Experimentation, Learning, and Appropriability in Early-Stage Ventures

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Keywords: Experimentation; Learning; Appropriability; Entrepreneurship; Software.
1 Introduction

Entrepreneurs are commonly advised to rely on cheap and frequent experiments to navigate the ambiguity of new venture creation. This perspective is promoted by a variety of practitioner-oriented approaches, such as Agile Development (Highsmith and Cockburn, 2001), Customer Discovery (Blank, 2007), and, in particular, the Lean Startup (Ries, 2011, 2017). More recently, also the academic literature has emphasized the importance of experimentation in entrepreneurship (Kerr, Nanda, and Rhodes-Kropf, 2014; Manso, 2016; Eisenhardt and Bingham, 2017).

However, while the learning benefits of experimentation are largely undisputed, we know little about its costs. The innovation literature has highlighted the role of appropriability, the capacity of a firm to appropriate the value created by its invention (Teece, 1986). This concern is central to early-stage ventures, which may be particularly vulnerable to misappropriation (Katila, Rosenberger, and Eisenhardt, 2008). While the practitioners’ perspective tends to dismiss this concern (Blank, 2013), experimentation requires disclosure of an early-stage idea to the market (Arrow, 1972) and thus may favor imitation. Therefore, appropriability is a good starting point in understanding the costs of experimentation.

The purpose of this study is to examine the role of appropriability in the process of entrepreneurial experimentation. More specifically, I investigate how the appropriability regime affects the choice of experimentation in early-stage ventures. Consistent with the practitioners’ terminology, I view experimentation as the disclosure of an incomplete product prior to market entry with the purpose of obtaining customer feedback. I argue that, when intellectual property (henceforth, IP) is weak, the learning benefit of experimentation may be offset by its imitation risk. Combining quantitative analysis and qualitative evidence, I find that ventures respond to appropriability concerns by experimenting less but entering the market earlier.

Experimentation is a common “management practice” in many innovative contexts. In the software industry, firms experiment by circulating beta products, incomplete software packages aimed at early adopters. In hardware, experimentation takes the form of prototyping, the process of building an early-stage product with basic features and cheap materials. In the research profession, academics often experiment by circulating a working paper, a preliminary version of a research paper. While taking different forms in different contexts, the strategic motivation driving these efforts is the need for obtaining market feedback prior to finalizing the product.

Drawing from the literatures on organizational learning and appropriability, I propose that experimentation is associated with both learning benefits and imitation risks. The goal of experimenting is to obtain information about the market and incorporate it into the product development process. Especially when the environment is particularly uncertain, this is an effective form of learning. However, experimentation requires disclosing at least part of the idea to the market, and
thus creates opportunities for imitation. The degree of appropriability is determined by two forms of IP: formal IP (henceforth, FIP), including patents, copyrights, trademarks, and secrecy, and informal IP (henceforth, IIP), including complexity, capital investment, lead time, and networks. If the venture can effectively protect its innovation through some form of IP, experimentation is a viable strategy, otherwise it is risky. Generally, IIP is developed in the long run, therefore FIP is the primary tool available to early-stage ventures. Based on this argument, I hypothesize that, when FIP is weak, ventures are less likely to experiment.

I test this hypothesis in the US software industry. Due to the low upfront cost of experimentation typical of this setting (Ewens, Nanda, and Rhodes-Kropf, 2018), ventures have the leeway to make decisions about experimentation based on strategic considerations rather than financial or regulatory constraints. I assemble a hand-collected dataset of over 1200 US-based software ventures founded in period 2012-2016. I analyze over 6,000 online sources to measure experimentation, exploiting the terminology of the software release life cycle (henceforth, SRLC). Specifically, I view instances of product testing (primarily, beta testing) as experiments.

I employ a novel identification strategy to causally estimate the impact of FIP on experimentation, exploiting a 2014 US Supreme Court ruling – Alice Corp v CLS Bank International (henceforth, Alice) – that has decreased the effectiveness of patenting business software relative to non-business software. I analyze the data using a difference-in-differences approach via linear regression and survival analysis. To guide the quantitative analysis and validate the findings, I rely on qualitative work in the form of interviews and a survey.

The analysis produces four main results. First, consistently with the hypothesis, affected ventures are less likely to experiment. Alice leads to lower propensity of experimentation (up to 5% lower, Table 2) and lower hazard of experimentation (up to 29% lower, Table 4). Second, affected ventures react by revising their broader strategy, and enter the market earlier. Alice leads to higher propensity of entry (up to 6% higher, Table 6) and higher hazard of entry (up to 25% higher, Table 5). Third, these patterns largely depend on ventures’ learning incentives. The negative effect of Alice on experimentation is driven by ventures with low learning incentive, while the positive effect of Alice on entry by those with high learning incentive. Fourth, the response to Alice is also moderated by the degree of competition. The negative effect of Alice on experimentation is driven by ventures facing high competition, while the positive effect of Alice on entry by those facing low competition. Overall, the evidence suggests that ventures perceive the learning-appropriability tension, and respond to a decrease in FIP by shifting from pre-entry learning to post-entry learning.

The paper proceeds as follows. Section 2 illustrates the theoretical framework, defining experimentation, highlighting the tension between learning and appropriability, and stating the hypothesis. Section 3 describes the empirical setting – the US software industry – explaining the reasons that make it an appropriate “laboratory” for the study of experimentation. Section
4 describes the data. Section 5 reports the empirical results, including the main effects of Alice on experimentation and entry, the moderating effects of learning incentive and competition, and a series of additional analyses. Section 6 concludes. The paper has been shortened due to space constraints: sections 2 and 3 have been shortened, section 6 has been removed, and the Appendix has been removed.

2 Theory

I describe a simple theoretical framework to guide the empirical analysis. I start by defining experimentation in the context of a new venture. I then discuss the implications of experimentation in terms of learning and appropriability. Finally, I state the main hypothesis, highlighting the tension between learning and appropriability.

2.1 Experimentation

The venture creation process starts when the founding team commits to an idea. This moment marks the beginning of the development stage. When the venture completes the development process, it enters the market, commercializing the product.1

Firms, by definition, start when a founding team identifies an opportunity, generates an idea, and decides to establish a firm to pursue it (Shane and Venkataraman, 2000; Alvarez, Barney, and Anderson, 2013). An idea can be seen as a set of beliefs and hypotheses regarding how the product creates value and how the firm captures part of that value (Felin and Zenger, 2017). Once this commitment is made, the venture seeks to assemble the necessary resources to build the product.

At some point in time, most ventures conclude the product development process, commercialize the product, and enter the market (Helfat and Lieberman, 2002). Market entry is a well-understood moment in the venture life cycle (Wu and Knott, 2006). Among practitioners, market entry is referred to as the product launch.2 Typically, the process of launching a product involves a variety of marketing investments. Furthermore, through the response of its customers, the venture obtains feedback, and can choose to adapt to this information by changing the product over time.

However, the venture can choose to seek market feedback prior to the completion of the product development process. I define an experiment as the disclosure of an incomplete product to the market prior to market entry.3 In other words, the venture can choose to test its hypotheses

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1I will be using the term “product” broadly to include any kind of product or service created for commercial purposes.

2This is widely discussed in the popular press. For example, see venturebeat.com/2016/11/12/how-to-launch-your-startup/ or agileana.com/blog/definitionsoflaunchvshardlaunchvbsbeta/.

3This notion of experimentation is different from the practice of AB testing (Kohavi, Deng, Frasca, Longbotham,
(Camuffo, Cordova, Gambardella, and Spina, 2018) by getting market feedback and revise them in preparation for market entry. Therefore, this notion of experimentation requires two elements. First, it requires disclosing an incomplete version of the product outside the organization. Purely internal testing is not part of this definition. Second, it requires that this process occurs prior to market entry. Product changes taking place after entry are not part of this definition.

Experimentation is a strategic move that ventures can take to learn about the market. More precisely, a venture chooses whether or not to experiment and, conditional on experimenting, the timing of the experiment. Experimenting early implies disclosing a more immature product, obtaining market feedback early, when adaptation costs are low. Experimenting late implies disclosing a semifinished product, accessing external information once most of the investment has been made, and adaptation costs are higher. Not experimenting, finally, implies entering the market directly with no prior product disclosure.

Experimentation affects performance through the channels of learning and appropriability. A common lens to examine the impact of strategy on performance is the value-based business strategy (Brandenburger and Stuart, 1996). Using this perspective, firm performance is determined by the value it creates and the value it captures. In the context of early-stage ventures, learning drives value creation, while appropriability drives value capture.

### 2.2 Learning

By circulating early-stage versions of the product to the market, the venture can learn about the preferences of its potential customer group and adapt the product development process accordingly.

