**The London School of Economics and Political Science**

*Essays in Empirical Corporate Finance*

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## Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).



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## Statement of co-authored work

I confirm that Chapter I was jointly co-authored with Professor Juanita González-Uribe, Professor Robyn Klingler-Vidra, and Dr. Su Wang and I contributed 35% of this work.

## Acknowledgement

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# Abstract

The aim of my thesis is to apply empirical methods to investigate corporate finance questions. In the first chapter, I investigate an unexplored role of venture capital (VC) investors on innovation: the potential value-add of due-diligence for firms involved in failed VC fundraising campaigns— i.e., startups that do not receive investment from the VC doing due diligence. We show that assignment to due-diligence leads to substantial increases in startup growth within two years of application, even for firms involved in failed fundraising campaigns. This new evidence implies that VCs' role in innovation affects many more firms than existing research has fully recognized, as it goes beyond their value-added effects on the portfolio companies in which they invest. In the second chapter, I collected a novel data set of the local planning applications in London and measure the quality of government by the speed of processing applications. I show causal evidence that the quality of local government can affect the households and firms' activities in housing markets and borrowing behaviors. The effects arise because the timing of property development is important to households and firms. The delay in planning permission will lead them to abandon the project and change behaviors in housing markets and borrowing. In the third chapter, I study the purchasing behaviors of council governments in England and the impacts on the supplier firms. Due to political motivations, council governments' purchasing patterns and procurement contract terms with local firms are different from that with nonlocal firms. Consequently, selling more goods and services to the local council government relative to other council governments results in more corporate investment.

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# Chapter I

## Failed Venture Capital Fundraising Campaigns and Startup Growth:

## The Value-Add of Venture Capital Due-diligence

**Failed Venture Capital Fundraising Campaigns and Startup Growth:**

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

**May 2022**


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



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

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

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

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

VCs recognize the importance of due diligence for returns (Gompers et al., 2020), and researchers have shown its value-add to portfolio companies (Cumming and Zambelli, 2016). Our novel premise is that due diligence can also add value to the companies that VCs assess but ultimately reject for investment.

This premise is consistent with anecdotal evidence: entrepreneurs often describe crucial learnings gained through engaging in VCs' due-diligence process, especially from failed fundraising campaigns.[3] However, it is not obvious: fundraising is a time consuming process that can distract founders from their ultimate goal of growing their companies and feedback from a VC that decides not to invest may not be constructive.

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

[3] For multiple examples, listen to podcast: The Pitch: https://gimletmedia.com/shows/the-pitch/episodes.

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

Our empirical setting helps us overcoming these challenges. First, our novel data comprises nearly 2,000 startups applying for capital from a VC seed fund in the United Kingdom Kingdom (hereafter "the Fund"), which is representative of other seed funds that are increasingly prevalent in startup ecosystems (Lerner and Nanda, 2020). Second, to measure startup performance, we rely on administrative UK (abridged) balance sheet data that we combine with traditional web sources to track further startup growth and venture capital fundraising from VCs other than the Fund. Finally, for identification, we exploit the Fund's structured and well-documented process of screening applicants for due-diligence, which, is consistent with the rise of systematic scoring via "scorecards" by early-stage VC funds (Malenko, Nanda, Rhodes-Kropff and Sundaresan, 2020). We construct an instrumental variable (IV) by exploiting two features of the Fund's selection process: (1) the quasi-random assignment of applicants to three reviewers with different scoring generosities and (2) the aggregation of reviewers' scores using pre-determined selection rules that vary over time and across applicants' locations.

Through this empirical strategy, we find novel evidence that VC due-diligence can be a (positive) driver of startup performance even for firms involved in failed VC fundraising campaigns— i.e., startups that do not become part of the portfolio of the VC conducting the due diligence. We find that assignment to due-diligence by the Fund leads to substantial increases in startup growth within two years of application (as measured by several proxies) even for those firms involved in failed fundraising campaigns with the Fund. Results are robust to different specifications, additions of controls, and other multiple robustness checks. In terms of economic magnitude, our results imply that assignment to due-diligence alone increases VC fundraising (from VCs other than the Fund) by £160K. This estimate corresponds to a 20% increase relative to the 75$^{th}$ percentile of the post-application fundraising

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

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

distribution, which has a long right tail and a mean of zero. By contrast, we find no evidence that startups' assignment to informal meetings not part of the Fund's due-diligence has casual meaningful effects on venture performance.[6]

The main implication from our findings is a broader impact of VCs on innovation than previously acknowleged, extending beyond value-add to their portfolio firms (less than 1 out of 100 applicants) and covering the entrepreneurs they interact with through due-diligence (approximately 30+ out of every 100 applicants). This implication is line with mounting evidence showing that investments by early-stage investors disproportionately affect the economy (Kortum and Lerner, 2000; Samila and Sorenson, 2011; Fehder and Hochberg, 2019; Opp, 2019).

The results provide compelling evidence that VC due-diligence can add value to entrepreneurs, and by extension, entrepreneurial clusters more broadly than currently understood, by impacting the growth of startups with failed fundraising campaigns. Therefore, networking frictions for startups seeking VC funding – which we understand as startups' inability to gain access to meet potential investors – can have profound implications for growth, as such meetings positively affect startup performance even when there is no investment (cf., Howell and Nanda, 2021). Our results are more likely to extend to other seed VCs, who like the Fund, target inexperienced entrepreneurs seeking specialized financing as the costs to start and develop businesses have fallen (Ewens, Nanda, and Rhodes-Kropf, 2018).

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

[6] We build an instrumental variable strategy to assess the impact of informal meetings by exploiting the selection rules for those meetings; See Section 3.2.

While not necessarily representative of all VCs, our results do represent this new type of VC that is increasingly prevalent in entrepreneurial markets.

The second limitation of our setting is our inability to distinguish the relative importance of specific due-diligence value-add mechanisms, which can be classified into two broad (non-mutually exclusive) categories. The first, which we refer to as *type improvement*, refers to the idea that by going through the VC due diligence process entrepreneurs can improve their businesses as they learn-by-doing, gather and process feedback from potential investors, and gain access to new information and networks. The second channel, which we refer to as *type discovery*, refers to how VC due diligence can also signal positive information about the prospects of the startup (and the entrepreneurs) to other prospective investors, the wider ecosystem, and the entrepreneurs themselves, leading to startup growth via improved access to market resources and entrepreneurial commitment and effort.

While we have no exogenous variation in our data for clean identification, the preponderance of evidence points to *type improvement* as a vital impact channel. Interviews with the Fund partners reveal that they perceive this to be the main mechanism given their commitment to provide substantive coaching to applicants regarding their go-to-market strategy and unit economics. We deploy several auxiliary tests to show that *type discovery*, instead, is less likely to play a main role. Despite this evidence, we acknowledge that none of the results are conclusive, and more research is needed in future work to help disentangle between the relative strength of the impact mechanisms.

Our findings contribute to two main bodies of literature. The first explores the role of VCs in innovation and the real economy. Most of this literature focuses on establishing the value-add of VC on their portfolio companies (Lerner and Nanda, 2020). There is evidence in this literature that due-diligence is essential for VC returns, both in survey evidence by Gompers et al. (2020), and more formally in Sorensen (2007). However, whether due diligence, and specially from failed rounds is a main driver of start-up performance remains understudied (see Cumming and Zambelli, (2016) for an exception looking into the value-add of due diligence for portfolio firms).

Our results complement research showing that early-stage investors have local spillover effects (Samila & Sorenson 2011; Fehder & Hochberg 2019), and VCs have a disproportionate contribution on innovation (Kortum and Lerner, 2000; Gonzalez-Uribe, 2020). We provide a channel for this contribution, as the due diligence process – in its own right – positively impacts a wider set of startups in an ecosystem. What incentivizes investors to add value to companies that do not become part of its portfolio? Ex-post, adding value through due-diligence appears inefficient because investors do not appropriate the performance improvements for the majority of firms that benefit from this value add. However, ex-ante, before the investment decision is made, the investors have incentives to add-value to all firms that make it to the due diligence stage as for these firms the probability of investment is non-zero, and the value-add can increases the acceptance probability of any term sheets offered. In addition, investorsm specially those recently established like the Fund, can also benefit indirectly from providing value-add through due diligence, for example, by building a value-add reputation that can improve future deal-flow and investor's discount rates (cf., Hsu, 2004; Sorensen, 2008).

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

The second literature we contribute to focuses on startups' life cycle, and the importance of interactions with different intermediaries in that process. Several papers in this literature have looked at entrepreneurs' potential learning via business plan competitions (Howell, 2020; McKenzie, 2019) and accelerators (Gonzalez-Uribe and Leatherbee, 2016; Gonzalez-Uribe and Reyes, 2020). Our results suggest that VCs may play a role in startup ecosystems similar to other intermediaries seeking to systematize the coaching of inexperienced entrepreneurs, such as business accelerators (Gonzalez-Uribe and Hmaddi, 2021, Hochberg, 2016). In the context of venture capital, research has acknowledged the growth of a so-called "spray and pray" strategy, in which early-stage VCs make a large number of small investments, at the expense of interacting more closely with founders, which lessens the learning opportunities post-investment (Ewens et al., 2018). We contribute to this literature by emphasizing the value-add from due-diligence by seed VCs, rather than post-judgement or post-investment activities by other early-stage investors. Our results substantiate the business opportunity that accelerators and incubators are exploiting; providing feedback and connections to early-stage companies can deliver added value to the startup and can be monetized.

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

## 1. Institutional Setting

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

### 1.1. The Fund

The Fund is a seed fund in the UK managed by a VC firm established in November 2016, which began investing in portfolio companies in March 2017.[7] The Fund specializes in investing in early-stage ventures operating in the software sector, broadly defined. It is business-model agnostic within that sector, covering direct-to-consumer businesses, platforms and deep tech. As is increasingly common among Seed funds, the Fund does online deal sourcing, relying on an online platform to receive applications for funding. This, the Fund contends, helps to democratize access to venture capital financing in the UK, by offering an open platform for application rather than entrepreneurs having to rely on social networks to get an introduction. By November 2019, the Fund had received nearly 2,000 online applicants, which constitute our analysis sample, and also, represents the end of the period in which the Fund was making new investments. While we cannot provide exact details of applicants to the Fund, some examples include companies seeking to advance the use of biometric data in security measures and to enable desk management in collaborative workplaces.

Also like other seed funds, the Fund's investment check size is between $50K-$5M, which attracts early-stage businesses seeking to raise seed capital before approaching more traditional VC funds for Series A investment.[8] These types of funds have continued to become ever more prevalent in recent years (Klingler-Vidra, 2016). The significant fall in the costs of starting and developing ideas, especially in the software industry (for example, with the advent of cloud services by Amazon in 2006), has led to increasingly inexperienced founders seeking venture capital financing (Ewens, Nanda and Rhodes-Kropf, 2018). New intermediaries have emerged in early-stage entrepreneurial finance markets, including this new breed of 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 and gain early investment access to the most promising candidates.

Also similar to other seed funds, the Fund uses a systematic approach to screen applicants for due-diligence than more traditional VCs. As we explain in more detail in Section 1.4, the selection process of the Funds involves two steps. The first is the allocation of the online applications to three reviewers (internal to the firm) that score the submission and record feedback. As we explain more fully in Section

[7] The Fund shared their data with us under a Non-disclosure agreement which prevents us from sharing more specific details about the setting.
[8] The average Seed stage investment in Europe was $1.9M in 2021, and the average Seed stage investment in the UK in 2019 was £0.57M. See https://www.bvca.co.uk/Portals/0/Documents/Research/Industry%20Activity/BVCA-RIA-2019, and see also https://assets.kpmg/content/dam/kpmg/uk/pdf/2021/04/venture-pulse-q1-2021.pdf.

1.4, the matching of the applicant to the reviewers is orthogonal to the quality of the application or the applicant. Reviewers' comments are later shared with founders, along with the selection outcome.

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

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

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 sharing the "data room", a pitch to the Investment Committee, hiring industry experts for external reviews and calling on other parties, including references provided by the founders, in order to validate the venture's claims and assumptions. Candidates that pass all three stages are presented with a term sheet summarizing the Fund's conditions for investment. Finally, the company agrees to the term sheet (or negotiates changes to the terms, that are agreed by the Fund), and the deal closes.

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

### 1.2. Application data

The Fund provided us with all the application data, including application scores assigned by each reviewer and the final selection decisions for each applicant. Our sample consists of all the 1,953 applicants seeking capital from the Fund during the March 2017 to June 2019 period.[9] Figure 2 shows the number of applications made each month. At the peak month, the number of applications was 140.

Based on the applications, we constructed several variables to use as controls in our empirical strategy: firm's location, age of firm as of application (relative to incorporation date), target amount to raise, funding stage (pre-seed, seed, or post-seed), business type (direct sales, platform, and deep technology), and also, founders' personal characteristics (e.g. gender, education). Table 1 reports summary statistics for the main variables in the application forms. On average, applicants have been incorporated for 2.61 years at the time of application and aim to raise an average of £1.6M. 13% of the applicants include at least one female founder. Figure 3 shows the location, stage and business type breakdown: 47.86% are in London, 45.27% are in seed-stage, and roughly half are categorized as direct sales businesses and half are platform businesses, with only a tiny minority of applicants in deep technology. The average number of founders per venture is 1.94.[10]

Although self-selection of companies 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. For example, companies with founders with prior VC fundraising and exit experiences are less likely to apply for funding through an online platform because they can reach out to their previous investors. From this perspective, 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.

Applicants to the Fund are comparable to the average firm securing seed financing in the UK, but appear smaller at the median, which is consistent with the idea that online applicants are less established and experienced. The average venture size for firms securing seed financing in the UK is £492K, which is slightly smaller than the average in our sample of £641K. However, at the median, our applicants look much smaller, with £23K in assets, relative to a median asset size of £184K for firms that secured seed in funding, which is consistent with the seed and pre-seed stage of the applicants. To produce this benchmark we collected information from Companies House for 257 ventures in the information and technology sector (the sector which all of the applicants to the Fund are also operating in) that raised seed funding in 2019 in UK (we collected this information from  Crunchbase and Preqin). By matching

[9] 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.

[10] This information is not provided by entrepreneurs in their applications but we sourced it form Crunchbase. We found1,178 ventures and 2,286 founder and co-founders. So the average number is 2286/1178=1.94.

the name, location and website of ventures, we collect 2018 total assets for 169 ventures from Companies House.

### 1.3. Outcome Data

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

First, we collect novel administrative data for businesses incorporated in the UK, which are most of the businesses applying to the Fund (80%). These data come from the business registry in the UK (Companies House; "CH") and includes 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 must include information on more detailed balance sheet accounts, employment data, and income statements in their filings, smaller firms are exempt.

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

We construct the following outcome variables from CH filings: log equity issuance, log number of directors appointed, log growth in assets, log growth in debt, and firm survival and liqidation in the sample period before and after application, separately.[11] Survival is an indicator variable that equals one if the the firm did not file for liquidation, closure, or dormancy after application by 2020. Liquidation is an indicator variable for firms that filed for liquidation after application and by 2020. Note that liquidation is not tantamount to bankrupcty in the UK as solvent firms also file liquidation paperwork (see Balloch, Djankov, Gonzalez-Uribe, and Vayanos, 2022). Directors include all individuals with a C-level job in the firm, e.g., Chief Executive Officer. As is common among early stage businesses, outcome variables are highly skewed. So, we rely on logarithmic transformations of the variables (after adding 1) to implement the regressions. In addition, for better interpretation of the regression coefficients, we focus on the gap between the median and the 75$^{th}$ , which we report in last rows of the tables for reference.

Our second strategy for collecting performance data follows the standard practice in the VC literature to measure venture performance using web-sources like Crunchbase and Linkedin, as these sites' coverage is likely to be better for seed-stage companies with no institutional investors relative to later-

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

stage data vendors' sources like PitchBook or VentureSource.[12] We construct the following outcome variables: Total funding, number of fundraising rounds, number of investors and number of employees after the application. Given their skewness, all outcome variables are added with 1 and logaritmized to implement the regressions. We can cover all applicants using this method, rather than UK businesses only as in the first method.

We also collect founders' education backgrounds and work experiences from their LinkedIn profiles whenever available, and supplement this information with co-founders work experiences from their Crunchbase webpages.[13] In terms of education, we code whether founders have secondary higher education (e.g. Bachelor's degree) from an elite university. Since most of the firms in our sample are UK firms, we operationalize elite university according to the Russell Group (e.g. top 20 UK universities) and the "Golden Triangle" (Oxford, Cambridge, UCL, LSE and Imperial) sets of universities. We also code and group universities according to 2020 global rankings, including Times Higher and ARWN (Academic Ranking of World Universities).

One novel feature of our data collection strategies is that we have information on fundraising from administrative data (Companies House) and web sources (Crunchbase and LinkedIn) for the applicants in UK. The administrative data includes equity sources other than specialized financing like VC, whereas the Crunchbase data mainly includes details of 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 that in the registry to gauge the degree of potential selective online posting. We find little evidence of selective posting (correlation between the 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.

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

[12] 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.

[13] 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.

### 1.4. Due-diligence Selection Process

In this section, we review the Fund's process to sort applicants involing two main steps that we now explain in detail: (1) reviewers' assignment and (2) the aggregation of scores using selection rules. We end by describing how the Fund communicates their decisions to applicants.

#### 1.4.1. Reviewer assignment

The first step in the due-diligence process is the assignment of three reviewers to each online applicant. Reviewers are internal to the Fund and are founding and managing partners (four out of 12), partners (four out of 12), and Associates (four out of 12). [14]

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

The assignment of applications to reviewers is done using proprietary software developed by the Fund for collaborating and managing spreadsheet-like inputs.[15] The software assigns application numbers to incoming applications, and classifies them according to the location of the business as self-reported by the applicants. There is a total of 16 regions, following the standard 12 region and nations classification of the UK, plus a further breakdown to best reflect local entrepreneurship clusters, and non-UK applicants.[16]

The software automatically assigns three reviewers to each applicant based on the location and reviewers' workload: staff are temporarily taken off the review assignment if they go on holiday or are busy with other tasks, like fundraising. In addition, the system prioritizes allocations to reviewers that have as regional focus the location of the applicant; six out of the 12 reviewers have a regional focus and act as investment lead for different regions, which vary from single cities (e.g., London) to larger

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

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

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

areas (e.g., Southwest of England, Scotland). The majority of regions (10 out of 16) have at least one designated investment lead. However, a "regional focus match" between applicants and reviewers is neither sufficient nor necessary for an assignment.[17]

In addition, the software also determines an "Investment Lead" among the three assigned reviewers. The Investment Lead oversees the assessment of the other two reviewers and chases them to complete their reviews within the Fund's 24-hour turnaround goal. The Investment Lead then collates the reviewers' assessments and communicates the decision to the applicants, as we explain in more detail in Section 1.4.3. The software prioritizes assignment of the Investment Lead role in line with the company's region, but availability constraints meant that in practice the regional match is not universal among applicants.[18]

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

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

### 1.4.2. Reviewers' scores and comments

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

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

Each reviewer observes the information in the application, annotates comments, and provides a score of {1, 2, 3, 4; where 4 is best} for the applicant using the Fund's software. There is substantial scoring heterogeneity across reviewers. We now summarize the results from our methodology to show this heterogeneity, and we present full details in Appendix 4.

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

There are two important features about the heterogeneity in reviewers' scoring generosity that we briefly summarize here, but we also describe in more detail in Appendixes 4 and 5.[19]

First, the scoring generosity of reviewers is unrelated to their skill in selecting applicants; see Appendix 4. 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 (-0.039; p-value 0.642) across reviewers; albeit with the caveat that the correlation is estimated with 12 observations.[20]

Second, the scoring generosity of reviewers is also unrelated to the content of the reviewers' comments: more generous, relative to less generous, reviewers provide equally toned comments, and discuss similar themes, including financing opportunities, employment plans, product improvements, or market strategy adjustments. We analyse the comments data by applying natural language programming techniques to assess the sentiment and the practical advice of the comments, as we explain in more

[19] In unreported analysis, we show that the scoring generosity is also unrelated to characteristics of the reviewers like their geographical focus, gender, or seniority (as measured by job postion: founding and manager partner, partner and associate).

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

detail in Appendix 5. The appendix shows no correlation between the content of reviewers' comments and their generosity, although comments of more generous reviewers are on average shorter .[21]

### 1.4.3. 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 faced a stricter selection rule than applicants elsewhere. The Fund changed selection rules in response to internal discussions regarding its investment thesis as part of their first investment year review. Senior partners perceived a need to treat entrepreneurs located outside London differently, to improve their chances of making it to due-diligence, and ultimately, investment. Their perception 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 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: (1) Pre-May 2018, (2) Post-May 2018-London, and (3) 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" (Malenko et al., 2021) 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 equal average scores but that have no "4". For example, a score combination of {1 2 4} has the same average score (2.33) as the combinations: {1 3 3} and {2 2 3}. Yet, neither alternative score combination leads to due-diligence assignment under the Post-May 2018 London regime. We note too that the only combination of scores that leads to no meeting is {1 1 1}; all other score combinations lead to either the offer of an informal meeting or to enter into the due-diligence process. The Fund considers {1 1 1} companies as non-venture backable, given the small size of market opportunity, the insufficient sophistication of the business, and/or the lack of technological talent (e.g. plans to outsource the Chief Technology Officer function).

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

Figure 5 shows the distribution of score combinations across distinct selection rule regimes. There are two main takeaways from the figure. First, specific scores are popular regardless of the regime—for example, {2 2 2} is always the most popular score across regimes. Second, the distributions of score combinations in the three regimes are similar, even though the selection outcome (due-diligence, informal meeting, and no meet) for specific scores varies across regimes. The patterns in the plot thus suggests that the scoring behavior of reviewers is independent of the selection rule. Kolmogorov-Smirnov tests show there is no significant difference in the scores' distributions between applications before and after the change in selection 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 persistence of the underlying heterogeneity in scoring across judges we discussed in the previos section and that we detail in Appendix 4.

#### 1.4.4. Communication of the Due-Diligence Selection to Applicants

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

The correspondence with founders uses three standardized email templates; see Appendix 1 for full transcripts. The wording used in the email is precise about the application's result, and whether the founders get to meet the Investment Lead, and the expectations for that meeting.[22] No email includes individual or average scores or the names of the reviewers; only the comments provided by each reviewer are included. While the Investment Lead signs the email, the applicants are unaware that the signer is part of the reviewing team. No email includes details on the selection rules either (which are also not available online nor shared outside the Fund). The emails include a general description of the

[22] 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?...*".

sorting method only.[23] 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' comments are included "as is" in the body of the message.

## 2. Empirical Strategy

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

### 2.1. Baseline Specification

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

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

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

Where $Y_i$ is the post-application outcome for applicant $i$, $Due\ diligence_i$ indicates the companies assigned to due-diligence and $\boldsymbol{Z}_i$ is a vector of controls at the time of the application including the outcome variable pre-application in all specifications (except for employment where we have no pre-application information), and log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. We condition on regional fixed effects in all specifications.

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

The major empirical challenge is that due-diligence selection by the Fund is endogenous. For example, a promising applicant with a high-potential business idea may attract venture capital (from other VCs) and grow, and at the same time, be chosen for due-diligence by the Fund. This endogeneity would

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

generate a positive correlation between $\varepsilon_i$ and $Due\ diligence_i$ in equation (1) and an upward bias to the estimate of $\rho$.

## 2.2. Identification Strategy

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

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

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

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

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

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.

For a given applicant *i*, *w*e calculate these fractions ($p_i^{s_1}$, $p_i^{s_2}$, $p_i^{s_3}$) 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). This feature of our instrument allows us to control for any additional effects that applicant specific unobservables may have on the decision to select the business for due diligence. To be sure, by dropping the review of applicant $i$ from the construction of the DAP instrument for applicant $i$, any additional information revealed during the assessment of the reviewers (e.g., web page searches about the company during the review process) or any discussions among reviewers about the applicant (for example, potential collusion, or influence by more senior staff if reviewers figure out the identity of co-reviewers outside of Airtable; see Section 1.4.1 for a discussion on the low probability of this event) is removed from the instruments' construction and thus does not contaminate it.

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

Our main estimation approach instruments due-diligence assignment with DAP. In robustness checks, we also present results using the predicted probability of assignment obtained from the probit model $\widehat{DAP} = P(DAP, Z)$ as the instrument for due-diligence assignment. When the endogenous regressor is a dummy, as due-diligence in our case, the estimator $\widehat{DAP}$ is asymptotically efficient in the class of estimators where instruments are a function of DAP and other covariates. However, the linear model has the advantage of facilitating the interpretability of the estimates when we include further controls in our regression like location fixed effects to control for the prioritaziation of regional matches in reviewer assignment (as we discuss in more detail below; see Section 1.4.1).

Specifically, we estimate the following two-stage model:

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

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

where the set of controls $\boldsymbol{Z}_i$ is the same in both stages and is the same as in equation (1). We condition on regional fixed effects in all specifications., so that we capture the regional focus of reviewers in the assignment.  We report heteroskedasticity-robust standard errors of our estimates. In unreported analysis, we show that results are robust to using bootstrapped standard errors.

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

### 2.2.1. First Stage

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

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

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

There are two main takeaways from Table 2. Across all specifications, the coefficient of DAP is positive and statistically significant, and the F-test of the excluded instruments is above the rule of thumb of 10. In terms of economic magnitude, our most conservative estimate of 0.94 in column 5 implies that a 10 percentage point increase in DAP is associated with a 9.4 percentage point increase in the likelihood of due diligence assignment. In terms of standard deviations, the coefficient in column 5 implies that an increase in one standard deviation of DAP, increases the due diligence assignment probability by 0.27 standard deviations.[24] We obtain similar results using a probit model (Panel B) —the implied marginal effect from the probit regressions in column (5) is 0.85—which is unsurprising given that the mean of due-diligence assignment is 0.31 and far from zero and one.

[24] 0.27=0.94×0.13/0.46, where 0.13 is the standard deviation of DAP and 0.46 is the standard deviation of due diligence.

### 2.2.2. Exclusion restriction

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

The conditional independence asumption is sufficient for causal interpretation of the reduced form results reported in Appendix 6. That is, our reduced-form estimates can be interpreted as the causal impact of being evaluated under a more or less stringent standard (i.e., as measured by the reviewers' generosity and the selection rule). 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.

This assumption, however, is not sufficient for a LATE interpretation of the two-stage least squares estimates. For such an interpretation, we would require the exclusion restriction assumption to hold—i.e., DAP impacts applicants' outcomes exclusively through the single channel of due-diligence assignment, and not through any other mechanism.

This exclusion restriction would fail if the outcomes of applicants with a high DAP were affected in some additional independent way other than through an increased likelihood of due-diligence assignment.[25] For example, a higher DAP could be associated with more hands-on treatment if reviewers that tend to score applicants generously, also spend more time on due-diligence, and this additional effort has an independent effect on applicants' performance.[26]

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

First, Appendix 7 shows that DAP does not correlate with the content in reviewers' comments (as measured by tone and themes covered; see Section 1.2.1, suggesting potential independence between reviewers' due-diligence quality (as proxied by "note-taking" during the application assessments) and their scoring generosity. This lack of correlation is consistent with the results in Appendix 5 showing that more generous reviewers do not write more positive, or differently themed, comments.

[25] 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).

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

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

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

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

In Section 4, we show that results are robust to controlling for Investment Lead fixed effects, which mitigates concerns that differences across due diligence by Investment Leads with different generosities drives the results.[27] Further, we show that results are robust to estimating models that exploit selection regime changes holding constant the trio of reviewers. This analysis restricts the sample to London applicants, for which the selection rule becomes more stringent post-May 2018 when the Fund adopts the Champion model. This robustness analysis is useful in relaxing the identification assumption, because the identification relies on variation from the DAP stemming from differences in selection rules, rather that differences in generosity across reviewers. Intuitively, we estimate due-diligence effects by comparing firms at the margin of selection rules under different regimes, holding constant the generosity of reviewers. A vital identification assumption in these alternative models is that reviewers' scoring generosity does not change across selection regimes. Consistent with this assumption, Figure 5 (and Kolmogorov-Smirnov tests explained in the notes) shows that the distributions of score combinations in the three regimes are indistinguishable (see Section 1.2.2).

#### 2.2.3. Monotonicity

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

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

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

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

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

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

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

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

This section presents our estimates of the causal effects of the Fund's due-diligence assignment on venture performance. We first show our baseline and LATE results on 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 4 presents ordinary least squares (OLS) and two-stage least squares estimates of the impact of the Fund's due-diligence assignment on venture fundraising after application. Panel A uses the entire sample, and Panel B excludes the 12 firms in the Fund's portfolio. 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, and log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. Robust standard errors are reported throughout.

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

The two-stage least squares estimates in columns 2, 4, 6, and 8 improve upon our OLS estimates by exploiting the plausibly exogenous variation in reviewer assignment. These two-stage least squares results confirm that applicants assigned to due-diligence raise more equity financing than otherwise similar applicants who were assigned to either informal meeting or no meet by the Fund. The coefficient in column 2 of Panel B implies that assignment to due-diligence leads to an additional £142K in equity fundraising within two years of applying to the Fund. To produce this estimate, we compare the increase in Column 2 with the 75$^{th}$ percentile in post-application log fundraising distribution and multiply it by the 75$^{th}$ 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 274 percentage points increase in fundraising, which corresponds to a 20 percent increase from the 75$^{th}$ percentile of the log fundraising distribution.[28]

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). The implied economic magnitude of the coefficient in column 8 is £45K, which is comparable to the £142K implied fundraising from column 2.[29] Finally, columns 3-4 and 5-6, respectively, show that the fundraising effects are explained by higher numbers of financing rounds and participation by a larger number of 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 4. In Section 2.1, we explained how the endogeneity of the Fund due-diligence selection would generate a positive correlation between $\varepsilon_i$ and $Due\ diligence_i$ in equation (1), and therefore, an upward bias to the estimate of $\rho$. 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 strictness (generosity) of their reviewers). However, we note that large standard errors mean that the difference between the two-stage least squares and the OLS estimates for all the fundraising proxies is not statistically significant.

