BANK FOR INTERNATIONAL SETTLEMENTS

BIS Working Papers

No 901

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Monetary and Economic Department

November 2020

JEL classification: G32, G38, M13, O3.

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This publication is available on the BIS website (www.bis.org).

ISSN 1020-0959 (print)
ISSN 1682-7678 (online)

Inside the Regulatory Sandbox: Effects on Fintech Funding

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November 3, 2020

Abstract

Policymakers around the world are adopting regulatory sandboxes as a tool for spurring innovation in the financial sector while keeping alert to emerging risks. Using unique data for the UK, this paper provides first evidence on the effectiveness of the world's first sandbox in improving fintechs' access to finance. Firms entering the sandbox see a significant increase of 15% in capital raised post-entry, relative to firms that did not enter; and their probability of raising capital increases by 50%. Our results suggest that the sandbox facilitates access to capital through two channels: reduced asymmetric information and reduced regulatory costs or uncertainty. Our results are similar when we exploit the staggered introduction of the sandbox and compare firms in earlier to those in later sandbox cohorts, and when we compare participating firms to a matched set of comparable firms that never enters the sandbox.

JEL Codes: G32, G38, M13, O3.

Keywords: fintech, regulatory sandbox, start-ups, venture capital.

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1 Introduction

The rapid growth of innovative companies that use new technology (so-called fintechs) has the potential to transform the financial sector fundamentally.[1] Fintechs hold the promise of spurring competition, leading to sizeable efficiency gains, more choice for consumers and enhanced financial inclusion. However, the potentially disruptive growth of firms offering novel products and services poses new challenges for financial stability and consumer protection.[2] Policymakers around the world are stepping up their efforts to foster innovation in the financial sector while keeping alert to emerging risks.

A landmark initiative was the creation of the "regulatory sandbox" by the United Kingdom's Financial Conduct Authority (FCA) in November 2015. Sandboxes offer fintechs a controlled testing environment in which they can try out their products on a limited set of customers under restricted authorisation. Testing occurs under close regulatory supervision: firms receive advice to help them navigate the complexities of regulations and to ease the route to authorization. Regulators, on the other hand, use sandboxes to learn about new financial technologies and emerging trends, as well as to identify associated risks before products are launched for the mass market.

A key objective of sandboxes is to foster innovation by facilitating fintechs' access to financing at early stages of development. Since fintechs offer new products in an environment of high regulatory uncertainty, they face serious challenges of asymmetric information and often struggle to raise enough capital to develop products and expand.[3] By now, around 50 countries have followed the UK and introduced their own regulatory sandbox, often with the goal of nurturing the fintech sector (Wechsler, Perlman and Gurung, 2018; Schizas, McKain, Zhang, Garvey, Ganbold, Hussain, Kumar, Huang,

[1]The Financial Stability Board defines the term fintech as: 'technologically enabled financial innovation that could result in new business models, applications, processes, or products, with an associated material effect on financial markets and institutions and the provision of financial services' (Financial Stability Board, 2017).

[2]Fintechs often rely on sweeping technological advancements (such as artificial intelligence, machine learning, blockchain technology, big data analytics, or the internet of things) that pose significant privacy, regulatory, and law-enforcement challenges. A further risk associated with fintechs is cyber risk (Aldasoro, Gambacorta, Giudici and Leach, 2020).

[3]While investors' enthusiasm for fintech start-ups has boomed since 2010, reaching over $200 billion across 5,000 deals worldwide in 2019, investments have been volatile, displaying for example a sharp decline in 2016 and 2017 (see Figure 1).

Wang and Yerolemou, 2019).[4] And yet, despite the wide-spread adoption of sandboxes and significant attention in the media and policy circles, little empirical evidence exists on whether sandboxes actually help fintechs raise funding. Nor is there any evidence on the underlying channels that could be at work.

In this paper, we analyze how entering the FCA's regulatory sandbox affects fintechs' ability to raise funding. We collect unique data on capital raised by fintechs in the UK for the period from 2014q1 to 2019q2. Our sample covers fintechs that joined the sandbox (treated firms), as well as a large group of comparable control firms. Granular data on funding raised, broken down by individual investor, as well as background information on firm age, size, industry, location, and CEO background allow us to investigate different channels through which the sandbox affects firms' access to capital. Our main finding is that entry into the sandbox is associated with a higher probability of raising funding and an increase in the average amount of funding raised by around 15% (or $700,000), relative to firms that did not enter the sandbox. Investigating the mechanism, our evidence suggests that regulatory sandboxes reduce information asymmetries and regulatory costs or uncertainty.

For identification, we rely on two complementary approaches. First, we focus on the sample of firms that are accepted into the sandbox and exploit the fact that these firms entered the sandbox in five different cohorts. Entry is staggered over rounds of six months, allowing us to compare a firm's capital-raising activity before and after participation in the sandbox, relative to firms that will enter the sandbox at a later stage. We find a highly significant and economically meaningful effect of entry on capital raised. Relative to firms that will enter the sandbox at a later date, entry into the sandbox is followed by a 14% to 15% increase in capital raised over the following two years. The increase in capital raised corresponds to about one standard deviation.

Selection into the sandbox is not random – as we discuss in Section 2 – and the entry date could be correlated with unobservable firm characteristics. Yet, we show that there

[4] At the international level, national regulators take part in the Global Financial Innovation Network, a global sandbox initiative led by the UK's FCA (Ehrentraud, Ocampo, Garzoni and Piccolo, 2020). See also a recent survey by the World Bank and Cambridge Center for Alternative Finance (CCAF) (2019) on regulating alternative finance.

are no differential pre-trends across firms, and that among the group of firms that enter the sandbox at some point, the specific entry date is uncorrelated with observable firm characteristics. Likewise, our results are robust to controlling for firm age, CEO gender, or location; and to the inclusion of firm fixed effects. These facts mitigate concerns that our results are explained by omitted variables or selection effects. We also find that including time-varying fixed effects at the industry level does not affect the size or significance of our coefficients in a substantive way, despite more than doubling the R^2. In other words, sandbox entry is likely orthogonal to unobservable time-varying industry characteristics, further reducing potential concerns about self-selection and omitted variable bias (Altonji, Elder and Taber, 2005; Oster, 2019).

To further strengthen identification, in a second step we compare sandbox fintechs to a set of control firms that never enters the sandbox. Using a coarsened exact matching approach, we select a sample of matched control firms that are statistically similar in terms of observable firm characteristics: age, CEO gender, industry, and location. We then estimate a difference-in-differences specification with firm and time fixed effects, comparing firms that enter the sandbox to those that never enter the sandbox. In the matched sample, we find almost identical effects to our baseline strategy: entry into the sandbox is associated with a relative 15.1% increase in funding raised.

After establishing that sandbox entry improves firms' access to funding, we investigate the underlying mechanisms. Specifically, we distinguish between the following channels: first, the sandbox is a 'marketing device', i.e. simply entering the sandbox leads to publicity and hence more funding, irrespective of actual firm performance or support. Second, the sandbox serves as a 'stamp of approval', i.e. it reduces information asymmetries, as being accepted into the sandbox signals high quality. And third, the sandbox reduces regulatory uncertainty or costs, i.e. the dedicated case officer helps sandbox firms in navigating uncertainties about legal challenges to their services or products.

Our results suggest that the sandbox reduces information asymmetries and regulatory costs. We find no support for the notion that sandboxes serve purely as a marketing device.[5] First, we show that the positive effect of sandbox entry on capital raised is particularly pronounced for smaller and younger firms, i.e. firms that are usually considered more opaque and hence subject to severe informational frictions (Hall and Lerner, 2010). We find similar results when we compare firms by type of funding. Entry into the sandbox increases deal volume especially for venture capital deals, which are more information-sensitive, compared to other types of deals. Second, we show that entry into the sandbox is followed by an increase in first-time investors and in the share of investors that are based outside the UK. Since new investors and investors that are located further away from the issuing firm are likely to face higher information asymmetries, we interpret this finding as evidence that the sandbox reduces information asymmetries. Finally, we show that firms with a CEO who has a personal background in (financial) law benefit less from entry into the sandbox. This is in line with anecdotal evidence that CEOs without prior experience in financial regulation benefit more from the guidance provided by case officers (Deloitte, 2019). If sandboxes promote all firms through marketing – irrespective of their underlying features – then we should not find any differential effects across firm types.

In principle, investors could learn about firms as their quality is gradually revealed to the market over time, irrespective of entry into the sandbox. Firms' ability to raise funding would then increase in lockstep. Instead, if investors learn about the quality of a firm because of the sandbox certification, firms' ability to raise funding will increase immediately upon entry. We find that the strongest effects on funding raised occur in the first two quarters upon entry. Four quarters after entry, the sandbox has a modest positive, but insignificant, effect on funding raised. This pattern hence suggests that entry into the sandbox acts as a certificate and signals firms' quality. The increase in funding raised does not reflect a gradual revelation of firms' quality.

