The Value-Add of Venture Capital Due Diligence for Venture Performance[1]

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Juanita González-Uribe	Robyn Klingler-Vidra	Su Wang	Xiang Yin
London School of Economics	King's College of London	University of Amsterdam	London School of Economics

We study the venture performance effects of Venture Capital (VC) due diligence—i.e., the process through which VCs scrutinize ventures 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 randomly allocated reviewers. We show that assignment to due diligence leads to substantial increases in venture capital fundraising and growth within two years of application, even for those firms that receive no eventual investment from the Fund. The due-diligence performance effects do not vary systematically across observable or unobservable applicant characteristics. By contrast, we find little evidence of venture performance effects from applicants' assignments to informal Fund meetings that are not part of the due diligence process. The results provide evidence that going through VCs' due diligence process adds value in the form of improved venture performance through three potential mechanisms: certification, coaching and self-validation. This new evidence implies that VCs' role in innovation affects many more firms, as it goes beyond their value-added effects on portfolio companies in which they invest. Therefore, frictions in the process through which startups seek and obtain VC due diligence can profoundly impact innovation and economic growth.

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[1] Corresponding authors: Juanita Gonzalez-Uribe (j.gonzalez-uribe@lse.ac.uk), Robyn Klingler-Vidra (robyn_klingler.vidra@kcl.ac.uk), Su Wang (s.wang2@uva.nl) and Xiang Yin (X.Yin5@lse.ac.uk).

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The role of Venture Capital (VC) in innovation has been the subject of much research. By now, we have a good understanding of how VCs add value to their portfolio firms via staged financing, contractual provisions, and active involvement post-investment (Lerner and Nanda, 2020). Yet, more informal evidence suggests that VCs have a broader role in innovation that extends beyond their value-add to portfolio firms. VCs meet with, request information from, and provide feedback to, 100 companies for every 1 in which they invest (Zider, 1998). Surveyed VCs rate the time spent on these deal selection activities as the most critical component to VC value-added, above post-investment activities (Gompers, Gornall, Kaplan, and Strebulaev, 2020). Consistent with this survey evidence, Sorensen (2007) estimates that post-investment activities contribute to only 40% of VC returns. The *due diligence* process—i.e., the process through which VCs conduct a formal, in-depth review of companies for potential investment – is particularly interesting given the extensive nature of the interactions and depth of information exchanged between VCs and startups. Likewise, entrepreneurs often mention the crucial learning from going through VCs' due diligence process, even (especially) from failed fundraising campaigns, as formative.

In this paper, we take an initial step in understanding more rigorously whether VC due diligence is a crucial driver of startup performance and thus an important and understudied role of VCs in innovation. We partner with a VC Seed fund in the UK (hereafter "the Fund"), which allows us to address several empirical challenges in exploring the effects of VC due diligence. Our novel data comprises nearly 2,000 startups applying for capital from the Fund, including those not selected for due diligence or investment, which is not observable in traditional VC data sources based on realized deals. 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 randomly allocated reviewers. To measure venture performance, we rely on administrative UK (abridged) balance sheet data that we combine with traditional web sources to track further venture capital fundraising from VCs other than the Fund.

Our main findings show that assignment to due diligence leads to substantial increases in venture capital fundraising within two years of application, even for those firms that receive no eventual investment from 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 increases VC fundraising (from VCs other than the Fund) by £160K. This estimate corresponds to a 20% increase relative to the 75th percentile of the post-application fundraising distribution, which has a long right tail and a mean of zero. By contrast, we find no evidence that ventures' assignment to informal meetings not part of the Fund's due diligence has meaningful effects on venture performance.

Our findings substantiate the hypothesis that going through VCs' due diligence adds value to ventures in the form of improved venture performance, implying that VCs' role in innovation is broader, extending beyond their post-investment activities and across a much larger number of firms than the

small set in which they invest. The results contribute to recent literature exploring how frictions in the process through which startups and VCs interact can profoundly impact innovation and economic growth (Howell and Nanda, 2021; Lerner and Nanda, 2020). Our analysis points to how high-potential entrepreneurs may still not reach their full potential if they remain at the fringes of VC close-knit networks (cf., Howell and Nanda, 2021; Lerner and Nanda, 2020).

Our results are consistent with the more informal evidence of the importance of due diligence for VCs and its potential impact on startup performance. VCs are known to devote substantial resources to conduct due diligence as they screen and scrutinize firms for potential investment. According to a recent survey, an average successful deal takes 83 days to close; and the average VC firm spends 118 hours on due diligence over that period (Gompers et al., 2020). The due-diligence process typically includes multiple stages and is often described as the deal "funnel," whereby VCs progressively increase the intensity with which they scrutinize a promising candidate as the venture progresses through the funnel towards investment. A rough outline of the due diligence process includes three main stages. The first stage is an initial consideration of a prospective deal by the individual who sources the deal, including meeting the founders and reviewing their business plan, at least once if the initial materials show potential. The second stage is further scrutiny by other members of the VC firm, a more formal pitch, and requests for additional information once the company passes the individual originator's filter. The third stage involves a formal investigation that includes a series of questionnaires and conversations that cover the product, market size, team, peer comparisons, and calling upon references and potential industry, technical and legal experts.

The due diligence process is crucial from the point of view of the VC: VCs only present term-sheets summarizing the conditions for investment to companies that pass this process. But, this process is also likely prized by entrepreneurs, even for unsuccessful fundraising campaigns. Going through VC due diligence processes can add value to entrepreneurs by the VCs providing them with feedback on their product and operational development, which increases performance (*coaching*).[2] VC due diligence selection can also provide an opportunity for ventures to signal their quality to the market (*certification*), as they are able to tell other VCs that they are undergoing due diligence. This signal leads to increased attention by other VCs and thus increasing fundraising probability and growth. Finally, the selection for due diligence and organisation of materials for review can also reveal the entrepreneurs' positioning and strengths (*self-discovery*), leading to increased entrepreneurial commitment (effort and capital) and increased venture performance through that channel.

[2] VCs ask for detailed financial, governance, legal and technical information from ventures that can be valuable for entrepreneurs to assemble and summarize, providing an opportunity for self-reflection and a better understanding of what VCs are looking for when making investments. In addition, entrepreneurs can also learn from the probing of VCs and the feedback provided in meetings. See Appendix 2 for the specific information required by the Fund. See also Section 1 for more details.

While the due diligence process is fairly well known, researchers have only informally hypothesized about its potential for value creation. The lack of data on firms applying for VC funding, and the endogeneity of VC due diligence application and decisions, have made it extremely difficult to investigate this hypothesis rigorously. Simply, there is no dataset that identifies all the startups that seek equity funding; instead, there is only data on startups that succeed in securing equity finance, from sources such as Crunchbase and Venture Source. In this paper, we overcome these crucial challenges by partnering with a Fund that shared comprehensive data of their applicants and due diligence process throughout the deployment of the fund's capital (a nearly three-year period).

Our empirical strategy uses data from all applicants to the Fund and designs a novel instrumental variables (IV) approach by taking advantage of two sources of variation in the Fund's due diligence selection process. First, the random allocation of applicants to trios of reviewers that independently evaluate applicants and provide (discrete) scores {1, 2, 3, 4; where 4 is best}. Second, the Fund's rule to aggregate reviewers' scores for selection (which varies over time and across applicants' locations).[3] For each applicant, we estimate the Due-diligence Assignment Probability (DAP) as the weighted sum of all three reviewers' potential score combinations. For each score combination, the weight corresponds to the selection rule valid at that place and time when the venture applies to the Fund, multiplied by the probability that the company receives such a score combination based on the scoring distribution (over other applicants) of the company's reviewers.[4]

Intuitively, the DAP combines the two sources of variation in the Fund's due diligence selection process into a single instrument that we can use for estimation. For example, the DAP correctly captures 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 also captures how this assignment also binds more when the selection rule that aggregates the three reviewers' scores overweights top scores—as under the "Champion model" commonly used by VC firms (see Malenko, Nanda, Rhodes-Kropf and Sundaresan, 2021).

Using the DAP, we estimate the local average treatment effect (LATE) of due diligence assignment. As in any IV estimation, our estimates are representative of the group of applicants whose treatment (due diligence assignment) responds to the instrument (DAP). We show that marginal applicants in our setting come from the middle of the venture quality distribution, as measured by reviewers' perceptions of venture characteristics at application, which is intuitive. The DAP will not affect the due diligence

[3] As we explain in more detal in Secton 1, the Fund uses different methods to aggregate reviewers' scores over time and across locations, for example, sometimes following "champion models" where applicants are selected for due diligence only if they receive a top score of "4" by at least one of the three reviewers.

[4] For illustration purposes, consider a strict selection rule where the only applicants selected for due diligence are those that secure a top score of "4" from each of the three reviewers. Suppose that the reviewers assigned to applicant X gave a score of "4" to 30%, 20%, and 50% of all other applicants they assessed (different from venture X). In this example, the DAP of applicant X would equal 3% (=30%*20%*50%).

assignment of the very top or very bottom applicants: these are clear cases that the Fund selects and rejects, respectively. Instead, the DAP is more binding for firms in the middle of the distribution. Quality assessment is murkier for those marginal firms, and thus, reviewers' generosity and selection rules' strictness have more bite in determining their due diligence assignment.

We explore several potential sources of treatment heterogeneity based on observable characteristics by cutting the data across applicant companies with headquarters in and outside London and across founders based upon personal characteristics, such as education, in terms of Rusell and Non-Rusell Group university degrees. We also explore the heterogeneity of impact along applicants' unobservable characteristics using several techniques, including Marginal Treatment Effects (Heckman and Vytlacil 2005). Overall, we find little evidence of impact heterogeneity across observable or unobservable characteristics: applicants of different quality whose treatment is affected by the instrument appear to benefit similarly from the VC Funds' due diligence. The support of firms impacted by the instrument helps explain the lack of treatment variation, but only partially. We cannot compare impacts across applicants at the (very) top and (very) bottom because the instrument (DAP) has no bite on their due diligence assignment. However, the effects are still indistinguishable across top and bottom middle-quality applicants, suggesting a more systematic reason behind the lack of heterogeneity, other than limited support.

Finally, we estimate the venture performance effects of assignment to informal meetings with the Fund's members that are not part of the firm's due diligence process. We use a similar identification strategy that exploits the Fund's rules to sort applicants that are not assigned to due diligence into two categories. First, those that they offer to meet informally ("informal meetings") and those that they do not offer to meet ("no meet") because they consider that are not venture backable. While ventures assigned to informal meetings outperform those assigned to the "no meet" category, the IV estimates suggest no apparent causal effect from the informal meetings on venture performance. These results lend credence to the value of interactions between startups and VCs when both parties are adequately incentivized because both parties have "skin in the game" (Taleb, 2018) in that the probability of moving to investment is not zero. This result is informative for policy design and suggests that "light touch" interventions that nudge VCs and founders simply to meet without any actual probability of potential investment may not be as effective in affecting venture performance.

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

First, our Fund's focus potentially trades-off external validity for internal validity. 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 nothing 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 specialized financing as the costs to start and develop businesses have fallen (Ewens, Nanda, and Rhodes-Kropf, 2018).[5] Like the Fund, these newer VCs specialize in pre-Series A businesses, do not shy away from sourcing deals online, and typically implement more scientific approaches to pre-screen applicants, often applying complex methods such as voting rules like the one used by the Fund, or even machine learning methodologies. While not necessarily representative of all VCs, our results represent this new type of VC that continues to become increasingly prevalent in entrepreneurial markets.

The second limitation of our setting is our inability to distinguish the relative importance of specific due diligence value-add mechanisms—i.e, certification, coaching, and self-discovery. While we have no exogenous variation in our data for clean identification, informal evidence points to coaching as a vital impact channel. Interviews with fund partners reveal that they perceive coaching, in which they provide substantive feedback on startups' go-to-market strategy, unit economics, and scaling. Fund partners' perception is that applicants often lack basic knowledge of how VC works, especially outside of London. They consider imparting this knowledge about raising VC, and the startups' market, product and operations, to applicants as part of their job. Informal evidence points to the limitations of certification because the Fund, as a rule, does not provide VC referrals to investment candidates that are not eventually selected. Additionally, we show that results are not driven by firms that enter into the third stage of the due diligence process where the Fund contacts third parties such as external experts and references, thus "spreading the word". Nevertheless, it is certainly possible that entrepreneurs can garner attention from other VCs by mentioning how they are under consideration by the Fund. Additional exercises point to a limited role for self-validation: we find no correlation between the tone of the feedback provided by reviewers (and shared with applicants) and applicants' subsequent performance for the subsample of rejected applicants (for which the channels of coaching and certification are not operational). However, none of these results is compelling, and it is clear more research is needed in future work to help disentangle between the relative strength of the due diligence value-add mechanisms.