Table 5 replicates the OLS and two-stage least squares regressions of Table 4, using other growth variables. Across all variables and panels, the two-stage least squares estimates are positive and

[28] £142K=2.74/13.46×£698K, where £698K is the 75$^{th}$ percentile of the web-fundraising distribution (median is 0); see Table 1.
[29] £45K=1.11/6.24×£255K, where £255K is the 75$^{th}$ percentile of the administrative-fundraising distribution (median is 0); see Table 1.

statistically significant. These results mitigate concerns that due diligence teaches entrepreneurs how to game VC and raise funds, but have no effects on real (i.e., non-financial) venture performance. The only exception in Table 5 is survival; meaning that we find significant impact on the intensive margin (fundraising and growth) but not on the extensive margin (survival).

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

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

Average performance of firms assigned to due-diligence does not vary significantly with founders' educational background either. Results are similar for other educational background proxies. Similarly, in unreported regressions, we also find no impact heterogeneity across different applicant characteristics like gender, business development stage, or business type.

### 3.2. Potential Channels

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

Due-diligence is a highly intense process that VC investors use to scrutinize potential investments. Relative to one-off, 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 to engage, by both entrepreneurs and VCs, as the real possibility of investment exists.

These characteristics of the due-diligence process lead us to postulate two broad mechanisms through which due-diligence can affect venture performance: *type improvement* and *type discovery*. 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 applicants as entrepreneurs gather through learning-by-doing or from the potential investors new skills and resources, relevant to their fundraising and company management abilities, which increases performance—we refer to this mechanism as *type improvement*. VC due diligence can also signal positive information about the prospects of the startup (and the entrepreneurs) to other prospective investors, the wider ecosystem, and the entrepreneurs themselves, leading to startup growth via improved access to market resources and entrepreneurial commitment and effort—we refer to this mechanism as *type discovery*.

The Fund is dedicated to *type improvement* through its intention to provide incisive feedback and to have the investment leads guide entrepreneurs through the investment process. 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. Throughout the due-diligence process, the Investment Lead coaches the founders by working with them to effectively complete the spreadsheets, and often actively helping fill out the form, especially if there is sufficient enthusiasm for the venture.

On the other hand, while the Fund is young and lacks a track record, *type discovery* effects could still be possible especially for businesses and founders for which there is higher ex-ante uncertainty about the company and the entrepreneurs.

The mechanisms of type improvement and type discovery 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 *type improvement* is a first-order mechanism of the Fund's due-diligence effects.

According to extensive interviews with the Fund's staff, type improvement is the mechanism of due-diligence they deem as most likely to affect venture performance. As explained in Section 1, the Fund emphasizes its value-add through type improvement, including through their commitment to coach entrepreneurs, possibly connecting them to potential suppliers and clients, and providing them with the opportunity for learning-by-doing. 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).[30] In an interview, a member of the Fund explained that they felt "that people were guided by what we told them they needed in the process, like 'You need a business plan.' They would become more prepared by the result of the meetings. By us telling them: this is what VCs are looking for." In another meeting, a Fund team member even mentioned how some Investment Leads could go as far as to fill out 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 both about what VCs are looking for when making investments and about the underlying economics of their ideas, and how to improve the pitch of their business.

We deploy four exercises to provide more formal evidence that the channel of type discovery instead appears not as dominant in our setting.

First, we show no correlation exists between the comments' sentiment provided by the reviewers (and shared with applicants via email) and applicants' subsequent performance for the subsample of rejected applicants, for which the mechanism of type improvement is not operational. Instead, if type discovery were the main driver of the results, we would expect performance improvements as founders (and possibly market participants if founders share the emails) react to the sentiment about the startup and the founders revealed by the reviewers. We detail these results in Appendix 10.

Second, we show that the 45 due-diligence finalists firms (i.e., those that reach the Opportunity Assessment) do not drive the results. Instead, if type discovery effects were first order, we would expect the results to be driven by this subsample. For due diligence finalists, the signal about the firm quality, and thus the potential for type discovery by founders, especially third parties, is possibly the strongest. The Opportunity Assessment involves third parties, such that the Fund calls upon industry experts, applicants' references, and often competitors to scrutinize the firm further. Thus, there is more evidence for being able to claim to a third party that the venture is near to the point of obtaining a term sheet. Instead, type improvement potential for due-diligence finalists may not necessarily be the strongest. See, for example, evidence of non-monotonicity of type improvement effects in firm quality (and founder characteristics) for business accelerators, investment readiness interventions, and business plan competitions; Gonzalez-Uribe and Reyes, 2020; Cusolito, Dautovic, and McKenzie, 2020; Howell, 2020). We detail these results in Appendix 11.

Third, in unreported analysis we show similar results across multiple data cuts proxying for varying degrees of business and founder type uncertainty: first-time versus serial entrepreneurs, pre-seed and

[30] This is increasingly the norm amongst seed stage VCs. In their well-known practical book on venture deals, Feld and Mendelson (2019) argue that VCs vary in how much importance they place on detailed financial models "…Some VCs are very spreadsheet driven. Some firms (usually those with associates) may go as far as to perform discounted cash flow analysis… Some will look at every line item and study in detail. Others will focus much less on the details but focus on certain things that matter the most to them…"

post-seed businesses, and deep-technology versus more standard-technology companies. Instead, we would expect more substantial effects for founders and startups with higher type uncertainty if type discovery was the main driver of results.

Fourth, we find no significant performance effects of rejected applicants' assignment to informal meetings with the Fund's members that are not part of the due-diligence process. Instead, if type discovery were dominant, then positive informal meeting effects would be likely as founders (and possibly third parties) could react to the signal that the Fund considers the idea to be venture backable.

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

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

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

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

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

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

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

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

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

where $p_{1_h}$ denotes the probability that reviewer $h$ gives a score of 1 (based on all other reviewed applicants except $i$).

Table 8 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 meeting outperform applicants assigned to no meet 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. One caveat from these results is potential lack of statistical power: only a small fraction of applicants that are not selected for due-diligence have no informal meetings (4%). Against the concern that results are driven by lack of power, we note that most IV coefficients actually flip signs and most are much smaller in absolute value.

The results from these four tests lead us to argue that the channel of type improvement is most likely to be the main driver of the Fund's due diligence effects. None of these results in isolation are conclusive, but, when taken together, they suggest a potentially primary role for type improvement rather than type discovery in our setting.

That being said, we recognize that our paper takes the first step in assessing the value add of due diligence of failed fund raising campaigns, and it is clear more research is needed in future work to help disentangle between the relative strength of the impact mechanisms.

### 3.3. External Validity

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

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

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

Figure 9 reports the MTE of due-diligence assignment for web 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 equity financing (from investors other than the Fund) do not vary systematically across unobservable characteristics. The flat shape of the MTE curve suggests that our LATEs are likely to apply to filers who are further from the margin.

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

In terms of extrapolaito of results outside of the Fund, we note that the Fund is representative of a growing set of early-stage VCs, but of course, not all VCs. As argued above, the Fund, like others investing at the seed stage, has a scientific approach to sorting applicants and focuses strongly on coaching. Other new intermediaries operating in a similar fashion include other funds also focusing on pre-Series A financing (like seed and pre-seed funds), as well as super angels and accelerators. These early-stage intermediaries seek to sort through the noise and train the most promising of the increasingly inexperienced new founders seeking specialized financing and expertise. We thus argue that our results are most representative of these new types of VCs, especially those recently established and seeking to secure high-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). Our finding points to a different implication: rather than explaining the performance of VC funds, aspects of the VC selection process (precisely, due diligence) impact a broader ecosystem of ventures, offering a new mechanism for these spillover effects.

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 many more companies than the ones in which they invest. 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 by early-stage VCs on venture performance. Our results suggest that rather than VCs affecting the performance of only 1 out of 100 ventures (e.g. the 1% they invest in), they provide value-add to a further 30 ventures (i.e., the average 30% they conduct due diligence on; see Gompers et al., 2020).

The evidence on VC post-investment value-add highlights the importance of VC for venture growth but remains silent on the implications of participation in the process to secure VC. Our study 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 type improvement or type discovery. In this way, our study supports the growing evidence of how frictions in the process through which entrepreneurs connect with VCs can have profound implications for innovation and growth. Our analysis points to how high-potential – but not the absolute top - 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). Instead, our finding suggests that more VC-fundable but not high-fliers would benefit from engagement in the VC due-diligence process.

Our results raise an important question regarding the incentives of the Fund, and more generally VCs, in adding value to firms that do not become part of their portfolio. Ex-post, adding value through due-diligence appears inefficient because the Fund does not appropriate the performance improvements for the majority of firms that benefit from this value add. However, ex-ante, before the investment decision is made, the Fund has incentives to add-value to all firms that make it to the due diligence stage as for these firms the probability of investment is non-zero, and the value-add can increases the acceptance probability of any term sheets offered. In addition, the Fund can also benefit indirectly from providing value-add through due diligence, for example, by building a value-add reputation that can improve future deal-flow and the discount rate the Fund can charge (cf., Hsu, 2004; Sorensen, 2008).

## 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 12 (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 post-May 2018. We also present results using an alternative specification that uses the residual variation in DAP as an instrument after netting out the reviewers' generosity. Intuitively, this identification strategy also holds constant the generosity of reviewers; the main difference is that it does not hold constant the trio of reviewers for that purpose. Instead, it holds constant the average generosity of the reviewer trio (as estimated by the reviewer fixed effects in Appendix 4). Appendix 12 (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 4 show evidence in this regard, as explained in Sections 1.2.2 and 2.2.2. Taken together, these results provide additional evidence that due-diligence assignment positively affects venture performance.

*Alternative Specifications.*—In unreported regressions we explore the sensitivity of our main results to alternative specifications. We show that our main results are robust to including controls for firm quality as measured by the firms' fixed effects (from firm and reviewer fixed effects models described in Appendix 4). These results are similar to our preferred specification, indicating that potential bias from omitted variables is likely slight in our setting. Finally, we also experiment with refinements of our DAP instrument to control for potential expertise differences across reviewers in evaluating applicants with different observable characteristics. In detail, we modify our estimates of the $p_i^{s_1}, p_i^{s_2}, p_i^{s_3}$ 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 engage with ventures in order to determine whether, and at which terms, to invest. Our novel data comprises nearly 2,000 startups applying for funding to a UK VC seed fund (Fund). For identification, we exploit the Fund's process of screening applicants for due-diligence, which features pre-determined selection rules based on the scores of quasi-randomly-allocated reviewers. 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. By contrast, we find little evidence of venture performance effects from applicants' assignments to informal meetings with Fund team members that are not part of the due-diligence process.

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

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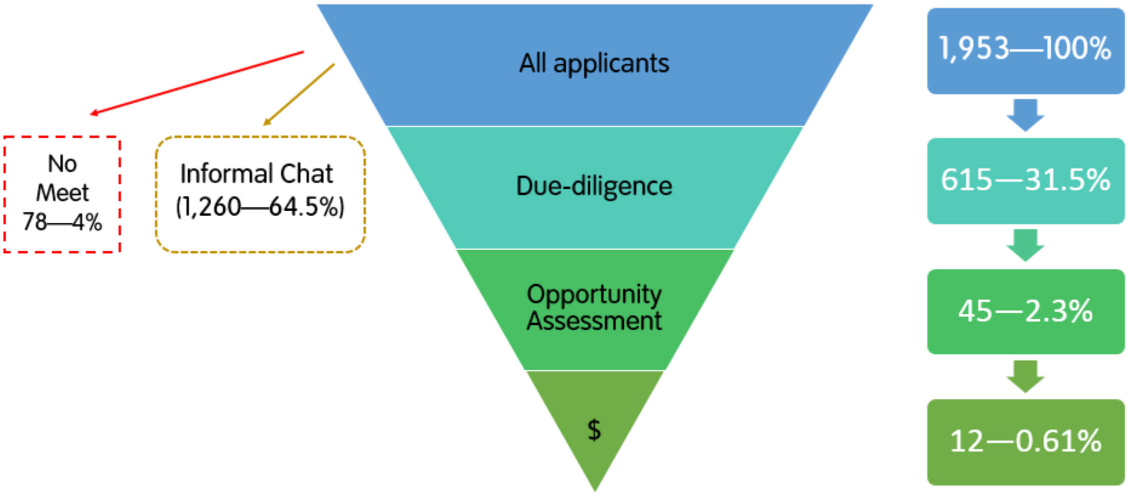
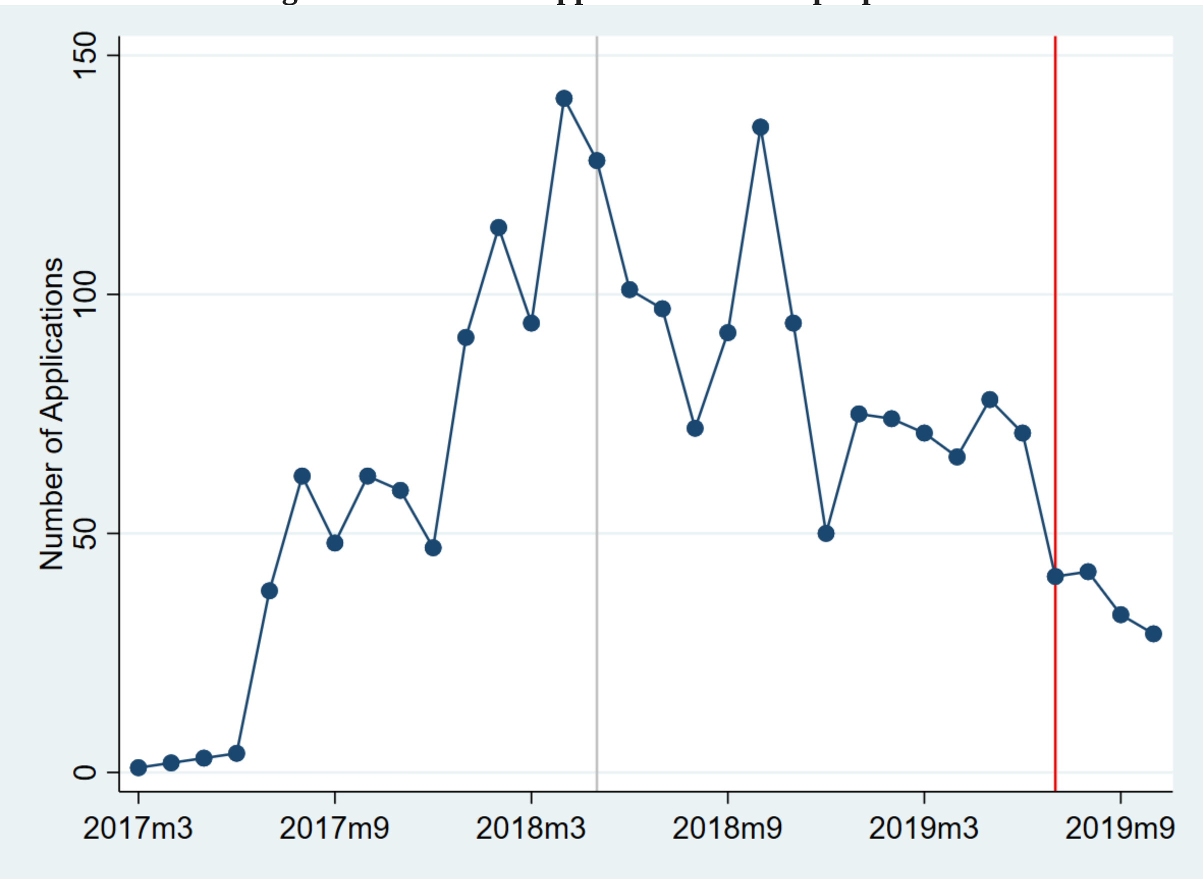
**Figure 1. Selection Funnel**



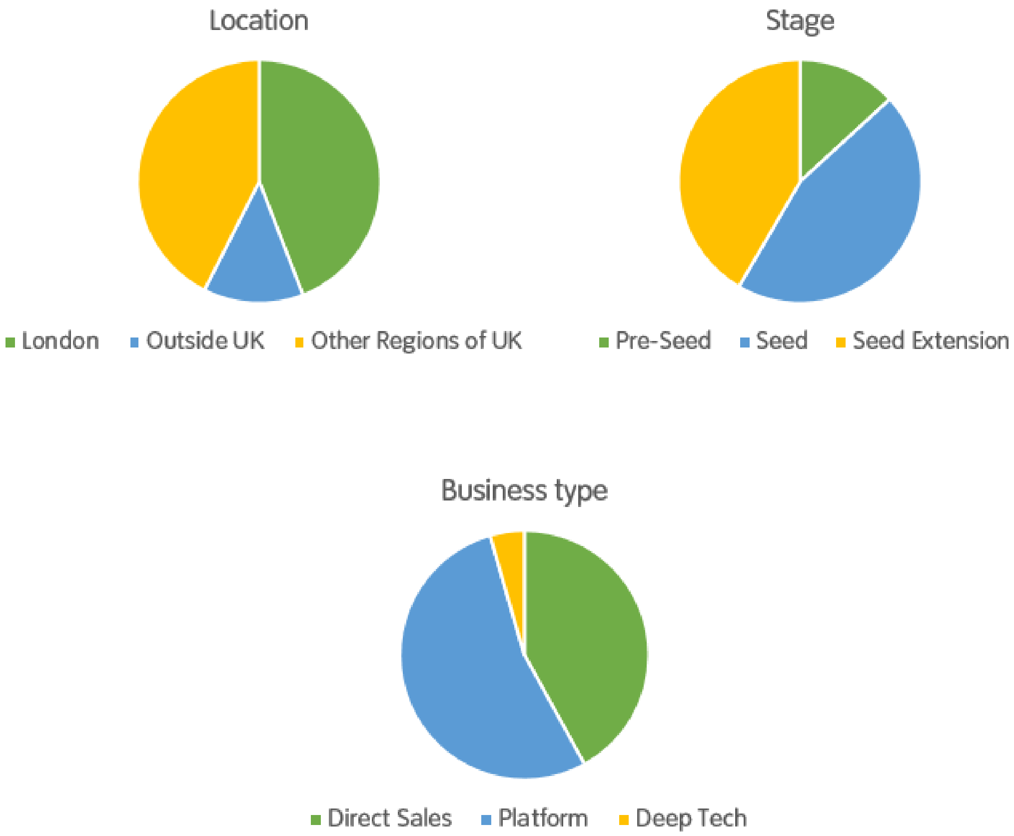

The figure plots the selection funnel of the Fund for the period between March 2017 and June 2019. Opportunity assessment corresponds to the third stage in the due diligence process includes hiring industry experts for external reviews and calling on other parties, including references provided by the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

**Figure 2. Number of applicants over sample period**




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

## Figure 3. Characteristics of 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.

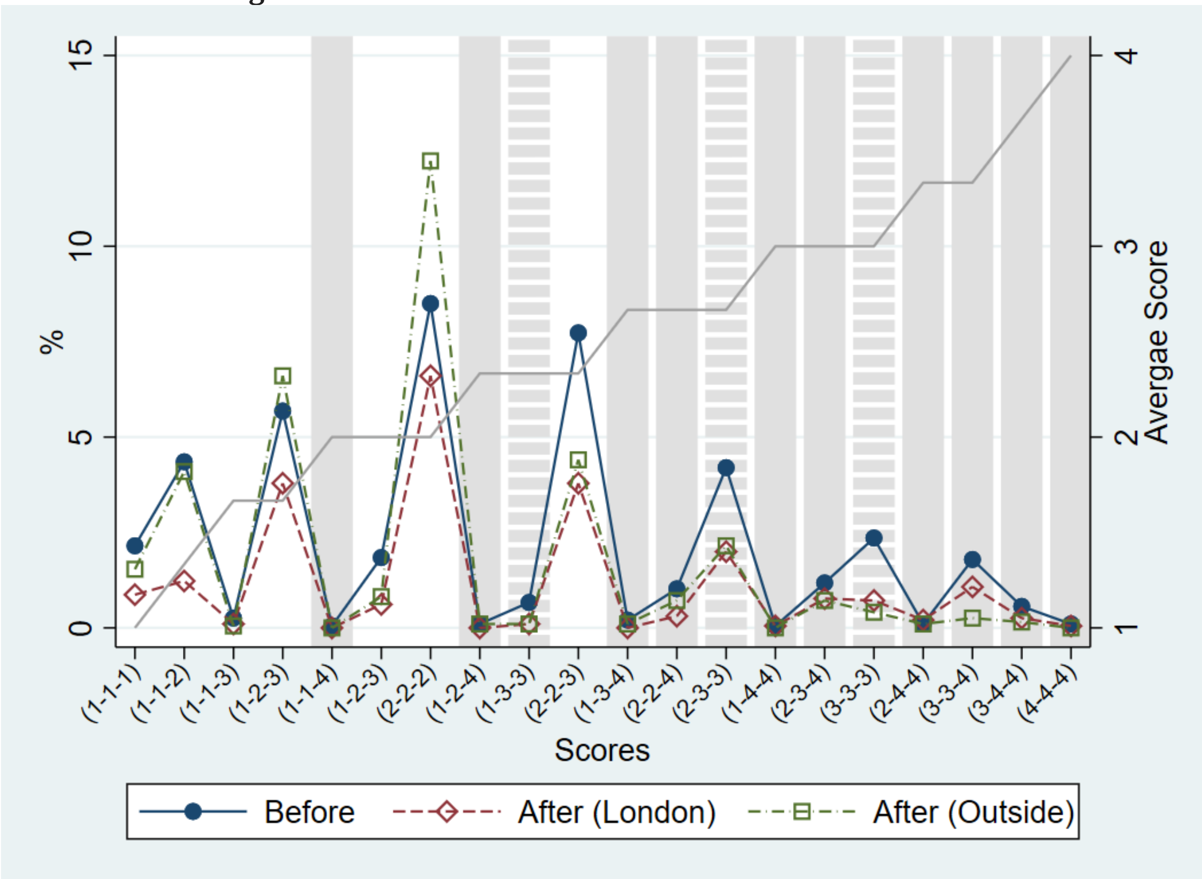
| | Number of Firms | Percent |
|---|---|---|
| | *By Location* | |
| London | 862 | 44.14% |
| Outside UK | 412 | 21.10% |
| Other Regions of UK | 679 | 34.77% |
| | *By Stage* | |
| Pre-Seed (under £100k) | 250 | 12.80% |
| Seed (£100k-1m) | 865 | 44.29% |
| Seed Extension (£200k-2m) | 838 | 42.91% |
| | *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**

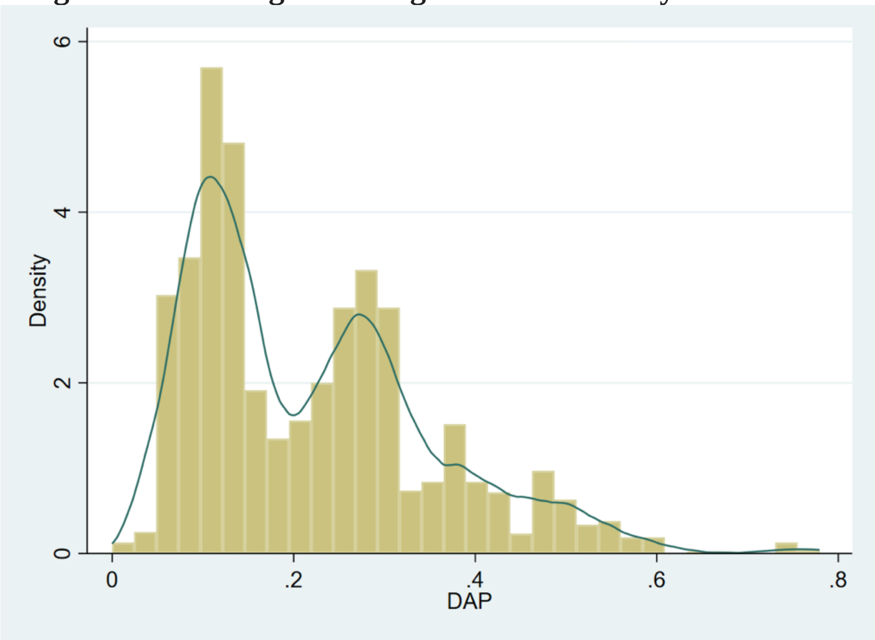



This figure plots the distribution of scores over time and locations. The left axis plots the fraction of scores for each score combination over the different selection regimes. The right axis plots the average score for each score combination; score combinations are sorted by average score. The bars 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) vs. Outside (Before) | 0.132 | 0.001 |
| London (After) vs. Outside (After) | 0.149 | 0.000 |
| London (Before) vs. London (After) | 0.103 | 0.021 |
| Outside (Before) vs. Outside (After) | 0.120 | 0.001 |

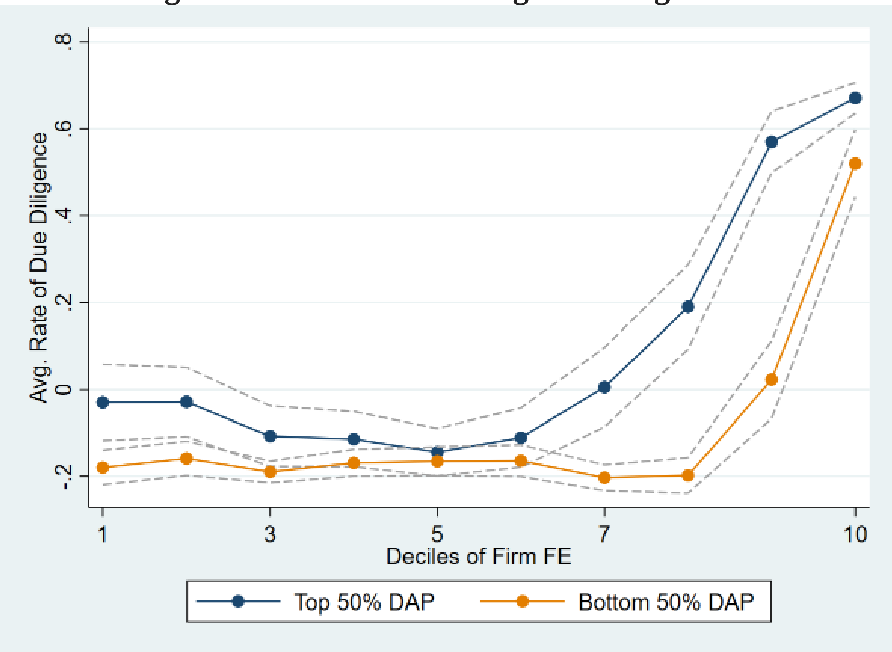
| Individual Score | Two-Sample Kolmogorov-Smirnov Test | |
|---|---|---|
| | Stat. | P Value |
| London (Before) vs. Outside (Before) | 0.089 | 0.000 |
| London (After) vs. Outside (After) | 0.113 | 0.000 |
| London (Before) vs. London (After) | 0.084 | 0.000 |
| Outside (Before) vs. Outside (After)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

**Figure 6. Due Diligence Assignment Probability Distribution**



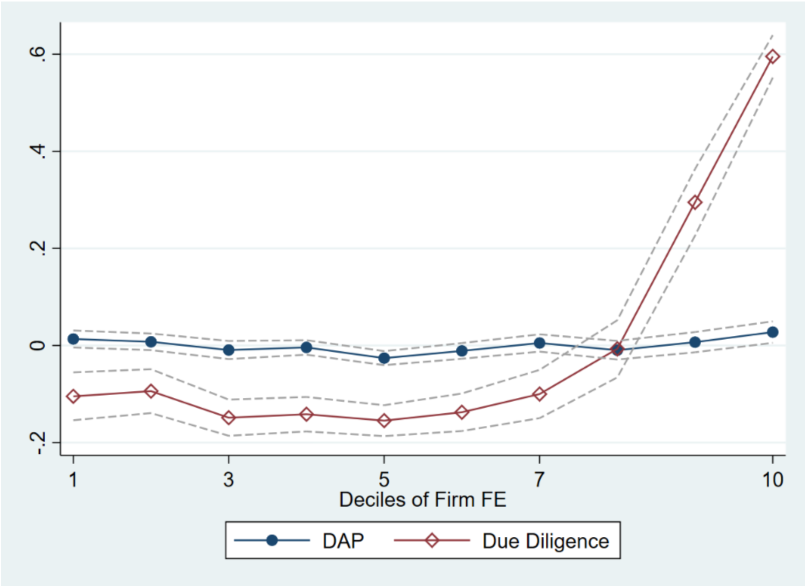

This figure plots the distribution of the Due Diligence Assignment Probability (DAP) across the sample applicants. For more details see Section 2.2.

**Figure 7. DAP and Due Diligence Assignment**




The figure plots the average rate of due diligence assignment (demeaned by region) 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.

