Limits to the Value of Entrepreneurial Experimentation? Startup Scaling Under Varying Demand Conditions

ABSTRACT

The benefits of experimentation for firm success and scaling are widely studied in prior research, yet its role under varying demand conditions remains underexplored. This paper examines whether startups change how much they experiment when facing greater demand-side opportunities and how experimentation in turn affects the scaling of these startups. We theorize that demand-side opportunities increase the opportunity costs of experimentation and that strategic commitments inherent in experimentation constrain scaling under favorable demand conditions. Leveraging the 2016 Indian demonetization as a quasi-experiment, we find that experimentation decreased among Indian software startups experiencing stronger demand and startups with greater (recent) experimentation scaled less under these conditions. Additional tests provide evidence consistent with the proposed theory. These findings contribute to the literatures on startup experimentation and scaling.

Keywords: Experimentation, scaling, scalability, demand conditions, demonetization, digital platform

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INTRODUCTION

Entrepreneurial experimentation, defined broadly as any market-based test aimed at assessing the viability and value of a startup's business ideas (Greenstein, 2007; Shelef et al., 2024), has been studied extensively in recent strategy and entrepreneurship research (Burnell et al., 2025; Contigiani, 2023; Gans, 2025; Kim, Posen, & Ganco, 2025; Kerr et al., 2014; Pillai, Goldfarb, & Kirsch, 2020). Experimentation allows startups to uncover critical aspects of their business models and customers, such as product functionality, willingness to pay, and workable revenue models (Burnell et al., 2025; McDonald & Eisenhardt, 2020; Pillai et al., 2020; Tidhar et al., 2025). Research suggests that such learning helps startups refine their offerings and improve product-market fit (Contigiani, 2023; Santamaria et al., 2024), while retaining strategic flexibility before making irreversible commitments (Novelli & Spina, 2024). Recent work has also highlighted the role of experimentation in startup scaling specifically, providing empirical evidence that intentional experimentation, even at the expense of delaying scaling, may enhance scaling success (Koning et al., 2022; Lee & Kim, 2024; Tidhar et al., 2025).

Although the extant literature provides many insights into the value of experimentation for startup outcomes like scaling, it is largely silent about how demand-side opportunities condition this relationship. This research gap takes on particular importance in the backdrop of a strong scholarly impetus towards a scientific experimental method for entrepreneurship, one that is guided by well-formed theories of value, careful delineation of causal mechanisms, and articulation of business hypotheses that are tested in real world experiments (e.g., Agarwal et al., 2025; Burnell et al., 2025; Camuffo et al., 2020; Felin et al., 2024). This scientific entrepreneurial approach builds on an intellectual tradition in which entrepreneurship is viewed as a process of discovery, wherein objective opportunities may be uncovered through learning and experimentation (Blank & Eckhardt, 2024; Kerr et al., 2014; McDonald & Eisenhardt, 2020; Shane & Venkataraman, 2000). Within this worldview, the value of experimentation to test out and implement the right product and business model for a startup may be patently beneficial.

However, other intellectual traditions that theorize about entrepreneurship as a process of creation (Alvarez & Barney, 2007) or effectuation (Sarasvathy, 2001) are less aligned with an experimental approach; instead emphasizing action over learning and shaping the opportunity space over discovering it. We posit that startups' demand environment, and environmental conditions more broadly, may define boundary conditions for when a scientific experimentation-based approach may be better for entrepreneurship, and for outcomes like startup scaling.

Accordingly, in this paper, we address two related questions: first, how do startups adjust their levels of experimentation in response to more (versus less) munificent demand conditions, and relatedly, how does (recent) experimentation affect the scaling of startups with greater (versus lesser) demand-side opportunities? Despite the well-documented benefits of experimentation, prior research has also highlighted some disadvantages, including the strategic commitments it entails (Gans et al., 2019), the reputational risks if products fail (Adner & Levinthal, 2024; Kerr et al., 2014; Sudhir et al., 2025), and the potential appropriability hazards through exposure to competitors (Contigiani, 2023; Shelef et al., 2024). Extending this line of research, we suggest that demand-side opportunities can change the costs and utility of experimentation; therefore, startups may adjust how much they experiment to more efficiently allocate their scarce managerial and financial resources. Thus, incorporating demand conditions may enrich our understanding of the relationship between experimentation and scaling, and the advantages of experimentation more broadly.

Building on these ideas, we posit that startups may reduce experimentation in response to greater demand-side opportunities. When demand-side opportunities are higher, this increases the inherent scalability of a startup's business, which encourage them to prioritize rapid customer acquisition over experimentation. Because startups are typically resource constrained (Fisher, 2012), the opportunity cost of experimentation becomes high in these conditions. Further, consistent with a creation view of entrepreneurship, rapid customer acquisition also allows startups to preempt rivals and lock in users (Eisenmann, 2006; Lee & Kim, 2024), especially in markets with demand-side increasing returns, helping them secure a larger customer base and a longer-term advantage. Meanwhile, the value of experimentation can diminish because high customer demand reduces the marginal benefit of further refinements to a startup's value proposition. In contrast, delaying scaling to continue experimentation may allow competitors to accumulate market resources and capture market demand, thus erecting barriers to a firm's future growth (Lee & Kim, 2024; Wright & Saiedi, 2024). We further theorize that the strategic commitments inherent in experimentation (Gans et al., 2019) may hinder scaling when (unexpected) demand-side opportunities arise. Experimentation commits startups to a particular learning trajectory (Chen et al., 2024; Contigiani, 2023; Zahra, 2021), making it difficult to reorient towards demand-side opportunities due to time compression diseconomies (Dierickx & Cool, 1989; Pacheco-de-Almeida, 2010). Consistent with an effectuation view of entrepreneurship, startups may need to work with available resources and have limited slack in such resources due to prior commitments, limiting their ability to take advantage of unanticipated demand side opportunities. Thus, actively engaging in experimentation diminishes startup scaling in relative terms when demand-side opportunities are high.

We test our hypotheses using the 2016 Indian demonetization as a quasi-experiment. On November 8, 2016, the Indian government unexpectedly withdrew all 500- and 1000-rupee currency notes from circulation and replaced them only gradually with new denominations. This sudden policy change created nationwide cash shortages (Adbi, Lee, & Singh, 2024; Banerjee et al., 2024) and effectively forced cash-using consumers to adopt mobile payments and other digital services. As a result, demonetization generated a plausibly exogenous surge in demand for digital startups offering products aligned with these emerging needs, providing a compelling context to address our research questions. Leveraging a matched difference-in-differences (DID) design, we compare digital (software) startups operating in segments significantly affected by demonetization with those in segments that weren't and evaluate experimentation with a novel measure based on changes to software startups' technology stacks. We find that startups facing stronger demand-side opportunities significantly *reduced* experimentation relative to those in the control group. These effects are especially pronounced among startups with limited (financial) slack, which are less able to overcome the trade-offs between experimentation and scaling, and among platform startups, for whom the opportunity costs of delaying scaling are likely higher and less reversible. Furthermore, recent experimentation prior to demonetization, which we suggest is plausibly unrelated to the demand shock itself, is negatively associated with post-demonetization scaling, as measured by increases in startups' web traffic.