We provide a set of further robustness checks. We rule out that the effect of the sandbox is driven purely by an increase in the supply of funds. We use matched investor-

[5] In principle, a marketing effect could also disproportionately affect smaller or younger firms by raising awareness among investors. As long as the sandbox selects innovative firms, the marketing effect and stamp of approval effect would be complementary. For expositional clarity we decide to treat both effects as distinct.

firm data and include investor*time fixed effects. This approach allows us to absorb any unobservable time-varying changes affecting investors. For example, contemporaneous tax reliefs or preferential treatment for fintech investments could act as confounding factors. We show that a firm entering the sandbox is more likely to raise capital even if we hold changes in the supply of capital fixed. We also use alternative estimation methods to account for the presence of zeros in our dependent variable, for example negative binomial regressions, and show that our results are insensitive to the chosen method. Further, we confirm our findings when we use nearest neighbor matching or propensity score matching instead of coarsened exact matching.

Despite the widespread adoption of sandboxes, to the best of our knowledge there exists no micro-evidence on their effectiveness. Regulatory sandboxes pursue different goals, for example promoting innovation and competition, increasing the consumer surplus, and facilitating fintechs' access to finance. While the short time span since their inception does not allow us to evaluate effects on consumer surplus or financial stability, our paper provides first evidence that sandboxes help young and innovative fintechs to raise capital and hence achieve at least one of their explicit goals. Our results suggest that sandboxes could become a crucial policy tool for harvesting the benefits of financial innovation.

Our paper contributes to the current debate on public policies to foster innovation (OECD, 2017; Auer, 2019).[6] A recent literature has established that fintechs face serious obstacles to raising capital (Block, Colombo, Cumming and Vismara, 2018; Haddad and Hornuf, 2019), despite the fact that their innovation provides value to innovators and investors (Chen, Wu and Yang, 2019). Market failures can lead to sub-optimal private-sector expenditure on research and development, necessitating public policies to foster innovation, eg through business incubators or accelerators (Gonzalzez-Uribe and Reyes, 2020).[7] Policy makers hence need to promote innovation in the financial sector, but

[6]For literature on financing innovation, see Kerr and Nanda (2015); Edler and Fagerberg (2017); Bloom, Van Reenen and Williams (2019). The discussion gained prominence in light of the decline in dynamism and productivity growth since the Great Recession (Doerr, Raissi and Weber, 2018; Doerr, 2019; Duval, Hong and Timmer, 2020).

[7]Biais, Bisiere, Bouvard and Casamatta (2019) show that equilibrium investment in computing capacity can be excessive. Brown and Davies (forthcoming) show that early-venture fundraising by entrepreneurs can be inefficient if information is costly, leading the entrepreneur to undertake bad

without compromising data privacy, financial stability or consumer welfare.[8] Regulatory sandboxes are among the most prominent policy tools to make the most of this trade-off (Restoy, 2019): by fostering innovation in a controlled environment, regulators hope to learn about new technologies and associated risks before they hit the mass market.[9]

We also relate to literature that investigates how to regulate and nurture fintechs (Arner, Barberis and Buckey, 2017; Zetzsche, Buckley, Barberis and Arner, 2017; Bank for International Settlements, 2019). Brummer and Yadav (2019) argue that the entry of fintechs and large technology companies into finance poses a policy trilemma: regulators have not been able to provide clear rules, maintain market integrity, and encourage financial innovation at the same time. The trilemma is particularly acute in the realm of fintechs, as new and untested technologies introduce unprecedented uncertainty about their risks and benefits.[10] Brummer and Yadav (2019) call for supplemental administrative tools to support not only innovation in the market, but regulation and experimentation as well. Sandboxes could be one such tool: they provide regulators with a tool for better gauging the potential welfare implications that innovations have for consumers before they are launched. An assessment of the effectiveness of sandboxes and an understanding of the channels through which they operate is hence indispensable.

The reminder of the paper is organized as follows. Section 2 provides background information on the UK regulatory sandbox. Section 3 gives an overview of our data and sample of fintechs. Section 4 explains our empirical strategy, reports the main results and provides evidence on the mechanisms at work. In Section 5 we present robustness tests. Section 6 concludes and discusses the relevance of our findings for public policy.

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projects and forgo profitable project.

[8]Recent research argues that the use of non-traditional data for eg credit scoring could lead to changes in consumer behaviour or discrimination (Fuster, Goldsmith-Pinkham, Ramadorai and Walther, 2018; Berg, Burg, Gombović and Puri, 2020).

[9]The Global Financial Innovation Network, which includes more than 50 financial authorities, central banks and international organisations, and focuses on regulatory sandboxes, reflects the widespread desire to provide fintech firms with an environment to test new technologies (Tsang, 2019; Ehrentraud et al., 2020).

[10]New technologies often rely on the analysis of vast amounts of data, which could lead to rising concentration, rent extraction, and price discrimination. At the same time, an efficient use of non-traditional data promises a reduction in costs and greater financial inclusion (Boissay, Ehlers, Gambacorta and Shin, forthcoming).

2 The UK regulatory sandbox

The UK has become a global fintech hub, especially in London. Over the last decade, around 18% of worldwide funding for fintech start-ups was raised in the UK (see Figure 1, panel (a)). This number is topped only by the US, where fintechs raise 47% of global funding. Notably, the UK increased its relative share from less than 15% in 2010-2014 to over 20% for the 2015-2019 period (see panel (b)). It thereby overtook China (with a share of 15%) in terms of volume. This is also reflected in the UK's local workforce. Reports by the HM Treasury (2016; 2019) note that more people work in the UK fintech sector than in New York's. However, despite these encouraging numbers, the report (and others like it) stresses that fintechs still face severe obstacles in raising capital. Since access to capital is crucial for young firms with high growth potential, its scarcity threatens to slow growth and innovation.

Partly in response to these worries, the Financial Conduct Authority (FCA) announced the world's first regulatory sandbox as part of its "Project Innovate" in 2015 (FCA 2015). The sandbox operates on a cohort basis with two six-month test periods per year. Since November 2016 five cohorts of firms have been accepted into the sandbox on the following dates: 7 November 2016, 15 June 2017, 5 December 2017, 3 July 2018, and 29 April 2019. Each cohort averages around 25 firms. In total 118 firms have been accepted into the FCA sandbox. The FCA publishes the list of accepted firms for each round of the sandbox. Most participants are start-ups and small and medium enterprises in retail banking (including payments), wholesale markets, retail and wholesale investment and lending, and insurance propositions.

The 'sandbox process' contains four distinct steps: application, selection, testing and exit. The FCA selects firms out of the pool of applicants based on different criteria. Precisely, firms must offer a genuine innovation that benefits UK consumers. The innovation should constitute and improvement over existing products and services, and hence promote competition.[11] Firms that fulfil these requirements and for which the

[11]Further requirements are that the firm possesses well-developed testing plans, including clear acceptance criteria, or that there are sufficient safeguards in place to protect the consumers and to provide redress in case of need.

FCA's input is deemed useful ('need for support criteria') are selected into the sandbox. They are assigned a dedicated case officer who helps to design the test setup, provides guidance to complete the necessary paperwork for authorization, and helps the firm navigate the regulatory environment throughout the process.[12]

Once firms are granted authorisation, they test their products in a limited market environment. Specifically, successful applicants set up their capabilities subject to regular reporting requirements to monitor how the technology is evolving and how the business is meeting its compliance targets. In this phase, firms have to familiarize themselves with the regulation with the help of case officers. After six months, firms submit a final testing report and exit the sandbox. They often apply for a permanent authorization upon completion.[13]

One of the main objectives of the sandbox is to attract investments toward fintechs by curbing regulatory uncertainty and costs (FCA 2015). Regulatory uncertainty discourages investment because investors are hesitant to invest in a firm that is offering products whose regulatory framework is unclear. Even innovative and successful companies are under constant threat of being shut down or forced to drastically change their business to comply with continuously evolving regulations. According to the World Bank's *Doing Business* report, regulatory restraints are a key barrier to innovation.[14] The sandbox reduces regulatory uncertainty, as regulators throughout the process are able to collect information on new products, identify new risks, and accordingly adapt existing or introduce new regulation swiftly. Advice by trained case officers promises to lower regulatory costs for firms and reduce the risk to firms of offering products that could be in violation of the regulatory environment.

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[12]For example, case officers help the selected firms to design and implement the tests, ensuring that appropriate safeguards are embedded in products under revision; they highlight the regulation relevant for the company; and facilitate engagement with FCA subject matter experts, reducing the expenditures on external regulatory consultants and helping firms better understand how they fit in the current regulatory framework.

[13]The final testing report summarizes the results of the testing and the planned way forward/next steps. Firms that want to continue with the business model that has been tested in the sandbox apply for a variation of permission. Alternatively, firms can get appropriate legal advice and agree with the FCA that authorisation is not required or reconsider their business model and regulatory position.