The learning process arising through experimentation is experiential and intentional\(^4\). Learning is experiential because it is based on direct experience on the market, rather than pure cognitive analysis (Levinthal and March, 1981; Gavetti and Levinthal, 2000). It is intentional because it follows an explicit, purposeful decision-making process (Lynn, Morone, and Paulson, 1996; Murray and Tripsas, 2004). The knowledge obtained through this process originates primarily in the feedback offered by customers, so it is essentially the result of processes such as “learning by using” (Rosenberg, 1982; Mukoyama, 2006) and “user innovation” (von Hippel, 1986; Gambardella, Raasch, and von Hippel, 2016).

While experimentation provides learning benefits, it would unlikely produce other sources

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\(^4\)Learning through experimentation is one specific form of learning. Naturally, entrepreneurs can learn in different ways. In section 6, I discuss alternative forms of learning relevant to entrepreneurship and their relation to experimentation.
of first-mover advantage (Lieberman and Montgomery, 1988, 1998). Because experimentation does not include complementary investments in marketing, it does not credibly play a preemptive function or create substantial switching costs.

Overall, experimentation allows the venture to learn about customers’ preferences at relatively low adaptation costs, and through this channel it enhances value creation.

### 2.3 Appropriability

Misappropriation is a critical factor in the survival of early-stage ventures. When ventures seek market feedback through experimentation, they are essentially giving the market access to information about their idea, thus being at risk of imitation (Lieberman and Asaba, 2006).

Ventures can alleviate the threat of imitation through IP protection. IP mechanisms can be formal or informal (Hall, Helmers, Rogers, and Sena, 2014). FIP includes legal tools such as patents, copyrights, trademarks (Ramello and Silva, 2006), and secrecy (Png, 2017a,b). IIP includes strategic tools such as complexity (Szulanski, 1996; Rivkin, 2000), lead time, capital requirements, and partners’ network positions (Hallen, Katila, and Rosenberger, 2014).

Therefore, after entry, the venture can protect its innovation through IIP. Prior to entry, absent some form of FIP, experimentation may lead to imitation and thus reduce value capture.

### 2.4 Learning-Appropriability Tension

The choice of experimentation affects performance through the channels of learning and appropriability. Learning via experimentation increases value creation, but the associated imitation risk decreases value capture. This creates a tension between learning and appropriability during the time window between the start of experimentation and entry, as illustrated in Figure 1.

If the venture is able to protect the idea through FIP, it can experiment safely, collecting market feedback but avoiding the threat of imitation. If FIP is weak, the venture is vulnerable to imitation, and experimentation may be risky. If ventures are at least partially rational, they would be responding to this trade-off. This argument leads to the hypothesis I seek to test.

**Hypothesis.** When formal intellectual property is weak, ventures are less likely to experiment.

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5Katila et al. (2008) examine this tension in the context of resource acquisition. When early-stage ventures seek to acquire resources by partnering with established firms, they may give potential partners access to their own resources and are, therefore, at risk of misappropriation.

6A stream of work in economic theory examines this “free riding” problem through the framework of bandit models (Bolton and Harris, 1999; Keller, Rady, and Cripps, 2005).

7The literature has documented that large firms make substantially different choices in terms of their IP portfolio, combining different elements of FIP and IIP (Levin, Klevorick, Nelson, and Winter, 1987; Cohen, Nelson, and Walsh, 2000; Graham, Merges, Samuelson, and Sichelman, 2009; Alcacer, Beukel, and Cassiman, 2017).

8Secrecy is a mixed form of IP. It is regulated by law but implemented by firms’ strategic choices.
3 Setting

I test this hypothesis in the context of the US software industry. Software is a fundamental sector of the economy (Arora, Branstetter, and Drev, 2013; Branstetter, Drev, and Kwon, 2018), driving innovation and growth in many developed countries. Furthermore, this context is appropriate for the study of experimentation for two reasons. First, one of the institutional aspects of this industry – the terminology used to describe the stages of the product development process – may be exploited to measure experimentation. Second, a recent development in the US intellectual property regulation affecting this industry offers an opportunity for causal identification.

3.1 Software Release Life Cycle

Unlike most other industries, software features a well-defined and largely shared terminology describing the stages of the product development process, generally known as the SRLC.

In particular, the practice of circulating an incomplete product is almost universally known as “beta testing”. MacCormack, Verganti, and Iansiti (2001) write

*We define a beta version as a version of the product that contains at least part of all the core component modules (even though these modules may be functionally incomplete), and hence can function as a system.*

Additional keywords commonly used to define incomplete products circulated for testing purposes include “alpha”, “pilot”, “trial”, and “prototype”. While these notions marginally differ, they all indicate an effort to disclose an incomplete product to the market with the objective of receiving feedback. Appendix A1 provides additional details about the SRLC.

This institutional aspect is critical for the measurement of experimentation. In most settings, it is challenging to distinguish commercialization efforts primarily devoted to learning (i.e., experiments) from commercialization efforts primarily seeking financial value (i.e., launches). In software, the SRLC terminology provides one way to overcome this problem. I will be treating evidence that a venture has made a testing effort as instance of experimentation.

Naturally, one concern with using this measurement approach is that companies might use the SRLC terminology strategically. However, I believe the incentive structure of early-stage ventures makes this scenario unlikely, as I discuss in detail in Appendix 1. Furthermore, my qualitative analysis found no clear evidence of a systematic bias in this respect. Most survey responses suggested that testing is largely viewed as a way to obtain feedback, and thus it is a form of experimentation. Therefore, while I cannot perfectly rule out this possibility, I believe its weight on the overall analysis is limited.
3.2 Alice Corp v CLS Bank International

Patent protection plays a multifaceted role in the context of early-stage ventures. Survey evidence by Graham and Sichelman (2008) and Graham et al. (2009) suggests that ventures use patents to prevent copying, secure financing, and enhance reputation. Hsu and Ziedonis (2013) suggest that patents provide “dual sources” of advantage, limiting competition and signaling quality. Ultimately, while not the only reason, the rationale for using patents to reduce the risk of imitation is relevant for ventures, despite the cost of patent filing.

The situation is more complex for ventures operating in the software industry (Bessen and Hunt, 2007; Allison and Mann, 2007; Lemley, 2012; Hall and MacGarvie, 2010). However, Graham et al. (2009) document that the use of patents in software ventures, while generally less pronounced than for ventures in other high-technology sectors (biotechnology, medical devices), is still relevant. The survey documents that 24% of software ventures hold patents or have applied for patents and that the average number of patents or patent applications per venture is 1.7 (Figure 1, page 1277). Further, software ventures seek patent protection primarily for two reasons: preventing imitation and enhancing reputation (Figure 3, page 1301). Finally, if they choose not to seek patent protection, software ventures do so primarily due to the cost of filing and the cost of enforcing (Table 2, page 1313).

Much of the uncertainty around patenting in software is due to history. Patenting software in the US system became established with the 1998 ruling State Street Bank & Trust Co. v Signature Financial Group, Inc by the US Court of Appeals for the Federal Circuit. This decision started the trend of patenting in business methods and software (Lerner, 2002). The trend decelerated with US Supreme Court rulings Bilski v Kappos in 2010 and Mayo Collaborative Services v. Prometheus Laboratories, Inc in 2012. These decisions created some degree of uncertainty in the patenting landscape, but did not radically change the environment. The occurrence of Alice in 2014 is widely considered the event that reshaped the IP regime in the US software industry.

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9Jeffrey Schox, a well-known Silicon Valley IP lawyer, discusses the reasons to file a patent for ventures. He lists the following as “bad reasons”: enforcement against competitors, preventing overlapping patenting from competitors, and generating license revenues. Instead, he recommends filing for the following “good reasons”: want to attract investments, deter patent infringement lawsuits, and facilitate partnerships with other firms. See www.youtube.com/watch?v=hyv75d4JG7c for a discussion.

10Firms that file patent applications obtain the status of “patent pending”. This is especially relevant since the introduction of AIPA in 1999 (Graham and Hegde, 2015), which made patent applications visible after 18 months from the time of filing.

11Financially constrained ventures often qualify for lower application fees and may choose to file provisional applications. Please see www.uspto.gov/patentsgettingstarted/patentbasics/typespatentapplications/provisionalapplication-patent. Provisional applications have become particularly common when the US moved to the “first-to-file” system with the American Invents Act, conforming to the other major economies around the world. The act was signed in September 2011 and entered into force in March 2013. Notice that this institutional change affected all firms in the US and thus does not confound the empirical analysis presented here.