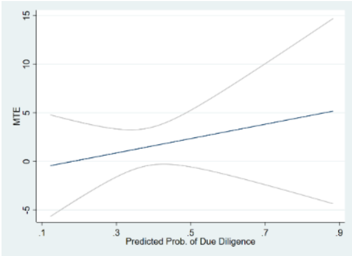
**Figure 8. DAP and Firm Characteristics at Applications**



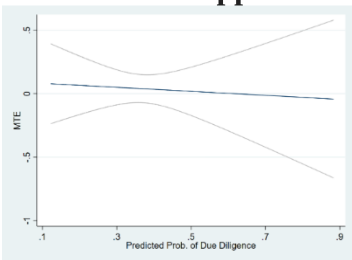

This figure plots the average due diligence assignment (demeaned by region) and DAP against deciles of applicant fixed effects. The applicant fixed effects are estimated in models regressing reviewer scores against full set of applicant and reviewer fixed effects; for more details see Section 1.2 and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

**Figure 9. Marginal Treatment Effects**

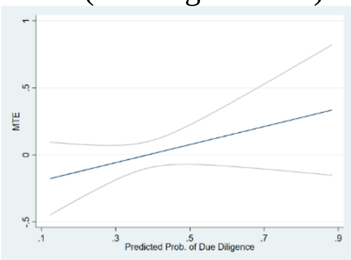
**Panel A -Treatment Curves over Common Support**



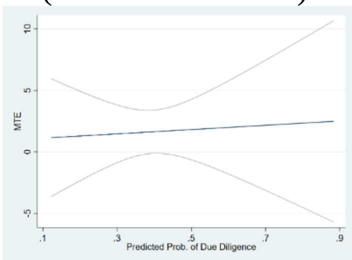

Ln(Funding Amount)




Ln(Number of Rounds)

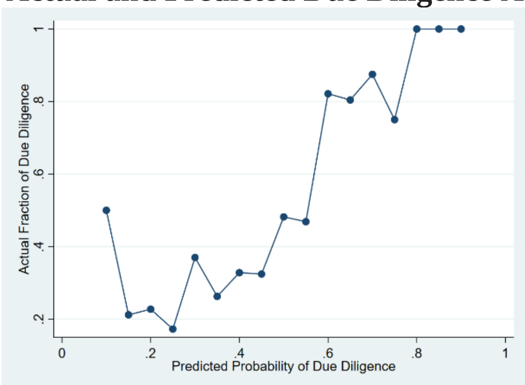



Ln(Number of Investors)




Ln(Issued Equity)

**Panel B – Actual and Predicted Due Diligence Assignment**




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

## Table 1. Summary Statistics

| Source | Variable | Mean | Std. Dev. | p5 | p25 | p50 | p75 | p95 | N |
|---|---|---|---|---|---|---|---|---|---|
| | *Application and Selection* | | | | | | | | |
| Application files | Age Business (since incorporation) | 2.61 | 2.96 | 0.00 | 1.00 | 2.00 | 4.00 | 7.00 | 1,953 |
| | Target Amount (£1000s) | 1,692 | 2,537 | 100 | 365 | 1,000 | 2,000 | 5,500 | 1,950 |
| | Target Close Date (Days) | 80 | 70 | 25 | 48 | 70 | 96 | 165 | 1,946 |
| | Total Addressable Market (£Billion) | 345 | 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 |
| LinkedIn | Female Founder | 0.13 | 0.33 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 | 1,785 |
| | Russell Education Founder | 0.17 | 0.37 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 | 1,953 |
| Fund's Selection | Due diligence(%) | 31.49 | 46.46 | 0.00 | 0.00 | 0.00 | 100.00 | 100.00 | 1,953 |
| | Opportunity assessment(%) | 2.30 | 15.49 | 0.00 | 0.00 | 0.00 | 0.00 | 100.00 | 1,953 |
| | Investment(%) | 0.61 | 7.81 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 1,953 |
| | *Firm Characteristics (All Firms, Web Sources)* | | | | | | | | |
| | *Pre- Application* | | | | | | | | |
| Crunchbase | 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 |
| LinkedIn | Serial Entrepreneur | 0.26 | 0.44 | 0.00 | 0.00 | 0.00 | 1.00 | 1.00 | 1,953 |
| | No. of Ventures Created by the Founder | 0.40 | 0.80 | 0.00 | 0.00 | 0.00 | 1.00 | 2.00 | 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 |

## Table 1 (Continued). Summary Statistics

| Source | Variable | Mean | Std. Dev. | p5 | p25 | p50 | p75 | p95 | N |
|---|---|---|---|---|---|---|---|---|---|
| | *Firm Characteristics (UK Firms, Administrative Data)* | | | | | | | | |
| | *Pre- Application* | | | | | | | | |
| Companies House | Assets (£1000s) | 641 | 15,635 | 0.00 | 0.00 | 23.13 | 167 | 1,044 | 1,548 |
| | ln(1+Assets) | 2.89 | 2.61 | 0.00 | 0.00 | 3.18 | 5.12 | 6.95 | 1,548 |
| | Debt (£1000s) | 610.87 | 16070.38 | 0.00 | 0.00 | 14.10 | 85.00 | 607.75 | 1,548 |
| | ln(1+Debt) | 2.58 | 2.38 | 0.00 | 0.00 | 2.71 | 4.45 | 6.41 | 1,548 |
| | Equity Issuance (£1000s) | 158 | 608 | 0.00 | 0.00 | 0.00 | 83 | 850 | 1,548 |
| | ln(1+Equity Issuance) | 2.39 | 2.70 | 0.00 | 0.00 | 1.10 | 5.12 | 7.44 | 1548 |
| | No. of Years Before App. | 2.67 | 2.67 | 0.00 | 1.00 | 2.00 | 4.00 | 8.00 | 1,548 |
| | *Post-Application* | | | | | | | | |
| Companies House | Assets (£1000s) | 1,066 | 18,470 | 0.00 | 1.00 | 86 | 545 | 3,199 | 1,548 |
| | ln(1+Assets) | 3.94 | 2.85 | 0.00 | 0.69 | 4.46 | 6.30 | 8.07 | 1,548 |
| | Debt (£1000s) | 817.83 | 17259.09 | 0.00 | 1.00 | 58.50 | 245.00 | 1821.33 | 1,548 |
| | ln(1+Debt) | 3.59 | 2.58 | 0.00 | 0.69 | 4.09 | 5.51 | 7.51 | 1,548 |
| | Equity Issuance (£1000s) | 385 | 933 | 0.00 | 0.00 | 0.00 | 255 | 2,387 | 1,548 |
| | ln(1+Equity Issuance) | 3.12 | 3.07 | 0.00 | 1.10 | 1.10 | 6.24 | 8.47 | 1,548 |
| | No. of Directors Appointed | 1.03 | 1.63 | 0.00 | 0.00 | 0.00 | 2 | 4 | 1,548 |
| | Survival | 0.81 | 0.39 | 0.00 | 1.00 | 1.00 | 1.00 | 1.00 | 1,548 |
| | Bankruptcy | 0.04 | 0.19 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 1,548 |
| | No. of Years After App. | 1.93 | 0.64 | 1.00 | 2.00 | 2.00 | 2.00 | 3.00 | 1,548 |
| | *Instrument Variables* | | | | | | | | |
| Constructed | DAP | 0.22 | 0.13 | 0.06 | 0.11 | 0.19 | 0.30 | 0.48 | 1,953 |
| | Regional DAP | 0.25 | 0.21 | 0.01 | 0.08 | 0.18 | 0.37 | 0.67 | 1,953 |

The table presents summary statistics of the variables used in the analysis. The variables are organized by source and time period as indicated by the first and second column of the table. The sample includes all 1,953 applicants to the Fund that were evaluated by the reviewers. Only a subsample of these 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) |
|---|---|---|---|---|---|---|---|---|
| | | | | Panel A-OLS | | | | |
| DAP | 1.09*** | 1.33*** | 1.09*** | 1.32*** | 0.94*** | 1.19*** | 0.93*** | 1.19*** |
| | (0.08) | (0.07) | (0.08) | (0.07) | (0.07) | (0.06) | (0.07) | (0.06) |
| Firm FE | | | | | 0.35*** | 0.37*** | 0.34*** | 0.37*** |
| | | | | | (0.02) | (0.01) | (0.02) | (0.02) |
| F-test of excl. IV | 185.64 | 361.00 | 185.64 | 355.59 | 180.33 | 393.36 | 176.51 | 393.36 |
| Controls | | Yes | | Yes | | Yes | | Yes |
| Obs. | 1953 | 1953 | 1941 | 1941 | 1953 | 1953 | 1941 | 1941 |
| R-sq | 0.0981 | 0.3589 | 0.0976 | 0.3618 | 0.0551 | 0.2916 | 0.2679 | 0.5390 |
| | | | | Panel B-Probit | | | | |
| DAP | 3.09*** | 4.53*** | 3.08*** | 4.52*** | 3.14*** | 6.09*** | 3.12*** | 6.07*** |
| | (0.23) | (0.28) | (0.23) | (0.28) | (0.26) | (0.37) | (0.26) | (0.37) |
| Firm FE | | | | | 1.17*** | 1.89*** | 1.15*** | 1.87*** |
| | | | | | (0.07) | (0.10) | (0.07) | (0.10) |
| F-test of excl. IV | 180.49 | 261.75 | 179.33 | 260.59 | 145.85 | 270.91 | 144.00 | 269.14 |
| Controls | | Yes | | Yes | | Yes | | Yes |
| Obs. | 1953 | 1953 | 1941 | 1941 | 1953 | 1953 | 1941 | 1941 |
| Pseudo R-sq | 0.08 | 0.32 | 0.08 | 0.33 | 0.23 | 0.55 | 0.23 | 0.55 |

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

**Table 3—Balance of Covariates Across DAP Quartiles**

| Variable | Q1 | Other Q | *p-value diff. in mean* | Q2 | Other Q | *p-value diff. in mean* | Q3 | Other Q | *p-value diff. in mean* | Q4 | Other Q | *p-value diff. in mean* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| App. Info | | | | | | | | | | | | |
| Age | 2.30 | 2.71 | *0.00* | 2.42 | 2.67 | *0.85* | 2.73 | 2.57 | *0.43* | 2.98 | 2.48 | *0.02* |
| ln(Age) | 0.98 | 1.08 | *0.00* | 1.01 | 1.07 | *0.42* | 1.09 | 1.05 | *0.58* | 1.14 | 1.03 | *0.06* |
| Female Founder | 0.14 | 0.12 | *0.62* | 0.12 | 0.13 | *0.11* | 0.14 | 0.13 | *0.91* | 0.11 | 0.13 | *0.22* |
| Russell Education of Founder | 0.14 | 0.18 | *0.46* | 0.17 | 0.17 | *0.99* | 0.17 | 0.17 | *0.93* | 0.20 | 0.16 | *0.41* |
| Amount | 1690.78 | 2416.15 | *0.33* | 1606.34 | 2444.41 | *0.78* | 3948.15 | 1663.73 | *0.07* | 1694.31 | 2413.00 | *0.58* |
| ln(Amount) | 6.72 | 6.60 | *0.18* | 6.58 | 6.65 | *0.42* | 6.67 | 6.62 | *0.67* | 6.55 | 6.66 | *0.92* |
| Target Close Days | 83.18 | 80.05 | *0.50* | 81.83 | 80.50 | *0.40* | 78.93 | 81.47 | *0.79* | 79.37 | 81.32 | *0.67* |
| ln(Target Close Days) | 4.23 | 4.22 | *0.29* | 4.24 | 4.22 | *0.26* | 4.22 | 4.22 | *0.18* | 4.19 | 4.23 | *0.19* |
| Total Addressable Market | 152.47 | 862.41 | *0.67* | 3269.07 | 39.36 | *0.08* | 3.91 | 1011.05 | *0.53* | 4.62 | 1073.79 | *0.50* |
| ln(Total Addressable Market) | 0.62 | 0.42 | *0.64* | 0.53 | 0.44 | *0.08* | 0.40 | 0.48 | *0.73* | 0.35 | 0.51 | *0.08* |
| Total Servicable Market | 247.10 | 6.77 | *0.39* | 9.03 | 63.04 | *0.69* | 11.33 | 67.00 | *0.11* | 1.30 | 75.24 | *0.67* |
| ln(Total Servicable Market) | 0.31 | 0.18 | *0.09* | 0.21 | 0.21 | *0.14* | 0.21 | 0.21 | *0.06* | 0.15 | 0.24 | *0.09* |
| London | 0.41 | 0.45 | | 0.40 | 0.45 | | 0.46 | 0.43 | | 0.50 | 0.42 | |
| Seed/Pre-Seed | 0.47 | 0.44 | *0.25* | 0.46 | 0.44 | *0.57* | 0.42 | 0.45 | *0.94* | 0.42 | 0.45 | *0.10* |
| Platform | 0.46 | 0.55 | *0.03* | 0.53 | 0.53 | *0.31* | 0.56 | 0.52 | *0.38* | 0.56 | 0.52 | *0.12* |
| Deep Tech | 0.03 | 0.05 | *0.10* | 0.02 | 0.05 | *0.95* | 0.04 | 0.04 | *0.47* | 0.07 | 0.03 | *0.08* |
| CH Info. Before App. | | | | | | | | | | | | |
| Asset (£1000s) | 217.48 | 767.68 | *0.51* | 194.55 | 785.47 | *0.57* | 1808.90 | 247.39 | *0.06* | 320.16 | 760.96 | *0.51* |
| Debt (£1000s) | 2.78 | 2.93 | *0.06* | 2.75 | 2.94 | *0.74* | 2.89 | 2.89 | *0.32* | 3.12 | 2.81 | *0.55* |
| Annual Equity Issuance (£1000s) | 177.83 | 740.68 | *0.52* | 105.08 | 774.86 | *0.54* | 1849.26 | 193.80 | *0.05* | 287.00 | 732.26 | *0.50* |
| ln(1+Debt) | 2.59 | 2.58 | *0.08* | 2.41 | 2.64 | *0.54* | 2.58 | 2.59 | *0.35* | 2.74 | 2.52 | *0.82* |
| Equity Issuance (£1000s) | 289.17 | 325.19 | *0.15* | 284.61 | 327.35 | *0.72* | 338.83 | 309.49 | *0.11* | 349.03 | 304.83 | *0.62* |
| ln(1+Equity Issuance) | 2.25 | 2.43 | *0.19* | 2.34 | 2.40 | *0.54* | 2.37 | 2.39 | *0.69* | 2.55 | 2.32 | *0.75* |
| Web Info. Before App. | | | | | | | | | | | | |
| Num. of Funding Rounds | 1.09 | 1.22 | *0.09* | 1.22 | 1.18 | *0.43* | 1.19 | 1.18 | *0.87* | 1.24 | 1.17 | *0.45* |
| ln(1+Num. of Funding Rounds) | 0.70 | 0.75 | *0.16* | 0.74 | 0.73 | *0.91* | 0.74 | 0.73 | *0.59* | 0.76 | 0.73 | *0.45* |
| Total Funding (£1000s) | 280.42 | 437.74 | *0.57* | 368.39 | 408.36 | *0.94* | 356.07 | 412.44 | *0.52* | 589.22 | 334.94 | *0.20* |
| ln(1+Total Funding) | 2.50 | 3.04 | *0.39* | 2.75 | 2.95 | *0.78* | 2.96 | 2.88 | *0.45* | 3.40 | 2.74 | *0.18* |
| Num. of Firms Created | 0.40 | 0.40 | *0.36* | 0.41 | 0.40 | *0.92* | 0.37 | 0.41 | *0.96* | 0.42 | 0.39 | *0.28* |
| ln(1+Num. of Firms Created) | 0.24 | 0.23 | *0.20* | 0.23 | 0.23 | *0.99* | 0.21 | 0.24 | *0.83* | 0.24 | 0.23 | *0.28* |

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

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

**Table 4—Due Diligence Assignment and Fundraising**

**Panel A—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 | 2.94*** | 2.81*** | 0.20*** | 0.18** | 0.10*** | 0.09* | 1.18*** | 1.21** |
| | (0.36) | (0.85) | (0.02) | (0.06) | (0.02) | (0.04) | (0.18) | (0.43) |
| N | 1953 | 1953 | 1953 | 1953 | 1953 | 1953 | 1548 | 1548 |
| R-sq | 0.1313 | 0.1039 | 0.1457 | 0.1156 | 0.0704 | 0.0415 | 0.1053 | 0.0709 |
| F Stat. | 401.49 | | 401.49 | | 401.49 | | 355.83 | |

**Panel B—Excluding Portfolio companies**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | ln(Funding) | | ln(#Number of Rounds) | | ln(#Investors) | | ln(Equity Issuance) (UK) | |
| | OLS | IV | OLS | IV | OLS | IV | OLS | IV |
| Due diligence | 2.86*** | 2.74** | 0.19*** | 0.18** | 0.10*** | 0.09* | 1.13*** | 1.11* |
| | (0.37) | (0.86) | (0.02) | (0.06) | (0.02) | (0.04) | (0.18) | (0.44) |
| N | 1941 | 1941 | 1941 | 1941 | 1941 | 1941 | 1537 | 1537 |
| R-sq | 0.1298 | 0.1032 | 0.1419 | 0.1120 | 0.0689 | 0.0405 | 0.1031 | 0.0694 |
| F Stat. | 397.38 | | 397.38 | | 397.38 | | 352.01 | |
| Reference: | | | | | | | | |
| 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. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

## Table 5 –Due Diligence and Economic Growth

### Panel A—Full sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Ln (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 | 0.51*** | 0.46** | 0.54*** | 0.93** | 0.56*** | 1.16*** | 0.22*** | 0.30*** | 0.07** | -0.11 |
| | (0.07) | (0.16) | (0.15) | (0.34) | (0.13) | (0.29) | (0.04) | (0.08) | (0.02) | (0.06) |
| N | 1953 | 1953 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 |
| R-sq | 0.1629 | 0.1382 | 0.0846 | 0.0662 | 0.0656 | 0.0350 | 0.0555 | 0.0319 | 0.0495 | -0.0042 |
| F Stat. | 401.49 | | 355.83 | | 355.83 | | 355.83 | | 355.83 | |

### Panel B—Excluding Portfolio companies

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Ln (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 | 0.50*** | 0.44** | 0.50*** | 0.89* | 0.54*** | 1.12*** | 0.21*** | 0.29*** | 0.07** | -0.11* |
| | (0.07) | (0.16) | (0.15) | (0.34) | (0.13) | (0.29) | (0.04) | (0.09) | (0.02) | (0.06) |
| N | 1941 | 1941 | 1537 | 1537 | 1537 | 1537 | 1537 | 1537 | 1537 | 1537 |
| R-sq | 0.1598 | 0.1357 | 0.0821 | 0.0636 | 0.0643 | 0.0346 | 0.0549 | 0.0309 | 0.0489 | -0.0062 |
| F Stat. | 397.38 | | 352.01 | | 352.01 | | 352.01 | | 352.01 | |
| Reference: | | | | | | | | | | |
| 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) |

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

**Table 6-Due Diligence and Economic Growth for Non-portfolio Firms: 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 | 1.77*** | 7.73*** | 0.10*** | 0.32** | 0.05** | 0.09 | 0.07 | 3.11** |
| | (0.40) | (1.96) | (0.02) | (0.11) | (0.02) | (0.08) | (0.22) | (1.17) |
| Due diligence*London | 1.35* | -6.29** | 0.13** | -0.16 | 0.06* | 0.02 | 1.55*** | -2.02 |
| | (0.65) | (2.29) | (0.04) | (0.14) | (0.03) | (0.09) | (0.33) | (1.32) |
| London | 1.25*** | 3.69*** | 0.08*** | 0.17*** | 0.06*** | 0.07* | 0.17 | 1.52** |
| | (0.33) | (0.78) | (0.02) | (0.04) | (0.01) | (0.03) | (0.18) | (0.51) |
| N | 1941 | 1941 | 1941 | 1941 | 1941 | 1941 | 1537 | 1537 |
| R-sq | 0.1166 | 0.0083 | 0.1307 | 0.0885 | 0.0530 | 0.0497 | 0.0816 | -0.0332 |
| F Stat. | | 27.49 | | 27.49 | | 27.49 | | 17.68 |

**Panel B—Founders' Educational Background**

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

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

## Table 7. Due Diligence and Economic Growth: sample cuts

### Panel A—Location

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

### Panel B—Founders' Educational Background

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

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

**Table 8. Informal Meetings and Fundraising**

**Panel A 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 | 3.07*** | -2.68 | 0.16*** | -0.13 | 0.12*** | -0.07 | 1.28*** | 8.70 |
| | (0.47) | (5.01) | (0.02) | (0.29) | (0.02) | (0.21) | (0.28) | (4.48) |
| N | 1338 | 1338 | 1338 | 1338 | 1338 | 1338 | 1025 | 1025 |
| R-sq | 0.0978 | 0.0303 | 0.1079 | 0.0459 | 0.0554 | 0.0001 | 0.0888 | -0.2286 |
| F Stat. | | 21.29 | | 21.29 | | 21.29 | | 11.02 |
| Reference: | | | | | | | | |
| 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 Economic Growth**

| | (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 |
| Informal Meeting | 0.38** | 1.17 | 0.45 | -2.21 | 0.28 | -0.20 | 0.11 | 0.03 | 0.12 | -0.22 |
| | (0.14) | (1.50) | (0.27) | (3.14) | (0.26) | (2.85) | (0.08) | (0.83) | (0.07) | (0.59) |
| N | 1338 | 1338 | 1025 | 1025 | 1025 | 1025 | 1025 | 1025 | 1025 | 1025 |
| R-sq | 0.1630 | 0.1030 | 0.0769 | 0.0092 | 0.0557 | 0.0435 | 0.0398 | 0.0185 | 0.0516 | 0.0020 |
| | | 21.29 | | 11.02 | | 11.02 | | 11.02 | | 11.02 |
| Reference: | | | | | | | | | | |
| 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) |

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

## ONLINE APPENDIX

**Appendix 1—Email templates**

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

*Due diligence email template*:

Hi ,

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

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

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

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

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

**The first reviewer's feedback is here;**

**The second reviewer's feedback is here;**

**The third reviewer's feedback is here;**

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

---------------------------------

*Informal Meeting email template:*

Hi ,

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

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

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

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

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

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

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

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

Best regards,

---

*No 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—Randomization Checks

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

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

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

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

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

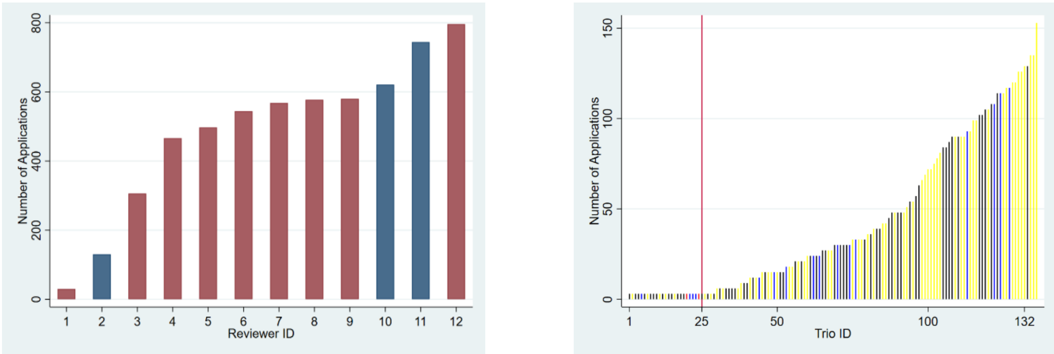
We provide evidence to support the assertion that the assignment of applications to reviewers is random conditional on the location of the applicant.

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

Table A31 below reports the F-tests and p-values of the reviewer fixed effects across the different business and applicant characteristics. We reject the equality of the reviewer fixed effects for all variables. The only exception is the location variable, where we reject of equality of reviewer fixed effects when we use as dependent variable an indicator variable for businesses in London.

## Figure A31—Distribution of Applications across Reviewers and Trios

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




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

## Table A31 Regional Composition of Each Reviewer's Assessment Samples

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

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

## Table A32—Randomization Checks across Business and Founder Characteristics

| Dependent Variable | Obs. | Reviewer F.E. | | Reviewer F.E. Conditional on Speciality of Region | | Reviewer F.E. Conditional on Region | |
|---|---|---|---|---|---|---|---|
| | | F Stat. | *p-Value* | F Stat. | *p-Value* | F Stat. | *p-Value* |
| Age | 5837 | 1.646 | (0.079) | 1.618 | (0.087) | 1.291 | (0.222) |
| ln(Age) | 5837 | 1.284 | (0.227) | 1.252 | (0.246) | 1.025 | (0.421) |
| Female Founder | 5340 | 0.966 | (0.475) | 0.946 | (0.494) | 0.667 | (0.771) |
| Russell Education of Founder | 5837 | 1.058 | (0.391) | 0.839 | (0.601) | 0.432 | (0.942) |
| Amount | 4872 | 0.585 | (0.843) | 0.580 | (0.847) | 0.643 | (0.793) |
| ln(Amount) | 4872 | 0.389 | (0.961) | 0.367 | (0.969) | 0.377 | (0.965) |
| Target Close Days | 4881 | 1.031 | (0.416) | 1.010 | (0.434) | 0.962 | (0.479) |
| ln(Target Close Days) | 4869 | 1.272 | (0.234) | 1.250 | (0.248) | 1.153 | (0.315) |
| Total Addressable Market | 4285 | 0.566 | (0.858) | 0.563 | (0.86) | 0.517 | (0.893) |
| ln(Total Addressable Market) | 4285 | 2.095 | (0.018) | 2.039 | (0.022) | 1.678 | (0.0719) |
| Total Servicable Market | 4285 | 1.053 | (0.396) | 1.037 | (0.411) | 1.043 | (0.405) |
| ln(Total Servicable Market) | 4285 | 0.780 | (0.660) | 0.740 | (0.701) | 0.606 | (0.826) |
| Seed/Pre-Seed | 5837 | 1.258 | (0.242) | 1.260 | (0.241) | 1.081 | (0.372) |
| Deep Tech | 5837 | 1.719 | (0.063) | 1.699 | (0.067) | 1.261 | (0.241) |
| Platform | 5837 | 2.301 | (0.008) | 2.287 | (0.009) | 1.380 | (0.175) |
| London | 5837 | 9.883 | (0.000) | | | | |
| London (Reviewers Assigned by Region Rules) | 3491 | 20.510 | (0.000) | | | | |
| London (Reviewers Assigned without Region Rules) | 2346 | 1.389 | (0.225) | | | | |
| Financial Status Before App. | | | | | | | |
| Asset (£1000s) | 4625 | 0.756 | 0.685 | 0.754 | 0.687 | 0.712 | 0.729 |
| ln(1+Asset) | 4625 | 1.147 | 0.319 | 1.148 | 0.319 | 1.014 | 0.431 |
| Debt (£1000s) | 4625 | 0.736 | 0.704 | 0.734 | 0.706 | 0.693 | 0.746 |
| ln(1+Debt) | 4625 | 0.839 | 0.601 | 0.856 | 0.584 | 0.840 | 0.600 |
| Equity Issuance (£1000s) | 4625 | 0.918 | 0.522 | 0.918 | 0.522 | 0.877 | 0.563 |
| ln(1+Equity Issuance) | 5837 | 0.653 | 0.784 | 0.653 | 0.784 | 0.668 | 0.770 |
| Num. of Funding Rounds | 5837 | 0.932 | 0.508 | 0.911 | 0.528 | 0.743 | 0.697 |
| ln(1+Num. of Funding Rounds) | 5837 | 0.809 | 0.631 | 0.773 | 0.668 | 0.579 | 0.847 |
| Total Funding (£1000s) | 5837 | 0.609 | 0.823 | 0.603 | 0.828 | 0.509 | 0.898 |
| ln(1+Total Funding) | 5837 | 0.630 | 0.804 | 0.635 | 0.801 | 0.578 | 0.848 |
| Num. of Firms Created | 5837 | 0.965 | 0.476 | 1.026 | 0.420 | 0.907 | 0.533 |
| ln(1+Num. of Firms Created) | 5837 | 0.957 | 0.484 | 0.996 | 0.447 | 0.874 | 0.565 |
| Serial Entrepreneur | 5837 | 0.817 | 0.623 | 0.832 | 0.608 | 0.744 | 0.697 |

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

## Appendix 4—Reviewer Heterogeneity in Scores

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

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

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

where $Score_{i,h}$ denotes the score assigned by reviewer $h$ to company $i$; $\mu_h$ and $\alpha_i$ 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.