[14]In a 2020 survey by software provider Finastra (2020) among over 750 fintech companies, only 4% of companies believe that there are no barriers to innovation from existing regulation; and almost half of respondents state that regulation is too tight.

Besides regulatory costs and uncertainty, another critical obstacle to capital access relates to information problems. Asymmetric information is particularly acute in venture capital markets, because issuers are mainly early-stage firms with no prior track records (Trester, 1998). Resolving information problems in such a environment requires that investors engage in an intensive and costly up-front screening and post-investment monitoring. The inherent uncertainty about the quality of new products and services offered by fintechs thus presents a serious obstacle to raising capital (Haddad and Hornuf, 2019).[15]

Sandboxes could significantly curb informational frictions. Regulatory oversight and continuous dialogue with the regulator during the testing period offers reassurance to investors that firms meet their regulatory obligations. In the UK context, adverse selection is potentially reduced because the FCA claims to select firms that offer genuine innovation with clear benefits to consumers.[16] Moral hazard may be reduced if close supervision by the FCA spurs firms to improve their governance and adopt more rigorous policies and processes.

While interest in regulatory sandboxes is strong, there are divergent views on their effectiveness and purposes (Allen, 2019; Quan, 2020). For example SEC Commissioner Peirce (2018) worries that regulators "facilitating and hosting the sandbox" may play the role of a gatekeeper and thereby slow down or even halt innovation. A Financial Times article considers a 'fintech sandbox' a regulatory approach harmful for consumers and one that could create an uneven playing field between the start-ups which are accepted into the sandbox and those which are not.[17] According to a report commissioned by the United Nations Secretary-General's Special Advocate for Inclusive Finance for Development (2019), sandboxes are 'not always the answer for regulating inclusive fintech'. In evaluating the effectiveness of sandboxes in helping fintechs raise capital, we hope to contribute to the debate on their costs and benefits.

[15]These barriers to entry could explain why the unit cost of financial intermediation remained stubbornly high for many years and has only recently started to decline (Philippon, 2016).

[16]The Financial Conduct Authority (2015) states that the selected firms are singled out as offering genuine, disruptive innovations that "lead to better outcomes for consumers through, for example, an increased range of products and services, reduced costs, and improved access to financial services".

[17]Financial Times Alphaville (2018), 'A "fintech sandbox" might sound like a harmless idea. It's not...'.

Enhancing firms' access to capital is an explicit and intermediate goal of sandboxes. A complementary long term policy objective is to boost competition or increase the consumer surplus. Additionally, sandboxes have the goal of promoting innovation while ensuring financial stability. The short time span since their inception does not yet allow us to evaluate effects on, for example, consumer surplus or financial stability, since these effects will only materialize over the coming years and pose significant measurement challenges. Instead, this paper focuses on the intermediate goal: whether sandboxes help young and innovative fintechs to raise capital – one explicit goal for regulatory sandboxes that can be already empirically investigated.

3 Data description and sample selection

PitchBook provides detailed data on capital raised at the deal level and is one of the most comprehensive sources of investment data for the fintech sector. We obtain data on all individual deals of the 118 sandbox firms, as well as deals for a random sample of around 1,400 control firms (more below), over the period 2014q1 to 2019q2. For each deal, PitchBook records detailed characteristics such as issuer name, deal date, deal amount, and type/purpose of the deal. The main types of deals are venture capital (VC) deals (including accelerators, incubators, seed, and angel deals), private equity (PE) deals (mainly for growth/expansion), and restructuring deals (including deals for mergers and acquisitions and buyout deals). VC, PE and restructuring deals represent around 63%, 7%, and 17% of the total number of deals.

We further collect any available information on the history of each company and the biography of the CEO (or founder). We obtain the year the company was founded, its primary industry classification, and the current business status (start-up, generating revenue/profitable, other). We also record the city where the company is headquartered. Information on the CEO includes gender, educational background, and occasionally the year of graduation. Unfortunately, information on all items is seldom available for every company. Finally, we collect information on the identity of each investor participating in each deal, as well as the size of investors' total portfolio. We also observe the country where the investor is headquartered, which allows us to separately investigate the behavior of foreign and UK-based investors. PitchBook also provides information on whether an investor is a new/first-time investor in the issuing firm.

3.1 Sandbox firms

In our primary analysis, we focus on firms that enter the sandbox in one of the five cohorts during our sample period. We manually identify sandbox firms in the PitchBook database, using the name and description of the company provided by the FCA. Out of the 118 firms that have been accepted into the sandbox, we are able to identify 106 in the PitchBook database.

We perform a series of steps to clean and prepare the data. First, seven firms entered the sandbox more than once. To avoid double-counting, for these firms we set the entry date at the date when they entered the sandbox for the first time. Second, some deals report no time of deal. We drop these observations (36 deals), since we cannot determine whether they took place before or after sandbox entry. We further drop observations with missing or zero deal size (83 deals). Finally, we require firms to report their primary industry, location, CEO gender, and founding date (these are the items with reasonably consistent coverage).

We then aggregate our deal data, which is at daily frequency, to the quarterly level. Since the focus of our analysis is on fintech start-ups, in our baseline specification we exclude sandbox firms that are (or belong to) large or listed firms and therefore do not report accounts or do not raise capital separately from the parent (eight firms, for example Lloyds or HSBC). However, we will use these larger firms when we investigate the mechanism. Finally, we trim log deal volume at the 1^{st} and 99^{th} percentile to keep outliers from driving our results. We then create a balanced panel, where we replace missing firm-quarter observations with zeros and exclude all observations prior to a firm's founding year. We end up with a sample of 908 firm-quarter observations for 56 firms from 2014q1 to 2019q2.

Our main outcome variables are *log(deal amt)*, which equals the log of (1+total capital raised) by firm f in quarter t; and dummy *capital raised*, which equals one if a firm raised any capital in a quarter, and zero otherwise. As main explanatory variable, for each firm we construct the dummy *post SB entry*, which takes on value zero before sandbox entry, and value one upon entry and thereafter. As firm-level controls, we use the log of (1+company age), dummy *male*, which takes on value one if the CEO is male and zero otherwise, and a dummy that indicates whether or not a firm is based in London. We further collect information on CEOs' biographies to create the dummy *CEO has law degree*, which takes on value one if a CEO holds a degree in law or has previous experience with financial law. Further, we collect information on the number of total investors and the number of new investors per deal, as well as the share of UK-based, non-UK-based, and US-based investors.

Table 1, panel (a) reports deal-level descriptive statistics. There is significant variation in deal characteristics, with an average deal size of \$4.7 million and a standard deviation of \$27.5 million. Out of all firm-quarter observations, firms raise debt in 6.1% of all cases. Panel (b) shows that the median (average) firm is four (six) years old as of 2019. Some 75% of our firms are less than six years old, 52% of firms are based in London. The median and average firm has one CEO, and around four-fifth of CEOs are male. The average CEO graduation year is 1998. The average deal has around 1.8 investors (with a maximum of 11), and 1.7 new investors (with a maximum of 10). Panel (c) shows that when capital is raised, the average share of investors based in the UK is around 60%. Out of the 40% of foreign investors, roughly half are US-based. The similar shares for total and new investors suggest that the majority of investors that invest in sandbox firms do so for the first time.

Our empirical strategy exploits the staggered design of the sandbox: firms enter in different cohorts. One implicit identification assumption is hence that a firm's characteristics are not systematically correlated with its cohort. To test this assumption, Table 2 shows results when we estimate firm-level regressions with different firm characteristics as outcome variable. As explanatory variable, we include dummies for each cohort. *Sandbox cohort 1* is the reference group. Column (1) shows that there are no systematic differences in firm age across cohorts, column (2) shows that – except for the fifth cohort – firms are not significantly more or less likely to be from London in later cohorts. Column (3) shows that whether a firm reports that it is already generating revenue or not does not differ across cohorts either. Columns (4) and (5) use a dummy with value one for companies with at least one male CEO and the number of CEOs as dependent variable. With the exception of cohort three, which is significantly more likely to have a male CEO, there are no statistically significant differences. In column (6) we use the firm fixed effects resulting from a regression of log deal amount on firm fixed effects as dependent variable. The fixed effects reflect all observable and unobservable time-invariant firm-level variation that is correlated with the amount raised. Column (6) shows no significant correlation with the firm's respective cohort.

We are aware that selection into the sandbox is not random – after all, the FCA aims to accept firms that offer an innovative product. Yet, Table 2 suggests that *among* the group of firms that enter the sandbox, the exact entry date is not systematically correlated with firm characteristics. Below, we show that there are also no differential pre-trends across firms, further supporting our identifying assumptions. These facts mitigate concerns that our results are explained by omitted variables or selection effects.