The current paper offers several contributions to strategy and entrepreneurship research. First, it bridges two interrelated yet largely disconnected literatures: one that focuses on the economic drivers of scalability such as demand conditions (Giustiziero et al., 2023; Li et al., 2023) and another that emphasizes learning through experimentation as a prerequisite for scaling (Lee & Kim, 2024; Tidhar et al., 2025). Integrating these perspectives, it advances both literatures by identifying demand-side opportunities as a critical boundary condition under which experimentation may be less effective or even detrimental for scaling. In turn, our findings highlight that demand-driven scalability and experimentation may act as strategic substitutes in enabling startup scaling. Second, in contrast to prior work that emphasizes the benefits of experimentation and the importance of learning "at the right time" (Tidhar et al., 2025: 114), this study uncovers when and why startups might choose to experiment *less*. By highlighting the opportunity costs of experimentation, it contributes to the literature on the potential boundary conditions of the experimental approach (Contigiani, 2023; Gans et al., 2019; Shelef et al., 2024). Lastly, we complement prior research on Indian demonetization, which has mainly examined its impacts on consumer behavior and social welfare (Agarwal et al., 2024; Chodorow-Reich et al., 2020). The findings demonstrate substantial scaling gains for digital startups that experienced positive post-demonetization demand conditions, providing empirical evidence for the role of policy interventions in shaping environmental conditions that affect startup growth.

THEORETICAL BACKGROUND

Entrepreneurial Experimentation and Scaling

Research suggests that experimentation plays a central role in how new entrepreneurial ventures navigate uncertainty about the potential value of their business ideas (Burnell et al., 2025; Gans et al., 2019; Kerr et al., 2014).[1] Broadly, entrepreneurial experimentation is understood to refer to market-based tests aimed at assessing the viability and value of a business idea (Greenstein, 2007; Shelef et al., 2024). At the same time, there remains considerable variation in how experimentation is defined (and measured) in the literature. Across various definitions, two defining characteristics of experimentation emerge. First, experimentation entails direct interaction with the market, which cannot be replaced by lab experiments or customer surveys (Chen et al., 2024; Greenstein, 2012). Second, experimentation constitutes an adaptive learning process through which entrepreneurs uncover critical aspects of their business and customer preferences, such as desired product functionality, customers' willingness to pay, viable revenue models, and interdependencies among business activities (Burnell et al., 2025; McDonald & Eisenhardt, 2020; Pillai et al., 2020; Tidhar et al., 2025). Market feedback from experimentation is then incorporated into entrepreneurial decision making, either by persisting with the current idea, modifying the product or business model, or pivoting strategically (Chen et al., 2024; Pillai et al., 2020; Rosenberg, 1982). Consistent with these commonalities in our understanding of experimentation, we adopt a broad definition that experimentation encompasses a firm's actions that modify and adapt product offerings to assess their feasibility and desirability in the market.[2] In digital firms, such experimentation often manifests as changes in the technological components their core digital products are comprised of (Dushnitsky & Stroube, 2021; Ketkar & Roche, 2024), which in turn motivates our measure of experimentation.

Scaling has received growing attention from both scholars and practitioners (Bohan et al., 2024; Hoffman & Yeh, 2018). Although the concept has been defined and operationalized in various ways, recent theorizing has converged on a shared understanding that moves beyond treating scaling and rapid

[1] We note that experimentation may not be a universal feature of all forms of entrepreneurship, as some ventures in more "traditional" or less innovation-intensive contexts may rely more on imitation and replication rather than on hypothesis testing and iterative learning to identify and pursue opportunities.
[2] Some existing studies focus specifically on experimentation that occurs before product launch and involves testing incomplete products (e.g., Contigiani, 2023). Much of the lean startup literature emphasizes this pre-entry phase, where experimentation is embodied in the notion of the minimum viable product (Ries, 2011). However, experimentation is not limited to this stage; rather, it is a broader process associated with new venture development more generally. Therefore, the definition we adopt in this study is more generalizable than pre-entry experimentation.

growth as interchangeable constructs (Ott & Eisenhardt, 2020). Scaling is increasingly understood to mean a type of exponential growth that results from the ability of firms to create increasing value without proportional increases in costs (Bohan et al., 2024; Coviello et al., 2024; Giustiziero et al., 2023; Somaya & You, 2024). The increasing prevalence of scaling can be partially ascribed to the advancement of digitalization, coupled with the emergence of scalable business models (Giustiziero et al., 2023; Li et al., 2023; Monaghan, Tippmann, & Coviello, 2020). Importantly, scaling is driven not only by supply-side economies of scale from digital resources but can also be affected by demand-side factors such as substitute products, customer acquisition costs, and positive network externalities (Giustiziero et al., 2023).

Although research on experimentation and scaling initially developed separately, recent work has begun to link these literatures by examining how entrepreneurial experimentation affects scaling outcomes. Effective scaling entails learning how and when to scale, and research suggests that successful firms use a sequential evolving process to learn (Ott & Eisenhardt, 2020) and tend to learn longer to avoid early commitment into scaling a business idea that might have a poor product-market fit (Lee & Kim, 2024). Thus, experimentation is often seen as a particular stage in the entrepreneurial process that precedes scaling, and the prevailing consensus is that intentionally accelerating scaling may increase the risk of startup failure due to insufficient experimentation and a lack of product-market fit (Lee & Kim, 2024; Tidhar et al., 2025).

Benefits and Costs of Entrepreneurial Experimentation

Highlighting its learning benefits, several studies in a variety of contexts provide empirical evidence that adopting an experimental approach enhances startup performance (Agarwal et al., 2025; Camuffo et al., 2020; 2025; Koning et al., 2022; Tidhar et al., 2025). For example, Koning et al. (2022) find that the adoption of A/B testing, a common experimentation tool, enhances growth among digital startups. Similarly, Pillai et al. (2020) demonstrate that the extent of a firm's experimentation, measured by the number of unique automobile models it releases, is positively associated with its subsequent success.

Despite these well-documented advantages, an emerging stream of research explores how entrepreneurs must balance the learning benefits of experimentation against its associated costs, and how this balance influences when and how firms engage in experimentation. Notably, these costs include not only direct expenditures but also indirect costs and potential implications for subsequent value creation and capture (Adner & Levinthal, 2024; Gans et al., 2019; Shelef et al., 2024). For example, experimentation may reveal information about a business idea to the market and increase the risk of imitation; thus, startups may be less likely to experiment when the appropriability regime is weak (Contigiani, 2023). Moreover, experimentation can be costly when experiments are conducted with non-representative samples, leading firms to implement product changes misaligned with actual consumer preferences, which may also constrain future value creation opportunities (Cao et al., 2020; List, 2022). In addition, experimentation may shape how the market reacts to the product that is ultimately released (Kerr et al., 2014), wherein experiments conducted with underdeveloped offerings lead early customers and stakeholders to form negative impressions of the firm and its product (Adner & Levinthal, 2024). Consistent with this view, recent evidence shows that entrepreneurs sometimes forgo learning opportunities from experimentation to mitigate the reputational risks associated with failed launches and in turn increase their chances of funding by investors (Sudhir, Yoo, & Zhou, 2025). The efficacy of experimentation may also be contingent on how it is embedded within the organization, generating stronger performance when it is supported by an informal (versus formal) structure that facilitates rapid learning and adaptation (Contigiani & Young-Hyman, 2022).