12Mark Lemley, a leading scholar in patent law, argues “I think Alice is a real sea change on the
Alice Corp\textsuperscript{13} owned four patents\textsuperscript{14} protecting an electronic escrow service, a system to reduce settlement risk in financial transactions. Alice argued that CLS Bank was infringing on these patents by using a similar technology. In response, CLS Bank sued Alice Corp in the US District Court for the District of Columbia in May 2007, seeking a declaration that such accusations were invalid. After the District Court declared the patents invalid, Alice Corp appealed the decision to the US Court of Appeal for the Federal Circuit. The Court of Appeal initially reversed the District Court’s opinion, then reheard the case “en banc”, and finally agreed with the District Court in May 2013, albeit in a very divided opinion\textsuperscript{15}. At this point, Alice petitioned for “certiorari”, thus essentially asking the Supreme Court to intervene on the case. Given the importance of the case, and the public opinion’s interest in it, the US Supreme Court agreed to examine the case in December 2013. The Supreme Court heard oral arguments on March 31, 2014 and issued its opinion on June 19, 2014\textsuperscript{16}.

The Supreme Court invalidated the patents due to the lack of patentable subject matter under Title 35 of the United States Code Section 101\textsuperscript{17}. The key statement in the text reads as follows:

\textit{We hold that the claims at issue are drawn to the abstract idea of intermediate settlement, and that merely requiring generic computer implementation fails to transform that abstract idea into a patent-eligible invention. We therefore affirm the judgment of the United States Court of Appeals for the Federal Circuit.}

This ruling has essentially institutionalized what is now known as the “Mayo-Alice rule” for evaluating computer-implemented patents: first, determine whether the claims are directed to a patent-ineligible concept; if so, examine whether the elements of the claims collectively transform the idea into a patent-eligible concept Tran (2016).

While the text is relatively generic and may be thought of as affecting all software patents, Alice is widely considered to have substantially diminished the effectiveness of patenting in a subset of the industry: business-related software. In fact, this is consistent with process leading to the ruling. Sunstein (2014) documents that:

\textit{Part of the motivation behind Alice was to highlight the increasingly powerful role of “patent trolls”. Alice Corp itself was defined as a patent troll by some observers. A patent troll – or, more formally, a nonpracticing entity (NPE) – is an organization that seeks to profit from enforcing patents rather than from commercial activities based on the patents (Pohlmann and Opitz, 2013).}

\textit{The patents involved are US5970479, US6912510, US7149720, and US7725375. The oldest and possibly most critical patent is probably US5970479 “Methods and apparatus relating to the formulation and trading of risk management contracts”.}

\textit{Tran (2015) reports that Federal Circuit Chief Judge Rader asserted “Nothing said today beyond our judgement has the weight of precedent.”}

\textit{A detailed account of the timeline of Alice can be found at www.patentprogress.org/cases/clsv-alice-corp-d-d-c/.}

\textit{The text of the Supreme Court decision is available at https://www.supremecourt.gov/opinions/slipopinion/13. The opinion was led by Justice Clarence Thomas. The decision was made unanimously.}
Justice Thomas did not write the only opinion in Alice; Justice Sotomayor wrote a concurring opinion, in which she was joined by Justices Ginsberg and Breyer, stating that she adhered to the view that "any claim that merely describes a method of doing business does not qualify as a "process" under Section 101."

Accordingly, much of the legal literature confirms that the primary impact of Alice was on business-related software. For example, Stern (2014) writes the following:

The Flook-Mayo-Alice rule would not seem to threaten the patent eligibility of those software patents that are not mere routine computerizations of preexisting business or financial expedients. By implication at least, the Alice opinion leaves room for patents on software that improves technological and industrial processes. Software on the internal functioning of computers would also appear patentable. Although the opinion did not say so, probably patents on software for encryption or data compression, if not deemed simply mathematics, would be left patent eligible.

Similarly, Tran (2015) writes the following:

As an “unwritten policy,” patent examiners currently view all claims reciting financial or business methods as presumably directed to abstract ideas: categorizing any subject matter relating to banking, investments, or payment transactions as either a matter of “fundamental economic practices” or “methods of organizing human activities.” A knowledgeable supervisory examiner opined that “it would be ‘very hard’ for applications related to financial subject matter to escape the designation of abstract ideas. The best hope for overcoming Section 101 rejections appears to be demonstrating that the invention is ‘significantly more’ than the abstract idea itself.”

To further validate the qualitative evidence, I analyzed a number of court decisions following Alice to measure its impact directly. This analysis is consistent with the conclusion that business-related software has been primarily affected. Please see Appendix 2 for details.

The decision was officially announced in June 2014, the final month of the October 2013 term. However, the outcome was largely anticipated in the previous months. Because oral arguments were planned for March 31st 2014, the parties, as well as a large number of other organizations (the “amici”, in legal terms), had filed their briefs in the early part of 2014. The vast majority of the briefs argued in favor of CLS Bank\textsuperscript{18}, suggesting the invalidation of the patents. Therefore, it is plausible to assume that in March 2014 the public already had a relatively clear expectation regarding the decision the Supreme Court would make. In fact, the popular press also documented

\textsuperscript{18}For more information, see patentlyo.com/patent/2014/03/software-patent-eligibility.html.
this process. For example, on March 31st 2014, the day of the oral arguments, Ars Technica writes the following:

Practically no observers expect Alice Corp. to keep its four patents, but the question of how many other patents get lassoed into today’s decision will resonate for years to come.

4 Data

I describe the data sources and the key variables used in the empirical analysis. The main analysis is based on a hand-collected dataset of US-based ventures. To guide the analysis and validate the findings, I rely on qualitative evidence obtained through a series of interviews and a survey.

4.1 Data Sources

The main analysis uses a hand-collected dataset of US-based early-stage software ventures. The sources required to build this dataset are generally only available in recent times, so I choose to focus on ventures founded in time window 2012-2016. I choose a random 10% from the US software venture population and collect data on this subsample\(^\text{19}\). The random selection ensures that the findings are generalizable to the population.

My qualitative work is based on two sources: a series of interviews and a survey. Throughout the paper, when appropriate I also use the popular press to support and contextualize the findings.

First, I conducted a series of semistructured interviews of software entrepreneurs in two waves. The first wave involved 13 pre-study interviews, seeking to explore the tensions that arise in the context of experimentation. The second wave involved 12 post-study interviews, seeking to validate the findings that emerged in the empirical analysis. I personally executed all the interviews, either on the phone or in person. Each interview lasted approximately 30 minutes. All interviewees were cofounders or top managers of the early-stage ventures.

Second, after doing the bulk of the empirical analysis, I circulated a survey to all the ventures in my sample\(^\text{20}\). The survey asked for information about ventures’ strategy and performance, seeking to infer the rationale for and the outcome of their choices of testing and launch. The survey had a 6% response rate (67 responses).

Finally, to better understand the impact of Alice, I examined a series of court rulings that cited Alice in their decision-making process. I analyzed a total of 153 Alice-citing decisions in

\(^{19}\)Handcollecting data on the entire population was not financially and operationally feasible. I discuss my current effort to increase the sample size using machine learning in the Discussion section.

\(^{20}\)The survey was approved by the University of Pennsylvania’s Institutional Review Board in June 2018.
period 2014-2017 (23% of the estimated total, according to LexisNexis). Please see Appendix 2 for details.

4.2 Data Collection Process

To build the core data, I start from Crunchbase data (Wu, 2015) and identify the population of software ventures founded in period 2012-2016\(^{21}\). Crunchbase does not provide clear information of companies’ activities. So I first select ventures associated with at least one of the following 7 macro categories: apps, artificial intelligence, data and analytics, gaming, Internet services, mobile, and software. The resulting list is the population of companies that are likely to be software-related. I then screen out companies that are not pure-play software and for-profit. This leads to a target population of 20807 ventures\(^{22}\). I extract a random sample of 2100 companies (approximately 10% of the population). After the screening process, I obtain a core dataset of 1203 pure-play ventures. Table A2 in Appendix 5 reports the mean differences in a set of observable variables between the sample and the rest of the population, confirming that the sample is largely similar to the population.

The central effort involves reconstructing the timing of the key moments of the ventures’ product development process: founding, testing, and launch. As discussed above, the software industry has established a relatively common terminology to define the stages of a product. I exploit this institutional aspect for measurement purposes\(^{23}\), by hand-collecting and coding announcements and news related to the ventures in the sample. I follow an approach similar to Marx, Gans, and Hsu (2014), who assemble a hand-collected dataset of ventures in the speech recognition industry to examine their strategy choices.

The final dataset combines data about product development timing, market, business model, founding team, patents, funding, and performance. The data collection process consists of the following steps:

1. I collect data about the timing of the product development process from company sources (website, Twitter, Facebook) and media (Google search, Crunchbase news archive, PRWeb).