3.2 Control firms

In addition to our first analysis, which focuses exclusively on firms that entered the sandbox, we implement a second analysis: We contrast the performance of sandbox firms with that of similar firms that never enter the sandbox. We first collect PitchBook data on a random sample of 1,400 fintechs with around 3,000 deals. For these firms, we collect information on deal size and date, as well as on age, CEO gender, location, industry, and business status, and create a 2014q1-2019q2 panel. We then use coarsened exact matching (CEM) to select a suitable sample of control firms for our sample of sandbox firms (Blackwell, Iacus, King and Porro, 2009).

CEM creates matches between the treated (sandbox) and control firms (non-sandbox), based on the set of controls: age, CEO gender, location, industry, and business status. Controls are coarsened to maximize balance of the matched data set and ensure that most treated observations have a match (Iacus, King and Porro, 2012). We end up with a sample of 54 sandbox firms, matched to 158 suitable control firms. Out of our 908 sandbox observations, 24 observations (two firms) result in no match. Our total sample of treated and control firms contains 3,820 firm-quarter observations and is balanced in terms of observable firm characteristics.

4 Empirical strategy and results

This section explains our empirical strategy and presents our main results. It then sheds light on the underlying mechanisms.

4.1 Sandbox entry and capital raised

How does entry into the regulatory sandbox affect firms' ability to raise capital? To investigate this relation, we first focus on the group of firms that entered the sandbox at some point and exploit the staggered entry dates of firms in different cohorts. Therefore, we compare sandbox firms that entered the sandbox in quarter t to firms that have not yet entered the sandbox in t, but will do so at a later date. As shown in Table 2, the exact entry date into the sandbox is not systematically correlated with firm characteristics among the group of firms that enter the sandbox, mitigating concerns about omitted variables or selection effects. Second, we compare firms that entered the sandbox to firms that never entered the sandbox, but are statistically indistinguishable in terms of observable firm characteristics. For this exercise, we focus on the set of sandbox and non-sandbox companies that we matched via coarsened exact matching.

We estimate the following regression at the firm-quarter level:

$$y_{f,t} = \beta \; post\ SB\ entry_{f,t} + controls_f \times post\ SB\ entry_{f,t} + \theta_f + \tau_t + \varepsilon_{i,t}. \qquad (1)$$

The dependent variable is either the logarithm of 1 plus the total deal amount for firm f in quarter t; or the dummy *capital raised*, which takes value 1 if the firm raises a positive amount of capital in a given quarter. The dummy *post SB entry* takes the value of one after sandbox entry, and zero for all quarters prior to entry. We further include a vector of time-invariant firm characteristics, interacted with the post dummy: log firm age in 2019, CEO gender, and a dummy for being headquartered in London. We cluster standard errors at the firm level to account for serial correlation.

In some specifications we include a set of fixed effects. Firm fixed effects (θ_f) control for unobservable time-invariant firm characteristics, such as industry or location. Time fixed effects (τ_t) control for common trends. To tighten identification and control for unobservable time-varying shocks on the industry level, we occasionally include industry*time fixed effects instead of time fixed effects. Industry*time fixed effects absorb any unobservable factors that affect firms within an industry over time.

Including firm fixed effects in Equation (1) represents a difference-in-differences specification: We compare firms that entered the sandbox at time t to firms that have not yet entered the sandbox at t, holding unobservable firm characteristics constant. Coefficient β hence indicates whether firms that enter the sandbox raise more or less capital, relative to firms that do not enter the sandbox at time t. If the sandbox facilitates firms' access to capital, we expect $\beta > 0$. The identification assumptions are i) that absent treatment, firms that enter the sandbox at time t would follow the same trend in capital raised as firms that enter the sandbox at a later date (parallel trends), and ii) that funding raised by one firm does not affect the funding raised by another firm (conditional independence). We provide direct evidence on parallel trends below. With regard to conditional independence, the focus of our analysis is on a set of relatively small fintech firms. Arguably, even a sizeable increase in capital raised by one of these firms would not deplete the amount of total capital available for other firms. For example, total venture capital investment in the UK stood at around £3.8bn in 2016. The average deal volume in our sample represents only a small fraction of this total.

Before moving to the regression analysis, Figure 2, panel (a) provides non-parametric evidence that firms raise more capital upon entering the sandbox. The horizontal axis plots the time dimension. A value of zero denotes the date at which a firm enters the sandbox, and the axis ranges from 8 quarters before to 12 quarters after sandbox entry. On the vertical axis, it shows the total funding raised per quarter (left axis), as well as the cumulative funding raised (right axis). The amount of capital raised increases sharply around time zero, i.e. when firms enter the sandbox. The increase in capital raised is particularly pronounced in the first year upon entry, and peters out after eight quarters.[18]

We now investigate this pattern in greater detail. Table 3 reports the results of Equation (1) over our sample period. Panel (a) restricts the analysis to our set of firms that join the sandbox at some point. In columns (1) to (4), we estimate the effect of the sandbox at the intensive margin for the treated sample and use log deal amount as dependent variable. Column (1) includes firm-level controls and shows that firms that enter the sandbox raise 9.3% more capital than firms that have not (yet) entered the sandbox. When we add firm fixed effects in column (2), the coefficient increases in size and statistical significance. Adding time fixed effects in column (3) leads to a further increase in the coefficient. Conditional on unobservable time-invariant firm characteristics and common shocks, firms see an increase in deal amount by about 14.8% after entering the sandbox, relative to firms that did not enter the sandbox. Evaluated at the average deal, this represents an increase in capital raised of $700,000. Finally, in column (4) we add industry*time fixed effects. The coefficient remains significant at the 5% level and similar in magnitude to column (3). The stability of the coefficient on *post SB entry* in light of an increase in R-squared from 0.076 to 0.157 from columns (2) to (4) suggests that our treatment variable (i.e. the timing of sandbox entry) is likely orthogonal to further unobservables, e.g. to self-selection and omitted variables (Altonji, Elder and Taber, 2005; Oster, 2019). This result is in line with findings in the balancedness test in Table 2.

In columns (5) to (6) we explore the effect of the sandbox at the extensive margin. The outcome variable is dummy *capital raised*, so we estimate logistic regressions. Column (5) includes firm controls; column (6) adds firm and time fixed effects. Both columns report average marginal effects. The estimated effects are economically large and statistically significant: the probability of raising capital increases by 3.1 percentage

[18] Some firms already know about their acceptance into the sandbox already a few months before their official entry. Some firms could disclose their still-informal relationship with the FCA to investors, which could explain the small increase in funding between $t = -1$ and $t = 0$.

points upon sandbox entry, relative to an average 6.1% probability of raising capital in a given quarter. In other words, entry into the sandbox is associated with an increase of about 50% in the probability of raising capital. Taken together, results in panel (a) suggest that entry into the sandbox has an economically and statistically significant effect on firms' ability to raise capital.

Before we compare sandbox firms to our set of matched control firms, we investigate whether there were any potential pre-trends across groups. To this end, we include a set of dummy variables in our baseline regression to trace out the quarter-by-quarter effects of sandbox entry on the logarithm of (1+deal amount):

$$log(1+amt)_{f,t} = \sum_{k=-4}^{K=12} \beta_k \; SB_{f,k} + \theta_f + \tau_t + \varepsilon_{f,t}. \tag{2}$$

Dummy variables 'SB' equal one in quarter k before or after sandbox entry. The omitted category is $k = 0$ and corresponds to the date of sandbox entry of firm f. Coefficient β_k is the estimated change in deal amount k quarters before or after entry. θ_f and τ_t denote firm and time fixed effects, standard errors are clustered at the firm level. Figure 2, panel (b) plots coefficients β_k and corresponding 90% confidence intervals. Average deal volume already increases significantly already in the first quarter after entry. The positive effect of entry into the sandbox peaks in the third quarter (13%) and dissipates after around two years. There is no discernible pre-trend: firms that enter the sandbox in quarter t did not raise more capital before entry than firms that enter the sandbox at a later stage. The immediate effect of entry on capital raised and the absence of any differential pre-trends supports our identification strategy.

Having established that sandbox entry is followed by an increase in firms' capital raised when we focus on the firms that enter the sandbox at some date, we now use information on our set of non-sandbox control firms. In Table 3, panel (b) repeats the same exercise as in panel (a), but for the sample of matched control firms. Each regression is now weighted by the respective CEM weights to ensure balancedness in co-variates.[19] The number of firm-quarter observations now increases to 3,820. Coefficient β now indi-

[19]In robustness tests, we report results using alternative matching methods. Both nearest neighbor and propensity score matching yield positive and statistically and economically significant coefficients.

cates whether entry into the sandbox improves firms' access to capital, relative to firms that never enter the sandbox but that are similar in terms of observable characteristics.

In panel (b) our estimated coefficients are similar in terms of sign, size, and significance to those in panel (a) across most specifications. In our most-stringent specification with firm and industry*time fixed effects in column (4), entry into the sandbox increases capital raised by 15.1% in panel (b) vs. 15.0% in panel (a). The probability of raising capital increases significantly as well, by around 3.3 percentage points (or 50%, relative to the mean) in the most conservative specification. Similar to the sandbox sample, coefficients increase in magnitude as we add control variables and fixed effects. In conclusion, Table 3 suggests that entry into the sandbox leads to a significant and economically large increase in capital raised and in the number of times firms raise capital. Furthermore, tightening the specification through additional controls and fixed effects leads to an increase in coefficient size.