Overall, our findings provide an initial step in understanding the role of VC due diligence. The results provide compelling evidence that VC due diligence can add value to entrepreneurs, and by extension, innovation ecosystems more broadly, by increasing venture performance. This evidence contributes to two main bodies of literature. The first explores the role of VCs in innovation. Most of this literature focuses on establishing the value-add of VC on their portfolio firms through several mechanisms,

[5] First, the Fund is a seed fund (pre-Series A) sourcing businesses online and attracting relatively inexperienced entrepreneurs. Second, the Fund is managed by a recently-established VC firm (in November 2016) with incentives to add value to applicants (even those they do not end up investing in) to increase their reputation and improve future deal sourcing. Third, the Fund has an above-average scientific approach to perform due-diligence, which can help systematize impact.

including active involvement and monitoring post-investment (Hellman and Puri, 2000; Bernstein, Giroud, and Townsend, 2017; Lerner and Nanda, 2020). While there is evidence in this literature that due diligence is essential, especially in recent survey evidence by Gompers et al. (2020), this evidence is primarily informal with a few exceptions, including Sorensen (2007), who estimates that deal sourcing and selection are more important than post-investment value-add in explaining returns to VC investors. We contribute to this literature by providing the first systematic evidence that VC due diligence, alone, can add value to entrepreneurs by improving venture performance. Our results imply that the role of VCs in innovation is broader than that emphasized in prior work and helps reconcile the disproportionate contribution of VC on innovation (Kortum and Lerner, 2000; Gonzalez-Uribe, 2020). Within that literature, our work also complements growing evidence of how networking frictions in the context of entrepreneurs seeking VC advice and financing can act as real impediments to growth and have profound implications on innovation (cf., Hochberg et al., 2007; Lerner and Nanda, 2020; Howell and Nanda, 2021). 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 focuses on how entrepreneurs learn throughout their life cycle, and the importance of certification, coaching, and self-discovery for early-stage founders. Several papers in this literature have looked at entrepreneurs' potential learning in other intermediaries in early-stage markets like business plan competitions (Howell, 2020; McKenzie, 2019) and accelerators (Gonzalez-Uribe and Leatherbee, 2016; Gonzalez-Uribe and Reyes, 2020). A common limitation has been distinguishing the impact mechanisms from accelerator participation (cf., Gonzalez-Uribe and Hmaddi, 2021). In the context of venture capital, research has acknowledged the growth of a so-called "spray and pray" strategy, in which early-stage venture capitalists make a large number of small investments, and thus offer minimal interaction, and so less learning opportunities, to early-stage ventures (Ewens et al., 2018). We share that same limitation but contribute to the literature by emphasizing the value-add from early-stage due diligence, rather than post-judgement or post-investment activities, of more specialized financial intermediaries.

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

The Fund is a seed fund in the UK managed by a VC firm established in November 2016, which began investing in portfolio companies in 2017. The Fund specializes in software but is business-model agnostic within that sector, covering direct-to-consumer businesses, platforms and deep tech. As is increasingly common among Seed funds, the Fund does online deal sourcing. The Fund relies 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 relying 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 wireless mobile phone charging.

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.[6] These types of funds have continued to become ever more prevalent in recent years. 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 more early-stage VC and super angels and business accelerators, seeking to sort through the increasing noise in ventures looking for eventual Series A, and coach the most promising candidates.

Also similar to other seed funds, the Fund uses a more systematic approach to screen applicants for due diligence than more traditional VCs. As we explain in more detail in Section 1.2 below, the selection process of the Funds involves two steps. The first is the allocation of the online applicantions to three random reviewers (internal to the firm) that score the submission and record feedback shared with founders. The second step 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: further due diligence by the Fund, informal meeting that is not part of the due diligence process, and those the Fund will not meet because they are deemed non-venture-backable. While this selection method is specific to the Fund, similar selection rules are commonly used by seed Funds. Within this move towards more systematic, and de-personalized decision-making by VCs, some funds even apply machine learning and big data techniques to the selection process.

[6] The average Seed stage investment in Europe was $1.9M in 2021, and the average Seed stage investment in the UK is £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.

Selection rules are not necessarily uncommon amongst more traditional VCs (see, for example, Malenko et al., 2021). To systematize judging of this increasing, and more variable, pool of applicants, venture capital firms are employing voting systems in order to both reduce the role of bias in scoring, and also, to help increase the chances of investing in superstar firms at the early stage (Malenko et al., 2021). However, the Fund is particularly systematic in its scientific approach to decision making that relies on the randomization of reviewers across applicants. As we explain in detail in Section 2, this randomization is helpful for us as researchers, and it forms the basis of our empirical strategy.

Finally, like traditional VC firms, the Fund engages in a more 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), following up, and meeting the founders. The second step includes further scrutiny by other members of the Fund if the Investment Lead continues to be enthusiastic after the meeting and more individual assessment. The third stage involves a more formal investigation (referred to as "Opportunity Assessment" by the Fund) that includes hiring industry experts for external reviews and calling on other parties, including references provided by the founders. Candidates that pass all three stages are presented with a term sheet summarizing the Fund's conditions for investment. Finally, if the company agrees to the term sheet, the deal closes.

Figure 1 shows the selection funnel of the Fund. By November 2019, roughly 30% of applicants had been assigned to due diligence, less than 3% had made it to the third stage of due diligence, and only 0.6% had secured funding from the Fund. This is consistent with findings elsewhere, that the way venture capital works is that VCs invest in only approximately 1% of the companies for whom they receive a business plan (Zider, 1998).

Applicants assigned to due diligence by the Fund can benefit and see increases in their performance through the three broad mechanisms of coaching, certification and self-discovery. The Fund is dedicated to its direct intention to provide incisive feedback and to coach entrepreneurs through the investment process. All applicants that begin the due diligence process are expected to complete very detailed spreadsheets with their cash-flow projections, 10-year view of their unit economics, and capitalization tables (see Appendix 2). The application form, which operates as a form of a “scorecard”, is designed to help the Fund ascertain the size of the market, the uniqueness of the product and technology, skills of the team, and their go-to-market strategy. According to the Fund partners, their application requirements are extensive, relative to other VCs or seed funds, and some entrepreneurs are discouraged by this requirement. Throughout the due diligence process the Investment Lead follows up with the founders regarding the spreadsheets, and often actively helps fill out the form, especially if there is sufficient enthusiasm for the venture. Self-discovery and certification effects are certainly also possible. We return to this point in Section 3, where we discuss the potential channels of impact.

1.1. Sample

The Fund provided us with all the application data, including application scores by each reviewer and final selection decisions. Our sample consists of 1,953 seeking capital from the Fund during March 2017 and June 2019[7] Figure 2 shows the number of applications made each month during that period. At the peak time, the monthly number of applications was 140.

Based on the applications, we constructed several variables to use as controls in our empirical strategy: firm's location, funding stage (pre-seed, seed, or post-seed), business type (direct sales, platform, and deep technology), age of firm as of application (relative to incorporation date), target amount to raise. 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, 13% include female founders, and aim to raise an average of £1.6M. Figure 3 shows the location, stage and business type breakdown: 47.86% are in London, 45.27% are in seed-stage, and in terms of business type, applicants are roughly half direct sales and half platform, with only a tiny minority of applicants in deep technology. The average number of founders per venture is 1.94.[8]

Although self-selection of companies into applying for funding online suggests a degree of sophistication possibly related to their probability of success and subsequent performance, other factors may play a role. First, 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 founders or referrals. Second, applicants with more advanced business models would not apply to the Fund for seed funding and would possibly go straight for Series A. For both reasons, the Fund's due diligence on the entrepreneurs in our sample is perhaps more likely to be associated with increases in venture performance than for the population of entrepreneurs seeking specialized financing as a whole.

1.2. Due diligence Selection Process

In this section, we review the Fund's process to sort applicants, which, as mentioned above, involves two steps that we now explain in detail: reviewers assignment and aggregation of scores using selection rules. We end by describing how the Fund communicates their decisions to applicants.

1.2.1. Reviewer assignment and scoring

[7] 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.

[8] This information is not provided by entrepreneurs in their applicatios 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.

The first step in the due diligence process is the assignment of three reviewers to each online applicant. This assignment is random conditional on location, as reviewers specialize along this dimension (e.g. the Fund had team members designated leads for the North of England and Scotland, and the Southeast of England and Wales, respectively. Reviewers are internal to the Fund and include senior partners, junior partners, associates, and analysts (we exclude scores provided by trainees and temps, which do not count for the Fund's selection). Each reviewer observes the information in the application, annotates comments, and independently provides a score to applicants of {1, 2, 3, 4; where 4 is best} in an internal platform. Reviewers individually score the application using Airtable, an online platform for managing spreadsheet-like inputs, without knowing which other reviewers are assigned, and without seeing their assessment.

We have 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). There are 132 "reviewer trios", with 44 (30) mean (median) reviews per trio and 3 (150) minimum and maximum reviews.

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 3. 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 and industry (the level of reviewer of assignment). 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 judges (as measured 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). In some parts of our analysis, we use the firm fixed effects estimated in these models as measures of reviewers' perceived quality (and fit with the Fund) based on applicants' characteristics at application.

There are two main features about the heterogeneity in reviewers' scoring generosity that we briefly summarize here, but we also describe in more detail in Appendix 3. First, the scoring generosity of reviewers appears unrelated to their skill in selecting applicants. We rank each reviewer's applications according to the reviewers' scores and separately according to subsequent fundraising 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 across reviewers (albeit with the caveat of 12 observations only).

The second feature about scoring generosity is that it is not related to the content of the comments. Appendix 4 shows that generosity is unrelated to content: more generous, relative to less generous,

reviewers provide equally toned comments, and are just as likely to discuss practical advice, which includes financing opportunities, employment plans, product improvements, or market strategy adjustments. We process the comments using machine learning methods. We analyse the comments data by applying Natural Language Programming techniques. In particular, we focus on the sentiment and the practical advice of the comments. We build a text classification model based on the pre-trained model, *Bidirectional Encoder Representations from Transformers* (BERT), which is trained on a large corpus of unlabelled text including the entire Wikipedia and Book Corpus.[9] Using manually read comments results, then we fine-tune the BERT model for sentiment and practical advice analysis.[10]

1.2.2. Aggregation of Scores: Selection Rules

The second step in the selection process is the aggregation of the three reviewers' scores by applying 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 have a stricter selection rule than applicants elsewhere. The Fund changed selection rules in response to internal discussions regarding its investment thesis. 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 is 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 remain underserved by specialized financiers, which echoes the well-known local preference of US VC investors (Lerner, 1995; Bernstein, Giroud and Townsend, 2016).

Figure 4 shows the selection rule for all the potential combinations of scores for the three distinct selection regimes: Pre May 2018, Post May 2018-London, and Post May 2018-Outside London. To illustrate the workings of selection rules, consider the example of London Post May 2018. The selection rule in that regime is the so-called "Champion Model" where the Fund only assigns applicants with a top score of "4" by at least one reviewer to due diligence. Any other combination of scores does not lead to due diligence assignment, even among score combinations with an equal average score than a score combination that includes 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

[9] 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).

[10] We randomly select 1000 comments and read them manually to classify them into positive or negative tone groups. Depending on whether the comments provide any practical advice on financing opportunities (e.g. participate in other programs, such as SEIS) or employment decisions (e.g. hire CTO or other key persons), and product improvements or market strategy, we also manually classify these 1000 comments into these two non-mutually exclusive categories manually.

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 size of market opportunity, the 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 (further due diligence, informal chat, and no meet) for specific scores vary 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 rule, 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 underlying heterogeneity in scoring across judges discussed in more detail in Appendix 3.

1.2.3. Communication of Sorting Results to Applicants

After aggregating the reviewer scores by applying the corresponding selection rule, the Fund communicates to applicants the result of their application. This communication occurs via email, with one of the reviewers acting as "Investment Lead". The Investment Lead is in charge of sending the email, following up, and meeting the founders. The Fund is strict with rule compliance: no informal chat converts into further due diligence. However, the Fund does accept reapplications. Although in practice, these are rare occurrences: 129 firms (6.6% of the sample) reapplied; we only keep the first application in our sample

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 of that meeting.[11] No email includes individual or average scores or the names of the reviewers. While the Investment Lead signs the email,

[11] The no meet 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?...".*

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.[12] Finally, all email templates include a copy of the reviewers' comments. 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' feedback "as is" in the body of the message.

1.3. Outcome Data

Table 1 presents summary statistics of the main outcome variables used in the regression analysis.

We use two complementary strategies to collect outcome data. First, we collect novel administrative data for businesses incorporated in UK (the majority, 80%) from the business registry in the UK (Compaies House; "CH") on registration, survival, bankruptcy, and annual equity fundraising, assets, and debt. The UK registry includes this information because UK firms submit mandatory annual accounts, albeit abridged relative to larger firms. While larger firms have to include information on more detailed balance sheet accounts, employment data, and income statements in their filings, smaller firms are exempt.

