Regulatory Sandboxes and Fintech Funding: Evidence from the UK

G. Cornelli S. Doerr L. Gambacorta O. Merrouche

October 2021

Abstract

Over 50 countries have introduced regulatory sandboxes to foster financial innovation. This paper conducts the first evaluation of their ability to improve fintechs' access to capital. Exploiting the staggered introduction of the UK sandbox, we compare firms in earlier to those in later cohorts. Firms entering the sandbox see an increase of 15% in capital raised post-entry; their probability of raising capital increases by 50%. We present evidence that the sandbox facilitates access to capital by reducing asymmetric information and regulatory costs. Controlling for time-varying confounding factors at the firm- or investor-level through fixed effects does not affect our findings.

JEL Codes: G24, G38, M13, O38.
Keywords: regulatory sandbox, fintech, start-ups, venture capital, innovation.

Giulio Cornelli (giulio.cornelli@bis.org), Sebastian Doerr (sebastian.doerr@bis.org) and Leonardo Gambacorta (corresponding author, leonardo.gambacorta@bis.org, +41 280 9164) are at the Bank for International Settlements, Monetary and Economic Department, Centralbahnplatz 2, CH-4002 Basel, Switzerland. Ouarda Merrouche (ouarda.merrouche@alumni.eui.eu) is at Université Paris Nanterre and EconomiX CNRS UMR 7235, Batiment G, 200, Avenue de la République, 92001 Nanterre cedex, France.
We would like to thank Matteo Aquilina, Raphael Auer, Matthias Bauer, Laura Blattner, Marcel Bluhm, Fabrizio Core, Nick Clark, Juanita González-Uribe, Ralph De Haas, Jon Frost, Luigi Guiso, Sebastian Hillenbrand, Beata Javorcik, Ivo Jenik, Michelle Lowry, Daphnée Papiasse, and Manos Schizas, as well as participants at the European Finance Association Annual Meeting, Swiss Winter Conference on Financial Intermediation, FINEST Autumn Workshop, SSES Annual Conference, the BIS research seminar, the European Bank for Reconstruction and Development research seminar, and meetings of the Inter-American Development Bank, the Asian Development Bank, the Arab Regional Fintech Working Group, and the BIS Innovation Hub. The views expressed here are those of the authors only, and not necessarily those of the Bank for International Settlements.

1 Introduction

The rapid growth of innovative companies that use new technology (fintechs) holds the promise of spurring competition, leading to sizeable efficiency gains and more choice for consumers. However, fintechs offer novel products in an environment of high regulatory uncertainty, so they often struggle to raise enough capital to develop products and expand (Haddad and Hornuf, 2019). Policymakers around the world are thus stepping up their efforts to foster innovation in the financial sector, as they have done with business accelerators or grants in other sectors (Howell, 2017; González-Uribe and Leatherbee, 2018).

A landmark initiative to nurture fintechs was the creation of the "regulatory sandbox" by the United Kingdom's Financial Conduct Authority (FCA). Established in November 2015, the sandbox offers fintechs a controlled testing environment in which they can try out their products on a limited set of customers. Testing occurs under close regulatory supervision: firms receive advice to help them navigate the complexities of regulations and to ease the route to authorisation. Regulators 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. By now, over 50 countries have followed the UK and introduced their own regulatory sandbox, often with the goal of nurturing the fintech sector (Wechsler et al., 2018).[1] And yet, despite their wide-spread adoption and significant attention in the media and policy circles, little systematic empirical evidence exists on whether sandboxes actually help fintechs raise funding.[2] Nor is there any evidence on the underlying channels that could be at work.

This paper analyses how entering the FCA's regulatory sandbox affects fintechs' abil-

[1] As of mid-2021, 57 jurisdictions operate one or more sandboxes, see the World Bank's Key Data from Regulatory Sandboxes across the Globe for current numbers.

[2] Anecdotal evidence suggests that a sandbox can help young firms attract investment. See Financial Times (2018), "A 'fintech sandbox' might sound like a harmless idea. It's not", December.

ity to raise funding. We collect unique data on capital raised by fintechs in the UK sandbox for the period from 2014q1 to 2019q2. Detailed deal-level data on funding raised, broken down by individual investor, as well as background information on firm age, size, industry, location, and its CEOs allows us to investigate different channels through which the sandbox could affect firms' access to capital. For identification, we focus on the sample of firms accepted into the sandbox and exploit the fact that these firms entered the sandbox in five 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 later. We show that *within* the group of firms entering the sandbox the entry date is uncorrelated with observable firm characteristics prior to entry and orthogonal to observable and unobservable time-varying firm characteristics.[3]

Our main finding is that entry into the sandbox is associated with an increase in the average amount of funding raised and a higher probability of raising funding. In firm-level regressions, we find that entry into the sandbox is followed by a 15% increase in capital raised (or $700,000) over the following two years, relative to firms that will enter the sandbox at a later date. Firms' probability of raising capital increases by 50%. The increase in capital raised corresponds to about one standard deviation.[4] There are no differential pre-trends across firms, suggesting that firms entering the sandbox in earlier cohorts do not behave systematically different from later entrants prior to entry.[5]

We then investigate the underlying mechanisms. Asymmetric information is particularly acute in venture capital markets, because issuers are mainly early-stage firms with no prior track records (Trester, 1998). Uncertainty about the quality of new products

[3]Specifically, we control for time-varying observable and unobservable firm characteristics through granular firm*time fixed effects. We also show that our results are robust to the inclusion of time-varying fixed effects at the investor level.

[4]The magnitude of our estimates is in line with findings on the effectiveness of public policy to foster innovation in other settings. Howell (2017) finds that R&D grants roughly double a firm's chance of receiving venture capital investment. For the UK, Bone et al. (2019) find that accelerators increase firms' fundraising activity by around 75%.

[5]Note that the introduction of the FCA sandbox was not accompanied by stricter regulation for non-sandbox firms that could have discouraged them from seeking financing and thereby have led to a relative increase in capital raised by sandbox firms.

and services offered by fintechs could thus present a serious obstacle to raising capital – especially in an environment of regulatory uncertainty (Haddad and Hornuf, 2019). Navigating the complexities of a constantly changing regulatory framework could further pose a significant cost to firms. Sandboxes could curb informational frictions through regulatory oversight and continuous dialogue between firms and the regulator during the testing period that offers reassurance to investors that firms meet their regulatory obligations. Additionally, 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 framework.

Our evidence is consistent with the notion that the sandbox reduces information asymmetries between investors and firms, as well as regulatory costs. We first show that the positive effect of sandbox entry on capital raised is stronger for smaller and younger firms, which are more opaque and hence subject to severe informational frictions (Hall and Lerner, 2010). Similarly, entry into the sandbox increases deal volume by more for venture capital deals, which are generally more information-sensitive, compared to other types of deals (Gompers, 1995; Howell, 2020). Second, we exploit granular data at the investor-firm level and show that firms in the sandbox raise more capital especially from investors based outside the UK and investors that have not previously invested into the firm. Since these investors likely face higher information asymmetries (Grinblatt and Keloharju, 2001; Ivković and Weisbenner, 2005), we interpret this finding as further evidence that the sandbox reduces informational frictions. Finally, we show that firms with a CEO who has a 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 the most from the guidance provided by case officers (Deloitte, 2019), and thereby from the reduction in regulatory costs.

These findings do not exclude the possibility that the sandbox has a general signalling effect: selection into the sandbox could serve as a stamp of approval and help all sandbox firms raise more capital. Regulatory approval could further indicate that a firm's product

is viable and will face fewer regulatory hurdles going forward. That said, our findings on the differential effects for small and young firms, and especially the larger effects found for capital raised from foreign and first-time investors, are consistent with the sandbox reducing informational frictions and facilitating fintechs' access to capital above and beyond a general signalling effect.