\(^{21}\)One may argue that relying on Crunchbase to identify the population of ventures may create itself a selection bias. I cannot exclude this possibility. However, Crunchbase is largely considered the most comprehensive dataset of ventures in the US, so there is arguably no better alternative. Additionally, my understanding is that Crunchbase uses multiple sources to find the companies, and such triangulation appears to limit the selection bias. When I investigated this issue with Crunchbase staff, a representative described their data sourcing process as follows: “Partially crowdfunded, partially through our Venture Program (3500+ investment firms feeding us their portfolio data), partially news publications and government/innovation agency partnerships, and then partially our data team of 45+ data analysts.”

\(^{22}\)This number is based on a bulk download of Crunchbase data in December 2017. The population based on the May 2018 data contains 22,593 ventures.

\(^{23}\)Davis, Muzrya, and Yin (2014) use product versioning to measure experimentation. I discuss the differences between their approach and my approach in Appendix 3.
I code evidence of disclosure of a preliminary product (‘alpha’, ‘beta’, ‘pilot’, ‘prototype’, ‘trial’, etc) as experiment and evidence of circulation of the final product (‘formal launch’, ‘official launch’, etc) as entry. When extracting and coding this information, I focus on the initial product of each venture. Besides testing and launch, I verify and update the time of founding, a piece of information that is generally inaccurate on Crunchbase. Please see Appendix 3 for details about this process.

2. I assign companies to their most relevant markets. Because there is no established categorization of software markets, I borrow the categorization of enterprise software used by Engineering360 and complement it with categories relevant to consumer software. This process leads to a comprehensive list of 27 markets. Please see Appendix 4 for details regarding this process.

3. I collect data about the business model. This includes function, revenue model, user type, delivery, and copyright status. This information is hand-collected through the analysis of company sources and is primarily used as control variables.

4. I collect data about founding teams. I obtain founders’ names from Crunchbase and collect the relevant information manually from LinkedIn. I use this information primarily as a control variable.

5. I collect data about patents, including issued patents via PatentsView and patent applications via PatEx (Graham, Stuart J.H. and Marco, Alan C. and Miller, 2018). The matching of the patent data to ventures was executed via a name-matching algorithm. Issued patents were matched based on the company name and assignee name. Patent applications were matched based on the cofounder name and inventor name.

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24 The qualitative evidence suggests that the vast majority of the ventures in the sample are single-product ventures. In my interviews, I have only found one case of a venture that developed multiple products. Such instances are likely to be rare, given that all companies in my sample are at most 5 years old. Therefore, the single-product assumption seems appropriate, and implies that the analysis should be executed at the venture-period level.

25 The enterprise software categorization is available at www.globalspec.com.

26 My qualitative work suggests that patenting is the main form of IP for software ventures, so I focus on patent data. Another important form is copyright. Copyright protects the expression of the idea, not the idea itself, but is believed to have value in the software setting. In an interview, a university-based technology transfer office suggested that they always recommend both filing a provisional patent application and registering a copyright. I inquired about obtaining access to data about registered copyrights with the US Copyright Office and realized that the data are accessible only at a prohibitive price.

27 The data are available at http://www.patentsview.org/download/.


29 PatEx, based on Public PAIR, also provides information on the organization applying for the patent. However, matching the cofounder name and inventor name is likely to be more comprehensive when analyzing early-stage ventures, which may or may not yet be incorporated.
6. I collect data about funding. This information is available on Crunchbase with generally sufficient accuracy. I use forms of early-stage funding (crowdfunding, convertible notes, angel investments) as control variables.

7. I collect data about performance, primarily in terms of survival and size. The survival data include information about time of failure and time of acquisition. Both are provided by Crunchbase but are occasionally inaccurate, so I revise them manually.

While designing and supervising the entire process, I hired and trained a team of nontechnical research assistants to execute steps 1, 2, 3, and 4 and a technical research assistant to execute step 5. This procedure created a dataset of 1203 ventures. In the venture-month panel used in the analysis, this leads to 53924 observations.

### 4.3 Product Development Timeline

The key outcome variables are experimentation and entry. I define experimentation as the disclosure of an incomplete product prior to launch and entry as the official launch of the product.

I operationalize experimentation as a binary variable that takes the following values: 1 if the venture is experimenting in that period, 0 if the venture is not experimenting and has not yet launched. In other words, experimentation is an “absorbing dummy variable”, a dummy that starts at zero and turns on when the venture executes its first experiment. Note that this variable takes a missing value after the company has launched because – by definition – experimentation cannot take place after entry. Entry is similarly operationalized: 1 if the venture has launched in or prior to that period, 0 if the venture has not yet launched.

Figure 2 reports the frequency of experimentation and launch in the data. I calculate the timing of experiment and entry at the month level. Figure 3 illustrates the distribution of the timing between founding and first experiment and the timing between founding and entry.

Testing is primarily devoted to learning, and that makes it a form of experimentation. In response to survey question “Why did you choose to test your product?”, the cofounder of a Chicago-based photo-sharing app venture stated the following:

*We did this for many reasons, including usability testing, feature refinement, to gather insights into onboarding/pricing, and more.*

Similarly, an employee of a sales/marketing software company based in San Diego confirmed they were searching for feedback on the technology and demand:

*We wanted to be sure the product worked the way it should and that the market was interested.*
Testing and launch are relatively distinct activities in this setting, consistently with the conceptual distinction between experimentation and entry discussed in Section 2. However, the difference between test and launch is generally not in whether the beta product is free and the final product is priced: many software companies offer the product for free, even when the product long after launch. The fundamental distinction is that testing comes with little or no marketing investment, while launch is primarily a marketing investment. To survey question “What did you do to launch your product”, a Boston-based financial analytics software founder responded as follows:

*We formally launched the product in September 2017 with press release and several major news articles announcing the release.*

Similarly, the marketing manager of an application development platform venture based in San Francisco suggested the following:

*PR tour, analyst briefings, Tech Crunch, blog and email announcements. Tech Crunch is generally a budget drain, but good for getting a headline.*

### 4.4 Alice

The key independent variable, the degree of FIP, is measured based on the 2014 US Supreme Court decision *Alice Corp - CLS Bank International*. As discussed in the previous section, this ruling has been widely interpreted as substantially decreasing the effectiveness of patenting for business-related software.

I operationalize *Alice* as a binary variable that equals 1 starting in March 2014 for ventures building business software, and 0 otherwise. Following the approach of Licht et al. (2018), I choose March 2014 as treatment date, because oral arguments are heard on March 31st 2014 and that is when the Supreme Court’s stance is likely to emerge. In the standard difference-in-differences language, the variable *Alice* is the interaction between *Treatment* and *Post* and takes a value of 1 if the venture-period observation is in the treatment group and takes place on or after March 2014.

It is useful to validate the assumption that Alice has indeed led to a decrease in FIP. If this assumption is valid, we would expect the affected patent classes to become less popular. The most appropriate patent class for this type of software is class 705 “Data Processing: Financial, Business Practice, Management, or Cost/Price Determination” (Hall and MacGarvie, 2010). Figure

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30 Court decisions are increasingly used for causal identification purposes. Examples of studies taking this approach include Cohen and Wang (2013), Crane and Koch (2016), Amihud and Stoyanov (2017), Mezzanotti (2017), and Licht, Poliquin, Siegel, and Li (2018).

31 The USPTO defines class 705 as “the generic class for apparatus and corresponding methods for performing data processing operations, in which there is a significant change in the data or for performing calculation operations wherein the apparatus or method is uniquely designed for or utilized in the practice, administration, or management of an enterprise, or in the processing of financial data.” Please see www.uspto.gov/.../defs705.htm.
4 reports the time series of the number of patent applications for class 705, the average number of applications for other software-related classes, and the average number of application for all other classes. The figure documents a clear drop in class 705 immediately after Alice and a much smaller change, on average, in the other classes. While the raw data show a relatively clear dynamic, the synthetic control method approach (Abadie, 2010) allows a more rigorous comparison. Figure 5 illustrates the estimated time series of the difference between patent applications in class 705 and the “synthetic” control group of either software-related classes or all classes. This analysis confirms the drop in patent applications for class 705 relative to other classes following the occurrence of Alice.

4.5 Market

To build the Alice variable and a number of other independent variables, I assign each venture to a market. I build a novel categorization of 27 software markets. Based on my reading of the legal literature and the popular press, I assume three markets to be in the treatment group (Business Applications, Accounting and Financial, Enterprise and Plant Management) and the remaining 24 in the control group\(^{32}\). Appendix 4 offers additional details of the categorization.

Figure 6 lists the markets and provides market-level information. Business Applications and Social Networking are the most represented markets. Markets based on more complex technologies, such as Enterprise and Plant Management or Engineering and Scientific, spend more time developing their product before testing in the market and tend to enter later.