4.2 Information asymmetries and regulatory costs

Having established that sandbox entry facilitates firms access to financing, we now investigate two potential channels through which the sandbox could affect firms' access to capital: by reducing information asymmetry, and by lowering regulatory costs and uncertainty.

Theory predicts that the benefits from reduced asymmetric information will be greater in information-sensitive environments (Bolton and Freixas, 2000). A large literature shows that adverse selection and moral hazard are particularly acute for young and small firms that are innovative (Petersen and Rajan, 1994; Czarnitzki and Hottenrott, 2011). These firms often have no prior track records; they are informationally opaque. As a consequence they have no access to public markets, but rely on private markets to grow and develop. Our sandbox firms are predominately young and small firms that offer novel products and services in a new sector (fintech). Venture capital is their main (and often sole) source of funding. A corollary is that venture capital investments are considerably riskier and less liquid than other assets. Resolving information problems in such an environment requires investors to engage in intensive and costly pre-investment due diligence and post-investment monitoring.

Further, based on a large literature that shows that relationships and distance matter (Degryse and Ongena, 2005; Bolton, Freixas, Gambacorta and Mistrulli, 2016), informational asymmetries are generally greater for new investors (i.e. investors that have not invested into the firm previously), and for investors that are based outside of the UK, since they have an informational disadvantage when investing into UK firms. UK investors know their home market better than foreign investors.[20]

A regulatory sandbox can reduce reduce asymmetric information and the associated information collection costs for investors. It provides reassurance to investors that firms are closely monitored and advised, as well as informed about the regulatory framework. Also, investors may perceive selection into the sandbox as a stamp of honor, a guarantee from the regulator that the firm is viable and innovative, as these are the criteria by which they are selected. In sum, if the sandbox helps in overcoming informational asymmetries, we expect it to have a stronger effect on younger or smaller firms; and lead to an increase in the share of new and/or foreign investors.

Table 4, panel (a), first investigates the differential effect of sandbox entry on young and small firms. Columns (1)-(2) report results for our baseline Equation (1), but interact dummy *post SB entry* with a dummy *old firm*, which takes the value of one if a firm's age is above the median value (i.e. if it is at least five years old).[21] Column (1) uses firm and year fixed effects, column (2) adds industry*time fixed effects. Across specifications, entry into the sandbox leads to an increase in capital raised for young firms ($old = 0$). Yet, the positive effect is largely offset for old firms, as can be seen from the negative and economically large coefficient on *old*.

Columns (3)-(4) repeat the exercise, but interact dummy *post SB entry* with a dummy *large firm*, which takes on value one if a firm is affiliated with listed companies, i.e. the firms we initially excluded from our baseline sample. Across specifications,

[20]Previous work has identified geographical distance as an important source of asymmetric information: investors tend to invest a larger fraction of their portfolio in stocks of geographically close firms (Coval and Moskowitz, 1999) and earn abnormal returns on stocks of firms that are geographically close (Ivković and Weisbenner, 2005).

[21]Average age is 3.2 years in the 'young' and 13.9 years in the 'old' group.

entry into the sandbox increases capital raised for small, but not for large firms. If anything, large firms raise less capital after entering the sandbox.[22] Taken together, the results in columns (1)-(4) suggest that the sandbox particularly helps opaque (i.e. young and small) firms to raise capital.

In panel (b) we test the effect of entry into the sandbox on the number of investors and the share of foreign investors in Table 4. In columns (1)-(2), the dependent variable is one plus the number of new investors (in logs) in a deal. The number of new investors increases by 30% post-sandbox entry. The size of the effect doubles when we add firm and industry*time fixed effects in column (2). Hence, there is an increase in new investors for firms that raise capital after entering the sandbox. Columns (3)-(6) look at the composition of new investors. Column (3) shows that there is no change in the share of UK investors, while column (4) shows a large and significant increase in investors from the US. Column (5) shows an economically large (but statistically insignificant) increase in non-UK investors. In conclusion, Table 4, panel (b) provides strong support for the hypothesis that the regulatory sandbox reduces informational asymmetries: firms that enter the sandbox attract new investors, and the new investor base is mostly comprised of foreign investors.

Finally, we provide indirect evidence for the effect of sandboxes on regulatory costs. Anecdotal evidence suggests that firms with a founder with a background in law benefit less from the sandbox, because the case officer's legal advice is less necessary (Deloitte, 2019).[23] To test this proposition more formally, we introduce an interaction term between *post SB entry* and a dummy *law* into Equation (1). The dummy *law* indicates whether a firm's founder has a background in law or the financial sector. Table 4, panel (a), columns (5)-(6) provide results. Column (5) uses firm and year fixed effects, column (6) adds industry*time fixed effects. Across specifications, entry into the sandbox leads to an increase in capital raised for firms without a 'law-degree' CEO ($law\ degree = 0$), while the positive effect is muted for firms with a CEO that has a background in law or

[22] This finding can be explained by the fact that two sandbox firms affiliated with large listed companies raised large amounts of funding prior to entry.

[23] Management literature has established that CEO experience is correlated with firm performance (Bhagat, Bolton and Subramanian, 2010; Gottesman and Morey, 2010).

the financial sector.

5 Extensions and robustness

This section provides extensions of our baseline specification and further robustness checks.

5.1 Investor-firm analysis

Any observed change in capital raised reflects demand and supply effects. On the one hand, firms with profitable investment opportunities want to raise more capital, irrespective of any changes in supply. On the other hand, investors could increase the supply of capital even if there is no change in underlying firm demand. The latter could be due to unobservable changes that relax investors' constraints, for example a change in the tax regime that reduces capital taxes on investments in fintechs. Hence, coefficient β in Equation (1) could reflect demand and unobservable supply forces.

Disaggregated data on the firm-investor level allow us to control for time-varying changes in the supply of capital and investigate whether the sandbox increases demand for capital. To this end, we estimate the following regression at the investor-firm-time level:

$$log(1 + amt)_{i,f,t} = \gamma \ post\ SB\ entry_{f,t} + controls_f + \theta_{i,f} + \tau_{i,t} + \varepsilon_{i,f,t}. \qquad (3)$$

The dependent variable is the amount invested by investor i to firm f in quarter t. Since we observe only the number of investors and the total deal size for each deal, we do not observe the individual amount invested by each investor. In our baseline estimation, we hence split total deal volume on a pro-rata basis. This is, if the deal size is \$100 and there are two investors, we assign \$50 to each investor. For robustness checks, we also split loan volume by investors 'size', measured by their total investments. For example, if the deal size is \$100 and there are two investors, one with aggregate investments of \$1500 and one with aggregate investments of \$500, we assign \$75 to the first and 25 to the second investor. In alternative specifications, we use dummy *capital raised* as dependent variable that takes on value one if a given investor invests in a given firm in quarter t (the extensive margin), and zero otherwise. This approach is hence insensitive to the chosen method of allocation. The dummy *post SB entry* takes the value of one after sandbox entry, and zero for all quarters prior to entry, and we include the vector of time-invariant firm characteristics interacted with dummy *post SB entry.* We cluster standard errors at the firm level.

To control for unobservable firm-investor factors, as well as unobservable investor-specific factors that vary over time (such as changes in the supply of capital due to potentially confounding policies), we include investor*firm ($\gamma_{j,f}$) and investor*time ($\tau_{j,t}$) fixed effects. The combination of both fixed effects allows shocks to affect each firm-investor combination heterogeneously and accounts for any change in investor characteristics (Khwaja and Mian, 2008; Jiménez, Ongena, Peydró and Saurina, 2014). For example, investor*firm fixed effects absorb any time-invariant firm and investor characteristics such as the (informational) distance between the firm and the investor. Time-varying fixed effects at the investor level control for e.g. unobservable changes in investor wealth, income, and tax exception schemes. Coefficient γ hence reflects the effect of entry into the sandbox on firms' demand for capital.

Table 5 reports the results of Equation (3) and shows that sandbox entry is associated with an increase in the demand for capital. In columns (1)-(3), the dependent variable is log(1+amount) based on a pro-rata split; in columns (4)-(6), it is based on the investor-size split. In columns (7)-(9), we use dummy *capital raised*, which takes value one if an investor invests in a given firm in quarter t. Across specifications, entry into the sandbox has a positive and significant effect on investment. This holds without fixed effects, when we add investor*firm and time fixed effects, as well as when we control for confounding supply factors through investor*time fixed effect. In general, the size of the coefficient increases when we tighten the specification. For example, in column (3) with investor*firm and investor*time fixed effects, entry into the sandbox leads to a 4.4% increase in the demand for capital from each investor. If instead we use as dependent variable amounts calculated proportionately to investors' portfolio size, in column (6) entry into the sandbox leads to a 6.2% increase in capital received when we include investor*time fixed effect.[24] Effects of sandbox entry are also economically and statistically meaningful along the extensive margin in columns (7)-(9). Entry into the sandbox leads to a 16.1% higher probability of raising capital in a given quarter when we control for investor*time fixed effects. Table 5 hence suggests that entry into the sandbox is followed by an increase in firms' demand for capital.