Experimentation and Theories of Entrepreneurship

Our conceptual understanding of experimentation also has a nuanced relationship with established theories of entrepreneurship, which can be broadly divided into two types (Alvarez et al., 2024)—entrepreneurship as a process of opportunity discovery (Shane & Venkatraman, 2000) and entrepreneurship as a process of creation or effectuation (Alvarez & Barney, 2007; Sarasvathy, 2001). Although both theoretical logics highlight the importance of experimenting with new ideas to understand their potential, the first perspective assumes that experimentation (e.g., the lean startup and scientific approaches) help uncover information about opportunities that “exist independent of entrepreneurs” (Blank & Eckhardt, 2023), and that entrepreneurs possess sufficient prior information to form cause-effect conjectures that can then be tested through experimentation (Camuffo et al., 2020). In this view, entrepreneurs reduce uncertainty through systematic hypothesis testing to discover demand (Alvarez et al., 2024). This logic implicitly assumes that entrepreneurs primarily engage in efficiently searching an exogenously given and fixed opportunity landscape (Sarasvathy, 2024). In contrast, the second stream of entrepreneurship theory, on creation (Alvarez & Barney, 2007) and effectuation (Sarasvathy, 2001), emphasizes stakeholder interactions that are less about validating hypotheses or obtaining information and more about taking action to seize the opportunities (Sarasvathy, 2024). Rather than emphasizing prediction and navigation of an exogenous environment, this second logic focuses on how entrepreneurs and stakeholders jointly interact to endogenously create the landscape within which they operate (Alvarez et al., 2024). These perspectives are increasingly viewed as complementary rather than competing, with each likely to apply under different conditions or at different stages of the entrepreneurial process. Accordingly, scholars have called for more research on the boundary conditions under which one logic becomes more applicable than the other (Fisher, 2012; Read et al., 2009). The current paper answers this call by highlighting demand conditions as a critical factor that may shape this theoretical boundary.

HYPOTHESES

Demand-side Opportunities and Entrepreneurial Experimentation

We posit that startups may reduce their experimentation in response to stronger demand-side opportunities. While experimentation can yield valuable insights, startups may also consider what they forgo by engaging in it. When demand is expanding rapidly, customers are inherently more likely to purchase the startup’s product and become easier to attract, exhibiting lesser loyalty to previously adopted products and lower customer acquisition costs (Fosfuri & Giarratana, 2009). At the same time, experimentation entails opportunity costs because firms have limited resources such as managerial attention, firm-specific human capital, and even financial resources (Fisher, 2012). Even though some ventures may have slack resources that can provide managerial latitude for experimentation and reduce the severity of trade-offs (Bradley, Wiklund, & Shepherd, 2011; Nohria & Gulati, 1996; Vanacker, Collewaert, & Paeleman, 2013), most early-stage firms possess limited slack and thus face binding resource allocation choices. The opportunity costs of experimentation imply that startups may need to make affirmative choices between seizing opportunities to scale and expending resources to experiment with new business ideas (Gans et al., 2019). Under favorable demand conditions, startups may have strong incentives to prioritize customer acquisition over experimentation. For example, firms may invest more heavily in advertising and marketing to facilitate product discovery, build market presence, stimulate word-of-mouth growth, and enable startups to increasingly attract more customers at lower costs (Eisenmann, 2006; Hoffman & Yeh, 2018). Thus, the foregone benefits of reallocating resources away from customer acquisition may become substantial.

In addition to resource constraints, emerging demand-side opportunities can increase the opportunity costs of experimentation by shifting the competitive landscape. The literature on strategic preemption contends that early movers can attain superior resources and gain advantages by preempting rivals in acquiring customers (Bain, 1956; Lieberman & Montgomery, 1998). For this reason, despite uncertainty about eventual payoffs, startups may invest in aggressive customer acquisition to lock out competitors. Further, capturing a larger customer base than rivals can itself serve as a source of advantage, as startups with larger installed bases become more attractive to subsequent adopters than competitors with smaller ones (Eisenmann, 2006; Schilling, 2002). Such advantages are particularly salient in products like software with extremely low marginal costs and significant demand-side increasing returns due to network effects (Giustiziero et al., 2023). These demand-side increasing returns can be further reinforced by customer switching costs, arising from users' habit formation or firm-specific product features (Lee & Kim, 2024; Suarez & Lanzolla, 2007). Thus, when demand-side opportunities emerge, startups may be incentivized to aggressively acquire customers and complementary resources before competitors do (Eisenmann, 2006). Conversely, delaying scaling to engage in experimentation may allow rivals to accumulate market resources and capture customer demand, creating subsequent barriers to growth for the focal firm (Wright & Saiedi, 2024).

Meanwhile, when demand-side opportunities are higher, the strategic value of experimentation may also be reduced. When a startup's product already experiences high demand, additional refinements are likely to yield more limited improvements to the value proposition as evidenced by its ultimate impact on customer purchase decisions. Moreover, prior research shows that customer lock-in can favor early, inferior products over later, superior ones (Arthur, 1989; Katz & Shapiro, 1994). Thus, the value that firms can ultimately capture from refining their business ideas through experimentation may be lower when demand side opportunities are high.

In summary, strong demand-side opportunities may increase the opportunity costs of experimentation and decrease the value generated from it, thus discouraging startups from engaging in extensive experimentation. Accordingly, we hypothesize the following:

> ***Hypothesis 1 (H1).*** *Startups reduce their levels of experimentation when they face greater demand-side opportunities.*

How Experimentation Affects Scaling when Demand-Side Opportunities Are High

Given the potential downsides of experimentation when demand-side opportunities emerge, a natural follow-up question is how *prior differences* in (recent) experimentation may affect startups' ability to capitalize on these opportunities for effective scaling. Engaging in experimentation may influence scaling in the face of demand-side opportunities for at least two reasons. First, while experimentation enables entrepreneurs to learn about customer preferences, it also commits startups to a "learning trajectory," which may weaken their ability to quickly scale. Learning through experimentation requires substantial time for reflection and review after each external interaction (Chen et al., 2021; Contigiani, 2023; Ries, 2017; Zahra, 2021). In addition, the process of absorbing and integrating new information into product improvements entails developing new capabilities, which can be subject to time compression diseconomies, such that accelerating such capability development results in disproportionately higher costs (Dierickx & Cool, 1989; Srikanth, Anand, & Stan, 2021). However, to capitalize on demand-side opportunities, startups need to rapidly mobilize existing resources, such as their workforce and managerial time and attention, to scale up the creation and delivery of offerings that meet customer needs. When a startup is actively engaged in experimentation, its ability to reallocate resources toward such scaling activities is more limited due to the strategic commitments inherent in experimentation (Gans et al., 2019; Jung, Mallon, & Wilden, 2024).

Second, even if a startup can augment its existing offerings quickly and at relatively low costs using insights gained from prior experimentation, rapidly expanding demand-side opportunities may reduce the expected returns from introducing these new features. This occurs because in high growth markets new customers may have preferences that differ significantly from those of the earlier customers that comprised the sample of the startup's experiments (Cao et al., 2024; Rietveld & Schilling, 2021). Moreover, if the experimentation yields presumed solutions that turn out to be a poor fit for (new) customers, it may impose reputational costs on the firm with customers and other stakeholders (Shelef et al., 2024). For example, prior research suggests that early experimentation failures may erode investor confidence and reduce startups' access to funding (Sudhir et al., 2025). This concern becomes particularly salient when demand-side opportunities surge and startups race to attract new customers. Negative perceptions stemming from failed experiments may undermine these efforts, weakening customer trust and investor enthusiasm when both are critical for scaling.