A key challenge for identification is that even among the group of firms that enter the sandbox at some point, the entry date could be correlated with firm characteristics. To address this point, we first show that among the group of firms that enter the sandbox at some point, the specific entry date is uncorrelated with observable firm characteristics such as age, revenue generation, or location. Second, we show that our firm-level results are robust to the inclusion of fixed effects. Specifically, including firm or industry*time fixed effects does not affect the size or significance of our coefficients in a substantive way, despite increasing the R^2 substantially. This mitigates concerns that our results are explained by omitted variables or selection effects (Altonji et al., 2005; Oster, 2019).

And yet, the firm-level analysis cannot fully rule out that unobservable time-varying factors at the investor or firm level might confound our estimates. On the one hand, investors could increase or decrease their supply of capital as firms enter the sandbox for reasons unrelated to the sandbox process itself. For example, a change in the tax regime that reduces capital taxes on investments in fintechs could relax investors' constraints. On the other hand, even within the group of firms that enter the sandbox, the exact entry date could be correlated with, for example, a change in the quality of the offered product or service. Firms could then have raised more capital irrespective of their entry into the sandbox.

The rich variation in our matched investor-firm level data allows us to account for these confounding factors through granular fixed effects. First, to control for unobservable firm-investor factors, as well as investor characteristics that vary over time, we include investor*firm and investor*time fixed effects in our regressions. The combination of both fixed effects allows shocks to affect each firm-investor combination heterogeneously and

accounts for any change in observable and unobservable investor characteristics (Jiménez et al., 2014). For example, time-varying fixed effects at the investor level control for changes in investor wealth, income, or tax exception schemes. Further, when we investigate the differential effect of entry into the sandbox on capital raised from foreign or first-time investors, we control for any observable and unobservable time-varying factors at the firm level through firm*time fixed effects (Khwaja and Mian, 2008). In essence, we compare the same firm raising capital from the same investor at different dates of entry into the sandbox.

Our estimated coefficients increase in magnitude when we control for unobservable investor characteristics through investor*time fixed effects. This finding implies that the positive effects from sandbox approval are firm-specific and that sandbox entry coincides with a higher supply of capital by investors in general. Were our results solely due to the higher general supply of capital by investors to sandbox firms, but independent of their entry into the sandbox, we would find no effect of sandbox entry on capital raised *after* accounting for investor-specific changes. The increase in coefficient size upon controlling for time-varying investor characteristics further suggests that the estimated coefficients in firm-level regressions likely represent a lower bound of the true effect.

Including firm*time fixed effects to control for time-varying firm characteristics leads to no material change in the estimated coefficients, while the R^2 increases by over 30 percentage points. The stability of the coefficient suggests that *within* the group of sandbox firms, the exact entry date into the sandbox is orthogonal to observable and unobservable time-varying firm characteristics (Oster, 2019).

The robustness of our results to the inclusion to firm*time fixed effects also mitigates the concern that firms might strategically postpone their capital raising activity until they enter the sandbox, possibly in the hope of raising additional capital. When we look at the differential effect of capital raised from foreign or first-time investors, firm*time fixed effects would absorb any such firm-specific shock. These heterogeneous effects at the firm-investor level are hence difficult to reconcile with the argument that firms strategically

postpone their capital raising activity.[6]

We provide a series of additional tests. We rule out that investors simply learn about firms as they gradually reveal their quality to the market over time, irrespective of entry into the sandbox. Then, firms' ability to raise funding would increase gradually. If instead investors learn about the quality of a firm because of the "sandbox certification", firms' ability to raise funding will increase immediately after 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 and insignificant effect on funding raised.

We also show that our results are robust to alternative estimation methods to account for the presence of zeros in our dependent variable, for example negative binomial regressions, or when we include additional fixed effects, such as cohort or contiguous-cohort fixed effects. Finally, we show that results are similar when 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 fintech firms that are statistically similar in terms of observable 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.

Regulatory sandboxes pursue additional goals besides facilitating fintechs' access to finance. For example, they aim at promoting innovation and competition, or increasing the consumer surplus. The short time span since their inception does not allow for an evaluation of their effects on the consumer surplus or financial stability yet. Nor is there systematic data available on firms' revenues, cash flow, or customer base.[7] Preliminary

[6]Note that the possibility that firms time their capital raising activity would not speak against our argument: if firms expect that they will be able to raise more capital after entry into the sandbox than they would have been able to raise beforehand, this would still imply that firms expect the sandbox to help them raise capital – through its signalling effect and/or reduction in asymmetric information. In other words, firms could engage in postponing their capital raising activity precisely because they expect that the sandbox will reduce screening and monitoring costs of investors and thereby make capital raising easier.

[7]We could find only a handful of sandbox firms that already provide detailed balance sheet data in

evidence, however, suggests that survival rates among sandbox firms are significantly higher than for the average startup.[8] That said, to the best of our knowledge, this paper provides the first systematic evidence that sandboxes help fintechs to raise capital – and hence achieve one of their explicit goals. Our results suggest that sandboxes, which have already been widely adopted, could become a useful policy tool for harvesting the benefits of financial innovation.[9]

Our paper contributes to the current debate on public policies to foster innovation (Kerr and Nanda, 2015; Lerner and Nanda, 2020). A recent literature has established that fintechs face serious obstacles to raising capital (Block et al., 2018; Haddad and Hornuf, 2019), despite the fact that their innovation provides value to innovators and investors (Chen et al., 2019). As market failures can lead to sub-optimal private-sector expenditure on research and development, public policies to foster innovation, for example through grants and business incubators or accelerators, can have sizeable benefits (Howell, 2017; González-Uribe and Leatherbee, 2018; Yu, 2020; González-Uribe and Reyes, 2021).[10] Policy makers hence want to promote innovation in the financial sector, and regulatory sandboxes have emerged as the most prominent tool to do so. Yet, evidence on their effectiveness is scarce.

We also relate to literature that investigates how to regulate fintechs (Arner et al., 2017; Zetzsche et al., 2017; Bank for International Settlements, 2019).[11] Buchak et al. (2018) show that the rapid growth of fintech lenders in the US is partly explained by lighter regulation (see also Thakor (2020)). Other studies show that the use of big data and machine learning can lead to algorithmic discrimination and changes in consumer

e.g. 'Companies House' or 'Capital IQ'. That said, a reoccurring feature of firms operating in the digital economy is that they aim to first achieve scale and capture market share, even if they are loss-making, before making significant profits, so that revenues or profits might be uninformative for young fintechs.

[8]Specifically, the FCA (2019) reports that around 80% of firms that successfully tested in the sandbox are still in operation (as of 2019), which is significantly higher than average numbers for startups. For example, the three-year survival rate of startups averages around 60% (Hyytinen et al., 2015).

[9]Cross-country evidence suggests the establishment of a sandbox is followed by a surge in fintechs' capital raising activity (Cornelli et al., 2021). These correlations are in line with our main findings.

[10]Brown and Davies (2020) show that early-venture fundraising can be inefficient if information is costly, leading entrepreneurs to undertake bad projects and forgo profitable ones.

[11]See also a survey by the World Bank and Cambridge Center for Alternative Finance (2019) on regulating alternative finance.

behaviour (Bartlett et al., 2019; Berg et al., 2020; Fuster et al., 2021). The entry of fintechs into finance thus constitutes a dilemma for policy makers: they need to promote innovation in the financial sector, but without compromising data privacy, financial stability or consumer welfare (Brummer and Yadav, 2019). New regulatory tools might thus be needed, and sandboxes could be one such tool: they provide regulators with the ability to support safe innovation by gauging the potential welfare implications of new products 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. It then reports the main results and provides evidence on the mechanisms at work. In Section 5 we present robustness tests. Section 6 concludes.