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

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

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

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

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

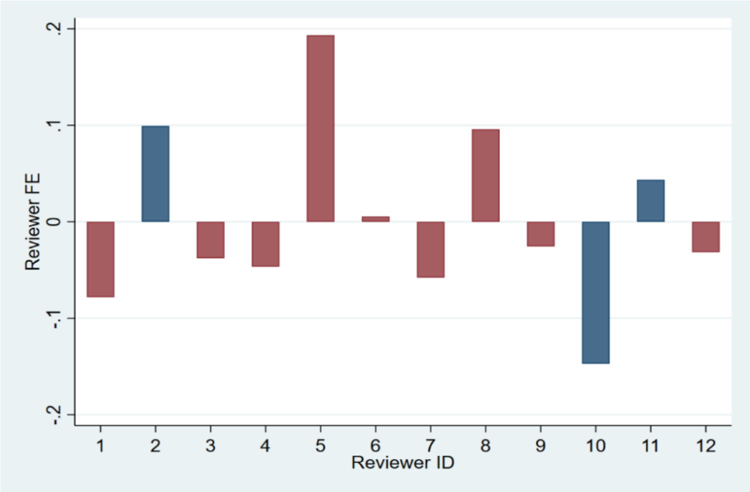
The second finding is the sizable *economic* significance of the scoring generosity heterogeneity. Figure A44 shows that generous reviewers (with positive FE) are twice as likely to assign a score of "3" or "4" than stricter reviewers with negative FE across all firm fixed effects deciles. On average,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

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

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

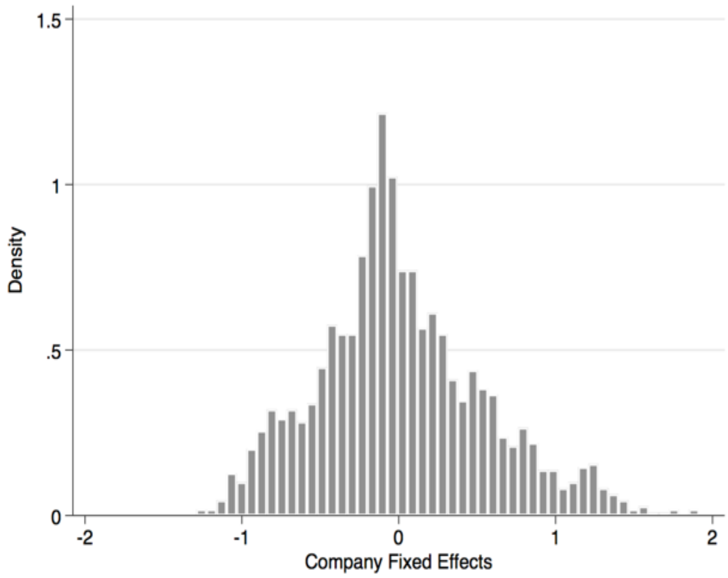
[3] We make sure the reviewer was assigned at least one application to review within 3 months of the firm's application date.

**Figure A41—Distribution of Reviewer Fixed Effects**



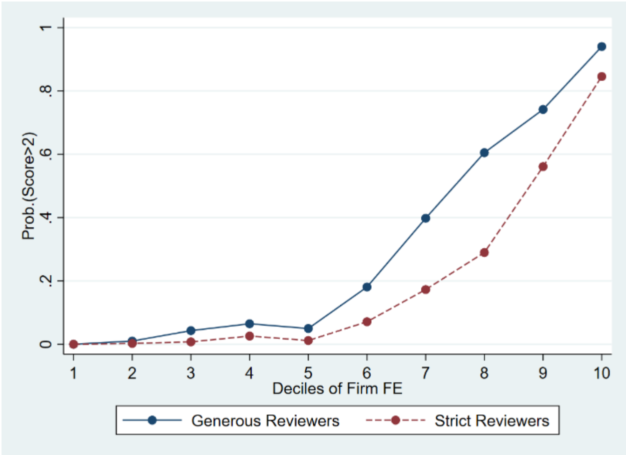

The figure plots the reviewer fixed effects for each reviewer in the sample based on the estimates of equation A41. Blue columns indicate female reviewers.

**Figure A42—Distribution of Applicant Fixed Effects**




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

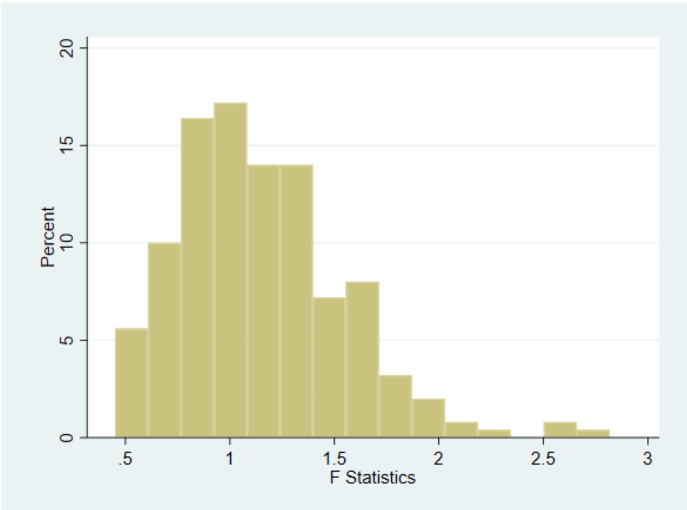
**Figure A43—Frequency of Scores Above 2 and Reviewer FE**



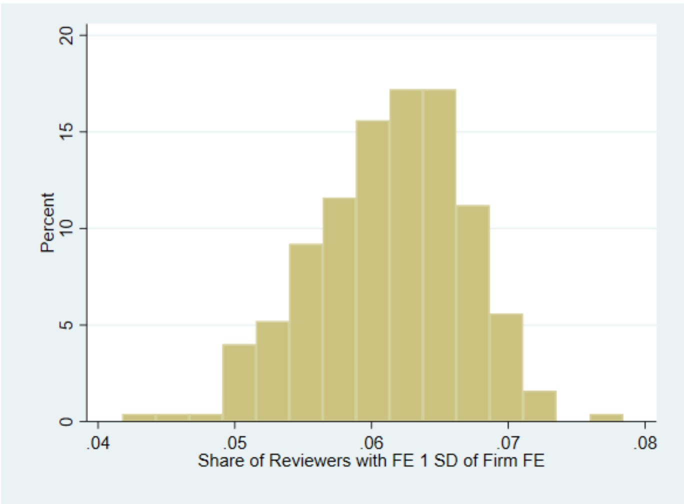

The figure plots the probability of a score higher than 2, separately for reviewers with positive and negative fixed effects (from Eq. A41).

**Figure A44—Placebo Tests Reviewer Fixed Effects**

**Panel A— Distribution of *F*-values**

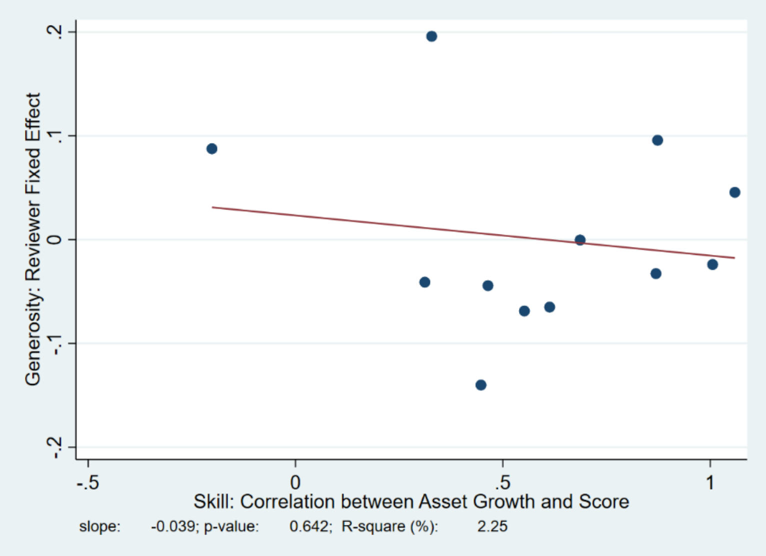



**Panel B— Fixed Effects One Standard Deviation Above/Below Applicant Effect**



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

**Figure A45—Reviewer Fixed Effects and Ranking Ability of Reviewers**




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

## Table A41—Balance of Covariates Across Generosity Quartiles

| Variable | Q1 | Other Q | *p-value diff. in mean* | Q2 | Other Q | *p-value diff. in mean* | Q3 | Other Q | *p-value diff. in mean* | Q4 | Other Q | *p-value diff. in mean* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| App. Info | | | | | | | | | | | | |
| Age | 2.61 | 2.61 | *0.51* | 2.49 | 2.65 | *0.22* | 2.55 | 2.63 | *0.95* | 2.85 | 2.55 | *0.03* |
| ln(Age) | 1.05 | 1.06 | *0.44* | 1.03 | 1.06 | *0.64* | 1.05 | 1.06 | *0.70* | 1.10 | 1.04 | *0.07* |
| Female Founder | 0.12 | 0.13 | *0.91* | 0.13 | 0.13 | *0.86* | 0.14 | 0.12 | *0.30* | 0.11 | 0.13 | *0.19* |
| Russell Education of Founder | 0.15 | 0.17 | *0.31* | 0.15 | 0.17 | *0.22* | 0.18 | 0.16 | *0.12* | 0.18 | 0.16 | *0.52* |
| Amount | 2542.83 | 2153.36 | *0.57* | 1728.48 | 2422.04 | *0.27* | 2210.44 | 2245.20 | *0.96* | 2623.39 | 2132.98 | *0.49* |
| ln(Amount) | 6.58 | 6.64 | *0.26* | 6.67 | 6.62 | *0.71* | 6.63 | 6.63 | *0.68* | 6.64 | 6.63 | *0.73* |
| Target Close Days | 82.08 | 80.51 | *0.92* | 82.16 | 80.34 | *0.37* | 80.30 | 81.08 | *0.63* | 78.67 | 81.40 | *0.58* |
| ln(Target Close Days) | 4.23 | 4.22 | *0.80* | 4.23 | 4.22 | *0.63* | 4.22 | 4.22 | *0.45* | 4.20 | 4.23 | *0.25* |
| Total Addressable Market | 1147.71 | 618.44 | *0.61* | 942.66 | 655.37 | *0.72* | 807.58 | 697.59 | *0.94* | 6.54 | 946.75 | *0.32* |
| ln(Total Addressable Market) | 0.46 | 0.45 | *0.87* | 0.48 | 0.45 | *0.33* | 0.46 | 0.45 | *0.97* | 0.42 | 0.47 | *0.20* |
| Total Servicable Market | 78.78 | 44.17 | *0.08* | 63.96 | 47.08 | *0.19* | 56.63 | 49.30 | *0.86* | 5.63 | 65.20 | *0.56* |
| ln(Total Servicable Market) | 0.24 | 0.20 | *0.10* | 0.22 | 0.20 | *0.80* | 0.19 | 0.21 | *0.62* | 0.18 | 0.22 | *0.37* |
| London | 0.43 | 0.44 | | 0.41 | 0.45 | | 0.47 | 0.43 | | 0.46 | 0.44 | |
| Seed/Pre-Seed | 0.45 | 0.44 | *0.27* | 0.45 | 0.44 | *0.95* | 0.44 | 0.44 | *0.49* | 0.43 | 0.45 | *0.75* |
| Platform | 0.51 | 0.53 | *0.10* | 0.51 | 0.53 | *0.84* | 0.55 | 0.52 | *0.08* | 0.53 | 0.52 | *0.95* |
| Deep Tech | 0.03 | 0.05 | *0.08* | 0.04 | 0.04 | *0.91* | 0.04 | 0.04 | *0.44* | 0.06 | 0.04 | *0.07* |
| <u>CH Info. Before App.</u> | | | | | | | | | | | | |
| Asset (£1000s) | 240.57 | 744.68 | *0.40* | 1220.37 | 434.29 | *0.11* | 667.45 | 628.52 | *0.99* | 278.42 | 740.50 | *0.39* |
| Debt (£1000s) | 2.81 | 2.92 | *0.26* | 2.90 | 2.89 | *0.63* | 2.87 | 2.91 | *0.58* | 3.01 | 2.86 | *0.19* |
| Annual Equity Issuance (£1000s) | 230.17 | 709.69 | *0.43* | 1175.80 | 409.59 | *0.13* | 643.19 | 595.98 | *0.96* | 239.40 | 713.07 | *0.38* |
| ln(1+Debt) | 2.53 | 2.60 | *0.38* | 2.59 | 2.58 | *0.54* | 2.54 | 2.60 | *0.51* | 2.69 | 2.56 | *0.30* |
| Equity Issuance (£1000s) | 254.53 | 333.07 | *0.15* | 353.71 | 303.76 | *0.18* | 320.74 | 315.11 | *0.87* | 325.95 | 314.39 | *0.81* |
| ln(1+Equity Issuance) | 2.30 | 2.41 | *0.36* | 2.39 | 2.39 | *0.56* | 2.37 | 2.39 | *0.50* | 2.49 | 2.36 | *0.27* |
| <u>Web Info. Before App.</u> | | | | | | | | | | | | |
| Num. of Funding Rounds | 1.13 | 1.20 | *0.08* | 1.18 | 1.19 | *0.98* | 1.20 | 1.18 | *0.57* | 1.22 | 1.18 | *0.20* |
| ln(1+Num. of Funding Rounds) | 0.72 | 0.74 | *0.12* | 0.73 | 0.74 | *0.68* | 0.74 | 0.73 | *0.50* | 0.75 | 0.73 | *0.17* |
| Total Funding (£1000s) | 381.68 | 402.71 | *0.85* | 400.61 | 397.51 | *0.99* | 367.47 | 412.53 | *0.44* | 459.00 | 382.50 | *0.49* |
| ln(1+Total Funding) | 2.72 | 2.95 | *0.36* | 2.92 | 2.90 | *0.67* | 2.84 | 2.93 | *0.68* | 3.16 | 2.84 | *0.31* |
| Num. of Firms Created | 0.38 | 0.41 | *0.23* | 0.38 | 0.41 | *0.42* | 0.42 | 0.39 | *0.29* | 0.41 | 0.40 | *0.32* |
| ln(1+Num. of Firms Created) | 0.21 | 0.24 | *0.08* | 0.22 | 0.23 | *0.65* | 0.25 | 0.22 | *0.22* | 0.24 | 0.23 | *0.32* |

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

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

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

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

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

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

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

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

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

We run the following type of regression:

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

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

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

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

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

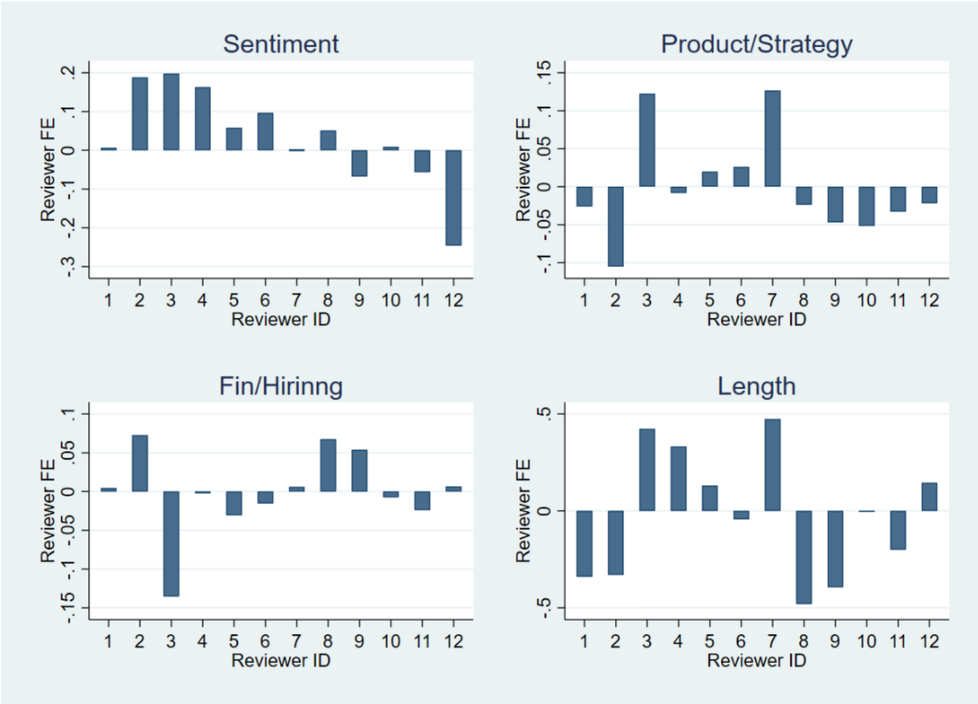
Figure A51 plots the distribution of fixed effects across reviewers. Figure A52 plots the distribution of applicant fixed effects.

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

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

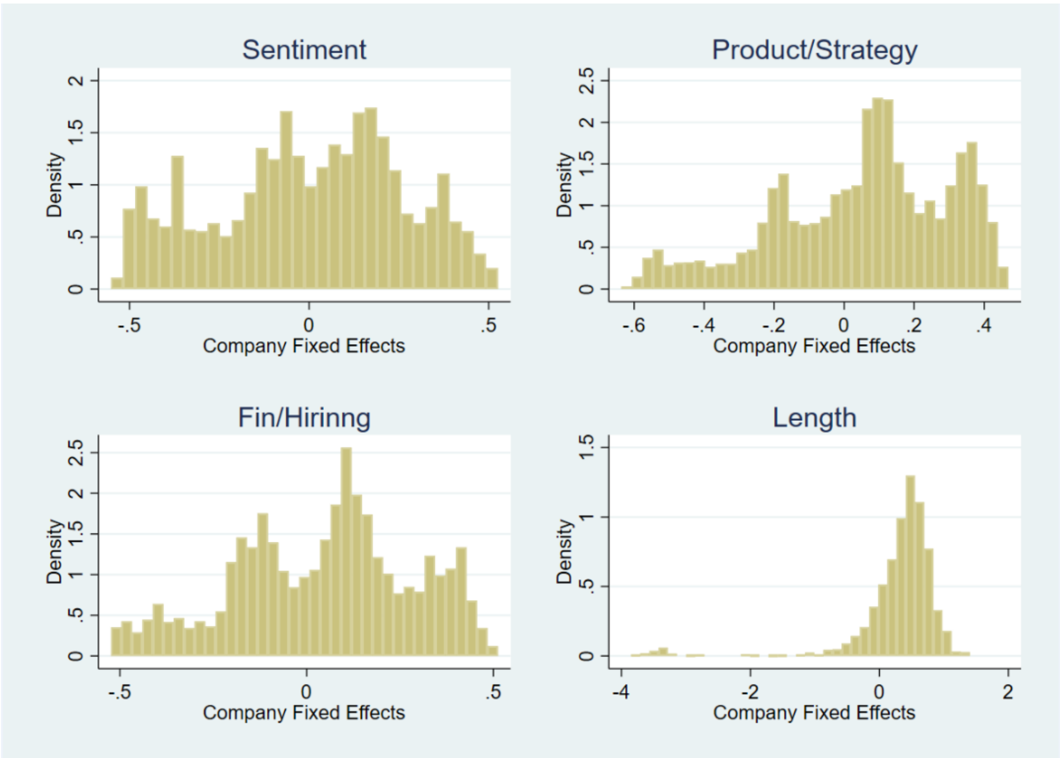
[5] In unreported analysis, we condition on scores to investigate whether comments vary across reviewers for a given score. We expand equation A51 to include reviewer-score fixed effects. We find evidence of heterogeneity conditional on score: the F-test on the joint significance of the reviewer-score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

**Figure A51 – Distribution of Reviewer Fixed Effects**



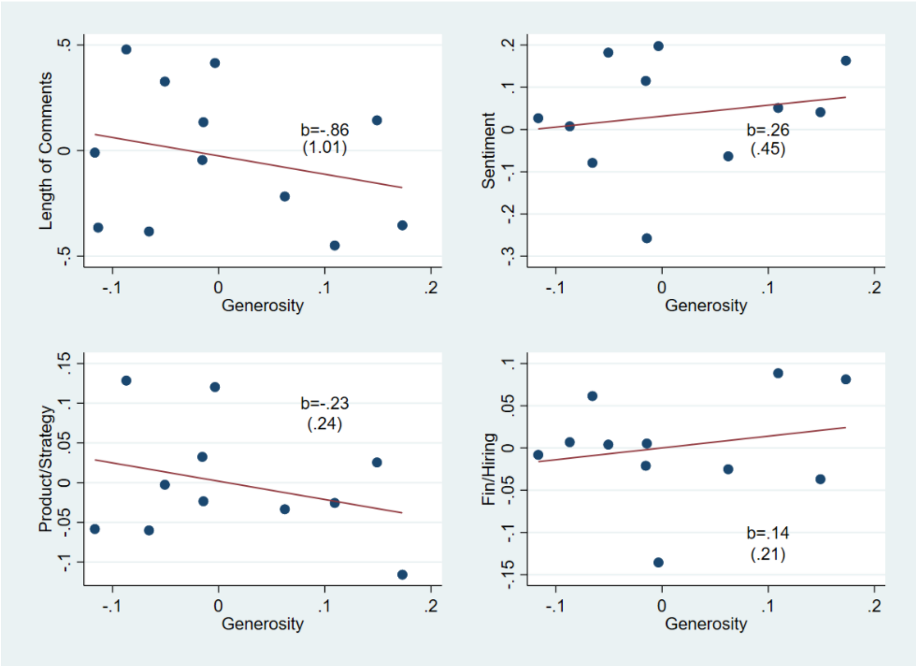

The figure plots the reviewer fixed effects for each reviewer in the sample based on the estimates of equation A51.

**Figure A52 – Distribution of Firm Fixed Effects**



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

**Figure A53 – Reviewers' Generosity and Comments' Content**




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

**Table A51 – Summary Statistics Comments' Content Measures**

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

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

**Table A52 – Reviewers' Generosity and Comments**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Sentiment | | Product / Strategy | | Financial / Hiring | | Length of Comments | |
| Generosity | 0.08 | 0.11 | -0.08 | -0.08 | 0.02 | 0.00 | -1.25*** | -1.23*** |
| | (0.06) | (0.06) | (0.06) | (0.06) | (0.06) | (0.06) | (0.11) | (0.11) |
| Constant | 0.43*** | 0.48*** | 0.75*** | 0.67*** | 0.57*** | 0.56*** | 3.96*** | 3.91*** |
| | (0.05) | (0.04) | (0.05) | (0.04) | (0.05) | (0.04) | (0.09) | (0.07) |
| N | 5177 | 5177 | 5177 | 5177 | 5177 | 5177 | 5794 | 5794 |
| R-sq | 0.1150 | 0.1173 | 0.0594 | 0.0580 | 0.0353 | 0.0364 | 0.1031 | 0.1031 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |

The table correlates the content of reviewer comments and generosity. The observations are at the firm-reviewer level, and generosity correspond to the reviewer fixed effects estimated in Appendix 4 (equation A41). In the regressions, we include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. Region and score fixed effects are also included in all regressions. The row Controls indicates the inclusion as controls of the firm fixed effects estimated in Appendix 4 (equation A41). Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

## Appendix 6—DAP and Venture Outcomes: Reduced Form Estimates

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

The table presents reduced form estimates regressing the different outcome variables against DAP. Controls include the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. Standard errors are robust. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

## Appendix 7—DAP and reviewers' comments

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

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

**Appendix 8—DAP and, Opportunity Assessment and Investment**

**Panel A—Probability of Opportunity Assessment and Investment**

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

**Panel B—Opportunity Assessment Performance**

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

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

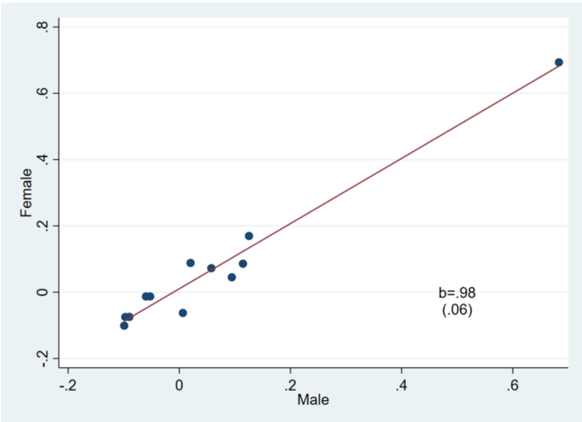
## Appendix 9—Monotonicity Tests

### Panel A- First Stage in Subsamples

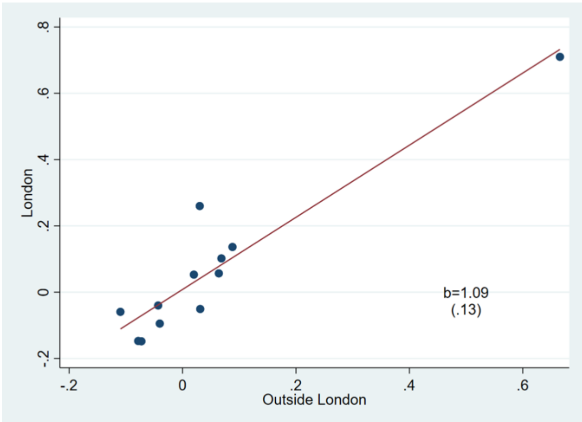
| | (1) London | (2) Outside London | (3) Female Founder | (4) Male Founder | (5) Russell | (6) Non-Russell | (7) Pre-Seed/ Seed | (8) Post-Seed |
|---|---|---|---|---|---|---|---|---|
| DAP | 1.39*** | 0.70*** | 0.87*** | 1.04*** | 0.96*** | 1.02*** | 1.04*** | 0.96*** |
| | (0.10) | (0.11) | (0.18)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

The table shows the correlation between

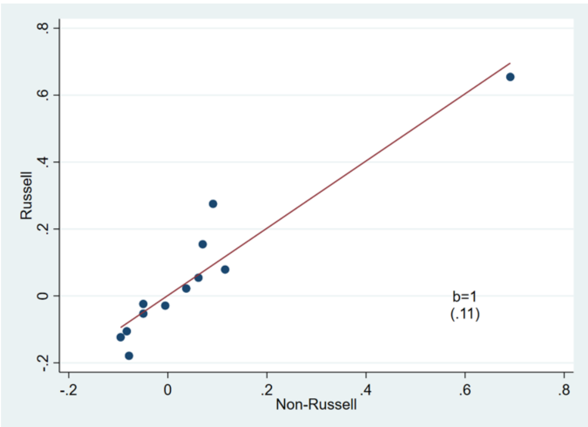
### Panel B – Correlation Between Subgroup-Specific Reviewer-level Generosity Measures



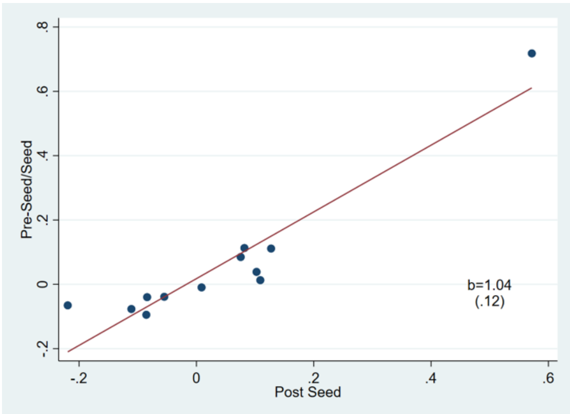

**Female v.s. Male**




**London v.s. Non-London**

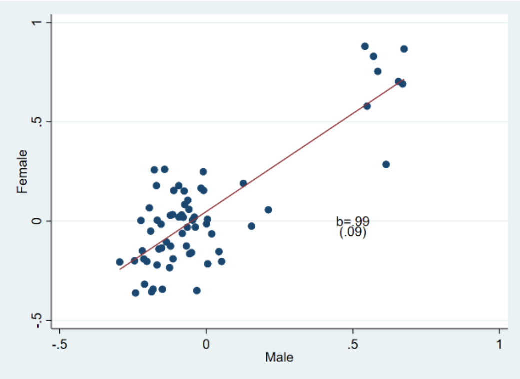



**Russel v.s. Non-Russell**

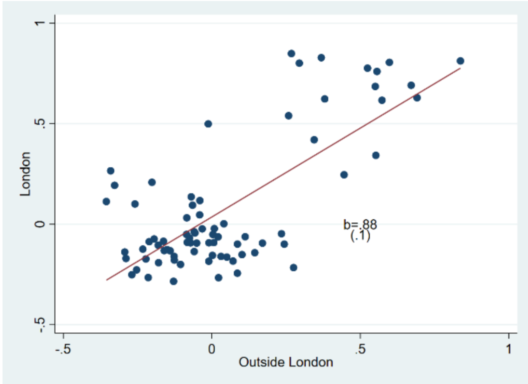



**Pre-seed/Seed and Post-Seed**

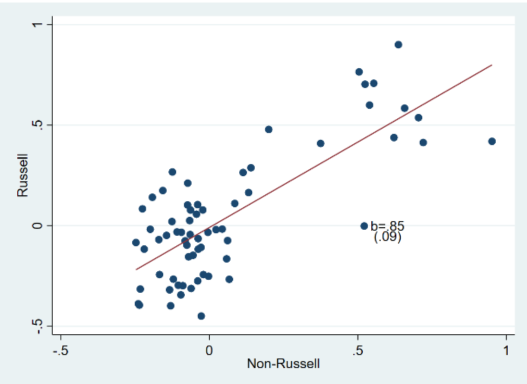
**Panel C – Correlation Between Subgroup-Specific Trio-level Generosity Measures**



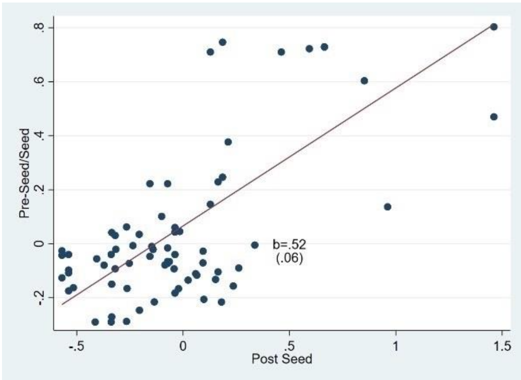

**Female v.s. Male**




**London v.s. Non-London**




**Russel v.s. Non-Russell**




**Pre-seed/Seed and Post-Seed**

The figure shows the correlations between trio level generosity for different groups of applicants. Trio level generosity is defined average rate of due diligence of for the assigned trio controlling 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 10—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 11—Dropping Opportunity Assessment Firms

Panel A—Fundraising

| | (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 | 2.81*** | 2.81*** | 0.20*** | 0.19*** | 0.10*** | 0.10** | 1.12*** | 1.14** |
| | (0.38) | (0.85) | (0.03) | (0.06) | (0.02) | (0.04) | (0.19) | (0.43) |
| N | 1905 | 1905 | 1905 | 1905 | 1905 | 1905 | 1505 | 1505 |
| R-sq | 0.1261 | 0.1002 | 0.1413 | 0.1127 | 0.0629 | 0.0371 | 0.1052 | 0.0723 |
| F Stat. | | 412.84 | | 412.84 | | 412.84 | | 375.26 |

Panel B—Economic Growth

| | (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 | 0.48*** | 0.44** | 0.48** | 0.85* | 0.55*** | 1.10*** | 0.20*** | 0.29*** | 0.06** | -0.09 |
| | (0.07) | (0.16) | (0.15) | (0.34) | (0.13) | (0.29) | (0.04) | (0.08) | (0.02) | (0.06) |
| N | 1905 | 1905 | 1505 | 1505 | 1505 | 1505 | 1505 | 1505 | 1505 | 1505 |
| R-sq | 0.1626 | 0.1384 | 0.0803 | 0.0630 | 0.0626 | 0.0357 | 0.0522 | 0.0282 | 0.0488 | 0.0049 |
| F Stat. | | 412.84 | | 375.26 | | 375.26 | | 375.26 | | 375.26 |

The table presents results from estimating Eq. (4). The outcome variable is specified in the title of each column. Due diligence is a dummy indicating the applicants assigned to further due diligence. The IV models instrument Due diligence with DAP, the due diligence assignment probability estimated as in Eq. (2). All columns include as controls the log transformations (log(1+x)) of variables in the application files: age, target amount to raise, target days to close the fundraising, total addressable market and total serviceable market. Region fixed effects are also included in the regressions. The F-stat corresponds to the F-statistic of the excluded instrument (DAP) in the respective first stage Eq. (3). 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 12-1—Robustness Checks Exclusion Restriction: Fundraising

| 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 | 4.16*** | 15.96*** | 0.26*** | 0.83*** | 0.14*** | 0.68*** | 1.88*** | 5.57** |
| | (0.66) | (4.13) | (0.04) | (0.24) | (0.03) | (0.19) | (0.35) | (1.82) |
| N | 829 | 829 | 829 | 829 | 829 | 829 | 777 | 777 |
| R-sq | 0.2100 | -0.2545 | 0.2244 | -0.0975 | 0.1440 | -0.4031 | 0.2187 | -0.0906 |
| F Stat. | | 15.06 | | 15.06 | | 15.06 | | 12.10 |

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

In Panel A, based on the main identification model, we add trio fixed effects, use location-based DAP estimated using reviewers' assessments over London-based companies only, and restrict the sample to London 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.