In summary, the inherent strategic commitments entailed in experimentation, along with potential learning costs and reputational risks, may hinder startups' ability to scale when demand-side opportunities arise. Accordingly, we hypothesize as follows:

> ***Hypothesis 2 (H2).*** *Recent experimentation is negatively associated with startup scaling under greater demand-side opportunities, such that startup firms scale less when they have engaged in more (as opposed to less) recent experimentation.*

METHODS

Empirically disentangling the effects of experimentation on scaling is challenging because endogeneity arises from the interdependence between a startup's scaling and experimentation decisions.

To address this issue, we leverage the Indian Demonetization “shock” of 2016, which produced a (plausibly) exogenous shock to demand for certain software startups. At 8 PM on November 8, 2016, the Prime Minister of India announced that starting at midnight two commonly used denominations of rupee currency notes—500 and 1000—would cease to be legal tender.[3] The policy remained undisclosed before its implementation, and the government and the Reserve Bank of India (RBI) did not print or distribute notes of the new denominations in advance (Chodorow-Reich et al., 2020). The voided notes represented 86% of the total cash in circulation (Crouzet, Gupta, & Mezzanotti, 2023), resulting in widespread cash shortages (Adbi et al., 2024; Banerjee et al., 2023) within a society and payment ecosystem reliant heavily on cash (Agarwal, Ghosh, & Ruan, 2024). The shortages were short-lived, and evidence indicates that cash availability had largely normalized by February 2017 (Crouzet et al., 2023). The stated objectives of the policy were to eliminate black money, reduce corruption, and remove counterfeit currency in circulation (Modi, 2016).[4]

Transactions using forms of noncash payment, such as credit and debit cards, checks, and other digital methods, were not hindered by the policy, creating a significant boost in demand for these alternatives, which is plausibly exogenous due to the prior secrecy and unanticipated nature of the policy (as well as being unlinked to its intended goals). In 2016, India had around 307.75 million smartphone users (22.9% of the population), with potential access to mobile payment services that offer convenience, lower transaction costs, and financial inclusion (Agarwal & Assenova, 2024; Ho et al., 2022).[5] The sharp decline in cash availability appears to have increased demand for a variety of services incorporating digital payment functionality (e.g., fintech services) and/or offering digital online substitutes for offline

[3] The old notes had to be deposited in banks by December 31, 2016, and exchanged for newly issued notes of different denominations, with withdrawals limited to 4,000 rupees (about $60) per person per day.
[4] However, later government narratives (consistent with the findings of this paper) also included the fostering of a digital financial ecosystem as one of the realized benefits of demonetization.
[5] https://www.statista.com/statistics/467163/forecast-of-smartphone-users-in-india/. https://www.huffpost.com/archive/in/entry/demonetisation-made-winners-of-companies-like-paytm-the-poor-are-still-counting-the-costs_in_5c0feb6be4b051c73eac3e11.

consumption (e.g., online shopping).[6] We take advantage of this quasi-experiment created by Indian demonetization to explore the relationship between demand-side opportunities and experimentation.

Data and Sample Construction

The main dataset used in this study includes information on Indian software startups compiled from three sources. We construct a list of startups headquartered in India and operating in the Information Technology (IT) industry using the PitchBook database, which provides comprehensive coverage of high-tech startups worldwide, including detailed firm profiles and historical fundraising records. We then complement this information with longitudinal data on monthly web traffic from Semrush,[7] a database that provides search engine optimization analytics and fine-grained user statistics on traffic volumes. Lastly, we collect data on web technology adoption from BuiltWith, a repository that offers information on the technology stacks that firms use to build their products, including when each technology was first installed and when it was uninstalled on their websites. These data enable tracking of technologies' adoption, retention, and abandonment over time (Dushnitsky & Stroube, 2021). Startups are linked across these data sources using website URLs, which serve as unique identifiers and eliminate the need for fuzzy matching based on firm names (Koning et al., 2022).

We apply a set of inclusion criteria to refine the sample. First, we exclude firms with missing information or only zero values for web traffic and web technology adoption. Because startups without any fundraising activity tend to have higher rates of missing data, the sample is restricted to startups that received at least one round of funding during the sample period, yielding 1,328 firms. Second, to reduce noise from extremely small segments, we exclude segments with very few firms (e.g., gaming, food tech, or real estate) and retain only those with at least 50 firms. Third, to ensure that the firms primarily operated in India at the time of demonetization, we use geographic web traffic data to filter the sample.

[6] https://www.forbes.com/sites/krnkashyap/2016/12/15/how-indias-demonetization-is-affecting-its-startups/

[7] Semrush estimates a website's monthly traffic by acquiring clickstream data from more than 200 million users worldwide from a variety of sources, such as intent service providers and browser trackers. Its estimations are based on keyword positions and their estimated traffic (i.e., search volume times average click-through rate). To avoid the influence of advertising and excessive search engine optimization, this study only considers organic traffic data in any month.

Specifically, only startups for which India-specific traffic accounted for at least 80% of their total web traffic are retained. These firms are further validated using the Internet Archive's Wayback Machine by examining historical website content to confirm India-focused operations (e.g., service locations and language). After applying these criteria, the sample used in the main analysis includes 830 firms over a three-year period from 2015 to 2017. The sample is unbalanced because some startups were founded at later points or exited (e.g., through acquisition or bankruptcy) before the end of the observation window.

Measures

Dependent variables

Our focal dependent variable for Hypothesis 1 and the key independent variable for Hypothesis 2 is startup experimentation. Prior research has employed various approaches to operationalize experimentation, often tailored to the industry context of the study. For example, experimentation has been captured by text analysis of news articles (Contigiani, 2023; Contigiani & Young-Hyman, 2022), by the release of different game genres in the mobile gaming industry (Ozcan & Eisenhardt, 2009), by the number of unique models launched in the automobile sector (Pillai et al., 2020), or by the adoption of A/B testing to make product adjustments in digital contexts (Koning et al., 2022). Drawing on the broad definition of experimentation as market-based tests that seek to assess the viability and value of a startup's business ideas (Greenstein, 2007; Shelef et al., 2024), we measure it in this paper through changes in a software firm's "tech stack" (Roche et al., 2024).

The choice of the technological components in a software firm's tech stack is inherently strategic as it influences the development of co-specialized assets and lays the foundation for future product development and refinement (Dushnitsky & Stroube, 2021; Kretschmer et al., 2022). Because of its strategic role in adapting to potential market opportunities through changes in their offerings, startups frequently revise their tech stacks over time (Ketkar & Roche, 2024). By iteratively adding, removing, or replacing technological components, startups conduct market-based tests to learn about which features resonate with target customers and how different technologies interact within the product architecture. This adaptive learning process enables startups to assess not only the technical viability of specific features but also their impact on market performance, thereby informing their decisions about product improvement. Thus, changes to the tech stack reflect fundamental experimentation as startups seek to learn from how customers respond to new product features in the process of finding a strong(er) product-market fit. Accordingly, we track startups' adoption patterns of web technologies and measure *Experimentation* as the log of one plus the number of technology components adopted or discarded (i.e., churned) relative to the previous period. A greater number of added or removed technologies indicates experimentation through reconfiguration of the technology stack, even if the total number of active components remains unchanged. We also construct an alternative measure based on the share of these changes and generate similar results. *Recent experimentation* is measured as the cumulative number of technology stack changes in the three months prior to demonetization. While this measure is not without limitations, it also reflects a core aspect of experimentation in digital startups and can be applied systematically with secondary data, enabling its potential use more broadly within the field.