4.6 Summary Statistics

I employ a variety of other variables throughout the analysis. I use static (non-time-varying) and dynamic (time-varying) controls. Static controls include business-model features, which I assume to be stable over time. Dynamic controls include financing features, which change over time.

Moreover, I measure two moderating variables to run subsample analyses: the degree of learning incentive and the degree of competition. The basis for constructing these variables is the category vector that Crunchbase provides for each venture\(^{33}\). In calculating these variables, I use the entire software population: all firms that belong to software-related macro categories founded in the period of 2000-2017.

\(^{32}\)I am currently working to verify the robustness of this assumption. One way to do so is to calculate the ratio of business software patents over all patents within each market prior to the shock, and assume markets with high ratio to be in the treatment group. This step requires a relative accurate matching of companies to patents, which is not straightforward for early-stage ventures. I discuss this further in the Discussion section.

\(^{33}\)There is a total of 713 unique categories in the data. The categories vary from very general (“software”, “education”) to very specific (“ad targeting”, “supply chain management”).

16
I measure learning incentive in two ways. First, I measure novelty by calculating the inverse of the age of the venture’s product category vector (Category Vector Youth). A more novel product is arguably less understood and thus requires more learning. Second, I measure the artificial intelligence (henceforth, AI) intensity as the presence of AI-related categories within the venture category vector (AI Intensity). Because AI is arguably a novel “general purpose technology”, its application is likely to require some learning. Thus these two measures are proxies for the incentive to learn that a venture has: when either of these two variables are high, the venture may need to learn more relative to peers with low values.

I measure competition as the presence of firms building similar products. I use the Jaccard Index (Bikard, 2013) as measure of similarity. I then calculate two measures of competition: number of firms within the same market with a Jaccard Index larger than 0.5 (Competition) and number of large firms within the same market with a Jaccard Index larger than 0.5 (Large-Firm Competition).

While they may appear similar, learning incentive and competition seek to measure different aspects of a venture. Consistently, they are not highly correlated among each other. Category Vector Youth is only weakly (negatively) correlated with Competition (-0.35) and Large-Firm Competition (-0.36). AI Intensity is essentially uncorrelated with Competition (-0.05) and Large-Firm Competition (-0.06).

Table 1A reports the summary statistics at the cross-sectional level. In particular, this table includes the static controls. Table 1B reports summary statistics at the panel level. This table also includes the dynamic controls.

## 5 Analysis

I test the proposed argument by examining the impact of a decrease in FIP on ventures’ strategic choices. I analyze how Alice affects the choice of experimentation – testing the learning-appropriability tension hypothesis – the timing of entry, and how learning incentive and competition affect these relationships. I finally run a series of additional analyses, exploring the correlation between experimentation and performance.

### 5.1 Empirical Strategy

I seek to investigate the impact of the decrease in FIP on ventures’ strategy. The ideal experiment would involve removing the access to FIP for some randomly chosen ventures, while making no change for the remaining ones. To approximate this scenario, I employ Alice as a “quasi natural experiment”. As discussed above, Alice is assumed to affect ventures in the three treatment mar-
kets (Business Applications, Accounting & Financial, Enterprise & Plant Management) starting in March 2014. I use two main approaches throughout the paper: linear regression and survival analysis.

I use linear regression to estimate the impact of Alice on the propensity of experimentation and entry. This specification is essentially a standard difference-in-differences model. The independent variable $Alice_{m,t}$ is the interaction between $Treatment_m$ and $Post_t$. Because I use venture and period fixed effects, the non-interacted variables $Treatment_m$ and $Post_t$ are perfectly collinear and must be omitted. I run linear regressions of the following form:

$$S1 \quad Strategy_{i,t} = \beta Alice_{m,t} + \Gamma_2 X_{2i,t} + A_j + V_t + P_t + \epsilon_{i,t}$$

where $i$ is the venture, $m$ is the market, $t$ is the period, $Strategy_{i,t}$ is a dummy denoting whether the venture makes a given strategic choice, $Alice_{m,t}$ is the Alice dummy (the interaction between variables $Treatment_i$ and $Post_t$), $A_j$ are the fixed effects for the number of months since founding, $V_t$ are venture fixed effects, $P_t$ are period fixed effects, and $X_{2i,t}$ are dynamic (time-varying) controls. The venture fixed effects control for the non-time-varying features of each venture, including the structural characteristics of the market, as ventures do not change markets over time.

When analyzing the timing of experimentation or launch, I turn to survival analysis (Kiefer, 1988; Leary and Roberts, 2005; Cleves, Gould, Gutierrez, and Marchenko, 2010; Allison, 2014; Kellogg, 2014). Given the binary treatment variable, this approach is akin to a survival difference-in-differences model Azevedo, Deng, Olea, and Weyl (2017). I run proportional hazard models of the following form:

$$S2 \quad h(Strategy_{i,t}|X_{i,t}) = h_0(t) e^{\beta Alice_{m,t} + \Gamma_1 X_{1i,t} + \Gamma_2 X_{2i,t} + M_m + F_t}$$

where $i$ is the venture, $m$ is the market, $t$ is the period, $h(T_i|X_{i,t})$ is the hazard rate of the strategic choice $Strategy_{i,t}$ of interest, $Alice_{m,t}$ is the Alice dummy, $M_m$ are market fixed effects, and $F_t$ are founding year fixed effects. $X_{1i,t}$ are static (time-invariant) controls, and $X_{2i,t}$ are dynamic (time-varying) controls. I estimate this equation both via parametric models (Weibull) and semi-parametric models (Cox). While it is generally not feasible to use venture fixed effects in hazard

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34 Given the nature of the shock I use, the work by Fricke (2017) suggest that the effect I find is a lower bound of the treatment effect. More recently, De Chaisemartin and D’Haultfoeuille (2018) argue this is a fuzzy difference-in-differences design and propose an alternative estimation approach.

35 Period fixed effects are equivalent to year-month fixed effects.

36 I run linear regressions primarily using the STATA command reghdfe. I also attempted to use nonlinear models (logistic and probit) to estimate the equation, but these models generally do not achieve convergence in this setting, possibly due to the large sample size. Therefore, I rely on the linear probability model. This appears to be the common approach taken in the literature when using panel data. This approach also has the advantage that it does not remove observations due to the “complete separation” problem typical in logistic and probit.

37 Hazard models are executed primarily using STATA commands streg and stcox. With the Cox regressions, I use
models (Allison and Christakis, 2006), I control for the non-time-varying characteristics of the market via market fixed effects.

Because the treatment affects markets and not ventures individually, it is plausible that there is correlation across ventures within the same market. Therefore, I use standard errors clustered at the market level throughout the analysis (Bertrand, Duflo, and Mullainathan, 2004).

### 5.2 Experimentation

I start the analysis by testing the main hypothesis and estimate the impact of FIP on the ventures’ choice to experiment. I use a linear specification to investigate the impact of Alice on experimentation. Table 2 reports the regression analysis based on specification S1. Columns 1 and 2 use the entire sample, including ventures founded prior to the shock and ventures founded after the shock. While the effect is not significant, the sign is negative, as expected. Columns 3-6 focus on ventures founded prior to the shock. The effect is negative and generally more significant, especially when using the oldest ventures in the data, those founded in 2012.

This first test validates the hypothesis. However, the effect is not strongly significant. In this setting, experimentation is a dummy that starts at 0, possibly switches to 1, and, if so, then remains 1 until there is a right-censoring event. Therefore, this is an outcome variable that has very limited variation. This makes the inference challenging, and this may arguably explain part of the low statistical significance of the results.

Furthermore, this setting has an additional nonstandard element: experimentation is defined only prior to entry. Once the venture has launched its product, by definition it can no longer experiment. Therefore, experimentation is a missing value in venture-period observations after entry. This creates an unusual sort of censoring. To partially alleviate this concern, I attempt to diminish the weight of the right-censoring by restricting the sample to the “initial periods” of the

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38 I do not analyze the frequency of experimentation because the data on the second test and the third test are noisy. Ventures typically announce their first test but do so less often for their subsequent tests. In addition, while the first test and launch are clearly distinguishable, it is difficult to distinguish different generations of tests. Naturally, questions around experimentation frequency and pacing of experiments remain interesting and should be examined with the appropriate data.

39 This setting has four sorts of censoring. Experimentation is left-censored in two ways: ventures that have not started experimenting when the data ends in December 2017 and ventures that fail or are acquired prior to starting experimentation. Experimentation is right-censored in two ways: ventures that launch and can no longer experiment and ventures that fail or are acquired prior to launch.

