problem*" and that, consequently, "*in order to make them happy and be on top of competitors, we have to make things very simple, paradoxically 'hiding' technical information that are* (sic) *redundant to them*." He also believed that "*in order to increase sales we must not advertise products, but solutions*." This entrepreneur set out to test the hypotheses derived from this theory to "*see whether the theory is going to work*." Among others, he ran an experiment on a specific product line: "*We started advertising some memory cards as solutions for specific devices* (…). *We found out that these cards started to sell much better than the others which were instead advertised as products* (…). *Right now memory cards represent a third of our business, and we are selling 5000 of them a month.*"

for the purpose of these experiments and the research team trained them extensively. Research assistants were undergraduate or graduate students selected based on their academic performance, basic knowledge of the entrepreneurial process, and communication and analytical skills. The research team interviewed research assistants and tested their communication and analytical skills through various activities (analysis of a business case, interviewing a participant and coding responses according to a simple, predefined coding scheme) to ensure they would be able to perform the tasks required by the project.

Research assistants had regular phone calls with participants that followed a predefined script of open- and closed-ended questions focusing on changes to the business model, decision-making, and performance outcomes. We targeted an interval of about 4 weeks between each interview, with some variation due to entrepreneurs' availability. In all three replication RCTs, we recorded telephone interviews and subjected them to random checks to ensure that research assistants were conducting calls in accordance with the guidelines provided by the research team.

The main variables used in this study refer to outcomes such as project termination, radical pivot, and amount of revenue and were therefore collected through closed-ended questions. Following an approach similar to that illustrated by Bloom and Van Reenen (2010), we also included a number of open-ended questions that elicited, without leading, what type of approach to decision-making each entrepreneur was using. Specifically, we instructed research assistants to code entrepreneurs' interviews for the occurrence and intensity of themes related to theory, hypotheses, tests, and evaluation. We provide more details about this in Section 5 of the Online Appendix. The data collection process continued for up to 18 observation points after the beginning of the program, corresponding to up to 16 months. In one of the RCTs (RCT4), we could only collect observations for 9 months after the beginning of the program due to funding constraints. We consider the duration of the data collection process in discussing our results.

5.7 | Measures

Following Camuffo et al. (2020), we focus on the following dependent variables.

Termination (referred to as "exit" in Camuffo et al., 2020) is a dummy equal to 1 if the firm terminated the project within the observation window, and 0 otherwise.

Number of Radical Pivots is the number of times the firm pivoted radically within the observation period. We calculated this variable from information collected during the interviews. During each interview, we referred to the BMC taught to decision-makers during the training program and asked decision-makers to describe any changes made to any of the nine dimensions of the BMC (value proposition, customers, channels, customer relationships, key activities, key partners, key resources, revenue streams, cost structure). We detected the presence of a radical pivot at time *t* if the firm reported a major change to its value proposition or customer segment, two key dimensions of its business. In general, entrepreneurs pivoted in the most literal sense: standing on some of their past knowledge and focusing and turning toward new factors that changed their overall product or business (Hampel et al., 2020). Table A12 in the Online Appendix provides examples of radical pivots.

Performance is measured as the firm's cumulative revenue in EUR at the date of the last observation available for each trial since the beginning of the training. This represents a different time period for each RCT. However, to control for these differences, we included a set of RCT dummies in our analyses.[5]

The key independent variable is *Intervention*, a dummy equal to 1 if the firm was in the treatment group, and 0 otherwise. We included a set of RCT dummies in our analyses to control for any difference across RCTs. We also employed instructor dummies.

5.8 | Methodology

We structured the analyses by outcome of interest (termination, radical pivot, performance). In line with Camuffo et al. (2020), we grounded our main analyses on linear regressions (OLS) in which we estimated our dependent variables as a function of the intervention and controls. In all the specifications, we clustered the standard errors at the intervention-instructor-RCT level and included RCT dummies to control for differences across RCTs. We present the results obtained in the large-scale analyses and those obtained in the individual RCTs. We then compare results across RCTs and highlight similarities and differences. Our results do not change if we run all the analysis with standard errors clustered only at intervention-RCT level.

Table 1 reports descriptive statistics and pairwise correlations between variables of the combined large sample (759 firms). Then, 34% of the firms in our sample terminated their projects within the observation window. Firms in our sample pivoted radically at most nine times during the observation window. Then, 59% of the sample never pivoted radically, and 6% pivoted radically more than twice. The average amount of revenue is EUR 15,753, with large variation in the sample because a substantial number of firms had zero revenue within the observation window. The number of firms participating in each RCT varies across all RCTs due to financial constraints and resource availability for each one.

Figure 1a,b provides a visual representation of our data. Figure 1a shows that the number of treated firms that terminated their projects within the observation window is higher than the number of control firms. It also shows that treated firms were more likely to radically pivot once, while control firms were more likely to radically pivot more times. Finally, Figure 1b shows that, on average, the revenue of treated firms grew faster than that of control firms.