We construct the following outcome variables from CH filings: equity issuance and its log growth, number of directors appointed, log growth in assets, log growth in debt, and firm survival in the sample period before and after application separately. Equity issuance and number of directors appointed are added with 1 and logarithmized. Using these data, we track annual outcomes during the four 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 represents a significant advantage relative to most other work in the VC literature.

For log of equity issuance, the median value is 1.10 and 75 percentile value is 6.24. We focus on the the difference between the median and 75th percentile, and the 75th percentile, as a reference for the interpretation of the coefficients. In addition, the median values of growth in assets and debt are 0.61 and 0.75, respectively. The median and 75th percentile for log of number of directors appointed are 0.00

[12] 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 amember 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…."* .

and 1.10, respectively. The average survival rate is 0.81. In the regressions using equity issuance as outcome variables, we also include the log of equity issuance before the application as control.

Our second strategy to collect performance data follows the tradition 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 rounds with no institutional investor than data vendors' sources like VentureSource.[13] We construct the following outcome variables: Total funding, number of fundraising rounds, number of investors and number of employees after the application All outcome varibles are added with 1 and logaritmized. In the regressions using fundraising information as outcome variables, we also include equivalent variables constructed using pre-application information as controls. We also collect founders' education backgrounds and working experiences from their LinkedIn webpage whenever available, and supplement this information with co-founders working experiences from their Crunchbase webpages.[14] We determine whether founders have secondary higher education from an elite school. Since most of the firms in our sample are UK firms, we also classify universities into Russel Group (e.g. top 20 UK universities) or the "Golden Triangle" (Oxford, Cambridge, UCL, LSE and Imperial). We also code and group universities according to global rankings, including Times Higher and ARWN (Academic Ranking of World Universities).

Table 1 reports summary statistics of the outcome variables. For fundraising, the average funding from web-sourced information is £1330K and the median is 0. The fundraising variable is highly skewed. So in the regressions, for these performance variables, we add £1,000 (the minimum) and take log of them In addition, for better interpretation of the regression coefficients, we foucs on the gap beteween median and the 75th percentile and the 75th percentile. Similarly, the number of funding rounds, number of employees and equity issuance are also highly skewed variables. The average number of investors post application is 1.02 and the average number of directors appointed is 0.48. The average survival rate after application if 0.82.

The data of Companies House also allows us to get a sense of how comparable the firms in our sample are to the average firm securing seed financing in UK. For that purpose, we collect information for 257 ventures in the information and technology sector that raised seed funding in 2019 in UK. By matching the name, location and website of ventures, we collect 2018 total assets for 169 ventures from

[13] 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 fundraising 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 startups 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.

[14] 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' working experience from their Crunchbase personal webpages.

Companies House. The average venture size for firms securing seed financing is £492K, which is slightly smaller that the average is £641K raised by the ventures in our sample pre-application. St the median, however, our applicants look much smaller with £23K in assest, relative to a median asset size of £184K for firms that secured seed in funding in 2019 in UK..

One novel feature of our data collection strategies is that we have information on fundraising from administrative data and web sources. The administrative data includes equity sources other than specialized financing like VC, whereas the Crunchbase data mainly includes equity-based investments made by angels, venture capitalists and private equity. Thus, the two variables are not directly comparable. However, we can cross-check self-reported fundraising online with those in the registry to gauge the degree of potential selective online posting. We find little evidence of selective posting (correlation between two variables is 0.39), which mitigates concerns of data quality from the web variables and lends credence to the analysis relying on online data for the companies that are not incorporated in the UK.

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 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 firms eventually selected for investment by the Fund (12 firms; 0.61% of the applicants).

Our baseline specification measures the correlation between assignment to due diligence and 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 further due diligence and $\boldsymbol{Z}_i$ is a vector of controls including the outcome variable pre-application in all specifications (except for employment where we have no pre-application information). In some other specification of equation (1) we include other controls such as: the stage and business type, the age of firm, and the target amount to raise. We report heteroskedasticity-robust standard errors.

The coefficient ρ captures the effect of the Fund's due diligence assignment and subsequent venture performance. When $\rho > 0$ we conclude that 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 (form 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: the random assignment of applicants to three reviewers and 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 randomization of reviewer trios, this fact 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 selection rules that the Fund uses to aggregate votes, and which change over time and location (London or outside London), as explained in Section 1.

Our instrument combines both sources of variation to estimate the exact probabilities of due diligence for every applicant as the result of its reviewer's scoring generosity and Fund's rules to aggregate scores. Thus, our instrument takes into account 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 reviewieng 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 and the selection rule that aggregates the three reviewers' scores overweights top scores—as under the "Champion model" used by the Fund for London applicants after the change in selection regime in 2018 and that is also commonly used by VC firms (see Section 1.2.2 and Malenko et al, 2021).

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

$$DAP_i = \sum_{s_1}\sum_{s_2}\sum_{s_3} p_i^{s_1} p_i^{s_2} p_i^{s_3} f(s_1, s_2, s_3) \qquad (2)$$

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; and $p_i^{s_1}$, $p_i^{s_2}$, $p_i^{s_3}$ are the fractions of applications assigned a score of s ($s_h = \{1\ 2\ 3\ 4\}$) by each of the three reviewers calculated for all reviews excluding the assessment of

applicant *i*.In other words, the decision 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* (as in the jacknife IV of Angrist, Imbens and Krueger, 1999).

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 sample 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 instrument for due diligence assignment. When the endogenous regressor is a dummy, as due diligence in our case, this estimator is asymptotically efficient in the class of estimators where instruments are a function of DAP and other covariates. The linear model has the advantage of facilitating the interpretability of the estimates when we include further controls in our regression like sector fixed effects and location fixed effects to control for variation in reviewers' skill and specialization (as we discuss in more detail below).

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 are the same as in equation (1). In the robustness section, we report results using other controls like the firm fixed effects that we estimate in the reviewer and firm fixed effects models in Section 1.2. We report heteroskedasticity-robust standard errors of our estimates, and in the robustness sction report results using bootstrap.

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, (ii) that DAP only impacts venture outcomes through due diligence assignment probability, 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. We now consider whether each of these conditions hold in our data.

2.2.1. First Stage

To examine 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.

Unconditionally, the probability of due diligence assignment is twice as high for firms with above-median DAP. More strikingly, Figure 7 shows the same evidence conditioning on a proxy for candidate's quality. The figure shows that the probability of due diligence assignment is always higher for applicants with above-median DAP, than below-median DAP, across all levels of applicant quality. Figure 7 ranks companies in the x-axis according to the firm fixed effect we estimated in the fixed effects models explained in Section 1.2. Recall that the firm fixed effect proxies for the level of quality perceived and agreed by reviewers (once the scoring heterogeneity across reviewers is removed from their scores). Figure 7 also shows that median quality applicants are the most responsive to the instrument, as revealed by the gap between the average due diligence curves for above- and below-media DAP. This is intuitive: the DAP is less likely to affect the due diligence assignment of the very top and very bottom applicants: these are clear cases that the Fund selects and rejects, respectively. Instead, the DAP is more binding for firms in the middle of the distribution. Quality assessment is murkier for those firms, and thus, reviewers' generosity and selection rules' strictness have more bite in determining their due diligence assignment.

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) and combinations of controls as specified in the bottom rows of each panel. There are two main takeaways. 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 (column 12) implies that an increase in the probability of due diligence assignment of 0.29 as JCP goes from 0 to 1. We obtain similar results using a probit model (Panel B)--the implied marginal effect from the Probit regressions in column (12) is 0.25—which is unsurprising given the that the mean of due diligence assignment is 0.31 and far from zero and one .

2.2.2. Instrument validity

Two additional conditions must hold to interpret our two-stage least squares estimates as the local average treatment effect (LATE) of due diligence assignment: (i) DAP only impacts outcomes through the probability of due diligence assignment and (ii) the impact of DAP on the probability of due diligence assignment is monotonic across applicants.

Figure 8 and Appendix 5 verify that DAP is as good as randomly assigned, which is as expected given the random assignment of reviewers. Figure 8 shows a flat relation between DAP and firm quality, as measured by the fixed effects estimates in Section 1.2. Appendix 5 shows indistinguishable applicant characteristics across different quartiles in the DAP distribution. 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 3). In addition, the average venture assigned to due diligence across regimes has indistinguishable ex-post performance.

Despite satisfying the independence assumption, the instrument would violate the exclusion restriction if DAP impacts future outcomes through channels other than due diligence probability. For example, a higher DAP could be associated with more hands-on treatment if more generous reviewers also spend more time on due diligence, and this additional time has an independent effect on performance.[15] If DAP impacts future outcomes through any other channels, then the resulting LATE would incorporate any additional impacts associated with DAP. The assumption that DAP only systematically affects applicants' outcomes through due diligence selection is fundamentally untestable, and our estimates should be interpreted with this caveat in mind.

However, four pieces of evidence suggest the exclusion restriction is reasonable in our setting. First, DAP does not correlate with the content in reviewers' comments (e.g., tone, financial aspects, etc.; 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.[16] Second, DAP does not predict investment by the Fund or selection into Opportunity Assessment by the Fund—i.e., passing to the third stage of the due diligence process where the Fund—which is contrary to the assumption that higher DAP leads to better quality due diligence. Third, results are robust to controlling for Investment Lead fixed effects, which mitigates concerns that differences across due-diligence by Investment Leads drives the results.[17] Fourth DAP is not correlated with Opportunity Assessment performance, as would be expected if DAP also proxies for due diligence quality. Appendix 6 shows that firms with higher DAP do not score higher in the Fund's formal review after the Opportunity Assessment. This formal review 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?". Reviewer's answer each question by scoring on a scale of 1 to 10; 10 being best.

We also relax our exclusion restriction by estimating models that exploit selection regime changes in further robustness checks, as we explain in more detail in Section 4. Intuitively, these alternative models estimate due diligence effects by comparing firms at the margin of selection rules under different selection regimes, holding constant the generosity of reviewers. This analysis restricts the sample to

[15] 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).
[16] There is also no discernable change in the type of comments provided by reviewers across selection rule regimes.
[17] These results are available upon request in order to conserve space.

London applicants, for which the selection rule becomes more stringent after May 2018 when the Fund adopts the Champion model for London applicants. A vital identification assumption in these alternative models is that reviewers scoring generosity does not change across selection rules. Consistent with this assumption, Figure 5 shows that the distributions of score combinations in the three regimes are similar (see Section 1.2.2).

Still, to the extent that the exclusion restriction is violated, our reduced-form estimates can be interpreted as the causal impact of DAP. For example, under the concern that generous reviewers affect outcomes through better due diligence, 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). These reduced-form estimates are available in Appendix 7. 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.

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

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. This bias is an increasing function on the number of individuals for whom the monotonicity assumption does not hold and the difference in the marginal treatment effects for those individuals for whom the monotonicity assumption does and does not hold. This bias is also a decreasing function of the first-stage relationship described by equation (3) (Angrist, Imben, and Robuin, 1996).

The monotonicty assumption implies that the first-stage estimates should be non-negative for all sub-samples. Appendix 8 presents these first-stage results separately by applicant gender, development stage, industry, and location. The first-stage results are consistently same-signed and sizable across all subsamples. The appendix also further explores how DAP varies across observably different applicants. We plot DAP calculated separately by gender, stage of development, location etc. Each plot reports the coefficient and standard errors from an OLS regression relating each measure of DAP. Consistent with the monotonicity assumption, we find that the slopes relating the relationship between DAP in one group and DAP in another group are non-negative. Finally, the appendix also plots scoring generosity measures that are calculated separately for restricted subsamples. The plots show a strong correlation

between the actual fixed effects and the fixed effects from the restricted samples. 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., $p_i^{s_1}$, $p_i^{s_2}$, $p_i^{s_3}$ in equation (2)) to differ across observable applicant characteristics in the same spirit as Mueller-Smith (2015).

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 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 used by the patent office to aggregate the decisions across judges. 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 3.

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 fundraising 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 3 presents OLS and two-stage least squares estimates of the impact of the Fund's due diligence assignment on venture fundraising after application. Panel A excludes the 12 firms in the Fund's portfolio, and Panel B uses the entire sample. The names of the outcome variables are as specified on the top rows of each column. All regressions include the amount raised pre-application as a control. The results are robust to including more control variables, as discussed in more detail in Section 4. Robust standard errors are reported throughout.