Our other core variable, employed as a dependent variable in testing Hypothesis 2, is startup scaling, which we measure in the current context by the growth of web traffic. Most private new ventures lack a financial track record, making it empirically challenging to assess their scaling using traditional financial metrics. However, for website- or application-based technology startups, monthly web traffic offers insight into customer demand and thus provides a good proxy for scaling for several reasons. First, it provides a direct measure of digital startups' initial traction and predictor of future customer acquisition (Cao et al., 2024). Higher traffic indicates that more users are discovering and interacting with the product, which is a core aspect of scaling in digital markets. Second, many software startups operate in markets with demand-side increasing returns, where growth in user base enhances product value for other users (Giustiziero et al., 2023). Web traffic captures these network-driven effects, reflecting not only raw adoption but also the potential for self-reinforcing growth. Third, unlike traditional firms, software startups can accommodate additional users at minimal marginal cost. Thus, increases in web traffic indicate expansion in scale without the need for proportional increases in resources, aligning with the conceptual definition of scaling.

For each startup, monthly web traffic is measured from January 2016 to December 2017, which captures 10 months before demonetization and 14 months after the event. This time frame was selected mainly due to the relatively sharp and short-lived shock created by demonetization, and due to the limited availability of data on Indian startups in the Semrush database prior to 2016. Although scaling has been measured in research with a range of different variables, their core characteristic is invariably that they constitute some form of growth, whether in employees, revenue, assets, or some other relevant change in firm size or performance (e.g., Giustiziero et al., 2023; Lee & Kim, 2024). Reflecting the exponential nature of the scaling process, the variable, *Scaling*, is constructed as the natural logarithm of monthly web traffic volume (Cao et al., 2024; Koning et al., 2022). Because web traffic can sometimes be zero for small pre-sales firms, one is added before applying the log transformation.

Independent and control variables

Because we run difference-in-differences (DID), the explanatory variable of interest is an interaction between treated group and post-treatment period. We measure *Treated* as a binary indicator set to one if the startup is in the mobile payments, fintech, or e-commerce segments; and zero otherwise. We measure *Post* as a binary indicator set to one if after November 2016.

Startups with more abundant financial resources may be more likely to experiment with new business ideas; hence we control for *Fundraising*, measured as the total amount of capital a startup has raised, logged to correct for skew. Because a startup's existing technology base may shape its experimentation decisions, we also control for *Tech stack*, measured as the logged number of technologies installed. When estimating the effect of recent experimentation on scaling, we additionally control for *Experimentation*, measured as the log of one plus the number of technology components adopted or discarded relative to the previous period. Further, because startups' scaling strategies may depend on the extent of available slack resources, we include *Time to last funding* to account for the potential impact of financial slack on scaling.

Empirical Design

Our empirical approach employs a DID design around the announcement of demonetization described above. Drawing from industry classifications, keyword searches, and vertical indices developed by Pitchbook, we categorize the sample into six distinct segments: mobile payments, fintech (excluding mobile payments), e-commerce, business productivity software, social software, and education software. We define the affected (treated) startups as a subset of software firms that either provided a direct substitute for cash payments or incorporated digital payment functionality into their offerings, which includes the mobile payments, fintech, and e-commerce segments.[8] Software startups in the remaining three industry segments serve as control groups. Figure 1 plots the average monthly web traffic across the treated and control groups over a two-year period. Visual inspection of the figure reveals no discernible difference in web traffic trends between the treatment and control groups in the months leading up to demonetization. However, a clear divergence emerges shortly after the intervention. It is important to note that the values are plotted on a log scale; thus, although visually small, the difference in post-demonetization web traffic amounts to a substantial 18.3%.

To examine how affected startups adjusted their experimentation in response to greater demand-side opportunities induced by demonetization, we estimate the following specification:

$$\text{Experimentation}_{it} = \beta_1 \text{ Treated}_i \times \text{Post}_t + \delta \text{X}_{i,t} + \alpha_i + \alpha_t + \varepsilon_{it} \qquad (1)$$

where i denotes firm and t denotes quarter. As noted earlier, changes in the technology stack are used to infer when and the extent to which a startup has engaged in experimentation. For a startup i in quarter t, $Experimentation_{it}$ is measured as the log of one plus the number of technology components adopted or discarded (i.e., churned) relative to the previous quarter.[9] $Treated_i$ is a binary variable equal to one for startups in the mobile payments, fintech, or e-commerce segments; and zero otherwise. $Post_t$

[8] An Online Supplement provides more details on the construction of treated and control groups, including definitions, examples, web traffic trends for each segment, and a difference-in-differences analysis predicting the impact of demonetization on web traffic. Consistent with our logical arguments, we observe a significant increase in web traffic in the three treated segments relative to the control segments.

[9] While changes in the tech stack can be observed on a daily basis, measuring experimentation at such a granular level introduces many zero observations, resulting in a noisy measure. Therefore, we aggregate the measure at the quarterly level instead.

equals one for quarters in or after Q4 2016 and zero otherwise. β_1 is the coefficient of interest that captures the differential change in experimentation for treatment group relative to control group post-demonetization. $X_{i,t}$ is a vector of firm-level covariates. α_i and α_t represent firm and year-quarter fixed effects. Standard errors are clustered at the firm level.

A potential concern with this empirical strategy is that startups in the treatment and control groups may differ systematically in observable characteristics that may influence the estimate of their response to demonetization. For example, startups that have received more funding may also engage in more experimentation due to greater resource availability. To mitigate such systematic non-randomness in the treatment and control subsamples, we adopt two approaches. First, the baseline model incorporates firm fixed effects and control variables within the DID regression framework. Second, to improve comparability between treated and control firms, a DID matching estimator is implemented using coarsened exact matching (CEM; Iacus et al., 2012). Specifically, we match treated and control startups based on firm age and total fundraising as of the month before demonetization. To demonstrate the validity of the matching process, we conduct balance tests comparing pre-treatment firm characteristics between treated and control groups. The results presented in Table 1 indicate no significant differences across key observables, confirming the effectiveness of the matching procedure.

To examine how recent experimentation impacts the scaling of startups post-demonetization, we estimate the following regression specification:

$$\text{Ln(Web traffic}_{it} + 1) = \beta_1 \text{ Treated}_i \times \text{Post}_t + \delta \text{X}_{i,t} + \alpha_i + \alpha_t + \varepsilon_{it} \qquad (2)$$

In this model, i indexes firms, and t indexes months. The dependent variable is measured as the natural logarithm of one plus monthly web traffic volume. $Treated_i$ is a binary indicator equal to one for startups in the treatment group and zero otherwise. $Post_t$ equals one if the observation month is in or after November 2016 and zero otherwise. We split the sample into high and low experimentation groups and estimate Equation (2) separately for each subsample. The coefficient of interest, β_1, captures the differential change in web traffic for treated firms relative to control firms following demonetization. By comparing the effect size of β_1 across two subsamples, we can assess whether recent experimentation amplifies or dampens startup scaling post-demonetization. $X_{i,t}$ is a vector of firm-level covariates, including total funding to date, number of web technologies installed, degree of experimentation, and time since the last funding round. α_i and α_t correspond to firm and year-month fixed effects that account for time-invariant firm characteristics and any time trend that may influence web traffic. Standard errors are clustered at the firm level.

RESULTS

Table 2 presents descriptive statistics for the matched sample, including 784 startups and 8,599 firm-quarter observations. 2,390 firm-quarter observations are in the treatment group and 6,209 in the control group. Sample descriptive statistics indicate that, first, startups in the sample experiment with an average of 3.8 technology components per quarter. Second, such experimentation is moderately correlated with fundraising. Third, the dependent variable exhibits considerable variation, highlighting the importance of understanding how startups adjust their levels of experimentation in response to emerging demand-side opportunities.