6 | RESULTS

6.1 | Termination

Table 2 shows our estimates of the impact of the intervention on termination, the first dependent variable investigated by Camuffo et al. (2020). Column (1) reports the results of a cross-sectional linear probability model that shows that the intervention raises the probability of termination by 9.8 percentage points (p.p.) ($p = .001$). In Columns (2–5), we report the results by RCT. The effect of the intervention on termination, which is weak and not very precise in the original study (RCT1), is stronger and more precise in the large-scale sample.

Table 3 reports the results of a Cox proportional hazard model in Column (1). We corroborated the proportionality assumption using the Schoenfeld residuals. We find that the hazard

[5]One source of differences could be different inflation rates across countries. The presence of RCT dummies accounts for this possible issue.

TABLE 1 Descriptive statistics and pairwise correlations.

	Variable	Obs	Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13
1	Termination	759	0.34	0.48	0	1	1.00												
2	Number of pivots	759	0.71	1.12	0	9	−0.10	1.00											
3	Not pivoting	759	0.59	0.49	0	1	0.07	−0.76	1.00										
4	Pivoting once	759	0.24	0.43	0	1	−0.01	0.15	−0.67	1.00									
5	Pivoting twice	759	0.11	0.31	0	1	−0.03	0.41	−0.42	−0.20	1.00								
6	Pivoting more than twice	759	0.06	0.25	0	1	−0.08	0.74	−0.31	−0.15	−0.09	1.00							
7	Performance (revenues)	759	15753.89	83051.78	0	1,489,026	−0.03	−0.04	0.03	−0.00	−0.02	−0.04	1.00						
8	Intervention	759	0.50	0.50	0	1	0.12	−0.03	−0.05	0.10	0.01	−0.08	0.04	1.00					
9	Average scientific intensity	759	2.23	1.23	0	5	−0.03	0.24	−0.28	0.14	0.15	0.13	0.11	0.14	1.00				
10	RCT1	759	0.15	0.36	0	1	0.03	−0.13	0.13	−0.05	−0.08	−0.08	−0.05	0.01	−0.21	1.00			
11	RCT2	759	0.33	0.47	0	1	0.11	0.07	−0.12	0.08	0.06	0.02	−0.12	−0.01	0.15	−0.30	1.00		
12	RCT3	759	0.17	0.38	0	1	−0.03	0.25	−0.22	0.08	0.11	0.16	−0.07	−0.01	0.07	−0.20	−0.32	1.00	
13	RCT4	759	0.34	0.48	0	1	−0.12	−0.17	0.19	−0.11	−0.09	−0.09	0.21	0.01	−0.05	−0.31	−0.51	−0.33	1.00

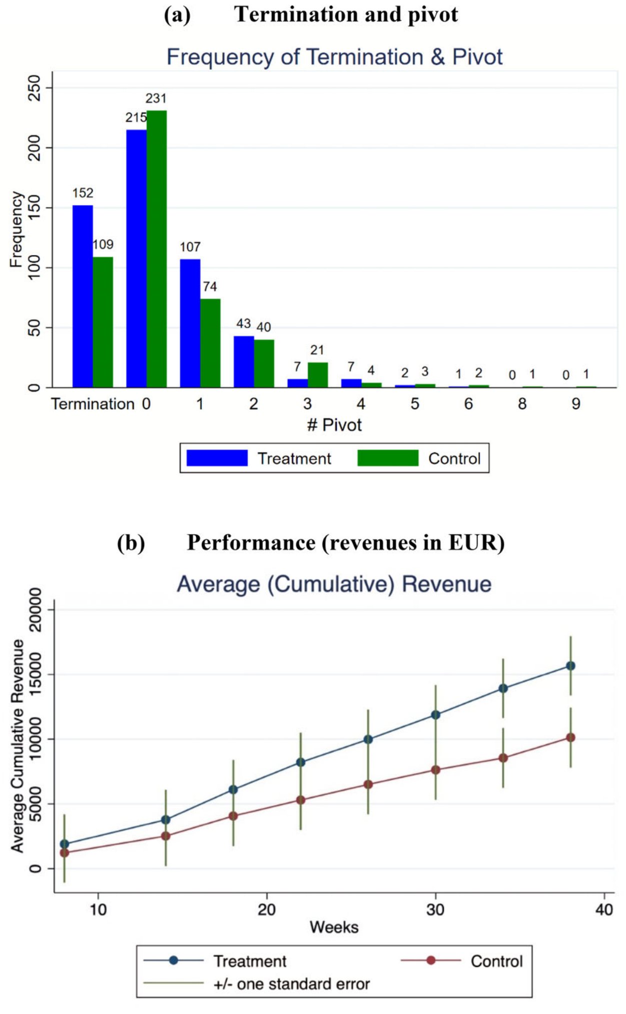

FIGURE 1 Termination, pivot, and performance.

rate of termination is higher for treated than control firms. In Column (2), we replicate this analysis using an OLS regression that predicts the week of termination. We find that, on average, treated firms terminate their projects about 2.7 weeks earlier than control firms ($p = .009$). Overall, treated entrepreneurs are more likely to terminate their projects and they do it earlier.