40 In principle, one potential way to correct this issue is to make the experimentation variable 0 even post-launch and add a dummy for all observations where this change has been made. This would be similar to the way nonresponses are sometimes dealt with in survey data. However, in this setting, the additional dummy would amount to adding a variable Entry in the regression. Subsection 5.2 shows that Alice affects Entry. Angrist and Pischke (2009) (page 64) recommends not including such variables (i.e. bad controls), so I do not use this approach.
ventures. An interpretation of this approach is that it focuses on the early stage of each venture, a period in which imitation is arguably easier, and thus the impact of Alice may be more relevant. To do so, I restrict the sample to ventures’ initial 16 or 10 months of life. Table 3 reports the results, showing the same negative pattern but stronger statistical significance.

Tables 2 and 3 reveal a negative impact of Alice on the propensity to experiment, supporting the hypothesis. An important assumption in making a causal interpretation in this setting is the parallel trend condition. To shed light on this condition, I run a period-specific treatment effect analysis (Acharya, Baghai, and Subramanian, 2014), where I decompose the treatment into a set of period-specific treatment dummies. Figure 7 plots coefficients around the event (12 periods before and 18 periods after). While confidence intervals are relatively large, the graph suggests that the slope of the treatment effect becomes negative around the occurrence of Alice, while providing no strong evidence of pre-event differential behavior.

I offer additional analysis in support of this result in Appendix 5. I discuss potential reasons that might lead to the violation of the parallel trends assumption and discuss why they do not appear to be at play here. In addition, I verify that the general result is robust to restricting the sample to a window around the event (Table A3) using alternative specifications of age (Table A4), and excluding markets with very low or very high patent propensity (Table A5). The general pattern continues to be that Alice leads to a lower propensity of experimentation, supporting the hypothesis.

To better understand whether Alice impacts the timing of experimentation, I turn to survival analysis. Using the terminology of this approach, the beginning of experimentation is the “failure event”. This methodology allows me to estimate the impact of Alice on the arrival of the failure event. Table 4 reports the result of the hazard regressions based on specification S2. Alice has a clear negative effect on the hazard of experimentation according to the Weibull estimates. The effect is nonsignificant according to the Cox model. I run two other common hazard models (Exponential, Gompertz) in Table A6 and find a negative effect in one case and a nonsignificant effect in the other case.

Furthermore, I provide a nonparametric estimation of the cumulative hazard function using the Nelson-Aalen approach in Figure 8. While the effect is not substantial, we see that the treatment group’s hazard curve is generally below that of the control group, suggesting that affected ventures are less likely to experiment than nonaffected ventures.

Overall, while the findings of the hazard analysis are weaker, the evidence found so far is consistent with the argument, such that ventures choose not to experiment when FIP weakens.

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41 I use 16 and 10 as cutoffs because the median time to launch is 17 months and median time to first test is 11 months.

42 Naturally, one may argue that ventures may still be doing “internal experimentation”, running testing inside the organization in high secrecy. I am unable to observe this. However, this activity does not qualify as experimentation,
The theorized mechanism is that the imitation risk associated with experimentation increases when FIP is weak, diminishing the expected value of experimenting.

Qualitative evidence confirms that ventures perceive the risk of imitation in the testing process. Regarding the survey question “Why did you choose to test your product?”, the founder of a Baltimore-based marketing software venture suggested the learning-appropriability trade-off is indeed perceived:

*Feedback is extremely important, but it is extremely difficult to test with the target market without disclosing details of the project. It is important to define a use case and target user and, ideally, the product is being built for one customer and can then be sold or offered to many customers. Most companies we know test products with a small targeted audience prior to release.*

5.3 Entry

The analysis shows that the occurrence of Alice leads early-stage ventures to experiment less, supporting the hypothesis. As ventures change their experimentation propensity, they may or may not change the rest of their strategy. In principle, they may simply avoid experimentation and simply “sit in the lab” until the product is ready for commercialization. Alternatively, Alice may change the timing of the entry decision. It may postpone the entry decision, if the lack of learning via experimentation makes the product development process slower. It may accelerate the entry decision, if the venture essentially compensates the lack of experimentation with faster time to market.

Market entry is primarily a matter of timing, rather than a matter of a choice, so I start with survival analysis. I turn to specification S2 to examine the effect of Alice on the timing of launch. Table 5 reports the results. Once again, I use two different approaches (Cox, Weibull). Alice has a strong positive effect on the hazard of entry when using Cox. The effect is also positive but generally less significant when using Weibull. I run two other common hazard models (Exponential, Gompertz) in Table A7, and find a consistently large positive effect.

I report the nonparametric estimation of the cumulative hazard function using the Nelson-Aalen approach in Figure 9. The evidence is not particularly clear: the treatment group’s curve is not consistently above that of the control group. However, there is at least some evidence of that for ventures founded in 2012 and for ventures founded after Alice.

I then run the analysis using linear regression using specification S1. Table 6 contains the results, confirming that Alice has a positive effect on the propensity to enter. The effect is particularly significant for ventures founded in 2012, those which experience the ruling.

given my definition, as it does not imply disclosure. In other words, the theorized trade-off does not apply.
Overall, this evidence suggests that Alice, besides diminishing the attractiveness of experimentation, induces ventures to enter the market earlier. If experimentation becomes risky, ventures may choose to skip pre-entry learning and enter the market earlier to start the learning process directly in the market.

Market entry is generally associated with investments in marketing. This effort may generate two classes of complementary assets: brand recognition and user base. Such assets create barriers to entry, a form of IIP. If FIP is no longer a feasible option, ventures may therefore choose to move faster and launch the product, protecting the product through IIP.

5.4 Learning

By diminishing the effectiveness of FIP, Alice leads ventures to experiment less and enter earlier. I argue that this pattern is driven by the learning-appropriability tension. To provide further evidence of this channel, I examine a critical moderating factor, the learning incentive. If the main benefit of experimentation is learning, ventures with stronger learning incentive should be less sensitive to Alice in terms of their experimentation strategy. Additionally, if early entry is motivated by need for learning, ventures with stronger learning incentive should respond more to Alice in terms of time to entry.

I analyze the impact of Alice for the different subsets of my sample: ventures with high incentive to learn and ventures with low incentive to learn. As discussed in section 4, I measure learning incentive with novelty (inverse age of category vector) and AI intensity (presence of AI-related categories in category vector).

I first examine how the Alice effect on experimentation changes depending on the learning incentive. I use S1 with the restriction of venture-period observations prior to 17 months of age (see Table 3 for details). Table 7 suggests that the negative effect of Alice is particularly strong for ventures with low need for learning.

This is consistent with the idea that experimentation is valuable only when learning is particularly important. Alternatively, in situations in which there is little uncertainty, experimentation is not strictly necessary. A quote from the co-founder of a San Francisco marketing software venture perfectly exemplifies this scenario:

43 An alternative explanation that may lead ventures to go to market faster may be the incentive to signal performance to stakeholders. The literature (Hsu and Ziedonis, 2013; Conti, Thursby, and Rotheraermel, 2013b; Conti, Thursby, and Thursby, 2013a) suggests that early-stage ventures often use patents as signals of quality to obtain investments. When patents are no longer available, ventures may choose to enter the market faster and signal quality through their performance.

44 An alternative way to run this analysis is by adding interaction terms. However, interactions are not easily interpreted in non-linear models. Therefore, for consistency across the two tests, I use subsample analysis.

45 This result is consistent with the theoretical work by Choi, Lévesque, and Shepherd (2008).
We did not run any tests before getting clients to use our product. This is because our product solved a problem that we had previously experienced ourselves, and we had a strong sense of what solutions would be well received. We also use our product internally, and it successfully solves the problems it was designed for. If our product were more hypothesis based, and we designed it purely based on external research and identifying needs, it would be more important to test it.

I use hazard specification S2 to examine the role of learning incentive on entry. Table 8 shows that the positive effect of Alice on the entry hazard increases in magnitude and significance as we move from a low learning incentive (columns 1 and 3) to a high learning incentive (columns 2 and 4). In other words, ventures with higher need for learning go to market earlier. This pattern reinforces the idea that ventures seek to start their learning process earlier by launching the product, when learning through experimentation becomes riskier.

Overall, this analysis confirms the importance of the role of the learning process in driving experimentation and entry, consistently with the proposed argument.

5.5 Competition

I perform a similar exercise with another critical moderating factor, the degree of competition. The cost of experimentation – the threat of imitation – is higher when the venture operates in a market space with high competition. If this argument is valid, we would expect that the degree of competition plays a role.