## Appendix 12-2—Robustness Checks Exclusion Restriction: Economic Growth

| Panel A: Variation in DAP Due to Policy Change | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (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 | 0.75*** | 1.42** | 1.19*** | 0.09 | 0.82*** | 0.19 | 0.37*** | 0.66* | 0.17*** | 0.01 |
| | (0.11) | (0.52) | (0.26) | (1.32) | (0.22) | (0.94) | (0.07) | (0.32) | (0.03) | (0.21) |
| N | 829 | 829 | 777 | 777 | 777 | 777 | 777 | 777 | 777 | 777 |
| R-sq | 0.2797 | 0.1171 | 0.2058 | 0.1007 | 0.1817 | 0.0754 | 0.1589 | 0.0237 | 0.1395 | 0.0325 |
| F Stat. | | 15.06 | | 12.10 | | 12.10 | | 12.10 | | 12.10 |

| Panel B: Use the Residual DAP as Instrument | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | Ln (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 | 0.51*** | 0.31 | 0.54*** | 0.42 | 0.56*** | 0.91* | 0.22*** | 0.21 | 0.07** | -0.16* |
| | (0.07) | (0.22) | (0.15) | (0.47) | (0.13) | (0.41) | (0.04) | (0.12) | (0.02) | (0.08) |
| N | 1953 | 1953 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 | 1548 |
| R-sq | 0.1629 | 0.1331 | 0.0846 | 0.0705 | 0.0656 | 0.0449 | 0.0555 | 0.0352 | 0.0495 | -0.0329 |
| F Stat. | | 146.28 | | 138.04 | | 138.04 | | 138.04 | | 138.04 |

In Panel A, based on the main identification model, we add trio fixed effects, use location-based DAP, and restrict the sample to London 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 ($\widehat{\epsilon_i}$) 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.

# Chapter II

## The Economic Impacts of the Quality of Government: Evidence from the Speed of Planning Permission

# The Economic Impacts of the Quality of Government: Evidence from the Speed of Planning Permission

Xiang Yin

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Does the quality of government have real effects on economic activities? I provide micro evidence from the housing market and firms’ borrowing behaviours in London. I focus on a single-dimension and verifiable task of bureaucrats—the permission for development plans of new buildings or house renovations to measure the quality of government. Using a hand-collected dataset of over 2.2 million planning applications from 2000 to 2020 in London, I show there is a causal and positive relationship between the speed of the application approval and the trading and value of both residential and commercial properties. Firms that own properties in London increase their chance of creating collateralized loans when exposed to faster planning approval. The effects arise because the timing of property development is important to households and firms. The delay in planning permission will lead them to abandon the project and change behaviours in housing markets and borrowing.

**Abstract:**

# Introduction

The relationship between the quality of government (QoG) and economic growth has been an important topic. Throughout the world, there have been many cases where the government has been playing important roles in the economy. For example, the structured adjustment programs launched under the "Washington Consensus" (Williamson 1990) in Africa failed to bring sustained growth; in China, the deep engagement of the government seems to help explain the growth of China's economy (Song, Storesletten and Zilibotti 2011). It attracts economists' attention towards the role of institutions and the quality of government with a growing realization that successful development requires not only functioning markets but also a functioning state.

However, there is limited evidence on the relationship between QoG and economic performance. On one hand, it's hard to define quality as there are multiple dimensions of the quality of institutions. On the one hand, there is a serious endogeneity problem when it comes to the causal impact of the quality of state on economic performance. Reverse causality problems can easily arise as it's not surprising that more economically developed countries are more capable of building higher quality institutions.

In this paper, I exploit a setting to address the two problems. First, I focus on a single-dimension and verifiable task of bureaucrats in London—the permission for development plans of new buildings or house renovations. The time efficiency of the bureaucrats in reviewing and approving the applications provides a precise and dynamic measure to the quantity the quality of government. Defining the quality of government is challenging, according to Kaufmann, Kraay, and Mastruzzi (2008), includes three dimensions, namely (i) the process by which governments are selected, held accountable, monitored, and replaced; (ii) the capacity of governments to manage resources efficiently and formulate, implement, and enforce sound policies and regulations; and (iii) the respect of citizens and the state for the institutions that govern economic and social interactions among them. The nature of the measure of the quality of government in this paper is closely related to the second category. In particular, it's about time efficiency. Then permission itself is not a validation of the value of the planned development project. But it might indirectly result in efficient (or inefficient) resources allocation in the economy.

Second, to identify any causal relationship between the quality of government and economic activities, I take advantage of the fact that the application and decision-making are processed at the local council[1] level with a randomly assigned case officer for each single application. Since local councils are autonomous in planning permission and case officers are heterogeneous in their speed of reviewing, there is considerable and exogenous variation in the speed for an individual applicant to get permission.

I examine the impacts on two sets of economic variables. The first set of outcome variables is about housing market activities. I find that a higher speed of planning approval can increase trading volume in the housing market and the value of properties. The effects are pronounced for regions with high intensity of planning applications, but are also spread out to nearby regions with not planning applications. In particular, advancing the approval by 8 weeks can increase the growth in trading volume by 8.9%. The second set of outcome variables I investigate is the borrowing behaviours of firms that own properties in London. For these firms, their decision and ability to borrow loans using the properties as collateral is much affected by the value of the properties as the planned development is an investment in the properties. I do find evidence in support of this hypothesis. For firms that have greater exposure to the faster councils and case officers, they are more likely to create a collateralized loan.

Furthermore, I also provide evidence that such patterns may imply welfare loss if the local governments (or bureaucrats) are slow in planning permission. This is because the effects are not showing that the time inefficiency from the government is just "rescheduling" the economic activities. Households and firms make decisions based on the optimal timing of developing the property or land they own. If they have to delay the development project, they will be forced to abandon some profitable development projects. This will result in economic inefficiency.

The main contribution of the paper is that it provides novel and micro evidence for the positive role of the quality of government in economic activities. Especially the quality of government in this paper is measured using a measure narrowly defined. It's not about the government officials' ability to judge the value of economic items or predict economic performance. It's more of non-economic dimension of the quality of government. The paper goes beyond other

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[1] There are 33 borough councils in London, and because of the data quality an capacityy, I will use data of 30 of them (see the Appendix).

papers to provide a causal relationship. In the previous literature, the quality of government is proxied by macro variables that can be endogenously correlated with the economic variables under examination, such as Porta et. al. (1999). It is measured by the time taken for the postal system to respond to letters to non-existent business addresses (Chong et al.2012). The latter is specific in defining quality of government. However, it can hardly be linked to any specific economic activities. My paper complements the empirical evidence about the impacts of the quality of government on economic activities with narrowly defined quality and specific economic activities.

On the other hand, my paper sheds light on the role of government in housing markets. Across the globe, there are a wide array of local government regulations influence the amount, location, and shape of residential development. There has been a strand of research to explore the impacts of such regulations in housing markets and the broader economy. While most of the papers focus on the supply and price of housing, the evidence is mixed. For example, Glaeser, Gyourko and Saks (2005) argued that regulation on the supply side contributes to the increasing housing price. Gyourko and Molloy (2014) review the literature and conclude that the effects is dependent on the types of regulation, so it's a case-by-case problem. More broadly, housing regulation can lead to sorting in labour market and household income. For example, Ganong and Shoag (2013) develop a model to show that housing supply regulation will lead to sorting by skills as long as low-skilled workers spend a disproportionate share of income on land. Moreover, the model predicts that housing supply constraints will cause income differentials across locations to persist by limiting migration from low-wage to high-wage areas. My paper takes a closer and different look at the role of regulation in the housing markets. First, lessening regulation (quick approval) can result in more trading of properties which is in line with most papers that regulation will reduce housing supply. Second, my paper extends the focus to firms' borrowing behaviours. Firms that use properties and lands as loan collateral can also benefit from deregulations.

The paper is followed by seven sections. The next section of the paper explains the institutional background of Local Planning Application in England; then I show the data sources and the measurement of the key variables; summary statistics of the data are then provided in the next section; following that, several stylized facts of planning permission are shown to facilitate the identification strategy; after the identification strategy, I show the results and discuss alternative interpretations; the last section concludes.

# Institutional Background

Planning permission is a type of legal code adopted in many countries to regulate the use and development of houses, buildings and lands. In England, planning permission is needed if you want to build something new, make a major change to your building, such as building an extension or change the use of your building. Its covers both residential and commercial real estate.

## Local Planning Authority

The application of planning permission is submitted to local authorities, the councils in UK. It’s the local council to process the evaluation and make decisions on issuing an approval. If the project needs planning permission and the applicant conduct the work without getting it, the applicant can be served an ‘enforcement notice’ ordering the applicant to undo all the changes you have made. It’s illegal to ignore an enforcement notice.

The local planning authority (LPA) will decide whether to grant planning permission for the applicant’s project based on its development plan. It will not take into account whether local people want it. To decide whether a planning application fits with its development plan, an LPA will look at multiple dimensions, which include not are not limited to: (1) the number, size, layout, siting and external appearance of buildings;(2) the infrastructure available, such as roads and water supply; any landscaping needs; (3) what you want to use the development for; (4) how your development would affect the surrounding area - for example, if it would create lots more traffic.

If the project is not consistent with the local authority’s plan, recommendations will be given to the applicant and refinements to the proposal by the applicant will follow before the LPA makes a final decision. Even if the LPA issues a refusal, the applicant can start a new application based on the original one after modifications on the proposal of the project.

A full planning application is required when making detailed proposals for developments which are not covered by a householder application or permitted development rights. This is commonly the case for new buildings of any kind and any ‘commercial’ project.

## Delegated or Committee

There two types of procedures for the decision makings of these applications. Once your application has been submitted, it's gone through the validation process, all consultations have been carried out and the statutory consultation period has expired a decision will be made- either approved or refused. In the process, planning applications can be determined either under delegated powers or by planning committee which is made up of elected members. Each Local Authority has an adopted Scheme of Delegation. This outlines which applications can be dealt with under delegated powers and which ones need to be determined by planning committee. The scheme of delegation for each Local Authority is different but it can usually be found with a bit of interrogation of the Local Authority website.

In most cases the scheme of delegation sets out "blanket" circumstances whereby applications are required to be determined by planning committee for example "all major applications" or "all waste applications". In addition, "controversial" applications or those considered to be of public interest are required to be decided by planning committee. In this case it usually defines a number of neighbour representations, so for example if 3 or more neighbouring properties object to a proposal. "Delegated powers" essentially delegate the determination of planning applications to planning officers. In this case, usually the planning officer will write a report outlining all of the planning considerations and make a recommendation to approve or refuse the application and detail any conditions and/or planning obligations. Usually then it will require a signature from a senior officer/team leader/head of planning to agree the recommendation and get the decision issued.

## Case Officer

Case officer plays an important role in the approval decision. Each case is assigned with a case officer. The case officer is the delegated person to make decision if the application doesn't need to go planning committee. When an application needs to go to planning committee, the case officer will produce a report with a recommendation – usually approve or refuse. It will also detail any recommended planning conditions and any required planning obligations. At the committee meeting details of the application will be presented to members by an officer, usually there is a limited time for supporter(s) (usually the applicant or agent) to address the committee and objector(s) are given the same opportunity. Following any questions and general debate Committee members then take a vote to either support the officer recommendation or go against the recommendation.

Given how tedious the procedures are, most applicants appoint an agent to apply for planning permission on their behalf. For example, the agent can be the architect, solicitor or builder.

### Time for Permission

In most cases, planning applications are decided within 8 weeks. In England, for unusually large or complex applications the time limit is 13 weeks; if an application is subject to an Environmental Impact Assessment, it is meant to be within 16 weeks. If the decision takes longer, you can appeal. Almost all applications will get permissions and they only differ in the time taken from application date to decision issued date. It is implied that the factors affecting the outcome of application is unlikely related to the economic value of the project. The application procedure is also not intended to reveal any signals of the economic "type" of the projects.

# 1. Data and Measurement

## Data Sources

There are three major data sets used in this paper. First, I hand collected all local planning application details from 30 London borough councils from January 2000 to December 2020. Second, I measure housing market transaction activities using Price Paid Data (PPD) of HM Land Registry. Third, I collected the data set Commercial and Corporate Ownership Data (CCOD) from 2017 to 2020 which shows registered land and property in England and Wales owned by UK companies. Fourth, I collected data from the registry of charge at Companies House about loans collateralized by real estate of companies incorporated in England.

The details and process of Local Planning Applications (LPA) are required to be publicized by each local authority. I collected all publicly available transactions from the online register portals of 30 borough councils in London. There are three other borough councils which don't have good coverage of key dates information (See Appendix for the summary of Applications by council name.). The data was collected in July 2021 and I keep all applications received from January 1 2000 to 31 December 2020, which indicates that for applications received later in the sample are less likely to be observed with their dates of decision. The key information I extracted from the application is the date of application received, decision date, decision status, the address of the property, type of application (full/partial), type of the decision-making process (via delegated power or committee), the agent's name and address, case officer's name. In the final collected dataset, there are over 2.2 million entries.

Price Paid Data (PPD) of HM Land Registry contains information on all property sales in England and Wales that are sold for value and are lodged for registration. Each entry informs the address, date, and value of the transaction of the property being sold. Therefore, the data set enables me to construct variables to characterize the behaviours in the London housing market.

To explore the financial impacts of the speed in planning permission, I focus on companies that own land or properties in London. To do that, I first match the addresses of properties and land of CCOD with the addresses of properties and land under planning application in LPA. I find About 46.5% of the firms that own properties in London have once applied for planning permission. Furthermore, I explored the borrowing behaviour of these firms using the registry of charge at Companies House. The charge is a claim that the company has an outstanding loan which is secured by the firm's cash flow, physical assets, land or properties. Given that there is a 21-day time limit for the registry of the charge to be valid after the creation of the charge, the registry of a charge specifying real estate as collateral signals the commencement of a collateralized lending relationship.

### Application-Level Speed

One key variable in the paper is the time for a planning application to get the approval decision. Less efficient local authorities will spend more time on every single application. Longer applications may impede the applicants to undertake the planned development of their houses, buildings, or lands, and furthermore, affect their financial decisions at the corporate level. Since there are statutory requirements on the speed of processing applications, applicants may have ex-ante expectations that the reviewing process will take up to the deadline. Observing that the most applications are issued approval on dates close to the 8-week deadline (32% in the week before the deadline), I take the following method to measure the speed for the approval of a single application:

$$TimeApproval_{j,t} = \frac{Days\ from\ Application\ to\ Approval\ Issuance_{j,t}}{56} \quad (1)$$

which is the normalized time length for the approval of application j received on date t. For applications with $TimeApproval_{j,t} = 1$, they are issued approval on the ordinary deadline- 8 weeks. For $TimeApproval_{j,t} < 1$ , the decision is made before the deadline and $TimeApproval_{j,t} > 1$, the application is after the deadline. The magnitude of this measure also informs how quickly the applicant gets the approval as the longer it takes to get the permission, the longer $TimeApproval_{j,t}$ is. For applications with reviewing process longer than 52 weeks or unfinished as of the date of data collection (December 2021), I set the time length to be 52 weeks. As the percentage of applications that can take up more than 52 weeks is less than 0.1%, it's not a big bias in capturing the relative time length for getting approval.

It should be noted that this is an *ex-post* measure of speed. It is the combined result of a set of expected or unexpected factors affecting the process of application reviewing upon the date of application. When used as an independent variable in a panel regression, it can carry information unobservable to the agents (the applicant, the local government, and the market) for the contemporaneous period. To address this issue, I construct the following variable which can specify the time window that we can observe the approval status of the application：

$$ApprovalRate_{j,t}^{S} = \begin{cases} 1, & \text{if the application has obtained approval as of date t+s} \\ 0, & \text{otherwise} \end{cases} \quad (2)$$

It's an indicator of if the application gets approved within *s* periods from the application date *t*. For example, $ApprovalRate_{j,t}^{8\,weeks}$ represents if the application gets approved before the deadline. It's another way to capture one dimension of the time efficiency of local councils. If the applicants don't value an earlier approval before the deadline, this measure is enough to the speed of the reviewing process for the applicants.

For the main results where I explore the impacts of the time efficiency of local governments, I create a measure that captures features of both measures mentioned above

$$Speed_{j,t} = 1 - \begin{cases} TimeApproval_{j,t} & \text{if } TimeApproval_{j,t} \leq 1 \\ E_i\left(TimeApproval_{j,t} \middle| TimeApproval_{j,t} > 1\right) & \text{otherwise} \end{cases} \quad (3)$$

$E_i\left(TimeApproval_{j,t} \middle| TimeApproval_{j,t} > 1\right)$ is the council specific expected time to get the approval if the applicant doesn't get the approval before the deadline. In the data, it takes on average 26 weeks to get the approval after the deadline, which implies that $E_i\left(TimeApproval_{j,t} \middle| TimeApproval_{j,t} > 1\right)$ is equal to 3.25. In general, the measure $Speed_{j,t}$ is decreasing with the actual days taken to get the approval. For applications that obtained approval before the deadline, $Speed_{j,t}$ ranges from 1 to 0; for applications that obtained approval after the deadline, $Speed_{j,t}$ is constantly at -2.25. This transformation creates a discontinuity in $Speed_{j,t}$ for applications approved around deadline dates. It's consistent with the government's discontinuous incentive around deadline dates. In addition, the government's behaviours in the two regimes (before and after the 8-week deadline) are uncorrelated[2] . In the main results, I focus on short-term effects (within 8 weeks), so the variation in time efficiency is not relevant for the identification. This measure has advantages

[2] In a council-week level regression that regress (1) *log of time to approval for applications approved before the 8-week deadline* against (2) *log of time to approval for applications approved after the 8-week deadline* controlling for council fixed effects and application week fixed effects, generates a coefficient of 0.017 (t stat. is 0.31).

over non-linear transformations of the number of days for approval. For example, compared to $-ln\ (TimeApproval_{j,t})$ , one unit of variation in $Speed_{j,t}$ have a fixed interpretation as 8 weeks.

### Housing Market

Governments' efficiency in processing planning applications can affect the housing market in terms of the trading volume and housing price. I take the advantage of property transaction data set (Price Paid Data (PPD) ) that enables me to quantify housing market activities at a very granular level. As the data set contains information for each property transaction the postcode, date and price, I construct measures of trading volume and average annual housing price increase at both council and postcode levels. In particular, I annualize the price growth for a single property based on the prices paid in two consecutive transactions. Price growth might be caused by the higher valuation of particular property characteristics, so I normalize price growth in the following way:

$$\mathrm{AnnualPriceGrowth}_{l,t} \equiv \frac{\ln\left(\frac{\mathrm{Price}_{l,t}}{\mathrm{Price}_{l,t-s}}\right)}{s/365} = \sum_{k=1}^{K} \beta_{k,t} X_{l,k} + \varepsilon_{l,t}$$

$$\mathrm{Adj.\,AnnualPriceGrowth}_{l,t} = \hat{\varepsilon}_{l,t}$$

Based on two consecutive transactions of property l on date $t - s$ and $t$, I can measure the price growth $\mathrm{AnnualPriceGrowth}_{l,t}$ as defined above. Then, to control for price growth induced by the market valuation of certain characteristics, I run a regression as above. $X_{l,k}$ represents a set of characteristics for property l which include: property type(detached, semi-detached and etc.), residential/commercial, tenure(Freehold/Leasehold), and $\beta_{k,t}$ is the time-varying valuation parameter for characteristic $k$. Consequently, $\varepsilon_{l,t}$ is the adjusted annual price growth for property l as observed on date t.

### Collateralized Borrowing

In addition to the evidence from the housing market, I will study the impacts of government efficiency on firms' financial decisions. The rationale is that a higher speed of planning permission can reduce the transaction costs and timing frictions which will enhance the collateral value of buildings and lands owned by corporations. Following this mechanism, affected companies are in a better position to borrow long term debt. For this purpose, I focus on the behaviours of firms that recently purchased real estate. These newly bought properties are highly likely to be refurbished or developed for new businesses. Therefore, the valuation of these properties might be impaired by the inefficiency of the local governments in assessing possible planning proposals for these properties.

I construct two variables to characterise companies' behaviours in long-term borrowing. One variable is an indicator of if the company is using real estate as collateral to borrow. This is constructed using the registry of charge at Companies House. The "charge" is the security a company gives for a loan. According to the details of the registry, it can be told if a firm uses any real estate it owns as collateral for borrowing. The second variable is the growth in the value of long-term debt. The information is from the balance sheet disclosed at Companies House. To ensure the growth in long-term debt capture the impacts of government efficiency, the date of purchasing real estate falls between two consecutive dates of disclosure of balance sheet information.

# Summary Statistics

In this section, I will provide summary statistics of the major datasets used in this paper. In particular, the hand-collected data set of all planning applications is a novel data set that provides us with a micro perspective on how local governments interact with economic activities.

## Local Planning Application

There are in total 2,211,715 applications from the 30 borough councils in London from January 2000 to December 2020. Only 53,869 (2.4%) of them don't get the approval (either because no decisions have been issued or refinement advice is not followed and rejections became permanent). Even though most applicants get the permissions finally, the time taken for the approval is uncertain. In the summary statistics shown in Table 1, the time efficiency of the local government's reviewing is measured in different ways. The average number of weeks taken for the process is 10.85 weeks. The median is 7 weeks and the longest application took over 20 years. Regarding the statutory time requirements, 72.1% meet the 8-week deadline; 11.2% of them are finished after the 8-week deadline and before the 13-week deadline; 2.8% of them are finished after the 13-week deadline and before the 16-week deadline; 9.3% are finished after all the deadlines. For applications exceeding the 8-week deadline, they can be large and complex development projects or subject to environmental impact assessment. However, it is not necessarily informed to the applicant from the beginning if the application is subject to the two later deadlines or not. The 27.9% the fail to meet the 8-week deadline can result from the inefficiency of the local bureaucrats.

In some councils' LPA portals, there is detailed information about the types of applications. Based on councils with the relevant information available, 81.2% of the applications are assisted by an agent, 4.5% of the applications are decided by a planning committee instead of delegated powers; 39.2% of them are full applications which are detailed proposals which are usually for new buildings and commercial projects. These features of the applications increase the complexity of the projects and prolong the approval process. To show that these characteristics are correlated with the complexity, I compare the distributions of the number of weeks for approval in subsamples of different types of applications in Figure 3. It can be observed that full applications assisted by an agent, and reviewed by a committee takes longer time to get the approval. The fourth plot in Figure 3 compares applications with and without the identity of the case officer (name, address, and email). Since the individual working efficiency of case officer is used as an instrument for identification, and 18.2% of the applications have no information of case officers. There is no significant difference between the two subsamples, so probably the missing information will not cause bias in the results.

In Figure 4, I plot the average efficiency of application approval over time. There is time series variation of the working efficiency of local governments. In the early 200s, the time efficiency is rather low. Only around 60% of the applications can meet the 8-week deadline. The speed is raised up due to regulation on the local planning application from the central government. The speed is at its lowest level in 2012. It can result from two factors. The first reason is the 2012 London Olympics, and priorities are given to development projects serviced for the Olympics. The second reason is the Local Planning Regulation 2012 following the Localism Act 2011 which modifies the general planning guidelines and this requires the local governments to adapt their work to the new regulations. Such adjustments increase the time needed for applications filed during this period.

I aggregate the application information into the council-month cohorts. Panel B of Table 1 provides the summary statistics for cohorts. Cohort level speed is also used as an important independent variable in the main regressions. It dynamically measures the council's speed in approving planning applications. From the summary statistics, it's shown that on average each council receives 294 applications in a month. The aggregation also keeps the properties of the application details variables, which implies there is no significant heterogeneity across councils. In the Appendix, I show summary statistics of application information for each council.

### Housing Market Transactions

The first set of outcome variables to examine is housing market transactions. In this paper, I focus on housing market activities in small areas at the postcode level (outward code and sector in the inward code[3]). In addition, the area should have at least one transaction of property

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[3] For example, the full postcode for the London School of Economics is WC2A 2AE, then the housing market it belongs to are properties under the postcodes begin with "WC2A 2".

during the period from 2000 to 2020. Consequently, there are 1,213 such geographical units which I call "housing submarkets". According to the summary statistics in Table 2, each housing submarket fil with local authority about 6 applications each month. This is 2% of the properties located in the local area. I then calculate the time efficiency and application details variables for each housing submarket each month based on all applications from the area. When I explore the impacts of the speed of planning approval on local housing market outcomes, I consider both council-wide speed and postcode level speed. Due to the possible spill-over effects to neighbouring areas, even areas where there are no planning applications can also be affected by the efficiency in planning application approval.

I focus on two measures of housing market activities: number of transactions and annualized price growth. The monthly average number of transactions for these housing submarkets is 0.78. The median number is 5. It suggests that transactions of properties in London are rather frequent. To overcome the problem of geographical heterogeneity, I also calculate year-on-year log growth in the number of transactions[4]. For properties in repeated transactions, I am able to calculate the annualized price growth. The average annualised price growth for repeated sales is 11% with the median equal to 8%.

## Companies' Ownership of Properties and Collateralized Borrowing

The second set of outcome variables to investigate are the borrowing behaviours of firms that are most likely affected by the development of properties and land. I focus on firms that own properties in the 30 London borough councils during the period when such ownership information is available, i.e. from November 2017 to December 2019. The data set contains 37,378 companies. On average, each company owns 2 properties in London while the median number is 1. So most companies own one property in London during the sample period, and the value of the property has an average value of £ 16.9 millions[5] . The application intensity is on average 8% which is higher than the application intensity for any properties in the local area

[4] To calculate the monthly year-on-year growth of transactions volume, I calculate it as $log(1 + N_t) - log(1 + N_{t-12})$ where $N_t$ is the number of transactions in month $t$ and $N_{t-12}$ is the number of transactions 12 months ago.

[5] The value of property is estimated in the following way: the price paid when purchasing × (1+ housing price index growth from the purchase date and the date for valuation).

(2%). It means commercial properties are more frequently applying for planning approval than residential properties.

Regarding the borrowing behaviours, I summarize the monthly observations of this set of companies in two aspects. First, from the administrative data of filings of balance sheet information, the firms owning properties in London have £152 million (£ 0.81 million for the median) total debt and £ 38.34 (£ 0.29 million for the median) long-term debt on average. In addition, the average annual growth of total debt is 3% and average annual growth in long-term debt is 11%. On the other aspect, each month, 2% of the companies create at least one collateralized loan each month.

To show that borrowing behaviours are interacted with planning applications, I plot the fraction of firms that create collateralized loans around the date of application. In Figure 5, there is a clear pattern that after planning application, there is an 1.2% rise in the probability of loan creation following the planning application, which is ¼ of the level in the month before the application. This shows that firms' decision to originate loans is followed by proposing development projects for the properties or land they own. Borrowing and planning applications are correlated for two reasons: (1) companies need financing for their projects and (2) the proposal of development projects can enhance the collateral value.