2 The UK regulatory sandbox

The UK, and especially London, has become a global fintech hub. Over the last decade, fintech start-ups raised around one-fifth of their worldwide funding in the UK (see Figure 1, panel (a)). This number is topped only by the US, where fintechs raise almost half of global funding. Notably, the UK increased its share from less than 15% in 2010-2014 to over 20% for the 2015-2019 period (see panel (b)). It thereby overtook China in terms of volume. However, despite these encouraging numbers, fintechs still face severe obstacles in raising capital (HM Treasury 2016; 2019).[12] Since access to capital is crucial for young firms with high growth potential, its scarcity threatens to slow innovation in the financial sector.

Partly in response to these worries, the Financial Conduct Authority announced the

[12]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), panel (b).

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. Between November 2016 and July 2019 (the end of our sample period) 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.[13] In total, 375 firms have applied and 118 have been accepted into the FCA sandbox. Each cohort averages around 25 firms. Crucially for our setting, the introduction of the sandbox was not accompanied by stricter regulation for non-sandbox firms that could have discouraged them from seeking financing, or mechanically benefited sandbox firms. Nor does the FCA provide insurance, in the sense that customers of sandbox firms are protected from any risks arising from using their products and services.

The FCA publishes the names of accepted firms for each round of the sandbox; it does not make the list of rejected firms available. The average firm in the sandbox are start-ups and small and medium enterprises in retail banking (including payments), wholesale markets, retail and wholesale investment and lending, and insurance propositions. Sandbox firms offer a wide array of new products and services. For example, firms offer a platform that facilitates the securitization of SME debt by digitising credit applications; an interest-free salary-advance and cash flow management product utilising distributed ledger technology; or an aggregation platform that facilitates investment in a diversified portfolio of P2P loans.

The 'sandbox process' contains four distinct steps: application, selection, testing and exit. The FCA selects firms out of the pool of applicants based on whether the firm offers a genuine innovation that benefits UK consumers. The innovation should constitute an improvement over existing products and services, and hence promote competition.[14] The age, size or profitability of a company is not an eligibility criteria, start-ups and incumbent

[13]We restrict our analysis to cohorts 1–5, as from cohort 6 onward, the FCA specified certain areas of innovation. For example, cohort 6 emphasized the topic of "make finance work for everyone and support the UK in the move to a greener economy", which could lead to selection effects.

[14]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.

are equally encouraged to apply.[15] Firms that fulfil these requirements and for which the FCA's input is deemed useful ('need for support criteria') are selected into the sandbox. Firms are assigned a dedicated case officer who helps to design the test setup, provides guidance to complete the necessary paperwork for authorisation, and helps firms navigate the regulatory environment throughout the process.[16]

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 can apply for a permanent authorisation upon completion.

One of the main objectives of the sandbox is to attract investments toward fintechs by reducing informational frictions (FCA 2015). Asymmetric information is particularly acute in venture capital markets, because issuers are mainly early-stage firms with no prior track records (Trester, 1998; Howell, 2020). 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).

Sandboxes could significantly curb informational frictions through regulatory oversight and continuous dialogue between firms and the regulator during the testing period that 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

[15]Firms that offer the following services are not eligible: deposit taking, insurance underwriting, and multilateral trading facilities.

[16]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, possibly reducing the expenditures on external regulatory consultants and helping firms better understand how they fit in the current regulatory framework.

firms that offer genuine innovation with clear benefits to consumers. Moral hazard may also be reduced if close supervision by the FCA spurs firms to improve their governance and adopt more rigorous policies and processes.

Besides information problems, another critical obstacle to capital access relates to regulatory costs and uncertainty. 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 might be forced to alter their business model to comply with continuously evolving regulations. According to the World Bank's *Doing Business* report, regulatory restraints are a key barrier to innovation.[17] The sandbox could reduce 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 going forward.

Anecdotal evidence suggests that the sandbox has delivered value to firms (Deloitte, 2019). For example, companies value the guidance on the application of regulation to innovative products and services, and highlight the benefits of detailing the risks relating to their business mode to the regulator. They further report that operating their technology successfully in a live and regulated environment helps them to signal credibility to investors. Firms further state that the route to authorisation is significantly simpler and faster when regulation is considered from the start and with the help of case officers (FCA 2019).

Enhancing firms' access to capital is an explicit goal of sandboxes. A complementary long term policy objective is to increase consumer welfare, for example by promoting competition and innovation while ensuring financial stability. The short time span since their inception does not yet allow us to evaluate these long term effects, since they will only

[17] In a 2020 survey by software provider Finastra, 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.

materialize over the coming years and pose significant measurement challenges. Instead, this paper empirically tests whether sandboxes help young and innovative fintechs to raise capital – a first step in assessing their usefulness.[18]

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 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. Further, each deal contains information on the individual investors and their location. 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, respectively.

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 a deal, as well as the size of each investor's total portfolio. We also observe the

[18]Views on the effectiveness and purposes of sandboxes diverge (Allen, 2019). For example there are worries that they may play the role of a gatekeeper and thereby slow down or even halt innovation (Peirce, 2018). Yet, preliminary evidence suggests that survival rates among sandbox firms are significantly higher than for average startups (FCA 2019). In systematically evaluating the effectiveness of sandboxes in helping fintechs raise capital, we hope to contribute to the debate on their costs and benefits.

country where the investor is headquartered, which allows us to separately investigate the behaviour of foreign and UK-based investors. PitchBook also provides information on whether an investor is a new/first-time investor in the issuing firm.

Sandbox firms The analysis focuses on firms that enter the sandbox in one of the five cohorts during the 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, we drop deals that report no time of deal (36 deals), since it is not possible to determine whether they took place before or after sandbox entry, and drop observations with missing or zero deal size (83 deals). Finally, firms must 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, the baseline specification excludes 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. To create a balanced panel, 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 firm-level 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 a value of zero before sandbox entry, and a value of one upon entry and thereafter. As firm-level controls, we use the log of (1+company age), dummy *male*, which takes on a value of 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 a value of 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.

To construct the investor-firm level panel, we collect data on all investors that take part in a given deal and then split deal volume across investors. 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 the 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 investor '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. We also define the dummy *capital raised* that takes on a value of one if a given investor invests in a given firm in quarter t (the extensive margin), and zero otherwise. This approach is insensitive to the chosen method of allocation.

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.

Control firms To contrast the performance of sandbox firms with that of similar firms that never enter the sandbox, we collect PitchBook data on a random sample of over 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). The final sample consists of 54 sandbox firms matched to 158 control firms. Out of our 908 sandbox observations, 24 observations (two firms) result in no match. The total sample of treated and matched control firms contains 3,820 firm-quarter observations and is balanced in terms of observable firm characteristics. Note that while the FCA does not make public the list of rejected firms, they could be part of our control sample.

4 Empirical strategy and results

This section explains our empirical strategy and presents the main results. We first show that sandbox entry is associated with an increase in capital raised. Investigating the mechanism, we then show that effects are stronger for younger and smaller firms, as well as for foreign or first-time investors, and firms with a CEO without prior experience in financial regulation – suggesting that the sandbox reduces informational frictions and regulatory costs. These findings are robust to the inclusion of granular time-varying fixed effects.

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 exploit the staggered design of the sandbox: firms enter in different cohorts. The identifying assumption is hence that among the group of firms that join the sandbox during the sample period, a firm's observable and unobservable characteristics are not systematically correlated with its entry date. To shed light on 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.

While selection into the sandbox is not random – after all, the FCA aims to accept firms that offer an innovative product – Table 2 suggests that *within* the group of firms that enter the sandbox, the exact entry date is not systematically correlated with observable firm characteristics. These facts mitigate concerns that our results are explained by omitted variables or selection effects. And yet, the firm-level analysis cannot fully rule out that unobservable time-varying factors at the investor or firm level confound our estimates. To this end, we estimate investor-firm level regressions in Section 4.2 to

show that controlling for observable and unobservable time-varying firm characteristics through granular fixed effects does not affect our estimates.