I first verify if the effect of Alice on experimentation is stronger for ventures facing stronger competition in Table 9, using S1 with the restriction of venture-period observations prior to 17 months of age (see Table 3 for details). As discussed in section 4, I measure competition in terms of number of firms and number of large firms building product sufficiently similar to the venture (Jaccard Index > 0.5). The negative effect is strongest in the subsample of ventures facing high competition by large firms, reported in column 4.

This pattern is consistent with the common wisdom. For example, The Economist discusses the perception among VC investors that early-stage ventures operating in market spaces too close to large firms are at high risk of imitation:

Venture capitalists, such as Albert Wenger of Union Square Ventures, who was an

46 Competition appears to interact in interesting ways with the degree of uncertainty of the product development process. In experimental work, Boudreau, Lacetera, and Lakhani (2011) find that higher uncertainty increases the positive effect of competition on quality of idea in the context of innovation tournaments.

47 The original article is available at www.economist.com/business/2018/06/02/americantechgiantsaremakinglifetoughforstartups. Additional discussion is available at promarket.org/googlefacebookskillzonewetakenfocusoffrewardinggeniusinnovationrewardingcapitalscale/.
early investor in Twitter, now talk of a “kill-zone” around the giants. Once a young firm enters, it can be extremely difficult to survive. Tech giants try to squash startups by copying them, or they purchase them early to eliminate a threat.

Table 10 examines the effect of Alice on the entry hazard of ventures facing low and high competition. While the pattern is less clear here, there is some evidence that the effect is more pronounced for ventures facing low competition. An interpretation is that ventures may choose to speed up market entry when the market is not competitive, and thus they can accumulate first-mover advantage. Instead, when the market is competitive, rushing to market is not helpful, and waiting and building a more developed product becomes a more attractive strategy.
References


Bertrand, M., Duflo, E., Mullainathan, S. 2004. How much should we trust differences-in-differences estimates?


Bikard, M. 2013. Simultaneous Discoveries as a Research Tool: Method and Promise.

Blank, S. 2013. Why the lean start-up changes everything.


This figure illustrates the learning-appropriability tension that arises during the experimentation phase. The vertical lines denote the key moments in the life cycle of a nascent venture: ideation (i.e. founding), start of experimentation (i.e. testing), entry (i.e. launch), and change (i.e. update). While the venture naturally receives market feedback and changes after entry, it may choose to obtain market feedback prior to entry through experimentation. Absent some form of FIP, experimentation creates a tension between learning and appropriability: the venture learns from market feedback while running the risk of imitation. Return to text.
This figure reports the frequency of testing (experimentation) and launch (entry). Testing is the disclosure of any form of incomplete product (alpha, beta, pilot, prototype, etc) to the market prior to entry. Launch is the formal introduction of the final product to the market. Measurement of second and third tests is generally difficult, therefore the analysis focuses on the first test. Appendix 3 provides details on the data collection process. Return to text.
This figure reports the timing of first test (start of experimentation) and of launch (entry). The vertical lines denote the median values of time to experiment (11 months) and time to launch (17 months) in the cross-sectional dataset. Appendix 3 provides details on the data collection process. Return to text.
This figure reports the time series of 1) the number of patent applications in class 705, 2) the average number of patent applications in software patent classes other than 705 (classes which received at least one application from a software venture in period 2000-2013), and 3) the average number of patent applications in all patent classes other than 705. The vertical line denotes the occurrence of Alice (assumed in March 2014). The trend in patent applications in class 705 drops after the event, while there is no clear change in the trend for the other time series. Return to text.
This figure reports the time series of 1) the difference between number of patent applications in class 705 and 2) synthetic control group. In the top graph, the synthetic control group is based on patent applications in software patent classes other than 705 (classes which received at least one application from a software venture in period 2000-2013). In the bottom graph, the synthetic control group is based on patent applications in all patent classes other than 705. The light lines are the placebo tests, with each placebo test assuming another given class is treated and building the synthetic control group using the remaining classes (including 705). The vertical line denotes the occurrence of Alice (assumed in March 2014). This figure is obtained with the STATA command `synthrunner` (Galiani and Quistorff, 2017) and is analogous to Figure 4 in Abadie (2010). Return to text.
This figure reports data about the 27 markets composing the software industry. The top graph reports the average number of ventures per market. The middle graph reports the average time to first test. The bottom graph reports the average time to launch (unconditionally on testing). In each graph, the top section includes treated markets (business software) and the bottom section includes control markets (non business software). Return to text.
This figure reports the coefficients from OLS regression $\text{Exp}_{it} = \sum_{\tau = -26, \ldots, +45} \beta_{\tau} \lambda_{\tau} + A_j + V_i + P_t + \epsilon_{it}$, where each $\lambda_{\tau}$ is a dummy that equals 1 if the observation is $\tau$ periods away from the event and the venture is part of the treatment group, and 0 otherwise. This method is similar to Acharya et al. (2014) figure 4. I omit coefficient $\tau = -13$ for graphical convenience, but the pattern is robust to different omitted coefficients. For space constraints, each graph reports 12 coefficients prior to the event and 18 coefficients after the event. The vertical line denotes the occurrence of Alice. The top graph uses all venture-period observations. The middle graph uses venture-period observations with age lower than 17 months. The bottom graph uses ventures founded in 2012. Return to text.
This figure reports Nelson-Aalen nonparametric estimates of the cumulative hazard function of experimentation for treatment group and control group. The top graph uses ventures founded in 2012. The middle graph uses ventures founded in 2013. The bottom graph uses ventures founded after the occurrence of Alice (assumed in March 2014). The vertical lines identify the time window in which Alice occurs in the analysis time of the ventures used for that graph. Return to text.
Figure 9. Nonparametric Estimate of Entry Cumulative Hazard Function

This figure reports Nelson-Aalen nonparametric estimates of the cumulative hazard function of entry for treatment group and control group. The top graph uses ventures founded in 2012. The middle graph uses ventures founded in 2013. The bottom graph uses ventures founded after the occurrence of Alice (assumed in March 2014). The vertical lines identify the time window in which Alice occurs in the analysis time of the ventures used for that graph. Return to text.
### Table 1A. Cross-Sectional Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alice</td>
<td>Dummy that equals 1 for ventures founded in treated markets starting March 2014.</td>
<td>0.08</td>
<td>0.28</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>Experiment</td>
<td>Dummy that equals 1 if the venture tests its product prior to launch.</td>
<td>0.58</td>
<td>0.49</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>Entry</td>
<td>Dummy that equals 1 if the venture launches its product.</td>
<td>0.83</td>
<td>0.38</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>Failure</td>
<td>Dummy that equals 1 if the venture fails.</td>
<td>0.21</td>
<td>0.41</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>Acquisition</td>
<td>Dummy that equals 1 if the venture gets acquired.</td>
<td>0.08</td>
<td>0.26</td>
<td>0</td>
<td>1</td>
<td>1203</td>
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<tr>
<td>Platform</td>
<td>Dummy that equals 1 if the venture uses a platform strategy.</td>
<td>0.38</td>
<td>0.49</td>
<td>0</td>
<td>1</td>
<td>1203</td>
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<tr>
<td>B2B</td>
<td>Dummy that equals 1 if the venture sells to enterprises.</td>
<td>0.51</td>
<td>0.5</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>On Premises</td>
<td>Dummy that equals 1 if the venture builds a on-premises software product.</td>
<td>0.09</td>
<td>0.28</td>
<td>0</td>
<td>1</td>
<td>1203</td>
</tr>
<tr>
<td>Found Team Size</td>
<td>Number of venture co-founders.</td>
<td>1.92</td>
<td>0.99</td>
<td>1</td>
<td>5</td>
<td>1174</td>
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<tr>
<td>Mean Jacc Index</td>
<td>Average Jaccard Index with all firms founded prior to venture’s founding.</td>
<td>0.04</td>
<td>0.03</td>
<td>0</td>
<td>0.14</td>
<td>1203</td>
</tr>
<tr>
<td>Cat Vector Youth</td>
<td>Log of inverse of age of category vector.</td>
<td>8.46</td>
<td>0.57</td>
<td>6.19</td>
<td>8.79</td>
<td>1203</td>
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<tr>
<td>AI Intensity</td>
<td>Presence of AI-related categories in venture’s category vector.</td>
<td>0.14</td>
<td>0.43</td>
<td>0</td>
<td>3</td>
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<tr>
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<td>Number of firms in same market with Jaccard Index larger than 0.5.</td>
<td>121.19</td>
<td>494.1</td>
<td>0</td>
<td>4579</td>
<td>1203</td>
</tr>
<tr>
<td>Large Direct Comp</td>
<td>Number of large firms in same market with Jaccard Index larger than 0.5.</td>
<td>126.12</td>
<td>494.04</td>
<td>0</td>
<td>4583</td>
<td>1203</td>
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</table>

This table reports descriptive statistics of the main variables at the venture level. The dataset contains 1203 ventures. Variable *founding team size* is missing for some ventures because data about founding teams were not available. 