# Stylized Facts about Planning Permissions

Before showing the economic impacts of government efficiency in reviewing applications, this section provides a characterization of patterns of how government manage the efficiency of the task. On one hand, I will show if the characteristics of the property or land are related to the time for getting the permission. Any potential inequality due to certain characteristics will bias the regression estimations of economic outcomes on government efficiency. On the other hand, I will study how governments allocate working capacity to pending applications with different durations. Local authorities may undertake prioritization strategies based on the duration to comply with statutory requirements. It can show how the temporary shocks in government efficiency can be extended to affect future applications.

## Random Allocation of Case Officers

To facilitate the identification strategy, I first show that applications are matched with case officers in a random way. That means there is no differences in the applicant's characteristics assigned to the case officers.

$$\mathrm{X}_{\mathrm{j,t}} = a_{c,t} + \gamma_{i,t} + \epsilon_{\mathrm{j,t}} \ (4)$$

In the specification above, the applicant's characteristics $\mathrm{X}_{\mathrm{j,t}}$ is regressed against cohort fixed effects (council-week level) $\gamma_{i,t}$ and case-officer-week fixed effects $a_{c,t}$. If applicants are randomly assigned to all case officers on duty in that week, the joint significance of $a_{c,t}$ should be minor.

I focus on case officers that have reviewed at least 30 applications from 2000 to 2020 and the applications are filed in at least two months. This leaves us with 2,829 case officers. According to Table, each case officer is on average allocated 13 applications each month when the case officer is on duty. In addition, an average case officer's workload is 4.7% of the cohort's size. So roughly every cohort of applications are split among 20 case officers.

The F-test for the existence of "style" in the application types assigned to case officers show evidence of random allocation. I also checked the randomness of the matching between agents and case officers, and between postcodes and case officers[6].

## The Decomposition of Speed

What determines the speed for the applicant to get the permission? There are broadly three categories of factors: (1) the application' characteristics; (2) case-officer specific efficiency; (3) cohort (council-week) specific factors. Application characteristics that increase the complexity of the proposal will slow down the process. Case officers with different capabilities and working styles will process applications at different speeds. This component which is constant over time is very important for the development of the identification strategy in this paper. Cohort specific factors capture any other unobservable factors from the applications and case officers but are common to all applications received by the council in a given week.

$$\mathrm{Speed}_{\mathrm{j,t}} = \sum_{\mathrm{k=1}}^{\mathrm{K}} \beta_{\mathrm{k}} \mathrm{X}_{\mathrm{k,j,t}} + a_c + \gamma_{i,t} + \epsilon_{\mathrm{j,t}} \quad (5)$$

where $Speed_{j,t}$ is decomposed into several parts shown on the right of the equation. $X_{k,j,t}$ represents a set of application characteristics: type of application (full/partial), decision-maker (committee or delegated), and the existence of an agent in the process. $a_c$ are case officer fixed effects. $\gamma_{i,t}$ are cohort fixed effect. In addition, I consider a package of fixed effects at different levels (F.E.): *postcode, agent, agent-officer , postcode-officer*. They capture the council's prioritization of certain areas (*postcode F.E.* ) or area-specific complexity of planning, the ability of agents in facilitating the process (*agent F.E.*), the potential connection between the officer with the agent (*agent-officer F.E.*), the officer's expertise/favouritism for certain regions (*postcode-officer F.E.*). $\gamma_{j,t}$ is cohort fixed effect with which the comparison is among applications received by the same council in the same week.

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[6] To test these, I focus on *share of applications for each case officer* in cohort-officer-agent and cohort-officer-postcode panel data sets. The p values of F-tests for joint significance of officer-agent fixed effects and officer-postcode fixed effects are 0.85 and 0.73 respectively.

In the regression results shown in Table 5, the application characteristics are proved to be correlated with the complexity of the proposals. According to the full specification in column (9), full applications take additional $0.22\times 8 = 1.76$ weeks to get the permission; applications assisted by an agent take additional 0.4 weeks; decision made by the planning committee instead of delegated power takes 10.4 more weeks. The results are robust after controlling for the package of fixed effects mentioned in the paragraph above.

Importantly, there are significant case officer fixed effects in speed of application approval. In table 5, across specifications (5) to (9), the F-tests for case-officer fixed effects show that case-officers are heterogeneous in their speed even after controlling for application characteristics. In Figure 5 (a), I show the distribution of case officer fixed effects. There is considerable heterogeneity. The average value of fixed effects is -0.0468 and the standard deviation is 0.456. 50.8% of the case officers have a positive speed fixed effect which means the efficient and inefficient case officers are equally distributed in the data. Furthermore, to show that individual case officer efficiency is because of low workload. I check the relationship between case officer fixed effects and the number of applications of each case officer in Figure 5(b). The correlation is almost zero. Therefore, the individual-specific style in efficiency does not result from variation in workload.

### Before and After Deadline: Independent Efficiency Management

It's very likely that the case officers have less incentive to boost the progress of the applications once they have passed the deadline. There are two possible reasons. One can be the unexpected complexity of the proposal will switch the application track from a normal "8-week" deadline to an extraordinary "13-week" or "16-week" deadline. The other possible reason is once the incentive to shorten the time length for decision issuance is smaller. Namely, the case officer won't be blamed twice if the application is postponed for one more day since it has exceeded the deadline. Figuring out this not only sheds light on the labour inputs of bureaucrats under time incentives, it also helps to design the reasonable time frame to observe the responses from applicants and the general housing market.

To test that, I focus on the dynamics of efficiency implied by the approval rates within different forwarding time periods for a given cohort of applications. If we define the conditional approval date after $s$ periods as the approval rate of all pending (i.e. undecided) applications at the beginning of period $s$, it's a dynamic measure of the speed for a cohort of applications multiple periods after of the application date. If the case officer maintains the same working style over time for that cohort, there should be a correlation across periods of the conditional approval rate. For example, if the case officer is fast in the first number of weeks after receiving a cohort of applications, this speed is expected to *carry on* to the following weeks until the deadline. After the deadline, an efficient case officer at the beginning is not different from a sluggish case officer at the beginning.

$$Cond.ApprovalRate_{i,t}^{S} \equiv \frac{ApprovalRate_{i,t}^{S}}{1-\sum_{\tau=1}^{S-1} ApprovalRate_{i,t}^{\tau}}$$

$$Cond.ApprovalRate_{i,t}^{S} = \rho Cond.ApprovalRate_{i,t}^{S+1} + b_i + c_t + e_{i,t} \quad (6)$$

As shown in the equations above, the conditional approval rate for a cohort of applications received by council $i$ in week $t$ is calculated as a function of marginal approval rates in the previous periods $ApprovalRate_{i,t}^{\tau}(\tau < s)$ and current period. In the second equation of a regression model, I explore the relationship between conditional approval rates in two consecutive weeks and control for council fixed effects and week fixed effects[7]. The hypothesis I made above about the break of incentive after the deadline will predict a positive coefficient $\rho$ up to the week before the deadline ($s = 7$) and then insignificant afterwards

In Figure 6, I show patterns of the conditional approval rates. Before the 8-week deadline, the conditional approval rate is increasing. It suggests that an undecided application is more and more likely to be approved when approaching the deadline. For an application not approved yet at the beginning of the 8$^{th}$ week, there is a 56% chance that it will get approval during the 8$^{th}$ week. For undecided applications, after the deadline has passed, the chance for them to be approved each week is reduced to less than 12%. As discussed above, the conditional approval rate contains information cohort-wise efficiency. There should be a "carry-on" of the unexpected efficiency over time if case officers keep the working style regardless of the deadline requirements, Figure 6(b) plots the coefficient $\rho$ of regression model (6). It shows evidence that, cohort-specific efficiency is only carried forward from the first week of application until the week before deadline (the 7-th week). It implies that, the speed of the approval for outstanding applications after the deadline is independent from the speed for

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[77] I also run similar regressions at the case officer level instead of council level, and I find similar results.

applications before deadline. This stylized fact is important for the design of identification strategy. In the main regressions, we examine the outcome variables in the month after the month of application. It means when observing the outcome variables, the cohort of applications filed in the previous month has been under review for four to eight weeks. This time window does not capture the whole life of a cohort of applications. But the results in Figure 6(b), show that this period (four to eight weeks after the application) carries a common component of efficiency that originates from the start of the life of the application. From the applicant's perspective, they have ex-ante expectation of the chance of getting permission before the deadline. The expectation is updated after the application is received by the local planning authority. They also receive information via the interactions with case officer. Most of the uncertainty of when to get the permission is addressed in the week before deadline. After the deadline, it's hard for them to form predict the timing of the permission.

# Identification Strategy

## Baseline Regression

The paper is to explore the causal effects of time efficiency on housing market outcomes and firms' financial decisions. For housing market outcomes, the baseline regression model is:

$$Y_{p,i,t+1} = \beta_0 Speed_{i,t} + \beta_1 Speed_{p,i,t} \times Intensity_{p,i,t} + \beta_2 Intensity_{p,i,t} + a_p + c_t + e_{p,i,t} \quad (7)$$

where observations are at small housing market unit level (outward code of postcode). $Y_{p,i,t}$ is the outcome variable for housing transactions that occurred in housing market $p$ of council $i$ in month $t+1$. The speed of council $i$ in processing applications in month $t$ is denoted as $Speed_{i,t}$. $Intensity_{p,i,t}$ is the ratio of the number of all pending applications at the beginning of month $t$ from housing market $p$ to the total number of houses in that area. So $Intensity_{p,i,t}$ is a measure of the intensity of application activities. In the regression, I also include fixed effects at the council, housing market and month levels.

$Intensity_{p,i,t}$ is heterogeneous across housing markets within the same council $i$, the coefficient before the interaction between speed and intensity of pending applications $Speed_{i,t} \times Intensity_{p,i,t}$ estimates the additional *localized* effect due to the higher intensity of ongoing applications in that small area of the housing market. This specification leaves the coefficient before $Speed_{i,t}$ to capture the *spill-over* effects of council level speed on housing market outcomes. The spill-over effects can arise from two sources: (1) positive externality to nearby areas with development projects; (2) current speed of application processing will affect the (expected) speed of future applications from areas that currently have no applications.

The variation used to estimate the spill-over effect is mainly the difference in speed across councils. The additional localized effect is identified using variation in the intensity of planning applications across housing markets within councils. The existence of spill-over effects depends on the degree of segregation of housing markets.

For firm-level outcomes, the specification has to be modified because one firm can have multiple properties located in different councils. So the baseline regressions can be rewritten as the following:

$$Y_{f,t+1} = \beta_0 Speed_{f,t} + \beta_1 Speed^*{}_{f,t} \times Intensity_{f,t} + \beta_2 Intensity_{f,t} + a_f + c_t + e_{f,,t} \quad (8)$$

where $Y_{f,t}$ denotes the outcome variable of firm $f$ in month $t$. $Speed_{f,t}$ is firm $f's$ exposure to local government efficiency: the weighted average of council-level speed of all properties owned by the firm. Each council's speed is weighted using the fraction of properties located in that council. On the other hand, $Speed^*{}_{f,t}$ only considers the set of properties that apply for planning permissions. $Intensity_{f,t}$ is the fraction of properties owned by firm $j$ that apply for planning permission. In addition, firm fixed effects and month fixed effects are included.

There are five outcome variables in the main results to show. For housing market analysis, I consider the number of transactions and the growth of price for sold properties in month $t$. For firm analysis, I consider the likelihood of starting a lending relationship collateralized by the real estate in the year following month $t$ and the growth in total debt and long-term debt. Since *Speed* linearly measures the time saved relative to the 8-week deadline (*Speed=0.5* means $0.5 \times 8$ =4 weeks before the deadline), the coefficients $\beta_0$ and $\beta_1$ can be interpreted as the impact of advancing the application by 8 weeks.

### Instrument Variable

The baseline regressions are subjected to several possible endogeneity problems. Speed is not necessarily uncorrelated with the outcome variables via other channels. There are two prominent endogeneity examples. First, a council may be pressured to raise the speed of reviewing applications if the local housing market is expected to go up by the applicants. Second, development projects that take longer time to review are usually larger commercial projects that belongs to fast-growing firms and possibly bring up positive externality to the neighbourhood. For identification, the ideal variation in speed should come from exogenous sources. To address the problem, I exploit the fact that the case officers are heterogeneous in their individual time efficiency management and there is randomness in the matching between applicants and case officers.

In most cases, one council have multiple case officers, so the applicant can't predict which case officer it will be assigned to. For each pool of pending applications for a given local housing market level or firm level, there is also a random component in the speed of their processing procedure. Any effects in the outcome variables arising from this exogenous component can be regarded as causal effects. I construct instrument variables by exploiting such exogeneity.

The case officer fixed effects from regression model (5) are used as building blocks for the instrument variables. There are four possible endogenous variables in the baseline regressions specified above: council-month level speed ( $Speed_{i,t}$ ), the postcode-month level speed ($Speed_{p,i,t}$), firm-month level exposure to local government's efficiency ($Speed_{f,t}$), the speed of the firm's applications ($Speed^{*}{}_{f,t}$). For each of them, it represents a pool of applications. I construct an instrument variable by aggregating the speed fixed effects of case officers assigned to these applications. To avoid spurious correlation between the instrument and endogenous variables, I leave out the applications behind the endogenous variables in my calculation of the case officer fixed effects.

## Discussion of Results

With the identification strategy, I am able to investigate the causal effects of local government's quality on economic outcomes. I exploit a very specific context of the quality of local governments-the speed in approving planning and development proposals. I focus on two sets of economic outcome variables. One set if the activities in housing market. The other set is the borrowing patterns of firms that own properties in London. Our sight is narrowed over these economic activities is because I hypothesize that timing is important in the housing market and therefor the delay of housing-quality improvement or land development can devalue the properties and affects the borrowing collateralized by the properties. Furthermore, to shed light on the welfare implications of government's inefficiency, I explore an alternative interpretation of the results as simply "rescheduling" effects.

### Housing Markets

Table 6 contains the results for housing markets. I examine the effects on the volume of properties transactions, the year-over-year growth of the volume, and the annualised price growth of repeated sales. In general, the results show evidence that higher speed of planning approval increases transactions in the housing market and the value of sold properties. The effects is not only limited to areas with high intensity of applications but also nearby areas in the council. It's a proof of positive spill-over effects.

In columns (1) to (3), the coefficients tell the relationship between speed of approving and housing market outcomes at council level. After controlling council and month fixed effects, higher speed is correlated with higher volume of transactions but not the value increase of properties. Such relationship is indeed causal. With a strong instrument (p-values of F tests for weak instrument is less than 0.001), the coefficients are still significant. Regarding the magnitude, advancing the approval by 8 weeks can increase the annual growth in transactions volume by 2.8%, which is economically significant compared to the median of the growth rate (0%) and even the $75^{th}$ percentile (27%). There is no causal effect on the price growth of repeated sales. This is still true after allowing for heterogeneity of application intensity (Column (6)). It suggests that the spill-over effect is not spread to the value of properties. This is still consistent with the idea that the price change comes from the quality added value. For properties in the neighbourhood of other development projects, they are affected to the extent that any future development on them become more profitable. They have an additional valuable "option" to increase their value. However, for those who select to be sold without exercising the option now, they may be not representative of the market. Therefore, the value increase brought by faster approval can't be captured by the repeated sales of nearby properties that are not under planning.

On the other hand, for areas with high intensity of applications, the effects are seen on both trading volume and the price growth of repeated sales. In columns (4) to (6), the regressions contain terms that account for the fact different areas have different intensity of planning. The effects can be very *localized* to the properties that are under application. The coefficients before the term $Speed * Intensity$ inform us about such localized effects. Indeed the effects are stronger for those regions with high intensity of planning applications. Especially, the effect on price growth of repeated sales is significant for the term $Speed * Intensity$. The interpretation of this effects is that advancing the approval by 8 weeks can increase the value of the properties under planning by 5.1%.

## Firms' Borrowing

Since the trading and value of properties are positively affected by higher speed of planning approval, firms that exposed to the housing market are also potentially affected. For firms that own properties or land in London, their borrowing behaviours will be reshaped by the local governments' efficiency as well. I examine firm's borrowing behaviours via the creation of collateralized loans and the annual change in long term debt and total debt. In general, the results found are in line with the effects on housing market. Firms do create more loans due to the speed of government's approval of planning proposals and it is accompanied by the increase in long term debt.

First, columns (1) to (3) show the effects for firms' exposure to council wide efficiency. The causal effects captured by coefficient in column (3) suggest that faster approval can increase the chance of creating a collateralized loan by 0.448%. The number is about one quarter of the average probability for a firm to create a loan in a given month (2%). The effects are not seen on the growth of debt. It can be due to two reasons. The first reason is the same as the discussions for housing market results. The value increase effects can't be spread out to nearby properties in contemporaneous month. The second reason is the data frequency. The value of the firm's debt is extracted from the annual balance sheet reports submitted to Companies House, it can't precisely correspond to the month of high speed of planning approval.

Second, columns (4) to (6) inform us about the localized effects. Similar to what we found for housing market, the effects are stronger for firms that have a higher fraction of the properties they own under planning. This has been preliminarily suggested by the pattern shown in Figure 5. The creation of loan is usually followed by the planning application. Beyond that, the coefficient in column (6) implies that if the planning application can be approved 8 weeks earlier, this can increase the chance of loan creation by 0.61%. Moreover, there is an effect observed on the long-term growth of 1.2% in annual term. Even though this number is small, it might be just because the noise due to annual frequency data issues blur the effects.

### Alternative Interpretation

An alternative interpretation of the results documented in Tables 6 and 7 is those are simply "rescheduling" effects. Since the planning regulation requires the development and constructions can only start after the permission of the proposal, any delay in the approval just *reschedule* the time for the start of the development. Namely, if there is more trading in the housing market now due to more planning proposals being passed with the local government's higher speed, there will be less trading later because less planning proposals need to be permitted in the future. Since almost 100% of the proposals are permitted eventually, the variation of the timing of approval only affects when the development can start.

Such rescheduling explanation implies that there is no welfare loss due to less efficiency from the government. This is different from the mechanism I want to highlight. The timing of when the development can start matters for the decision making of firms and household. If they learn that the development has to be postponed, they will cancel the investment in the development because the optimal timing of housing market will be passed.  Therefore, there will be a welfare loss due to the late approval of the planning proposal.

To test this alternative explanation, I conduct a placebo test where the dependent variable is the outcome of housing market and firm's borrowing 3 months after the application. By specifying 3 months, all applications in the cohort defined in monthly intervals have passed the 8-week deadline. In that time, higher speed as define in equation (3) will predict less workflows needed for the government. For the *rescheduling* effect explanation to be true, in the third month, reversed effects will be expected.

Before showing the placebo test results, to validate that speed is negatively correlated with the workflows after the 8-week deadline, I show the pattern in Figure 7. The figure plots the sensitivity of the approval rate each week after the application date to the speed measure. The sensitivity coefficients are positive before the deadline. It means according to the *speed* measure, more efficient local governments/ case officers do work more before the deadline. However, after the deadline, the sensitivity coefficients are negative. It simply suggests those efficient case officers work less after the deadline. The placebo test is just exploring if there is any effects of speed on the outcome variables in the month after the deadline. The results are shown in Panel B of Table 8. In contrast to the *rescheduling* effect explanation, there is no effect. So the effects documented in the main regressions is not a mechanical result of more approvals.

The evidence does prove that timing in the housing market is important for households and firms to make their decisions in trading and borrowing. The inefficiency of local governments in planning approval will hinder them to seize the optimal timing and therefore result in welfare loss.

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# Figures and Tables

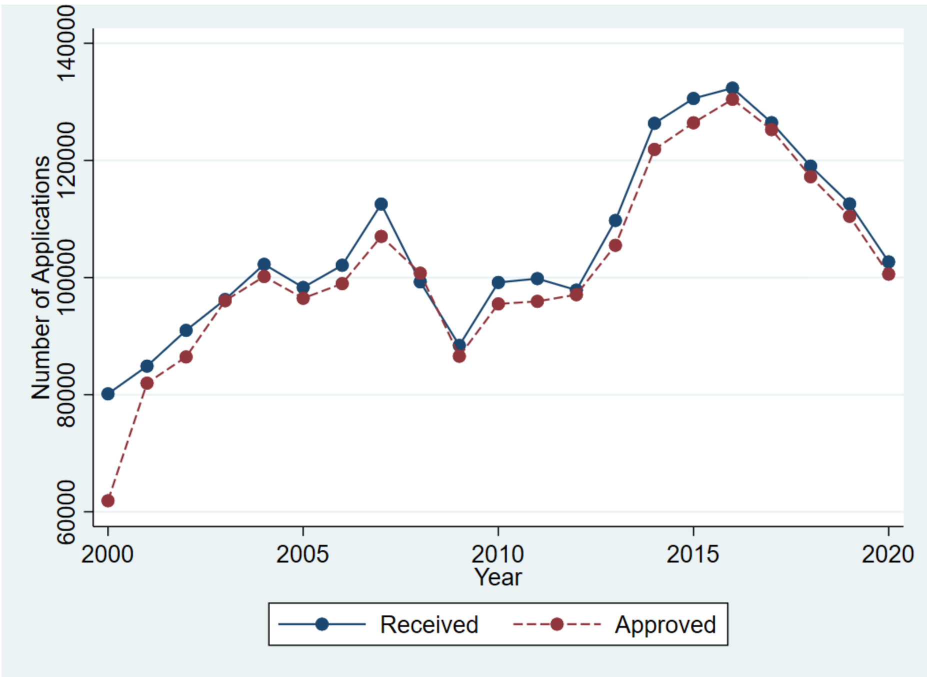



Figure 1: The Number of Applications by Year of Receiving /Approving

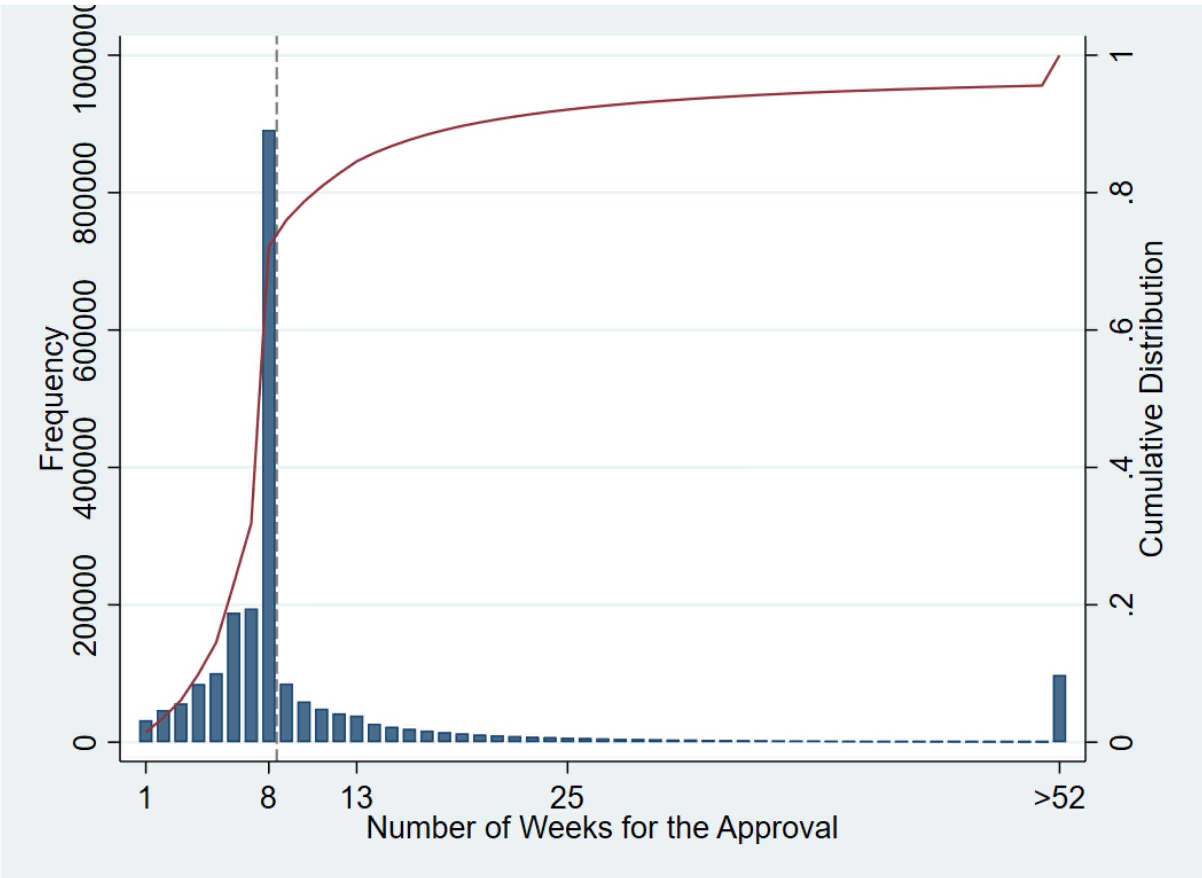
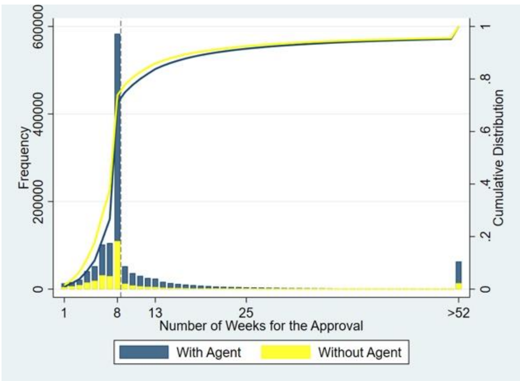
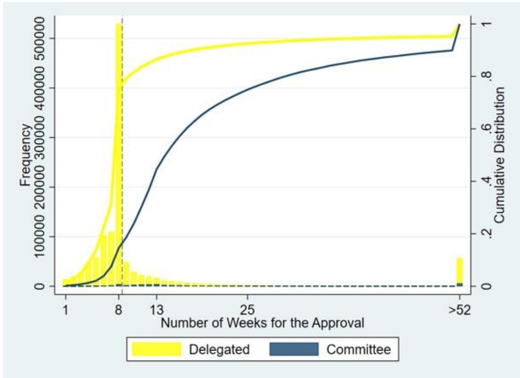



Figure 2: Distribution of Number of Weekes for the Approval

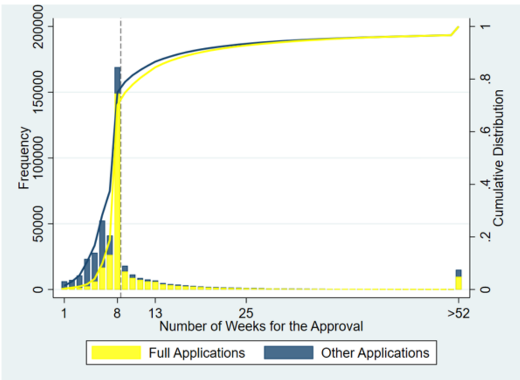
Notes: The figure plots the distribution of the number of weeks taken to issue the approval. The histogram (left axis) is the frequency of observations for each week. The line (right axis) plots the cumulative distribution of the time taken for approval. Applications taken more than 52 weeks are grouped in one category “>52 weeks”.

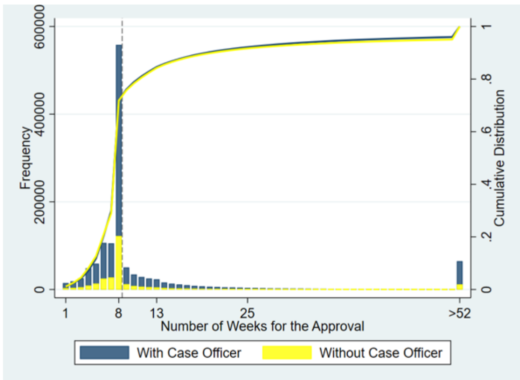



(a) With Agent v.s. No Agent

(b) Delegated v.s. Committee



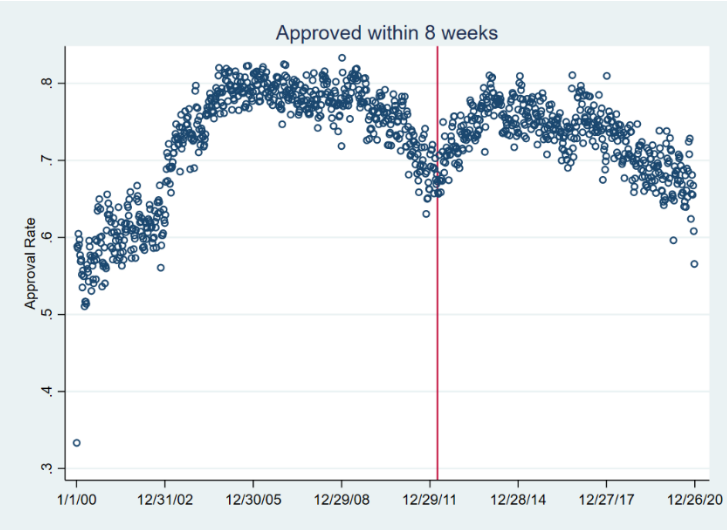




(c) Full v.s. Partial Application

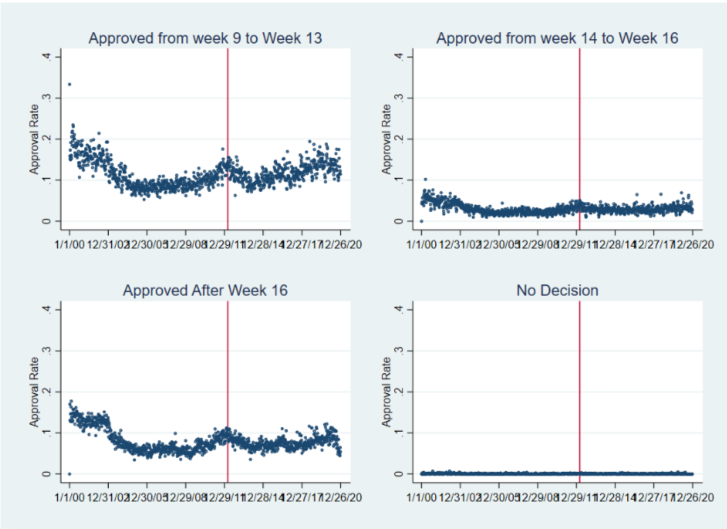
(d) With v.s. without Application.

