Failed Venture Capital Fundraising Campaigns and Startup Growth:

The Value-Add of Venture Capital Due-diligence[1]

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We investigate an unexplored role of venture capital (VC) investors on innovation; the potential value-add of due-diligence for companies involved in failed VC fundraising campaigns— i.e., startups that do not receive investment from the VC doing due-diligence. By VC due-diligence we mean the multi-stage process through which VCs scrutinize businesses for potential investment. Our novel data comprises nearly 2,000 startups applying for funding to a UK VC seed fund (Fund). For identification, we exploit the Fund's process of screening applicants for due-diligence, which features pre-determined selection rules based on the scores of quasi-randomly allocated reviewers with different scoring generosities. We show that assignment to due-diligence leads to substantial increases in venture growth within two years of application, even for companies involved in failed fundraising campaigns. VC due-diligence comprises "type improvement" and "type discovery" mechanisms; tentative evidence suggest that type improvement (including coaching, learning-by-doing, and network support) may be primary. This new evidence implies that VCs' role in innovation affects many more companies (approximately 30+ out of every 100 applicants) than existing research has fully recognized, as it goes beyond their value-added effects on the portfolio companies in which they invest (less than 1 out of 100 applicants). Therefore, frictions in the process through which startups seek and obtain VC funding can have profound implications for ecosystem-wide innovation and growth.

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Venture capital (VC) investors fund startups that become some of the world's most innovative, and most valuable, companies. In the US, VC-backed companies account for about 41% of total market capitalization, 62% of public companies' research and development (R&D) spending, and 48% of patent value (Gornall and Strebulaev, 2021).

A relationship between VC and innovation is said to be pervasive in clusters around the world, including London, Shanghai, Silicon Valley, and Tel Aviv (Mallaby, 2022; Klingler-Vidra, 2018). This link is also not just a curiosity: research shows that VC investors contribute to innovation through the "smart money" they provide to their portfolio companies (e.g. the companies in which they invest), for example, through the operational expertise they share and the professional networks they make available (Lerner and Nanda, 2020).

In this paper, we offer a new line of research, which seeks to examine the potential impact that VCs have on the wider ecosystem of companies they do not fund. We are motivated by the observation that VCs spend significant resources closely interacting with entrepreneurs outside of their investment portfolio. For every one company in which they invest, VCs consider 100 companies, and interact closely with 30 companies as they scrutinize prospective investments (Gompers, Gornall, Kaplan, and Strebulaev, 2020).

The scrutiny that VCs complete in order to underwrite an investment is a highly interactive and multi-staged process known as "due-diligence". It begins after an initial screening, when VCs consider a business plan or high-level pitch in order to determine if the startup is venture backable, given their fund's mandate. It then proceeds in multiple stages of the so-called deal "funnel," whereby VCs progressively increase the intensity with which they scrutinize a narrowing set of promising candidates for potential investment.[2]

VCs recognize the importance of due-diligence for driving their returns (Gompers et al., 2020), and researchers have shown its value-add to portfolio companies (Cumming and Zambelli, 2016). Our novel premise is that due-diligence can also add value to the companies that VCs assess but ultimately reject for investment. Given their industry expertise and experience in financing, VC investors may be better than venture founders at evaluating the likely success of an early-stage firm, and at designing growth strategies, and prioritizing tasks (Axelson, 2007; Sariri, 2022). This premise is consistent with anecdotal evidence: entrepreneurs often describe crucial learnings gained through engaging in VCs' due-diligence process, even from failed fundraising campaigns.[3] However, it is not obvious that going through VC

[2] This view of due-diligence reflects the conception that industry analysts, such as PitchBook, use to describe the various arenas in which investors, over the course of numerous interactions with ventures and external sources, assess potential businesses for investment. See, for instance, https://pitchbook.com/blog/due-diligence-checklist-for-vc-pe-and-ma-investors. We note that in other papers the term due-diligence makes reference to only the last stage of the selection funnel, see for example Gompers et al., (2020).
[3] For multiple examples, listen to podcast: The Pitch: https://gimletmedia.com/shows/the-pitch/episodes.

due-diligence should constitute a value-add to entrepreneurs. Fundraising is a time-consuming process that can distract founders from their goal of growing their companies and feedback from a VC that decides not to invest may not be constructive. Moreover, in the absence of informational frictions, entrepreneurs would know just as much as potential investors about the potential success of their companies, thus limiting responsiveness to feedback. Finally, overconfident founders might not respond to informative but low-stakes feedback (Howell, 2021).

Empirically determining whether going through the process of VC due-diligence affects the growth of ventures involved in failed fundraising campaigns is challenging. Observing the companies that engage in due-diligence, but do not ultimately obtain investment, is rare.[4] Moreover, tracking venture growth is challenging as many companies that attempt to raise VC funding never raise financing or have publicly-available financial records.[5] Finally, selection for due-diligence is endogenous since VCs decide to conduct due-diligence based on a number of observable and unobservable factors. Comparing the growth of companies that go through VC due-diligence with those that do not may yield biased estimates of VC due-diligence effects if VCs select the companies with the highest growth prospects.

To shed light on the potential value-add of VC due diligence in failed fund-raising rounds, we partnered with a VC seed fund in the United Kingdom (hereafter "the Fund”). Our novel data comprises nearly 2,000 ventures applying for capital from the Fund, which is representative of other seed funds that are increasingly prevalent in innovation ecosystems (Lerner and Nanda, 2020). To measure venture performance, we draw on administrative UK (abridged) balance sheet data that we combine with traditional web sources to track venture growth and funding from VCs other than the Fund. Finally, for identification, we exploit the Fund's structured and well-documented process of screening applicants for due-diligence, which is consistent with the rise of systematic assessment via “scorecards” by early-stage VC funds (Malenko, Nanda, Rhodes-Kropf and Sundaresan, 2020). We construct an instrumental variable (IV) by exploiting two features of the due-diligence selection process: (1) the quasi-random assignment of applicants to three reviewers with different scoring generosities and (2) the aggregation of reviewers' scores using pre-determined rules that vary over time and across applicants’ locations.

Through this empirical strategy, we find novel evidence that VC due-diligence can be a positive driver of venture performance even for companies involved in failed VC fundraising campaigns— i.e., ventures that do not become part of the portfolio of the VC conducting the due-diligence. We find that assignment to due-diligence by the Fund leads to significant increases in venture growth within two

[4] Most existing papers on the impact of VC on venture growth rely on databases of ventures (e.g. Preqin, Crunchbase) that detail rounds of equity investment raised, not the details of the VC that startups attempted to secure (see Kaplan and Lerner, 2016). Such datasets, therefore, only show the results of successful funding campaigns. They do not indicate all the ventures that sought investment from VCs; so, there is no record of how much due-diligence was completed.

[5] This possibility contributes to survivor bias, in that only the successful ventures, in terms of VC funding campaigns, are observed.

years of application (as measured by several proxies) including for those companies involved in failed fundraising campaigns with the Fund. Results are robust to different specifications, additions of controls, and other multiple robustness checks. In terms of economic magnitude, our results imply that assignment to due-diligence alone increases VC funding (from VCs other than the Fund) by £142K. This estimate corresponds to a 21% increase relative to the 75th percentile of the post-application funding distribution, which has a long right tail and a mean of zero. By contrast, we find no evidence that ventures' assignment to informal meetings that are not part of the Fund's due-diligence has casual meaningful effects on venture performance.[6]

The first primary implication from our findings is that investors have private information about startups' prospects and potential success of growth strategies, and going through seed VCs' due-diligence can help resolve these informational frictions for founders.[7] This evidence adds to prior work demonstrating that business plan competitions also help resolve these types of frictions (Howell, 2020; 2021). What incentivizes investors to share their private information with companies that do not become part of their portfolio? Ex-post, adding value through due-diligence appears inefficient because investors do not capture the performance improvements for most companies that benefit from this value add. However, ex-ante, before the investment decision is made, the investors have incentives to share their insights with all companies that make it to the due-diligence stage; for these companies the probability of investment is non-zero, and their likelihood of accepting a Fund' term-sheet (if one is offered) likely increases if they think the Fund adds value. In addition, investors, especially those recently established like the Fund, can also benefit indirectly from sharing their insights through due-diligence, for example, by building a value-add reputation that can improve future deal-flow and investor's discount rates (cf., Hsu, 2004; Sorensen, 2008).

The second main implication from our findings is that VCs have a broader impact on innovation than previously acknowledged, extending beyond value-add to their portfolio companies (less than 1 out of 100 applicants) and instead covering the wider-pool of entrepreneurs they interact with through due-diligence (approximately 30+ out of every 100 applicants). A back-of-the-envelope calculation (based on our findings) points to the first order nature of this broader impact: the Fund's due-diligence effects for a given firm involved in a failed fundraising round amounts to a third of the Fund's total investment effects on its average portfolio firm (including any portfolio selection effects).[8] This implication is in line with mounting evidence showing that investments by early-stage investors disproportionately affects innovation ecosystems (Kortum and Lerner, 2000; Samila and Sorenson, 2011; Fehder and

[6] We build an instrumental variable strategy to assess the impact of informal meetings by exploiting the selection rules for those meetings; See Section 3.2.
[7] Further consistent with the idea that the Fund has private informaton we show that application scores predict performance for firms outside the due-diligence sample (see Section 3).
[8] The ratio between our estimated due-diligence effect and a simple OLS estimate comparing the performance of portfolio firms to other applicants ranges between 0.39

Hochberg, 2019; Opp, 2019). Therefore, networking frictions for ventures seeking VC funding – which we understand as ventures' inability to gain access to meeting potential investors – can have profound implications for growth, as such meetings positively affect venture performance even when there is no investment (cf., Howell and Nanda, 2021).

While our setting allows us to overcome challenges in estimating VC due-diligence's value-add, there are at least two important limitations.

First, our use of data from the Fund potentially trades-off external for internal validity. Our identification strategy measures the effect of due-diligence assignment for marginal applicants. It is possible that the impact of due-diligence assignment is different for applicants who are not on the margin. To partially address this issue, we show that the effects of due-diligence assignment appears constant across applicants of different growth potential by estimating marginal treatment effects (MTEs; Heckman and Vytaclyl, 2005). While our analysis provides rigorous evidence that VC due-diligence *can* add value to a wider set of entrepreneurs, we are cognizant that it has little to say about how systematic this value-add is across VC firms. Our data is from only one Fund. Yet, it is representative of a new breed of VCs targeting the increasingly inexperienced entrepreneurs seeking early-stage specialized financing (Lerner and Nanda, 2020). Like the Fund, these VCs specialize in pre-Series A investments, do not shy away from sourcing deals online, and typically implement more information technology-enabled approaches to pre-screen applicants. This includes applying complex methods such as voting rules like the one used by the Fund, or even machine learning methodologies in order to screen and score potential investments. While not necessarily representative of all VCs, our results do represent this new type of VC that is increasingly prevalent in entrepreneurial markets.

The second limitation of our setting is our inability to cleanly distinguish the relative importance of specific due-diligence value-add mechanisms, which conceptually can be classified into two broad categories, although they are not likely to be mutually exclusive in practice. The first, which we refer to as *type improvement*, refers to the idea that by going through the VC due-diligence process entrepreneurs can improve their businesses as they learn-by-doing, gather and process feedback from potential investors, and gain access to new information and networks. The second channel, which we refer to as *type discovery*, refers to how selection for due-diligence can serve as a form of quality assurance that enables venture growth. For example, by improving access to market resources through certification—i.e., signaling quality to the market and reducing search frictions between VCs and entrepreneurs.[9] Type discovery can also constitute increasing entrepreneurial commitment and effort through validation—i.e., revealing entrepreneurs' true quality to themselves. [10]

While we have no exogenous variation to differentiate between the two mechanisms, the setting and results from auxiliary tests suggest that *type improvement,* rather than *type discovery*, is a primary mechanism. Interviews with the Fund partners reveal that they perceive *type improvement* to be the main mechanism given their commitment to providing substantive coaching to applicants regarding their go-to-market strategy and unit economics. The feedback from the Fund and the information gathered, and distilled, by virtue of going through the due-diligence process would improve the venture's framing of their pitch for future prospective investors, and their business strategy and execution. The Fund's founder is a well-known entrepreneur who had a high-profile exit and is well known in the community for knowing how to scale a business. Consistent with *type-improvement* effects we show that firms increase the number of technologies they use to build their product (as extracted from BuiltWith) in the 12 months following the application to the Fund (see also Koning, Hasan and Chatterji, 2019).

By contrast, a *type discovery* channel is less likely in this setting because while the Fund we study is their founders first investment fund, and was newly created at the time of study. Moreover, the Fund's due-diligence assignment decisions are privately informed to applicants rather than widely publicized. Therefore, while entrepreneurs can communicate their due-diligence selection to potential investors, assignment to the Fund's due-diligence is unlikely to primarily serve *type discovery* functions, in contrast to other publicly visible settings like business plan competitions (see Howell, 2020). Consistent with this idea, we find no evidence of validation or certification effects. Validation effects from the Fund's selection would lead to abandonment after due-diligence rejection. However, we find no robust effect of due-diligence assignment on venture survival. Evidence of certification derived from the Fund's selection would lead to stronger effects from due-diligence assignment for businesses with higher growth potential uncertainty, as measured by their founders' experience, stage of development and type of innovations. Instead, we find similar effects across businesses with serial vs. first-time founders, post-seed vs. pre-seed stages, and standard vs. deep technology innovations. In addition, we find no robust effects on web-traffic as certification effects would predict. Results from additional supplementary tests further support the expectation that *type discovery* is unlikely to play a main role in this setting.

[9] In the sense of matching models such as Inderst and Muller (2004), Sorensen (2007), and Ewens, Gorbenko and Korteweg (2018).

[10] Learning about entrepreneurial quality plays a pivotal role in many models of firm dynamics (see Jovanovic, 1982; Ericson & Pakes, 1995; and Berk et al., 2004).

Our findings contribute to two main bodies of literature. The first explores the role of VCs in innovation and economic growth more broadly. Most of this literature focuses on establishing the value-add of VC on their portfolio companies (Lerner and Nanda, 2020). There is evidence in this literature that due-diligence is essential for VC returns, both based upon survey results by Gompers et al. (2020), and more formally in Sorensen (2007). However, whether VCs add-value to innovation ecosystems more broadly, beyond their portfolio companies, remains understudied. Our results complement research showing that early-stage investors have local spillover effects (Samila & Sorenson 2011; Fehder & Hochberg 2019), and VCs have a disproportionate contribution on innovation (Kortum and Lerner, 2000; Gonzalez-Uribe, 2020). We provide a channel for this contribution, offering novel evidence that the due-diligence process by seed VCs – in its own right – positively impacts a wider set of ventures by helping resolve informational frictions for founders.

Our work also extends growing evidence of how networking frictions in the context of entrepreneurs seeking VC financing can act as real impediments to growth (cf., Hochberg et al., 2007; Lerner and Nanda, 2020; Howell and Nanda, 2021). We show that it is not necessarily networking opportunities in general (like the informal meetings with Fund), but rather, intensive meetings and information exchange, with the intention of early-stage funding, that can drive potential performance effects. Our work also complements new avenues exploring the impact of contextual and cognitive factors in shaping selection processes (e.g., Malenko et al., 2021; Dushintsky and Sarkar, 2021; Kahneman et al., 2021). We do this by examining the extent to which VCs' tendencies to provide high or low scores affect due-diligence selection.

The second literature we contribute to focuses on ventures' life cycle. Our results suggest that seed VCs may play a role in innovation ecosystems similar to other intermediaries seeking to systematize the coaching of inexperienced entrepreneurs, such as business accelerators (Hochberg, 2016; Gonzalez-Uribe and Leatherbee, 2016; Gonzalez-Uribe and Reyes, 2020). In the context of venture capital, research has acknowledged the growth of a so-called "spray and pray" strategy, in which early-stage VCs make a large number of small investments, at the expense of interacting more closely with founders, which lessens the potential value-add post-investment (Ewens, Nanda and Rhodes-Kropf, 2018). We highlight the potential increased value-add pre-investment by seed VCs and other early-stage investors from the growth of this strategy. Our results substantiate the business opportunity that accelerators and incubators are exploiting; providing feedback and connections to early-stage companies can deliver added value to the value and can be monetized.

The rest of this paper proceeds as follows. In Section 1, we describe the context and data. In Section 2, we detail the empirical strategy and present results. We discuss the interpretation of results and their external validity in Section 3. We present robustness checks in Section 4 and offer concluding remarks in Section 5.

1. Institutional Setting

In this section we start by providing a general description of the Fund and its applicants' data. We then describe the outcome data we collected to measure the ventures' post-application growth. Finally, we describe the Fund's selection process for due-diligence, which we exploit to build our empirical strategy as we explain in detail in the next section.

1.1. The Fund

The Fund is a seed fund managed by a UK-based VC firm established in November 2016, which began investing in portfolio companies in March 2017.[11] The Fund specializes in investing in early-stage ventures operating in the software sector, broadly defined. It is business-model agnostic within that sector, covering direct-to-consumer businesses, platforms and deep technology. As is increasingly common among seed funds, the Fund does online deal sourcing, relying on an online platform to receive applications for funding. This, the Fund contends, helps to democratize access to venture capital financing in the UK, by offering an open platform for application rather than entrepreneurs having to rely on social networks to get an introduction. By November 2019, the Fund had received nearly 2,000 online applicants, which constitute our analysis sample, and also, represents the end of the period in which the Fund was making new investments. While we cannot provide exact details of applicants to the Fund, some examples include companies seeking to advance the use of biometric data in security measures and to enable desk management in collaborative workplaces.

Also like other seed funds, the Fund's investment check size is between $50K-$5M, which attracts early-stage businesses seeking to raise seed capital before approaching more traditional VC funds for Series A investment.[12] These types of seed funds have become ever more prevalent in recent years (Klingler-Vidra, 2016). The significant fall in the costs of starting and developing ideas, especially in the software industry (for example, with the advent of cloud services by Amazon in 2006), has led to increasingly inexperienced founders seeking venture capital financing (Ewens, Nanda and Rhodes-Kropf, 2018). New intermediaries have emerged in early-stage entrepreneurial finance markets, including this new breed of early-stage VC, super angels and business accelerators, seeking to sort through the increasing noise in ventures looking for eventual Series A, and coach and gain early investment access to the most promising candidates.

[11] The Fund shared their data with us under a Non-disclosure agreement which prevents us from sharing more specific details about the setting.

[12] The average Seed stage investment in Europe was $1.9M in 2021, and the average Seed stage investment in the UK in 2019 was £0.57M. See https://www.bvca.co.uk/Portals/0/Documents/Research/Industry%20Activity/BVCA-RIA-2019, and see also https://assets.kpmg/content/dam/kpmg/uk/pdf/2021/04/venture-pulse-q1-2021.pdf.

Also similar to other seed funds, the Fund uses a systematic approach to screen applicants for due-diligence than more traditional VCs. As we explain in more detail in Section 1.4, the selection process of the Funds involves two steps. The first is the allocation of the online applications to three reviewers (internal to the firm) that score the submission and record feedback. As we explain more fully in Section 1.4, the matching of the applicant to the reviewers is orthogonal to the quality of the application or the applicant. Reviewers' comments are later shared with founders, along with the selection outcome, via email.

The second step in the selection process is the aggregation of scores from the three reviewers according to some pre-determined rule unbeknownst to applicants, which varies over time and by location. After these two steps, the Fund classifies applicants into three buckets: (1) further due-diligence "due-diligence"), (2) informal meeting that is not part of the due-diligence process, and so not leading to a potential investment ("informal meeting"), and those the Fund will not meet because they are deemed non-venture-backable ("no meeting").

While this selection method is specific to the Fund, similar selection rules are commonly used by seed VCs. Moreover, traditional VCs have been increasingly employing voting systems in order to reduce the role of bias in scoring, and in the hope of increasing the chances of investing in "unicorns" at the early stage (Malenko et al., 2021). The Fund is representative of this trend, as it employs a scientific approach to decision making, one that relies on the quasi-randomization of reviewers across applicants. As we explain in detail in Section 2, this quasi-randomization is helpful for us as researchers, and it forms the basis of our empirical strategy for testing the impact of due-diligence on venture performance.

Finally, like traditional VC firms, the Fund engages in an intense due-diligence process for the group of companies that pass the initial pre-screening filter. The first step in that process is inviting the selected founders to meet. One of the applicant's reviewers acts as the "Investment Lead," sending a template email (see Appendix 1), asking for more information, and meeting the founders. The second step includes further scrutiny by other members of the team if the Investment Lead continues to be enthusiastic after the meeting. The third stage involves a more formal investigation (referred to as "Opportunity Assessment" by the Fund) that includes the applicant sharing a "data room", delivering a pitch to the Investment Committee, and the Fund hiring industry experts to complete external reviews, in order to validate the venture's claims and assumptions. Candidates that pass all three stages are presented with a term sheet summarizing the Fund's conditions for potential investment. Finally, the company agrees to the term sheet (or negotiates changes to the terms, that are agreed by the Fund), and the deal closes.

Figure 1 shows the Fund's selection funnel. By November 2019, roughly 30% of applicants had been assigned to due-diligence, less than 3% had made it to Opportunity Assessment – the final stage due-diligence – and only 0.6% had secured funding from the Fund. The contours of this funnel are broadly consistent with findings elsewhere, which depict VCs as sourcing ratios that correspond to 100 potential companies for investment, conducting due-diligence (starting by meeting founders) on 30% of those companies, and ultimately investing in only approximately 1% of the 100 companies (Zider, 1998; Gompers et al., 2020).

1.2. Application data

The Fund provided us with all the application data, including application scores assigned by each reviewer and the final selection decisions for each applicant. Our sample consists of all the 1,953 applicants seeking capital from the Fund during the March 2017 to June 2019 period.[13] Figure 2 shows the number of applications made each month. At the peak month, the number of applications was 140.

Based on the applications, we constructed several variables to use as controls in our empirical strategy: applicant's location, age of company at the time of application (relative to incorporation date), target amount to raise, funding stage (pre-seed, seed, or post-seed), business type (direct-to-consumer, platform, and deep technology), and also, founders' personal characteristics (e.g. gender, education). Table 1 reports summary statistics for the main variables in the application forms. On average, applicants have been incorporated for 2.61 years at the time of application and aim to raise an average of £1.6M. In terms of gender, 13% of the applicants include at least one female founder. Figure 3 shows the location, stage and business type breakdown: 47.86% are in London, 45.27% are at the seed-stage, and roughly half are categorized as direct-to-consumer businesses and half are platform businesses, with only a minority of applicants in deep technology. The average number of founders per venture is 1.94.[14]

Although self-selection of companies applying for funding online suggests a (lower) degree of sophistication possibly related to their probability of success and subsequent performance, other factors may play a role. For example, companies with founders with prior VC fundraising and exit experiences are less likely to apply for funding through an online platform because they can reach out to their previous investors. Consistent with this idea, we show that applicants to the Fund are comparable to the average company securing seed financing in the UK, but appear smaller at the median.[15] The average

[13] The Fund was founded in November 2016. We use data staring on March 2017. This period of time represents two things. First, the remainder of the time it took to close the fund (e.g. raise money from limited partners). Second, in the first months, as the Fund structure was finalized, there was no systematic record keeping of applicants or selection process.
[14] This information is not provided by entrepreneurs in their applications but we sourced it form Crunchbase. We found1,178 ventures and 2,286 founder and co-founders. So the average number is 2286/1178=1.94.
[15] A total of 257 companies raised seed funding in the UK in 2019 accoridng to Crunchbase and Preqin. We were able to match 169 of those by name and location to companies that reported total assets in 2018 with Companies House. Thus, this comparison to UK seed funded companies is based upon the information we collected from Companies House for 169 ventures in the information and technology sector (the sector which all of the applicants to the Fund are also operating in) that according to Curnchbase and Preqin raised seed funding in 2019 in UK.

asset size for companies securing seed financing in the UK is £492K, which is slightly smaller than the average in our sample of £641K. However, at the median, our applicants look much smaller, with £23K in assets, relative to a median asset size of £184K for companies that secured seed in funding (consistent with the seed and pre-seed stage of the applicants).

1.3. Outcome Data

We use two complementary strategies to collect outcome data for the Funds' applicants.

First, we collect novel administrative data for applicants incorporated in the UK, which are most of the ventures applying to the Fund (80% of all applicants). These data come from the business registry in the UK (Companies House; "CH") and includes information on registration, survival, liquidation, and annual equity issuance, assets, and debt. The UK registry includes this information because UK ventures submit mandatory annual financial accounts, albeit abridged relative to larger firms. While larger firms must include more detailed information on balance sheet accounts, employment data, and income statements in their filings, smaller companies are exempt.

Using these data, we track annual outcomes during the years around the applications from 2017 to 2020. Because the average applicant applied in 2018, and the latest administrative records were extracted in 2020, all outcomes measure performance within an average of 1.90 years since application. Access to administrative data on a venture-specific basis represents a significant advantage relative to most other work in the VC literature.

We construct the following outcome variables from CH filings: log equity issuance, log number of directors appointed, log growth in assets, log growth in debt, and company survival and liquidation in the sample period before and after application, separately.[16] Survival is an indicator variable that equals one if the company did not file for liquidation, closure, or dormancy after application by 2020. Liquidation is an indicator variable for companies that filed for liquidation after application and by 2020. Note that liquidation is not tantamount to bankruptcy in the UK as solvent companies also file liquidation paperwork to winddown their company (see Balloch, Djankov, Gonzalez-Uribe, and Vayanos, 2022). Directors include all individuals with a C-level job in the company, e.g., Chief Executive Officer (following UK terminology). As is common among early-stage ventures, outcome variables are highly skewed. So, we rely on logarithmic transformations of the variables (after adding 1) to implement the regressions. In addition, for better interpretation of the regression coefficients, we focus on the gap between the median and the 75th percentile, which we report in the last rows of the tables for reference.