Text.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Definition</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
<th>N</th>
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</thead>
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<td>Alice</td>
<td>Dummy that equals 1 for ventures in treated markets starting March 2014.</td>
<td>0.2</td>
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<td>Experimentation</td>
<td>Dummy that equals 1 if the venture has started testing its product.</td>
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<tr>
<td>Entry</td>
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<td>0.5</td>
<td>0</td>
<td>1</td>
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</tr>
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<td>Crowdfunding</td>
<td>Dummy that equals 1 if the venture receives funding via crowdfunding.</td>
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<td>Conv Note</td>
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</tr>
<tr>
<td>Angel</td>
<td>Dummy that equals 1 if the venture receives funding via angel.</td>
<td>0.002</td>
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<td>0</td>
<td>1</td>
<td>53924</td>
</tr>
<tr>
<td>Mean Jacc Index</td>
<td>Average Jaccard Index with all firms founded prior to venture’s founding.</td>
<td>0.04</td>
<td>0.03</td>
<td>0</td>
<td>0.14</td>
<td>53924</td>
</tr>
<tr>
<td>Cat Vector Youth</td>
<td>Log of inverse of age of category vector.</td>
<td>8.48</td>
<td>0.53</td>
<td>6.19</td>
<td>8.79</td>
<td>53924</td>
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<tr>
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<td>128.01</td>
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<tr>
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<td>Number of large firms in same market with Jaccard Index larger than 0.5.</td>
<td>132.95</td>
<td>526.83</td>
<td>0</td>
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<td>53924</td>
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</table>

This table reports descriptive statistics of the main variables at the venture-period level. The dataset contains 1203 ventures, 72 periods, and 53924 venture-period observations. Variable *experimentation* is only defined prior to entry, therefore it is missing for venture-period observations after entry. Return to text.
Table 2. Effect of Alice on Experimentation Propensity

<table>
<thead>
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<th>(5)</th>
<th>(6)</th>
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<td>Founded 2012-2013</td>
<td>Founded 2012</td>
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<td>-0.024</td>
<td>-0.025</td>
<td>-0.025</td>
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<td>x</td>
<td>x</td>
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<td>x</td>
</tr>
<tr>
<td>Age FEs</td>
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<td>x</td>
<td>x</td>
<td>x</td>
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<td>x</td>
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<td>Venture FEs</td>
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<td>x</td>
<td>x</td>
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<td>Period FEs</td>
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<td>0.65</td>
<td>0.66</td>
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<td>1.876</td>
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</tbody>
</table>

This table reports estimates of the impact of Alice on experimentation. The estimation uses specification S1. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is experimentation. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Columns 1-2 use the entire sample, columns 3-4 ventures founded in 2012-2013, and columns 5-6 ventures founded in 2012. Return to text.
Table 3. Effect of Alice on Experimentation Propensity - Early Stage

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<td>-0.052**</td>
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<tr>
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<td>Age FEs</td>
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</table>

This table reports estimates of the impact of Alice on experimentation. The estimation uses specification S1. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is experimentation. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Columns 1-2 use venture-period observations such that age<17, and columns 3-4 venture-period observations such that age<11. Return to text.
Table 4. Effect of Alice on Experimentation Hazard

<table>
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<tr>
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<td>0.074</td>
<td>-0.155**</td>
<td>-0.364***</td>
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<td>Weibull</td>
<td>Weibull</td>
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</table>

This table reports estimates of the impact of Alice on experimentation. The estimation uses specification S2. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is time of experimentation. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Return to text.
This table reports estimates of the impact of Alice on entry. The estimation uses specification S2. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is time of entry. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel.

Return to text.
Table 6. Effect of Alice on Entry Propensity

<table>
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<td>0.031</td>
<td>0.035</td>
<td>0.035</td>
<td>0.064**</td>
<td>0.064**</td>
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<tr>
<td></td>
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<td>x</td>
<td>x</td>
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</tr>
<tr>
<td>Age FEs</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
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<td>Venture FEs</td>
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</tbody>
</table>

This table reports estimates of the impact of Alice on entry. The estimation uses specification S1. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The outcome variable is entry. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Columns 1-2 use the entire sample, columns 3-4 ventures founded in 2012-2013, and columns 5-6 ventures founded in 2012. Return to text.
Table 7. Heterogeneous Effects of Alice on Experimentation Propensity - Learning

<table>
<thead>
<tr>
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</tr>
<tr>
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<td>High</td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
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<td>-0.007</td>
<td>-0.062**</td>
<td>0.006</td>
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<tr>
<td></td>
<td>(0.035)</td>
<td>(0.031)</td>
<td>(0.026)</td>
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<td>x</td>
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<td>x</td>
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<td>x</td>
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<td>0.59</td>
<td>0.60</td>
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<td>OLS</td>
<td>OLS</td>
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</tbody>
</table>

This table reports estimates of the impact of Alice on experimentation. The estimation uses specification S1. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is experimentation. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Column 1 uses ventures with low novelty, column 2 ventures with high novelty, column 3 ventures with low AI, and column 4 ventures with high AI. Novelty is measured by logarithm of the inverse of the age of the category vector (low = below median, high = above median). AI Intensity is measured by presence of AI-related categories in the category vector (low = zero, high = one or more). Return to text.
Table 8. Heterogeneous Effects of Alice on Entry Hazard - Learning

<table>
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<th>(4) Entry</th>
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<td>Low</td>
<td>High</td>
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<td>0.693**</td>
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<td>(0.102)</td>
<td>(0.077)</td>
<td>(0.297)</td>
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<td>x</td>
<td>x</td>
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<tr>
<td>Dyn Controls</td>
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<td>x</td>
<td>x</td>
<td>x</td>
</tr>
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<td>Market FEs</td>
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<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Founding Year FEs</td>
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<td>x</td>
<td>x</td>
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</table>

This table reports estimates of the impact of Alice on entry. The estimation uses specification S2. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is time of entry. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Column 1 uses ventures with low novelty, column 2 ventures with high novelty, column 3 ventures with low AI, and column 4 ventures with high AI. Novelty is measured by logarithm of the inverse of the age of the category vector (low = below median, high = above median). AI Intensity is measured by presence of AI-related categories in the category vector (low = zero, high = one or more). Return to text.
<table>
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<th></th>
<th>(1)</th>
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<td></td>
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<td>-0.027</td>
<td>-0.073*</td>
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<td>(0.036)</td>
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<td>x</td>
<td>x</td>
<td>x</td>
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<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
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<td>Venture FEs</td>
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<td>F</td>
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This table reports estimates of the impact of Alice on experimentation. The estimation uses specification S1. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is experimentation. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Column 1 uses ventures with low competition, column 2 ventures with high competition, column 3 ventures with low large-firm competition, and column 4 ventures with high large-firm competition. Competition is measured by the number of firms in the same market and with Jaccard Index higher than 0.5 relative to the focal venture (low = below median, high = above median). Large-firm competition is measured by the number of firms with at least 1000 employees in the same market and with Jaccard Index higher than 0.5 relative to the focal venture (low = below median, high = above median). Return to text.
Table 10. Heterogeneous Effects of Alice on Entry Hazard - Competition

<table>
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<tr>
<th></th>
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<th>Entry Large-Firm Competition Low</th>
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<td>0.343*</td>
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<td>(0.228)</td>
<td>(0.155)</td>
<td>(0.184)</td>
<td>(0.145)</td>
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<tr>
<td>St Controls</td>
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<td>x</td>
<td>x</td>
<td>x</td>
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<tr>
<td>Dyn Controls</td>
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<td>x</td>
<td>x</td>
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<td>x</td>
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<tr>
<td>Founding Year FEs</td>
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<td>x</td>
<td>x</td>
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</table>

This table reports estimates of the impact of Alice on entry. The estimation uses specification S2. Each column reports coefficients and standard errors in parentheses. Standard errors are clustered at market level. Stars denote * p<0.10, ** p<0.05, *** p<0.01. The outcome variable is time of entry. Static controls include platform, B2B, on-premises, and founding team size. Dynamic controls include three lagged values of crowdfunding, convertible note, and angel. Column 1 uses ventures with low competition, column 2 ventures with high competition, column 3 ventures with low large-firm competition, and column 4 ventures with high large-firm competition. Competition is measured by the number of firms in the same market and with Jaccard Index higher than 0.5 relative to the focal venture (low = below median, high = above median). Large-firm competition is measured by the number of firms with at least 1000 employees in the same market and with Jaccard Index higher than 0.5 relative to the focal venture (low = below median, high = above median). Return to text.