Figure 3 Distribution of Number of Weekes for the Approval by Type of Applications

Notes: The figure plots the distribution of the number of weeks taken to issue the approval by the type of application. According to the existence of agents to assist the applicant, applications are divided into two subsamples as in Panel (a); by the decision-making process, applications are divided into reviewed by "delegated power" and "committee" as shown in Panel (b); . The histogram (left axis) is the frequency of observations for each week. The line (right axis) plots the cumulative distribution of the time taken for approval. Applications taken more than 52 weeks are grouped in one category ">52 weeks".




(a) 8-week deadline




(b) Other deadlines and no decision issuance

Figure 4 The Fraction of Applications Approved in Different Time Frame over Time

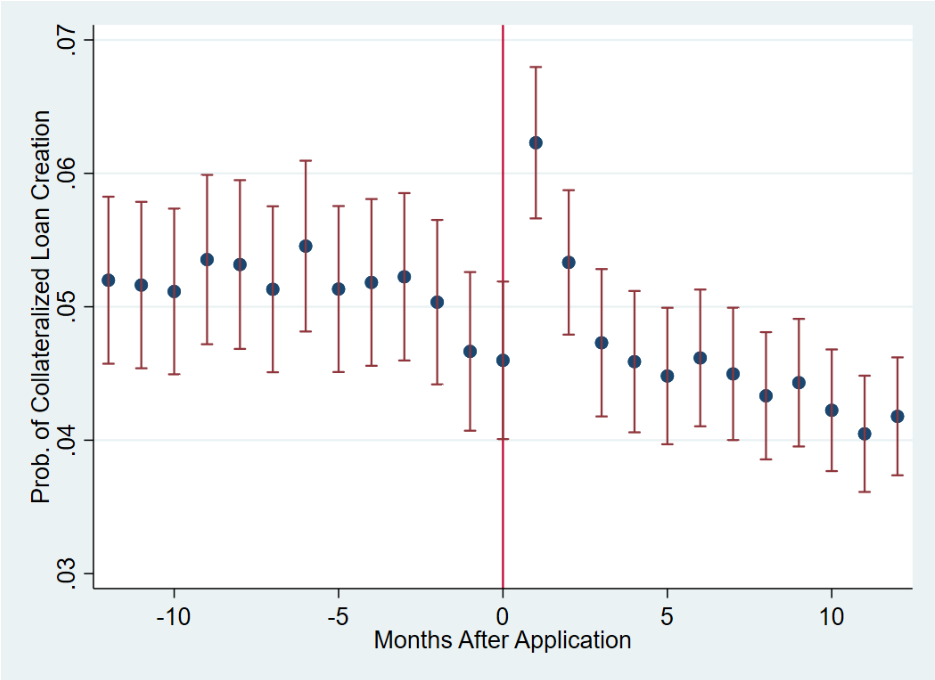
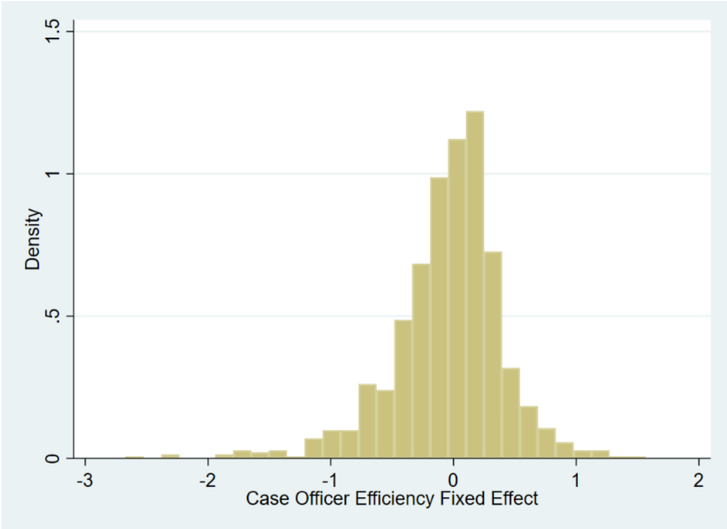
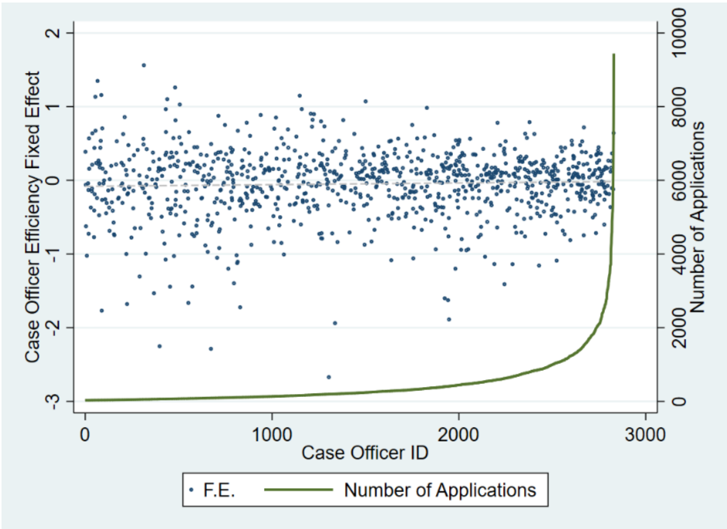



Figure 5 Creation of Collateralized Loans Around Planning Application Date

Notes: The figure plots the average rate of collateralized loan creation for firms that send planning applications to local authorities for properties they owned in London around the month of application. 95% confidence ranges are also shown for each average number.



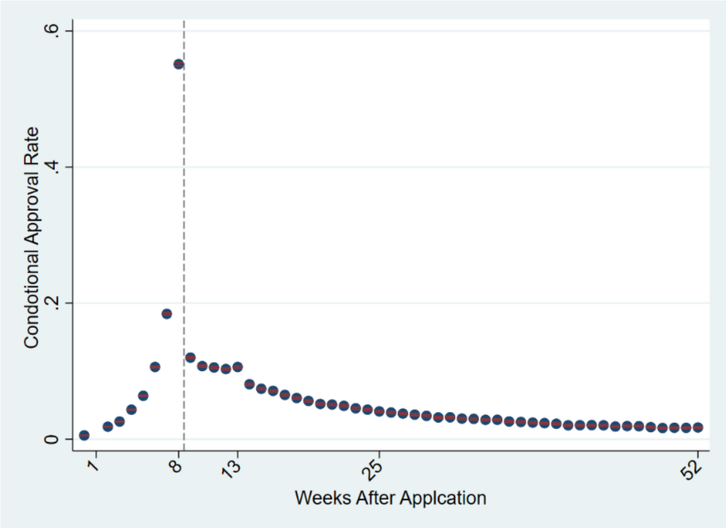

(a) The Distribution of F.E.



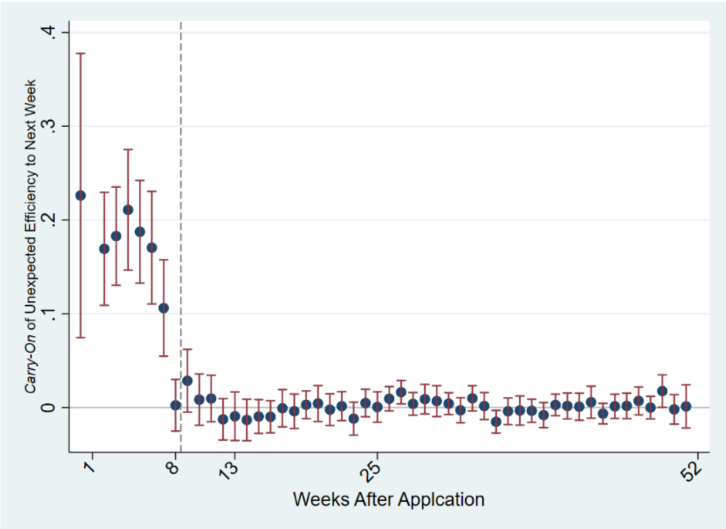

(b) Fixed Effects and Number of Applications

Figure 6 Case Officer Efficiency Fixed Effects

Notes: The figures plot characteristics of case officer efficiency fixed effects as obtained from equation (5). Panel (a) plots the distribution of the fixed effects. Panel (b) plots the relationship between fixed effects and the number of applications reviewed. In the horizontal axis of Figure 5(b), 2,829 case officers are sorted by the number of applications they reviewed. The dots are the fixed effects of case officers. The slope of the fitted line of the fixed effects against the ranking by number of applications is 0.000(*p-value*=0.201).




(a) Average Conditional Approval Rate




(b) The "Carry-On" of Unexpected Efficiency to Next Week

Figure 7 Conditional Approval Rate

Notes: The figures plot dynamic patterns for a given cohort (council-week) of applications. Panel (a) is the average approval rate of pending applications in different weeks after the application ("Conditional Approval Rate"). Panel (b) shows the "carry-on" of unexpected efficiency to next week. The coefficient is obtained by running the conditional approval rate of the next week on the conditional approval rate for the current week, controlling for council and week fixed effects. 95% confidence ranges of the coefficients are also shown in the plot.

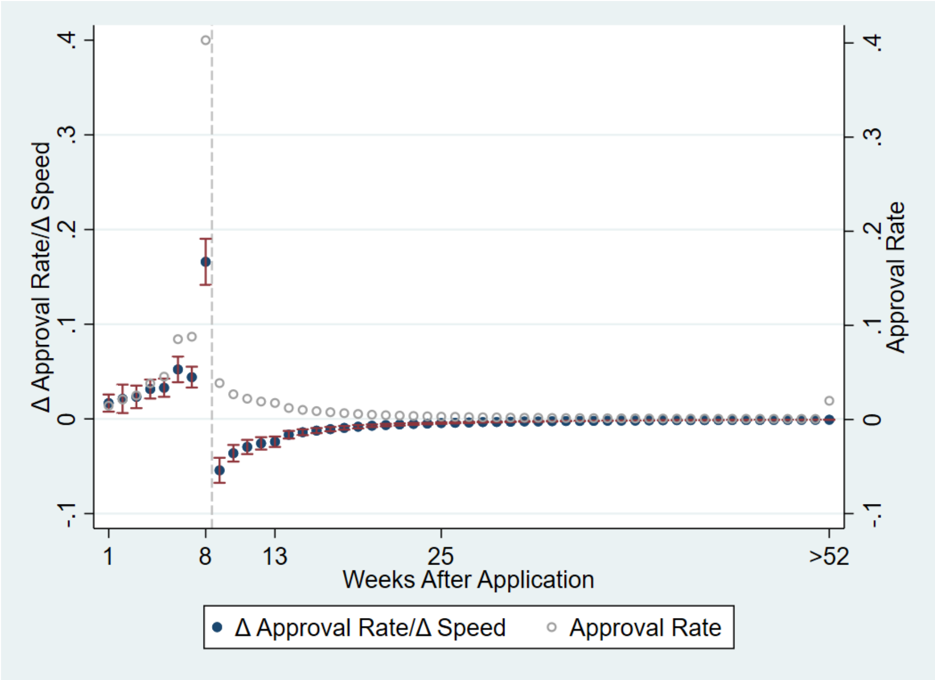



Figure 8 The Impacts of Speed on Workflows

Table 1: Summary of Planning Applications

Notes: The table shows the summary statistics for planning applications. Panel A is the summary of the individual applications. Panel B is the summary of the council-month cohorts of applications. Variables shown in the summary include the number of weeks taken for obtaining approval, *Speed* which is constructed according to the definition in equation (3), dummy variables indicating if the approval is issued within certain time frames, and application characteristics (applied with agent, decision made by committee, full application indicator, case officer is allocated). For application characteristics, not all councils provide the information via their online portal, so there are missings in these variables. In the cohort level summary, in addition to the variables mentioned above, the number of applications in each cohort is also summarized. Statistics in the summary include the observations, mean, standard deviation, minimum, maximum and the 25$^{th}$, 50$^{th}$ and 75$^{th}$ percentiles.

| *Stat.* | *Obs.* | *Mean* | *Sd* | *Min* | *P25* | *P50* | *P75* | *Max* |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Application Level** | | | | | | | | |
| *Time Efficiency:* | | | | | | | | |
| Number of Weeks for Approval | 2,157,846 | 10.85 | 22.84 | 1 | 6 | 7 | 8 | 1071 |
| Speed | 2,211,715 | -0.39 | 1.18 | -4.53 | 0.00 | 0.13 | 0.25 | 1.00 |
| Approved within 8 Weeks | 2,211,715 | 72.1% | 0.45 | | | | | |
| Approved from Week 9 to Week 13 | 2,211,715 | 12.4% | 0.33 | | | | | |
| Approved from Week 14 to Week 16 | 2,211,715 | 3.1% | 0.17 | | | | | |
| Approved After Week 16 | 2,211,715 | 9.9% | 0.30 | | | | | |
| *Application Details:* | | | | | | | | |
| With Agent | 1,620,513 | 81.2% | 0.39 | | | | | |
| Committee | 1,261,977 | 4.5% | 0.21 | | | | | |
| Full Application | 741,798 | 39.2% | 0.49 | | | | | |
| With Case Officer | 2,211,715 | 81.8% | 0.39 | | | | | |
| **Panel B: Council-Month** | | | | | | | | |
| *Time Efficiency:* | | | | | | | | |
| Number of Applications | 7,530 | 293.50 | 165.09 | 1 | 194 | 267 | 363 | 1538 |
| Number of Weeks for Approval | 7,530 | 11.42 | 5.33 | 0.71 | 8.68 | 10.06 | 12.45 | 154.00 |
| Speed | 7,530 | -0.45 | 0.47 | -4.53 | -0.61 | -0.33 | -0.16 | 0.91 |
| Approved within 8 Weeks | 7,530 | 73.9% | 0.13 | 0.00 | 0.68 | 0.77 | 0.83 | 1.00 |
| Approved from Week 9 to Week 13 | 7,530 | 10.6% | 0.07 | 0.00 | 0.06 | 0.09 | 0.13 | 0.67 |
| Approved from Week 14 to Week 16 | 7,530 | 2.9% | 0.02 | 0.00 | 0.01 | 0.02 | 0.04 | 0.50 |
| Approved After Week 16 | 7,530 | 10.0% | 0.08 | 0.00 | 0.05 | 0.08 | 0.13 | 1.00 |
| *Application Details:* | | | | | | | | |
| With Agent | 6,023 | 80.1% | 0.18 | 0.00 | 0.73 | 0.82 | 0.92 | 1.00 |
| Committee | 4,254 | 4.6% | 0.04 | 0.00 | 0.02 | 0.04 | 0.06 | 0.40 |
| Full Application | 2,496 | 38.4% | 0.31 | 0.00 | 0.00 | 0.43 | 0.67 | 0.89 |
| With Case Officer | 5,766 | 83.7% | 0.27 | 0.00 | 0.75 | 1.00 | 1.00 | 1.00 |

Table 2: Summary of Housing Market Information

Notes: The table shows the summary statistics for 1,213 postcode level housing markets in a monthly frequency from 2000 to 2020. Variables shown in the summary include the number of applications in the housing market for a given month, application intensity (the number of applications divided by the number of properties), time efficiency-related variables, application characteristics (applied with an agent, decision made by committee, full application indicator, case officer is allocated), the transactions in the housing market next month (number of transactions, the year-over-year log growth in the number of transactions, and annualized growth of price paid). For application characteristics, not all councils provide the information via their online portal, so there are missings in these variables. Statistics in the summary include the observations, mean, standard deviation, minimum, maximum and the 25th, 50th and 75th percentiles.

| *Stat.* | *Obs.* | *Mean* | *Sd* | *Min* | *P25* | *P50* | *P75* | *Max* |
|---|---|---|---|---|---|---|---|---|
| Number of Applications | 304,683 | 5.96 | 6.61 | 0 | 1 | 4 | 9 | 117 |
| Application Intensity | 304,683 | 0.02 | 0.02 | 0.00 | 0.00 | 0.01 | 0.03 | 1.00 |
| *Time Efficiency:* | | | | | | | | |
| Number of Weeks for Approval | 231,010 | 10.75 | 13.87 | 0.00 | 6.63 | 7.92 | 10.50 | 824.00 |
| Speed | 232,472 | -0.39 | 0.79 | -4.53 | -0.67 | -0.18 | 0.13 | 1.00 |
| Approved within 8 Weeks | 232,472 | 70.3% | 0.27 | 0.00 | 0.63 | 0.82 | 1.00 | 1.00 |
| Approved from Week 9 to Week 13 | 232,472 | 10.3% | 0.18 | 0.00 | 0.00 | 0.00 | 0.14 | 1.00 |
| Approved from Week 14 to Week 16 | 232,472 | 2.7% | 0.09 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| Approved After Week 16 | 232,472 | 9.3% | 0.18 | 0.00 | 0.00 | 0.00 | 0.13 | 1.00 |
| *Application Details:* | | | | | | | | |
| With Agent | 164,415 | 79.9% | 0.26 | 0.00 | 0.69 | 0.89 | 1.00 | 1.00 |
| Committee | 122,467 | 4.3% | 0.12 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| Full Application | 76,075 | 39.2% | 0.36 | 0.00 | 0.00 | 0.38 | 0.71 | 1.00 |
| With Case Officer | 232,472 | 81.8% | 0.32 | 0.00 | 0.75 | 1.00 | 1.00 | 1.00 |
| *Transaction Activities Next Month*: | | | | | | | | |
| Number of Transactions | 304,683 | 0.78 | 9.07 | 0.00 | 0.00 | 5.00 | 12.00 | 212.00 |
| Growth in Transactions | 304,683 | -0.03 | 0.61 | -4.60 | -0.34 | 0.00 | 0.27 | 4.98 |
| Annualized Price Growth | 209,653 | 0.11 | 0.22 | -3.25 | 0.05 | 0.08 | 0.14 | 7.67 |

## Table 3: Summary of Firms' Information

Notes: The table shows the summary statistics for 37,378 firms that own London properties in a monthly frequency from November 2017 to December 2019. Variables shown in the summary include the number of properties owned by the firm for a given month, application intensity (the number of applications divided by the number of properties), time efficiency-related variables, application characteristics (applied with an agent, decision made by committee, full application indicator, case officer is allocated), the borrowing behaviours of the firm next month (the value of total liabilities and long-term liabilities, the annual growth of them, the number of collateralized loans created and indictor of loan creation ). For application characteristics, not all councils provide the information via their online portal, so there are missings in these variables. If one firm has multiple properties, the time efficiency and application details variables are weighted by the value of the property. Statistics in the summary include the observations, mean, standard deviation, minimum, maximum and the 25th, 50th and 75th percentiles.

| *Stat.* | *Obs.* | *Mean* | *Sd* | *Min* | *P25* | *P50* | *P75* | *Max* |
|---|---|---|---|---|---|---|---|---|
| Number of Properties | 761,736 | 2.03 | 10.74 | 1 | 1 | 1 | 2 | 1334 |
| Value of Properties(Millions) | 761,736 | 169.67 | 1175.70 | 0.00 | 22.50 | 44.10 | 94.00 | 8287.50 |
| Application Intensity | 761,736 | 0.08 | 0.26 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| *Time Efficiency:* | | | | | | | | |
| Number of Weeks for Approval | 85,192 | 10.30 | 11.05 | 0.00 | 6.00 | 8.00 | 9.00 | 172.00 |
| Speed | 87,149 | -0.54 | 1.23 | -4.53 | -1.30 | 0.00 | 0.19 | 1.00 |
| Approved within 8 Weeks | 87,149 | 71.2% | 0.43 | 0.00 | 0.20 | 1.00 | 1.00 | 1.00 |
| Approved from Week 9 to Week 13 | 87,149 | 12.2% | 0.31 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| Approved from Week 14 to Week 16 | 87,149 | 3.2% | 0.17 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| Approved After Week 16 | 87,149 | 10.7% | 0.29 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| *Application Details:* | | | | | | | | |
| With Agent | 73,763 | 88.8% | 0.30 | 0.00 | 1.00 | 1.00 | 1.00 | 1.00 |
| Committee | 51,260 | 2.0% | 0.14 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |
| Full Application | 33,750 | 37.7% | 0.47 | 0.00 | 0.00 | 0.00 | 1.00 | 1.00 |
| With Case Officer | 87,149 | 95.9% | 0.19 | 0.00 | 1.00 | 1.00 | 1.00 | 1.00 |
| *Borrowing Behaviours:* | | | | | | | | |
| Long Term Liab. (Millions) | 550,632 | 38.34 | 1735.66 | 0.00 | 0.00 | 0.29 | 1.01 | 242521.00 |
| Log Growth in Long Term Liab. | 550,632 | 0.11 | 1.70 | -16.68 | -0.03 | 0.00 | 0.02 | 16.00 |
| Total Liab. (Millions) | 550,632 | 152.00 | 6806.15 | 0.00 | 0.32 | 0.81 | 2.18 | 1063610.00 |
| Log Growth in Total Liab. | 550,632 | 0.03 | 0.83 | -16.72 | -0.04 | 0.00 | 0.09 | 13.28 |
| Number of Loans Created | 761,736 | 0.05 | 0.73 | 0.00 | 0.00 | 0.00 | 0.00 | 137.00 |
| Positive Loan Creation | 761,736 | 0.02 | 0.15 | 0.00 | 0.00 | 0.00 | 0.00 | 1.00 |

Table 4 The Allocation of Applications to Case Officers

Notes: This table shows the allocation of applications to the 2,829 case officers. In Panel A, shows summary statistics of the number and share of applications for the case officers in each cohort (council-month). Panel B shows the randomness in the allocation of application details. F tests statistics and p-values (in the brackets) for the joint significance of case officer-month fixed effects are shown.

| Panel A: Allocation of Applications in Each Cohort | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *Stat.* | *Obs.* | *Mean* | *SD* | *Min* | *p25* | *p50* | *p75* | *Max* |
| Number of Applications | 97,968 | 12.95 | 12.21 | 1 | 4 | 10 | 19 | 568 |
| Share of Applications | 97,968 | 0.047 | 0.045 | 0.001 | 0.013 | 0.037 | 0.065 | 1.000 |

| Panel B: Randomness in the Allocation | | | |
|---|---|---|---|
| | Full Application | With Agent | Committee Flag |
| Obs. | 491,778 | 1,152,944 | 942,841 |
| F test for Case Officer-Month F.E. | 0.58 | 0.78 | 0.86 |
| | (1.00) | (1.00) | (0.94) |

Table 5 The Impacts of Application Types and Case Officer Fixed Effects on Speed

Notes: The table show results of the regression model in equation (5) that explores the impacts of application types on the speed of approval and the existence of case-officer fixed effects. In specifications (1)-(9), council-month cohort fixed effects are included. In specifications (5) to (9), Case Officer fixed effects are included. In addition, the table shows F-test results for the joint significance of individual case officer fixed effects with t statistics and p values (in the bracket).

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Full Application | -0.28*** | | | -0.23*** | | -0.26*** | | | -0.22*** |
| | (0.00) | | | (0.00) | | (0.00) | | | (0.00) |
| With Agent | | -0.10*** | | -0.05*** | | | -0.08*** | | -0.05*** |
| | | (0.00) | | (0.00) | | | (0.00) | | (0.00) |
| Committee Flag | | | -1.26*** | -1.38*** | | | | -1.17*** | -1.30*** |
| | | | (0.00) | (0.01) | | | | (0.00) | (0.01) |
| Constant | -0.42*** | -0.45*** | -0.41*** | -0.37*** | -0.47*** | -0.42*** | -0.47*** | -0.42*** | -0.38*** |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| N | 741,798 | 1,620,513 | 1,261,972 | 655,328 | 2,211,526 | 741,749 | 1,620,353 | 1,261,839 | 655,282 |
| R-sq | 0.163 | 0.144 | 0.191 | 0.209 | 0.198 | 0.217 | 0.211 | 0.247 | 0.253 |
| Cohort F.E. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Officer F.E. | | | | | Yes | Yes | Yes | Yes | Yes |
| | | | | F-Test for Case Officer F.E. | 46.107 | 41.790 | 42.675 | 36.232 | 36.321 |
| | | | | | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

Table 6 The Impacts of Speed on Housing Markets

Notes: This table shows the results of the main regression model (7) which examines the impacts of planning approval speed on housing market activities. In columns (1) to (3), the regression excludes terms related to the intensity of application. *OLS* means the regression has no controls; *F.E.* means the regression includes postcode and month fixed effects; IV uses the instrument constructed based on case officer speed fixed effects and at the end of each panel I show F-test statistics and p-values for the instruments. Each of the three panels has different outcome variable. Standard errors are clustered for housing submarkets (sector level postcode) and shown in brackets. *, **, *** are for 5%,1% and 0.q% significance levels respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | F.E. | IV | OLS | F.E. | IV |
| | | | Panel A: ln(1+Number of Transactions) | | | |
| *Speed* | 0.105*** | 0.037*** | 0.083* | 0.067*** | 0.025*** | 0.030* |
| | (0.004) | (0.004) | (0.040) | (0.005) | (0.004) | (0.013) |
| *Speed*Intensity* | | | | 2.170*** | 0.090*** | 0.099* |
| | | | | (0.099) | (0.026) | (0.043) |
| *Intensity* | | | | 15.448*** | 4.641*** | 1.096*** |
| | | | | (0.086) | (0.065) | (0.232) |
| *N* | 304,683 | 304,683 | 304,683 | 304,683 | 304,683 | 304,683 |
| *R-sq* | 0.002 | 0.719 | 0.072 | 0.132 | 0.741 | 0.074 |
| *F-Stat. for IV* | | | 141.000 | | | 36.164 |
| | | | (0.000) | | | (0.000) |
| | | | Panel B: Growth of Number of Transactions | | | |
| *Speed* | 0.003 | 0.015*** | 0.028** | 0.002*** | 0.013** | 0.021* |
| | (0.002) | (0.003) | (0.011) | (0.000) | (0.005) | (0.011) |
| *Speed*Intensity* | | | | 0.072*** | 0.075** | 0.089** |
| | | | | (0.024) | (0.032) | (0.036) |
| *Intensity* | | | | -0.233*** | -0.046 | 0.282 |
| | | | | (0.056) | (0.071) | (0.297) |
| *N* | 304,683 | 304,683 | 304,683 | 304,683 | 304,683 | 304,683 |
| *R-sq* | 0.000 | 0.124 | 0.013 | 0.000 | 0.157 | 0.035 |
| *F-Stat. for IV* | | | 141.000 | | | 36.164 |
| | | | (0.000) | | | (0.000) |
| | | | Panel C: Annualised Price Growth of Repeated Sales | | | |
| *Speed* | -0.030*** | -0.001 | 0.025 | -0.031*** | -0.003 | -0.015 |
| | (0.001) | (0.002) | (0.050) | (0.001) | (0.002) | (0.017) |
| *Speed*Intensity* | | | | 0.080*** | 0.068** | 0.051** |
| | | | | (0.024) | (0.024) | (0.018) |
| *Intensity* | | | | -0.163*** | 0.013 | 0.017 |
| | | | | (0.020) | (0.026) | (0.010) |
| *N* | 209,653 | 209,633 | 209,633 | 209,633 | 209,633 | 209,633 |
| *R-sq* | 0.004 | 0.091 | 0.019 | 0.004 | 0.090 | 0.053 |
| *F-Stat. for IV* | | | 153.020 | | | 35.450 |
| | | | (0.000) | | | (0.000) |