To analyse how entry into the sandbox affects firms ability to raise capital, 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 a value of one if the firm raises capital in a given quarter. The dummy *post SB entry* takes a 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 control for unobservable time-varying shocks common to all firms within an industry, we occasionally include industry*time fixed effects instead of time fixed effects.

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 (Stable Unit Treatment Value Assumption,

SUTVA). We provide direct evidence on parallel trends below. With regard to SUTVA, 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. In regressions at the investor-firm level in Section 4.2 we further show that including time-varying effects at the investor level strengthen our main finding. As we explain below, this suggests that the sandbox does not lead to a reallocation of existing funds from non-sandbox to sandbox firms, but rather increases the overall amount of available funding.

Before moving to the regression analysis, panel (a) in Figure 2 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. The vertical axis 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 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.[19]

We now investigate this pattern in greater detail. Table 3 reports the results of Equation (1) over our sample period. In columns (1) to (4), we estimate the effect of the sandbox at the intensive margin for the treated sample and use the 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

[19]Some firms 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 $t = 0$.

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 time-varying unobservables at the industry level (Altonji, Elder and Taber, 2005; Oster, 2019). This pattern is in line with results in the balancedness test in Table 2.

In columns (5) and (6) we explore the effect of the sandbox at the extensive margin. The outcome variable is the 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 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 Table 3 suggest that entry into the sandbox is associated with an economically and statistically significant effect on firms' ability to raise capital.[20]

Firms entering the sandbox in earlier cohorts could behave systematically different from later entrants prior to entry. To investigate whether there were any potential pre-trends across groups, we include a set of dummy variables in the 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

[20]Did the sandbox increase aggregate investment? We find that the aggregate amount of capital invested into UK fintechs increases in the quarters after a new cohort enters into the sandbox. This suggests that without the sandbox, investors would have invested less on aggregate because frictions would have been more severe. In other words, the sandbox increases the aggregate amount of available capital, rather than simply reallocating a fixed pool of funds.

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 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.

4.2 Information asymmetries and regulatory costs

Having established that sandbox entry is associated with improved access to financing, we now examine the potential channels through which the sandbox could affect firms' access to capital. A regulatory sandbox could reduce asymmetric information and the associated information collection costs: 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. Further, a dedicated case officer that helps to navigate the legal environment could reduce firms' costs to understand and comply with the regulatory framework, thereby reducing regulatory costs.

Information asymmetries in the form of adverse selection and moral hazard are particularly acute for young and small firms (Petersen and Rajan, 1994; Czarnitzki and Hottenrott, 2011), firms that often have no prior track records and are informationally opaque. Identifying viable firms is even more difficult in an uncertain and dynamic environment (Gompers, 1995; Bolton and Freixas, 2000). Fintechs offer novel products and services in an environment of high uncertainty, so we expect informational frictions to be acute – especially among younger and smaller entrants.

Further, a large literature shows that a closer relationship or shorter physical distance between investors and firms reduces informational frictions (Grinblatt and Keloharju, 2001; Degryse and Ongena, 2005).[21] Informational asymmetries are thus expected to be greater for first-time investors (i.e., investors that have not invested into a firm previously), and for investors that are based outside of the UK, since they have an informational disadvantage when investing into UK firms. To investigate these hypotheses, in what follows we perform analyses at the firm and investor-firm level.

Firm-level analysis Columns (1)–(4) in Table 4 investigate 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 a value of one if a firm's age is above the median (if it is at least four years old).[22] 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 a value of one if a firm is affiliated with listed companies, i.e., the firms we initially excluded from our baseline sample. Across specifications, 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.[23]

To shed further light on the role of informational frictions, we compare venture capital deals to other types of deals in columns (5)–(6). 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 (Howell, 2020). We should therefore expect the

[21] For example, 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).

[22] Average age equals 3.2 years in the 'young' group and 13.9 years in the 'old' group.

[23] 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.

estimated effects of entry into the sandbox to be stronger for venture capital deals than for other deals. In column (5) we introduce dummy *VC*, which takes on a value of one if a deal is classified as venture capital deal, and zero otherwise. The effect of sandbox entry on capital raised is economically larger and statistically significant 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. Results are similar when we include time-varying effects at the industry level in column (6). Taken together, the results in columns (1)–(6) suggest that the sandbox particularly helps opaque firms to raise capital.

We also provide indirect evidence that sandboxes reduce 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).[24] To test this proposition more formally, we introduce an interaction term between *post SB entry* and a dummy *law* into Equation (1). Columns (7)–(8) provide results. Column (7) uses firm and year fixed effects, column (8) adds industry*time fixed effects. Across all 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 around half as large for firms with a CEO that has a background in law or the financial sector.

Foreign and first-time investors Firm-level results suggest that the sandbox facilitates firms' access to capital by reducing information asymmetries. We now shed further light on the information channel by investigating the effect of sandbox entry on capital raised from foreign and first-time investors. Crucially, granular investor-firm data allow us to overcome the challenge that our firm-level results could be biased by confounding factors at the investor or firm level. To this end, we estimate variants of the following

[24]The management literature has established that CEO or founder experience and skill is correlated with firm performance (Bhagat et al., 2010; Gottesman and Morey, 2010; Bernstein et al., 2017).

regression at the firm-investor-quarter level:

$$log(1+amt)_{i,f,t} = \delta_1\ post\ SB\ entry_{f,t} + \delta_2\ investor\ type_i + \delta_3\ post\ SB\ entry_{f,t} \times\ investor\ type_i + \theta_{i,f} + \tau^1_{i,t} + \tau^2_{c,t} + \nu_{i,f,t}. \quad (3)$$

The dependent variable is the amount invested by investor i in firm f in quarter t, split on a pro-rata basis. In robustness tests we also split deal volume by investors size. The dummy *post SB entry* takes a value of one after sandbox entry, and zero for all quarters prior to entry. We include the vector of time-invariant firm characteristics interacted with dummy *post SB entry* as controls. Standard errors are clustered at the firm level.[25] To test whether the sandbox alleviates informational frictions, we interact *post SB entry* with the dummies *foreign investor* or *new investor*, denoted by *investor type*$_i$ in regression Equation (3), that take on a value of one if an investor is based outside the UK or has not invested into the firm prior to its entry into the sandbox. As informational frictions are more severe for new or foreign investors, we expect entry into the sandbox to lead to a larger increase in capital raised from these investors, so $\delta_3 > 0$.

Coefficients in regression Equation (3) could be biased if entry into the sandbox is correlated with confounding investor or firm factors. Investors could adjust their overall supply of capital as firms enter the sandbox. For example, a change in the tax regime that reduces capital taxes on investments in fintechs could relax investors' constraints. Likewise, investors could gain expertise in evaluating fintechs and thereby face lower screening or monitoring costs over time. Further, even within the group of firms that enters the sandbox, the exact entry date could be correlated with unobservable firm characteristics. While results in Table 2 and Table 3 suggest that entry into the sandbox is not systematically correlated with firm observable or time-varying factors at the industry*time level, firm-level regression cannot fully account for unobservable firm characteristics that vary over time.

Investor-firm level data allow us to include a rich set of fixed effects to overcome

[25] Results remain virtually identical when we cluster at the firm and investor level (unreported).

this challenge. First, to control for unobservable firm-investor factors, as well as unobservable investor-specific factors that vary over time, we include investor*firm ($\theta_{i,f}$) and investor*time ($\tau^1_{i,t}$) 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). 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.