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

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 reviewer assignment (Panel A). These two-stage least squares results confirm that applicants assigned to due diligence raise more equity financing than otherwise similar applicants given informal or no meetings with the Fund. The coefficient in column 2 implies that assignment to due diligence leads to an additional £155K in equity fundraising within 2 years of applying to the Fund. To produce this estimate, we compare the increase in Column 2 with the 75th percentile in post-application log fundraising distribution and multiply by the 75th percentile of the (levels) post-application fund raising distribution, given the right skewness of this variable (see Table 1). Column 2 in Panel A shows a sizable 299 percentage points increase in fundraising, which corresponds to a 23 percent increase from the 75th percentile of the log fundraising distribution.[18] A unique advantage of our setting is that we can contrast results using web-based proxies for fundraising (column 2) and administrative data (column 8). Lending credence to the fundraising effects of due diligence assignment, the implied economic magnitude of the coefficient in column 8 is £126K, which is remarkably similar to the £155K implied fundraising from column 2[19]. Finally, columns 3-4 and 5-6, respectively, show that the fundraising effects are explained by higher numbers of financing rounds and investors.

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 3. In Section 2.1, we explained how the endogeneity of Fund 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.

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 their high marginal costs 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

[18] £155K=23%×£698K, where £698K is the 75th percentile of the web-fundraising distribution (median is 0); see Table 1.
[19] £126K=1.76/6.24×£450K, where £450K is the 75th percentile of the administrative-fundraising distribution (median is 0); see Table 1.

strictness (generosity) 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 fundraising proxies is not statistically significant.

Table 4 replicates the OLS and two-stage least squares regressions of Table 3, using other growth variables. Across all variables and panels, the two-stage least squares estimates are positive and statistically significant. These results mitigate concerns that due-diligence teaches entrepreneurs how to game VC and raise funds, but have no effects on actual venture capital performance. The only exception in Table 4 is survival; meaning that we find significant impact on the intensive margin (fundraising and growth) but not on the extensive margin (survival).

Table 5 presents OLS and two-stage least squares subsample results by applicant location (London versus out of London; Panel A) and founder educational background (Panel B). 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 capital derived from 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, which can shape 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 these resources that increase the likelihood of their performance (Klofsten et al., 2019).

Firms in London generally perform better, and the OLS shows that London firms assigned to due diligence perform better than other due diligence assigned firms that are not in London. By contrast, the IV results show no evidence of different causal effects of due diligence assignment across London and Non-London firms.

Average performance of firms assigned to due diligence does not vary significantly with founders' educational background, as measured in the table by secondary higher education from a Russel Group university. Results are similar for other educational background proxies. Similarly, in unreported regressions, we also find no impact heterogeneity across different applicant characteristics like gender, education background, business development stage, or industry.

3.2. Potential Channels

Why are there such large benefits from the Fund's due diligence assignment? This section tentatively explores the potential mechanisms that might explain our venture fundraising and growth findings.

Due diligence is usually a highly intense process that VC investors use to scrutinize potential investments. Relative to more informal meetings with VCs, a due diligence process is characterized by

a higher volume of interactions, deeper and more meaningful discussions, and a higher commitment from entrepreneurs and VCs as the real possibility of investment exists.

These characteristics of the due diligence process lead us to postulate three 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 likely non-mutually exclusive in practice.

Going through VC due diligence processes can add value to entrepreneurs by providing them with new skills and resources, which increases performance (*coaching*). VC due diligence selection can also offer an opportunity for ventures to signal their type to the market (*certification*), leading to increased attention by other VCs and thus increasing fundraising probability and growth. Finally, the mere selection for due diligence can reveal the entrepreneurs' potential success, leading to increased entrepreneurial commitment (effort and capital) and increased venture performance through that channel (*self-discovery*).

These mechanisms are fundamentally untestable in our setting because we have no independent variation. This lack of variation explains why our discussion on mechanisms is only tentative. With this limitation in mind, this section builds a case "by exclusion," arguing that the preponderance of formal and informal evidence suggests that coaching is a first-order mechanism of the Fund's due diligence effects.

We start by focusing on the sub-sample of applicants not assigned to due diligence. This focus is helpful because potential certification and coaching effects are not operational for this subsample of firms, as they are not assigned due diligence. Still, we argue self-discovery effects are possible: assignment to informal meetings could provide valuable information to founders regarding the degree to which their idea is venture backable. Moreover, founders may also react to the feedback from reviewers as it communicates general attitudes by skilled investors about the business. Therefore, we posit that evidence of improved performance from informal meeting assignments (relative to no meets) would constitute evidence of potential self-discovery from due diligence. Similarly, evidence of a positive association between reviewers' comments' tone and subsequent performance of rejected firms (i.e., no due diligence assignment) would also suggest possible self-discovery from due diligence.

To test for these self-discovery effects, we start by estimating baseline models exploring the impact of 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 classifies rejects as "non-venture backable," such that meeting with the founders is not worth the time of the Fund's staff. 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 assignment 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

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

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

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

$$IMAP_i = 1 - p_{1_1} p_{1_2} p_{1_3} \qquad \text{(5b)}$$

where p_{1_h} denotes the probability that reviewer h gives a score of 1 (based on all other reviewed applicants except i). Table 6 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 meetings within two years of application. 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, and most flip sign. Though, we note that large standard errors mean that the difference between the two-stage least squares and the OLS estimates for all the fundraising proxies is not statistically significant.[20] We argue that informal meeting assignments' lack of self-discovery effects counters the first-order nature of the self-discovery due diligence channel while remaining cognizant that we cannot entirely rule out.

We present further evidence against the importance of this channel in our setting by exploiting the content of reviewers' feedback. Further consistent with the idea that self-discovery is not the primary

[20] Lack of differences in coefficients may be due to limited power as only 4% of the applicants have no meetings.

driver of results, we find no correlation between venture performance and the content of reviewers' comments as proxied by their tone and actionability; results are summarized in Appendix 9.

Regarding potential *certification*, we note that this channel is operational only indirectly in that the Fund, as a rule, makes no VC introductions for firms outside their portfolio. The Fund is concerned about signaling effects from recommending other VCs to invest in companies they are refusing investment. Indirect certification effects are nevertheless possible: firms can seek to increase their bargaining power with other VCs by arguing that the Fund is considering them for investment. We argue that certification effects are most likely for firms that reach the third stage of the due diligence process. In this stage, third parties are involved, such that the Fund calls upon industry experts, applicants' references, and often competitors to further scrutinize the firm. Against the idea that pure certification effects are the only driver of due diligence impacts, we show that results continue to hold after excluding the 45 firms that made it to the third stage of the Fund's due diligence process from the regression analysis (see Appendix 10).

According to extensive interviews with the Fund's staff, *coaching* is the mechanism of due diligence they deem as most relevant. As explained in Section 1, the Fund is unique in its strong emphasis on coaching. All applicants that begin the due diligence process are expected to complete very detailed spreadsheets with their cash-flow projections, unit economics, and capitalization tables (see Appendix 2). This requirement is not standard across VCs or seed funds.[21] In interviews, members of the Fund mentioned how some Investment Leads could go as far as to fill out the excel spreadsheets for the founders. Regardless, going through the exercise of thinking deeply about the unit economics of the business and how the VCs can make money can teach entrepreneurs a lot, both about what VCs are looking for when making investments and the underlying economics of their ideas.

3.3. External Validity

One potential caveat of our results is that we estimate the Fund's due diligence effects for the marginal due diligence assignee. Recall that our instrumental variable strategy identifies the Fund's due diligence impact on applicants whose DAP alters due diligence assignment. This local average treatment effect may or may not reflect the average treatment effect of the Fund's due diligence on 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

[21] 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…"

DAPs—that is, as we go from stricter reviewers 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 our local average treatment effects are likely to apply to applicants further from the margin.

To calculate the MTE function, we 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 fundraising, number of rounds, number of investors, and administrative fundraising. Panel A shows that the MTE function is flat, suggesting that the effects of the Fund's due diligence on specialized financing (from other investors different from the Fund) do not vary systematically across unobservable characteristics. The flat shape of the MTE curve suggests that our local average treatment effects 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. 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 spareness 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 quality. This limitation helps explain why our two-stage least squares exceed the OLS estimates, even though MTE reveals little treatment heterogeneity among applicants in the common support.

A second potential caveat is that the Fund is "special" from other VCs, limiting the extrapolations from the Fund's due diligence impacts to different contexts. As argued above, The Fund is unique because it has a scientific approach to sorting applicants and focuses strongly on coaching. However, this focus is not necessarily special when comparing the Fund to the newer intermediaries in entrepreneurial finance markets that continue to grow over time. These new intermediaries include other funds also focusing on pre-Series A financing (like seed and pre-seed funds), as well as super angels and accelerators. These new intermediaries seek to sort through the noise and train the most promising of the ever increasingly inexperienced new founders seeking specialized financing as the costs of starting and developing businesses have fallen over time. We thus argue that our results are most representative of these new types of VCs, especially those recently established and seeking to secure good quality deal flow in the future by building their reputation as value-added VCs.

3.4. Discussion and Contribution to Literature

Our findings show that VC due diligence can add value to entrepreneurs as measured by improved venture performance, even for those entrepreneurs that are not selected for investment. This new evidence implies that the role of venture capital role in innovation goes beyond their value-added effects on portfolio companies in which they invest.

Extant literature strives to understand the drivers of VC-backed firms' performance, often either 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).

Data availability partly explains this focus: realized deals comprise traditional VC data sources, and thus the larger pools of entrepreneurs *applying* for VC are not observable. Yet, through their due diligence process, VCs meet with, request information from, and provide feedback to 100 companies for every one they invest in (Zider, 1998). Therefore, due diligence warrants study, especially since VCs perceive it as the most critical value-add component (Gompers et al., 2020). Our study thus contributes to the literature by offering a novel assessment of the impact of the due diligence process on venture performance.

The evidence on VC post-investment value-add highlights the importance of VC for venture growth but remains silent on the process to secure VC. Our study also extends the literature by providing a window into VC decision-making. Our results imply that a helpful step in securing VC involves "growing through due diligence" understood broadly—either through coaching, self-discovery, or certification. In this way, our study supports the growing evidence on how frictions in the process through which entrepreneurs connect with VCs can have profound implications for innovation and growth. Our analysis points to how high-potential entrepreneurs may still not reach their full potential if they remain at the fringes of VC close-knit networks (cf., Howell and Nanda, 2021; Lerner and Nanda, 2020).

4. 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 11 (Panel A) 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 after 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 reviewers' trio (as estimated by the reviewer fixed effects in Appendix 3). Appendix 11 (Panel B) shows that the IV results using this alternative identification strategy are also robust. A vital identification assumption in these alternative models is that reviewers scoring generosity does not change across selection rules. Figure 5 and Appendix 3 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 additional control variables measured at application. Likewise, results are robust to including controls for firm quality as measured by the firms' fixed effects (from firm and reviewer fixed effects models described in Appendix 3). These results are similar to our preferred specification, indicating that potential bias from omitted variables is likely slight in our setting. The appendix also shows that results are robust to changes in functional form: using a probit in the first stage and using bootstrapped standard errors to account for generated regressors in the first stage. 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 $p_i^{s_1}$, $p_i^{s_2}$, $p_i^{s_3}$ 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 scrutinize ventures 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 randomly allocated reviewers. We show that assignment to due diligence leads to substantial increases in venture capital fundraising and growth within two years of application, even for those firms that receive no eventual investment from the Fund. The due-diligence performance effects do not vary systematically across observable or unobservable applicant characteristics. By contrast, we find little evidence of venture performance effects from applicants' assignments to informal Fund meetings that are not part of the due diligence process. 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 portfolio firms post-investment. The VC due diligence process is a systemic opportunity to add value to the larger number of ventures that enter the early-stage financing funnel. Therefore, frictions in the process through which startups seek and obtain VC due diligence can profoundly impact innovation and economic growth.