[16] In the regressions using log equity issuance, we include the pre-application log equity issuance as a control.

Our second strategy for collecting performance data follows the standard practice in the VC literature to measure venture performance using web-sources like Crunchbase and LinkedIn, as these sites' coverage is likely to be better for seed-stage companies with no institutional investors relative to later-stage data vendors' sources like Pitchbook, Preqin or VentureSource.[17] We construct the following outcome variables: funding, number of employees, number of funding rounds, number of investors after the application. Given their skewness, all outcome variables are added with 1 and logaritmized to implement the regressions. We can cover all applicants using this method, rather than UK businesses only as in the first method.

We also collect founders' educational backgrounds and previous work experiences from their LinkedIn profiles whenever available, and supplement this information with co-founders work experiences from their Crunchbase webpages.[18] In terms of education, we code whether founders have completed tertiary education (e.g. Bachelor's degree) from an elite university. Since most of the applicants in our sample are UK companies, we operationalize elite university according to the Russell Group (e.g. top 20 UK universities) and the "Golden Triangle" (Oxford, Cambridge, UCL, LSE and Imperial) sets of universities. We also code and group universities according to 2020 global rankings, including Times Higher and ARWN (Academic Ranking of World Universities).

Collectively, our data collection strategies comprise us having information on funding from administrative data (Companies House) and web sources (Crunchbase and LinkedIn) for the applicants in the UK. However, we note that the two variables are not directly comparable for several reasons. The administrative data includes equity sources other than specialized financing like VC. In contrast, the Crunchbase data mainly includes investments made by angels (though possibly not all), venture capitalists, private equity, and exit events (e.g., IPOs or acquisitions). Further, any rounds involving the use of convertible instruments are not recorded as equity issuance (until conversion) in Companies House but rather as debt, which we look at separately in the data (see Gonzalez-Uribe and Paravisini, 2018). Information from the two sources also possibly captures different periods post-application. Crunchbase data is updated continuously. Instead, companies file administrative data yearly asynchronously, implying that for some applicants, we have only one filing post-application. However, we can cross-check self-reported funding online with that in the registry to gauge the degree of potential

[17] Howell (2020) focuses on interim performance indicators, through data gathered via CB Insights, CrunchBase, LinkedIn and AngelList, rather than on ultimate exit (IPO, trade sale, or other) returns. Similarly, Ewens and Townsend (2020) use Crunchbase for information on further funding as "Crunchbase's coverage is likely to be better than VentureSource for seed rounds with no institutional investor." Hu and Ma (2020) also collect data on ventures using Crunchbase and PitchBook. Gonzalez-Uribe and Leatherbee (2017), Yu (2020), Hallen, Bingham, and Cohen (2016) also study the impact of accelerators by collecting venture performance and founder backgrounds from venture's websites, LinkedIn, Amazon Web Services, AngelList, and Crunchbase.

[18] We extract higher education backgrounds for 1981 founders who provide their education information on LinkedIn webpages. We then combine 1801 founders' working experience from LinkedIn pages and 2092 founding team members' work experiences from their Crunchbase personal webpages.

selectiveness in reporting to online repositories. We find little evidence of selective posting (correlation between the two variables is 0.39), which mitigates data quality concerns from the web variables and lends credence to the analysis relying on online data for the companies incorporated outside the UK.

Table 1 reports summary statistics of the outcome variables. The average (median) assets post-application are £1,066K (£86K). The average number of employees is 6.09, and the average number of directors appointed post-application is 1.03. The average survival rate and average number of investors post application are, respectively, 0.81 and 1.02. Post application, average (median) total funding and equity issuance is £1,330 K (£0), £385 (£0), respectively.

1.4. Due-diligence Process

In this section, we review the Fund's process to sort applicants for due diligence, and describe the engagement of the Fund staff with the applicants during the due-diligence process.

1.4.1. Reviewer assignment

The first step in the due-diligence process is the assignment of three reviewers to each online applicant. Reviewers are internal to the Fund and are founding and managing partners (four out of 12), partners (four out of 12), and Associates (four out of 12). [19]

There are 12 reviewers in our data, including three female reviewers. The average (median) number of applicants assessed by a single reviewer is 400 (566), and the minimum (maximum) is 30 (796). Therefore, the way to think about the data is as comprising relatively few reviewers, but where each reviewer evaluates a relatively large number of applications. Appendix 3 details the distribution of applications across reviewers and reviewer trios.

The assignment of applications to reviewers is done using proprietary software developed by the Fund for collaborating and managing spreadsheet-like inputs.[20] The software assigns application numbers to incoming applications, and classifies them according to the location of the business as self-reported by the applicants. There is a total of 16 regions, following the standard 12 region and nations classification

[19] The compensation of the Fund's staff is not directly tied to their reviews. In addition, prior to our analysis, there was also no introspection by the Fund in terms of reviewers' scores. All investors have carry, with the Managing Partners (who form the Investment Committee) having a greater share of carry. The carry structure would suggest that staff would not disregard offhand reviewer duties. Moreover, the three reviewer system can also provide incentives for judicious assessment: as explained in more detail below, one reviewer acts as investment lead collating all scores, meaning that a reviewer's scores of a given applicant are seen by at least one other member of the Fund (if the reviewer is not the investment lead). We exclude scores provided by trainees and temps, which do not count for the Fund's selection.

[20] The Fund originally used Zapier to manage the reviewer allocations. But, eventually developed their own proprietary software to manage reviewer allocations.

of the UK, plus a further breakdown to best reflect local entrepreneurship clusters, and non-UK applicants.[21]

The software automatically assigns three reviewers to each applicant based on their location and reviewers' workload: staff are temporarily taken off the review assignment if they go on holiday or are busy with other tasks, like completing other funding deals. In addition, the system prioritizes allocations to reviewers that have as regional focus the location of the applicant; six out of the 12 reviewers have a regional focus and act as an investment lead for different regions, which vary from single cities (e.g., Cambridge) to larger areas (e.g., Southwest of England, Scotland). The majority of regions (10 out of 16) have at least one designated investment lead. However, a "regional focus match" between applicants and reviewers is neither sufficient nor necessary for an assignment.[22]

In addition, the software also determines an "Investment Lead" among the three assigned reviewers. The Investment Lead oversees the assessment of the other two reviewers and chases them to complete their reviews within the Fund's 24-hour turnaround goal. The Investment Lead then collates the reviewers' assessments and communicates the decision to the applicants, as we explain in more detail in Section 1.4.3. The software prioritizes assignment of the Investment Lead role in line with the company's region, but availability constraints meant that in practice the regional match is not universal among applicants.[23]

The other two reviewers in a trio cannot see the co-reviewer's assessment through the review software (Airtable), although it is possible, they learn about it; we discuss how we address this possibility in our methodology in Section 2. From a practical perspective it is worth noting that the reviewers do not share an office, which lowers the probability of coordinating reviews, as the Fund chose early on to not have a permanent office. Instead, their intention is to "be on trains" around the country so that they could be a presence and network outside of London, and their model involves a combination of working-from-home and hot-desking in various co-working spaces.

[21] The 16 regions are Cambridge, East Midlands, East of England, London, Non-UK, North East, North West, Northern Ireland, Republic of Ireland, Scotland, South Central, South East, South West, Wales, West Midlands and Yorkshire and the Humber.

[22] The six reviewers with a regional focus also evaluate applicants from all regions. The effective pool of reviewers is 12 for most locations (9 out of 16; 56.3%), 11 for 6 out 16 locations (37.5%), and 10 for 1 out of 16 locations (6.25%). The regions with 11 reviewers in the pool are: East of England, Non-UK, North East, Northern Ireland, Scotland, South Central. The region with 10 reviewers in the pool is Wales. For regions with designated investment leads, the average number of companies reviewed by an investment lead (i.e., a reviewer focused on that area) is 70% (Cambridge has the minimum with 37% and London is the maximum with 94%). The six regions wth no investment lead are: East of England, Non-UK, Republic of Ireland, South Central and South East, and Yorkshire and the Humber.

[23] For regions with designated investment leads, the average number of companies with an investment lead that has a regional focus is 64% (Cambridge has the minimum with 23% and Scotland is the maximum with 86%).

The automatic assignment means that the Fund does no deliberate assignment of applicants to reviewers on the basis of characteristics of applications other than location (on which we can condition). The Fund aims to balance the potential selection advantages of reviewer specialization –in terms of regional focus only – with the potential bias reductions of arbitrary (and multiple) assessments. One key conclusion from this institutional context is that effective random assignment of applications to reviewers conditional on location is plausible. Consistent with this assumption, we show in Appendix 3 that conditional on location, the sample of applicants is balanced across reviewers.

1.4.2. Reviewers' scores and comments

Each reviewer observes the information in the application, and based on that, provides a score and annotates optional comments using the Fund's software.

Scores are discrete numbers ranging between 1 and 4, where 4 is best. Scores are not shared with applicants, but they are a crucial input for due-diligence assignment as we explain in detail below (Section 1.4.3). There is substantial scoring heterogeneity across reviewers. We now summarize the results from our methodology to show this heterogeneity, and we present full details in Appendix 4.

We construct a dataset with reviewer scores as the unit of observation (so three observations per company) and regress the scores against applicant and reviewer fixed effects and controls for location. We strongly reject the hypothesis that the reviewer fixed effects for the different reviewers are the same (p-value<0.01). In terms of economic magnitude, more generous reviewers are twice as likely to provide a score of 3 or 4 relative to stricter reviewers (as measured in terms of positive and negative reviewer fixed effects, respectively). We run several checks to make sure that the heterogeneity tests are not spurious, using the methodology in Fee, Hadlock and Pierce (2013). Consistent with the quasi-random assignment of reviewers, we show in Appendix 4 that applicants assigned to more and less generous reviewers look very similar based on observable characteristics at the time of the application.

The scoring generosity of reviewers is unrelated to their skill in selecting applicants as we show in Appendix 4. We rank each reviewer's applications according to their scores and separately according to subsequent funding performance (see Section 1.3 for more details on outcome data). We measure skill as the correlation between those two ranks. The relation between generosity and skill is nil (-0.039; p-value 0.642) across reviewers; albeit with the caveat that the correlation is estimated with 12 observations.[24] In unreported analysis, we also show that the scoring generosity is also unrelated to

[24] This is not to say that reviewers have no skill at discerning applicants' potential and predicting growth. In unreported regressions, we regress applicants' performance against the firm potential proxy, controls for due-diligence, opportunity assessment and investment, and other controls like characteristics from the application and location fixed effects. The firm potential proxy is highly predictive of subsequent performance, which substantiates the idea that reviewers can discern applicants' potential.

characteristics of the reviewers like their gender, geographical focus, or seniority (as measured by job position: founding and manager partner, partner and associate).

The optional comments annotated by reviewers play no independent role in due-diligence assignment—only reviewers' scores determine the Fund's due-diligence selection, as we explain in detail in Section 1.4.3. Yet, the comments are shared with applicants via email as we explain below (Section 1.4.4).

Not all applications have comments (159 have no comments), yet the majority have comments from the three reviewers (88%;1,727/1,953). At the reviewer level, 11 out of the 12 reviewers annotated comments for at least one application. The average fraction of applications per reviewer with comments is 88.72%.

Comments are usually short with a mean of 55 words in length (roughly two phrases), and the maximum comment has 30 words. Comments are on average neutral in tone, not exhibiting a clearly positive or negative sentiment. We use natural language processing (NLP) techniques to analyze comments' content as we explain in detail in Appendix 5.

The content of the reviewers' comments is unrelated to their scoring generosity. Appendix 5 shows that more generous reviewers, relative to less generous reviewers, have equally-toned comments, although they are on average shorter.[25]

1.4.3. Aggregation of Scores: Selection Rules

The second step in the selection process is the aggregation of the three reviewers' scores according to a pre-determined selection rule that varies over time and by location. Before May 2018, the Fund used the same selection rule for ventures headquartered in any location. Beginning in May 2018, however, applicants for London faced a stricter selection rule than applicants elsewhere. The Fund changed selection rules in response to internal discussions regarding its investment thesis as part of their first investment year review. Senior partners perceived a need to treat entrepreneurs located outside London differently, to improve their chances of making it to due-diligence, and ultimately, investment. Their perception was that UK VC money chases too few deals outside of London given the inconvenience involved in scrutinizing potential deals. Therefore, talented entrepreneurs outside of the capital remained underserved by specialized financiers, which echoes the well-known local preference of VC investors (Lerner, 1995; Bernstein, Giroud and Townsend, 2016).

[25] This is not to say that reviewers exhibit no heterogeneity in comments' style. Appendix 5 shows joint significance of reviewer fixed effects in specifications regressing comments' content measures against reviewer and company fixed effects. Yet, this heterogeneity in comments' style is uncorrelated to the scoring heterogeneity across reviewers.

Figure 4 shows the selection rule for all the potential combinations of scores for the three distinct selection regimes: (1) Pre-May 2018, (2) Post-May 2018-London, and (3) Post-May 2018-Outside London. To illustrate the workings of the selection rules, consider the example of London Post-May 2018. The selection rule in that regime is the so-called "Champion Model" (Malenko et al., 2021) where the Fund only assigns to due-diligence the applicants with a top score of "4" by at least one reviewer. Any other combination of scores does not lead to due-diligence assignment, even among score combinations with equal average scores but without a "4". For example, a score combination of {1 2 4} has the same average score (2.33) as the combinations: {1 3 3} and {2 2 3}. Yet, neither alternative score combination leads to due-diligence assignment under the Post-May 2018 London regime. We note too that the only combination of scores that leads to no meeting is {1 1 1}; all other score combinations lead to either the offer of an informal meeting or to enter into the due-diligence process. The Fund considers {1 1 1} companies as non-venture backable, given the aims of their Fund's investment mandate. Reasons for this determination are small size of market opportunity, the insufficient sophistication of the business, and/or the lack of technological talent (e.g. plans to outsource the Chief Technology Officer function).

Figure 5 shows the distribution of score combinations across distinct selection rule regimes. There are two main takeaways from the figure. First, specific scores are popular regardless of the regime—for example, {2 2 2} is always the most popular score across regimes. Second, the distributions of score combinations in the three regimes are similar, even though the selection outcome (due-diligence, informal meeting, and no meeting) for specific scores varies across regimes. The patterns in the plot thus suggests that the scoring behavior of reviewers is independent of the selection rule. Kolmogorov-Smirnov tests show there is no significant difference in the scores' distributions between applications before and after the change in selection rules, nor between London and non-London applications (see notes in Figure 5). We note that this pattern is not mechanical as reviewers are aware of the selection rules. Rather, the pattern is likely a manifestation of the persistence of the underlying heterogeneity in scoring across reviewers we discussed in the previous section and that we detail in Appendix 4.

1.4.4. Communication of the Due-Diligence Selection to Applicants

After aggregating the reviewer scores by applying the corresponding selection rule, the Fund communicates the result of their assessment to applicants. This communication occurs via email, with the reviewer acting as Investment Lead overseeing compiling and sending the email, following up, and, if relevant, meeting the founders. The Fund is strict with rule compliance: no informal meeting ever converted into further due-diligence; the informal meeting is considered a gesture of good will, and not a predecessor to future investment consideration. However, the Fund does accept reapplications. Although in practice, reapplications are rare occurrences: 129 companies (6.6% of the sample) reapplied; we only keep the first application in our sample and can confirm that all those who received "no meeting" or "informal meeting" in their first application did not later move to "due-diligence" in their second application.

The correspondence with founders uses three standardized email templates; see Appendix 1 for full transcripts. The wording used in the email is precise about the application's result, and whether the founders get to meet the Investment Lead, and the expectations for that meeting.[26] No email includes individual or average scores or the names of the reviewers. While the Investment Lead signs the email, the applicants are unaware that the signer is part of the reviewing team. No email includes details on the selection rules either (which are also not available online nor shared outside the Fund). The emails include a general description of the sorting method only.[27] Finally, all email templates include a copy of the reviewers' comments, without an indication of who the reviewers are. As the Fund explained to us, the Investment Lead compiles a "top and tail" for the email message that goes out to the founder(s) with standard text above and below, and then the three reviewers' comments are included "as is" in the body of the message.

1.4.5. Due-diligence process

For the companies selected for due-diligence, the first step of the process is to formally meet with the investment lead. The meeting with the investment lead can also include other members of the VC fund's team, and typically takes place in person and lasts for 45 to 60 minutes. The initial due-diligence meeting involves talking through the materials included in the application, including the unit economics (e.g. the cost of sale per unit), the scalability of the business, the revenue growth model, and key performance indicators (KPIs) and objectives (see Appendix 2). This first meeting marks the beginning of what the Fund calls "Discovery."

There are internal and external elements to the discovery stage of the due-diligence process. The internal aspects include a review of the startup's finances, legals, team, and technology, and sometimes a regulatory review if the business area is deemed to have notable regulatory risk. Through the meetings

[26] The no meeting email reads "… *We've completed our initial assessment and have concluded **we're not currently the right investor** for you…*" The informal meet email reads "… *We've completed our initial assessment and have concluded **we're not currently the right investor** for you**. However, we would like to meet** to share our feedback with you directly, learn more about your venture, and stay in touch ahead of your next raise. Would {suggested day and time} work for you for a call or coffee*?..."By contrast, the further due-diligence email reads "…*We've completed our initial assessment and would like to meet to **take our review further**. Would {suggested day and time} work for you for a call or coffee?...".*

[27] The following is an excerpt taken from the standardized email templates *"… We approach our initial review with the belief that any startup could be a generation-defining business. In order to surface those opportunities, we believe three separate minds are better than one. Three of our team members, including two Investment Leads and a member of the Executive Team, independently review the materials you've shared to consider whether we are the right investment partner for you at this point of the journey. We aim to get this initial review done and share our feedback within a couple of days of receiving a full submission. We move forward if any one of the reviewers sees enough potential in the opportunity…."*

that comprise this stage of the process, members of the Fund will review with the founding team the product's unique value proposition as well as the capitalization table ("cap table"), including the equity stakes held by existing investors, the value of equity in the proposed round, and stakes held by key members of the team. The external discovery elements comprise external experts in a given market, sector or skill area being asked (by the Fund) to meet with the founder(s) and then write a brief report assessing the strength of the technology, their market assumptions and evidence of growth and opportunity. Through the internal and external engagements with the startup, the Fund asks the founder(s) to detail numerous aspects of the business, including a 10-year view on their unit economics as well as their uniqueness relative to other companies in the space. There are typically three to five meetings of the founder(s) and Investment Lead (as well as other members of the team) in order to learn about and assess the investment opportunity, in which members of the Fund ask probing questions and the founder(s) collate and present responses (e.g. by giving access to their "data room" with details of financials, intellectual property, human resources, and more).

Assuming that the Discovery phase concludes satisfactorily, then the Investment Lead prepares the company to meet with the Investment Committee. Legal, operational and technical due diligence questionnaires are completed in advance of the Investment Committee. Other items on the investment checklist are also gathered, such as the current cap table. Ahead of the Investment Committee meeting, the Investment Lead fills out an opportunity assessment form, and completes a review of the material (following the same thought process and criteria as the initial selection process but including the results of the Discovery phase). The full set of materials are reviewed by each voting member of the committee ahead of the Investment Committee meeting with the startup.

The Investment Committee meeting represents the opportunity for companies to formally present to the members of Fund's leadership. The Investment Lead also attends and can participate in the discussion section both with the company and in the Investment Committee-only discussion. The Investment Committee format is a 20-minute pitch from the company followed by a 40-minute discussion with the Investment Committee (including non-voting members where appropriate). Then there is a discussion, up to 30-minutes, without the company, in which the Investment Committee reviews the opportunity assessment scores and the new review score, which was assigned upon completion of the Discovery phase. The Investment Committee members each individually write up their investment recommendations, which are then considered when making the final investment decision. The Investment Committee vote requires a 2 of 3 majority to pass. Further proposals may be made (different terms including amount and equity proposed), and conditions precedent may be set (e.g. discussion of team hires, other investors' behavior or commitment, engagement with particular types of support etc.).

After the Investment Committee has concluded, the Investment Lead shares the outcome with the founder(s) by phone or in person. If the result is a "pass", meaning that the Fund will not make an investment, detailed feedback is given in terms of areas that raised concern and where adjustments should be made to the business model, product, team, etc. If the Investment Committee votes positively, meaning that they decide to offer a term sheet, then the rationale for the terms offered are communicated and a negotiation around the deal terms ensues.

2. Empirical Strategy

This section explains how we exploit the selection process of the Fund to build an instrumental variables (IV) strategy to assess causal effects of the Fund's due-diligence.

2.1. Baseline Specification

The final dataset is a cross-section where the unit of observation is an applicant *i* to the Fund. We present results including and excluding the companies eventually selected for investment by the Fund (12 companies; 0.61% of the applicants).

Our baseline specification measures the correlation between the assignment to due-diligence and the venture's subsequent performance. We estimate the following type of regression:

$$Y_i = \gamma + \rho Due\ diligence_i + \boldsymbol{Z}_i + \varepsilon_i \qquad (1)$$

Where Y_i is the post-application outcome for applicant i, $Due\ diligence_i$ indicates the companies assigned to due-diligence and $\boldsymbol{Z}_i$ is a vector of controls at the time of the application including the amount raised pre-application, and log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. We condition on location fixed effects in all specifications. Robust standard errors are reported throughout.

The coefficient ρ captures the effect of the Fund's due-diligence assignment and subsequent venture performance. When $\rho > 0$ we conclude that the Fund's due-diligence adds value to entrepreneurs by increasing venture performance.

The major empirical challenge is that due-diligence selection by the Fund is endogenous. For example, a promising applicant with a high-potential business idea may attract venture capital (from other VCs) and grow, and at the same time, be chosen for due-diligence by the Fund. This endogeneity would generate a positive correlation between ε_i and $Due\ diligence_i$ in equation (1) and an upward bias to the estimate of ρ.

2.2. Identification Strategy

To address potential endogeneity, we need an instrument that affects the likelihood of due-diligence assignment but does not affect the venture performance through any other mechanism.

To construct such an instrument, we exploit the two features of the Fund's selection process as explained in Section 1: (1) the quasi random assignment of applicants to three reviewers and (2) the aggregation of reviewers' scores using pre-determined selection rules. As discussed in Section 1, there is substantial variation across reviewers in scoring generosity. Together with the quasi-randomization of reviewer trios, this feature of the process is the basis for the first source of exogenous variation in due-diligence assignment that we exploit for our identification strategy. The second source of exogenous variation is the pre-determined selection rules that the Fund uses to aggregate scores, and which change over time (pre and post 2018) and location (London or outside London), as explained in Section 1.

Our instrument combines both sources of variation in order to estimate the exact probabilities of due-diligence for every applicant. It takes into account the fact that the selection decision is based on the aggregation of the three reviewer scores, so the impact of each reviewer's generosity depends on the other reviewers in the reviewing trio and the selection rule valid for that application. For example, the instrument will correctly capture how the random assignment to a reviewer that tends to provide top scores binds the most when the other two reviewers tend to offer low scores. It will also capture how such an assignment will also bind more when the selection rule that aggregates the three reviewers' scores over-weights top scores, as under the "Champion model" commonly used by VC firms (Malenko et al, 2021).

In detail, we estimate our instrument, the "Due-diligence Assignment Probability" (DAP), for each applicant *i* as:

$$DAP_i = \sum_{s_1 \in \{1,2,3,4\}} \sum_{s_2 \in \{1,2,3,4\}} \sum_{s_3 \in \{1,2,3,4\}} p_{1(-i)}^{s_1} p_{2(-i)}^{s_2} p_{3(-i)}^{s_3} f(s_1, s_2, s_3) \qquad (2)^{28}$$

Where $f(s_1, s_2, s_3)$ corresponds to the selection rule used by the Fund to aggregate the scores of the three reviewers. The variable $p_{h(-i)}^{s_h}$ corresponds to the fraction of applications assigned a score of s_h by reviewer *h* of applicant *i*, excluding applicants *i's* score from the computation. For example, if the first reviewer of applicant *i* assessed 10 applicants other than *i*, and the reviewer assigned a score of 2 to four of those applications, then $p_{1(-i)}^{2} = \frac{4}{10} = 0.4$.

Note that by design the score for applicant *i* does not enter into the computation of its instrument for due-diligence assignment, thus removing the dependence on the endogenous regressor for applicant *i*

[28] $DAP_i = p_{1(-i)}^{1} p_{2(-i)}^{1} p_{3(-i)}^{1} f(1,1,1) + p_{1(-i)}^{1} p_{2(-i)}^{2} p_{3(-i)}^{1} f(1,2,1) + \cdots + p_{1(-i)}^{4} p_{2(-i)}^{4} p_{3(-i)}^{4} f(4,4,4)$.

(as in the jacknife IV of Angrist, Imbens and Krueger, 1999). This feature of our instrument allows us to control for any additional effects that applicant-specific unobservables may have on the decision to select the business for due-diligence. To be sure, by dropping the review of applicant i from the construction of the DAP instrument for applicant i, any additional information revealed during the assessment of the reviewers (e.g., web page searches about the company during the review process) or any discussions among reviewers about the applicant (for example, potential collusion, or influence by senior staff if reviewers figure out the identity of co-reviewers outside of Airtable; see Section 1.4.1 for a discussion on the low probability of this event) is removed from the instruments' construction and thus does not contaminate it.

There is substantial variation in the distribution of DAP (mean of 0.22, range from 0.00 to 0.78). Figure 6 shows the distribution of DAP across the sample of applicants.