Second, we can control for unobservable time-varying factors at the firm level through firm*time fixed effects ($\tau^2_{c,t}$). For example, these fixed effects absorb changes in firm sales, management, or product quality. That is, we investigate the differential effect of entry into the sandbox on foreign or new investors only among the group of firms that enter the sandbox at some point, holding all time-varying unobservable firm fundamentals constant. In essence, in these saturated specifications we compare the *same* firm raising capital from the *same* investor at different dates of entry into the sandbox (Jiménez et al., 2014).

Table 5, panel (a) shows that entry into the sandbox leads to an increase in capital raised also at the investor-firm level. Column (1) includes investor*firm and year fixed effects and shows that entry into the sandbox is associated with an increase in capital raised of 2.7% from the average investor. Controlling for confounding time-varying factors at the investor level through investor*time fixed effects in column (2), the effect size increases in magnitude: entry into the sandbox leads to a 4.4% increase in capital raised from each investor. The fact that the coefficient increases when we include investor*time fixed effects implies that the positive effects from sandbox approval are firm-specific. If, instead, higher supply of capital by investors that invest in sandbox and non-sandbox firms would affect all firms in an investor's portfolio, we would find no effect of sandbox entry on capital raised by sandbox firms *after* accounting for changes in the supply of

capital across all firms of an investor. Another implication is that the estimated coefficient β in Equation (1) likely represents a lower bound of the true effect.[26]

In columns (3)–(6) we investigate whether the effect of sandbox entry on capital raised depends on investor characteristics related to informational frictions, based on Equation (3).[27] Column (3) employs investor*firm and investor*year fixed effects and shows that entry into the sandbox is associated with an increase in capital raised of 3.4% for domestic investors, and 4.7% for foreign investors. Both coefficients are significant at the 5% level. The interaction specification allows us to address the key identification challenge for our study: even among the group of firms that enter the sandbox at some point, the date of entry could be correlated with unobservable firm characteristics. Column (4) thus includes time-varying fixed effects at the firm level ($\tau^2_{c,t}$) that control for any observable and unobservable confounding factors at the firm level. Results show that firms raise significantly more capital from foreign investors even after we account for time-varying firm characteristics.

Columns (5) and (6) report a near-identical picture for new investors. Entry into the sandbox leads to an increase in capital raised of 2.7% from 'old' and of 4.5% from new investors when we employ investor*firm and investor*time fixed effects. The estimated coefficients are highly significant statistically and economically. Column (6) confirms that firms also raise more capital from new investors after entry into the sandbox when we include firm*time fixed effects. The coefficient on the interaction term increases slightly in magnitude, from 1.8% to 1.9%, and remains significant at the 1% level.

Results in panel (a) of Table 5 thus suggest that entry into the sandbox leads to a significant increase in capital raised, especially from investors that are subject to more

[26]Further, these findings suggest that the sandbox does not lead to a reallocation of aggregate capital, but increases the amount of available funding. If sandboxes lead to a reallocation of existing funds from non-sandbox to sandbox firms instead, we would expect a decline in the coefficient δ_1 in Equation (3) when we hold changes in capital supply constant. An increase in funding for sandbox firms would then imply a decrease in available funds for non-sandbox firms, so δ_1 would be upward-biased. The fact that the coefficient increases in magnitude suggests that the sandbox increases the aggregate supply of capital.

[27]For consistency, we restrict the sample to firms connected to at least two investors and investors connected to at least two firms in each quarter. Estimated coefficients in columns (3) and (5) are similar for the full sample.

severe informational frictions. Importantly, including firm*time fixed in our regressions leads to no material change in the estimated coefficients, while the R^2 increases by over 30 percentage points. Unobservable firm characteristics that could be correlated with a firms' entry date are hence unlikely to explain our finding, reducing potential concerns about self-selection and omitted variable bias (Altonji et al., 2005; Oster, 2019). This finding echoes the results from Table 2 and Table 3 that showed that the entry date is uncorrelated with observable firm characteristics and that including time-varying fixed effects at the industry level does not affect our results.

Beyond firms raising more capital from foreign and new investors post entry, does the *number* of investors change? To this end, we analyse the effect of entry into the sandbox on the number of investors and the share of foreign investors in panel (b) of Table 5. In columns (1)–(2), the dependent variable is the log of one plus the number of new investors 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). Consistent with results in panel (a), there is an increase in the number of new investors for firms that raise capital after entering the sandbox. Columns (3)–(5) 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 the number of US investors. Column (5) shows an economically large (but statistically insignificant) increase in the number of non-UK investors.

In conclusion, Table 5 provides support for the hypothesis that the regulatory sandbox reduces informational asymmetries. Firms that enter the sandbox raise more capital from investors based outside the UK and investors that have not previously invested into the firm. The positive effect of entry into the sandbox on capital raised is not explained by potentially confounding time-varying investor characteristics; nor is it explained by unobservable firm characteristics that could be correlated with the entry date.

5 Additional tests

Table 6 provides extensions and robustness checks to our baseline specification in Equation (1). First, we look at the increase in funding raised immediately around the entry date 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 explanations, columns (1)–(3) in panel (a) focus on different horizons post-sandbox entry. Column (1) reports results for Equation (1), but only includes the two quarters after sandbox entry in the sample. Column (2) instead excludes quarters one and two after entry, and column (3) 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 to a steady decline in the coefficient size, with the coefficient turning insignificant in column (3). This pattern is consistent with our findings that entry into the sandbox reduces informational frictions and reassures investors of lower regulatory risks. The increase in funding raised does not reflect a gradual revelation of firms' quality.

Column (4) restricts the sample to the eight quarters prior and 12 quarters after sandbox entry; column (5) 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.

Due to the nature of our data, our dependent variable takes on a value of zero in several quarters. We thus 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 (6), we estimate a negative binomial regression. In column (7), we estimate a Tobit random effects regression with log deal amount as dependent variable and report average marginal effects with robust standard errors.[28] 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 magnitude of the effect is similar to our baseline regressions.

Finally, column (8) employs fixed effects for each cohort, accounting for the fact that unobservable factors could affect firms in the same cohort. Confirming our previous results (the 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. In column (9) we add contiguous-cohort fixed effects. Potentially, whether a firm participates before or after six months (ie among two contiguous cohorts) is more 'random' than before or after eg three years. Yet, results show that baseline results remain almost unaffected.

Panel (b) replicates results in Table 3, but makes use of information on our set of non-sandbox control firms. Each regression is now weighted by the respective CEM weights to ensure balancedness in co-variates.[29] The number of firm-quarter observations now increases to 3,820. Coefficient β now indicates 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. Our estimated coefficients are similar in terms of sign, size, and significance to those in our baseline table. 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% compared to 15.0% in column (4) of Table 3.

Panel (c) further shows that our results are robust to the use of alternative matching

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

[29] 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.

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 one and three. 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.

The investor-level analysis assumes a pro-rata split of deal volume across investors. In Table 7, panel (a) splits deal 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. Panel (b) uses the dummy *capital raised* as dependent variable, which takes on a value of 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. Results remain unaltered for the alternative outcome variables: entry into the sandbox leads to a significant increase in capital raised, especially among foreign and new investors. These findings are unaffected by the inclusion of time-varying fixed effects at the investor or firm level.

6 Conclusion

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 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 successful 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 have already set up a regulatory sandbox (World Bank, 2020). 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 systematic analysis of their effectiveness and the underlying channels.

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. The short time span since their inception does not allow for an evaluation of the effects of regulatory sandboxes on consumer surplus or financial stability (yet). There is also no systematic data available on e.g. firms' revenues, cash flow, or customer base.[30] However, early results suggest that survival rates among sandbox firms are significantly higher than those of the average startups. The FCA (2019) reports that around 80% of firms that successfully tested in the sandbox are still in operation (as of 2019), which is significantly higher than average numbers for startups. For example, the three-year survival rate of startups averages around 60% (Hyytinen et al., 2015).