6.2 | Radical pivoting

Table 4 shows our estimates of the impact of the intervention on radical pivoting, the second dependent variable investigated by Camuffo et al. (2020). Column (1) reports the results of a cross-sectional regression, estimated by OLS, where the dependent variable is the number of radical pivots made by the firms within the observation window. Overall, the intervention does not have a precise linear impact on the number of radical pivots ($p = .486$). In Columns (2–5), we report the results by RCT. Interestingly, only RCT1's results reveal a linear effect of the

TABLE 2 Termination OLS cross section.

	(1)	(2)	(3)	(4)	(5)
	Termination	Termination	Termination	Termination	Termination
	OLS	OLS	OLS	OLS	OLS
	Cross section	Cross section	Cross section	Cross section	Cross section
Variables	**Full sample**	**RCT1**	**RCT2**	**RCT3**	**RCT4**
Intervention	0.098 (.001)	0.035 (.647)	0.096 (.044)	0.158 (.084)	0.097 (.035)
Constant	0.284 (.175)	0.316 (.219)	0.374 (.001)	0.730 (.012)	0.287 (.002)
Observations	759	116	250	132	261
R-squared	.076	.183	.034	.138	.026
Dummies for instructors	Yes	Yes	Yes	Yes	Yes
Dummies for RCTs	Yes	-	-	-	-
Clustered errors	Intervention instructor RCT	Intervention instructor	Intervention instructor	Intervention instructor	Intervention instructor

Note: Robust *p*-value in parentheses. Our balance checks have shown that three variables in two RCTs were unbalanced between the treatment and control group despite randomization ("Background: Economics" and "Background: STEM", for RCT 2 and "Self-regulation," for RCT3). To address this issue, in this and all subsequent tables, we conservatively included these variables as controls in all specifications that relate to the individual RCTs. In the specifications that relates to the full sample we control for the interaction between each RCT dummies and the variable that was unbalanced in that specific RCT. Results are similar when not controlling for these variables.

TABLE 3 Termination time.

	(1)	(2)
	Hazard of termination	Week of termination
Variables	**Survival—Full sample**	**OLS—Full sample**
Intervention	0.365 (.000)	−2.699 (.009)
Constant		43.350 (.000)
Observations	759	759
R-squared		.280
Dummies for instructors and RCTs	Yes	Yes
Clustered errors	Intervention instructor RCT	Intervention instructor RCT

Note: Robust *p*-value in parentheses.

intervention on the number of radical pivots. This result did not replicate in the subsequent RCTs.

Results from a multinomial probit, reported in Table 5, shed light on this finding. Columns (1), (2), and (3) refer, respectively, to the probability that a firm pivots radically once, twice, or more than twice *vis-á-vis* the no-radical-pivot baseline. In Figure 2, we show the marginal effects of the intervention calculated at the observed values for the entire sample. The

TABLE 4 Number of pivots.

Variables	(1)	(2)	(3)	(4)	(5)
	# Radical pivots	# Radical pivots	# Radical pivots	# Radical pivots	# Radical pivots
	OLS—Cross section	OLS—Cross section	OLS—Cross section	OLS—Cross section	OLS—Cross section
	Full sample	RCT1	RCT2	RCT3	RCT4
Intervention	−0.053 (.486)	0.261 (.021)	0.011 (.860)	−0.474 (.194)	−0.038 (.588)
Constant	0.693 (.175)	0.536 (.217)	1.245 (.000)	1.174 (.451)	0.435 (.000)
Observations	759	116	250	132	261
R-squared	.128	.105	.071	.073	.019
Dummies for instructors	Yes	Yes	Yes	Yes	Yes
Dummies for RCTs	Yes	-	-	-	-
Clustered errors	Intervention instructor RCT	Intervention instructor	Intervention instructor	Intervention instructor	Intervention instructor

Note: Robust *p*-value in parentheses.

TABLE 5 Pivot multinomial probit.

Variables	(1)	(2)	(3)
	Pivoting only once	Pivoting twice	Pivoting more than twice
	Multinomial probit	Multinomial probit	Multinomial probit
	Cross section	Cross section	Cross section
	Full sample	Full sample	Full sample
Intervention	0.350 (.014)	0.112 (.515)	−0.282 (.077)
Constant	−1.363 (.000)	−2.062 (.000)	−2.429 (.000)
Observations	759	759	759
Dummies for instructors and RCTs	Yes	Yes	Yes
Clustered errors	Intervention instructor RCT	Intervention instructor RCT	Intervention instructor RCT

Note: Robust *p*-value in parentheses.

intervention raises the probability of pivoting radically once or twice and lowers the probability of not pivoting radically or pivoting radically more than twice. When we look at the full sample, the intervention does not have a precise impact on the probability of not pivoting radically. It increases the probability of pivoting radically once by 8.3 p.p. ($p = .003$), does not have a precise impact on the probability of pivoting radically twice, and decreases the probability of pivoting