Our main estimation approach instruments due-diligence assignment with DAP. In robustness checks, we also present results using the predicted probability of assignment obtained from the probit model $\widehat{DAP} = P(DAP, Z)$ as the instrument for due-diligence assignment. When the endogenous regressor is a dummy, as due-diligence in our case, the estimator $\widehat{DAP}$ is asymptotically efficient in the class of estimators where instruments are a function of DAP and other covariates. However, the linear model has the advantage of facilitating the interpretability of the estimates when we include controls in our regression like location fixed effects.

Specifically, we estimate the following two-stage model:

$$Due\ diligence_i = \mu + \beta DAP_i + \boldsymbol{Z}_i + e_i \quad (3)$$

$$Y_i = \theta + \alpha Due\ \widehat{diligence}_i + \boldsymbol{Z}_i + \omega_i \quad (4)$$

where the set of controls $\boldsymbol{Z}_i$ is the same in both stages and is the same as in equation (1). We condition on location fixed effects in all specifications to control for the level of randomization (see Section 1.4.1). We report heteroskedasticity-robust standard errors of our estimates. In unreported analysis, we show that results are robust to using bootstrapped standard errors.

The coefficient of interest is α which estimates the Local Average Treatment Effect (LATE) of due-diligence assignment for applicants whose treatment is affected by DAP. The conditions necessary to interpret these two-stage least squares estimates as the causal impact of due-diligence assignment are: (i) that DAP is associated with due-diligence assignment (i.e., first-stage), (ii) that DAP only impacts venture outcomes through the due-diligence assignment probability (i.e., exclusion restriction), and (iii) that applicants assigned to due-diligence by a low DAP would also have been assigned to due-diligence had they had a higher DAP(i.e., monotonicity). We now show supportive evidence for each of these conditions in our data.

2.2.1. First Stage

Unconditionally, the probability of due-diligence assignment is twice as high for companies with above-median DAP (41.6 vs. 21.3%). To examine further the first-stage relationship between DAP and due-diligence assignment, we start with visual evidence and then summarize equation (3) estimates showing healthy first-stage F-statistics.

Figure 7 provides a visual representation of our first stage. Figure 7 shows that for any level of applicant potential, applicants with above-median DAP have higher or equal probability of due-diligence assignment. Figure 7 ranks companies in the x-axis according to their potential as measured by the applicant fixed effect we estimated in the fixed effects models explained in Section 1.2. Recall that the applicant fixed effects proxy for the applicants' potential as perceived and agreed by reviewers at the time of application (once the scoring heterogeneity across reviewers is removed from their scores). Figure 7 also shows that the DAP has a stronger impact on due-diligence assignment for higher potential applicants, as revealed by the vertical difference between the due-diligence assignment curves for above- and below-median DAP. The DAP is less likely to affect the due-diligence assignment of the very bottom applicants, as these are clear cases that the Fund rejects as non-venture backable. Instead, the DAP is more binding for companies that stand a chance of selection given their perceived potential according to reviewers.

We formally test the relevance of DAP using the standard first-stage F-tests of the excluded instruments (Stock and Yogo, 2005). Table 2 summarizes results from several specifications of equation (3), including different models (linear, Panel A; probit, Panel B), samples (full and excluding portfolio companies) and combinations of controls as specified in the bottom rows of each panel.

There are two main takeaways from Table 2. Across all specifications, the coefficient of DAP is positive and statistically significant, and the F-test of the excluded instruments is above the rule of thumb of 10. In terms of economic magnitude, our most conservative estimate of 0.94 in column 5 implies that a 10 percentage point increase in DAP is associated with a 9.4 percentage point increase in the likelihood of due-diligence assignment. In terms of standard deviations, the coefficient in column 5 implies that an increase in one standard deviation of DAP, increases the due-diligence assignment probability by 0.27 standard deviations.[29] We obtain similar results using a probit model (Panel B) —the implied marginal effect from the probit regressions in column (5) is 0.85—which is unsurprising given that the mean of due-diligence assignment is 0.31 and far from zero and one.[30]

[29] 0.27=0.94×0.13/0.46, where 0.13 is the standard deviation of DAP and 0.46 is the standard deviation of due-diligence.

[30] A marginal effect of 0.85 implies that a one standard deviation increase in DAP (0.13) is associated with an increase of 11 percentage points in the likelihood of due-diligence (0.85×0.13=11.05%). This economic magnitude is comparable to that in Galasso and Schankerman (2014).

2.2.2. Exclusion restriction

The institutional details discussed in Section 1.4.1 suggest that the assignment of applicants to reviewers is plausibly random conditional on location fixed effects, lending some a priori credibility to the conditional independence assumption. Figure 8 and Table 3 provide additional evidence in support of the assumption that DAP is as good as if randomly assigned. Figure 8 shows a flat relationship between DAP and company potential, as measured by the applicant fixed effects estimates in Section 1.2. Table 3 shows indistinguishable applicant characteristics across different quartiles in the DAP distribution.

The conditional independence assumption is sufficient for causal interpretation of the reduced form results reported in Appendix 6. That is, our reduced-form estimates can be interpreted as the causal impact of being evaluated under a more or less stringent standard (i.e., as measured by the reviewers' generosity and the selection rule).[31] This assumption, however, is not sufficient for a LATE interpretation of the two-stage least squares estimates. For such an interpretation, we would require the exclusion restriction assumption to hold—i.e., DAP impacts applicants' outcomes exclusively through the single channel of due-diligence assignment, and not through any other mechanism.

This exclusion restriction would fail if the outcomes of applicants with a high DAP were affected in some additional independent way other than through an increased likelihood of due-diligence assignment.[32] For example, a higher DAP could be associated with more hands-on treatment if reviewers that tended to score applicants generously, also spent more time on due-diligence, and this additional effort has an independent effect on applicants' performance.[33]

However, three pieces of evidence suggest the exclusion restriction is reasonable in our setting.

First, Appendix 7 shows that DAP does not correlate with the content in reviewers' comments (as measured by tone and themes covered). See Section 1.2.1, suggesting potential independence between reviewers' due-diligence quality (as proxied by "note-taking" during the application assessments) and their scoring generosity. This lack of correlation is consistent with the results in Appendix 5 showing that more generous reviewers do not write more positive, or differently themed, comments, though they do write shorter comments, but only marginally so (a one standard deviation increase generosity, is associated with 5 fewer words in comments that on average include 50 words; see Appendix 5).

[31] Our reduced-form estimates are very similar to the two-stage least squares estimates throughout, consistent with the strong first-stage relationship between the DAP and applicants' outcomes.

[32] Because applicants are not made aware of their DAP, as they do not know the generosity of their reviewers, the selection rules, or even their scores, entrepreneurial reactions to DAP are unlikely (e.g., feelings of injustice that can affect performance).

[33] DAP could also reflect better underlying venture potential if it proxies for selection skills. However, scoring generosity is not correlated to predicting ability across reviewers, as discussed in Section 1.2 (and explained in more detail in Appendix 4).

Second, Appendix 8 (Panel A) shows that DAP does not predict investment by the Fund or selection into Opportunity Assessment by the Fund—i.e., passing to the final stage of the Fund's due-diligence process; see Section 1. This is contrary to the assumption that higher DAP leads to better quality due-diligence.

Third, and similarly, DAP is not correlated with Opportunity Assessment performance, as would be expected if DAP also proxies for due-diligence quality. Panel B in Appendix 8 shows that companies with higher DAP do not score higher in the Fund's formal review after the Opportunity Assessment. This Opportunity Assessment scores companies in ten categories, in question format. Questions include "Is this a crowded market?", "Can it produce venture scale returns?", "Is the Business Model Proven?", and "Are the team capable of executing the plan?". Reviewers answer each question by scoring on a scale of 1 to 10; 10 being best.

We acknowledge that the assumption that DAP only systematically affects applicants' outcomes through due-diligence assignment is fundamentally untestable, and our estimates should be interpreted with this caveat in mind. Therefore, we deploy two main robustness tests that relax this identification assumption.

In Section 4, we show that results are robust to controlling for Investment Lead fixed effects, which mitigates concerns that differences across due-diligence processes led by Investment Leads with different generosities drives the results.[34] Further, we show that results are robust to estimating models that exploit selection regime changes holding constant the trio of reviewers. This analysis restricts the sample to London applicants, for which the selection rule becomes more stringent post-May 2018 when the Fund adopts the Champion model. This robustness analysis is useful in relaxing the identification assumption, because the identification relies on variation from the DAP stemming from differences in selection rules, rather that differences in generosity across reviewers. Intuitively, we estimate due-diligence effects by comparing companies at the margin of selection rules under different regimes, holding constant the generosity of reviewers. A vital identification assumption in these alternative models is that reviewers' scoring generosity does not change across selection regimes. Consistent with this assumption, Figure 5 (and Kolmogorov-Smirnov tests explained in the notes) shows that the distributions of score combinations in the three regimes are indistinguishable (see Section 1.2.2).

2.2.3. Monotonicity

The final condition to interpret our results as the LATE of due-diligence assignment is that the impact of DAP on due-diligence assignment is monotonic across applicants. In our setting, the monotonicity assumption requires that a higher DAP does not decrease the likelihood of due-diligence. This

[34] These results are available upon request in order to conserve space.

assumption would be violated, for example, if reviewers differ in the types of applicants they score more generously.

If the monotonicity assumption is violated, our two-stage least squares estimates would still be a weighted average of marginal treatment effects, but the weights would not sum up to one (Angrist, Imbens and Rubin, 1996; Heckman and Vitaclyl, 2005). The monotonicity assumption is, therefore, necessary to interpret our estimates as a well-defined LATE. Otherwise, the LATE will be biased. The bias is an increasing function on the number of individuals for whom the monotonicity assumption does not hold and on the difference in the marginal treatment effects for those individuals for whom the monotonicity assumption does and does not hold (Dobbie and Song, 2015). This bias is also a decreasing function of the first-stage relationship described by equation (3) (Angrist, Imbens, and Robuin, 1996).

The monotonicity assumption implies that the first-stage estimates should be non-negative for all sub-samples. Appendix 9 presents these first-stage results separately by applicant gender (at least one female founder vs all male), location (London vs. Non-London), education background of founders (Russel vs. Non-Russell universities) and stage of development (pre-seed and seed vs. post-seed). The first-stage results are consistently same-signed and sizable across all sub-samples; see Panel B in the appendix. Appendix 9 also further explores how reviewers' generosity varies across observably different applicants as measured by characteristics at application. For each characteristic (e.g., gender), we estimate two reviewer (and trio-level) generosities defined as the reviewer (trios' average) generosity estimated using each sub-sample of applicants (e.g., at least one female founder vs all male). We then compare the two generosities per reviewer (and trios) so constructed per characteristic. Consistent with the monotonicity assumption, for each characteristic, we find that the slopes relating the relationship between the generosity measures for reviewers and trios in the two sub-samples are strongly positively correlated. In further robustness checks, we also relax the monotonicity assumption by letting our leave-one-out estimates of the fractions of applications assigned a specific score by the corresponding reviewers (i.e., $p_{1(-i)}^{s_1}$, $p_{2(-i)}^{s_2}$, $p_{3(-i)}^{s_3}$ in equation (2)) to differ across the same applicant characteristics, in the same spirit as Mueller-Smith (2015). The results from these robustness checks are quantitatively similar to our main results.

2.3. Connection between the Empirical Strategy and the Judge Leniency literature

Our identification strategy is similar to the one used in the "judge leniency" literature, starting with Kling (2006), who uses random assignment of judges to estimate the effects of incarceration on employment. More recently, Gonzalez-Uribe and Reyes (2020) employ the random assignment of judge panels to assess the impact of participation in a business accelerator on venture performance. Our main point of departure between these approaches is that the Fund studied here aggregates the reviewers' scores using complex selection rules, whereas the business accelerator uses reviewers' average scores. In that sense, the paper closest to us is Galasso and Schankerman (2014), who use the random assignment of (multiple) judges to estimate the effects of patent invalidation on citations and construct an invalidation index based on the judges' majority rule, as used by the patent office to aggregate the decisions across judges. The two main conceptual differences between our setting and the one in Galasso and Schankerman (2014) are that (i) reviewers in our setting provide a numerical score {1, 2, 3, 4} rather than a binary pass or fail decision and that (ii) the system used by the Fund to aggregate scores is not a simple majority but involves a more complex set of voting rules that change over locations and across time (see Section 1.1.2). Still, the basic assumption behind the different identification strategies is that reviewers differ in their scoring generosity (in our case, and judges in patent invalidation propensity in the case of Galasso and Schankerman (2014), for example). We perform various tests to check this, as summarized in Section 1.2.1 and thoroughly explained in Appendix 4.

3. The Impact of Due-diligence Assignment on Venture Performance

This section presents our estimates of the causal effects of the Fund's due-diligence assignment on venture performance. We first show our baseline and LATE results on funding proxies and then on other venture growth variables. Then, we discuss the potential channels behind the results. We finalize this section with a discussion on external validity. We delay the discussion of several robustness checks to Section 4.

3.1. Main results

Table 4 presents ordinary least squares (OLS) and two-stage least squares estimates of the impact of the Fund's due-diligence assignment on funding after application to the Fund (e.g. future funding from other VC funds). Panel A uses the entire sample, and Panel B excludes the 12 companies in the Fund's portfolio. The names of the outcome variables are as specified on the top rows of each column.

The OLS estimates show that applicants assigned to due-diligence have significantly higher subsequent funding (by VCs other than the Fund) than other applicants (see columns 1, 3, 5 and 7). This positive association between due-diligence assignment and performance holds across all different funding proxies, across both web-based and administrative UK data (Column 7). Notably, the positive correlation is there even when we exclude the Fund's 12 portfolio companies, implying that these portfolio companies do not drive the OLS results (see Panel B).

The two-stage least squares estimates in columns 2, 4, 6, and 8 improve upon our OLS estimates by exploiting the plausibly exogenous variation in due-diligence assignment. These two-stage least squares results confirm that applicants assigned to due-diligence raise more funding than otherwise similar applicants who were assigned to either informal meeting or no meeting by the Fund. Column 2 in Panel A shows a sizable 281 percentage points increase in funding, which corresponds to a 21 percent increase from the 75th percentile of the post-application log funding distribution.[35] The coefficient in column 2 of Panel B implies that assignment to due-diligence leads to an additional £142K in funding (from VC investors other than the Fund) within two years of applying to the Fund. To be sure, this is for firms involved in failed fundraising campaigns with the Fund, rather than those who succeed in becoming portfolio companies of the Fund. To produce this estimate, we compare the increase in Column 2 with the 75th percentile in post-application log funding distribution and multiply it by the 75th percentile of the (levels) post-application fund raising distribution, given the right skewness of this variable (see Table 1).[36] Appendix 6 summarizes the implied economic magnitudes of the coefficients in Tables 4 and 5.

A unique advantage of our setting is that we can contrast results using web-based proxies for funding (column 2) and administrative data (column 8). Column 8 in Panel A shows a sizable 121 percentage points increase in equity issuance, which corresponds to a 19 percent increase from the 75th percentile of the post-application log equity issuance distribution, which is comparable to the 20% percent increase in funding from column 2 (Panel B; The implied coefficient of Panel A is 20%; see Appendix 6).[37] The implied economic magnitude of the coefficient in column 8 of Panel A is £45K in equity issuance, which is of a similar order of magnitude to the £142K implied funding from column 2, despite the differences in the two variables explained in Section 1.3.[38] Results are indistinguishable both in terms of coefficient estimates and their implied magnitudes, if we use the coefficient estimates in Panel B (See Appendix 6 for more details). Finally, columns 3-4 and 5-6, respectively, show that the funding effects are explained by higher numbers of subsequent financing rounds and participation by a larger number of investors. The coefficients in columns 4 and 6 imply an increase of 16% (0.33) in the number of rounds, and 8% (0.16) in the number of investors; see Appendix 6.

As is common in IV, there is a positive difference between the two-stage least squares and the OLS estimates for all variables and panels in Table 4. In Section 2.1, we explained how the endogeneity of the Fund's due-diligence selection would generate a positive correlation between ε_i and $Due\ diligence_i$ in equation (1), and therefore, an upward bias to the estimate of ρ. Thus, a natural question asks why the two-stage least squares estimates exceed the OLS point coefficients.

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[35] 21%=2.81/13.46, where 13.46 is the 75th of the log funding distribution post-application (median is 0); see Table 1.

[36] £142K=2.74/13.46×£698K, where £698K is the 75th percentile of the web-based funding distribution (median is 0); see Table 1. For more details on implied coefficient magnitudes see also Appendix 6.

[37] 19%=1.21/6.24, where 6.24 is the 75th of the log equity issuance distribution post-application (median is 0); see Table 1. For more details on implied coefficient magnitudes see also Appendix 6.

[38] £45K=1.11/6.24×£255K, where £255K is the 75th percentile of the administrative-based funding distribution (median is 0); see Table 1. For more details on implied coefficient magnitudes see also Appendix 6.

Our explanation for the positive differences is that the benefits from due-diligence among the applicants at the selection margin tend to be relatively high, reflecting the more substantial frictions that they encounter when attempting to enter into due-diligence elsewhere of acquiring due-diligence elsewhere (cf., Card, 2001). By applicants at the selection margin, we mean the so-called "compliers"—i.e., applicants that would have received a different due-diligence assignment if not for their DAP (e.g., applicants that would (not) have been assigned to due-diligence had it not been for the strictness (generosity) of their reviewers). However, we note that large standard errors mean that the difference between the two-stage least squares and the OLS estimates for all the funding proxies is not statistically significant.

Table 5 replicates the OLS and two-stage least squares regressions of Table 4, using other growth variables. Across all variables and panels, the two-stage least squares estimates are positive and statistically significant. The only exception in Table 5 is survival; meaning that we find significant impact on the intensive margin (funding and growth) but not on the extensive margin (survival). We return to these results in Section 3.2 when we discuss the potential channels behind the results.

The results on employment and asset growth help mitigate concerns that due-diligence teaches entrepreneurs how to game VC and raise funds, but have no effects on real (i.e., non-financial) venture performance. The implied magnitudes are of a similar order of magnitude as the implied economic magnitude of the venture funding effects. Appendix 6 shows that the implied magnitude of the coefficients in Panel A of Table 5 corresponding to the number of employees, asset growth, debt growth, and number of directors is, 22% (1.55), 45%(£76K), 59%(£51K) and 27% (0.55), respectively. Results are indistinguishable both in terms of coefficient estimates and their implied magnitudes, if we use the coefficient estimates in Panel B (See Appendix 6 for more details).

3.2. Heterogeneity

Tables 6 and 7 present OLS and two-stage least squares sub-sample results by applicant location (London versus out of London; Panel A in both tables) and founder educational background (Russell indicates tertiary education from a Russell Group university; Panel B in both tables). Applicant location is an important margin given the Fund's investment thesis that partly focuses on selecting top performers outside London. Founder education is an important margin given research that has found that entrepreneurial performance is shaped by the social and human capital derived from the location of university studies (Klingler-Vidra, 2021; Kenney et al., 2013; Batjargal, 2007). The university at which one studies has been found to affect entrepreneurs' social networks (e.g. social capital), which can affect their entrepreneurial orientation and capabilities, and also, their entrepreneurial knowledge and skills (e.g. human capital). To be sure, research has found that studying at so-called "entrepreneurial universities" endows alumni with the human and social capital resources that increase the likelihood of their higher entrepreneurial performance (Klofsten et al., 2019).

Companies in London generally perform better, and the OLS results show that London companies assigned to due-diligence perform better than other due-diligence-assigned companies that are not in London. But, the IV results show no evidence of different causal effects of due-diligence assignment across London and Non-London companies. The only exception is in the web-based funding proxy (Column 2, Panel A. Table 6): where the IV results point to lower funding effects from due-diligence for London applicants. However, the effects are not robust across different funding or economic growth proxies.

The average performance of companies assigned to due-diligence does not vary significantly with founders' educational background either. Results are similar for other educational background proxies. Similarly, in unreported regressions, we also find no impact heterogeneity across different applicant characteristics like gender, business development stage, or business type (e.g. deep technology, direct-to-consumer, and platforms).

4. Discussion

In this section we discuss the potential mechanisms of impact, the external validity of the findings, and the implications of the results.

4.1. Mechanisms

Why are there such considerable benefits from the Fund's due-diligence assignment? Meaningful interactions with potential investors can be valuable for entrepreneurs, if information frictions exist such that *investors, rather than entrepreneurs,* have private information about business prospects and paths to growth, given their industry experience and long experience in financing (Axelson, 2007; Howell, 2020; 2021). Relative to one-off, informal meetings with VCs, a due-diligence process is characterized by a significant volume of interactions, deeper and more meaningful discussions, and a commitment to engage, by both entrepreneurs and VCs, as the real possibility of investment exists.

These characteristics of the due-diligence process lead us to postulate two broad mechanisms through which due-diligence can affect venture performance. We present these mechanisms as different because they are conceptually distinct, but we note that they are non-mutually exclusive in practice.

Going through VC due-diligence processes can add value to applicants as entrepreneurs gather new skills and other resources like business connections relevant to their funding and company management abilities. These improvements are obtained through learning-by-doing or by interacting with the potential investors, which increases performance—we refer to this mechanism as *type improvement*. Assignment to the Fund's due-diligence can also serve as a form of quality assurance that leads to venture growth—we refer to this mechanism as *type discovery*. In this mechanism, selection to due-diligence can provide businesses with a de facto certification, helping them improve their performance through their greater confidence in approaching potential investors and customers.[39] Going through the Fund's due-diligence can reveal and validate to the entrepreneurs their quality, which in turn can influence their performance, as they adjust their commitment and investment in the business as a response to the quality signal. [40]

In practice, cleanly differentiating *type discovery* and *type improvement* mechanisms in our setting is challenging as they likely operate in tandem, and we have no exogenous variation to isolate the effects. Our goal in this section, is therefore, not to fully rule out any mechanism per se, but rather explore whether there is evidence that either type of mechanism is of primary importance in our setting. The results from this endeavour helps inform the relative importance of different types of informational frictions that appear most prevalent in early stage ventures.

According to extensive interviews with the Fund's staff, type improvement is the mechanism of due-diligence they deem as most likely to affect venture performance. The Fund is dedicated to type improvement through its intention to provide incisive feedback and to have the Investment Leads guide entrepreneurs through the investment process. All applicants that begin the due-diligence process are expected to complete very detailed spreadsheets with their capitalization tables, cash-flow projections, and unit economics (see Appendix 2).[41] Throughout the due-diligence process, the Investment Lead coaches the founders by working with them to effectively complete the spreadsheet, giving feedback on their pitch, product and business model, also possibly connecting them to potential suppliers and clients, and providing them with the opportunity for learning-by-doing in compiling and presenting this information. In an interview, a member of the Fund explained that they felt "that people were guided by what we told them they needed in the process, like 'You need a business plan.' They would become more prepared by the result of the meetings. By us telling them: this is what VCs are looking for." In another meeting, a Fund team member even mentioned how some investment leads could go as far as to fill out sections of the Excel spreadsheets for the applicant. Founders are likely to be receptive of the Fund's advice: the Fund's founder is a well-known entrepreneur who had a high-profile exit, and is well-known in the community for knowing how to effectively scale a business.

By contrast, a *type discovery* channel is less likely in this setting because the Fund was newly created at the time of study and corresponds to the first investment fund raised by its general partners. Therefore,

[39] See for example the matching models in Inderst and Muller (2004), Sorensen (2007), and Ewens, Gorbenko and Korteweg (2018).
[40] Learning about entrepreneurial quality plays a pivotal role in many models of firm dynamics (see Jovanovic, 1982; Ericson & Pakes, 1995; and Berk et al., 2004).
[41] This is increasingly the norm amongst seed stage VCs. In their well-known practical book on venture deals, Feld and Mendelson (2019) argue that VCs vary in how much importance they place on detailed financial models "…Some VCs are very spreadsheet driven. Some firms (usually those with associates) may go as far as to perform discounted cash flow analysis… Some will look at every line item and study in detail. Others will focus much less on the details but focus on certain things that matter the most to them…"

assignment to the Fund's due-diligence is unlikely to provide a de facto validation and certification to founders. Moreover, the Fund's due-diligence assignment is privately informed to businesses rather than widely publicized, so unlikely to provide external validation.

Consistent with a first-order role for type improvement, we show that due-diligence assignment leads to changes in ventures' so-called "technology stacks"—i.e., the set of elements used to develop applications (e.g., database, back-end frameworks, front-end frameworks)—within 12 months of applying to the Fund (results are robust to excluding the Fund's portfolio companies; see Table 8). We collect these data from BuiltWith a analysis platform for web technologies; see Koning, Hasan and Chatterji. (2019).[42]

The results from several auxiliary results provide more formal evidence that *type discovery*, instead, appears less dominant in our setting.

First, in unreported analysis we instead show no robust evidence of changes in web-traffic around application dates, which goes against the importance of potential certification effects. We collect data on web traffic from SimilarWeb a market intelligence platform that estimates website and app growth metrics; see Koning, Hasan and Chatterji. (2019)

Second, also in unreported analysis, we find similar effects across businesses with serial vs. first-time founders, post-seed vs. pre-seed stages, and standard vs. deep technology innovations. Instead, if certification effects from the Fund's selection were first order, we would expect stronger effects from due-diligence assignment for businesses with higher uncertainty about their business potential.

Third, Table 5 shows no robust effect of due-diligence assignment on venture survival. By contrast, validation provided the Fund's selection would lead to abandonment after due-diligence rejection.

Fourth, we find no correlation between the sentiment of the reviewers' comments' and applicants' subsequent performance for the subsample of applicants rejected for due-diligence. For these applicants there is no *type improvement* as they are not selected for due-diligence, but there could be type discovery effects if founders react to the sentiment about the venture and the founders revealed by the reviewers. We detail these results in Appendix 10.