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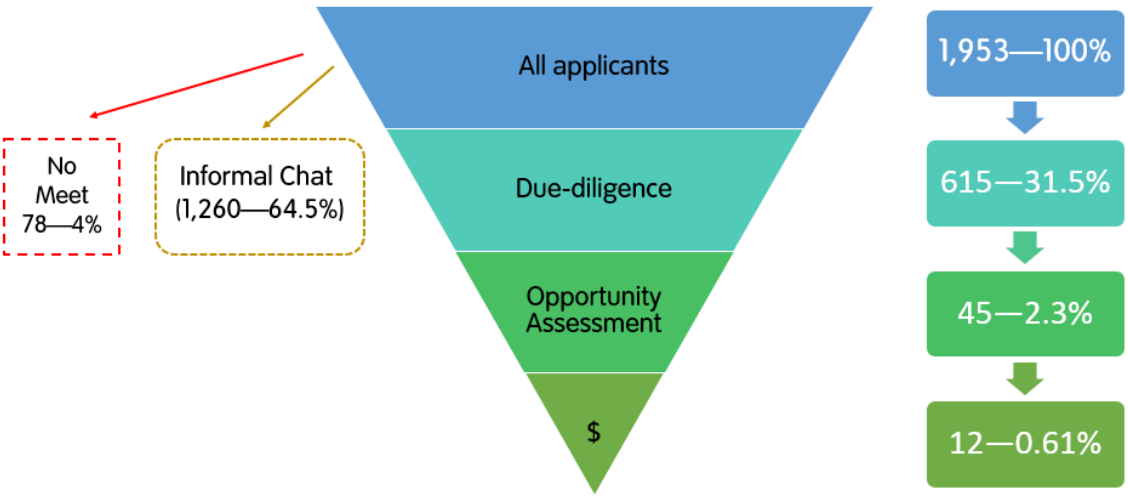
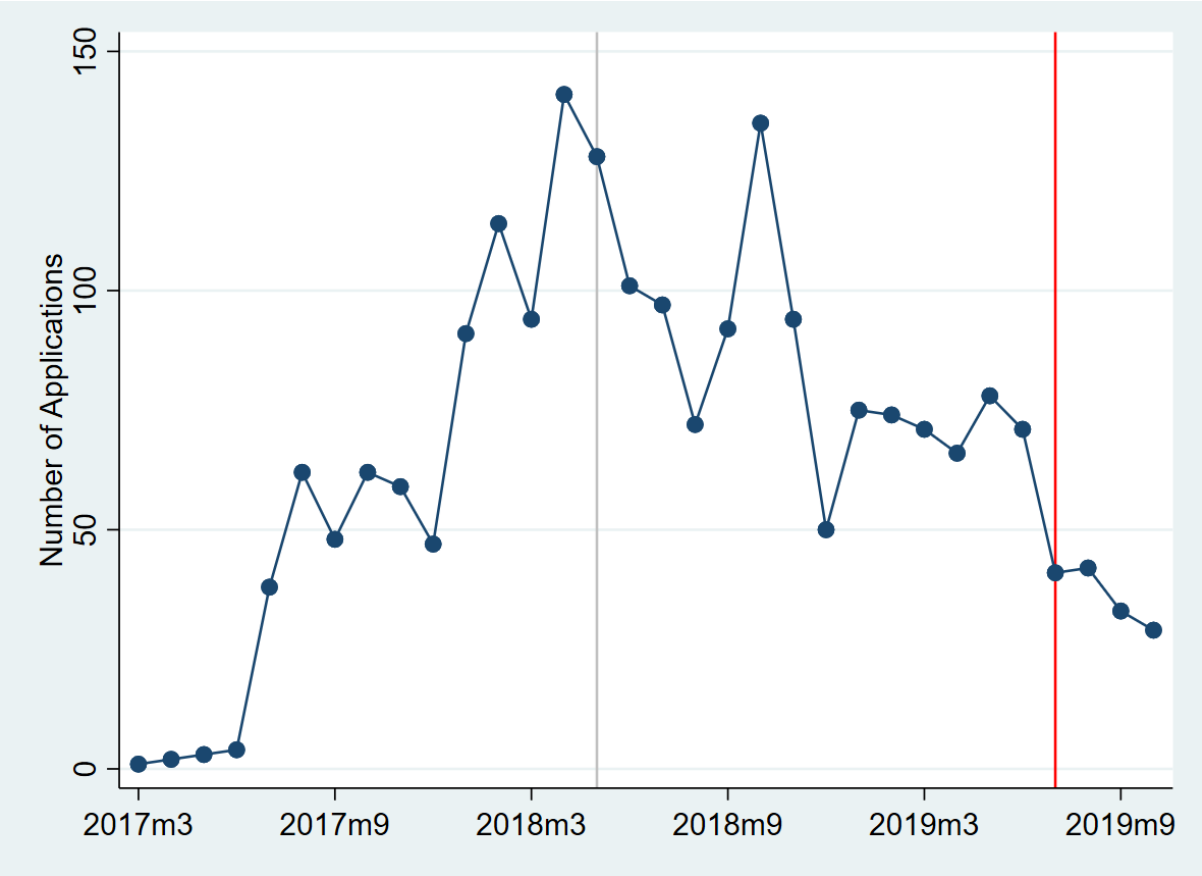
Figure 1. Selection Funnel



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 Ventures 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.

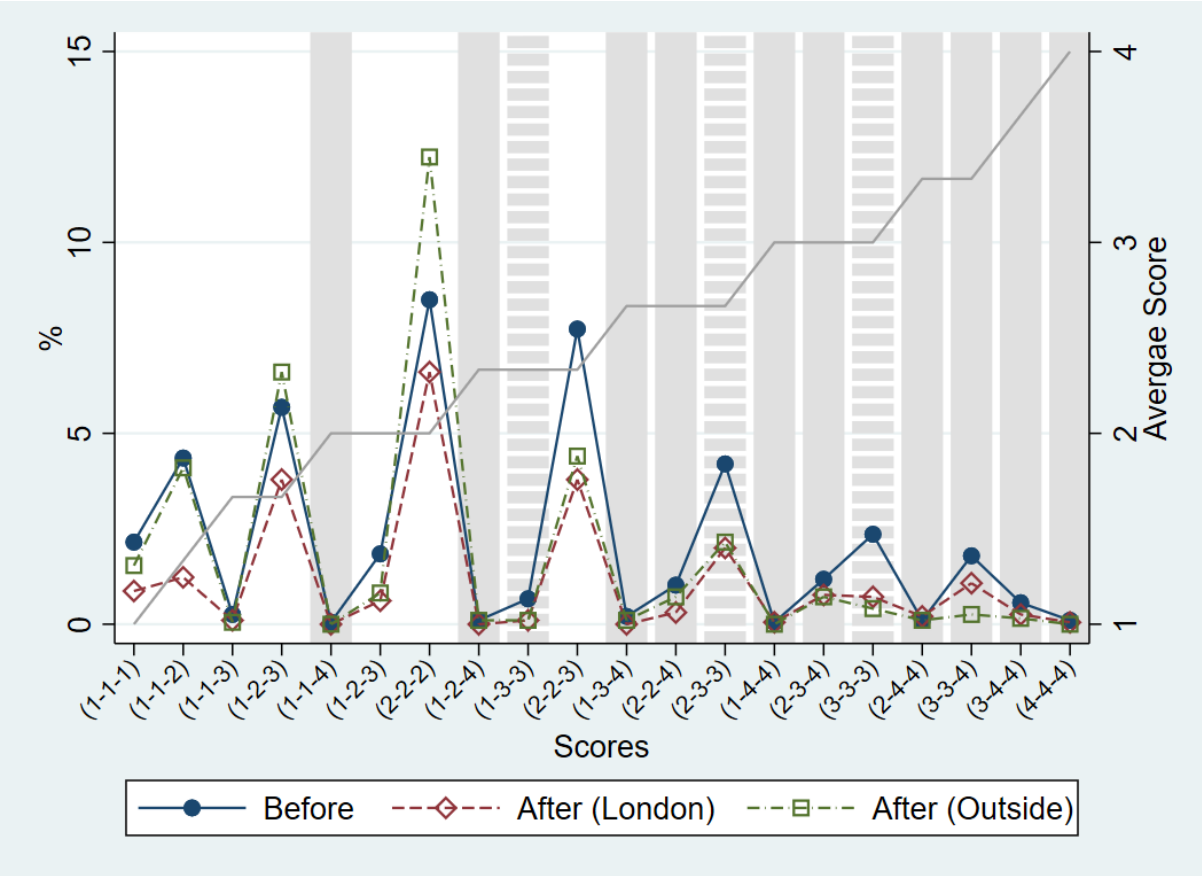
	Number of Firms	Percent
	By Location	
London	862	44.14%
Outside UK	412	21.10%
Other Regions of UK	679	34.77%
	By Stage	
Pre-Seed (under £100k)	250	12.80%
Seed (£100k-1m)	865	44.29%
Seed Extension (£200k-2m)	838	42.91%
	By Business Type	
Deep Tech	83	4.26%
Direct Sales Led	836	42.92%
Platform	1,029	52.82%

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 Meet	No Meet	No Meet
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.

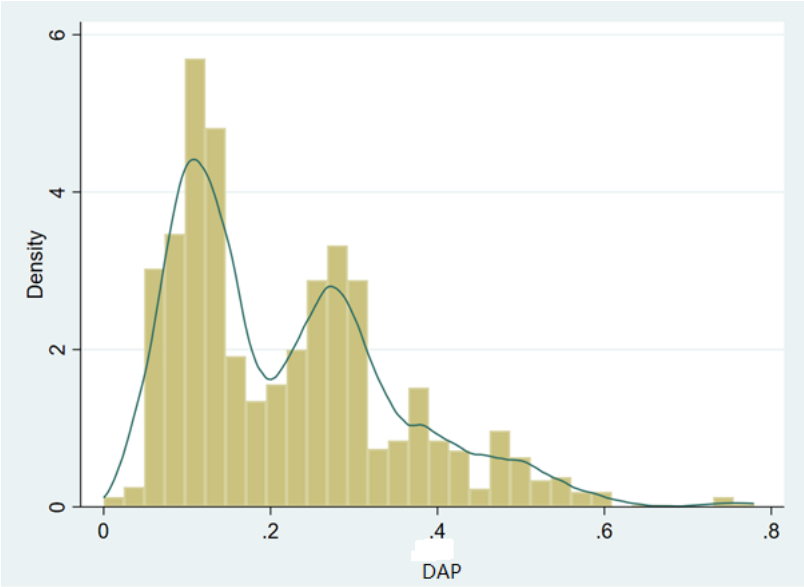
Figure 5. Distribution of Scores over Time and Location



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.

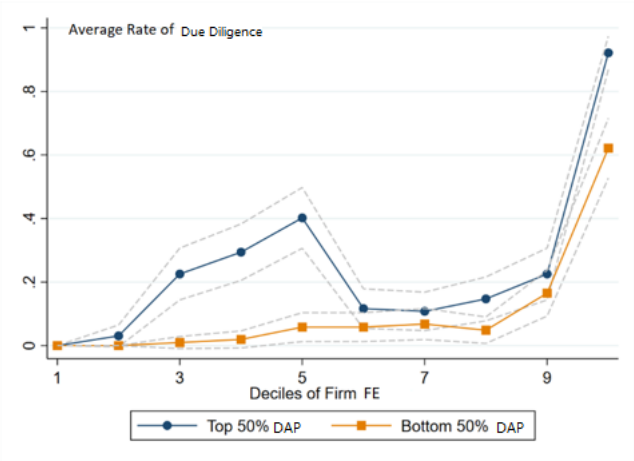
Trio Scores	Two-Sample Kolmogorov-Smirnov Test	
	Stat.	P Value
London (Before) v.s. Outside (Before)	0.132	0.001
London (After) v.s. Outside (After)	0.149	0.000
London (Before) v.s. London (After)	0.103	0.021
Outside (Before) v.s. Outside (After)	0.120	0.001
Individual Score	**Two-Sample Kolmogorov-Smirnov Test**	
	Stat.	P Value
London (Before) v.s. Outside (Before)	0.089	0.000
London (After) v.s. Outside (After)	0.113	0.000
London (Before) v.s. London (After)	0.084	0.000
Outside (Before) v.s. Outside (After)	0.109	0.000

Figure 6. Due Diligence Assignment Probability Distribution



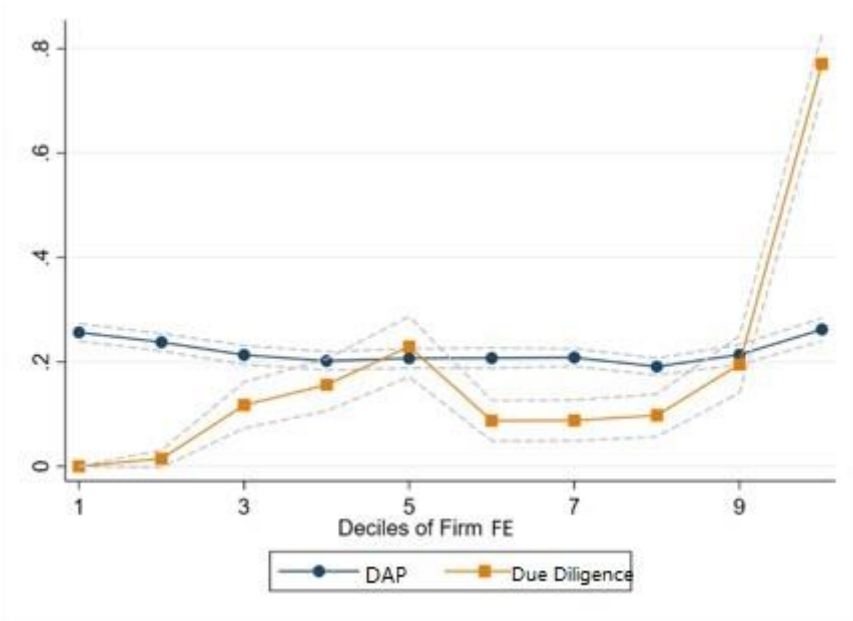
This figure plots the distribution of the Due Diligence Assignment Probability (DAP) across the sample applicants. For more details see Section 2.2.