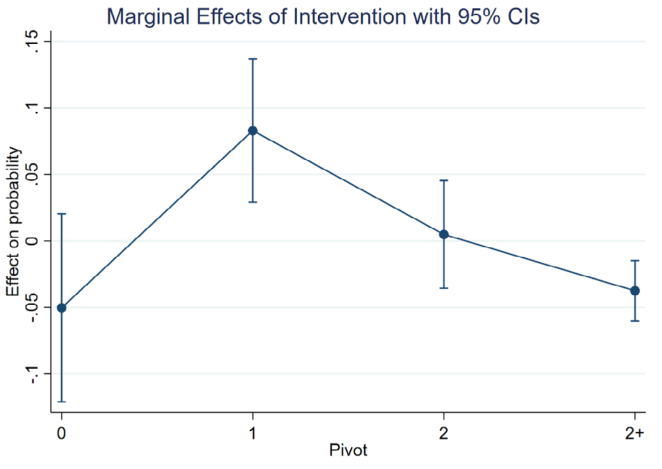

FIGURE 2 Marginal effects of intervention on pivot.

radically more than twice by 3.7 p.p. ($p = .001$). Results from the individual RCTs (not reported for brevity, but available upon request) are consistent with these patterns.[6]

Note that this result is *different* from Camuffo et al. (2020), who found that the treatment was linearly positively associated with the number of pivots. The difference between these results depends on the fact that in Camuffo et al.'s (2020) sample most firms pivoted radically only once. The sample was not large enough to include enough firms pivoting more than once, and thus could not detect the curvilinear effect. In this sample the number of firms pivoting more than once equals 11% and those pivoting more than twice 6%. Despite the number of firms pivoting twice not being extremely large, this larger sample obtained through replication enables us to see evidence consistent with the existence of a nonlinear effect, which resonates with the idea that treated entrepreneurs do not pivot indefinitely, in a trial-and-error sort of way. Rather, they pivot in a more "focused" way because, through theories and tests, they learn more efficiently which variations on their original idea are potentially more valuable. In line with this intuition, Section 6 of the Online Appendix presents additional analyses showing that treated entrepreneurs who radically pivot perform better if they do so only once. In contrast, this association is not evident in the control group.

6.3 | Performance

Although Camuffo et al. (2020) did not predict the effect of the intervention on performance, they explored the question empirically. We follow their original study and present, in Table 6, the results of OLS regressions estimating the impact of the intervention on the cumulative revenue of firms in our sample (in EUR) at the date of the last observation in each trial.

Results show that, on average, treated firms earned EUR 6999.327 more than control firms ($p = .030$). The small effect size reflects the fact that many firms in the total sample earned no revenue as they are early-stage start-ups that started their activities with our training program. Within the observation period, some of the firms started earning revenues on the order of a

[6]For RCT1, the multinomial probit model does not converge because only a few firms pivoted radically more than once.

TABLE 6 Performance OLS.

Variables	(1) Revenue OLS cross section Full sample	(2) Revenue OLS cross section RCT1	(3) Revenue OLS cross section RCT2	(4) Revenue OLS cross section RCT3	(5) Revenue OLS cross section RCT4
Intervention	6999.327 (.030)	10,799.493 (.125)	1517.117 (.136)	3253.979 (.027)	12,227.935 (.164)
Constant	−2999.664 (.369)	−4899.746 (.403)	−466.455 (.855)	5808.953 (.257)	6297.301 (.344)
Observations	759	116	250	132	261
R-squared	.085	.220	.023	.096	.036
Dummies for instructors	Yes	Yes	Yes	Yes	Yes
Dummies for RCTs	Yes	-	-	-	-
Clustered errors	Intervention instructor RCT	Intervention instructor	Intervention instructor	Intervention instructor	Intervention instructor

Note: Robust *p*-value in parentheses.

dozen 1000 EUR, in line with the average amount earned by start-ups in their first few months of operation. The increase in revenue between the times of the first and last interview ranges from 0 to EUR 1,489,026, with a revenue increase of EUR 29,568 for the firms in the 90th percentile.

We use the *suest* STATA package to compare intervention coefficients across the four RCTs. Results show that, overall, there are no clear differences across the four RCTs for the termination, performance, and radical pivoting analyses. The only exception is the RCT1 intervention coefficient in Table 4, where we observe a more precise difference compared with the coefficients in RCT3 and 4, in line with the discussion on the specificities of RCT1 as regards the overall number of radical pivots made by entrepreneurs.

6.4 | Instrumenting scientific intensity

Our analyses so far have provided estimates of the intention-to-treat effect, which does not account for the possibility that participants may not comply with the intervention (Gelman et al., 2020). We next take advantage of the large scale of our sample to address this issue. We asked the research assistants who conducted the regular phone interviews with participants to use a predefined coding scheme (based on 16 items) to assess their level of scientific intensity as decision-makers (on a scale from 0 to 5). During the phone interviews, the research assistants asked open-ended questions whose content was coded to measure whether participants used theories and experiments to form, test, and update their beliefs. Interviewees did not know and were unaware of the coding scheme. We measured scientific intensity at each observation point (about once every 4 weeks).

Table 7 reports the results from an analysis in which the level of scientific intensity is explained as a function of our intervention. Results show a positive impact of the intervention on scientific intensity, confirming that the intervention was successful in nudging entrepreneurs to operate like scientists when making decisions. This result provides a positive response to Zellweger and Zenger's (2022) inquiry into the teachability of a scientific approach to entrepreneurial decision-making. As such, it is a noteworthy and significant finding in its own right.