Fifth, we find no significant performance effects of rejected applicants' assignment to informal meetings with the Fund's members that are not part of the due-diligence process. Instead, if type

[42] 1,526 (1,284) out of 1,953 applicants adopted new technologies after (within 12 months of) the application date. In unreported analysis, we also collected information from Product Hunt, but we find no evidence of effects. The vast majority of our companies do not launch products through the website (114 of them do. 26 launched at least one product within 12 months of the application date).

discovery were dominant, then positive informal meeting effects would be likely as founders (and possibly third parties) could react to the signal that the Fund considers the idea to be venture backable, though not considered for investment by this specific VC fund.

To show that no venture performance effects exist from informal meetings, we start by estimating baseline models exploring the impact of the allocation to informal meetings on subsequent venture performance. We run the following type of regressions

$$Y_i = \tilde{\gamma} + \tilde{\rho} Informal\ Meeting_i + \boldsymbol{Z}_i + \tilde{\varepsilon}_i \qquad (1b)$$

where $Informal\ Meeting_i$ is a dummy that indicates informal meeting assignment, and all other variables remain the same as defined above.

The primary empirical challenge is that informal meeting selection by the Fund is endogenous as the Fund only decides to meet with those that are "worth the time of the Fund." This endogeneity would generate a positive correlation between $\tilde{\varepsilon}_i$ and $Informal\ Meeting_i$ in equation (1b) and an upward bias to the estimate of $\tilde{\rho}$.

To address potential endogeneity, we need an instrument that affects the likelihood of informal meeting assignments but does not affect the venture performance through any other mechanism. To construct such an instrument, we exploit the random assignment of applicants to reviewers and the informal meeting selection rule. As explained in Section 1, across all selection regimes, the only combination of scores that leads to "no meeting" is {1 1 1}, that is a score of "1" by all the three reviewers of the applicant.

In detail, we estimate the following system of equations:

$$Informal\ Meeting_i = \tilde{\mu} + \tilde{\beta} IMAP_i + \boldsymbol{Z}_i + \tilde{e}_i \ \ (3b)$$

$$Y_i = \tilde{\theta} + \tilde{\alpha} \widehat{Informal\ Meeting_i} + \boldsymbol{Z}_i + \widetilde{\omega_i} \ \ (4b)$$

where $IMAP_i$ stands for "Informal Meeting Assignment Probability," which we estimate for every company as:

$$IMAP_i = 1 - p^1_{1(-i)} p^1_{2(-i)} p^1_{3(-i)} \qquad (5b)$$

where $p^1_{h(-i)}$ denotes the probability that applicant's *i* reviewer number h {1,2,3} gives a score of 1 (based on all other reviewed applicants except i). For example, if the second reviewer of applicant *i* assessed 20 applicants other than *i*, and the reviewer assigned a score of 1 to five of those applications, then $p^1_{2(-i)} = \frac{5}{20} = 0.25$.

Table 9 presents results from estimating equations (3b) and (4b) using two-stage least squares. Standard errors are heteroskedasticity robust.

The OLS estimates (columns 1, 3, 5, and 7) of equation (1b) show that, on average, applicants assigned to informal meetings outperform applicants assigned to no meeting within two years of application. These results are consistent with the Fund's assessment of which businesses are venture backable. However, the two-least squares estimates (columns 2, 4, 6, and 8) show little evidence of causal effects on performance from those meetings: no coefficient is statistically significant. One caveat from these results is potential lack of statistical power: only a small fraction of applicants that are not selected for due-diligence have no informal meetings (4%). Against the concern that results are driven by lack of power, we note that most IV coefficients actually flip signs and are much smaller in absolute value.

One issue with interpreting the results in Table 9 is statistical power: very few companies are not invited for an informal meeting, which may make it hard for us to distinguish the effects of rejection if any exist. However, unreported power tests suggest our sample is big enough to distinguish the effects of informal meetings (assuming an effect of the same size as the due-diligence effects reported in Tables 4 and 5). Another concern is that the signal from an informal meeting may be too weak, relative to the potential signal of due-diligence assignment, given that most companies get at least the chance of an informal meeting. However, the results from the Fund's selection are not publicly available, so it is not publicly known that only a few companies are not invited to meet with the Fund. Also against this concern, we find similar results, when we split the sample into two periods, and focus only on the first months when it is even less likely to be publicly known that the Fund extended an informal invitation to all almost all companies rejected from due-diligence.

Overall, the results from the different auxiliary tests lead us to argue that the channel of *type improvement* is most likely to be the main driver of the Fund's due-diligence effects. None of these results in isolation are conclusive, but, when taken together, they suggest a potentially primary role for *type improvement* rather than *type discovery* in our setting. That being said, we recognize that our paper takes the first step in assessing the value-add of due-diligence of failed fund raising campaigns, and it is clear more research is needed in future work to help disentangle between the relative strength of the impact mechanisms.

4.2. External Validity

The results so far indicate that assignment to due-diligence by the Fund improves venture performance for marginal applicants whose due-diligence assignment is affected by the instrument. How much can we extrapolate from these results to other types of applicants and VC funds?

Our instrumental variable strategy identifies the Fund's due-diligence impact on marginal applicants whose DAP alters due-diligence assignment. This LATE may or may not reflect the average treatment effect of the Fund's due-diligence for all applicants. We estimate Marginal Treatment Effects (MTE; Heckman and Vytaclyl, 2005) to investigate heterogeneous treatment effects across unobservable applicant characteristics. In our setting, MTE estimates illustrate how the outcomes of applicants on the margin of due-diligence change as we move from low to high DAPs—that is, as we go from stricter to more generous reviewers and rules. Thus, the MTE estimates shed light on the types of applicants who benefit most from due-diligence and whether the LATE is likely to apply to applicants further from the margin.

To calculate the MTE function, we follow Doyle (2007) and predict the probability of due-diligence assignment using a probit model with DAP as the only explanatory variable. Using a local quadratic estimator, we then predict the relationship between each outcome and the predicted probability of due-diligence assignment. Then, we evaluate the first derivative of this relationship at each percentile of the predicted due-diligence assignment probability using the local quadratic regression coefficients. We calculate standard errors using the standard deviation of MTE estimates from a bootstrap procedure with 250 iterations.

Figure 9 reports the MTE of due-diligence assignment for web funding, number of rounds, number of investors, and administrative funding. Panel A shows that the MTE function is flat, suggesting that the effects of the Fund's due-diligence on equity financing (from investors other than the Fund) do not vary systematically across unobservable characteristics. The flat shape of the MTE curve suggests that our LATEs are likely to apply to filers who are further from the margin.

Naturally, an important caveat is that we can estimate MTEs only for applicants in the common support—i.e., in the part of the applicants' potential distribution for which there are both selected and rejected due-diligence applicants. Therefore, we can extrapolate from LATE to applicants further from the margin, but not at the very top or very bottom of the distribution (where the sample has only "never takers" and "always takers," respectively). Panel B in Figure 9 shows the range of common support and depicts the sparseness of the untreated (treated) sample at the top (very bottom) of the distribution. The lack of common support above the 0.5 propensity score shows that we cannot extrapolate the LATE beyond applicants of average potential. This limitation can help explain why our two-stage least squares exceed the OLS estimates, even though MTE reveals little treatment heterogeneity among applicants in the common support.

In terms of extrapolation of results outside of the Fund, we note that the Fund is representative of a growing set of early-stage VCs, but of course, not all VCs. As argued above, the Fund, like others investing at the seed stage, has a systems-based approach to sorting applicants and focuses strongly on coaching. Other new intermediaries operating in a similar fashion include other funds also focusing on pre-Series A financing (like seed and pre-seed funds), as well as super angels and accelerators. These early-stage intermediaries seek to sort through the noise and train the most promising of the increasingly inexperienced new founders seeking specialized financing and expertise. We thus argue that our results are most representative of these new types of seed-stage VCs, especially those recently established and seeking to secure high-quality deal flow in the future by building their reputation as value-added VCs.

4.3. Implications

The first main implication from the results in Section 3 is that investors have private information about startups' prospects and growth strategies and going through seed VCs' due-diligence can help resolve these informational frictions for founders. Instead, in the absence of such frictions, entrepreneurs can observe the quality of their venture, and understand how to best grow their businesses, implying no growth effects from due diligence interactions with seed VCs. Our results are consistent with previous work pointing to the existence of informational frictions in early stage markets, and the role of business plan competitions in helping resolve those frictions (Howell, 2020; 2021).

What incentives do seed VCs have in sharing their private information with companies that do not become part of their portfolio? Ex-post, doing so appears inefficient because the Fund does not appropriate the information-related performance improvements for the majority of companies that benefit from this value add. However, ex-ante, before the investment decision is made, the Fund has incentives to share their private information with companies that make it to the due-diligence stage as for these companies the probability of investment is non-zero, and the value-add can increases the acceptance probability of any term sheets offered. In addition, the Fund can also benefit indirectly from providing value-add through due-diligence, by building a value-add reputation that can improve future deal-flow and the discount rate the Fund can charge (cf., Hsu, 2004; Sorensen, 2008).

The second primary implication is that role of VC investors in innovation goes beyond their value-added effects on portfolio companies in which they invest. Extant literature strives to understand the role of VCs on innovation, often seeking to unpack the extent to which it is VCs' ability to make decisions (or, in industry parlance, to "pick winners") that drives their performance (Gompers et al., 2020), or their efforts to "build winners" through the feedback and networking that they offer to portfolio companies (Baum and Silverman, 2004). Our finding points to a different implication: aspects of the VC selection process (precisely, due-diligence) impact a broader ecosystem of ventures, offering a new mechanism for potential spillover effects of VC investment. Through their due-diligence process, VCs meet with, request information from, provide feedback, and as we show: add-value, to many more companies than the ones in which they invest.

How important are due-diligence effects: are they first-order or a minor curiosity? To partially answer this question, we perform a back-of-the-envelope calculation comparing the magnitude of our estimated due-diligence effects of failed fundraising campaigns with the Fund's "total investment effects" on its portfolio firms (including both "pick winners" and "build winners" effects). We measure the "total investment effects" as performance differences between firms in the investment portfolio of the Fund and the rejected applicants (using an OLS regression controlling for covariates; see Appendix 11). We find that due diligence effects are between 0.39 and 0.57 times the respective investment effects across fundraising and real growth variables. The exceptions are number of employees, growth in debt, and number of directors, the three growth variables for which there are no significant total investment effects. Taking these calculations at face value implies that due-diligence effects are of a similar order of magnitude as total investment effects, rather than a minor oddity.

The evidence on VC post-investment value-add highlights the importance of VC for venture growth but remains silent on the implications of participation in the process to secure VC. Our study supports the growing evidence of how frictions in the process through which entrepreneurs connect with VCs can have profound implications for innovation and growth. Rather than a sunk cost for founders, engagement in due-diligence processes acts as a value-add for their growing venture. Our analysis points to how high-potential entrepreneurs may still not reach their full potential if they remain outside the fringes of VC close-knit networks (cf., Howell and Nanda, 2021; Lerner and Nanda, 2020). Our finding suggests these entrepreneurs would benefit from engagement in the VC due-diligence process.

5. Robustness Checks

Threats to Exclusion Restriction.—As discussed previously, interpreting our two-stage least-squares estimates as the causal impact of the Fund's due-diligence assignment requires our DAP instrument to affect applicants' outcomes only through the channel of due-diligence assignment rather than through alternative channels such as higher-quality due-diligence. To further explore this issue, we relax our exclusion restriction by including reviewer trio fixed effects in estimating equations (3) and (4) that hold constant the generosity of reviewers and identify due-diligence effects based on the change in selection regime. Appendix 12 shows that results continue to hold for this alternative identification approach when we restrict the sample to London applicants with the most stringent selection rule post-May 2018. We also present results using an alternative specification that uses the residual variation in DAP as an instrument after netting out the reviewers' generosity. Intuitively, this identification strategy also holds constant the generosity of reviewers; the main difference is that it does not hold constant the trio of reviewers for that purpose. Instead, it holds constant the average generosity of the reviewer trio (as estimated by the reviewer fixed effects in Appendix 4). A vital identification assumption in these alternative models is that reviewers' scoring generosity does not change across selection rules. Figure 5 and Appendix 4 show evidence in this regard, as explained in Sections 1.2.2 and 2.2.2. Taken together, these results provide additional evidence that due-diligence assignment positively affects venture performance.

Alternative Specifications.—In unreported regressions we explore the sensitivity of our main results to alternative specifications. We show that our main results are robust to including controls for company potential as measured by the applicants' fixed effects (from applicant and reviewer fixed effects models described in Appendix 4). These results are similar to our preferred specification, indicating that potential bias from omitted variables is likely slight in our setting. Finally, we also experiment with refinements of our DAP instrument to control for potential expertise differences across reviewers in evaluating applicants with different observable characteristics. In detail, we modify our estimates of the $p_{1(-i)}^{s_1}, p_{2(-i)}^{s_2}, p_{3(-i)}^{s_3}$ in equation (2) to reflect the industry and location of the applicant—i.e., only the decisions of other applicants in the same industry and location of applicant i enter into the computation of its instrument. Results are similar between the main specification and refined DAP versions. None of the estimates in the robustness checks suggest that our preferred estimates are invalid.

5 Conclusion

We study the venture performance effects of Venture Capital (VC) due-diligence—i.e., the process through which VCs engage with ventures in order to determine whether, and at which terms, to invest. Our novel data comprises nearly 2,000 ventures applying for funding to a UK-based VC seed fund (Fund). For identification, we exploit the Fund's process of screening applicants for due-diligence, which features pre-determined selection rules based on the scores of quasi-randomly-allocated reviewers. We show that assignment to due-diligence leads to substantial increases in venture capital funding and growth within two years of application, even for those companies that receive no eventual investment from the Fund. By contrast, we find little evidence of venture performance effects from applicants' assignments to informal meetings with Fund team members that are not part of the due-diligence process.

VC due-diligence comprises type improvement and type discovery mechanisms; tentative evidence suggests that type improvement (including coaching, learning-by-doing, and network support) may be primary. The results provide evidence that going through VCs' due-diligence process adds value in the form of improved venture performance. This new evidence implies that VCs' role in innovation goes beyond their value-added effects on only their own portfolio companies. The VC due-diligence process is a systemic opportunity to add value to the larger number of ventures (approximately 30 out of 100) that enter the early-stage financing funnel. Therefore, frictions in the process through which ventures seek VC financing can profoundly impact the innovation and economic growth capabilities of a wider set of ventures than previously acknowledged.

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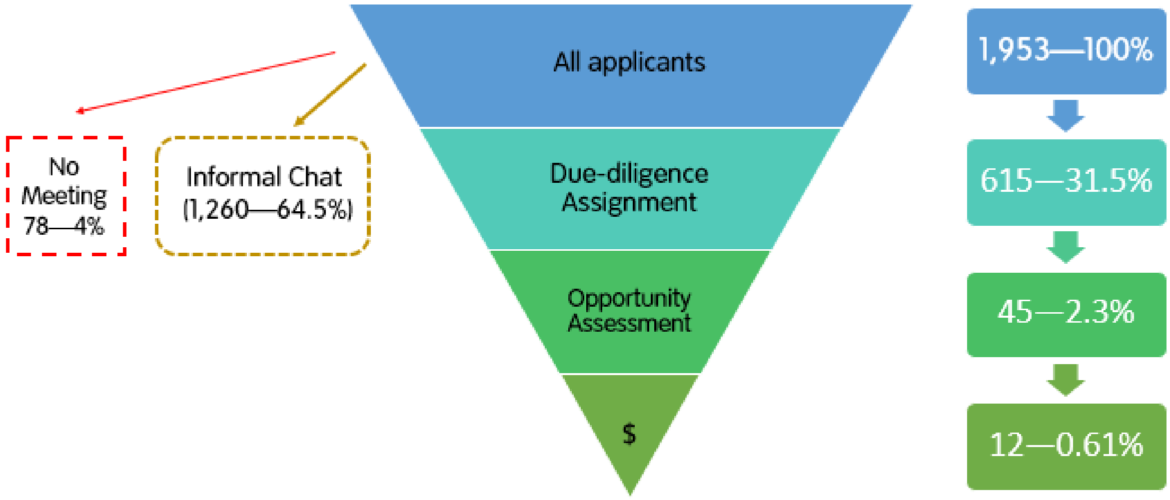
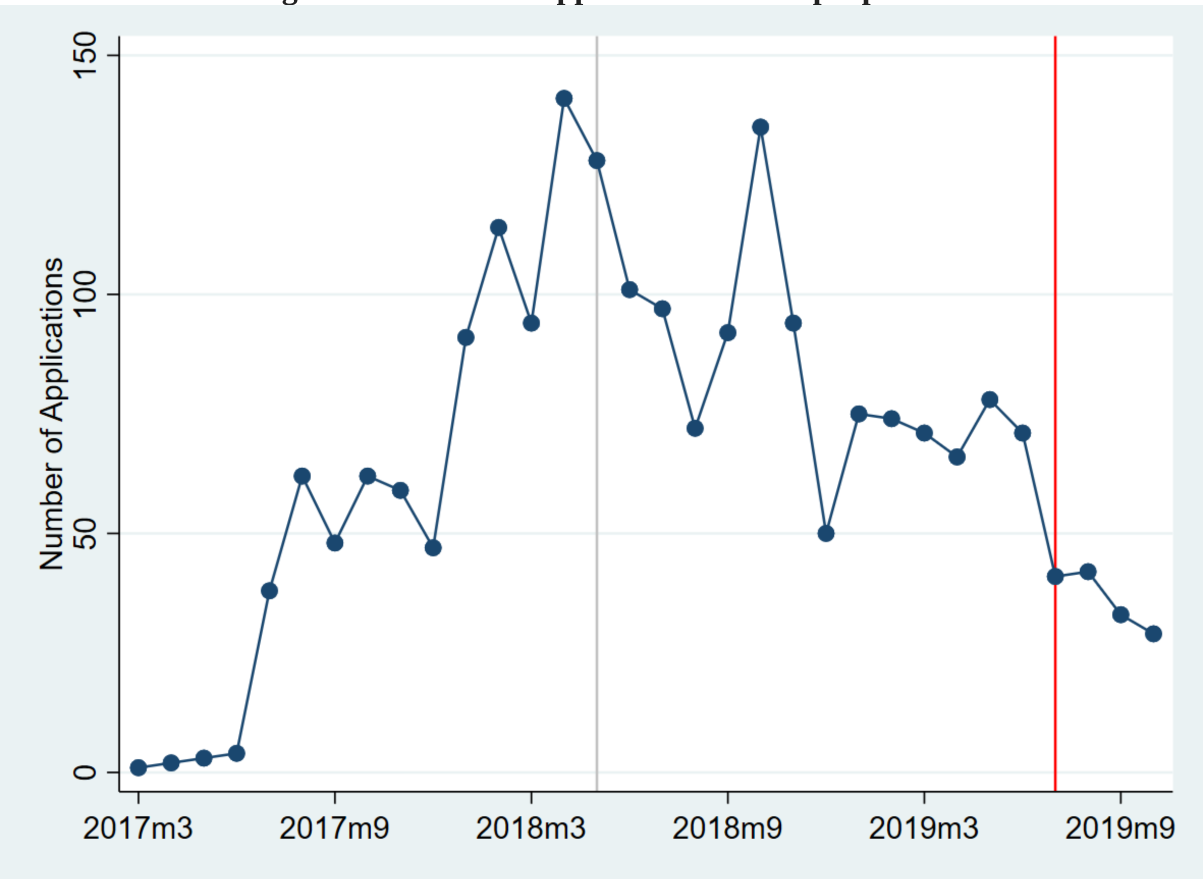
Figure 1. Selection Funnel



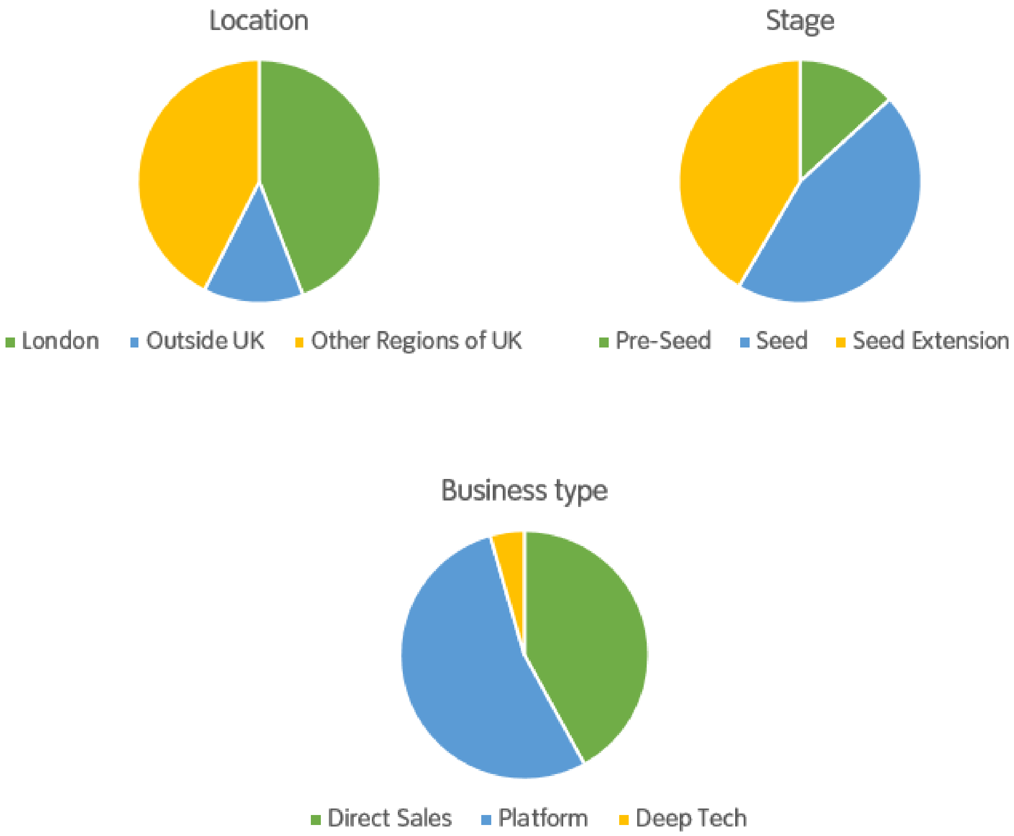
The figure plots the selection funnel of the Fund for the period between March 2017 and June 2019. Opportunity assessment corresponds to the third stage in the due diligence process includes hiring industry experts for external reviews and calling on other parties, including references provided by the founders; see Section 1 for more details.

Figure 2. Number of applicants over sample period



This figure plots the distribution of Fund applicants over the sample period. The grey line indicates the date where the Fund changes the selection regime—May 28 2018; see Section 1.2.2 for more details. The red line indicates the end of our sample, which coincides with the end of the investment period of the Fund.

Figure 3. Characteristics of Businesses at Application



This figure shows the distribution of applicants across locations, development stage and business type at the time of application. The details of the distribution are in the table below.

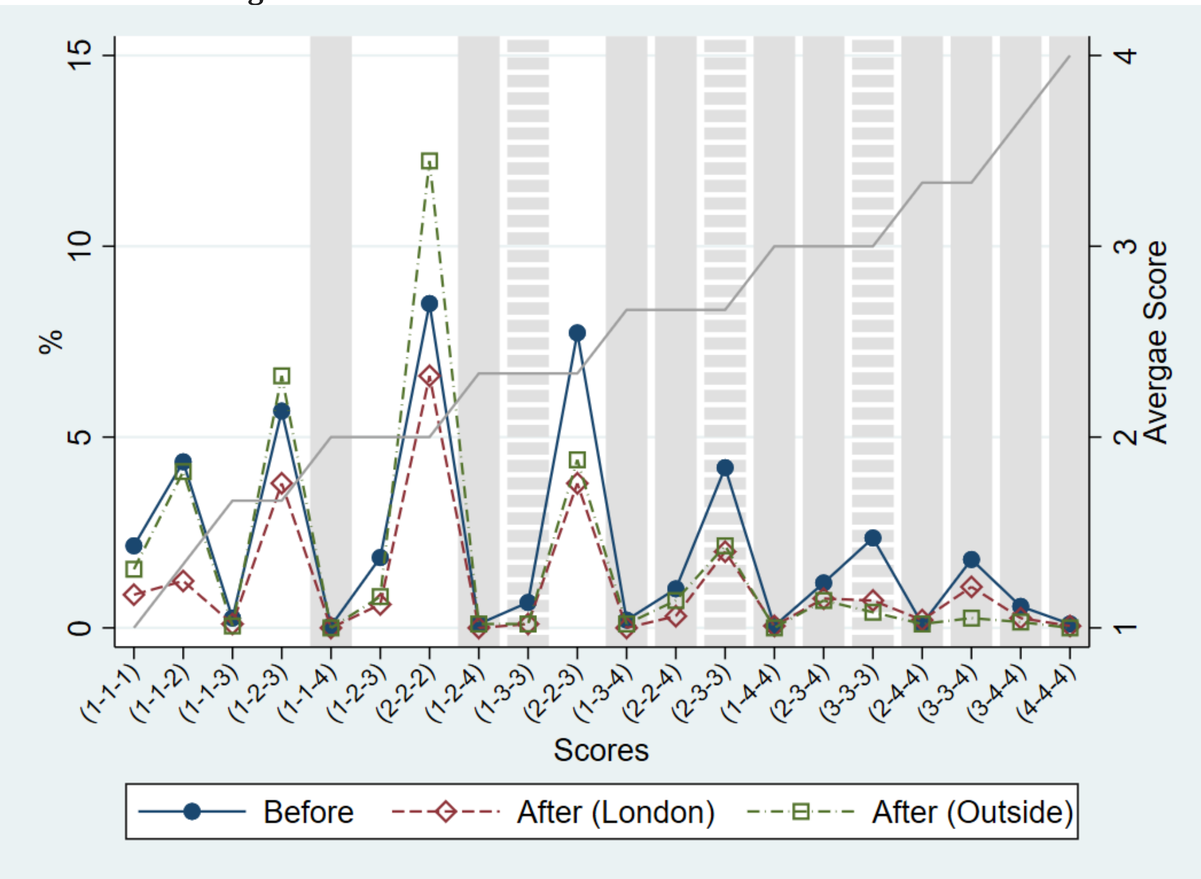
	Number of Companies	Percent
	By Location	
London	862	44.14%
Outside UK	412	21.10%
Other Regions of UK	679	34.77%
	By Stage	
Pre-Seed (under £100k)	250	12.80%
Seed (£100k-1m)	865	44.29%
Seed Extension (£200k-2m)	838	42.91%
	By Business Type	
Deep Tech	83	4.26%
Direct Sales Led	836	42.92%
Platform	1,029	52.82%

Figure 4. Due Diligence Selection Rules over Time and Location

				Average Score	Pre—May 2018	Post—May 2018	
						London	Outside
1	1	1	1	1.00	No Meeting	No Meeting	No Meeting
2	1	1	2	1.33	Informal Chat	Informal Chat	Informal Chat
3	1	1	3	1.67	Informal Chat	Informal Chat	Informal Chat
4	1	2	2	1.67	Informal Chat	Informal Chat	Informal Chat
5	1	1	4	2.00	Due diligence	Due diligence	Due diligence
6	1	2	3	2.00	Informal Chat	Informal Chat	Informal Chat
7	2	2	2	2.00	Informal Chat	Informal Chat	Informal Chat
8	1	2	4	2.33	Due diligence	Due diligence	Due diligence
9	1	3	3	2.33	Due diligence	Informal chat	Informal Chat
10	2	2	3	2.33	Informal Chat	Informal Chat	Informal Chat
11	1	3	4	2.67	Due diligence	Due diligence	Due diligence
12	2	2	4	2.67	Due diligence	Due diligence	Due diligence
13	2	3	3	2.67	Due diligence	Informal Chat	Informal Chat
14	1	4	4	3.00	Due diligence	Due diligence	Due diligence
15	2	3	4	3.00	Due diligence	Due diligence	Due diligence
16	3	3	3	3.00	Due diligence	Informal Chat	Due diligence
17	2	4	4	3.33	Due diligence	Due diligence	Due diligence
18	3	3	4	3.33	Due diligence	Due diligence	Due diligence
19	3	4	4	3.67	Due diligence	Due diligence	Due diligence
20	4	4	4	4.00	Due diligence	Due diligence	Due diligence

The figure summarizes the selection rules used by the Fund to aggregate reviewers' scores over time and location. The scores are sorted by average score. See Section 1.2.2 for more details.