Figure 7. DAP and Due Diligence Assignment



The figure plots the average rate of due diligence assignment against deciles of firm 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.

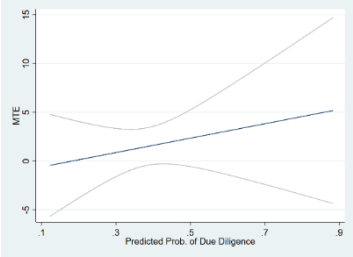
Figure 8. DAP and Random Assignment



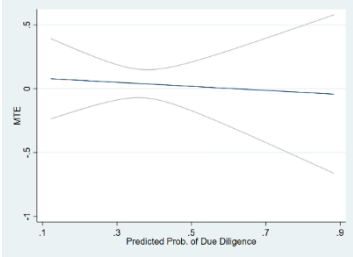
This figure plots the average due diligence assignment and DAP against deciles of applicant fixed effects. The applicant fixed effects are estimated in models regressing reviewer scores against full set of applicant and reviewer fixed effects; for more details see Section 1.2 and Appendix 3

Figure 9. Marginal Treatment Effects

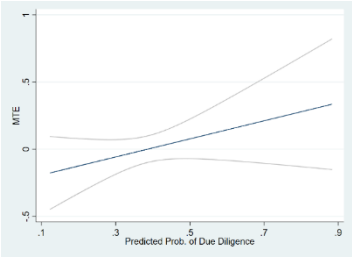
Panel A -Treatment Curves over Common Support



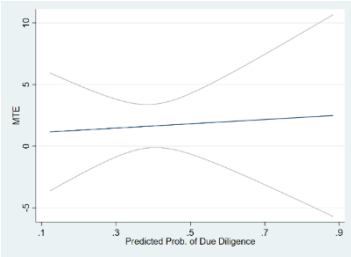
Ln(Funding Amount)



Ln(Number of Rounds)

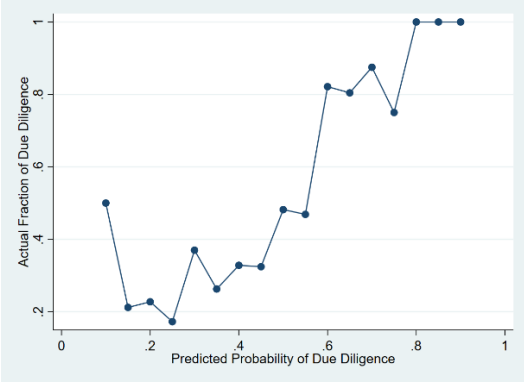


Ln(Number of Investors)



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 files	Age Business (since incorporation)	2.61	2.96	0.00	1.00	2.00	4.00	7.00	1,953
	Female founder	0.13	0.33	0.00	0.00	0.00	0.00	1.00	1,785
	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	1725	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
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
Crunchbase									
	Pre- Application								
	Funding rounds	0.47	1.06	0.00	0.00	0.00	0.00	3.00	1,953
	Total funding ($1000s)	306	1,105	0.00	0.00	0.00	0.00	2,000	1,953
	Number of Investors	0.83	2.48	0.00	0.00	0.00	0.00	5.00	1,953
	No. of Years Before App.	2.61	2.96	0.00	1.00	2.00	4.00	7.00	1,953
	Post-Application								
Crunchbase	Founding Rounds	1.28	1.90	0.00	0.00	0.00	2.00	5.00	1,953
	Total funding ($1000s)	1,330	3,362	0.00	0.00	0.00	698	8,634	1,953
	Number of Investors	1.02	1.19	0.00	0.00	1.00	2.00	3.00	1,953
Linkedin	Number of Employees	6.09	11.38	1.00	1.00	2.00	7.00	27.00	1,953
Companies House									
	Pre- Application								
	Assets (£1000s)	641	15,635	0.00	0.00	23.13	167	1,044	1,548
	Equity Issuance (£1000s)	158	608	0.00	0.00	0.00	83	850	1,548
	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
	Equity Issuance (£1000s)	385	933	0.00	0.00	0.00	255	2,387	1,548
	No. of Years After App.	1.93	0.64	1.00	2.00	2.00	2.00	3.00	1,548

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 firms 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)	(9)	(10)	(11)	(12)
						Panel A-- OLS						
DAP	1.09***	0.45***					0.97***	0.38***	0.98***	0.36***	0.88***	0.29***
	(0.08)	(0.08)					(0.12)	(0.20)	(0.18)	(0.18)	(0.18)	(0.18)
Reviewer FE			2.47***	0.84***			0.70	0.34			0.62	0.37
			(0.35)	(0.36)			(0.34)	(0.45)			(0.56)	(0.43)
Applicant FE					0.39***	0.37***			0.37***	0.37***	0.37***	0.37***
					(0.11)	(0.13)			(0.07)	(0.04)	(0.04)	(0.04)
F-test of excl. IV	185.64	31.64					116.16	17.83	196.00	26.45	121.00	13.14
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953
						Panel B-- Probit						
DAP	3.09***	1.94***					2.74***	1.61***	3.28***	2.02***	2.86***	1.71**
	(0.23)	(0.37)					(0.70)	(0.71)	(0.73)	(0.72)	(0.74)	(0.77)
Reviewer FE			7.05***	3.84***			2.18	1.71			2.59*	1.64
			(1.25)	(1.37)			(1.25)	(1.13)			(1.26)	(2.08)
Applicant FE					1.21***	2.52***			1.25***	2.53***	1.26***	2.53***
					(0.30)	(0.31)			(0.13)	(0.20)	(0.15)	(0.23)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953

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 firm reviewer fixed effects; see Appendix 3. Controls include the following variables: the firm's stage, business type, log of age and log of target amount to raise. 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—Due Diligence Assignment and Fundraising

Panel A—Excluding Portfolio companies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.32***	2.99***	0.15***	0.31***	0.07***	0.12**	0.47**	1.76***
	(0.26)	(0.85)	(0.02)	(0.07)	(0.01)	(0.05)	(0.15)	(0.49)
N	1,941	1,941	1,941	1,941	1,941	1,941	1,537	1,537
R-sq	42.76%	41.38%	3.25%	0.10%	1.38%	0.68%	24.81%	20.86%
F Stat.		120.50		120.50		120.50		99.56
Reference:								
P50	0.69		0.69		0.69		1.10	
P75	13.46		1.10		1.10		6.24	
P75-P50	12.76		0.41		0.41		5.14	

Panel B—Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.36***	3.11***	0.16***	0.32***	0.07***	0.12**	0.52***	1.87***
	(0.26)	(0.85)	(0.02)	(0.07)	(0.01)	(0.05)	(0.15)	(0.49)
N	1,953	1,953	1,953	1,953	1,953	1,953	1,548	1,548
R-sq	42.61%	41.09%	3.48%	0.26%	1.48%	0.80%	24.71%	20.42%
F Stat.		125.38		125.38		125.38		107.10

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). All columns include the fundraising variables in the pre-application period as controls in the estimation, and so does the respective first stage. 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 4 –Due Diligence and Economic Growth

Panel A—Excluding Portfolio companies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ln (Number of Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(Num. of Appointed Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.38***	0.82***	0.29*	0.92*	0.32**	1.30***	0.14***	0.42***	0.08***	-0.12
	(0.06)	(0.21)	(0.14)	(0.44)	(0.12)	(0.37)	(0.03)	(0.11)	(0.02)	(0.07)
N	1,941	1,941	1,537	1,537	1537	1,537	1,537	1,537	1,537	1537
R-sq	2.40%	-0.75%	0.31%	-1.17%	0.51%	-4.27%	1.18%	-3.21%	0.86%	-4.68%
F Stat.	121.10		107.10		107.10		107.10		107.10	
Reference:										
P50 (Mean)	1.10		0.61		0.75		0.00		(0.81)	
P75	2.08		2.08		1.95		1.10			
P75-P50 (SD)	0.98		1.47		1.20		1.10		(0.40)	

Panel B—Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Ln (Number of Employees)		Growth in Assets (UK)		Growth in Debt (UK)		ln(Num. of Appointed Directors) (UK)		Survival (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	0.64***	1.13***	0.73***	1.32*	0.62***	1.83***	0.25***	0.57***	0.08***	-0.15
	(0.08)	(0.29)	(0.18)	(0.58)	(0.15)	(0.50)	(0.04)	(0.15)	(0.02)	(0.09)
N	1,953	1,953	1,548	1,548	1,548	1,548	1,548	1,548	1,548	1,548
R-sq	4.84%	2.13%	1.39%	0.48%	1.34%	-3.84%	2.56%	-1.86%	0.67%	-4.70%
F Stat.	121.88		107.88		107.88		107.88		107.88	

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). 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 Fundraising: sample cuts

Panel A—Location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.20***	5.83***	0.11***	0.43***	0.05**	0.09	-0.08	2.97**
	(0.33)	(1.70)	(0.02)	(0.12)	(0.02)	(0.08)	(0.20)	(1.14)
Due diligence*London	0.49	-4.66*	0.14**	-0.23	0.06*	0.02	1.36***	-1.65
	(0.52)	(1.89)	(0.04)	(0.14)	(0.03)	(0.09)	(0.30)	(1.24)
London	0.54*	2.25***	0.08***	0.20***	0.06***	0.07*	0.05	1.24*
	(0.26)	(0.65)	(0.02)	(0.05)	(0.01)	(0.03)	(0.16)	(0.49)
N	1,941	1,941	1,941	1,941	1,941	1,941	1,537	1,537
R-sq	44.51%	32.72%	18.67%	6.24%	6.65%	3.86%	28.28%	10.41%
F Stat.		51.84		51.84		51.84		48.64

Panel B—Founders' Educational Background

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.45***	3.13***	0.16***	0.33***	0.08***	0.14**	0.51**	1.60**
	(0.28)	(0.91)	(0.02)	(0.08)	(0.02)	(0.05)	(0.16)	(0.52)
Due diligence*Russell	-0.63	-0.59	-0.00	-0.13	-0.03	-0.09	0.01	1.05
	(0.71)	(2.38)	(0.06)	(0.18)	(0.04)	(0.13)	(0.41)	(1.32)
Russell	1.00*	0.92	0.05	0.08	0.02	0.03	0.69**	0.30
	(0.40)	(0.87)	(0.03)	(0.06)	(0.02)	(0.05)	(0.23)	(0.51)
N	1,941	1,941	1,941	1,941	1,941	1,941	1,537	1,537
R-sq	44.51%	32.72%	18.67%	6.24%	6.65%	3.86%	28.28%	10.41%
F Stat.		59.70		59.70		59.70		59.70

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 the fundraising outcome variables in the pre-application period as controls in the estimation, and so does the respective first stage. 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. Informal Meetings and Fundraising

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		Δ(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Informal Meeting	3.33***	-7.31	0.17***	-0.44	0.12***	-0.10	0.28	4.13
	(0.40)	(5.54)	(0.02)	(0.35)	(0.02)	(0.21)	(0.23)	(2.98)
N	1,611	1,608	1,611	1,608	1,611	1,608	1,244	1,242
R-sq	1.48%	-13.67%	1.18%	-13.52%	1.10%	-2.45%	0.05%	-9.0%
F Stat.	18.33		18.33		18.33		18.33	
Reference:								
P50	0.69		0.69		0.69		1.10	
P75	13.46		1.10		1.10		6.24	
P75-P50	12.76		0.41		0.41		5.14	

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). 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 meet 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—Reviewer Heterogeneity

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:

$$Score_{i,h} = \mu_h + \alpha_i + X_{i,h} + \epsilon_{i,h} \quad (A31)$$

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 quality 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.

We have a total of 12 reviewers in our sample and 1,953 applicants. Each reviewer fixed effect is estimated using an average of 488 observations on average, and a minimum or 30. There are 132 reviewer trios, and the average number of applications reviewed per trio is 44 with a minimum of 3.[2] Figure A31 below shows the distribution of applications, over the 12 reviewers (Panel A) and over the 132 trios (Panel B).

Figure A32 plots the distribution of fixed effects across reviewers. Figure A33 plots the distribution of applicant fixed effects.

There are four main findings from estimating equation (A31):

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

[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.
[2] In robustness checks we drop applications reviewed by trios that reviewed fewer than 25 applications as robustness checks.

significant (as reviewers are randomly assigned by design). 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).[3] 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.[4] 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 A34 (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 A34 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 firm fixed effects deciles. On average, this probability is 31.1% for applicants with generous reviewers and 17.9% for applicants with stricter reviewers. Relying on a panel of reviewers rather than on individual reviewers, and using aggregating systems that are not reliant on consensus, helps mitigate the effect of reviewer heterogeneity by helping offset scores from strict and lenient reviewers. However, it does not fully correct it, because reviewers panels are small, with only three individual reviewers assigned per applicant.[5]

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 A35 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 fundraising post application ("actual rank") and

[3] 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.