Nonetheless, this paper provides an important first step in assessing the usefulness of one of the most-widely adopted 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

[30]We could find only a very small number of sandbox firms that already provide detailed balance sheet data in e.g. 'Companies House' or 'Capital IQ' databases. Further, an important feature of firms operating in the digital economy is that they often aim to first achieve scale and capture market share, even if they are loss making, before turning a profit. Even if there were data on e.g. profits, it would not necessarily allow for a sensible evaluation of the success of fintechs.

based on their experience.[31]

[31] Cornelli et al. (2021) show in cross-country regressions that fintechs raise significantly more capital on aggregate after a country has established a sandbox.

References

Allen, H. J. (2019) "Regulatory sandboxes", *George Washington Law Review*, 87.

Altonji, J. G., T. E. Elder, and C. R. Taber (2005) "Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools", *Journal of Political Economy*, 113 (1), pp. 151–184.

Arner, D. W., J. Barberis, and R. P. Buckey (2017) "Fintech, regtech, and the reconceptualization of financial regulation", *Nw. J. Int'l L. & Bus.*, 371.

Bank for International Settlements (2019) "Big techs in finance: Opportunities and risks", *Annual Economic Report*, Chapter 3.

Bartlett, R., A. Morse, R. Stanton, and N. Wallace (2019) "Consumer-lending discrimination in the fintech era", *NBER Working Paper*, No 25943.

Berg, T., V. Burg, A. Gombović, and M. Puri (2020) "On the rise of fintechs: Credit scoring using digital footprints", *Review of Financial Studies*, 33 (7), pp. 2845–2897.

Bernstein, S., A. Korteweg, and K. Laws (2017) "Attracting early-stage investors: Evidence from a randomized field experiment", *Journal of Finance*, 72 (2), pp. 509–538.

Bhagat, S., B. J. Bolton, and A. Subramanian (2010) "CEO education, CEO turnover, and firm performance", *Working Paper*.

Blackwell, M., S. Iacus, G. King, and G. Porro (2009) "CEM: Coarsened exact matching in stata", *The Stata Journal*, 9 (4), pp. 524–546.

Block, J. H., M. G. Colombo, D. J. Cumming, and S. Vismara (2018) "New players in entrepreneurial finance and why they are there", *Small Business Economics*, 50 (2), pp. 239–250.

Bolton, P. and X. Freixas (2000) "Equity, bonds, and bank debt: Capital structure and financial market equilibrium under asymmetric information", *Journal of Political Economy*, 108 (2), pp. 324–351.

Bone, J., J. González-Uribe, C. Haley, and H. Lahr (2019) "The impact of business accelerators and incubators in the UK".

Brown, D. C. and S. Davies (2020) "Financing efficiency of securities-based crowdfunding", *Review of Financial Studies*, 33 (9), pp. 3975–4023.

Brummer, C. and Y. Yadav (2019) "Fintech and the innovation trilemma", *Georgetown Law Journal*, 107.

Buchak, G., G. Matvos, T. Piskorski, and A. Seru (2018) "Fintech, regulatory arbitrage, and the rise of shadow banks", *Journal of Financial Economics*, 130 (3), pp. 453–483.

Chen, M. A., Q. Wu, and B. Yang (2019) "How valuable is fintech innovation?", *Review of Financial Studies*, 32 (5), pp. 2062–2106.

Cornelli, G., S. Doerr, L. Franco, and J. Frost (2021) "Global investment in fintech: patterns, drivers and implications", *BIS Quarterly Review*, September.

Coval, J. D. and T. J. Moskowitz (1999) "Home bias at home: Local equity preference in domestic portfolios", *Journal of Finance*, 54 (6), pp. 2045–2073.

Czarnitzki, D. and H. Hottenrott (2011) "R&D investment and financing constraints of small and medium-sized firms", *Small Business Economics*, 36 (1), pp. 65–83.

Degryse, H. and S. Ongena (2005) "Distance, lending relationships, and competition", *Journal of Finance*, 60 (1), pp. 231–266.

Deloitte (2019) "A journey through the FCA regulatory sandbox", *Center for Regulatory Strategy EMEA*.

Financial Conduct Authority (2015) "Regulatory sandbox", *November*.

Financial Conduct Authority (2019) "The impact and effectiveness of innovate", *April*.

Fuster, A., P. Goldsmith-Pinkham, T. Ramadorai, and A. Walther (2021) "Predictably unequal? The effects of machine learning on credit markets", *Journal of Finance*.

Gompers, P. A. (1995) "Optimal investment, monitoring, and the staging of venture capital", *Journal of Finance*, 50 (5), pp. 1461–1489.

González-Uribe, J. and M. Leatherbee (2018) "The effects of business accelerators on venture performance: Evidence from start-up Chile", *Review of Financial Studies*, 31 (4), pp. 1566–1603.

González-Uribe, J. and S. Reyes (2021) "Identifying and boosting "gazelles": Evidence from business accelerators", *Journal of Financial Economics*, 139 (1), pp. 260–287.

Gottesman, A. A. and M. R. Morey (2010) "CEO educational background and firm financial performance", *Journal of Applied Finance*, 20 (2).

Grinblatt, M. and M. Keloharju (2001) "How distance, language, and culture influence stockholdings and trades", *Journal of Finance*, 56 (3), pp. 1053–1073.

Haddad, C. and L. Hornuf (2019) "The emergence of the global fintech market: Economic and technological determinants", *Small Business Economics*, 53 (1), pp. 81–105.

Hall, B. H. and J. Lerner (2010) "The financing of R&D and innovation", *Handbook of the Economics of Innovation*, 1, pp. 609–639.

HM Treasury (2016) "UK FinTech: On the Cutting Edge",Technical report.

HM Treasury (2019) "UK FinTech: State of the Nation",Technical report.

Howell, S. T. (2017) "Financing innovation: Evidence from R&D grants", *American Economic Review*, 107 (4), pp. 1136–64.

Howell, S. T. (2020) "Reducing information frictions in venture capital: The role of new venture competitions", *Journal of Financial Economics*, 136 (3), pp. 676–694.

Hyytinen, A., M. Pajarinen, and P. Rouvinen (2015) "Does innovativeness reduce startup survival rates?", *Journal of Business Venturing*, 30 (4), pp. 564–581.

Iacus, S. M., G. King, and G. Porro (2012) "Causal inference without balance checking: Coarsened exact matching", *Political Analysis*, 20 (1), pp. 1–24.

Ivković, Z. and S. Weisbenner (2005) "Local does as local is: Information content of the geography of individual investors' common stock investments", *Journal of Finance*, 60 (1), pp. 267–306.

Jiménez, G., S. Ongena, J.-L. Peydró, and J. Saurina (2014) "Hazardous times for monetary policy: What do twenty-three million bank loans say about the effects of monetary policy on credit risk-taking?", *Econometrica*, 82 (2), pp. 463–505.

Kerr, W. R. and R. Nanda (2015) "Financing innovation", *Annual Review of Financial Economics*, 7, pp. 445–462.

Khwaja, A. I. and A. Mian (2008) "Tracing the impact of bank liquidity shocks: Evidence from an emerging market", *American Economic Review*, 98 (4), pp. 1413–1442.

Lerner, J. and R. Nanda (2020) "Venture capital's role in financing innovation: What we know and how much we still need to learn", *Journal of Economic Perspectives*, 34 (3), pp. 237–61.

OECD (2017) "Fostering Innovation in the Public Sector", *OECD Publishing*, Paris.

Oster, E. (2019) "Unobservable selection and coefficient stability: Theory and evidence", *Journal of Business & Economic Statistics*, 37 (2), pp. 187–204.

Peirce, H. (2018) "Beaches and bitcoin: remarks before the Medici Conference", *SEC, May*, 2.