Figure 5. Distribution of Scores over Time and Location

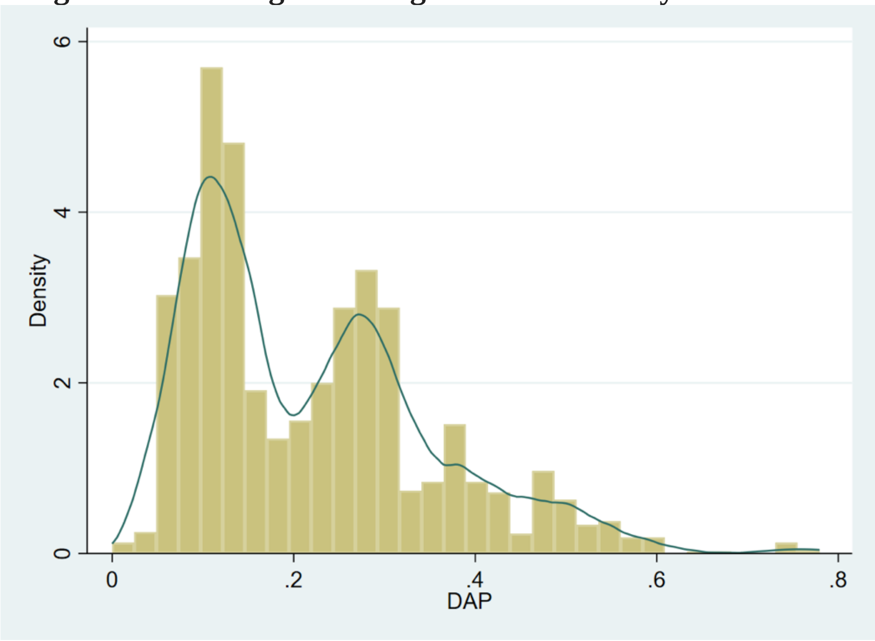


This figure plots the distribution of scores over time and locations. The left axis plots the fraction of scores for each score combination over the different selection regimes. The right axis plots the average score for each score combination; score combinations are sorted by average score. The bars in grey represents scores that lead to due diligence according to the rule. The dashed bars in grey represents scores whose mapping into due diligence are effectively affected by the selection regime change (See Figure 4).The score distributions are not statistically different over time. We perform Kolmogorov-Smirnov tests comparing the distribution scores across time and locations. We summarize results below.

Trio Scores	Two-Sample Kolmogorov-Smirnov Test	
	Stat.	P Value
London (Before) vs. Outside (Before)	0.132	0.001
London (After) vs. Outside (After)	0.149	0.000
London (Before) vs. London (After)	0.103	0.021
Outside (Before) vs. Outside (After)	0.120	0.001

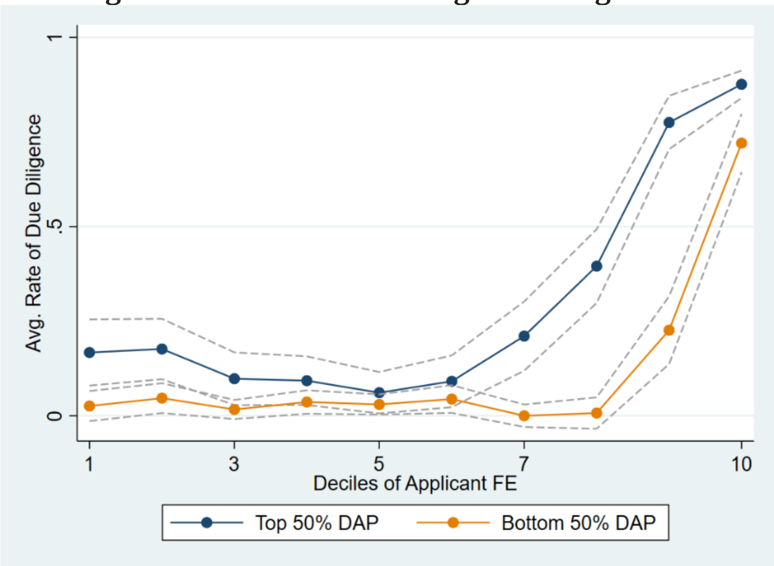
Individual Score	Two-Sample Kolmogorov-Smirnov Test	
	Stat.	P Value
London (Before) vs. Outside (Before)	0.089	0.000
London (After) vs. Outside (After)	0.113	0.000
London (Before) vs. London (After)	0.084	0.000
Outside (Before) vs. Outside (After)	0.109	0.000

Figure 6. Due Diligence Assignment Probability Distribution



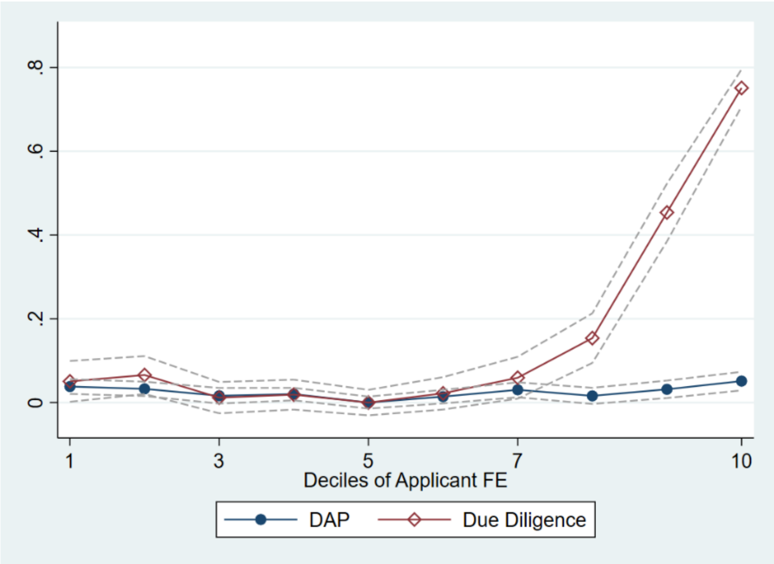
This figure plots the distribution of the Due Diligence Assignment Probability (DAP) across the sample applicants. For more details see Section 2.2.

Figure 7. DAP and Due Diligence Assignment



The figure plots the average rate of due diligence assignment (demeaned by region and rescaled for illustration purposes) against deciles of applicant fixed effects for two subsamples: applicants with DAP above and below the median DAP of 0.22. The applicant fixed effects are estimated in models regressing reviewer scores against full set of applicant and reviewer fixed effects; for more details see Section 1.2 and Appendix 3.

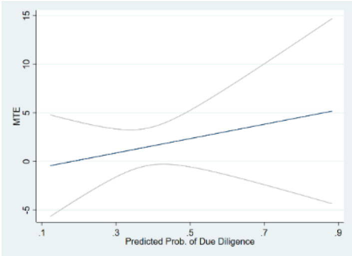
Figure 8. DAP and Company Characteristics at Application



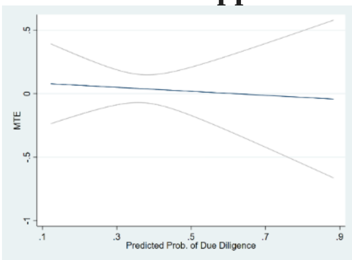
This figure plots the average due diligence assignment (demeaned by region and rescaled for illustration purposes) and DAP against deciles of applicant fixed effects. The applicant fixed effects are estimated in models regressing reviewer scores against full set of applicant and reviewer fixed effects; for more details see Section 1.2 and Appendix 3

Figure 9. Marginal Treatment Effects

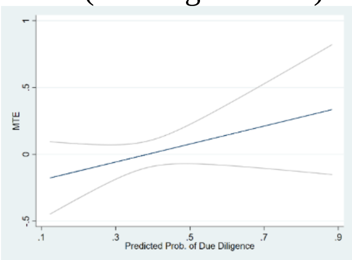
Panel A -Treatment Curves over Common Support



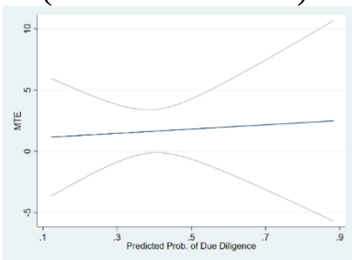
Ln(Funding Amount)



Ln(Number of Rounds)

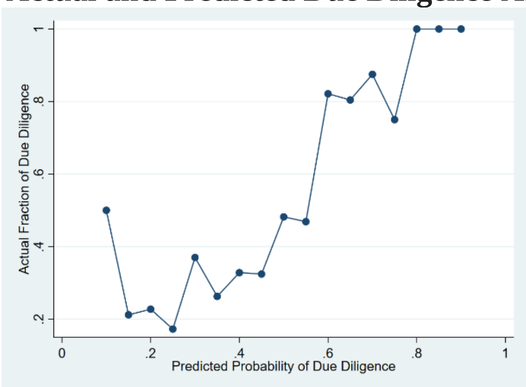


Ln(Number of Investors)



Ln(Issued Equity)

Panel B – Actual and Predicted Due Diligence Assignment



The figures in Panel A plot marginal treatment effects and associated 95% confidence intervals. We predict the probability of due diligence assignment using DA. We then predict the relationship between each outcome and the predicted probability of due diligence assignment using a local quadratic estimator wit bandwidth 0.15. The estimates of the first derivative of this relationship are then evaluated at each percentile of predicted probability. Standard errors are calculated using a bootstrap with 250 iterations. Panel B plots the due diligence assignment against the predict probability of due-diligence. For predicted probability of due diligence above 0.8 we have no common support. For more details see Section 3.3.

Table 1. Summary Statistics

Source	Variable	Mean	Std. Dev.	p5	p25	p50	p75	p95	N
	Application and Selection								
Application files	Age Business (since incorporation)	2.61	2.96	0.00	1.00	2.00	4.00	7.00	1,953
	Target Amount (£1000s)	1,692	2,537	100	365	1,000	2,000	5,500	1,950
	Target Close Date (Days)	80	70	25	48	70	96	165	1,946
	Total Addressable Market (£Billion)	345	1,725	0.02	1.00	8.00	50	1,000	1,435
	Total Serviceable Market (£ Billion)	45	269	0.00	0.08	0.50	3.45	80	1,435
LinkedIn	Female Founder	0.13	0.33	0.00	0.00	0.00	0.00	1.00	1,785
	Russell Education Founder	0.17	0.37	0.00	0.00	0.00	0.00	1.00	1,953
Fund's Selection	Due diligence(%)	31.49	46.46	0.00	0.00	0.00	100.00	100.00	1,953
	Opportunity assessment(%)	2.30	15.49	0.00	0.00	0.00	0.00	100.00	1,953
	Investment(%)	0.61	7.81	0.00	0.00	0.00	0.00	0.00	1,953
	Company Characteristics (All Companies, Web Sources)								
	Pre- Application								
Crunchbase	Number of funding rounds (# Rounds)	0.47	1.06	0.00	0.00	0.00	0.00	3.00	1,953
	Total funding ($1000s) (Funding)	306	1,105	0.00	0.00	0.00	0.00	2,000	1,953
	Number of Investors (# Investors)	0.83	2.48	0.00	0.00	0.00	0.00	5.00	1,953
	ln(# Rounds)	0.73	0.29	0.00	0.00	0.00	0.00	1.39	1953
	ln(Funding)	2.90	5.00	0.00	0.00	0.00	0.00	7.60	1953
	Ln(# Investors)	0.78	0.47	0.00	0.00	0.00	0.00	1.79	1953
	No. of Years Before App.	2.61	2.96	0.00	1.00	2.00	4.00	7.00	1,953
LinkedIn	Serial Entrepreneur	0.26	0.44	0.00	0.00	0.00	1.00	1.00	1,953
	No. of Companies Created by the Founder	0.40	0.80	0.00	0.00	0.00	1.00	2.00	1,953
	Post-Application								
Crunchbase	Number of funding rounds (# Rounds)	1.28	1.90	0.00	0.00	0.00	2.00	5.00	1,953
	Total funding ($1000s) (Funding)	1,330	3,362	0.00	0.00	0.00	698	8,634	1,953
	Number of Investors (# Investors)	1.02	1.19	0.00	0.00	1.00	2.00	3.00	1,953
	ln(# Rounds)	0.93	0.40	0.00	0.00	0.00	1.10	1.79	1953
	ln(Funding)	5.56	6.56	0.00	0.00	0.00	13.46	15.97	1953
	ln(# Investors)	0.87	0.28	0.00	0.00	0.69	1.10	1.39	1953
Linkedin	Number of Employees (# Employees)	6.09	11.38	1.00	1.00	2.00	7.00	27.00	1,953
	ln(# Employees)	1.21	1.16	0.00	0.00	1.10	2.08	3.33	1,953
BuiltWith	Num. of Web Tech Adoptions	15.83	23.60	0.00	0.00	9.00	24.00	54.00	1,953

Ln(# of Web Tech Adoptions)	1.88	1.54	0.00	0.00	2.30	3.22	4.01	1,953
1(# of Web Tech Adoptions>0)	0.66	0.47	0.00	0.00	1.00	1.00	1.00	1,953

Table 1 (Continued). Summary Statistics

Source	Variable	Mean	Std. Dev.	p5	p25	p50	p75	p95	N
	Company Characteristics (UK Companies, Administrative Data)								
	Pre- Application								
Companies House	Assets (£1000s)	641	15,635	0.00	0.00	23	167	1,044	1,548
	ln(Assets)	2.89	2.61	0.00	0.00	3.18	5.12	6.95	1,548
	Debt (£1000s)	611	16,070	0.00	0.00	14	85	608	1,548
	ln(Debt)	2.58	2.38	0.00	0.00	2.71	4.45	6.41	1,548
	Equity Issuance (£1000s)	158	608	0.00	0.00	0.00	83	850	1,548
	ln(Equity Issuance)	2.39	2.70	0.00	0.00	1.10	5.12	7.44	1548
	No. of Years Before App.	2.67	2.67	0.00	1.00	2.00	4.00	8.00	1,548
	Post-Application								
	Assets (£1000s)	1,066	18,470	0.00	1.00	86	545	3,199	1,548
	ln(Assets)	3.94	2.85	0.00	0.69	4.46	6.30	8.07	1,548
	Debt (£1000s)	818	17,259	0.00	1.00	58	245	1,821	1,548
	ln(Debt)	3.59	2.58	0.00	0.69	4.09	5.51	7.51	1,548
	Equity Issuance (£1000s)	385	933	0.00	0.00	0.00	255	2,387	1,548
	ln(Equity Issuance)	3.12	3.07	0.00	1.10	1.10	6.24	8.47	1,548
	No. of Directors Appointed	1.03	1.63	0.00	0.00	0.00	2	4	1,548
	Survival	0.81	0.39	0.00	1.00	1.00	1.00	1.00	1,548
	Liquidation	0.04	0.19	0.00	0.00	0.00	0.00	0.00	1,548
	No. of Years After App.	1.93	0.64	1.00	2.00	2.00	2.00	3.00	1,548
	Post- relative to Pre-Application								
	Growth in Assets	1.05	2.45	-2.82	0.00	0.61	2.08	5.91	1,548
	Growth in Debt	1.01	2.12	-2.40	0.00	0.75	1.95	4.81	1,548
	Instrument Variables								
Constructed	DAP	0.22	0.13	0.06	0.11	0.19	0.30	0.48	1,953
	Regional DAP	0.25	0.21	0.01	0.08	0.18	0.37	0.67	1,953

The table presents summary statistics of the variables used in the analysis. The variables are organized by source and time period as indicated by the first and second column of the table. The sample includes all 1,953 applicants to the Fund that were evaluated by the reviewers. Only a subsample of these compaies are incorporated in UK, and for these ventures we collect abridged balance sheet information from Companies House. For more details on data sources see Section 1.1.

Table 2. DAP and Due Diligence Assignment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A-OLS				
DAP	1.09***	1.33***	1.09***	1.32***	0.94***	1.19***	0.93***	1.19***
	(0.08)	(0.07)	(0.08)	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)
Applicant FE					0.35***	0.37***	0.34***	0.37***
					(0.02)	(0.01)	(0.02)	(0.02)
F-test of excl. IV	185.64	361.00	185.64	355.59	180.33	393.36	176.51	393.36
Controls		Yes		Yes		Yes		Yes
Obs.	1953	1953	1941	1941	1953	1953	1941	1941
R-sq	0.0981	0.3589	0.0976	0.3618	0.0551	0.2916	0.2679	0.5390
				Panel B-Probit				
DAP	3.09***	4.53***	3.08***	4.52***	3.14***	6.09***	3.12***	6.07***
	(0.23)	(0.28)	(0.23)	(0.28)	(0.26)	(0.37)	(0.26)	(0.37)
Applicant FE					1.17***	1.89***	1.15***	1.87***
					(0.07)	(0.10)	(0.07)	(0.10)
F-test of excl. IV	180.49	261.75	179.33	260.59	145.85	270.91	144.00	269.14
Controls		Yes		Yes		Yes		Yes
Obs.	1953	1953	1941	1941	1953	1953	1941	1941
Pseudo R-sq	0.08	0.32	0.08	0.33	0.23	0.55	0.23	0.55

The table presents results from estimating Eq. (3). The outcome variable is Due diligence, which corresponds to a dummy indicating the applicants assigned to further due diligence. DAP is the due diligence assignment probability estimated as in Eq. (2). Reviewer and applicant FE correspond to the fixed effects estimated in models regressing scores against applicant and reviewer fixed effects; see Appendix 3. Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. Standard errors are robust, except in columns with reviewer or applicant FE where we bootstrap standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively

Table 3—Balance of Covariates Across DAP Quartiles

Variable	Q1	Other Q	*p-value diff. in mean*	Q2	Other Q	*p-value diff. in mean*	Q3	Other Q	*p-value diff. in mean*	Q4	Other Q	*p-value diff. in mean*
App. Info												
Age	2.30	2.71	*0.00*	2.42	2.67	*0.85*	2.73	2.57	*0.43*	2.98	2.48	*0.02*
ln(Age)	0.98	1.08	*0.00*	1.01	1.07	*0.42*	1.09	1.05	*0.58*	1.14	1.03	*0.06*
Female Founder	0.14	0.12	*0.62*	0.12	0.13	*0.11*	0.14	0.13	*0.91*	0.11	0.13	*0.22*
Russell Education of Founder	0.14	0.18	*0.46*	0.17	0.17	*0.99*	0.17	0.17	*0.93*	0.20	0.16	*0.41*
Amount	1690.78	2416.15	*0.33*	1606.34	2444.41	*0.78*	3948.15	1663.73	*0.07*	1694.31	2413.00	*0.58*
ln(Amount)	6.72	6.60	*0.18*	6.58	6.65	*0.42*	6.67	6.62	*0.67*	6.55	6.66	*0.92*
Target Close Days	83.18	80.05	*0.50*	81.83	80.50	*0.40*	78.93	81.47	*0.79*	79.37	81.32	*0.67*
ln(Target Close Days)	4.23	4.22	*0.29*	4.24	4.22	*0.26*	4.22	4.22	*0.18*	4.19	4.23	*0.19*
Total Addressable Market	152.47	862.41	*0.67*	3269.07	39.36	*0.08*	3.91	1011.05	*0.53*	4.62	1073.79	*0.50*
ln(Total Addressable Market)	0.62	0.42	*0.64*	0.53	0.44	*0.08*	0.40	0.48	*0.73*	0.35	0.51	*0.08*
Total Serviceable Market	247.10	6.77	*0.39*	9.03	63.04	*0.69*	11.33	67.00	*0.11*	1.30	75.24	*0.67*
ln(Total Serviceable Market)	0.31	0.18	*0.09*	0.21	0.21	*0.14*	0.21	0.21	*0.06*	0.15	0.24	*0.09*
London	0.41	0.45		0.40	0.45		0.46	0.43		0.50	0.42	
Seed/Pre-Seed	0.47	0.44	*0.25*	0.46	0.44	*0.57*	0.42	0.45	*0.94*	0.42	0.45	*0.10*
Platform	0.46	0.55	*0.03*	0.53	0.53	*0.31*	0.56	0.52	*0.38*	0.56	0.52	*0.12*
Deep Tech	0.03	0.05	*0.10*	0.02	0.05	*0.95*	0.04	0.04	*0.47*	0.07	0.03	*0.08*
CH Info. Before App.												
Asset (£1000s)	217.48	767.68	*0.51*	194.55	785.47	*0.57*	1808.90	247.39	*0.06*	320.16	760.96	*0.51*
Debt (£1000s)	2.78	2.93	*0.06*	2.75	2.94	*0.74*	2.89	2.89	*0.32*	3.12	2.81	*0.55*
Annual Equity Issuance (£1000s)	177.83	740.68	*0.52*	105.08	774.86	*0.54*	1849.26	193.80	*0.05*	287.00	732.26	*0.50*
ln(Debt)	2.59	2.58	*0.08*	2.41	2.64	*0.54*	2.58	2.59	*0.35*	2.74	2.52	*0.82*
Equity Issuance (£1000s)	289.17	325.19	*0.15*	284.61	327.35	*0.72*	338.83	309.49	*0.11*	349.03	304.83	*0.62*
ln(Equity Issuance)	2.25	2.43	*0.19*	2.34	2.40	*0.54*	2.37	2.39	*0.69*	2.55	2.32	*0.75*
Web Info. Before App.												
Num. of Funding Rounds	1.09	1.22	*0.09*	1.22	1.18	*0.43*	1.19	1.18	*0.87*	1.24	1.17	*0.45*
ln(# Rounds)	0.70	0.75	*0.16*	0.74	0.73	*0.91*	0.74	0.73	*0.59*	0.76	0.73	*0.45*
Total Funding (£1000s)	280.42	437.74	*0.57*	368.39	408.36	*0.94*	356.07	412.44	*0.52*	589.22	334.94	*0.20*
ln(Funding)	2.50	3.04	*0.39*	2.75	2.95	*0.78*	2.96	2.88	*0.45*	3.40	2.74	*0.18*
Num. of Companies Created	0.40	0.40	*0.36*	0.41	0.40	*0.92*	0.37	0.41	*0.96*	0.42	0.39	*0.28*
ln(# Companies Created)	0.24	0.23	*0.20*	0.23	0.23	*0.99*	0.21	0.24	*0.83*	0.24	0.23	*0.28*

Serial Entrepreneur	0.27	0.26	*0.22*	0.26	0.26	*0.96*	0.25	0.27	*0.57*	0.27	0.26	*0.48*

The table compares applicants' characteristics (at application) across the different quartiles of Due Diligence Assignment Probability (DAP).