5.2 Further robustness tests

Panel (a) in Table 6 provides extensions and robustness checks to our baseline specification in Equation (1). In column (1), the dependent variable is the logarithm of 1 plus the total deal amount for firm f in quarter t. In investigating the mechanism, we have shown that younger and smaller firms – usually seen as more opaque and therefore more sensitive to a reduction in information asymmetry – benefit more from entry into the sandbox. To shed further light on the role of informational frictions, we compare venture capital deals to other types of deals. Due to their early-stage nature, venture deals entail more uncertainty and information barriers, and thus potentially require more active screening on the part of the investor. We should therefore expect the estimated effects to be stronger for venture capital deals than for other deals. When we introduce dummy VC, which takes on value on if a deal is classified as venture capital deal, and zero otherwise, in column (1), we see that the effect of sandbox entry on capital raised is economically larger and statistically significant especially for venture capital deals (for which the effect is about twice as large, compared to other deals), further corroborating our results that the sandbox reduces informational asymmetries.

Due to the nature of our data, our dependent variable takes on the value of zero in several quarters. To address the issue of 'many zeros', we estimate our baseline specification using non-linear models that account for the mass of zeros for firms that do not raise capital. Using absolute deal volume as dependent variable in column (2), we estimate a negative binomial regression. In column (3), we estimate a Tobit random ef-

[24]The mean number of investors per deal is 2.4. Based on columns (3) and (6), the aggregate effect (conditional on supply effects) of $0.044*2.4 = 0.106$ or $0.062*2.4 = 0.149$ per deal is similar to coefficients in Table 3, panel (a), column (2).

fects regression with log deal amount as dependent variable and report average marginal effects with robust standard errors.[25] Results show that entry into the sandbox has a positive and significant effect on total capital raised by fintechs under negative binomial and Tobit regressions as well. The effect size is similar to our baseline regressions.

Column (4) employs fixed effects for each cohort, accounting for the fact that unobservable factors could affect firms in the same cohort. Confirming our previous results (absence of pre-trends and the fact that *among* the group of firms that enter the sandbox, the exact entry date is not systematically correlated with firm characteristics), cohort fixed effects do not affect our estimated coefficient in a statistically or economically meaningful way. Columns (5) and (6) narrow the time window around the entry date into the sandbox. Column (5) restricts the sample to the eight quarters prior and 12 quarters after sandbox entry; column (6) restricts the sample to the four quarters prior and eight quarters after sandbox entry. The coefficient on sandbox entry remains statistically significant and large in magnitude, confirming the visual impression in Figure 2: the main effect of sandbox entry on capital raised materializes in the first two years upon entry into the sandbox.

The sharp increase in funding raised in the quarters immediately following sandbox entry allows us to shed further light on the role of the sandbox in reducing information asymmetries. In principle, the market could learn about firms' quality over time as this quality is gradually revealed to the public. This revelation could have happened irrespective of entry into the sandbox, leading to a steady increase in firms' ability to raise funding – this effect might be subsumed in our post dummy. Instead, if investors learn about the quality of a firm because of the sandbox certification, firms' ability to raise funding should increase immediately upon entry. To disentangle these two effects, columns (7)-(9) focus on different horizons post-sandbox entry. Column (7) reports results for Equation (1), but only includes the two quarters after sandbox entry in the sample. Column (8) instead excludes quarters one and two after entry, and column (9) excludes quarters one to four after entry. Results show that the strongest effects occur in the first two quarters upon entry; excluding the two or four quarters after entry leads

[25]These models do not allow us to include firm and time fixed effects.

to a steady decline in the coefficient size, with the coefficient turning insignificant in column (9). This pattern hence further suggests that entry into the sandbox acts as a certificate and signals firms' quality (in line with results in Section4.2). The increase in funding raised does not reflect a gradual revelation of firms' quality.

Panel (b) in Table 6 shows that our results are robust to the use of alternative matching estimators. Based on the full sample of sandbox and control firms, columns (1)-(4) use nearest neighbor (NN) matching, and columns (5)-(8) use propensity score (PS) matching instead of coarsened exact matching. Columns (1)-(2) and (5)-(6) match on firm age, firm location, and CEO gender; columns (3)-(4) and (7)-(8) additionally match on firm industry. Across specifications, we vary the number of nearest neighbors between 1 and 3. Similar to panel (b) in Table 3 for Equation (1), results show an economically and statistically significant effect of entry into the sandbox on capital raised in all specifications. Irrespective of the chosen matching method, sandbox entry leads to an increase in the amount of capital raised, relative to firms that do not enter the sandbox.

6 Conclusion

This paper establishes that firms entering the UK regulatory sandbox raise significantly more capital in the quarters after entry. Our results suggest that the sandbox reduces information asymmetries and regulatory costs. We thereby provide the first evidence that sandboxes achieve one of their key goals: to help young and innovative fintechs to raise capital. This finding suggests that sandboxes could become a crucial policy tool for harvesting the benefits of financial innovation.

Our paper contributes to the current debate on public policy to foster innovation (OECD, 2017). Policy makers face the challenge of promoting innovation in the financial sector without compromising data privacy, financial stability or consumer welfare. To meet this objective, over 50 jurisdictions are planning to or have already set up a regulatory sandbox. By fostering innovation in a controlled environment, regulators hope to learn more about new technologies and the associated risks before they reach consumers. Despite the wide-spread adoption of sandboxes, to the best of our knowledge, we provide the first rigorous analysis of their effectiveness.

Our results do not necessarily imply that sandboxes are unambiguously welfare-enhancing. Operating sandboxes often requires public funds, and helping young firms raise capital is only one objective besides others, for example increasing consumer welfare or maintaining financial stability.[26] The short time span since their inception does not allow us to evaluate the effects of regulatory sandboxes on consumer surplus or financial stability (yet). Nonetheless, we believe that we provide an important first step in evaluating the effectiveness of one of the most-widely used policy tools to foster financial innovation. Our findings can be seen as an encouragement for policymakers to scale up experimentation in sandboxes and share the lessons learned by means of regular publications and guidelines based on their experience. Information sharing could also help to mitigate the risk that sandboxes create an uneven playing field between participating and non-participating firms.

[26]Note that the FCA's sandbox is also financed by levies from regulated firms.

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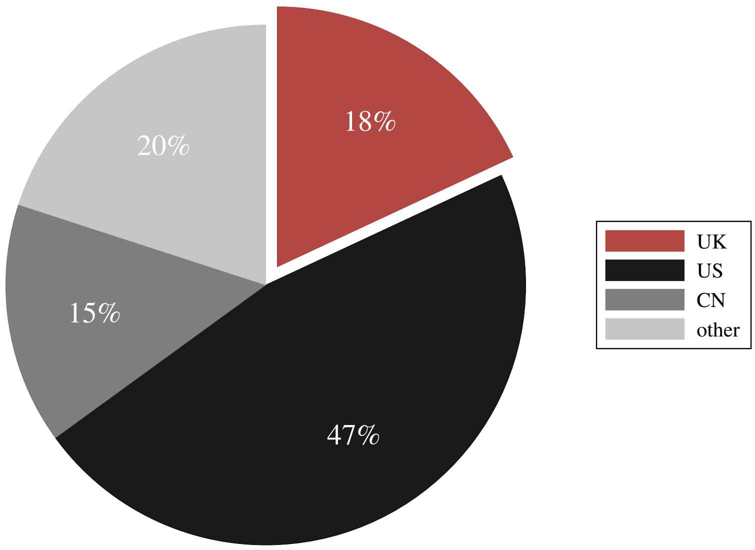
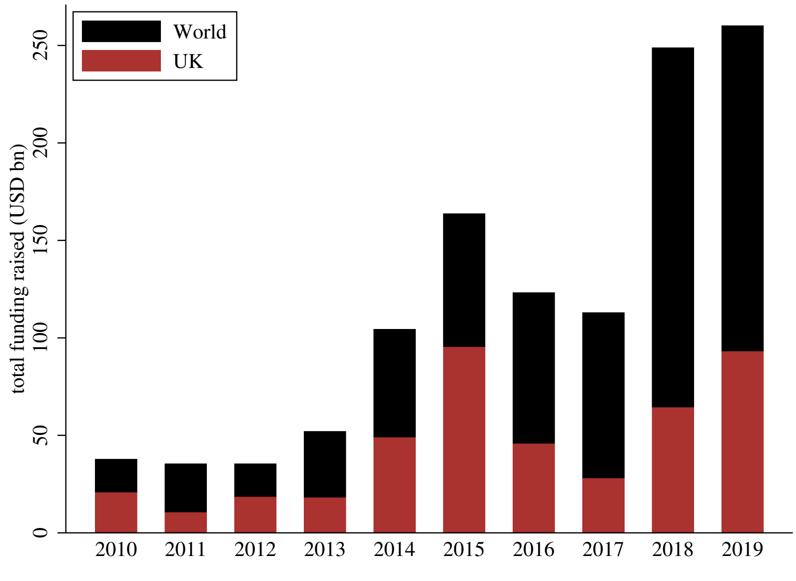
Figures and tables