Petersen, M. A. and R. G. Rajan (1994) "The benefits of lending relationships: Evidence from small business data", *Journal of Finance*, 49 (1), pp. 3–37.

Thakor, A. V. (2020) "Fintech and banking: What do we know?", *Journal of Financial Intermediation*, 41, p. 100833.

Trester, J. J. (1998) "Venture capital contracting under asymmetric information", *Journal of Banking & Finance*, 22 (6-8), pp. 675–699.

Wechsler, M., L. Perlman, and N. Gurung (2018) "The state of regulatory sandboxes in developing countries", *Working Paper*.

World Bank (2020) "Global Experiences from Regulatory Sandboxes".

World Bank and Cambridge Center for Alternative Finance (2019) "Regulating Alternative Finance: Results from a Global Regulator Survey", *World Bank*.

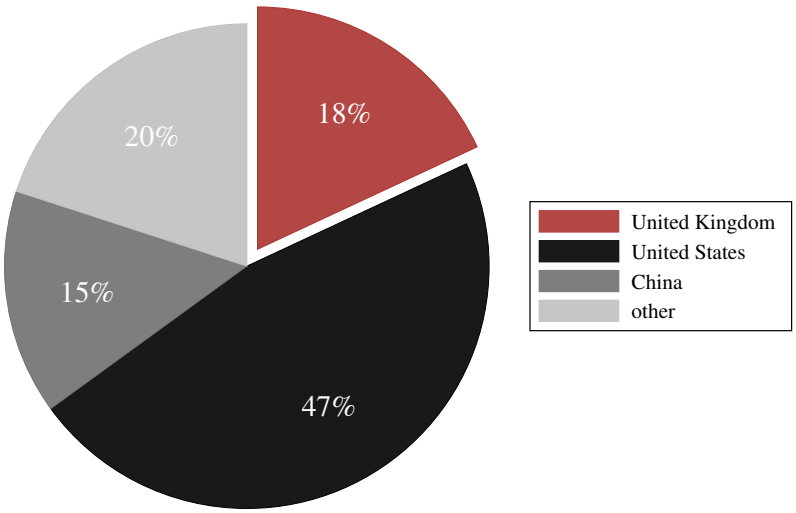
Yu, S. (2020) "How do accelerators impact the performance of high-technology ventures?", *Management Science*, 66 (2), pp. 530–552.

Zetzsche, D. A., R. P. Buckley, J. N. Barberis, and D. W. Arner (2017) "Regulating a revolution: From regulatory sandboxes to smart regulation", *Fordham Journal of Corporate and Financial Law*, 23.

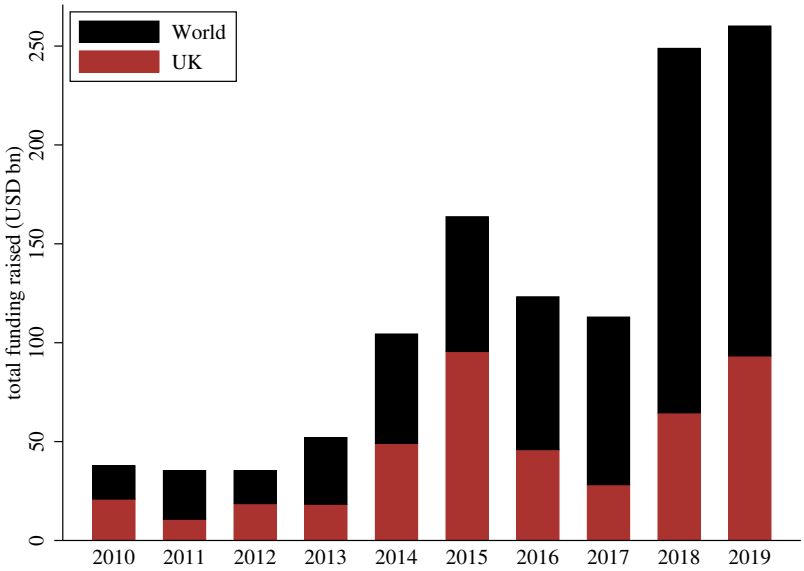
Figures and tables

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

(a) On average (2010-2019)



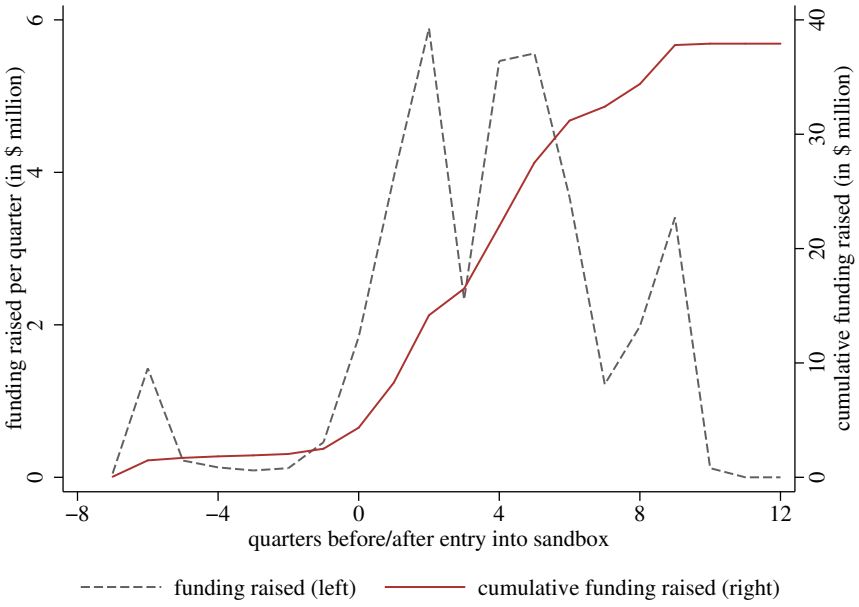
(b) Over time



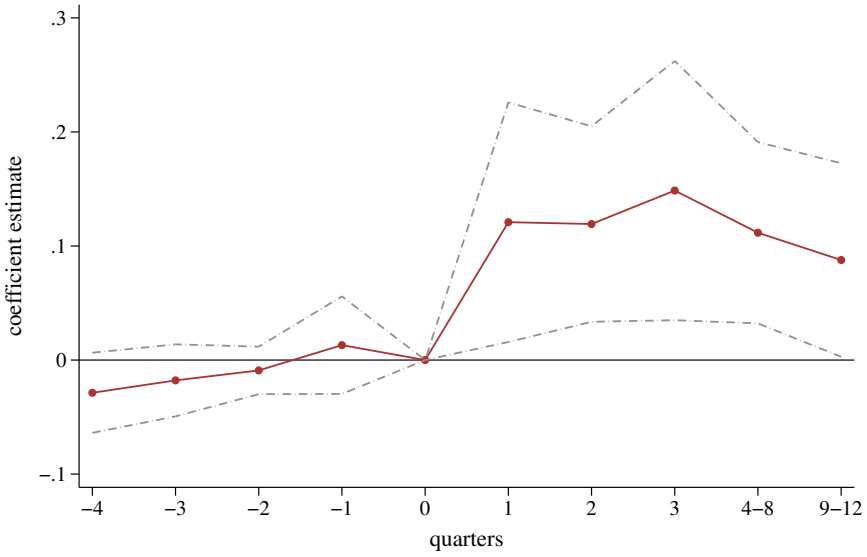
Note: Panel (a) shows the share of total funding raised by fintechs in the UK, US, China, and the rest of the world, averaged over the period 2010-2019. Panel (b) plots the total funding raised (in $bn) by fintechs in the UK in relation to worldwide funding raised by fintechs over the period 2010-2019. 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.

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.