Table 4—Due Diligence Assignment and Funding
Panel A—Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	2.94***	2.81***	0.20***	0.18**	0.10***	0.09*	1.18***	1.21**
	(0.36)	(0.85)	(0.02)	(0.06)	(0.02)	(0.04)	(0.18)	(0.43)
N	1953	1953	1953	1953	1953	1953	1548	1548
R-sq	0.1313	0.1039	0.1457	0.1156	0.0704	0.0415	0.1053	0.0709
F Stat.	401.49		401.49		401.49		355.83	

Panel B—Excluding Portfolio companies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	2.86***	2.74**	0.19***	0.18**	0.10***	0.09*	1.13***	1.11*
	(0.37)	(0.86)	(0.02)	(0.06)	(0.02)	(0.04)	(0.18)	(0.44)
N	1941	1941	1941	1941	1941	1941	1537	1537
R-sq	0.1298	0.1032	0.1419	0.1120	0.0689	0.0405	0.1031	0.0694
F Stat.	397.38		397.38		397.38		352.01	
Reference:								
P75	13.46		1.10		1.10		6.24	

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5 –Due Diligence and Economic Growth

Panel A—Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.51***	0.46**	0.54***	0.93**	0.56***	1.16***	0.22***	0.30***	0.07**	-0.11
	(0.07)	(0.16)	(0.15)	(0.34)	(0.13)	(0.29)	(0.04)	(0.08)	(0.02)	(0.06)
N	1953	1953	1548	1548	1548	1548	1548	1548	1548	1548
R-sq	0.1629	0.1382	0.0846	0.0662	0.0656	0.0350	0.0555	0.0319	0.0495	-0.0042
F Stat.	401.49		355.83		355.83		355.83		355.83	

Panel B—Excluding Portfolio companies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.50***	0.44**	0.50***	0.89*	0.54***	1.12***	0.21***	0.29***	0.07**	-0.11*
	(0.07)	(0.16)	(0.15)	(0.34)	(0.13)	(0.29)	(0.04)	(0.09)	(0.02)	(0.06)
N	1941	1941	1537	1537	1537	1537	1537	1537	1537	1537
R-sq	0.1598	0.1357	0.0821	0.0636	0.0643	0.0346	0.0549	0.0309	0.0489	-0.0062
F Stat.	397.38		352.01		352.01		352.01		352.01	
Reference:										
P75	2.08		2.08		1.95		1.10			

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6-Due Diligence and Economic Growth for Non-portfolio Companies: sample cuts

Panel A—Location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.77***	7.73***	0.10***	0.32**	0.05**	0.09	0.07	3.11**
	(0.40)	(1.96)	(0.02)	(0.11)	(0.02)	(0.08)	(0.22)	(1.17)
Due diligence*London	1.35*	-6.29**	0.13**	-0.16	0.06*	0.02	1.55***	-2.02
	(0.65)	(2.29)	(0.04)	(0.14)	(0.03)	(0.09)	(0.33)	(1.32)
London	1.25***	3.69***	0.08***	0.17***	0.06***	0.07*	0.17	1.52**
	(0.33)	(0.78)	(0.02)	(0.04)	(0.01)	(0.03)	(0.18)	(0.51)
N	1941	1941	1941	1941	1941	1941	1537	1537
R-sq	0.1166	0.0083	0.1307	0.0885	0.0530	0.0497	0.0816	-0.0332
F Stat.		27.49		27.49		27.49		17.68

Panel B—Founders' Educational Background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	3.00***	2.46*	0.19***	0.18**	0.10***	0.10*	1.16***	1.01*
	(0.40)	(0.97)	(0.03)	(0.06)	(0.02)	(0.04)	(0.20)	(0.50)
Due diligence*Russell	-0.79	1.57	0.02	-0.03	-0.04	-0.08	-0.28	0.35
	(0.86)	(2.77)	(0.06)	(0.17)	(0.04)	(0.13)	(0.42)	(1.31)
Russell	0.88	0.08	0.03	0.05	0.01	0.03	0.93***	0.71
	(0.47)	(1.00)	(0.03)	(0.06)	(0.02)	(0.05)	(0.24)	(0.50)
N	1941	1941	1941	1941	1941	1941	1537	1537
R-sq	0.1314	0.1008	0.1432	0.1127	0.0695	0.0402	0.1146	0.0797
F Stat.		22.16		22.16		22.16		22.71

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Due Diligence and Economic Growth: sample cuts

Panel A—Location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.13	0.71	-0.15	1.89	0.04	1.99*	0.01	0.72**	0.07*	-0.42*
	(0.09)	(0.42)	(0.19)	(0.98)	(0.16)	(0.88)	(0.05)	(0.25)	(0.03)	(0.17)
Due diligence*London	0.58***	-0.17	0.97***	-0.92	0.64**	-0.74	0.27***	-0.50	0.04	0.40*
	(0.12)	(0.47)	(0.27)	(1.08)	(0.23)	(0.96)	(0.07)	(0.28)	(0.04)	(0.19)
London	-0.04	0.24	0.11	0.87*	0.18	0.78*	-0.06	0.24*	0.00	-0.15*
	(0.07)	(0.18)	(0.14)	(0.42)	(0.13)	(0.37)	(0.04)	(0.11)	(0.03)	(0.07)
N	1941	1941	1537	1537	1537	1537	1537	1537	1537	1537
R-sq	0.1454	0.1144	0.0789	0.0000	0.0524	-0.0512	0.0347	-0.1161	0.0365	- 0.1448
F Stat.		27.49		17.68		17.68		17.68		17.68

Panel B—Founders' Educational Background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.50***	0.37*	0.54**	0.88*	0.62***	1.33***	0.19***	0.22*	0.07**	-0.11
	(0.08)	(0.19)	(0.17)	(0.40)	(0.14)	(0.35)	(0.04)	(0.10)	(0.02)	(0.07)
Due diligence*Russell	-0.04	0.26	-0.26	-0.13	-0.40	-1.25	0.09	0.32	-0.01	-0.01
	(0.14)	(0.47)	(0.32)	(1.01)	(0.27)	(0.84)	(0.09)	(0.28)	(0.05)	(0.16)
Russell	0.42***	0.31	0.64***	0.58	0.42**	0.69*	0.14**	0.06	0.07*	0.07
	(0.08)	(0.17)	(0.18)	(0.37)	(0.15)	(0.31)	(0.05)	(0.10)	(0.03)	(0.06)
N	1941	1941	1537	1537	1537	1537	1537	1537	1537	1537
R-sq	0.1780	0.1520	0.0901	0.0719	0.0681	0.0343	0.0672	0.0379	0.0528	-0.0028
F Stat.		22.16		22.71		22.71		22.71		22.71

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectiv

Table 8. Due Diligence and Technology Adoptions

Panel A Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+Num. of Tech Adoptions)		Num. of Tech Adoptions		1(Num. of Tech Adoptions>0)	
	OLS	IV	OLS	IV	OLS	IV
Due Diligence	0.20***	0.70***	2.61*	14.38***	0.06*	0.18**
	(0.05)	(0.13)	(1.25)	(2.88)	(0.03)	(0.06)
N	1953	1953	1953	1953	1953	1953
R-sq	0.0241	-0.0317	0.0168	-0.0327	0.0253	-0.0006
F Stat.		401.49		401.49		401.49

Panel B—Excluding Portfolio companies

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+Num. of Tech Adoptions)		Num. of Tech Adoptions		1(Num. of Tech Adoptions>0)	
	OLS	IV	OLS	IV	OLS	IV
Due Diligence	0.20***	0.70***	2.63*	14.50***	0.06*	0.18**
	(0.05)	(0.13)	(1.28)	(2.91)	(0.03)	(0.06)
N	1941	1941	1941	1941	1941	1941
R-sq	0.0242	-0.0320	0.0169	-0.0330	0.0250	-0.0001
F Stat.		397.38		397.38		397.38

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. The outcome variables are constructed based on the number of technology adoptions on the applicant's website within 12 months after the application. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9. Informal Meetings and Funding

Panel A Funding

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		Δ(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Informal Meeting	3.07***	-2.68	0.16***	-0.13	0.12***	-0.07	1.28***	8.70
	(0.47)	(5.01)	(0.02)	(0.29)	(0.02)	(0.21)	(0.28)	(4.48)
N	1338	1338	1338	1338	1338	1338	1025	1025
R-sq	0.0978	0.0303	0.1079	0.0459	0.0554	0.0001	0.0888	-0.2286
F Stat.		21.29		21.29		21.29		11.02
Reference:								
P75	13.46		1.10		1.10		6.24	

Panel B Economic Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Informal Meeting	0.38**	1.17	0.45	-2.21	0.28	-0.20	0.11	0.03	0.12	-0.22
	(0.14)	(1.50)	(0.27)	(3.14)	(0.26)	(2.85)	(0.08)	(0.83)	(0.07)	(0.59)
N	1338	1338	1025	1025	1025	1025	1025	1025	1025	1025
R-sq	0.1630	0.1030	0.0769	0.0092	0.0557	0.0435	0.0398	0.0185	0.0516	0.0020
		21.29		11.02		11.02		11.02		11.02
Reference:										
P75	2.08		2.08		1.95		1.10			

The table presents results from estimating Eq. (4b) in the sample of applicants rejected from due diligence. The outcome variable is specified in the title of each column. Informal Meeting is a dummy indicating the rejected applicants assigned to informal meetings. The IV models instrument Informal Meeting with IMAP, the informal meeting assignment probability estimated as in Eq. (5b). Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-stat of the excluded regressor (IMAP) in Eq. (3b). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

ONLINE APPENDIX

Appendix 1—Email templates

In this Appendix we present the email templates. For each email template the emphasis in **bold** is our own.

Due diligence email template:

Hi ,

Thanks for taking the time to share your ambition with us through the [application platform]... **We've completed our initial review and would like to meet to take our review further. Would work for you for a call or a coffee?**

We're well aware that your time is precious when building a startup, so we aim to review and provide you with what we hope is constructive feedback quickly.

We approach our initial review with the belief that any startup could be a generation-defining business. In order to surface those opportunities, we believe three separate minds are better than one. Three of our team members, including two Investment Leads and a member of the Executive Team, independently review the materials you've shared to consider whether we are the right investment partner for you at this point in your journey.

We aim to get this initial review done and share our feedback within a couple of days of receiving a full submission. We move forward if any one of the reviewers sees enough potential in the opportunity.

In the spirit of transparency, we've included each reviewer's feedback below which we can review in more detail when we meet.

The first reviewer's feedback is here;

The second reviewer's feedback is here;

The third reviewer's feedback is here;

Thanks again for considering us as a potential partner and for sharing your opportunity with us.
Best regards,

Informal Meeting email template:

Hi ,

Thanks for taking the time to share your ambition with us through the [application platform]…

We're well aware that your time is precious when building a startup, so we aim to review and provide you with what we hope is constructive feedback quickly.

We've completed our initial review and have concluded we're not currently the right investor for you. However, we would like to meet to share our feedback with you directly, learn more about your venture and stay in touch ahead of your next raise. Would work for you for a call or a coffee?

We approach our initial review with the belief that any startup could be a generation-defining business. In order to surface those opportunities, we believe three separate minds are better than one. Three of our team members, including two Investment Leads and a member of the Executive Team, independently review the materials you've shared to consider whether we are the right investment partner for you at this point in your journey.

We aim to get this initial review done and share our feedback within a couple of days of receiving a full submission. We move forward if any one of the reviewers sees enough potential in the opportunity.

In the spirit of transparency, we've included each reviewer's feedback below. We hope it's useful as you continue to pursue your venture.

The first reviewer's feedback is here;
The second reviewer's feedback is here;
The third reviewer's feedback is here;

Thanks again for considering us as a potential partner and for sharing your opportunity with us and I look forward to meeting you.

Best regards,

No meeting email template:

Hi ,

Thanks for taking the time to share your ambition with us through the [application platform]…

We're well aware that your time is precious when building a startup, so we aim to review and provide you with what we hope is constructive feedback quickly.

We approach our initial review with the belief that any startup could be a generation-defining business. In order to surface those opportunities, we believe three separate minds are better than one. Three of our team members, including two Investment Leads and a member of the Executive Team, independently review the materials you've shared to consider whether we are the right investment partner for you at this point in your journey.

We aim to get this initial review done and share our feedback within a couple of days of receiving a full submission. We move forward if any one of the reviewers sees enough potential in the opportunity.

We've completed our initial review and have concluded we're not currently the right investor for you. If you feel that we have missed something substantial you can update your pitch, otherwise we are happy to consider your opportunity again after you have made further progress. We also recognise that you may prove our decision wrong with time.

In the spirit of transparency, we've included each reviewer's feedback below. We hope it's useful as you continue to pursue your venture.

The first reviewer's feedback is here;
The second reviewer's feedback is here;
The third reviewer's feedback is here;

Thanks again for considering us as a potential partner and for sharing your opportunity with us.

Best regards,

Appendix 2—Example Data from the Fund

Web Application
Company name
Application date
What does the company do?
Web address
Contact email
Contact phone
City
Full name
Linked-In profile
When was the company founded?
Who is the customer?
What do you sell or plan to sell?
What stage is the company at?
What is the funding stage appropriate to the company?
How much are you hoping to raise?
Intended close date
Is this your first round of financing? If not please give a short history of funding since formation.
Please give links to any content you wish to share
Total addressable market (£)
Total serviceable market (£)
Document upload
Stage
How did you hear about us?
Business type

Initial review data
Date of application
Date of completion
Days to complete?
Reviewers
Reviews complete
Review score dates
(Internal) comments
External comments
Names with external comments
Actual review scores
All score array
Score array
Core score array
Max reviewer score
Min reviewer score
Reviewer scores

All reviewers
High scorer
Reviewer 2 random number
Reviewer 3 random number
Reviewer 4 random number
Review facilitator
Investment team reviewer
Score 1
Score 2
Score 3
TOTAL score
Recommended next step
Contact team by
Meet team by
Meet the team score
All perceived types
Perceived types by reviewers
Perceived stage by reviewers
Location - city
Location - region

Opportunity assessment (pre-investment committee)
Investment committee member
Date added
Company name
Stage
Is this a crowded market?
Is the market ready for the product?
Can it produce venture scale returns?
Is the business model proven?
Is there traction?
Is there risk this cannot be built?
Are the team capable of executing the plan?
Is the solution already built?
How close is the cap table to the Fund's recommended norm? Does it need fixing?
Is the company built on the platform of a 3rd party and dependent upon continued good relations?
Are the management team sufficiently independent - i.e. do they have conviction?
Are the management team sufficiently open - i.e. do they listen to advice?
Is the company likely to need more capital in future than could reasonably be raised?

Is there a legal risk of being sued for patent or copyright infringement? Are there outstanding legal issues?
Is there a risk the company has material security issues? Has it had a security audit?
Risk Score
Review Score
Status
IR and Checklist
Risk of regulatory approvals or changes impacting the business
Future Enterprise Value
Enterprise Value Justification
Disposal Mechanism
Value at Fund's Exit

Appendix 3—Randomization Checks

There are 12 reviewers in our data, including three female reviewers. The average (median) number of applicants assessed by reviewers is 400 (566), and the minimum (maximum) is 30 (796). In terms of "reviewer trios", there are 132 in total, with 44 (30) mean (median) and 3 (150) minimum (maximum) reviews per trio.Figure A31 below shows the distribution of applications, over the 12 reviewers (Panel A) and over the 132 trios (Panel B).

The proprietary software assigns numbers to incoming applications and classifies them according to the location of the business as self-reported by the applicants. There is a total of 16 regions, following the standard 12 region and nations classification of the UK, plus a further breakdown to best reflect local entrepreneurship clusters, and non-UK applicants. The locations are Cambridge, East Midlands, East of England, London, Non-UK, North East, North West, Northern Ireland, Republic of Ireland, Scotland, South Central, South East, South West, Wales, West Midlands and Yorkshire and the Humber.

Some reviewers (6 out of 12) have an explicit geographical focus. Table A31 shows the regional sample composition for each reviewer, and details reviewers' regional focus. The table shows that the reviewers with the regional focus are more likely to be assigned applicants that are located within their regions. For example, the table shows that the regional distribution of applicants for reviewer 12 is concentrated relative to the overall regional distribution of applicants in London, Southwest and Wales (50.9% vs. 44.%, 8.1% vs. 4.2%, and 1.8% vs. 0.9%), which correspond to this reviewers' geographical focus areas.

Yet, the regional focus match between applicants and reviewers is neither sufficient nor necessary for an assignment. Table A31 shows that all but two reviewers (Reviewer 1 and 2) assess applicants from all 16 regions. The remaining two reviewers assess 10 (Reviewer 1) and 14 (Reviewer 2) regions, respectively. These reviewers are also those with the fewest number of applications as they are newer to the firm, and which helps explain why their assessment sample not cover all the regions.

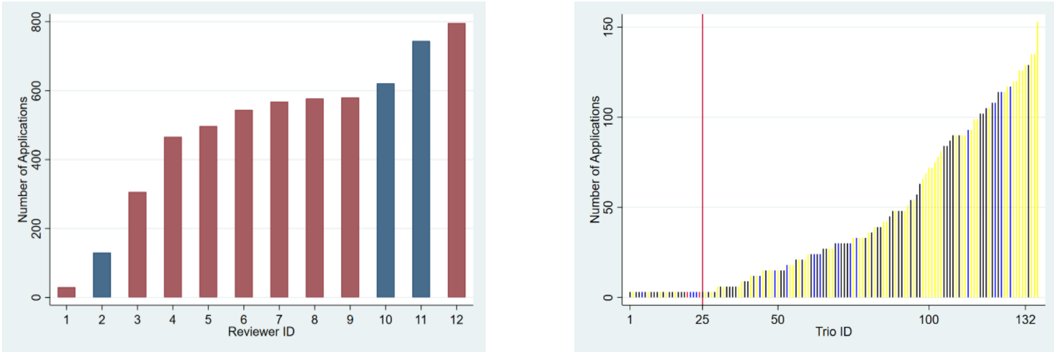
The pool of reviewers for applicant assignment is 12 for 9 of the 16 locations (56.3%), 11 for 6 of the 16 locations (37.5%), and 10 for 1 of the 16 locations (6.25%). The regions with 11 reviewers in the pool are: East of England, Non-UK, North East, Northern Ireland, Scotland, South Central. The region with 10 reviewers in the pool is Wales.

We provide evidence to support the assertion that the assignment of applications to reviewers is random conditional on the location of the applicant. We regress businesses' and applicants' characteristics at application against reviewer fixed effects. We test for balance in sample composition across reviewers by assessing the joint significance of the reviewer fixed effects. The dependent variables are: the age of the business, the gender of the founding team (female equals 1 if at least one founder is female), the stage of development (a dummy indicating a pre-seed or seed company), the business model (a dummy indicating companies doing direct sales), the total addressable and serviceable markets and the target amounts (all as reported by the applicants), and the location of the business (a dummy that equals one for businesses in London).

Table A32 below reports the F-tests and p-values of the reviewer fixed effects across the different business and applicant characteristics. We reject the equality of the reviewer fixed effects for all variables. The only exception is the location variable, where consistent with the regional allocation we reject of equality of reviewer fixed effects when we use as dependent variable an indicator variable for businesses in London.

Figure A31—Distribution of Applications across Reviewers and Trios

Panel A—Distribution over Reviewers **Panel B—Distribution over Trios**



The figure plots the number of applications evaluated by each reviewer (Panel A) and by each trio of reviewers (Panel B).

Table A31 Regional Composition of Each Reviewer's Assessment Samples

Reviewer ID	No. of Reviewed Applications	Wales	Republic of Ireland	Northern Ireland	East Midlands	North East	Eeast England	Cambridge	Yorkshire & Humber	South Central	West Midlands	North West	South West	South East	Scotland	Non-UK	London	**Geographic focus**
ALL	***5859***	***0.9%***	***1.1%***	***1.4%***	***1.2%***	***1.4%***	***1.5%***	***1.8%***	***2.2%***	***2.6%***	***4.0%***	***4.5%***	***4.2%***	***4.4%***	***4.9%***	***20.0%***	***44.2%***	
12	795	***1.8%***	0.8%	0.8%	1.0%	1.0%	1.0%	1.0%	1.0%	1.9%	2.0%	2.1%	***8.1%***	3.1%	1.9%	21.6%	***50.9%***	London, Southwest + Wales
11	742	0.9%	0.4%	0.4%	***2.3%***	0.5%	1.5%	1.8%	1.3%	5.7%	***7.8%***	2.3%	3.0%	3.4%	2.4%	19.0%	***47.3%***	London, Midlands + Oxford
10	618	0.5%	0.5%	1.5%	1.0%	1.0%	0.6%	1.5%	1.3%	0.8%	3.7%	2.8%	3.1%	7.8%	3.7%	15.9%	***54.5%***	London
8	582	1.2%	0.7%	1.5%	0.7%	1.5%	1.4%	1.7%	2.2%	3.4%	4.6%	4.8%	4.8%	6.0%	5.0%	13.6%	46.7%	
9	580	0.7%	1.0%	1.0%	1.2%	2.1%	1.7%	1.7%	1.9%	2.2%	4.3%	4.5%	3.1%	3.6%	3.8%	24.0%	43.1%	
7	568	0.4%	1.6%	1.9%	0.4%	1.1%	1.2%	1.1%	2.6%	1.6%	3.2%	***8.6%***	4.0%	1.1%	***14.3%***	26.1%	31.0%	Scotland + Northwest
6	538	***1.3%***	1.3%	1.3%	1.3%	0.4%	1.7%	2.2%	2.0%	1.7%	3.0%	3.5%	4.5%	5.4%	5.6%	20.3%	44.6%	
5	498	0.2%	1.4%	1.6%	2.2%	1.6%	1.0%	1.8%	2.6%	2.0%	4.0%	7.0%	4.2%	4.4%	7.0%	13.9%	45.0%	
4	468	0.2%	2.8%	***4.1%***	0.2%	***4.1%***	1.3%	1.3%	6.2%	3.0%	2.6%	5.3%	2.6%	3.6%	2.4%	27.4%	33.1%	Northeast + Northern Ireland
3	307	1.6%	0.7%	0.7%	0.3%	1.3%	***4.6%***	***4.2%***	1.3%	3.6%	2.9%	5.2%	1.6%	3.3%	5.9%	26.4%	36.5%	Cambridge
2	134	0.0%	1.5%	0.0%	3.0%	2.2%	3.7%	6.0%	1.5%	1.5%	5.2%	5.2%	5.2%	10.4%	4.5%	4.5%	45.5%	
1	29	0.0%	3.4%	3.4%	3.4%	0.0%	0.0%	3.4%	6.9%	0.0%	10.3%	17.2%	10.3%	10.3%	0.0%	0.0%	31.0%	

This table presents the regional composition of each reviewers' assessment samples. The underlined and italic cells indicate the regions of focus of the different reviewers.

Table A32—Randomization Checks across Business and Founder Characteristics

Dependent Variable	Obs.	Reviewer F.E.		Reviewer F.E. Conditional on Speciality of Region		Reviewer F.E. Conditional on Region	
		F Stat.	*p-Value*	F Stat.	*p-Value*	F Stat.	*p-Value*
Age	5837	1.646	(0.079)	1.618	(0.087)	1.291	(0.222)
ln(Age)	5837	1.284	(0.227)	1.252	(0.246)	1.025	(0.421)
Female Founder	5340	0.966	(0.475)	0.946	(0.494)	0.667	(0.771)
Russell Education of Founder	5837	1.058	(0.391)	0.839	(0.601)	0.432	(0.942)
Amount	4872	0.585	(0.843)	0.580	(0.847)	0.643	(0.793)
ln(Amount)	4872	0.389	(0.961)	0.367	(0.969)	0.377	(0.965)
Target Close Days	4881	1.031	(0.416)	1.010	(0.434)	0.962	(0.479)
ln(Target Close Days)	4869	1.272	(0.234)	1.250	(0.248)	1.153	(0.315)
Total Addressable Market	4285	0.566	(0.858)	0.563	(0.86)	0.517	(0.893)
ln(Total Addressable Market)	4285	2.095	(0.018)	2.039	(0.022)	1.678	(0.0719)
Total Servicable Market	4285	1.053	(0.396)	1.037	(0.411)	1.043	(0.405)
ln(Total Servicable Market)	4285	0.780	(0.660)	0.740	(0.701)	0.606	(0.826)
Seed/Pre-Seed	5837	1.258	(0.242)	1.260	(0.241)	1.081	(0.372)
Deep Tech	5837	1.719	(0.063)	1.699	(0.067)	1.261	(0.241)
Platform	5837	2.301	(0.008)	2.287	(0.009)	1.380	(0.175)
London	5837	9.883	(0.000)				
London (Reviewers Assigned by Region Rules)	3491	20.510	(0.000)				
London (Reviewers Assigned without Region Rules)	2346	1.389	(0.225)				
Financial Status Before App.							
Asset (£1000s)	4625	0.756	0.685	0.754	0.687	0.712	0.729
ln(Assets)	4625	1.147	0.319	1.148	0.319	1.014	0.431
Debt (£1000s)	4625	0.736	0.704	0.734	0.706	0.693	0.746
ln(Debt)	4625	0.839	0.601	0.856	0.584	0.840	0.600
Equity Issuance (£1000s)	4625	0.918	0.522	0.918	0.522	0.877	0.563
ln(Equity Issuance)	5837	0.653	0.784	0.653	0.784	0.668	0.770
Num. of Funding Rounds	5837	0.932	0.508	0.911	0.528	0.743	0.697
ln(# Rounds)	5837	0.809	0.631	0.773	0.668	0.579	0.847
Total Funding (£1000s)	5837	0.609	0.823	0.603	0.828	0.509	0.898
ln(Funding)	5837	0.630	0.804	0.635	0.801	0.578	0.848
Num. of Companies Created	5837	0.965	0.476	1.026	0.420	0.907	0.533
ln(# Companies Created)	5837	0.957	0.484	0.996	0.447	0.874	0.565
Serial Entrepreneur	5837	0.817	0.623	0.832	0.608	0.744	0.697

The table shows the F test of the joint significance of reviewer fixed effects for different dependent variables. The last two rows represent two subsamples: reviewers assigned by geographical focus rules and reviewers assigned without geographical rules. Specification (1) includes no controls; specification (2) include a dummy "speciality" indicating if the region is focused by any reviewers; specification (3) includes region specific fixed effects.