The Effect of Demand-Side Opportunities on Experimentation

The DID estimation results are presented in Table 3. All three specifications show a significant reduction in the total amount of experimentation following demonetization. Specifically, demand-side opportunities induced by demonetization are associated with a decrease in experimentation by 32.9 percentage points ($\beta = -0.329$, $p = .000$), corresponding to a 28.0% decline relative to the control group. This finding is consistent with Hypothesis 1, indicating that startups reduce experimentation to better capitalize on demand-side opportunities.

We further examine the validity of the assumption that the treated and control firms follow similar pre-demonetization trends in experimentation (Angrist & Pischke, 2008) by using an event study design. Model (4) estimates quarter-by-quarter changes in experimentation around the time of demonetization, using the quarter immediately prior to the event (*t-1*) as the baseline. The results suggest that the coefficients on all pre-demonetization indicators (from *t-7* to *t-2*) are indistinguishable from zero, suggesting no preexisting trend in the data. Consistent with our DID results, a substantial decrease in experimentation is observed in the post-demonetization periods (from *t* to *t+4*). Figure 3 exhibits the coefficient plots from the event study and corroborates the parallel trends shown in Figure 2.

To ensure robustness, the above analysis is replicated using an alternative measure of experimentation, operationalized as the share (as opposed to number) of technologies entering or leaving the firm's technology stack. As shown in Model (5) of Table 3, the results remain unchanged when experimentation is normalized by the firm's technology stack size, indicating that the findings are not sensitive to this alternative measure. Taken together, these findings are consistent with the theoretical conjecture that emerging demand-side opportunities induce startups to allocate fewer resources toward experimental activities. The sections below further explore two heterogeneous treatment effects and conduct mechanism tests that provide additional evidence for the theoretical mechanisms we propose.

Heterogeneity in the treatment effect

Financial slack. The trade-off between experimentation and scaling may be less pronounced for startups with greater slack resources, as they can simultaneously allocate resources to both activities. Thus, if the opportunity cost mechanism is the core one at play, startups with limited slack should experience a larger decline in experimentation. To test this hypothesis, we use financial slack, measured as the number of months since the last funding round, as a proxy for a startup's overall slack resources. We split the sample at the median value and classify startups with above-median values as low-slack (i.e., financially constrained) and those below the median as high-slack. The results (reported in the Online Appendix) indicate that the DID coefficient is significantly larger in magnitude for the low-slack subsample (β = -0.495, p = .000) compared to the high-slack group (β = -0.236, p = .005). A seemingly unrelated regression (SUR) test confirms that the difference in coefficients across these subsamples is statistically significant (p = .018). This finding is consistent with the logic that startups weigh the opportunity costs of delaying scaling against the potential learning benefits from experimentation when making decisions about how to allocate their scarce resources.

Platform business model. We further explore heterogeneous treatment effects by distinguishing between platform-based and non-platform startups, since platform-based firms often derive greater preemptive benefits and first-scaler advantages. Therefore, if firms are motivated to accelerate customer acquisition to capture preemption benefits, we expect to observe a greater reduction in experimentation among platform-based firms. To identify whether a firm adopts a platform-based business model, we rely on the business descriptions provided by Pitchbook. If the description contains the keywords "platform" and/or "marketplace," we code the firm as a platform. The results (reported in the Online Appendix) show that the effect of demand-side opportunities is significantly larger for platform-based firms (β = -0.470, p = .000) than for non-platform firms (β = -0.203, p = .016). The SUR test indicates that this difference is statistically significant (p = .015). Together, these findings are consistent with high preemption value serving as a key mechanism underlying the observed reduction in experimentation.

Mechanism tests

Although the findings discussed in the previous section are compelling, this section seeks to offer additional evidence that heightened opportunity costs may plausibly explain the observed reduction in experimentation. If increased opportunity costs are indeed the primary driver of the findings regarding experimentation, one would expect to see a decrease in the addition of premium (i.e., paid) web technologies to software startups' post-demonetization tech stack, which tend to be more sophisticated and thus require more time and resources to implement and test. The results (reported in the Online Appendix) of estimating Equation (1) using the number of paid technologies added as the dependent variable. The negative DID coefficient (β = -0.040, p = .018) implies that startups became less likely to engage in costly experimentation activities, consistent with the opportunity cost explanation.

Next, we examine whether startups redirected their investments toward demand-facing activities, proxied by the adoption of marketing technologies, including those related to data analytics and advertising. The constructed measure sums up the number of such technologies adopted by the focal firm in each quarter and takes the natural logarithm of one plus this value. The results (reported in the Online Appendix) show a significant positive effect (β = 0.326, p = .017), indicating an increased investment in marketing technologies post-demonetization. Similarly, we use paid web traffic as a proxy for marketing expenses to test whether the affected startups allocated greater resources to marketing to attract customers after demonetization. The results shown in Model (3) are consistent with the previous findings: the estimated DID coefficient is positive and statistically significant (β = 0.040, p = .055), indicating higher resource allocation toward customer acquisition and consequently an implied opportunity cost of experimentation after demonetization. Together, these results are consistent with the rationale that startups reduced experimentation after demonetization due to the high opportunity cost of not acting on emerging demand-side opportunities.

The Role of Recent Experimentation in Scaling

Having identified the impact of demand-side opportunities on experimentation, this section examines evidence for Hypothesis 2: whether recent experimentation negatively impacts the scaling of startups in the presence of strong demand-side opportunities, which may in turn explain why startups reduce experimentation in the post-demonetization period. In general, estimating the effect of a firm's experimentation on its scaling is empirically challenging because firms typically incorporate scaling considerations into their decisions to experiment; put simply, the choice to experiment is endogenous. However, because the demonetization shock was so sudden and unexpected, experimentation by firms in the immediate period before the shock is plausibly exogenous to the demand conditions created by the shock, while at the same time impacting the ability of firms to benefit from the resulting demand-side opportunities.

The results in Models (1) and (2) of Table 4 suggest that the treatment effect is stronger for startups with lower levels of experimentation than for those with higher levels of experimentation. Specifically, the coefficient on *Treat × Post* is positive in the low experimentation subsample (β = 0.647, p = .001), but statistically indistinguishable from zero in the high experimentation subsample (β = 0.172, p = .239). These findings are consistent with Hypothesis 2, suggesting that investments in experimentation might be "sticky" and represent strategic commitments that are difficult to reverse in the short run. Thus, startups engaged in intensive experimentation may be less capable of withdrawing resources from experimentation and reallocating them to respond quickly to emerging demand-side opportunities. Alternatively, experimentation may entail substantial organizational commitments to exploratory learning from new product variants introduced into the market, which may make it difficult to reorient into more "exploitative" efforts to scale up in the market (March, 1991).

To assess whether these results are an artifact of how prior experimentation is defined, we use alternative time windows to define prior experimentation and observe consistent results (see Supplementary Table 3). Moreover, we construct a measure to indicate a startup's accumulated experimentation within one, two, and three years of founding. Consistent with the consensus that early-stage experimentation helps startups achieve product-market fit and generally benefits scaling, we find that those with higher levels of accumulated experimentation exhibit greater scaling post-demonetization (see Supplementary Table 4). Thus, the negative effects of experimentation on startup scaling do not appear to be a general relationship, but one that is contingent on the presence of significant demand-side opportunity, consistent with the overall thesis of this paper.