Figure 1: **Total funding raised by fintech start-ups**

(a) On average (2010-2019)



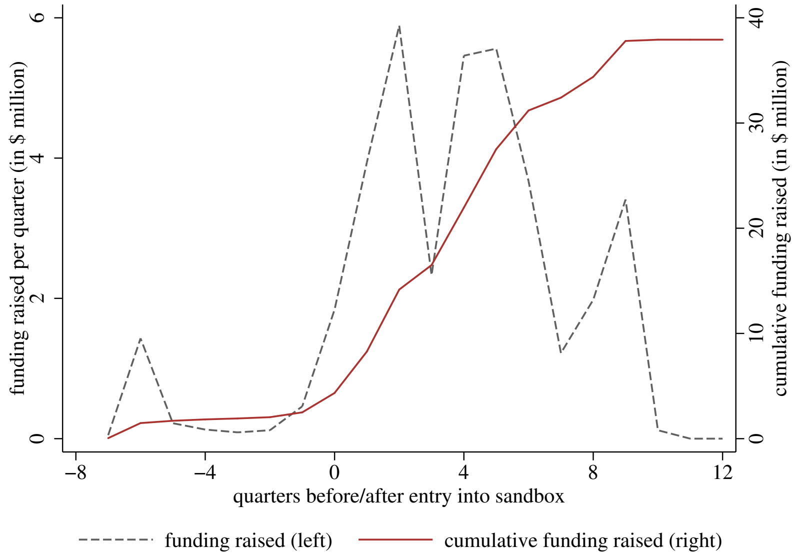
(b) Over time



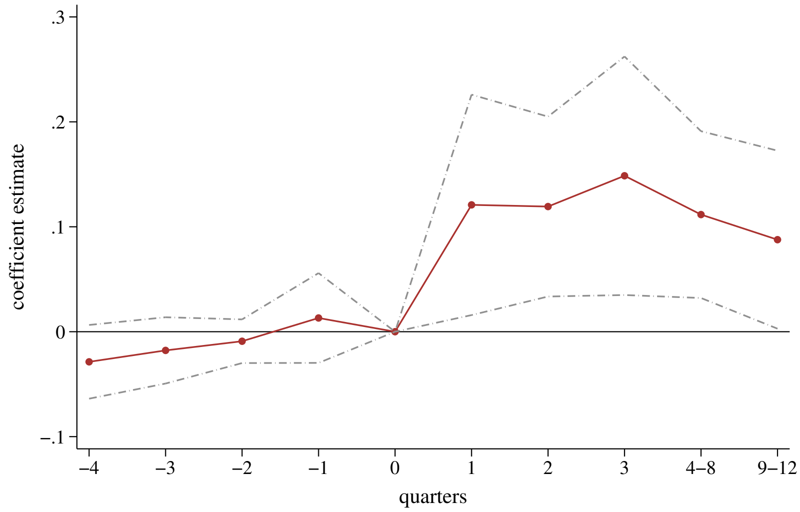
Note: Panel (a) shows the share of total funding raised by fintechs in the UK, US, China, and the rest of the world, averaged over the period 2010-2019. Panel (b) plots the total funding raised (in $bn) by fintechs in the UK in relation to worldwide funding raised by fintechs over the period 2010-2019. The sample includes completed deals and deals that have been announced/are in progress and for which PitchBook has information on deal size, deal location and deal date. Source: PitchBook Data Inc.

Figure 2: **Funding raised by sandbox firms**

(a) Deal volume around sandbox entry date



(b) Coefficient plot: pre-trends



Note: Panel (a) plots total quarterly funding raised (left axis) and cumulative funding raised (right axis, both in $mn) by our sample of sandbox-fintech firms. Negative values on the horizontal axis denote the quarters before sandbox entry, zero the quarter of entry, and positive values the quarters post-sandbox entry. Panel (b) shows coefficient estimates of β_k from Equation (2). Value zero on the horizontal axis corresponds to the date of entry, and β_k is the estimated change in deal amount t quarters before or after entry. Dashed lines represent 90% confidence intervals. Source: PitchBook Data Inc.

Table 1: **Descriptive statistics**

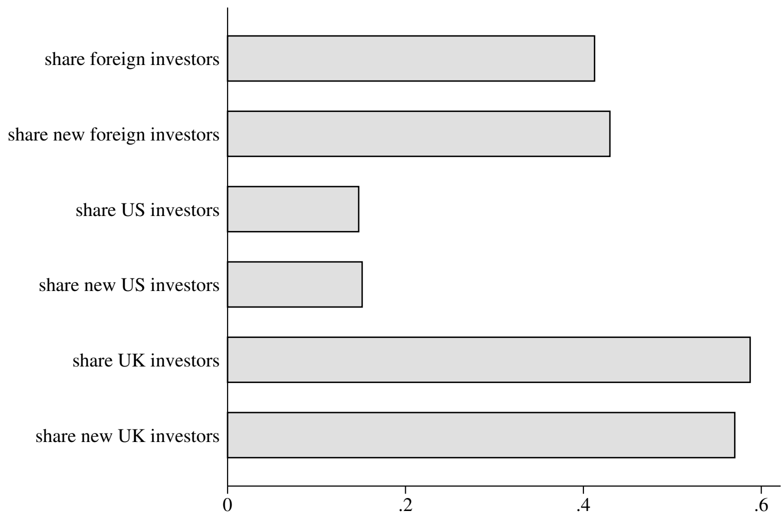
(a): Firm characteristics

Variable	Obs	Mean	Std. Dev.	Min	Max	P25	P50	P75
deal amount (USD mn)	908	4.683	27.495	0	387	0	0	0
log(deal amount)	908	.029	.154	0	1.583	0	0	0
capital raised	908	.061	.239	0	1	0	0	0

(b): Age, location, and CEOs

Variable	Obs	Mean	Std. Dev.	Min	Max	P25	P50	P75
age (as of 2019)	56	6.464	10.105	0	58	3	4	5
log(company age)	56	1.663	.7	0	4.078	1.386	1.609	1.792
firm based in London	56	.518	.504	0	1	0	1	1
CEO is male	56	.804	.401	0	1	1	1	1
CEO graduation year	18	1997.944	10.149	1982	2011	1991	1997	2007
nr. of CEOs	56	1.107	.366	1	3	1	1	1

(c): Investors



Note: Panels (a) and (b) provide summary statistics for main firm-quarter and firm-level variables. Panel (c) plots the share of total and new investors from the UK, the US, or US and other countries. Source: PitchBook Data Inc.

Table 2: **Firm characteristics and sandbox cohort**

VARIABLES	(1) age	(2) london	(3) revenue	(4) CEO male	(5) Nr CEOs	(6) firm FE
sandbox cohort 2	-3.448	0.097	0.260	0.221	0.052	0.028
	(3.872)	(0.207)	(0.202)	(0.181)	(0.170)	(0.018)
sandbox cohort 3	-4.716	-0.045	0.295	0.364**	0.034	0.015
	(3.817)	(0.243)	(0.225)	(0.152)	(0.153)	(0.019)
sandbox cohort 4	-3.424	0.055	0.079	0.164	0.042	-0.002
	(3.964)	(0.206)	(0.207)	(0.187)	(0.129)	(0.014)
sandbox cohort 5	5.784	-0.420**	-0.080	0.114	-0.091	-0.007
	(8.136)	(0.199)	(0.239)	(0.221)	(0.091)	(0.014)
Observations	56	56	56	56	56	56
R-squared	0.114	0.112	0.074	0.077	0.017	0.125

Note: This table reports results for firm-level regressions with different firm characteristics as outcome variables. As explanatory variable, we include dummies for each cohort, where *sandbox cohort 1* is the reference group. The outcome variables (from left to right) are firm age, a dummy with value one if a firm is located in London, a dummy with value one if a firm reports that it is already generating revenue, a dummy with value one if the CEO is male, and the number of CEOs. Column (6) uses the firm fixed effects, resulting from a regression of log deal amount on firm fixed effects, as dependent variable. Standard errors are robust. Source: PitchBook Data Inc. *** p<0.01, ** p<0.05, * p<0.1