Appendix 4—Reviewer Heterogeneity in Scores

We provide evidence of systematic differences across reviewers in scoring generosity by exploiting the multiple reviewers assignment per applicant to run fixed effects models of application scores against reviewer and applicant fixed effects. Our approach is similar to the methodologies in papers assessing the importance of managers in corporations (cf. Bertrand and Schoar, 2003) and general partners in limited partnerships (Ewens and Rhodes-Kropf, 2015). The idea is that reviewer fixed effects would be jointly significant if reviewers systematically vary in their tendency to assign high or low scores to applicants.

We begin by decomposing individual scores into applicant and reviewer fixed effects using the following regression:

$$Score_{i,h} = \mu_h + \alpha_i + X_{i,h} + \epsilon_{i,h} \quad (A41)$$

where $Score_{i,h}$ denotes the score assigned by reviewer h to company i; μ_h and α_i are full sets of reviewer and applicant FE. $X_{i,h}$ denote control variables we include in the estimation to reflect the level of randomization level—i.e., location of applicants.[1] The reviewer fixed effects are meant to capture heterogeneity across reviewers in their scoring generosity. By contrast, the applicant fixed effects can be understood as the underlying potential and fit of the applicants that all reviewers agree on; they represent "adjusted scores" after controlling for potential systematic differences in scoring generosity across reviewers.

Figure A42 plots the distribution of fixed effects across reviewers. Figure A43 plots the distribution of applicant fixed effects.

There are three main findings from estimating equation (A41):

First, there is statistically significant heterogeneity in scoring generosity across reviewers: the F-test on the joint significance of the reviewer fixed effects is 10.63 (p-value of 0.00). By contrast, if reviewer heterogeneity was irrelevant (or nonsystematic), then reviewer fixed effects would not be jointly significant (as reviewers are quasi-randomly assigned by design). Consistent with the quasi-random assignment of reviewers to applicants, Table A41 confirms that the scoring heterogeneity is not related to differences in the types of applicants that reviewers assess: the sample of applicants is balanced across different quartiles of reviewer generosity.

[1] In some specifications we also include other controls like the reviewers' perception of the stage and busines type of the business, but these controls are immaterial.

To address concerns regarding the validity of *F*-tests in the presence of high serial correlation (Wooldridge, 2002), we scramble the data 500 times, each time randomly assigning reviewers' scores to different applicants in the same spirit as in Fee, Hadlock, and Pierce (2013).[2] In this scrambled samples we hold constant the number of projects evaluated by each reviewer, make sure that each applicant receives three scores from reviewers specialized in the same location and available at the time of application.[3] Then we proceed to estimate the "scrambled" applicants' and reviewers' fixed effects and test the joint significance of the latter in each scrambled sample. The distribution of the scrambled *F*-tests is plotted in Figure A44 (Panel A). Lending credence to the statistically significant reviewer heterogeneity in our setting, we reject the null of "no joint significance of the reviewer fixed effects" in only 4.4% of the placebo assignments (the largest estimated placebo *F*-test is 3.12).

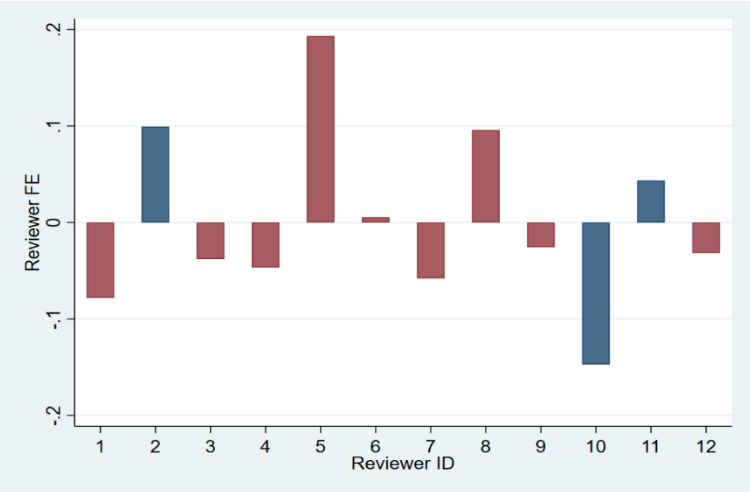
The second finding is the sizable *economic* significance of the scoring generosity heterogeneity. Figure A44 shows that generous reviewers (with positive FE) are twice as likely to assign a score of "3" or "4" than stricter reviewers with negative FE across all applicant fixed effects deciles. On average, this probability is 31.1% for applicants with generous reviewers and 17.9% for applicants with stricter reviewers

The third finding is that these systematic differences across reviewers are unrelated to the reviewers' skill in distinguishing high potential applicants and instead reflect reviewers' propensities to assign high or low application scores. Figure A45 shows a nil correlation between reviewers' generosity and their ability to correctly rank applicants. We measure reviewers' ranking ability using the correlation between a "reviewers' s ranks" and "actual ranks." To produce this correlation, for every reviewer we rank the companies she evaluated based on (i) average annual funding post application ("actual rank") and (ii) the reviewer's score ("reviewer's rank). Figure A45 is a scatterplot of each reviewer's generosity and ranking ability for the 12 reviewers in our sample.

[2] In the parallel literature, when seeking to identify the "style" of managers using an endogenous assignment of (movers) managers to multiple companies (e.g., Bertrand and Schoar, 2003), concerns have been raised regarding the validity of *F*-tests in the latter settings on the grounds of (a) the particularly acute endogeneity in samples of job movers and (b) the high level of serial correlation in most of the variables of interest (see Fee, Hadlock, and Pierce, 2013). The first reason for concern is not at play in our setting, as reviewers are randomly assigned by design, but the second concern may still apply. Regarding the second concern, Heckman (1981) and Greene (2001) discuss the ability of small sample sizes per group to allow for meaningful estimates of fixed effects with a rule of thumb of eight observations per group.

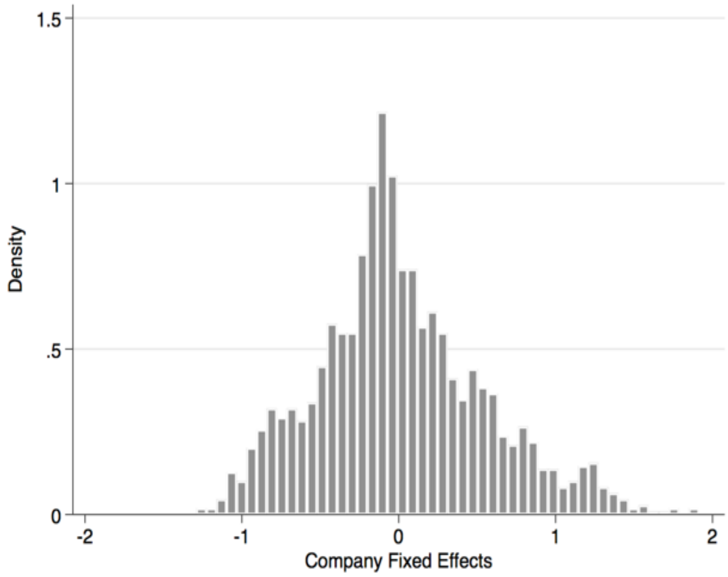
[3] We make sure the reviewer was assigned at least one application to review within 3 months of the company's application date.

Figure A41—Distribution of Reviewer Fixed Effects



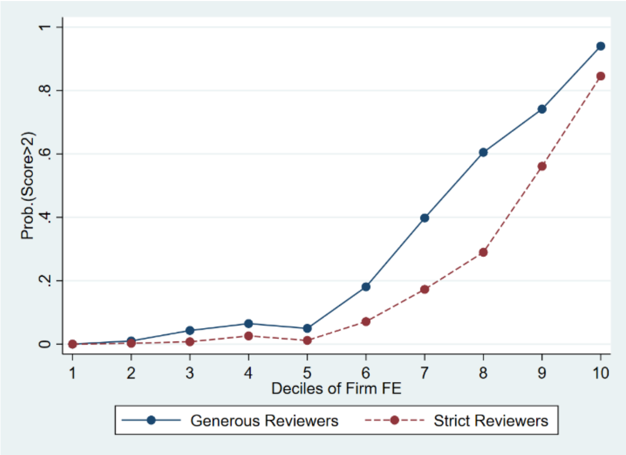
The figure plots the reviewer fixed effects for each reviewer in the sample based on the estimates of equation A41. Blue columns indicate female reviewers.

Figure A42—Distribution of Applicant Fixed Effects



The figure plots the applicant fixed effects for each applicant in the sample based on the estimates of equation A41.

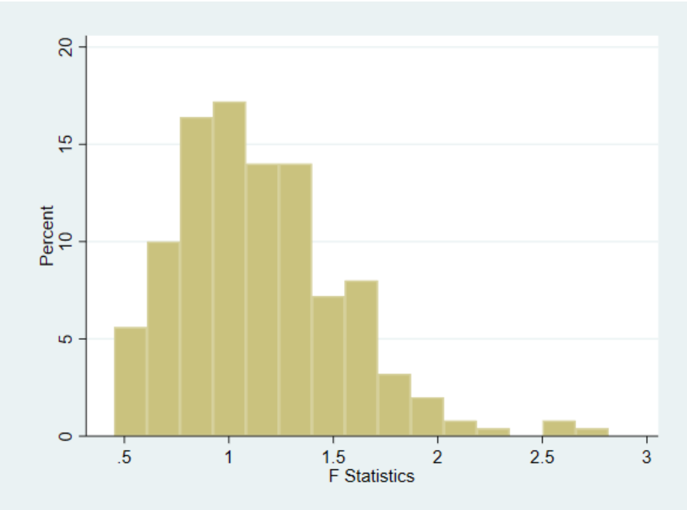
Figure A43—Frequency of Scores Above 2 and Reviewer FE



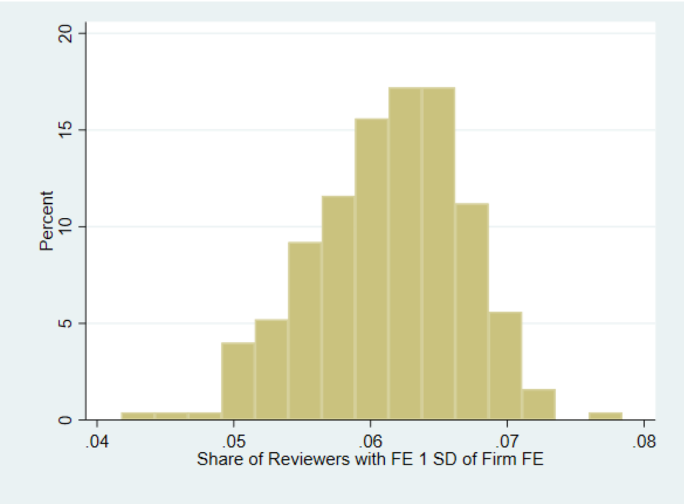
The figure plots the probability of a score higher than 2, separately for reviewers with positive and negative fixed effects (from Eq. A41).

Figure A44—Placebo Tests Reviewer Fixed Effects

Panel A— Distribution of *F*-values

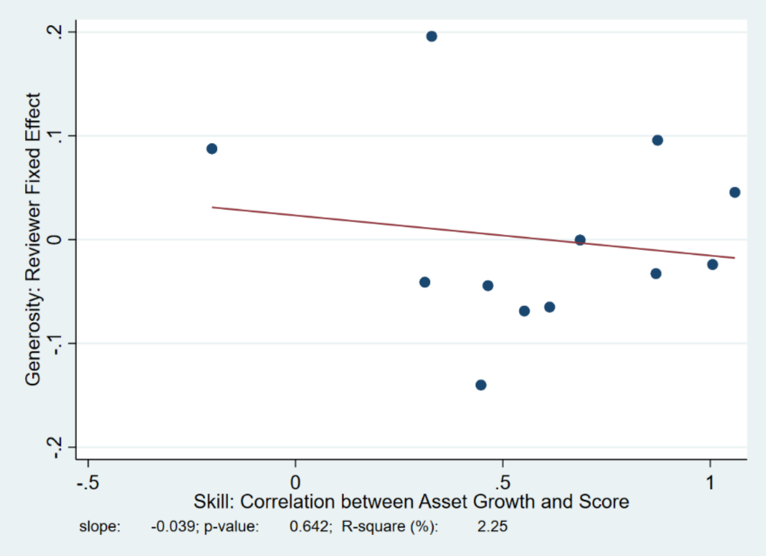


Panel B— Fixed Effects One Standard Deviation Above/Below Applicant Effect



This figure plots the distribution of *F*-tests on the joint significance of the reviewer fixed effects in 500 placebo assignments.

Figure A45—Reviewer Fixed Effects and Ranking Ability of Reviewers



This plot is a scatter plot of reviewers' scoring generosity and ranking ability. We measure reviewer' ranking ability using the correlation between a "reviewers' rank" and "actual rank". To produce this correlation, for every reviewer we rank the applicants she evaluated based on 1) average annual funding post application ("actual rank") and 2) the reviewer's score (" reviewer's rank").

Table A41—Balance of Covariates Across Generosity Quartiles

Variable	Q1	Other Q	*p-value diff. in mean*	Q2	Other Q	*p-value diff. in mean*	Q3	Other Q	*p-value diff. in mean*	Q4	Other Q	*p-value diff. in mean*
App. Info												
Age	2.61	2.61	*0.51*	2.49	2.65	*0.22*	2.55	2.63	*0.95*	2.85	2.55	*0.03*
ln(Age)	1.05	1.06	*0.44*	1.03	1.06	*0.64*	1.05	1.06	*0.70*	1.10	1.04	*0.07*
Female Founder	0.12	0.13	*0.91*	0.13	0.13	*0.86*	0.14	0.12	*0.30*	0.11	0.13	*0.19*
Russell Education of Founder	0.15	0.17	*0.31*	0.15	0.17	*0.22*	0.18	0.16	*0.12*	0.18	0.16	*0.52*
Amount	2542.83	2153.36	*0.57*	1728.48	2422.04	*0.27*	2210.44	2245.20	*0.96*	2623.39	2132.98	*0.49*
ln(Amount)	6.58	6.64	*0.26*	6.67	6.62	*0.71*	6.63	6.63	*0.68*	6.64	6.63	*0.73*
Target Close Days	82.08	80.51	*0.92*	82.16	80.34	*0.37*	80.30	81.08	*0.63*	78.67	81.40	*0.58*
ln(Target Close Days)	4.23	4.22	*0.80*	4.23	4.22	*0.63*	4.22	4.22	*0.45*	4.20	4.23	*0.25*
Total Addressable Market	1147.71	618.44	*0.61*	942.66	655.37	*0.72*	807.58	697.59	*0.94*	6.54	946.75	*0.32*
ln(Total Addressable Market)	0.46	0.45	*0.87*	0.48	0.45	*0.33*	0.46	0.45	*0.97*	0.42	0.47	*0.20*
Total Servicable Market	78.78	44.17	*0.08*	63.96	47.08	*0.19*	56.63	49.30	*0.86*	5.63	65.20	*0.56*
ln(Total Servicable Market)	0.24	0.20	*0.10*	0.22	0.20	*0.80*	0.19	0.21	*0.62*	0.18	0.22	*0.37*
London	0.43	0.44		0.41	0.45		0.47	0.43		0.46	0.44	
Seed/Pre-Seed	0.45	0.44	*0.27*	0.45	0.44	*0.95*	0.44	0.44	*0.49*	0.43	0.45	*0.75*
Platform	0.51	0.53	*0.10*	0.51	0.53	*0.84*	0.55	0.52	*0.08*	0.53	0.52	*0.95*
Deep Tech	0.03	0.05	*0.08*	0.04	0.04	*0.91*	0.04	0.04	*0.44*	0.06	0.04	*0.07*
<u>CH Info. Before App.</u>												
Asset (£1000s)	240.57	744.68	*0.40*	1220.37	434.29	*0.11*	667.45	628.52	*0.99*	278.42	740.50	*0.39*
Debt (£1000s)	2.81	2.92	*0.26*	2.90	2.89	*0.63*	2.87	2.91	*0.58*	3.01	2.86	*0.19*
Annual Equity Issuance (£1000s)	230.17	709.69	*0.43*	1175.80	409.59	*0.13*	643.19	595.98	*0.96*	239.40	713.07	*0.38*
ln(Debt)	2.53	2.60	*0.38*	2.59	2.58	*0.54*	2.54	2.60	*0.51*	2.69	2.56	*0.30*
Equity Issuance (£1000s)	254.53	333.07	*0.15*	353.71	303.76	*0.18*	320.74	315.11	*0.87*	325.95	314.39	*0.81*
ln(Equity Issuance)	2.30	2.41	*0.36*	2.39	2.39	*0.56*	2.37	2.39	*0.50*	2.49	2.36	*0.27*
<u>Web Info. Before App.</u>												
Num. of Funding Rounds	1.13	1.20	*0.08*	1.18	1.19	*0.98*	1.20	1.18	*0.57*	1.22	1.18	*0.20*
ln(# Rounds)	0.72	0.74	*0.12*	0.73	0.74	*0.68*	0.74	0.73	*0.50*	0.75	0.73	*0.17*
Total Funding (£1000s)	381.68	402.71	*0.85*	400.61	397.51	*0.99*	367.47	412.53	*0.44*	459.00	382.50	*0.49*
ln(Funding)	2.72	2.95	*0.36*	2.92	2.90	*0.67*	2.84	2.93	*0.68*	3.16	2.84	*0.31*
Num. of Companies Created	0.38	0.41	*0.23*	0.38	0.41	*0.42*	0.42	0.39	*0.29*	0.41	0.40	*0.32*
Ln(# Companies Created)	0.21	0.24	*0.08*	0.22	0.23	*0.65*	0.25	0.22	*0.22*	0.24	0.23	*0.32*

Serial Entrepreneur	0.24	0.27	*0.13*	0.26	0.26	*0.92*	0.28	0.25	*0.19*	0.27	0.26	*0.47*

The table compares applicants' characteristics (at application) across the different quartiles of reviewers' generosity.

Appendix 5 – Measuring Comments' Style and its Heterogeneity Across Reviewers

We use text analysis tools to analyse the content of the reviewers' comments. We build a text classification model based on the pre-trained model, Bidirectional Encoder Representations from Transformers (BERT). BERT has been trained on a large corpus of unlabelled text including the entire Wikipedia and Book Corpus.[4]

We fine-tune the BERT model to classify reviewers' comments in terms of their sentiment and practical advice by using a random sample that we read manually. BERT is designed to pre-train deep bidirectional representations from unlabelled text. For more details, see Devlin et. Al (2018) and Vaswani (2017).

In detail, we randomly select 1000 comments and read them manually to classify them as positively, negatively or neutrally toned. We also classify the comments into two additional non-mutually exclusive categories, depending on whether the comments provide any practical advice on financing opportunities (e.g. participate in other programs, such as the seed enterprise investment scheme that is a tax incentive program for individual investments in UK startups), or employment decisions (e.g. hire a chief technology officer or other key persons), and product improvements or market strategy. We then use this manual classification to train BERT and construct four measures of comments' content: *Sentiment* (increasing in positive tone), *Finance and Hiring*, *Product and Strategy*, and Length (word count). Table A51 presents summary statistics of the comments' content measures so-constructed.

Having classified comments in terms of their length, sentiment and practical advice, we then start by investigating the relation between scoring generosity and comments' content. Table A52 shows no evidence of a statistically significant correlation between the content of reviewers' comments and their generosity, although more generous reviewers write shorter comments on average.

The lack of variation in comments' content by reviewers' generosity does not necessarily imply that reviewers do not vary in the ways in which they provide comments. We turn to investigating further whether reviewers vary in terms of their comments to applicants.

We run regressions of the different measures of comments' content against applicant and reviewer fixed effects. Like our exploration of heterogeneity in reviewers' scoring, the idea behind this approach is that reviewer fixed effects would be jointly significant if reviewers systematically vary in their length and style of comments to applicants.

We run the following type of regression:

$$Content_{i,h} = \mu_h + \alpha_i + X_{i,h} + \epsilon_{i,h} \quad (A51)$$

where $Content_{i,h}$ denotes different proxies for the content of the comments provided by reviewer h to company i; μ_h and α_i are full sets of reviewer and applicant FE. $X_{i,h}$ denote location fixed effects, score

[4] BERT is designed to pre-train deep bidirectional representations from the unlabelled text by jointly conditioning on both left and right contexts. As a result, the pre-trained BERT model can be fine-tuned with just one additional output layer to create state-of-the-art models for a wide range of NLP tasks. For more details, see Devlin et. Al (2018) and Vaswani (2017).

fixed effects, and log transformation (log (1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market .

The reviewer fixed effects are meant to capture heterogeneity across reviewers in their comments' length and style. By contrast, the applicant fixed effects can be understood as the underlying comments that all reviewers agree on; they represent "adjusted comments" after controlling for potential systematic differences in comment styles' across reviewers.

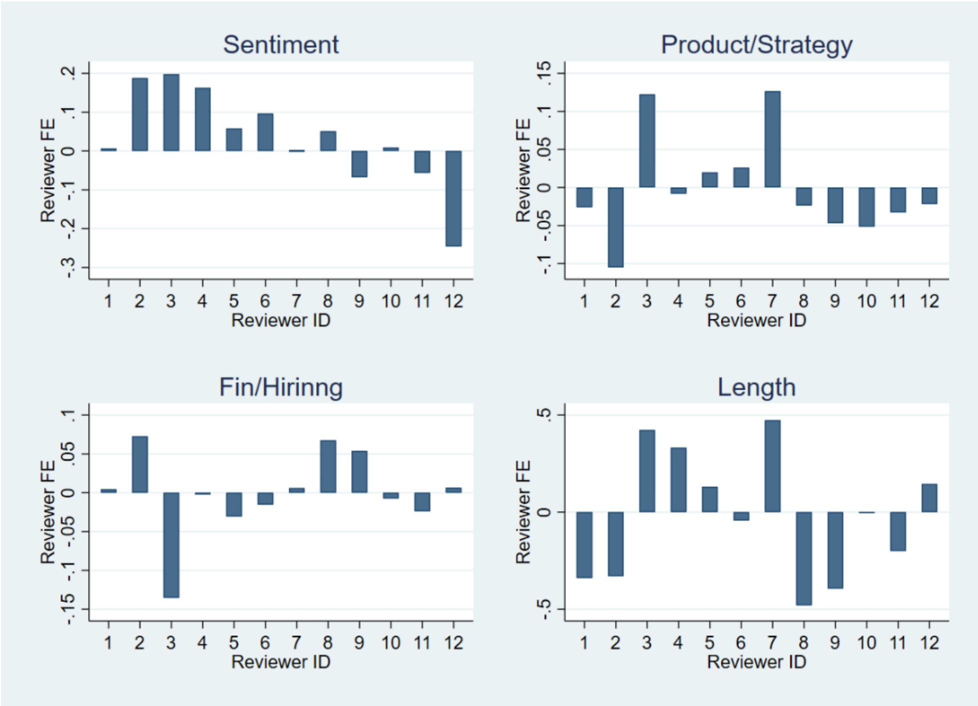
Figure A51 plots the distribution of fixed effects across reviewers. Figure A52 plots the distribution of applicant fixed effects.

We find statistically significant heterogeneity in comments' styles across reviewers: the F-test on the joint significance of the reviewer fixed effects is 73.08 (p-value of 0.00) for sentiment, 12.64 (p-value of 0.00) for finance/hiring, 8.77 (p-value of 0.00) for product/strategy and 111.47 (p-value of 0.00) for length. By contrast, if reviewer heterogeneity in comments' content was irrelevant (or nonsystematic), then reviewer fixed effects would not be jointly significant (as reviewers are quasi randomly assigned by design).[5]

We provide additional evidence of the lack of systematic variation in the type of comments across between more and less generous reviewers by correlating the generosity of reviewers (as measured by the reviewer fixed effects from regression A41) and the reviewer fixed effects we estimate in regression A51. We find no significant correlation between generosity and any of the reviewer fixed effects based on the content proxies, including length. Figure A53 shows the nil correlation between reviewers' generosity and the different proxies of the content in reviewers' comments.

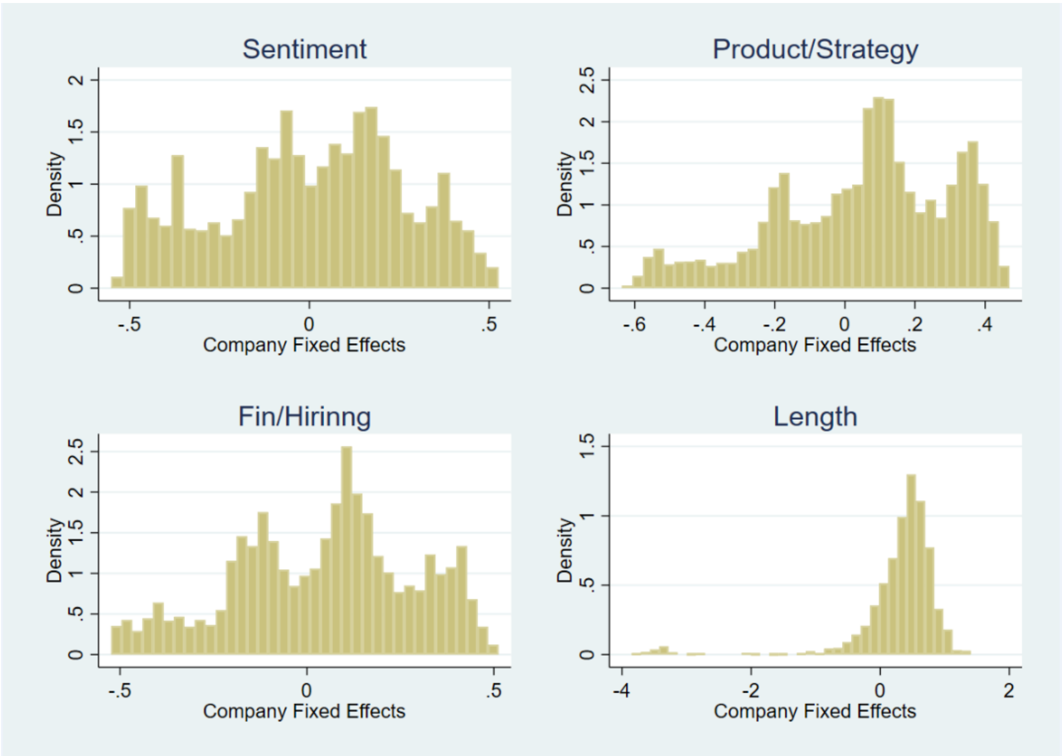
[5] In unreported analysis, we condition on scores to investigate whether comments vary across reviewers for a given score. We expand equation A51 to include reviewer-score fixed effects. We find evidence of heterogeneity conditional on score: the F-test on the joint significance of the reviewer-score fixed effects is 38.84 (p-value of 0.00) for tone, 4.97 (p-value of 0.00) for finance, 5.35 (p-value of 0.00) for operations and 32.16 (p-value of 0.00) for length.