DISCUSSION

In this study, we examine how startups adjust their ongoing experimentation in response to demand-side opportunities and how experimentation, in turn, affects the scaling of startups under these conditions. Specifically, we leverage a policy intervention—the 2016 Indian demonetization—as the empirical setting that created an unexpected increase in demand for a subset of startups. Using a rich dataset of Indian software startups, we find that stronger demand-side opportunities are associated with a substantial reduction in experimentation. Further, this effect is especially pronounced among startups more constrained by the trade-off between experimentation and scaling, such as those with limited financial slack, and those for whom delaying scaling is more costly and less reversible, such as platform-based startups. Together, the empirical evidence is consistent with the conjecture that in the presence of stronger demand-side opportunities, the heightened opportunity cost of experimentation, combined with lowered value of experimentation, discourages startups from experimenting and instead pushes them toward rapid scaling to outpace competitors.

Building on the existing literature that emphasizes the importance of experimentation in scaling, we then assess how recent experimentation influences the scaling of startups exposed to greater demand-side opportunities. Our findings indicate that startups with lower levels of pre-demonetization experimentation benefited disproportionately from the surge in demand. We theorize that the strategic commitments inherent in experimentation and the potential costs of product refinement and reputational loss may constrain startups' ability to take advantage of emerging demand-side opportunities, thus diminishing their scaling relative to less experiment-intensive peers.

Limitations

The findings of this study are subject to several caveats, which may present promising directions for future research. A key limitation is that the results are derived from an idiosyncratic setting, raising questions about their generalizability. In principle, the theoretical insights gained can extend to contexts characterized by high scalability-driven opportunity costs, which may arise from a variety of scalability enablers, such as compelling value propositions, demand disruption for substitutes, highly scalable resources, significant network effects, or social contagion effects like virality. Future research could test the proposed theoretical mechanisms in other settings to assess their generalizability. Furthermore, the opportunity cost logic proposed in this study builds on the assumption that startups reduce experimentation under demand-side opportunities in pursuit of better short-term performance. However, there may also be longer-term advantages to maintaining higher levels of experimentation even in the presence of demand-side opportunities. An important area for future inquiry would be to examine how reductions in experimentation affect long-term performance outcomes, such as higher survival rates, more fundraising, or an increased likelihood of acquisition. Moreover, while the reconfiguration of startups' technology stacks reflects a critical aspect of experimentation, it does not capture all experimentation activities within a firm. Future research can collect finer-grained data to shed light on other aspects of experimentation, such as the choice of revenue models and pricing strategies.

Contributions

Despite the limitations noted above, this study contributes to the literature in three important ways. First, it advances ongoing research on startup scaling strategies (e.g., Lee & Kim, 2024; Tidhar et al., 2025) by identifying a critical boundary condition under which experimentation may be less effective or even detrimental for scaling. To date, most studies in this domain have emphasized experimentation as a valuable learning mechanism that facilitates scaling, without explicitly considering the role of demand conditions. The current study addresses an intriguing knowledge gap in our understanding of whether experimentation plays a different role under varying demand conditions. It indicates that demand-side opportunities and experimentation may act as strategic substitutes in driving startup scaling. Extant research suggests that a firm's scalability is a function of the interaction between its resource bundles and market demand characteristics (Giustiziero et al., 2023; Li et al., 2023). While this literature implies that high scalability leads to successful scaling, it often treats the process of aligning resource bundles and market demand in generating actual scaling as a black box. Meanwhile, a parallel stream of research emphasizes experimentation as a key learning mechanism that facilitates this process and enhances scaling. However, this literature broadly assumes that startups follow a sequential path of experimentation first and scaling later, while overlooking how demand-side opportunities may shift the trade-offs between the need to experiment and the imperative to scale. Albeit apparently interrelated, these two literatures have hitherto remained largely disconnected in our understanding of firm scaling. By integrating insights from both literatures, this study provides a more comprehensive understanding of the roles played by demand-driven scalability and experimentation in firm scaling. The findings suggest that scalability advantages arising from demand side might substitute for the value of experimentation in successful scaling. More broadly, this paper contributes to the ongoing debate in entrepreneurship about whether entrepreneurs operate in a discovery context—where they develop new businesses with clear goals in mind and adopt a scientific, experiment-driven approach—or in a creation context, in which they proactively shape the environment as they move forward rather than making calculated decisions (Alvarez & Barney, 2007; Camuffo et al., 2020; Leatherbee & Katila, 2020; Sarasvathy, 2001). By highlighting the important role of demand environments, this study responds to recent calls to delineate and empirically test the boundary conditions under which each theoretical lens offers a more appropriate perspective on entrepreneurial action (Alvarez et al., 2024).

Second, this study contributes to the literature on experimentation by highlighting its potential downsides in the entrepreneurial process (Contigiani, 2023; Gans et al., 2019). In contrast to the emphasis of prior work on when firms should experiment (Chen et al., 2024; Shelef et al., 2024) or the importance of learning "at the right time" (Tidhar et al., 2025: 114), this study uncovers when and why startups might choose to experiment *less*. It theorizes that demand-side opportunities raise the opportunity cost of experimentation and increase the benefits of accelerating scaling to preempt rivals, which in turn reduce the need for experimentation. In this respect, our theorizing about the opportunity cost of experimentation responds to recent calls for research on "the shadow cost of learning by experimentation" (Gans et al., 2019: 752). More broadly, this study contributes to entrepreneurship theory by identifying a boundary condition under which creation or effectual logic becomes more useful than the scientific method in new venture strategy (Alvarez et al., 2024; Fisher, 2012; Read et al., 2009).

Lastly, this study adds to the literature on Indian demonetization by focusing on its implications for firm strategy, whereas most existing research has examined its impacts on individual spending and social welfare (e.g., Agarwal et al., 2024; Chodorow-Reich et al., 2020). Although digital payments have gained widespread popularity around the world, India remained heavily cash-dependent prior to demonetization but has now experienced a revolution in digital finance. The early slow adoption of digital payments can be attributed to inherent frictions in the country, such as low internet penetration, limited access to banking infrastructure, and a prevailing preference for cash transactions. This study suggests that demonetization may have helped overcome these obstacles to digital payment adoption. Although demonetization was a temporary policy intervention, the findings indicate that its effects on startup scaling have persisted over time. In addition, there were substantial demand-side gains in adjacent software industry segments that incorporate digital payment functionality. Overall, these findings highlight the important role that policy interventions can play in helping entrepreneurial firms overcome demand-side constraints and accelerate long-term growth.

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TABLES AND FIGURES

Table 1. Pre-demonetization characteristics for treatment and control sample

	Mean_control	Mean_treated	Mean_differences	t-statistic	p-value
		Before matching			
Fundraising(ln)	0.49	0.75	-0.26	-3.94	0.00
Tech stack(ln)	3.65	3.61	0.04	0.77	0.44
Firm age	4.39	4.02	0.36	2.04	0.04
Platform BM	0.59	0.64	-0.04	-1.06	0.29
Time to last funding	15.47	12.48	2.99	2.54	0.01
Ongoing experimentation	1.38	1.49	-0.11	-1.29	0.20
		After matching			
Fundraising(ln)	0.74	0.74	0.00	0.02	0.99
Tech stack(ln)	3.56	3.60	-0.04	0.58	0.56
Firm age	4.00	4.00	0.00	0.02	0.98
Platform BM	0.60	0.65	-0.05	1.26	0.21
Time to last funding	13.42	11.87	1.55	-1.48	0.14
Ongoing experimentation	1.40	1.42	-0.02	0.29	0.77

Notes. Matching variables are firm age and fundraising in the month just before demonetization.