Table 8 presents the results of a cross-sectional specification estimated using two-stage least squares where the intervention is used as an instrument for the average level of scientific intensity across the observation period. Results on termination, reported in Column (1), show that a one-unit increase in the average scientific intensity increases the probability of terminating by 28.3 p.p. ($p = .003$). Column (2) shows that a one-unit increase in the average scientific intensity increases the probability of radically pivoting once (vs. 0 or more than once) by 24.1 p.-p. ($p = .001$). Looking at the effect on performance, Column (3) shows that a one-unit increase in the average scientific intensity is associated with an increase of EUR 20,126.24 ($p = .038$).

6.5 | Additional analyses

Our three dependent variables—termination, radical pivot, and revenue—depend on correlated entrepreneurial choices. Estimating the three equations separately might thus not account for the correlation across the errors of these equations. Table A14 of the Online Appendix presents seemingly unrelated regression results, which control for any covariance across the three regressions. The regression coefficients obtained simultaneously are largely consistent with those presented in the previous sections.

TABLE 7 Scientific intensity.

	(1)	(2)	(3)	(4)	(5)
	Scientific intensity	Scientific intensity	Scientific intensity	Scientific intensity	Scientific intensity
	OLS cross section	OLS cross section	OLS cross section	OLS cross section	OLS cross section
Variables	Full sample	RCT1	RCT2	RCT3	RCT4
Intervention	0.319 (.000)	0.581 (.002)	0.196 (.072)	0.296 (.281)	0.321 (.015)
Constant	1.285 (.006)	1.155 (.006)	2.493 (.000)	1.082 (.172)	2.086 (.000)
Observations	759	116	250	132	261
R-squared	.173	.178	.090	.064	.028
Dummies for instructors	Yes	Yes	Yes	Yes	Yes
Dummies for RCTs	Yes	-	-	-	-
Clustered errors	Intervention instructor RCT	Intervention instructor	Intervention instructor	Intervention instructor	Intervention instructor

Note: Robust p-value in parentheses.

TABLE 8 Instrumenting scientific intensity.

Variables	(1) Termination 2SLS cross-section Full sample	(2) Pivoting once 2SLS cross-section Full sample	(3) Revenue 2SLS cross-section Full sample
Average scientific intensity	0.283 (.003)	0.241 (.001)	20,126.240 (.038)
Constant	−0.089 (.655)	−0.360 (.009)	−29,560.150 (.067)
Observations	759	759	759
Dummies for instructors and RCTs	Yes	Yes	Yes
Clustered errors	Intervention instructor RCT	Intervention instructor RCT	Intervention instructor RCT

Note: Robust *p*-value in parentheses.

We also ran, as a robustness check, a copula model for the joint determination of the binary variables termination and pivoting once, using the STATA package *rbicopula*. We report the results in Table A15 of the Online Appendix. The estimated dependence between error terms of the two regressions is different from zero (Wald test of theta = 0: Prob > $chi^2 = 0.054$), and the total average marginal effects of intervention on the joint probability *Pr*(termination = 1, pivoting only once = 1) is positive ($B = 0.056$, $p = .000$). Overall, the treatment plays two roles: first, it increases the two separate unconditional probabilities; second, it increases the joint probability as well, consistent with our framework.

7 | THEORETICALLY BASED INTERPRETATION OF FINDINGS

7.1 | Entrepreneurial decision-making process

In this section, we develop a framework that provides a plausible, theoretically based explanation of our empirical results. To streamline this discussion, we use a stylized representation of the entrepreneurial decision-making process in which an entrepreneur evaluates ideas in exploration stages: In each stage, they evaluate one idea. At the end of each stage, they decide whether to commit to developing the idea, pivot to a new idea, or terminate the process. This representation is consistent with the one illustrated by Gans et al. (2019). When the entrepreneur commits or terminates, the process ends. When they pivot, the process starts again with a new idea, and the entrepreneur can pick one of the three options again.

At the end of each stage, the entrepreneur has a subjective belief (a probability) about whether the idea will be successful. We assume that they commit to the idea if this probability is higher than a threshold. Thus, when they identify an idea with a probability of being successful that is higher than the threshold, they commit to it. If instead the probability is lower than the threshold, they pivot or terminate.

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To understand the process, consider that if the entrepreneur reaches a given stage, they must have discarded all the ideas explored in the previous stages and decided each time to pivot instead of terminating. Thus, if they reach stage i, they have explored $i-1$ ideas (starting with idea 1), all these previous ideas were below the threshold, the idea they are currently evaluating is the ith idea, and they have pivoted $i-1$ times.

Figure 3 provides a snapshot of the entrepreneur's options at the generic stage i. We represent the probabilities of this process in a simple way: We denote by p_i the subjective probability of success of the ith idea at the end of stage i and by $\tilde{p}$ the subjective probability threshold. At the end of stage i, if the subjective probability of success of the ith idea is higher than the threshold, that is, $pi>\tilde{p}$, the entrepreneur commits to this idea, and the process ends. If it is smaller, that is, $pi<\tilde{p}$, they either terminate or pivot to another idea.