Table 1: **Descriptive statistics**

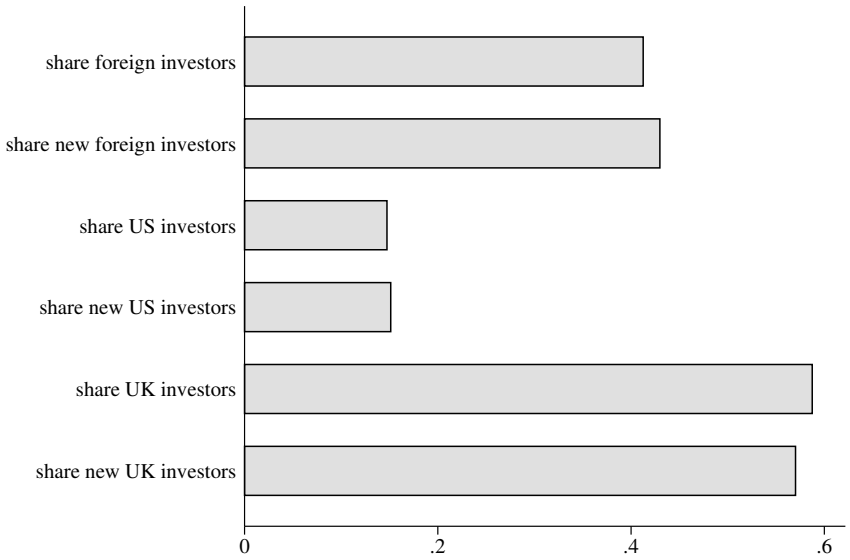
(a): Firm characteristics

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
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.

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 a value of 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 a value of 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. *** p<0.01, ** p<0.05, * p<0.1

Table 3: **Entry into the sandbox and capital raised**

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	capital raised	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	-	-	-	✓	-	-

Note: This table 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 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 on a value of one if the firm raises a positive amount of capital in a given quarter in columns (5)–(6). *post SB entry* is a dummy with a value of 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*, as controls. Standard errors are clustered at the firm level. Columns (5)–(6) report average marginal effects from logistic regressions with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

Table 4: **Information asymmetry and CEO background**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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	0.106**	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)				
venture capital					0.178***	0.182**		
					(0.054)	(0.086)		
post SB entry × venture capital					0.191**	0.187**		
					(0.094)	(0.097)		
post SB entry × law degree							-0.103*	-0.121*
							(0.058)	(0.065)
Observations	908	855	995	931	908	855	908	855
R-squared	0.098	0.161	0.239	0.383	0.390	0.450	0.095	0.160
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	-	✓	-	✓	-	✓	-
Industry*Time FE	-	✓	-	✓	-	✓	-	✓

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 a value of 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*, as controls. *old firm* is a dummy with a value of one for firms above the median in terms of firm age; *large firm* is a dummy with a value of one for firms associated with large listed firms; and *law degree* is a dummy with a value of one for firms that have a CEO with a law degree. Standard errors are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: **Investor analysis**

(a): Accounting for investor and firm characteristics

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	0.027**	0.044**	0.034**		0.027**	
	(0.011)	(0.020)	(0.020)		(0.013)	
post SB entry × foreign investor			0.013**	0.013***		
			(0.005)	(0.004)		
post SB entry × new investor					0.018***	0.019***
					(0.007)	(0.006)
Observations	41,745	41,717	34,249	34,249	34,249	34,249
R-squared	0.031	0.267	0.267	0.592	0.266	0.593
Investor*Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	-	-	-	-	-
Investor*Time FE	-	✓	✓	✓	✓	✓
Firm*Time FE	-	-	-	✓	-	✓

(b): The share of new and foreign 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	-	✓	✓	✓	✓

Note: Panel (a) 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, based on a pro-rata split. *post SB entry* is a dummy with a value of one after sandbox entry, and zero for all quarters prior to entry. *foreign investor* and *new investor* are dummies that take on a value of one if the investor is not headquartered in the UK or has not invested into the firm prior to its entry into the sandbox. 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*, as controls. Standard errors are clustered at the firm level. *** 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)
	q1-2	q3+	q5+	-8 to +12	-4 to +8	Neg Bin	Tobit	Cohort FE	Cont Cohort FE
VARIABLES	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	deal amt	log(deal amt)	log(deal amt)	log(deal amt)
post SB entry	0.161**	0.136*	0.043	0.138**	0.188**	0.113**	0.149*	0.148**	0.145**
	(0.083)	(0.075)	(0.064)	(0.066)	(0.081)	(0.053)	(0.088)	(0.064)	(0.063)
Observations	643	812	716	762	591	908	908	908	908
R-squared	0.106	0.110	0.108	0.105	0.127			0.093	0.096
Firm FE	✓	✓	✓	✓	. ✓	-	-	✓	✓
Time FE	✓	✓	✓	✓	✓	-	-	✓	✓

(b): Matched control firms – CEM

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	log(deal amt)	log(deal amt)	log(deal amt)	log(deal amt)	capital raised	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	-	-	-	✓	-	-

(c): 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	✓	✓	✓	✓	✓	✓	✓	✓

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)–(5) and (8); or total deal amount in columns (6) and (7). Columns (1)–(5) narrow the time window around the entry date into the sandbox. Columns (6) and (7) estimate negative binomial and Tobit regressions and report average marginal effects with robust standard errors. Column (8) employs fixed effects for each individual cohorts level, column (9) uses fixed effects for contiguous cohorts. Panel (b) uses the sample of sandbox firms and the sample of control firms and uses coarsened exact matching, panel (c) 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. *** p<0.01, ** p<0.05, * p<0.1

Table 7: **Investor-firm analysis – alternative outcome variables**

(a): Total capital invested

VARIABLES	(1) by size log(deal amt)	(2) by size log(deal amt)	(3) by size log(deal amt)	(4) by size log(deal amt)	(5) by size log(deal amt)	(6) by size log(deal amt)
post SB entry	0.040***	0.062**	0.041**		0.040**	
	(0.014)	(0.027)	(0.026)		(0.027)	
post SB entry × foreign investor			0.025**	0.027**		
			(0.010)	(0.011)		
post SB entry × new investor					0.028**	0.028**
					(0.009)	(0.010)
Observations	41,745	41,717	34,249	34,249	34,249	34,249
R-squared	0.033	0.236	0.236	0.561	0.237	0.562
Investor*Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	-	-	-	-	-
Investor*Time FE	-	✓	✓	✓	✓	✓
Firm*Time FE	-	-	-	✓	-	✓

(b): Any capital invested

VARIABLES	(1) dummy debt raised	(2) dummy debt raised	(3) dummy debt raised	(4) dummy debt raised	(5) dummy debt raised	(6)) dummy debt raised
post SB entry	0.104*	0.161**	0.155**		0.159**	
	(0.061)	(0.053)	(0.056)		(0.051)	
post SB entry × foreign investor			0.082**	0.090***		
			(0.034)	(0.027)		
post SB entry × new investor					0.107**	0.105**
					(0.012)	(0.012)
Observations	41,745	41,717	34,249	34,249	34,249	34,249
R-squared	0.024	0.263	0.263	0.616	0.262	0.616
Investor*Firm FE	✓	✓	✓	✓	✓	✓
Time FE	✓	-	-	-	-	-
Investor*Time FE	-	✓	✓	✓	✓	✓
Firm*Time FE	-	-	-	✓	-	✓

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 split deal volume by aggregate investors size in panel (a). Panel (b) uses dummy *capital raised* as dependent variable that takes on a value of 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. *foreign investor* and *new investor* are dummies that take on a value of one if the investor is not headquartered in the UK or has not invested into the firm prior to its entry into the sandbox. All regressions include time-invariant firm characteristics log age, CEO gender, and London dummy, interacted with *post SB entry*, as controls. Standard errors are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1