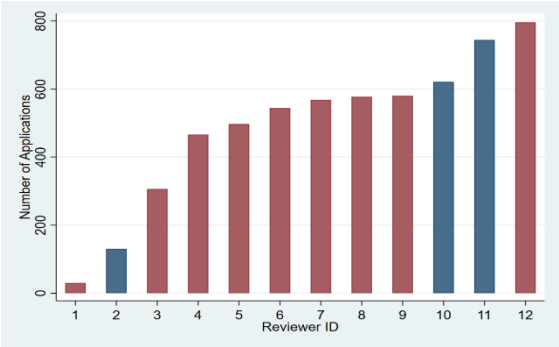
[4] We make sure the reviewer was assigned at least one application to review within 3 months of the firm's application date.

[5] The small size also explains why random assignment does not deal with this issue: for a given project the scores of overly generous reviewers may not tend to cancel out those of overly strict reviewers, as the probability that both types of reviewers will be randomly assigned to the same project does not tend to 1.

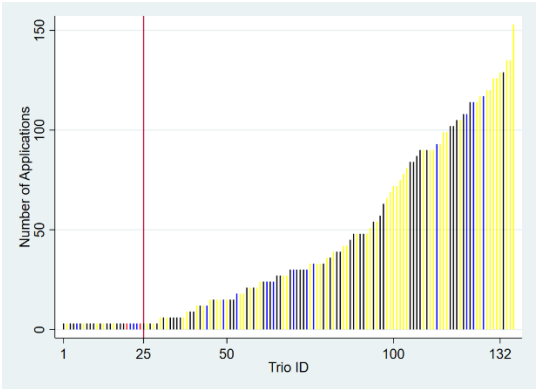
(ii) the reviewer's score ("reviewer's rank). Figure A35 is a scatterplot of each reviewer's generosity and ranking ability for the 12 reviewers in our sample.

Figure A31—Distribution of Applications across Reviewers and Trios

Panel A—Distribution over Reviewers

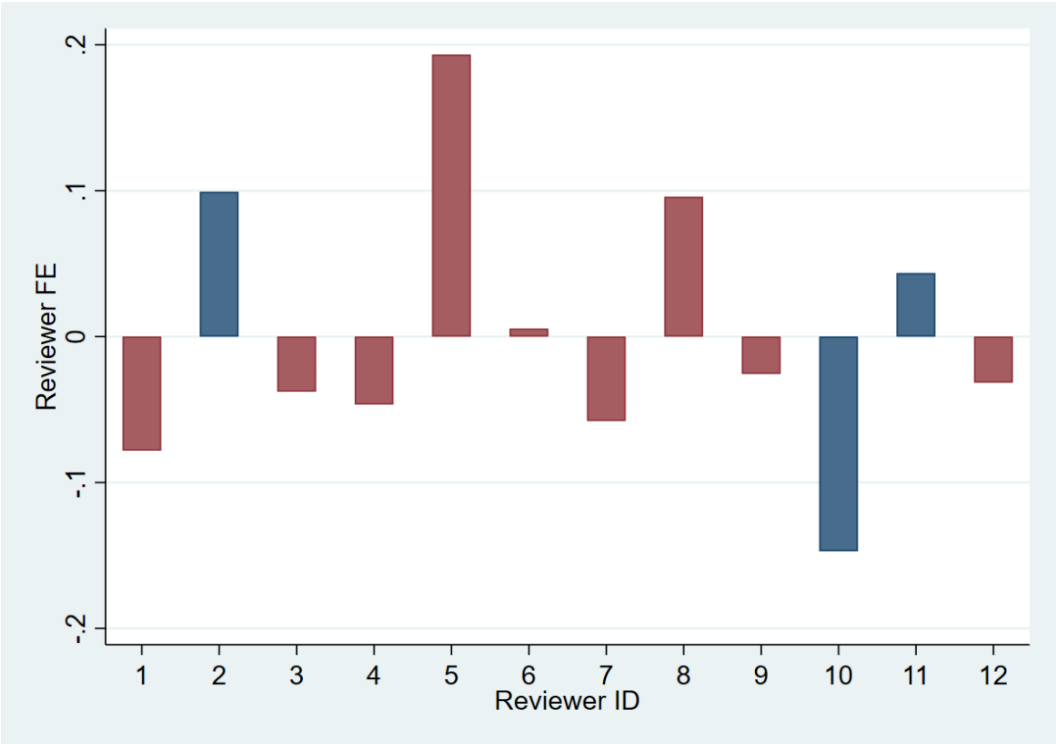


Panel B—Distribution over Trios



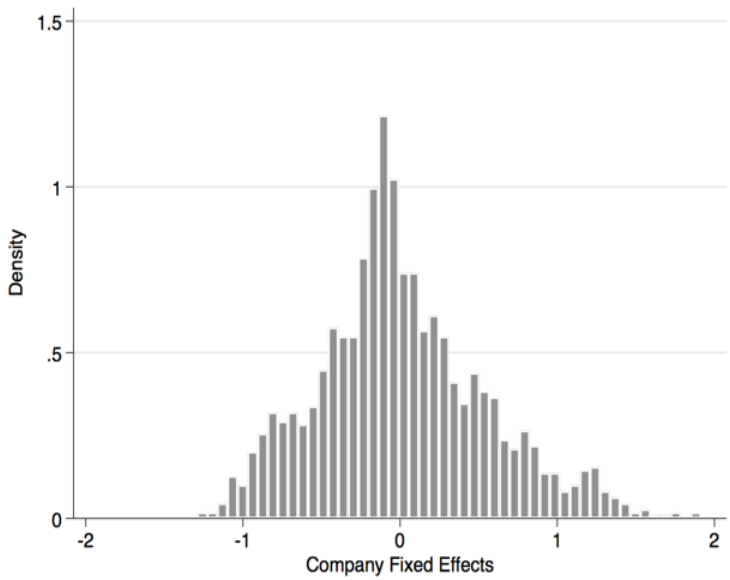
The figure plots the number of applications evaluated by each reviewer (Panel A) and by each trio of reviewers (Panel B).

Figure A32—Distribution of Reviewer Fixed Effects



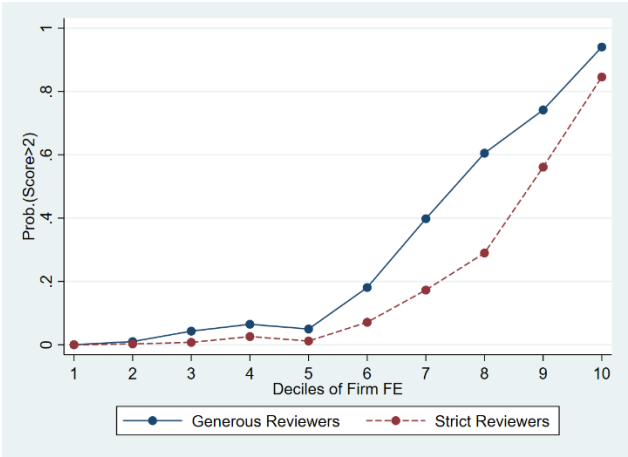
The figure plots the reviewer fixed effects for each reviewer in the sample based on the estimates of equation A31. Blue columns indicate female reviewers.

Figure A33—Distribution of Applicant Fixed Effects



The figure plots the applicant fixed effects for each applicant in the sample based on the estimates of equation A31.

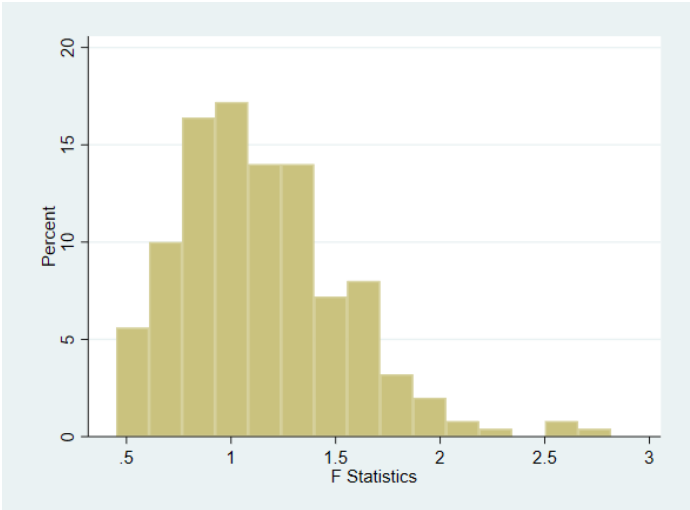
Figure A34—Frequency of Scores Above 2 and Reviewer FE



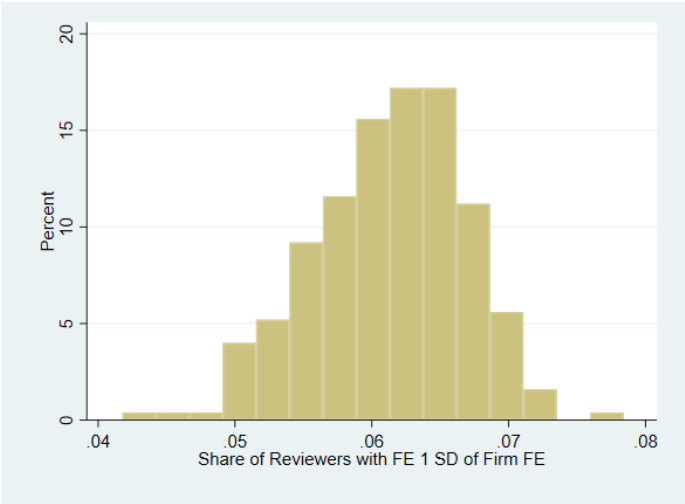
The figure plots the probability of a score higher than 2, separately for reviewers with positive and negative fixed effects (from Eq. A31).

Figure A34—Placebo Tests Reviewer Fixed Effects

Panel A— Distribution of *F*-values

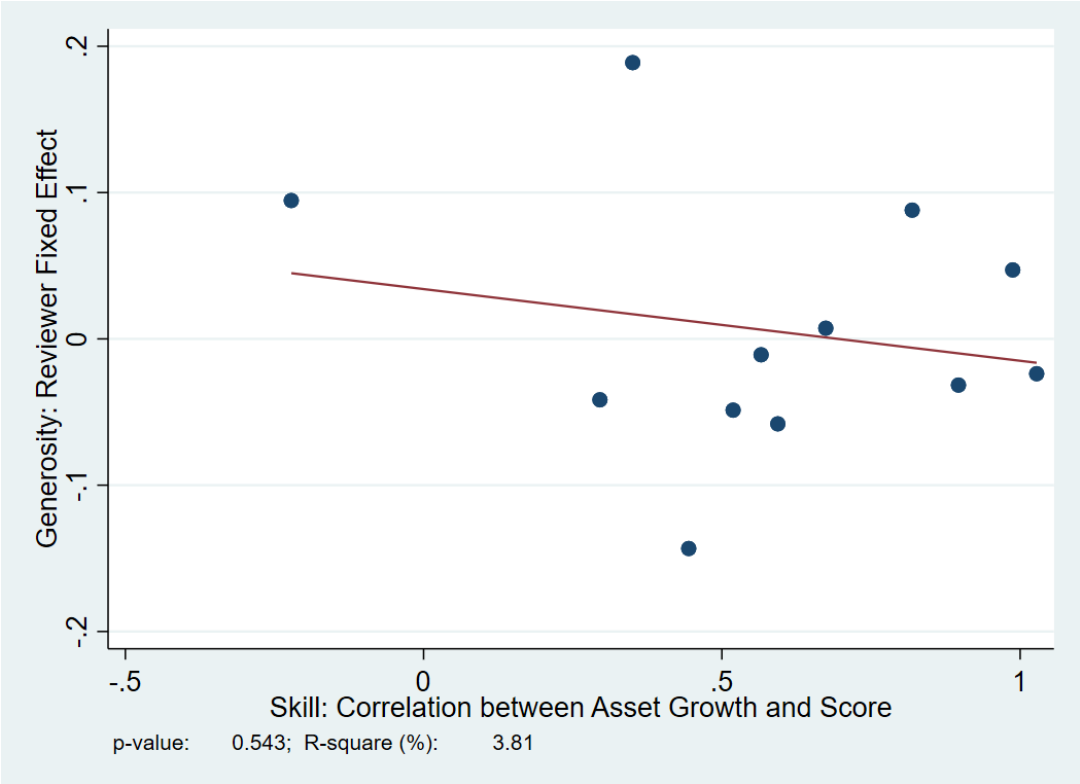


Panel B— Fixed Effects One Standard Deviation Above/Below Applicant Effect



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

Figure A35— Figure 6. 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 fundraising post application ("actual rank") and 2) the reviewer's score (" reviewer's rank").