To choose between termination and pivot, the entrepreneur thinks of new ideas that they could explore in the next stage $i+1$. Suppose that $\widehat{p}_{i+1}$ is the expected probability of success of an idea that they can explore in this new stage. This is the expected probability, at the end of stage i, of the subjective probability of success p_{i+1} that they will observe at the end of the new stage. If this expected probability is lower than the threshold, that is, $\widehat{p}_{i+1}<\tilde{p}$, they terminate because both the probability of success of the current idea and the expected probability of success of the new idea are smaller than the threshold. As shown in Figure 3, this corresponds to the event $pi<\tilde{p}$ and $\widehat{p}_{i+1}<\tilde{p}$, together with the event that all subjective probabilities of the ideas in previous stages are below the threshold, that is, $p1<\tilde{p}$, $p2<\tilde{p}$, ..., $pi-1<\tilde{p}$.

If instead the entrepreneur expects a higher probability of success for this new $(i+1)$th idea, they pivot to the new stage. In this case, as for termination, $pi<\tilde{p}$ and all the probabilities in previous stages are smaller than the threshold. However, the expected probability of success of the next-stage idea is now higher than the threshold, that is $\widehat{p}_{i+1}>\tilde{p}$.

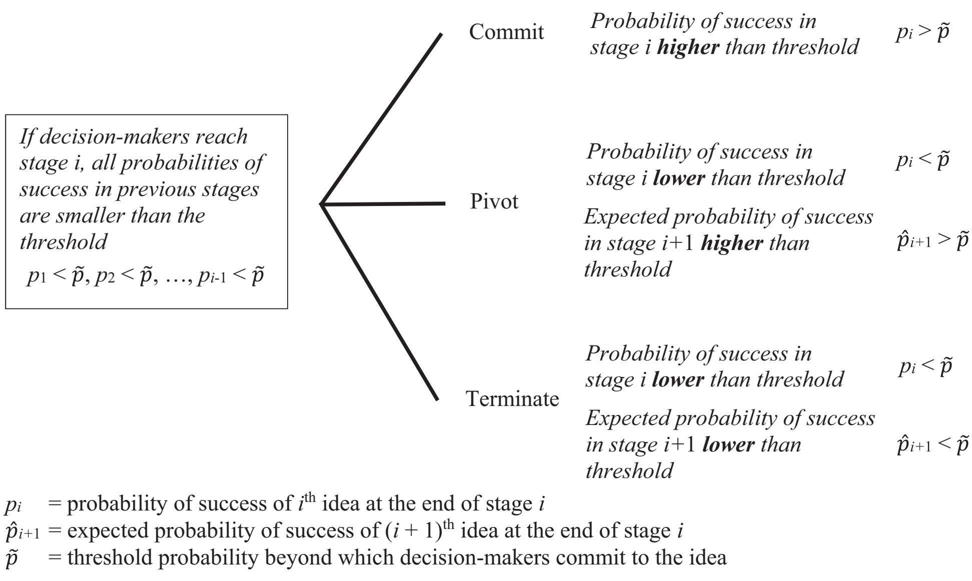

FIGURE 3 Decision-makers' options at stage i. p_i is the probability of success of ith idea at the end of stage i. $\widehat{p}_{i+1}$ is the expected probability of success of $(i+1)$th idea at the end of stage i. $\tilde{p}$ is the threshold probability beyond which decision-makers commit to the idea.

If the entrepreneur moves to the new stage $i + 1$, at the end of this new stage (i.e., after exploring the new idea), they update $\widehat{p}_{i+1}$ to p_{i+1}. If the update is still higher than the threshold, they commit to the new idea; if it is smaller, they terminate or pivot following the same logic of stage i at the end of the new stage $i+1$.[7]

7.2 | Decision rules for termination, pivoting, and commitment

To summarize, if an entrepreneur has reached the end of stage i, all the probabilities of the i—1 ideas up to the beginning of stage i are smaller than the threshold. Then, at the end of stage i:

- The entrepreneur terminates their project if the probability of success p_i of the ith idea and the expected probability of success of the next stage idea, $\widehat{p}_{i+1}$, are both smaller than the threshold, that is, $pi<\tilde{p}$ and $\widehat{p}_{i+1}<\tilde{p}$.
- The entrepreneur pivots if the probability of success p_i of the ith idea is smaller than the threshold and the expected probability of success of the next stage idea, $\widehat{p}_{i+1}$, is higher than the threshold, that is, $pi<\tilde{p}$ and $\widehat{p}_{i+1}>\tilde{p}$.
- The entrepreneur commits if the probability of success p_i of the ith idea is higher than the threshold, that is $pi>\tilde{p}$.

Figure 3 summarizes the decision rules for termination, pivot, and commitment at any exploration stage i.