Table 3: **Baseline table**

(a): Sandbox firms

VARIABLES	(1) log(deal amt)	(2) log(deal amt)	(3) log(deal amt)	(4) log(deal amt)	(5) capital raised	(6) capital raised
post SB entry	0.093*	0.137**	0.148**	0.150**	0.031*	0.031**
	(0.054)	(0.056)	(0.064)	(0.070)	(0.017)	(0.014)
Observations	908	908	908	855	908	616
R-squared	0.016	0.076	0.093	0.157		
Firm FE	-	✓	✓	✓	-	✓
Time FE	-	-	✓	-	-	✓
Industry*Time FE	-	-	-	✓	-	-
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

(b): Matched control firms

VARIABLES	(1) log(deal amt)	(2) log(deal amt)	(3) log(deal amt)	(4) log(deal amt)	(5) capital raised	(6) capital raised
post SB entry	0.107**	0.119**	0.124**	0.151**	0.025***	0.033***
	(0.053)	(0.055)	(0.056)	(0.063)	(0.009)	(0.008)
Observations	3,820	3,819	3,819	3,779	3,820	2,007
R-squared	0.026	0.087	0.093	0.133		
Firm FE	-	✓	✓	✓	-	✓
Time FE	-	-	✓	-	-	✓
Industry*Time FE	-	-	-	✓	-	-
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

Note: This table presents results from firm-quarter level regression Equation (1). Panel (a) uses the sample of firms that entered the sandbox at some point during our sample. Panel (b) uses the sample of sandbox firms and the sample of matched control firms selected via coarsened exact matching. The dependent variable is either the logarithm of 1 plus the total deal amount for firm f in quarter t in columns (1)-(4); or the dummy *capital raised* that takes value 1 if the firm raises a positive amount of capital in a given quarter in columns (5)-(6). *post SB entry* is a dummy with value one after sandbox entry, and zero for all quarters prior to entry. All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry.* Standard errors are clustered at the firm level. Columns (5)-(6) report average marginal effects from logistic regressions with robust standard errors. Source: PitchBook Data Inc. *** p<0.01, ** p<0.05, * p<0.1

Table 4: **Evidence on the mechanism**

(a): Information asymmetry and CEO background

VARIABLES	(1) log(deal amt)	(2) log(deal amt)	(3) log(deal amt)	(4) log(deal amt)	(5) log(deal amt)	(6) log(deal amt)
post SB entry	0.106**	0.109**	0.229**	0.101*	0.193***	0.215***
	(0.043)	(0.046)	(0.092)	(0.054)	(0.072)	(0.075)
post SB entry × old firm	-0.072**	-0.064*				
	(0.031)	(0.034)				
post SB entry × large firm			-1.832***	-0.906***		
			(0.582)	(0.054)		
post SB entry × law degree					-0.103*	-0.121*
					(0.058)	(0.065)
Observations	908	855	995	931	908	855
R-squared	0.098	0.161	0.239	0.383	0.095	0.160
Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	-	✓	-	✓	-
Industry*Time FE	-	✓	-	✓	-	✓
Cluster	Firm	Firm	Firm	Firm	Firm	Firm

(b): New investors

VARIABLES	(1) Nr inv	(2) Nr inv	(3) % UK inv	(4) % US inv	(5) % nonUK inv
post SB entry	0.309**	0.629*	0.003	0.201**	0.163
	(0.121)	(0.327)	(0.084)	(0.078)	(0.136)
Observations	800	769	769	769	769
R-squared	0.005	0.143	0.207	0.129	0.117
Firm FE	-	✓	✓	✓	✓
Industry*Time FE	-	✓	✓	✓	✓
Cluster	Firm	Firm	Firm	Firm	Firm

Note: Panel (a) presents results from firm-quarter level regression Equation (1), based on the sample of firms that entered the sandbox at some point during our sample. The dependent variable is the logarithm of 1 plus the total deal amount for firm f in quarter t. *post SB entry* is a dummy with value one after sandbox entry, and zero for all quarters prior to entry. All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry*. *old firm* is a dummy with value one for firms above the median in terms of firm age; *large firm* is a dummy with value one for firms associated with large listed firms; and *law degree* is a dummy with value one for firms that have a CEO with a law degree. Standard errors are clustered at the firm level. Panel (b) presents results from firm-quarter level regression Equation (1), based on the sample of firms that entered the sandbox at some point during our sample. The dependent variable is the log number of new investors in columns (1)-(2), the share of UK-based investors in column (3), the share of US-based investors in column (4), and the share of non-UK-based investors in column (5). All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry*. Standard errors are clustered at the firm level. Source: PitchBook Data Inc. *** p<0.01, ** p<0.05, * p<0.1

Table 5: **Disentangling demand and supply effects**

VARIABLES	(1) pro rata log(deal amt)	(2) pro rata log(deal amt)	(3) pro rata log(deal amt)	(4) by size log(deal amt)	(5) by size log(deal amt)	(6) by size log(deal amt)	(7) capital raised	(8) capital raised	(9) capital raised
post SB entry	0.018**	0.027**	0.044**	0.024**	0.040***	0.062**	0.104***	0.104*	0.161*
	(0.009)	(0.011)	(0.020)	(0.011)	(0.014)	(0.027)	(0.032)	(0.061)	(0.093)
Observations	41,745	41,745	41,717	41,672	41,672	41,633	41,836	41,836	41,836
R-squared	0.000	0.031	0.267	0.000	0.033	0.236	0.000	0.024	0.263
Investor*Firm FE	-	✓	✓	-	✓	✓	-	✓	✓
Time FE	-	✓	-	-	✓	-	-	✓	-
Investor*Time FE	-	-	✓	-	-	✓	-	-	✓
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Note: This table presents results from firm-investor-quarter level regression Equation (3), based on the sample of firms that entered the sandbox at some point during our sample. The dependent variable is the logarithm of 1 plus total capital invested by each investor. Columns (1)-(3) use a pro-rata split, columns (4)-(6) split loan volume by aggregate investors size. Column (7)-(9) use dummy *capital raised* as dependent variable that takes value one if a given investor invests in a given firm in quarter *t* (the extensive margin). *post SB entry* is a dummy with value one after a firm entered the sandbox, and zero for all quarters prior to entry. All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry*. Standard errors are clustered at the firm level. Source: PitchBook Data Inc. *** p<0.01, ** p<0.05, * p<0.1

Table 6: **Further robustness tests**

(a): Alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	VC	Neg Bin	Tobit	Cohort FE	-8 to +12	-4 to +8	q1-2	q3+	q5+
VARIABLES	log(deal amt)	deal amt	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)
post SB entry	0.077	0.113**	0.149*	0.148**	0.138**	0.188**	0.161**	0.136*	0.043
	(0.052)	(0.053)	(0.088)	(0.064)	(0.066)	(0.081)	0.083	0.075	0.064
venture capital	0.178***								
	(0.054)								
post SB entry × venture capital	0.191**								
	(0.094)								
Observations	908	908	908	908	762	591	643	812	716
R-squared	0.390			0.093	0.105	0.127	0.106	0.110	0.108
Firm FE	✓	-	-	✓	✓	✓	✓	✓	. ✓
Time FE	✓	-	-	✓	✓	✓	✓	✓	✓
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

(b): Nearest neighbor and propensity score matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	NN	NN	NN	NN	PS	PS	PS	PS
	nn(1)	nn(3)	nn(1)	nn(3)	nn(1)	nn(3)	nn(1)	nn(3)
VARIABLES	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)
post SB entry	0.027***	0.027***	0.030**	0.030**	0.042***	0.064*	0.053***	0.040**
	(0.008)	(0.008)	(0.015)	(0.015)	(0.011)	(0.021)	(0.011)	(0.016)
Observations	3,820	3,820	2,132	2,132	3,820	3,820	2,839	2,839
age	✓	✓	✓	✓	✓	✓	✓	✓
london	✓	✓	✓	✓	✓	✓	✓	✓
CEO gender	✓	✓	✓	✓	✓	✓	✓	✓
industry	-	-	✓	✓	-	-	✓	✓

Note: This Table presents results from firm-quarter level Equation (1). In panel (a), the dependent variable is either the logarithm of 1 plus the total deal amount for firm f in quarter t in columns (1) and (5)-(9); or total deal amount in columns (3) and (4). Panel (a), column (1) uses dummy *VC* that takes on value on if a deal is classified as venture capital deal, and zero otherwise. Columns (2) and (3) estimate negative binomial and Tobit regressions and report average marginal effects with robust standard errors. Column (4) employs fixed effects at the cohort level, columns (5)-(9) narrow the time window around the entry date into the sandbox. Panel (b) uses the sample of sandbox firms and the sample of control firms and uses nearest neighbor and propensity score matching, based on one or three nearest neighbors. The dependent variable is the logarithm of 1 plus the total deal amount for firm f in quarter t. *post SB entry* is a dummy with value one after sandbox entry, and zero for all quarters prior to entry. All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry*. Standard errors are clustered at the firm level. Source: PitchBook Data Inc. *** p<0.01, ** p<0.05, * p<0.1

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