Figure A51 – Distribution of Reviewer Fixed Effects



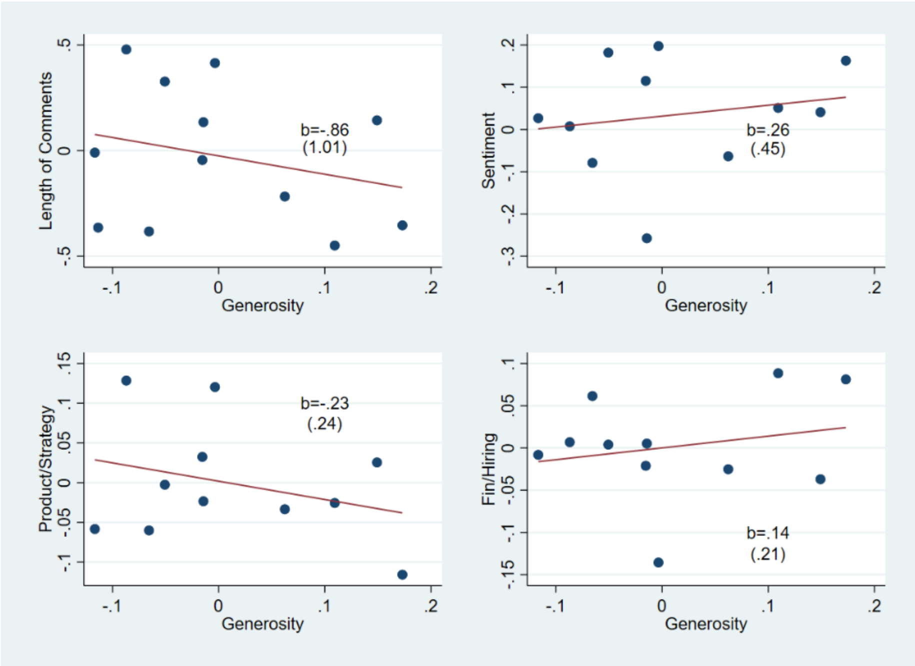
The figure plots the reviewer fixed effects for each reviewer in the sample based on the estimates of equation A51.

Figure A52 – Distribution of Applicant Fixed Effects



The figure plots the applicant fixed effects for each applicant in the sample based on the estimates of equation A51.

Figure A53 – Reviewers' Generosity and Comments' Content



The figure shows scatter plots of reviewers' scoring generosity and different proxies of the content in reviewers' comments.

Table A51 – Summary Statistics Comments' Content Measures

	Mean	Sd	p5	p25	p50	p75	p95	Obs.
Sentiment	0.492	0.377	0.020	0.060	0.641	0.870	0.900	5177
Product/Strategy	0.629	0.377	0.037	0.185	0.843	0.963	0.980	5177
Fin/Hiring	0.538	0.365	0.027	0.103	0.722	0.848	0.962	5177
Length of Comments	3.547	1.347	0.000	3.332	3.932	4.357	4.875	5794
Word Counts	55.393	40.120	0	27	50	77	130	5794

The table shows the summary statistics of comments' content measures. Length of comments is the log transformation (log(1+x)) of word counts of non-symbol words (such as comma, question mark etc.) in the comment text. There are missing observations in the variables for two reasons: (1) the reviewer didn't make comments; (2) there is not enough information in the comment text for the algorithm to assign values to these observations.

Table A52 – Reviewers' Generosity and Comments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sentiment		Product / Strategy		Financial / Hiring		Length of Comments	
Generosity	0.08	0.11	-0.08	-0.08	0.02	0.00	-1.25***	-1.23***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.11)	(0.11)
Constant	0.43***	0.48***	0.75***	0.67***	0.57***	0.56***	3.96***	3.91***
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.09)	(0.07)
N	5177	5177	5177	5177	5177	5177	5794	5794
R-sq	0.1150	0.1173	0.0594	0.0580	0.0353	0.0364	0.1031	0.1031
Controls	No	Yes	No	Yes	No	Yes	No	Yes

The table correlates the content of reviewer comments and generosity. The observations are at the applicant-reviewer level, and generosity correspond to the reviewer fixed effects estimated in Appendix 4 (equation A41). In the regressions, we include as controls the log transformations (log(1+x)) of variables in the application files:

age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region and score fixed effects are also included in all regressions. The row Controls indicates the inclusion as controls of the company fixed effects estimated in Appendix 4 (equation A41). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 6—DAP and Venture Outcomes

Table A61 - DAP and Venture Outcomes

Panel A-Implied Coefficient Magnitudes

	Funding	# Rounds	# Investors	Equity issuance (UK)	# Employees	Growth in Assets (UK)	Growth in Debt (UK)	# Directors (UK)
Coefficient (Panel A: full sample)	2.81	0.18	0.09	1.21	0.46	0.93	1.16	0.3
Coefficient (Panel B: excluding portfolio companies)	2.74	0.18	0.09	1.11	0.44	0.89	1.12	0.29
log P75	13.46	1.1	1.1	6.24	2.08	2.05	1.95	1.1
level P75	698	2	2	255	7	167	85	2
Implied effect in percentage	21%	16%	8%	19%	22%	45%	59%	27%
Implied effect in level	146	0.33	0.16	49	1.55	76	51	0.55
Implied effect in percentage (excluding portfolio companies)	20%	16%	8%	18%	21%	43%	57%	26%
Implied effect in level (excluding portfolio companies)	142	0.33	0.16	45	1.48	73	49	0.53

Panel B-Reduced Form Estimates

	Panel A: Funding – Full Sample				
	ln(Funding)	ln(# Rounds)	ln(# Investors)	ln(Equity Issuance) (UK)	
	(1)	(2)	(3)	(4)	
DAP	3.73**	0.24**	0.12*	1.65**	
	(1.13)	(0.08)	(0.05)	(0.59)	
N	1953	1953	1953	1548	
R-sq	0.1030	0.1109	0.0516	0.0828	
	Panel B: Economic Growth – Full Sample				
	ln(#Employees)	Growth in Assets (UK)	Growth in Debt (UK)	ln(# Directors) (UK)	Survival (UK)
	(1)	(2)	(3)	(4)	(5)
DAP	0.62**	1.27**	1.59***	0.41***	-0.14
	(0.23)	(0.47)	(0.40)	(0.12)	(0.08)
N	1953	1548	1548	1548	1548
R-sq	0.1319	0.0803	0.0624	0.0405	0.0461

Panel A summarizes the implied coefficient estimates relative to the 75th percentiles of each outcome variable for the entire sample of applicants. Panel B presents reduced form estimates regressing the different outcome variables against DAP. Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 7—DAP and reviewers' comments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sentiment		Product / Strategy		Financial / Hiring		Length of Comments	
DAP	-0.04	-0.04	-0.10*	-0.09	0.03	0.03	-0.58***	-0.57***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.08)	(0.08)
Constant	0.44***	0.49***	0.78***	0.69***	0.56***	0.56***	4.09***	4.02***
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.09)	(0.07)
N	5177	5177	5177	5177	5177	5177	5794	5794
R-sq	0.1149	0.1169	0.0600	0.0584	0.0354	0.0365	0.0886	0.0893
Controls	No	Yes	No	Yes	No	Yes	No	Yes

The table correlates the content of reviewer comments and DAP. The observations are at the applicant-reviewer level, and DAP is a constant measure for a given applicant across reviewers. In the regressions, we include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region and score fixed effects are included in all regressions. There are a few cases that reviewers don't have comments (results are robust to replacing the variables of comments' style with zero in those instance). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 8—DAP and, Opportunity Assessment and Investment

Panel A—Probability of Opportunity Assessment and Investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Opportunity Assessment				Investment			
DAP	0.04	0.03	0.00	-0.00	0.01	0.01	0.01	0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Applicant FE			0.07***	0.07***			0.01*	0.01
			(0.01)	(0.01)			(0.00)	(0.00)
Controls		Yes		Yes		Yes		Yes
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953
R-sq	0.0010	0.0151	0.0716	0.0799	0.0007	0.0145	0.0027	0.0159

Panel B—Opportunity Assessment Performance

Question	mean	sd	p25	p50	p75	Obs	Correlation with DAP	*p-value*
Is this a crowded market?	5.19	1.73	4.00	5.33	6.50	45	-0.10	(0.504)
Is the market ready for the product?	5.34	1.46	4.42	5.50	6.17	45	-0.14	(0.345)
Can it produce venture scale returns?	4.79	1.32	4.00	4.55	5.50	45	-0.18	(0.229)
Is the business model proven?	6.63	1.47	5.50	7.00	7.67	45	-0.01	(0.950)
Is there traction?	6.55	1.55	5.50	6.83	7.50	45	-0.02	(0.869)
Is there risk this cannot be built?	5.67	1.56	4.50	5.50	7.00	45	-0.07	(0.635)
Are the team capable of executing the plan?	5.40	1.40	4.67	5.50	6.50	45	-0.01	(0.23)
Is the solution already built?	5.34	1.41	4.13	5.50	6.10	45	-0.07	(0.626)
How close is the cap table to the Fund's recommended norm? Does it need fixing?	4.73	2.06	3.00	4.75	5.50	45	-0.23	(0.111)
Is the company built on the platform of a 3rd party and dependent upon continued good relations?	6.13	1.97	5.00	6.00	8.00	45	-0.17	(0.261)
Are the management team sufficiently independent - i.e. do they have conviction?	3.26	1.16	2.42	3.00	4.00	45	-0.12	(0.405)
Are the management team sufficiently open - i.e. do they listen to advice?	4.21	1.20	3.00	4.00	5.00	45	-0.14	(0.328)
Is the company likely to need more capital in future than could reasonably be raised?	6.62	1.27	6.00	7.00	7.50	45	0.06	(0.674)
Is there a legal risk of being sued for patent or copyright infringement? Are there outstanding legal issues?	4.44	1.78	3.00	4.00	5.75	45	0.05	(0.736)
Is there a risk the company has material security issues? Has it had a security audit?	5.10	1.85	3.50	5.00	6.54	45	0.11	(0.45)
Risk Score	422.45	56.00	385.88	420.17	465.00	45	-0.23	(0.120)

Panel A presents results from regressing Opportunity Assessment (a variable indicating applicants that made it to the Fund's third stage of due diligence) and Investment (a variable indicating applicants that are in the Fund's investment portfolio) against due diligence assignment probability(DAP).Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the funding, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. Panel B shows the summary statistics of opportunity assessment results at the applicant level. The opportunity assessment involves scoring for 15 questions (scale of 10) and providing risk score. For each question and risk score, I first take the average across different reviewers for each company and summarize the statistics as shown above. In particular, we show their' correlation coefficients with DAP and the corresponding p-values.

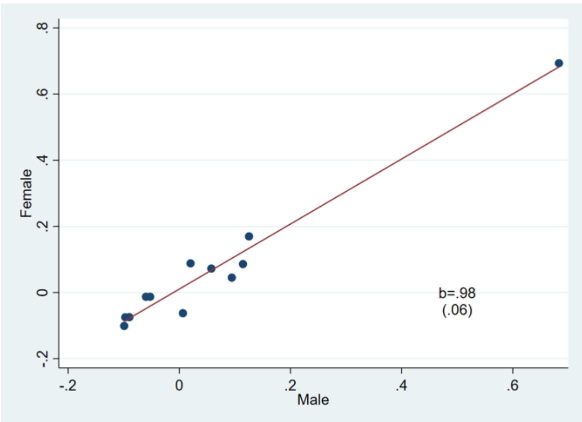
Appendix 9—Monotonicity Tests

Panel A- First Stage in Subsamples

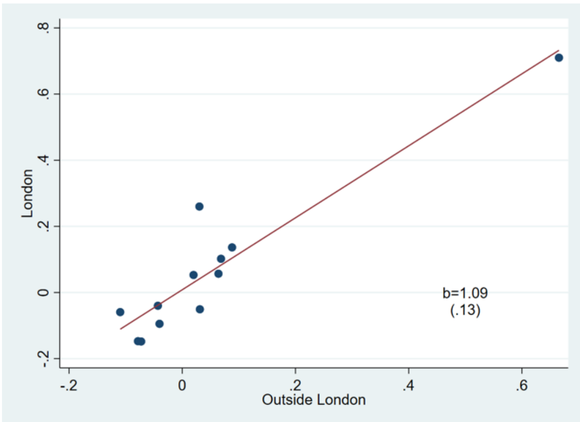
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	London	Outside London	Female Founder	Male Founder	Russell	Non-Russell	Pre-Seed/ Seed	Post-Seed
DAP	1.39***	0.70***	0.87***	1.04***	0.96***	1.02***	1.04***	0.96***
	(0.10)	(0.11)	(0.18)	(0.08)	(0.19)	(0.08)	(0.09)	(0.14)
Constant	-0.03	0.18***	0.12**	0.08***	0.12*	0.09***	0.06**	0.19***
	(0.02)	(0.03)	(0.04)	(0.02)	(0.05)	(0.02)	(0.02)	(0.04)
F Stat. of excluded instruments	205.54	40.37	23.97	164.25	25.87	159.43	140.18	49.86
N	861	1087	397	1551	327	1621	1509	439
R-sq	0.2301	0.0549	0.0949	0.1211	0.1184	0.1152	0.0972	0.0923

The table shows the correlation between

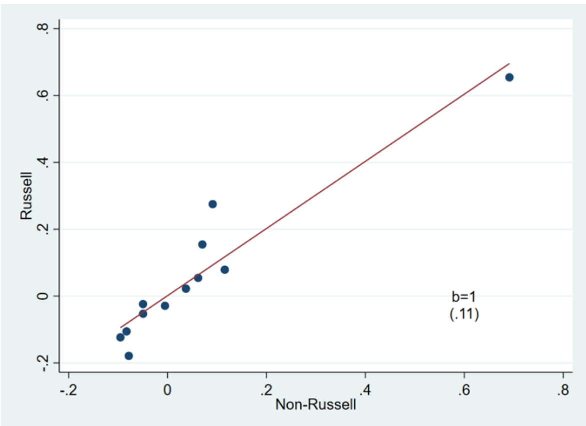
Panel B – Correlation Between Subgroup-Specific Reviewer-level Generosity Measures



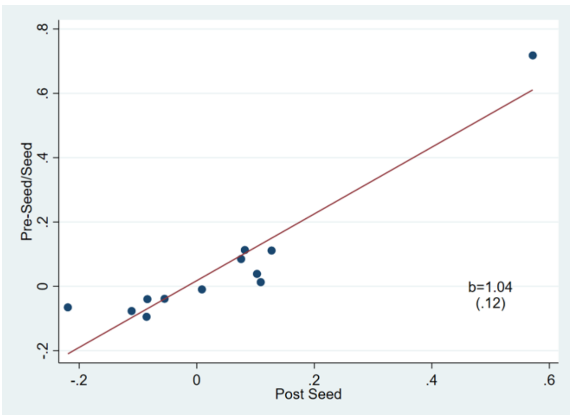
Female v.s. Male



London v.s. Non-London

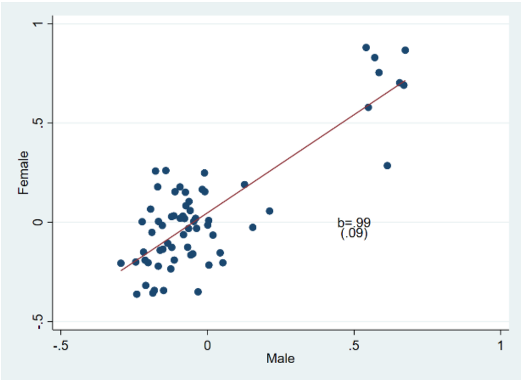


Russel v.s. Non-Russell

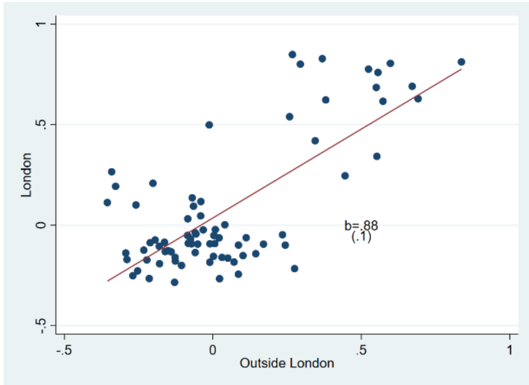


Pre-seed/Seed and Post-Seed

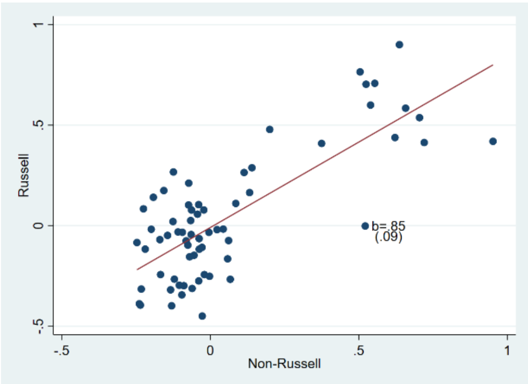
Panel C – Correlation Between Subgroup-Specific Trio-level Generosity Measures



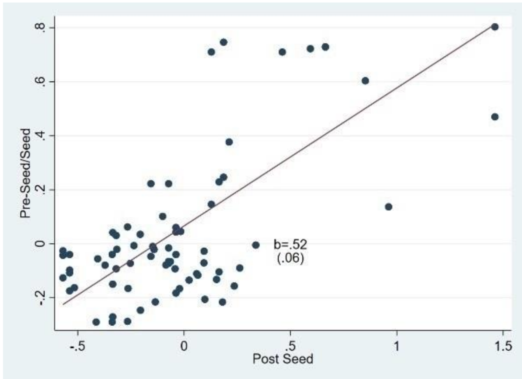
Female v.s. Male



London v.s. Non-London



Russel v.s. Non-Russell



Pre-seed/Seed and Post-Seed

The figure shows the correlations between trio level generosity for different groups of applicants. Trio level generosity is defined average rate of due diligence of for the assigned trio controlling for applicant fixed effects. We take the average generosity for each group over all available years of data. The solid line shows the best linear fit estimated using OLS relating each trio generosity measure. The four pairs of groups of applicants are: female v.s. male founder, London v.s. Outside London companies, founder with v.s. without Russell group education, early stage (pre-seed and seed) v.s. advanced stage (seed Extension).

Appendix 10—Content feedback and performance of rejected applicants

Panel A: Pre-application characteristics, applicant FE, no DD sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ln(Funding)	ln(#Rounds)	ln(#Investors)	ln(Equity Issuance) (UK)	ln(# Employees)	Growth in Assets (UK)	Growth in Debt (UK)	ln(# Directors) (UK)	Survival (UK)
Sentiment	0.23	0.25	0.15	2.03	0.07	0.28	0.47	0.09	-0.08
	(2.75)	(0.14)	(0.12)	(1.17)	(0.45)	(1.45)	(1.04)	(0.23)	(0.12)
N	1324	1324	1324	1016	1016	1016	1016	1016	1016
R-sq	0.1347	0.1608	0.0867	0.1262	0.2139	0.0909	0.0715	0.0684	0.0894

Panel B: Pre-application characteristics, other characteristics of comments, applicant FE, no DD sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ln(Funding)	ln(#Rounds)	ln(#Investors)	ln(Equity Issuance) (UK)	ln(# Employees)	Growth in Assets (UK)	Growth in Debt (UK)	ln(# Directors) (UK)	Survival (UK)
Sentiment	-0.12	0.23	0.12	2.02	0.10	0.22	0.49	0.09	-0.08
	(2.77)	(0.13)	(0.13)	(1.16)	(0.43)	(1.51)	(1.05)	(0.24)	(0.12)
N	1324	1324	1324	1016	1016	1016	1016	1016	1016
R-sq	0.1354	0.1624	0.0945	0.1268	0.2141	0.0914	0.0733	0.0687	0.0901

The table presents results from regressing outcomes against different proxies for the content of the feedback provided by reviewers. The sample corresponds to rejected applicants. We control for pre-application variables and applicant fixed-effects. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively

Appendix 11—Due diligence effects versus performance of portfolio companies

Table A11-Investment by the Fund and Venture Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	ln(Funding)	ln(# Rounds)	ln(# Investors)	ln(Equity Issuance) (UK)	ln(# Employees)	Growth in Assets (UK)	Growth in Debt (UK)	ln(# Directors) (UK)	Survival (UK)
Investment by the Fund	7.14***	0.36*	0.18**	2.11*	0.57	1.87*	0.74	0.02	0.06
	(1.45)	(0.15)	(0.06)	(1.02)	(0.32)	(0.74)	(0.71)	(0.14)	(0.11)
N	1953	1953	1953	1548	1953	1548	1548	1548	1548
R-sq	0.0822	0.0811	0.0259	0.0543	0.1074	0.0711	0.0410	0.0166	0.0265
Coefficients Comparisons:									
DD Effect (OLS)/Investment Effect (OLS)	**0.41**	**0.56**	**0.56**	**0.56**	0.89	**0.29**	0.76	11.00	1.17
DD Effect (IV)/Investment Effect (OLS)	**0.39**	**0.50**	**0.50**	**0.57**	0.81	**0.50**	1.57	15.00	-1.83

The table presents OLS estimates of the impacts of investment from the Fund on ventures' performance. In addition, in the bottom of the table, I provide comparisons between the investment effects and due diligence effect. The OLS and IV estimates of due diligence effects are the corresponding coefficients in Table 4 and 5.

Appendix 12—Robustness Checks Exclusion Restriction

Table A121-Funding

Panel A: Variation in DAP Due to Policy Change								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	4.16***	15.96***	0.26***	0.83***	0.14***	0.68***	1.88***	5.57**
	(0.66)	(4.13)	(0.04)	(0.24)	(0.03)	(0.19)	(0.35)	(1.82)
N	829	829	829	829	829	829	777	777
R-sq	0.2100	-0.2545	0.2244	-0.0975	0.1440	-0.4031	0.2187	-0.0906
F Stat.		15.06		15.06		15.06		12.10

Panel B: Use the Residual DAP as Instrument								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(# Rounds)		ln(# Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	2.94***	3.80**	0.20***	0.21**	0.10***	0.21***	1.18***	0.34
	(0.36)	(1.22)	(0.02)	(0.08)	(0.02)	(0.05)	(0.18)	(0.58)
N	1953	1953	1953	1953	1953	1953	1548	1548
R-sq	0.1313	0.1011	0.1457	0.1156	0.0704	0.0136	0.1053	0.0564
F Stat.		146.28		146.28		146.28		138.04

In Panel A, based on the main identification model, we add trio fixed effects, use location-based DAP estimated using reviewers' assessments over London-based companies only, and restrict the sample to London companies. In Panel B, by running the following regression: $DAP_i = \beta \sum_{h=1}^{3} Score_{i,h}/3 + \epsilon_i$, we obtain the residual DAP ($\widetilde{\epsilon_i}$) and then use residual DAP as the instrument instead of DAP. We include year FE throughout. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A122-Economic Growth

Panel A: Variation in DAP Due to Policy Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.75***	1.42**	1.19***	0.09	0.82***	0.19	0.37***	0.66*	0.17***	0.01
	(0.11)	(0.52)	(0.26)	(1.32)	(0.22)	(0.94)	(0.07)	(0.32)	(0.03)	(0.21)
N	829	829	777	777	777	777	777	777	777	777
R-sq	0.2797	0.1171	0.2058	0.1007	0.1817	0.0754	0.1589	0.0237	0.1395	0.0325
F Stat.		15.06		12.10		12.10		12.10		12.10

Panel B: Use the Residual DAP as Instrument

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(# Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(# Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.51***	0.31	0.54***	0.42	0.56***	0.91*	0.22***	0.21	0.07**	-0.16*
	(0.07)	(0.22)	(0.15)	(0.47)	(0.13)	(0.41)	(0.04)	(0.12)	(0.02)	(0.08)
N	1953	1953	1548	1548	1548	1548	1548	1548	1548	1548
R-sq	0.1629	0.1331	0.0846	0.0705	0.0656	0.0449	0.0555	0.0352	0.0495	-0.0329
F Stat.		146.28		138.04		138.04		138.04		138.04

In Panel A, based on the main identification model, we add trio fixed effects, use location-based DAP, and restrict the sample to London companies. In Panel B, by running the following regression: $DAP_i = \beta \sum_{h=1}^{3} Score_{i,h}/3 + \epsilon_i$, we obtain the residual DAP ($\widetilde{\epsilon_i}$) and then use residual DAP as the instrument instead of DAP. We include year FE throughout. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.