Table 2. Descriptive statistics

	Mean	SD	Min	Max	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) Experimentation(ln)	1.57	1.05	0.00	4.44							
(2) Treat	0.28	0.45	0.00	1.00	0.05						
(3) Post	0.44	0.50	0.00	1.00	0.22	-0.01					
(4) Fundraising(ln)	0.53	0.84	0.00	5.05	0.25	0.14	0.24				
(5) Tech stack(ln)	2.59	1.76	0.00	5.12	0.05	0.00	-0.00	0.00			
(6) Firm age	4.41	2.26	1.00	11.00	-0.09	-0.10	-0.04	0.24	-0.01		
(7) Platform BM	0.61	0.49	0.00	1.00	0.05	0.07	0.00	0.02	0.01	-0.19	
(8) Time to last funding	16.24	16.01	0.00	131.00	-0.06	-0.10	0.36	-0.18	-0.28	0.22	-0.10

Notes. The sample includes firm-quarter observations between January 2015 and December 2017.

Table 3. Difference-in-differences regressions of demand-side opportunities on experimentation by Indian software firms

	Ln (1 + added and dropped technologies)				Added and dropped technologies / Technologies in tech stack
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Unmatched	Matched			
Treat × Post	-0.341***	-0.345***	-0.329***		-0.060***
	(0.053)	(0.059)	(0.058)		(0.013)
t-7				0.060	
				(0.199)	
t-6				0.010	
				(0.185)	
t-5				0.005	
				(0.190)	
t-4				-0.041	
				(0.162)	
t-3				-0.118	
				(0.147)	
t-2				-0.005	
				(0.150)	
t				-0.336***	
				(0.130)	
t+1				-0.354**	
				(0.146)	
t+2				-0.518***	
				(0.145)	
t+3				-0.277*	
				(0.159)	
t+4				-0.391**	
				(0.158)	
Fundraising(ln)			0.076	0.073	0.006
			(0.062)	(0.064)	(0.018)
Tech stack(ln)			0.266***	0.261***	-0.019
			(0.041)	(0.041)	(0.020)
Observations	9,114	8,599	8,599	8,599	8,599
R-squared	0.021	0.020	0.032	0.044	0.047
Number of firms	830	784	784	784	784
Firm FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes

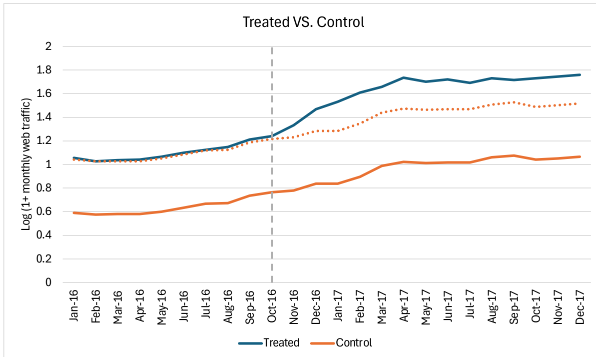
Notes. This table reports the difference-in-differences tests that examine the impacts of the demonetization on a startup's experimentation. The unit of observation is the firm-quarter. In column (1), the analysis is based on the full sample; in columns (2) to (5), the analyses are based on a matched sample. In columns (1) to (4), the dependent variable is the log of one plus the number of added or dropped technologies. In column (5), the dependent variable is the number of added or dropped technologies divided by the total number of technologies in a firm's technology stack.
*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are in parentheses.

Table 4. The role of (recent) experimentation in Indian software firms' web traffic

	Log (1+ monthly web traffic)			
	Full sample		Founding < 2014	
	(1)	(2)	(3)	(4)
VARIABLES	High	Low	High	Low
Treat × Post	0.172	0.647***	-0.044	0.693**
	(0.146)	(0.190)	(0.190)	(0.269)
Fundraising(ln)	0.208	0.392*	0.158	0.755
	(0.135)	(0.226)	(0.196)	(0.568)
Tech stack(ln)	1.309***	1.628***	1.122***	1.549***
	(0.205)	(0.267)	(0.335)	(0.442)
Experimentation(ln)	-0.002	-0.003	-0.004	0.000
	(0.005)	(0.006)	(0.006)	(0.007)
Time to last funding	-0.039**	-0.090***	-0.056**	-0.102***
	(0.019)	(0.028)	(0.025)	(0.035)
SUR test (*p*-value)	0.046**		0.025**	
Observations	8,778	7,678	4,938	4,601
R-squared	0.368	0.303	0.323	0.262
Number of firms	397	345	216	202
Firm FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes

Notes. The dependent variable is the log of one plus the total number of monthly page visits. In columns (1) and (2), heterogeneous treatment effects of pre-demonetization experimentation are tested by splitting the sample based on the sample median in the main analyses. Firms with above- (below-)median experimentation are classified as high- (low-)experimentation firms. The SUR *p*-value of the difference between the coefficients of *Treat × Post* of the high-experimentation and low- experimentation groups is reported in the middle of columns (1) and (2). Columns (3) and (4) limit the sample to startups founded before 2014 to examine the heterogeneous treatment effect of experimentation, revealing a stronger effect. The SUR *p*-value of the difference between the coefficients of *Treat × Post* of the high-experimentation and low- experimentation groups is reported in the middle of columns (3) and (4). The results are consistent when using three-way interactions (see Tables S1 and S2).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors are in parentheses.

Figure 1. Trends in web traffic by segment: Before and after demonetization



Notes. This figure shows trends in web traffic averaged across three treated groups and three control groups between January 2016 and December 2017. The dotted orange lines represent an upward shift of the control group trends (solid orange lines) for visual comparison purposes.

Figure 2. Plot of experimentation in a matched sample

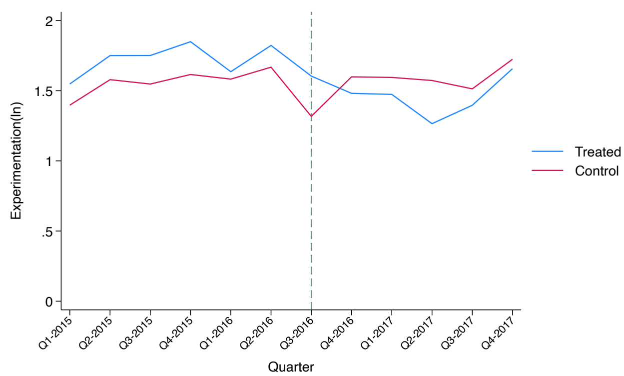
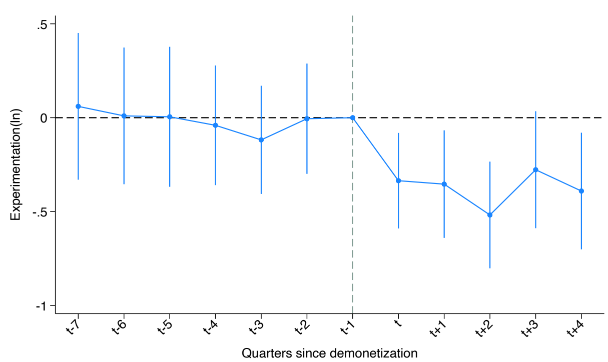


Figure 3. Event study plots for experimentation



Notes. This figure shows the event study covering the period from seven quarters before to five quarters after demonetization.

[The online appendix has been omitted due to space constraints]