7.3 | The effects of the scientific approach: Efficient search versus "methodic doubt"

The scientific approach can affect decision-making in two ways. On the one hand, entrepreneurs who adopt a scientific approach use more clearly defined theory and validation procedures when they explore ideas in each stage. This generates a more *efficient search*. A more efficient search raises the probability of success p_i of ideas in earlier stages because it combines two effects: a higher probability of success and the ability to identify successful ideas earlier. This is because a theory enables entrepreneurs to rank ideas based on their value at the outset of the process. Entrepreneurs will then work first on the ideas that they expect to have higher probability of success. When one of the ideas they are working on proves unsuccessful, they pivot to other ideas, but these will have a lower expected probability of success. Nonscientific entrepreneurs, not guided by theory, are, instead, more likely to learn about the value of ideas as they work on them rather than ex ante, in a trial and error sort of way. Thus, they start with some idea, and if it proves to be unsuccessful, the new idea they pivot to will not have an expected probability of success as low as in the case of scientific entrepreneurs because ideas are evaluated as they emerge in subsequent stages of exploration and not ranked beforehand with the help of the theory.

[7]Clearly, because the new information acquired during the exploration stage $i + 1$ updates the expected probability of success of the $(i + 1)$th idea, it can very well be that at end of stage i this expected probability is higher than the threshold, that is $\widehat{p}_{i+1}>\tilde{p}$, and therefore the entrepreneur pivots. However, at the end of stage $i+1$, p_{i+1} may still fall below the threshold, that is, $pi+1<\tilde{p}$.

As a result, efficient search generates higher probabilities *pi* and $\widehat{p}_{i+1}$ in earlier exploration stages, and a more rapid decay across stages. If efficient search was the only mechanism associated with the scientific approach, treated firms would show a higher probability to commit, a lower probability to terminate, and a higher probability of pivoting at earlier stages, which would decline rapidly.

However, the scientific approach also instills in entrepreneurs a *methodic doubt*. The very fact that entrepreneurs who adopt this approach develop a theory for why their ideas may be successful, makes them aware that this is one of the potential logics for their scenarios or opportunities. As a result, they become more cautious about the success of their ideas because they realize that what they pursue is plausible, but not certain. Not only are they aware that other logics, paths or scenarios are possible, and could be more successful, but they are also aware that the one that they pursue could be unsuccessful because there are elements that they did not foresee or account for. Compared to entrepreneurs who do not adopt a scientific approach, these entrepreneurs set a higher threshold $\tilde{p}$ for accepting a given probability of success. If methodic doubt was the only mechanism associated with the scientific approach, treated firms would show a higher probability to terminate, and a lower probability of pivoting or committing.

Since efficient search and methodic doubt go in opposite directions, we need to reconcile these two effects. In this respect, the higher rate of termination implies that methodic doubt dominates efficient search. At the same time, treated entrepreneurs are more likely to pivot once as opposed to not pivoting or pivoting indefinitely. This is in line with our conjecture that theories enable them to start with more plausible ideas and they reduce more markedly the expected value of ideas as they run more pivots within a given domain of search (e.g., a product, market, or set of customers). To summarize, our explanation of the patterns that we observe empirically is that: (a) methodic doubt dominates efficient search in terminations in that the higher acceptance threshold $\tilde{p}$ implied by the former dominates the higher probabilities of success p_i in each stage i implied by the latter and (b) the more efficient search implied by the scientific approach raises the probability of success of early ideas to a greater extent than the probability of success in later pivots, producing a more marked decline of the probability of success of ideas across stages ($\widehat{p}_{i+1} > \tilde{p}$ more likely in earlier stages i).[8]

7.4 | Performance implications

Our logic cannot predict whether our empirical results that scientific entrepreneurs terminate to a greater extent, especially in earlier stages, and pivot only a few times, are associated with higher performance. This is an empirical question. However, Tables 6 and 8 (Column 3) and Figure 1b provide reasonable evidence that scientific entrepreneurs exhibit higher performance

[8]A numerical example can help intuition. Suppose that, for scientific entrepreneurs, the shares of entrepreneurs who terminate, pivot, or commit in three subsequent stages are, respectively: (1) 0.4, 0.5, and 0.1; (2) 0.5, 0.3, and 0.2; and (3) 0.6, 0.1, and 0.3. At the end of stage (3), the share of terminations, pivot 0, 1, or 2 times, are, respectively: $0.4 + 0.5 \times 0.5 + 0.5 \times 0.3 \times 0.6 = 0.74$; 0.5; $0.5 \times 0.7 = 0.35$; $0.5 \times 0.3 \times 0.1 = 0.015$; $0.1 + 0.5 \times 0.2 + 0.5 \times 0.3 \times 0.3 = 0.245$. Suppose that, for nonscientific entrepreneurs, the equivalent shares in the three stages are 0.3, 0.4, and 0.3 in all three stages, which yields the following shares of terminations, pivots 0-1-2, and commitments: 0.468; 0.6; 0.24; 0.064; and 0.468. In this example, scientific entrepreneurs exhibit a higher probability of termination, a probability of pivoting that falls more rapidly across stages, and a lower probability of commitment. This generates a higher share of terminations, a higher share of pivots 1-time and a lower share of pivots 0 or 2-times.

than their nonscientific counterfactuals. Even if we do not find that they expand their domain of search, their more questioning behavior about termination and pivoting still generates better performance than the control group.