Appendix 4 –Reviewer Fixed Effects and Email Content

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Sentiment					Product/Strategy					Financial/Hiring				
DAP	0.06	0.05		-0.06	0.01	0.01	0.01		0.01	0.01	-0.02	-0.02		-0.02	-0.05
	(0.04)	(0.04)		(0.06)	(0.06)	(0.04)	(0.04)		(0.06)	(0.06)	(0.04)	(0.04)		(0.06)	(0.06)
Firm FE		0.19***			0.19***		0.01			0.01		-0.08***			-0.08***
		(0.01)			(0.01)		(0.01)			(0.01)		(0.01)			(0.01)
Reviewer FE			0.50**	0.68**	0.20			0.01	-0.02	-0.05			-0.06	0.01	0.22
			(0.18)	(0.26)	(0.25)			(0.18)	(0.26)	(0.26)			(0.18)	(0.26)	(0.26)
Constant	0.49***	0.50***	0.42***	0.40***	0.47***	0.64***	0.64***	0.64***	0.64***	0.64***	0.53***	0.52***	0.53***	0.52***	0.49***
	(0.01)	(0.01)	(0.03)	(0.04)	(0.03)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)
N	6179	6179	6179	6179	6179	6189	6189	6189	6189	6189	6189	6189	6189	6189	6189
R-sq	0.77%	7.57%	0.86%	0.88%	7.58%	0.40%	0.42%	0.40%	0.40%	0.42%	0.81%	2.12%	0.81%	0.81%	2.14%

The table correlates the content of reviewer external comments and firm fixed effects reviewer fixed effects and DAP. See Section

Appendix 5—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	2.44	2.67	0.96	2.52	2.63	0.97	2.57	2.62	0.99	2.91	2.51	0.95
Female Founder	0.14	0.13	0.97	0.14	0.12	0.96	0.13	0.13	0.99	0.10	0.14	0.91
Target Amount (£1000s)	1438.5	1778.1	0.89	1911	1619.2	0.91	1594	1725.2	0.96	1831.4	1647	0.94
Target Close Date (Days)	84.11	78.18	0.93	80.21	79.5	0.99	77.68	80.35	0.97	76.62	80.68	0.95
Total Addressable Market (£Billion)	517.67	286.74	0.89	232.52	381.61	0.93	342.08	345.76	1.00	284.56	364.94	0.96
Total Serviceable Market (£ Billion)	62.02	39.3	0.93	32.96	48.96	0.95	48.83	43.73	0.98	35.97	48.03	0.96
Location/Stage/Business Type:												
London	44.74%	43.93%	0.99	44.06%	44.16%	0.99	45.73%	43.60%	0.97	41.96%	44.84%	0.95
Outside UK	14.98%	12.75%	0.95	17.42%	11.95%	0.87	10.16%	14.37%	0.90	10.65%	14.18%	0.92
Other Regions of UK	31.58%	35.85%	0.93	31.15%	35.97%	0.92	35.37%	34.57%	0.99	41.13%	32.70%	0.86
Pre-Seed	16.15%	12.21%	0.91	12.28%	13.52%	0.97	10.59%	14.07%	0.92	13.77%	13.02%	0.98
Seed	45.80%	45.02%	0.99	43.53%	45.78%	0.96	47.75%	44.38%	0.95	43.79%	45.68%	0.97
Seed Extension	38.05%	42.77%	0.92	44.20%	40.70%	0.94	41.67%	41.55%	1.00	42.44%	41.29%	0.98
Direct Sales	41.50%	42.16%	0.99	47.22%	40.25%	0.89	40.13%	42.61%	0.96	39.10%	42.95%	0.94
Platform	52.98%	53.73%	0.99	48.11%	55.36%	0.88	57.62%	52.19%	0.91	55.51%	52.89%	0.96
Deep Tech	5.52%	4.10%	0.95	4.68%	4.39%	0.99	2.24%	5.20%	0.89	5.39%	4.15%	0.95
CH Info. Before App.												
Asset (£1000s)	1736.8	266.6	0.93	276.8	752.7	0.98	200.96	794.05	0.97	324.39	747.36	0.98
Debt (£1000s)	1750.6	221.74	0.92	173.24	745.42	0.97	125.4	780.03	0.97	365.81	693.41	0.98
Annual Equity Issuance (£1000s)	169.58	154.64	0.98	178.8	152.18	0.97	142.86	163.87	0.97	144.17	163.25	0.97
Web Info. Before App.												
Number of Funding Rounds	0.45	0.47	0.98	0.43	0.48	0.97	0.51	0.46	0.96	0.48	0.46	0.99
Total Funding ($1000s)	274.09	317.4	0.97	346.45	293.12	0.96	307.85	305.97	1.00	297.62	309.31	0.99

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

Appendix 6—DAP, Opportunity Assessment, and Investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OLS—Opportunity Assessment												
DAP	0.04	0.06					0.02	0.05	0.01	0.04	0.00	0.03
	(0.03)	(0.04)					(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Reviewer FE			0.12	0.13			0.07	0.07			0.06	0.07
			(0.09)	(0.09)			(0.10)	(0.10)			(0.10)	(0.10)
Applicant FE					0.08***	0.08***			0.08***	0.08***	0.08***	0.08***
					(0.01)	(0.01)			(0.01)	(0.01)	(0.01)	(0.01)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953
OLS—Investment												
DAP	0.01	-0.00					0.00	-0.02	0.01	-0.00	0.00	-0.02
	(0.01)	(0.02)					(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Reviewer FE			0.08	0.06			0.07	0.09			0.07	0.09
			(0.04)	(0.04)			(0.05)	(0.05)			(0.05)	(0.05)
Applicant FE					0.01	0.01			0.01	0.01	0.01	0.01
					(0.00)	(0.00)			(0.00)	(0.00)	(0.00)	(0.00)
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953	1,953

The table presents results from regressing Opportunity Assessment (a variable indicating applicants that made it to the Fund's third stage of due diligence; Panel A) and Investment (a variable indicating applicants that are in the Fund's investment portfolio; Panel B) against due diligence assignment probability (DAP).

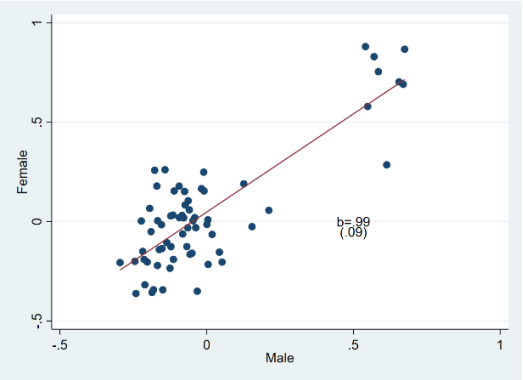
Appendix 7—DAP and Venture Outcomes: Reduced Form Estimates

	Panel A: Fundraising			
	ln(Funding)	ln(#Number of Rounds)	ln(#Investors)	ln(Equity Issuance) (UK)
	(1)	(2)	(3)	(4)
DAP	3.32***	0.34***	0.13**	2.03***
	(0.90)	(0.07)	(0.05)	(0.52)
N	1953	1953	1953	1548
R-sq	42.14%	1.31%	0.41%	24.90%

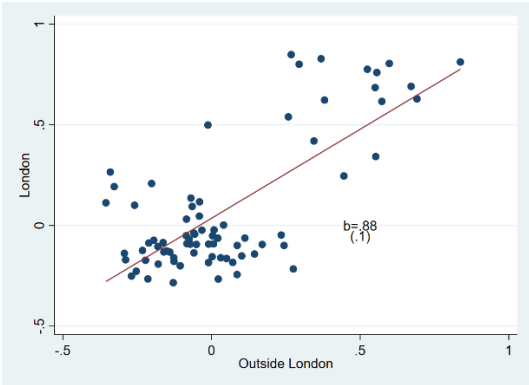
	Panel B: Economic Growth				
	ln(Number of Employees)	Growth in Asset (UK)	Growth in Debt (UK)	ln(Number of Appointed Directors) (UK)	Survival (UK)
	(1)	(2)	(3)	(4)	(5)
DAP	0.92***	1.07*	1.49***	0.47***	-0.12
	(0.23)	(0.48)	(0.40)	(0.11)	(0.07)
N	1548	1548	1548	1548	1548
R-sq	1.15%	0.35%	0.91%	1.06%	0.17%

The table presents results from regressing the outcome variables against DAP. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

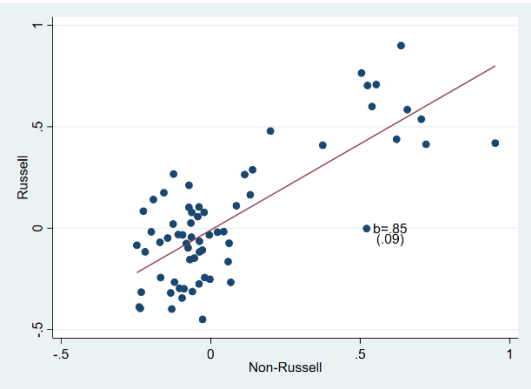
Appendix 8—Monotonicity Tests



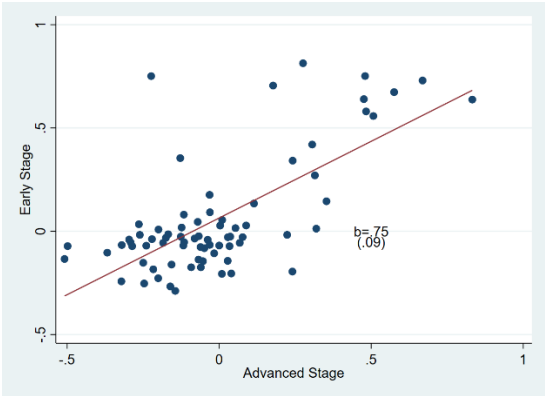
Female v.s. Male



London v.s. Non-London



Russel v.s. Non-Russell



Early Stage v.s. Advanced Stage

Notes: These figures show the correlation 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 firm fixed effects (score). 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 firms, founder with v.s. without Russell group education, early stage (pre-seed and seed) v.s. advanced stage (seed Extension).

Appendix 9—Content feedback and performance of rejected firms

	ln(Funding)	ln(#Number of Rounds)	ln(#Investors)	ln(Equity Issuance) (UK)
	(1)	(2)	(3)	(4)
Sentiment	1.53	0.03	-0.02	1.30
	(0.95)	(0.03)	(0.04)	(0.72)
Product/Strategy	-1.46*	-0.07	-0.12**	-1.14*
	(0.74)	(0.04)	(0.04)	(0.47)
Financial/Hiring	-2.54**	-0.11**	-0.16***	-1.43**
	(0.78)	(0.04)	(0.04)	(0.47)
Constant	-1.43	0.25	0.65	-5.44
	(6.59)	(0.33)	(0.34)	(4.11)
N	1325	1325	1325	1017
R-sq	43.78%	53.02%	12.63%	29.36%

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

Appendix 10—Dropping Opportunity Assessment Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.25***	3.01***	0.15***	0.33***	0.07***	0.13**	0.43**	1.75***
	(0.27)	(0.86)	(0.02)	(0.07)	(0.01)	(0.05)	(0.15)	(0.48)
N	1905	1905	1905	1905	1905	1905	1505	1505
R-sq	42.54%	41.04%	3.19%	-0.85%	1.20%	-0.06%	24.36%	20.28%
F Stat.		131.01		131.01		131.01		97.84

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 the fundraising variables in the pre-application period as controls in the estimation, and so does the respective first stage. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). The sample includes only firms that did not make it to the Opportunity Assessment stage. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix 11—Robustness Checks Exclusion Restriction

Panel A: Variation in DAP Due to Policy Change								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	2.32***	6.00*	0.28***	0.34	0.15***	0.56**	1.57***	2.28*
	(0.51)	(3.00)	(0.04)	(0.26)	(0.03)	(0.19)	(0.28)	(1.06)
N	829	829	862	829	829	829	777	777
R-sq	51.46%	42.77%	21.54%	5.23%	12.20%	-23.02%	36.57%	25.77%
F Stat.		20.57		20.99		20.99		17.70

Panel B: Use the Residual DAP as Instrument								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Funding)		ln(#Number of Rounds)		ln(#Investors)		ln(Equity Issuance) (UK)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Due diligence	1.36***	4.74***	0.16***	0.37***	0.07***	0.28***	0.52***	1.02
	(0.26)	(1.33)	(0.02)	(0.11)	(0.01)	(0.07)	(0.15)	(0.66)
N	1953	1953	1953	1953	1953	1953	1548	1548
R-sq	0.4261	0.3695	0.0348	-0.0218	0.0148	-0.1033	0.2471	0.2412
F Stat.		79.33		81.66		81.66		76.72

In Panel A, based on the main identification model, we add trio fixed effects and restrict the sample to London firms. 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.