7.5 | Comparison with Camuffo et al.'s study

Compared with Camuffo et al. (2020), our study provides similar (albeit stronger) evidence about termination. It provides distinctive evidence instead with regard to pivoting, providing evidence consistent with the idea that the scientific approach does not generate more pivots but, rather, implies more focused pivoting. Our multiple RCTs and larger sample reveal that scientific decision-makers more efficiently and quickly navigate their search space across stages and have weaker beliefs about the benefits of multiple additional pivots. We also provide a theoretically based explanation of the empirical results, suggesting that the effect of methodic doubt dominates that of efficient search in terminations, whereas the opposite occurs in pivoting. Of course, it would be important for future research to investigate this effect further and provide a more direct test of these mechanisms.

8 | DISCUSSION AND CONCLUSIONS

This article provides a large-scale empirical investigation of the implications of a scientific approach to entrepreneurial decision-making (Camuffo et al., 2020; Zellweger & Zenger, 2023). It responds to Bettis et al.'s (2016) call for systematic replication to consolidate the credibility of preliminary findings in strategic management research by replicating the results of Camuffo et al. (2020)—the first and only empirical test of the theory-based approach to decision-making—on a large-scale sample.

The paper contributes to entrepreneurship research in multiple ways. First, it responds to a lively recent debate about the benefits and teachability of the scientific approach by showing that entrepreneurs not only can be taught to make decisions using it but also that the use of this approach is associated with superior performance (Sergeeva et al., 2021; Zellweger & Zenger, 2022). Across contexts, time, and institutional settings, we find that a relatively short treatment embedded in a training program can lead entrepreneurs to adopt a scientific approach when making decisions and benefit from it.

Second, our large-scale investigation reveals novel aspects of the way in which a scientific approach affects outcomes, showing that decision-makers using this approach (1) are more likely to terminate their projects and do so earlier; (2) are more likely to pivot, but do so in a more focused way (few pivots as opposed to none or many); and (3) perform better.

Third, it contributes to research in this area with a theoretically based interpretation of the results that provides insights into the mechanisms underlying the effects of the scientific approach. Our framework maintains that the scientific approach operates through two mechanisms. The first, *efficient search*, has to do with the ability to use theories to identify exploration opportunities and efficiently test them. The second, *methodic doubt*, has to do with the ability to question ideas. Entrepreneurs adopting the scientific approach are aware that they deal with unknowns and, hence, adopt tighter decision rules.

Our empirical results are consistent with a theoretical scenario in which the effect of methodic doubt dominates that of efficient search in the case of termination, whereas efficient search prevails in the case of pivots. A scientific approach is associated with the ability to rank ideas ex ante implying a more marked decay of the value of ideas across pivots, and the combined effect of methodic doubt and efficient search is associated with higher performance. Of course, while our sample reveals these patterns, other samples (under other conditions, contexts or with different agents) might yield different results. It would be important for future research to explore the contingencies that affect the relative importance of the two mechanisms.

This article also contributes to a larger debate on the design of field experiments. While interventions that focus on small samples and a specific context are fundamental in discovering interesting patterns, the results of this study highlight the importance of further corroborating those results with larger-scale investigations. The latter not only enable researchers to verify the validity of the initial findings, they also enable them to discover important aspects of the phenomenon that would not be visible otherwise, and that are essential for subsequent theory-building efforts. Compared to Camuffo et al. (2020), our larger-scale study highlights crucial differences. First, Camuffo et al. (2020) did not find a precise effect of the intervention on termination in most of their regressions; their limited sample size did not include enough terminations to fully detect this effect. We instead find a positive and clear effect on termination across our three new RCTs, with the scientific approach potentially mitigating overcommitment to non-promising ideas which, in turn, could explain why most new ventures fail (Fairlie et al., 2019; Nobel, 2011).

Second, Camuffo et al. (2020) detected a positive effect of the intervention on the number of pivots, which again was driven by the fact that, in their sample, only a few firms pivoted more than once. In the larger sample of this study, we find instead evidence consistent with the intuition that the intervention makes pivoting more focused: Treated firms are more likely to pivot once or twice, but less likely to pivot more than that. Third, the larger sample size made the results about performance more robust, allowing us to precisely estimate the intention-to-treat effects. Fourth, with four RCTs we were also able to precisely estimate the complier average casual effect using an index of scientific intensity instrumented by the intervention.

This article has shown that the impact of a scientific approach on termination, pivot, and performance is overall robust across different contexts in terms of sign, despite variations in the effect size and precision of the estimates. The investigation of the possible factors contributing to such variations would be very important and insightful, although it would require a research project specifically designed to that purpose. We believe that future research would derive valuable insights from pursuing this line of investigation.

Finally, our study has practical implications. It shows that it is possible to teach entrepreneurs to operate like scientists, even within the context of relatively short interventions embedded in business support programs. It also shows that this approach leads to superior performance through a very precise path: better idea selection and focused pivots. In contexts in which these are desirable outcomes, therefore, the application of a scientific approach to entrepreneurial decision-making would be valuable.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available on request from the corresponding author.

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Additional supporting information can be found online in the Supporting Information section at the end of this article.

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