Table 7 The Impacts of Speed on Firms' Borrowing

Notes: This table shows the results of the main regression model (7) which examines the impacts of planning approval speed on firms' behaviours. In columns (1) to (3), the regression excludes terms related to the intensity of application. *OLS* means the regression has no controls; *F.E.* means the regression includes postcode and month fixed effects; IV uses the instrument constructed based on case officer speed fixed effects and at the end of each panel I show F-test statistics and p-values for the instruments. Each of the three panels has different outcome variable. Standard errors are clustered for housing submarkets (sector level postcode) and shown in brackets. *, **, *** are for 5%,1% and 0.q% significance levels respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | F.E. | IV | OLS | F.E. | IV |
| | | | Panel A: ln(1+Number of Loans) | | | |
| *Speed* | 0.004*** | -0.001** | 0.005* | 0.004*** | -0.001 | 0.019* |
| | (0.000) | (0.000) | (0.002) | (0.000) | (0.000) | (0.009) |
| *Speed*Intensity* | | | | 0.006*** | 0.006*** | 0.011* |
| | | | | (0.000) | (0.000) | (0.005) |
| *Intensity* | | | | -0.003*** | -0.004*** | -0.095*** |
| | | | | (0.001) | (0.001) | (0.004) |
| *N* | 761736 | 761736 | 761736 | 761736 | 761736 | 761736 |
| *R-sq* | 0.000 | 0.016 | 0.016 | 0.000 | 0.016 | 0.018 |
| *F-Stat. for IV* | | | 160.100 | | | 65.152 |
| | | | (0.000) | | | (0.000) |
| | | | Panel B:100*1(Number of Loans>0) | | | |
| *Speed* | 0.355*** | -0.157*** | 0.448** | 0.407*** | -0.105* | 0.430* |
| | (0.044) | (0.045) | (0.158) | (0.045) | (0.045) | (0.210) |
| *Speed*Intensity* | | | | 0.530*** | 0.531*** | 0.610*** |
| | | | | (0.042) | (0.042) | (0.038) |
| *Intensity* | | | | 0.169* | 0.284*** | 6.179*** |
| | | | | (0.071) | (0.070) | (0.285) |
| *N* | 761736 | 761736 | 761736 | 761736 | 761736 | 761736 |
| *R-sq* | 0.000 | 0.019 | 0.019 | 0.000 | 0.020 | 0.021 |
| *F-Stat. for IV* | | | 160.100 | | | 65.152 |
| | | | (0.000) | | | (0.000) |
| | | | Panel C: Annual Growth of Long-Term debt | | | |
| *Speed* | -0.018** | -0.012* | 0.037 | -0.020** | -0.013* | -0.056 |
| | (0.006) | (0.006) | (0.020) | (0.006) | (0.006) | (0.081) |
| *Speed*Intensity* | | | | 0.012* | 0.012* | 0.012* |
| | | | | (0.006) | (0.006) | (0.006) |
| *Intensity* | | | | 0.026** | 0.028** | 0.039 |
| | | | | (0.010) | (0.010) | (0.032) |
| *N* | 550632 | 550632 | 550632 | 550632 | 550632 | 550632 |
| *R-sq* | 0.000 | 0.012 | 0.012 | 0.000 | 0.012 | 0.012 |
| *F-Stat. for IV* | | | 325.050 | | | 107.380 |
| | | | (0.000) | | | (0.000) |
| | | | Panel D: Annual Growth of Total Debt | | | |
| *Speed* | 0.006* | 0.010*** | 0.013 | 0.006* | 0.010*** | 0.030 |
| | (0.003) | (0.003) | (0.010) | (0.003) | (0.003) | (0.039) |
| *Speed*Intensity* | | | | 0.002 | 0.002 | -0.010 |
| | | | | (0.003) | (0.003) | (0.021) |
| *Intensity* | | | | 0.006 | 0.007 | -0.014 |
| | | | | (0.005) | (0.005) | (0.015) |
| *N* | 550632 | 550632 | 550632 | 550632 | 550632 | 550632 |
| *R-sq* | 0.000 | 0.011 | 0.011 | 0.000 | 0.011 | 0.011 |
| *F-Stat. for IV* | | | 325.050 | | | 107.380 |
| | | | (0.000) | | | (0.000) |

Table 8 Placebo Tests of the Main Regressions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | ln(1+Number of Transactions) | Growth of Number of Transactions | Annualized Price Growth | ln(1+Number of Loans) | 100*1(Number of Loans>0) | Growth of Long Term Debt | Growth of Total Debt |
| | Panel A: Outcome variables 1 month before the application ($Y_{t-1}$) | | | | | | |
| *Speed* | 0.064 | -0.499 | -0.065 | -0.029 | -5.689 | -0.461 | -0.079 |
| | (0.157) | (0.302) | (0.075) | (0.051) | (3.598) | (0.280) | (0.076) |
| *Speed*Intensity* | -0.625 | 1.257 | 0.131 | 0.005 | 0.802 | 0.057 | 0.004 |
| | (0.455) | (0.786) | (0.202) | (0.015) | (0.844) | (0.056) | (0.024) |
| *Intensity* | 1.542*** | 0.607* | 0.125 | 0.020 | -4.365** | -0.089 | -0.034 |
| | (0.233) | (0.300) | (0.095) | (0.020) | (1.369) | (0.085) | (0.036) |
| *N* | 275,803 | 275,803 | 168,145 | 604,620 | 604,620 | 422,667 | 422,667 |
| *R-sq* | 0.001 | 0.049 | 0.011 | 0.001 | 0.002 | 0.002 | 0.000 |
| *F-Stat. for IV* | 35.500 | 35.500 | 33.730 | 62.337 | 62.337 | 98.504 | 98.504 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| | Panel B: Outcome variables 3 months after the application ($Y_{t+3}$) | | | | | | |
| *Speed* | -0.032 | -0.527 | -0.140 | 0.032 | -7.757 | -0.083 | 0.049 |
| | (0.158) | (0.304) | (0.095) | (0.057) | (3.612) | (0.178) | (0.077) |
| *Speed*Intensity* | -1.093 | 1.065 | 0.339 | -0.003 | 0.558 | -0.010 | -0.004 |
| | (0.557) | (0.591) | (0.222) | (0.018) | (0.848) | (0.053) | (0.023) |
| *Intensity* | 1.135*** | 0.503 | 0.215* | -0.007 | -5.786*** | -0.056 | -0.036 |
| | (0.234) | (0.303) | (0.100) | (0.023) | (1.376) | (0.084) | (0.036) |
| *N* | 295,803 | 295,803 | 190,500 | 627,717 | 627,717 | 452,398 | 452,398 |
| *R-sq* | 0.002 | 0.058 | 0.053 | 0.000 | 0.002 | 0.000 | 0.000 |
| *F-Stat. for IV* | 42.520 | 42.520 | 31.005 | 85.409 | 85.409 | 112.034 | 112.034 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |

# Appendix

Table A1 Summary Statistics of Application Information by Council

| *Council Name* | | *Number of Weeks for Approval* | *Speed* | *Approved within 8 Weeks* | *Approved from Week 9 to Week 13* | *Approved from Week 14 to Week 16* | *Approved After Week 16* | *With Agent* | *Committee* | *Full Application* | *With Case Officer* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Barking & Dagenham | *Obs.* | 24,459 | 24,475 | 24,475 | 24,475 | 24,475 | 24,475 | 24,475 | | | 24,475 |
| | *Mean* | 10.96 | -0.37 | 73.6% | 13.1% | 3.5% | 9.7% | 81.8% | . | . | 99.9% |
| Barnet | *Obs.* | 138,129 | 139,894 | 139,894 | 139,894 | 139,894 | 139,894 | | | | 139,894 |
| | *Mean* | 9.83 | -0.25 | 77.7% | 10.7% | 2.6% | 7.8% | | | | 88.7% |
| Bexley | *Obs.* | 51,206 | 52,045 | 52,045 | 52,045 | 52,045 | 52,045 | 52,045 | 52,045 | 52,045 | 52,045 |
| | *Mean* | 9.98 | -0.27 | 80.0% | 9.0% | 2.4% | 6.9% | 66.9% | 6.0% | 74.5% | 84.9% |
| Brent | *Obs.* | 71,685 | 72,393 | 72,393 | 72,393 | 72,393 | 72,393 | 72,393 | | | 72,393 |
| | *Mean* | 12.72 | -0.61 | 73.7% | 13.1% | 3.0% | 9.2% | 84.4% | | | 93.7% |
| Bromley | *Obs.* | 96,566 | 97,646 | 97,646 | 97,646 | 97,646 | 97,646 | 97,646 | 97,646 | 97,646 | 97,646 |
| | *Mean* | 12.14 | -0.54 | 71.0% | 17.9% | 3.2% | 6.8% | 74.7% | 8.7% | 65.0% | 63.2% |
| City | *Obs.* | 18,249 | 21,870 | 21,870 | 21,870 | 21,870 | 21,870 | | | | 21,870 |
| | *Mean* | 13.83 | -0.98 | 52.4% | 12.9% | 4.1% | 14.1% | | | | 83.2% |
| Croydon | *Obs.* | 86,122 | 87,130 | 87,130 | 87,130 | 87,130 | 87,130 | 87,130 | 87,130 | | 87,130 |
| | *Mean* | 10.08 | -0.27 | 73.2% | 13.6% | 3.1% | 8.9% | 63.2% | 3.0% | | 64.0% |
| Ealing | *Obs.* | 100,331 | 101,639 | 101,639 | 101,639 | 101,639 | 101,639 | 101,639 | 101,639 | 101,639 | 101,639 |
| | *Mean* | 16.63 | -1.11 | 68.4% | 13.7% | 3.0% | 13.7% | 81.6% | 2.8% | 42.7% | 40.9% |
| Enfield | *Obs.* | 73,197 | 74,168 | 74,168 | 74,168 | 74,168 | 74,168 | 74,168 | 74,168 | 74,168 | 74,168 |
| | *Mean* | 12.79 | -0.63 | 74.9% | 11.2% | 2.6% | 10.0% | 81.0% | 3.3% | 41.0% | 63.2% |
| Greenwich | *Obs.* | 59,454 | 60,327 | 60,327 | 60,327 | 60,327 | 60,327 | 60,327 | | | 60,327 |
| | *Mean* | 12.85 | -0.63 | 69.0% | 12.8% | 3.2% | 13.6% | 65.5% | | | 89.0% |

Table A1(Continued) Summary Statistics of Application Information by Council

| *Council Name* | | *Number of Weeks for Approval* | *Speed* | *Approved within 8 Weeks* | *Approved from Week 9 to Week 13* | *Approved from Week 14 to Week 16* | *Approved After Week 16* | *With Agent* | *Committee* | *Full Application* | *With Case Officer* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Hackney | *Obs.* | 58,508 | 58,656 | 58,656 | 58,656 | 58,656 | 58,656 | 58,656 | | | 58,656 |
| | *Mean* | 19.00 | -1.38 | 65.0% | 10.2% | 3.3% | 21.3% | 96.5% | | | 97.9% |
| Hamlets | *Obs.* | 44,551 | 45,262 | 45,262 | 45,262 | 45,262 | 45,262 | 45,262 | 45,262 | 45,262 | 45,262 |
| | *Mean* | 15.52 | -0.97 | 63.2% | 13.2% | 4.1% | 18.0% | 77.1% | 2.9% | 0.0% | 99.7% |
| Hammersmith & Fulham | *Obs.* | 70,655 | 71,757 | 71,757 | 71,757 | 71,757 | 71,757 | 71,757 | 71,757 | 71,757 | 71,757 |
| | *Mean* | 11.38 | -0.45 | 76.0% | 8.4% | 2.8% | 11.3% | 82.4% | 2.6% | 71.3% | 54.9% |
| Haringey | *Obs.* | 53,477 | 53,740 | 53,740 | 53,740 | 53,740 | 53,740 | 53,740 | 53,740 | | 53,740 |
| | *Mean* | 13.85 | -0.74 | 71.4% | 12.2% | 3.0% | 13.0% | 86.2% | 2.1% | | 58.9% |
| Harrow | *Obs.* | 36,553 | 38,224 | 38,224 | 38,224 | 38,224 | 38,224 | 38,224 | | | 38,224 |
| | *Mean* | 10.26 | -0.30 | 50.0% | 34.7% | 5.4% | 5.6% | 100.0% | | | 100.0% |
| Havering | *Obs.* | 57,062 | 57,218 | 57,218 | 57,218 | 57,218 | 57,218 | 57,218 | 57,218 | | 57,218 |
| | *Mean* | 11.64 | -0.46 | 75.9% | 11.4% | 3.0% | 9.4% | 100.0% | 6.3% | | 100.0% |
| Hillingdon | *Obs.* | 76,221 | 76,397 | 76,397 | 76,397 | 76,397 | 76,397 | 76,397 | | | 76,397 |
| | *Mean* | 14.94 | -0.87 | 71.9% | 10.7% | 3.4% | 13.7% | 100.0% | | | 99.9% |
| Hounslow | *Obs.* | 54,701 | 54,717 | 54,717 | 54,717 | 54,717 | 54,717 | | 54,717 | | 54,717 |
| | *Mean* | 11.29 | -0.41 | 72.4% | 13.0% | 3.6% | 10.9% | | 3.3% | | 100.0% |
| Islington | *Obs.* | 58,085 | 62,978 | 62,978 | 62,978 | 62,978 | 62,978 | 62,978 | 62,978 | | 62,978 |
| | *Mean* | 10.98 | -0.48 | 66.1% | 10.9% | 3.7% | 11.6% | 79.7% | 5.2% | | 100.0% |
| Kensington&Chels | *Obs.* | 98,483 | 103,016 | 103,016 | 103,016 | 103,016 | 103,016 | | 103,016 | | 103,016 |
| | *Mean* | 9.52 | -0.25 | 76.0% | 10.0% | 2.5% | 7.1% | | 5.4% | | 60.0% |

Table A1(Continued) Summary Statistics of Application Information by Council

| *Council Name* | | *Number of Weeks for Approval* | *Speed* | *Approved within 8 Weeks* | *Approved from Week 9 to Week 13* | *Approved from Week 14 to Week 16* | *Approved After Week 16* | *With Agent* | *Committee* | *Full Application* | *With Case Officer* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Kingston | *Obs.* | 59,981 | 61,146 | 61,146 | 61,146 | 61,146 | 61,146 | | | | 61,146 |
| | *Mean* | 10.99 | -0.40 | 72.2% | 13.5% | 3.3% | 9.1% | | | | 92.2% |
| Lambeth | *Obs.* | 84,037 | 85,122 | 85,122 | 85,122 | 85,122 | 85,122 | 85,122 | 85,122 | 85,122 | 85,122 |
| | *Mean* | 11.57 | -0.46 | 69.6% | 14.9% | 3.3% | 10.9% | 77.9% | 3.9% | 63.8% | 69.6% |
| Lewisham | *Obs.* | 51,925 | 52,579 | 52,579 | 52,579 | 52,579 | 52,579 | 52,579 | | | 52,579 |
| | *Mean* | 11.72 | -0.48 | 70.2% | 13.6% | 3.3% | 11.6% | 75.8% | | | 90.2% |
| Merton | *Obs.* | 63,712 | 64,492 | 64,492 | 64,492 | 64,492 | 64,492 | 64,492 | 64,492 | | 64,492 |
| | *Mean* | 10.68 | -0.36 | 77.0% | 11.0% | 2.5% | 8.3% | 100.0% | 3.3% | | 100.0% |
| Redbridge | *Obs.* | 86,470 | 86,470 | 86,470 | 86,470 | 86,470 | 86,470 | 86,470 | | | 86,470 |
| | *Mean* | 9.72 | -0.21 | 77.0% | 11.1% | 3.4% | 8.4% | 79.2% | | | 99.8% |
| Richmond | *Obs.* | 96,585 | 96,686 | 96,686 | 96,686 | 96,686 | 96,686 | 96,686 | 96,686 | | 96,686 |
| | *Mean* | 9.90 | -0.24 | 76.2% | 13.3% | 2.8% | 7.6% | 78.2% | 3.2% | | 100.0% |
| Sutton | *Obs.* | 40,361 | 40,559 | 40,559 | 40,559 | 40,559 | 40,559 | 40,559 | 40,559 | 40,559 | 40,559 |
| | *Mean* | 9.33 | -0.17 | 81.3% | 10.0% | 2.4% | 5.9% | 44.9% | 3.8% | 22.5% | 95.4% |
| Waltham | *Obs.* | 43,985 | 46,748 | 46,748 | 46,748 | 46,748 | 46,748 | 46,748 | | | 46,748 |
| | *Mean* | 10.85 | -0.43 | 69.4% | 12.3% | 3.5% | 8.8% | 81.8% | | | 99.8% |
| Wandsworth | *Obs.* | 94,231 | 113,802 | 113,802 | 113,802 | 113,802 | 113,802 | 113,802 | 113,802 | | 113,802 |
| | *Mean* | 8.68 | -0.26 | 66.4% | 8.9% | 2.8% | 4.7% | 87.8% | 7.8% | | 97.9% |
| Westminster | *Obs.* | 208,865 | 210,559 | 210,559 | 210,559 | 210,559 | 210,559 | | | | 210,559 |
| | *Mean* | 9.60 | -0.21 | 73.5% | 12.2% | 3.4% | 10.1% | | | | 83.5% |

# Chapter III

## Does Buying Local Spur Corporate Investment?

# Does Buying Local Spur Corporate Investment?

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**Abstract**

To spur the growth and investment of small businesses, local governments give preferential treatment to local suppliers in their purchases. Given that, I examine if sales to the local government result in more physical capital investment than sales to nonlocal governments. I construct a novel and granular data set of the purchases of 308 councils in England with corporate suppliers in monthly frequency from 2011 to 2020. First, I document that compared to non-local councils, suppliers receive more specialised contracts from the local council and maintain a more persistent customer-supplier relationship with it. Next, to identify the causal relationship between local sales and suppliers' outcomes, I exploit exogenous demand shocks with spatial fixed effects on the councils' boundaries. I find that local sales reduce the uncertainty of firms' cash flows while keeping expected cash flows unchanged. It implies that the customer-supplier relationship with the local council is not just more persistent but also more exclusive. Consequently, I find local sales result in 9.7% higher annual growth in fixed assets than sales to non-local sales. The results suggest that the uncertainty in cash flows, one underexplored channel, helps explain government purchases' impacts on suppliers. Overall, this paper highlights some novel patterns of governments' purchases and their effects on firms' growth and investment.

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# 1. Introduction

While the role of direct government purchases in boosting firm growth and investment, especially for small businesses, has been appreciated in some recent papers [1], little is known about the demand-side patterns specific to governments and their relevance to the observed effects on the suppliers' behaviours. Demand-side factors are important in the shaping of young firms' growth and investment path (For example, Foster et al. (2016) and Syverson (2011)). What makes the effects of government purchases on suppliers intriguing may rely on how the government entities purchase. The scarcity of the demand-side perspective in the literature is mainly due to limited micro and comprehensive records of the purchases of governments at the firm level. In this paper, I construct a novel data set of purchases of councils (the local government bodies) in England, and document the preferential treatment of councils towards local suppliers and the impacts of such "Buy-Local" policies on firms' behaviours.

The purchases of councils in England provide an ideal setting for studying the purchases patterns. Councils are significant customers for firms in England. In 2019, the total purchases of councils from firms in England is £68 billion and 65% the suppliers are small and medium businesses. Each of the 308 local councils can be regarded as an autonomous customer. They are geographically exclusive, have own local elections and decision committee (councillors), make fiscal budget and spend independently. These features make them distinctive customers whose purchases with individual firms can be explicitly identified and tracked throughout the history of customer-supplier relationship. More importantly, councils' spending is unlikely to be confounded with the taxation policies. Councils in England have limited revenue-raising powers as only 12% of the taxes are collected locally and the rates are not set by local authorities.

As the first step in the paper, I document evidence that Councils treat local and non-local suppliers differently. First, contract terms for local suppliers are more specialized in one single category of products/services. Second, the supplier-customer relationship with local council persists longer. The existence of local preferential treatment is not surprising since government bodies' spending behaviours are intensively regulated by political incentives. There is a sharp gap between local and non-local suppliers in terms of political incentives for councils because the election of councillors are elected locally. Previous research (for example, Brogaard et al. (2015) and Schoenherr (2019)) have provided evidence on the association of political connection with distortive allocation of procurement contracts. In contrast to the ex ante selection discussed in these papers, my paper focuses on the ex post differences in contract terms and follow-up sales between customer-supplier relationships initiated by local and non-local

[1] For example, Cohen and Malloy (2016), Ferraz et al. (2015) and Hvide and Meling (2020).

government purchasers. It's important to highlight this distinction because I aim to study the causal relationship between ex post local preferential treatment and firms' behaviours which might confound with the differentiated ex ante selections of suppliers' characteristics.

Establishing the causal relationship between local preferential treatment and suppliers' investment is difficult. It's naive to simply regress firms' outcomes on the firm's share of sales to local council in its sales to all councils. It suffers from two endogenous problems. First, as mentioned above, there is a great chance that councils have different ex ante selection criteria between local and non-local firms. Then results observed for suppliers with greater sales exposure to local council might be attributed to heterogeneity in firm characteristics before the sales. Second, mechanically local and non-local councils have different distances to the supplier. Distance increases the transaction costs and information asymmetry which reduces the profitability of local sales and make it difficult to maintain the customer-supplier relationship.

Ideally, the empirical strategy should allow me to compare two identical firms that are geographically approximate and randomly assigned procurement contracts from the local council and a non-local council respectively. To implement this idea of identification, I take the following steps. First, I restrict comparison to be conducted among suppliers within small geographical regions. This ensures that for any pair of suppliers in comparison, they are equally distant to any council. Another merit of such "spatial fixed effect" is to alleviate the concern that other confounders that might be spatially heterogeneous, for example, labour market conditions which are usually localized ( Manning and Petrongolo (2017)).

Second, I exploit sales to councils induced by demand shocks from councils.I find that councils' demand shocks are uncorrelated with any differential selection in firm characteristics and contemporaneous contracts' terms between local and non-local suppliers. It means in times of unexpected demand shock, councils' selection of suppliers is uniform to any suppliers, regardless of their locality to the council. The second step solves the ex ante selection problem. To construct council level demand shocks, I take advantage of the disclosure of fiscal budget of each council in October of the preceeding fiscal year. A large positive gap between budgeted and actual aggregate spending suggests unexpected demand for public goods and services, which possibly result from natural disasters and pandemics. For example, in 2020, due to the outbreak of COVID-19, the unexpected spending growth is much higher in councils with more elderly population.

I therefore combine both building blocks in the empirical strategy. Consequently, the identification compares suppliers located within narrow distance to councils' boundaries on the two sides. The two groups of firms may have different levels of share of sales to the local council. I exploit the proportion of difference that is exogenously induced by demand shock from one of the councils near the boundary. This is similar to a “diff-in-diff” empirical design. I can test the exogeneity of demand shocks by checking the parallel trends.

With the identification strategy, I first investigated the impacts of sales to the local council on the ex post evolution of total sales of the supplier. I focus on both total sales to the public sector (all councils) and the private sector (the difference between total firm sales and sales to all councils). I find that over the next three years, there is no significant effects on the annual growth rate of both sales to total public sector and private sector. However, I do find evidence that sales to local council reduces the uncertainty of sales growth at aggregate level. I use absolute values of growth in sales as a measure of volatility. Both the total public sector sales and total firm sales are becoming less volatile following a procurement contract with local council.

The results about total sales are consistent with the local preferential treatment documented. On the one hand, more persistent customer-supplier relationship with local council increases the expected cash flows. On the other hand, over specialized contracts from local council limit the scope and capacity of suppliers and crowd out the sales to other councils and possibly private sector as well. The combined effects lend no advantage for local sales to guarantee higher expected cash flows in the future but it does reduces the uncertainty in future cash flows.

It then leaves the question of local council's purchases on supplier's investment intriguing. Any causal effects observed in firm's behaviours can not be attributed to the quantity. There are papers documenting evidence on the positive effects of the quantity of government purchases on suppliers investment ( Cohen and Malloy (2016), Ferraz et al. (2015) and Hvide and Meling (2020)) and they underscore government purchase's role in alleviating financial constraint. The results found on the uncertainty of cash flows suggest another under explored role of local government's purchase.

Then I turn to use the empirical strategy to investigate the effects of local council' purchases on firm's financial and investment decisions. First, I investigated suppliers' growth in total assets. On average, compared to non-local purchases, local purchases result in annual total asset growth by 9.70% in a period of three years to follow. This magnitude is relatively sizable which is also sufficient to move a firm located in the median to $75^{th}$ percentile in the distribution. Growth in fixed assets is the major contribution. Second, I also explored the financing decisions. While there is no significant changes in the leverage (the ratio of total liabilities to total asset), the share of long term debt increases. Third, the impacts on survival (dormant, dissolved, bankrupt or stopped filings ) rate within one year after the contract ends is modestly negative. The survival rate is reduced by -4.53%, in comparison to the unconditional survival rate of 75.02% among all councils' suppliers in their first year after the most recent procurement contract ends.

The evidence above shows that government purchases spur suppliers' investment via an understudied channel: addressing the uncertainty concerns faced with firms. In theory, uncertainty is indeed a significant factor for firms' investment and financial decision (for example Bloom et al. (2007) and Bloom (2009)). Yet, it's surprising that the effects can be so economically significant. It indicates that small and medium firms in England are constrained by "uncertainty". The significance of uncertainty to the decision making of small businesses are in particular important for at least two reasons. First, for small businesses, the fixed asset investment are relatively more irreversible than large firms. Ghosal and Loungani (2000) highlights that the investment-uncertainty relationship is in particular more negative for small firms. Second, owners of small businesses have exaggerated expectation of uncertainty ( Bloom et al. (2020)). In line with research about demographics and risk-taking (for example, Kerr et al. (2017) and Dohmen et al. (2017)), I find the effects of sales to the local council are more pronounced for suppliers whose directors are female and elder.

Finally, I investigated the impacts of political turnover at council level on suppliers' sales, investment, and financing. I focus on councils experiencing unexpected political turnover [2] and study whether it alters the effects I identified earlier. This exercise serves two purposes. First, it test if political incentive is indeed the key motive of local preferential treatment to suppliers. I do find that unexpected political turnover predict a higher likelihood of break-up of local customer-supplier relationship. Second, it informs us if firms act in accordance with the anticipation of future cash flows. I find that before the ongoing contract ends, the unexpected political turnover weakens the effects of local sales on firm's investment.

Even though I manage to build a causal relationship between sales to the local council and suppliers' behaviours, there are at least two major limitations no note. First, the findings are marginal effects. For each £1 of sales to local council, the effects on suppliers is using £1 sales to non-local councils as benchmark. I can't causally measure the overall effect of £1 government spending as previous papers have done ( Cohen and Malloy (2016), Ferraz et al. (2015) and Hvide and Meling (2020)). Second, I am unable rule out all alternative possible channels that explain the effects. With the identification strategy, I am confident to rule out transaction costs and information advantage as the channels that explain the positive effects of sales to the local council and firm investment in this setting. In addition, the patterns in the quantity of sales and total debt don't support financial constraint as the explanation. Of course, there are other possible channels. For example, by being a supplier to local council, a firm has better access to credit market and labour market. However, in the paper, I have used multiple tests to confirm that reduction in the uncertainty in cash flows is a major explanation. The uncertainty

[2] Unexpected political turnover events are local elections that changed the ruling party and the advantage of the ruling party over the second largest party is below 15%, in terms of the fraction of councilors.

of growth in total sales are indeed more volatile. It's consistent with councils' local preferential treatment in contract terms and customer-supplier relationship. Moreover, I find heterogeneous results on gender and age of firms' directors that are consistent with attitudes towards uncertainty.

My paper contributes to the literature about the demand side factors' influence on firms' behaviours. Demand is as important as productivity in explaining firm growth patterns. Our findings are consistent with those of Pozzi and Schivardi (2016) and Foster et al. (2016), who highlight the importance of demand factors and shocks in explaining firm dynamics. In their setting, heterogeneity of demand factors or the population characteristics of consumers are assumed in functional form. In my paper, I give an exact example of heterogeneous demand that can be explicitly characterized. There are a few papers focusing on the *local preference* similar to my setting. Except for those contexts of financial markets [3], micro examples of demand preference in the product market is rare. Bronnenberg et al. (2012) tracks the brand preferences of immigrants and highlights the importance of preference for incumbent firms' advantage. My paper is distinguished to these papers, because I have detailed information about both heterogeneous demand characteristics and suppliers' behaviours.

This paper is closely related to an extensive literature quantifying the impacts of government spending on firms in the private sector. There are in general two categories of research under this topic. The first type of research takes macro perspectives and study the impacts of government spending on broader economy as a whole. For example, government spending affects the wage and cost of capital (For example, Alesina et al. (2002) and Ramey (2013)). The second type provides firm level evidence. They focus on firms with direct links with the government. Non-pecuniary links (usually social networks between firms' directors and politicians), often termed as "political connections", are believed to bring privileges to the firms [4] and shape their investment strategies and performance ( Akcigit et al. (2018)).Pecuniary links, namely supplier-customer relationship, are less studied due to data constraint. Among the few of them, Ferraz et al. (2015) studies the impacts of obtaining federal government's procurement contracts on firm growth in Brazil; Cohen and Malloy (2016) investigate the behaviours of government dependants using information about major government customers disclosed by public corporations in the US; Hvide and Meling (2020) use Norwegian road procurement data to show even

[3]See examples about stocks selection and funds management in Hau and Rey (2008), Coval and Moskowitz (2001), Van Nieuwerburgh and Veldkamp (2009). Wolf (2000) on home bias in intranational trade.

[4]The range of benefits provided by governments to favored firms include preferential access to credit ( Cull and Xu (2005); Johnson and Mitton (2003); Khwaja and Mian (2005)); preferential treatment by government-owned enterprises ( Dinc (2005)) and for procurement ( Goldman et al. (2013)); relaxed regulatory oversight of the company in question or stiffer regulatory oversight of its rivals ( Kroszner and Stratmann (1998)); lighter taxation ( Arayavechkit et al. (2018)); allocation of public subsidies to R&D ( Fang et al. (2018)); and government bailouts of financially distressed firms ( Faccio and Parsley (2006)).

temporary demand shocks have long-term impacts on startups.

Furthermore, this paper is related to literature about procurement. The majority of these papers focus on efficiency of the design of procurement contracts or the delivery of public goods and services. For example, Decarolis et al. (2021) discuss how buyers can affect the performance of the contracts; Decarolis et al. (2020) the relationship between bureaucracy competence and procurment outcomes; Bajari and Lewis (2009) discussed the relevance of time incentives for procurement costs. In contrast to them, I focus on the impacts of heterogeneous contracts characteristics on suppliers' behaviours.

The paper is structured as follows. The next section discusses the background, the construction of the data set. In section 2, I construct the key measures and present the summary statistics. In section 3, I document evidence of the local preferential treatment in councils' purchases. Then in section 4, I detail the empirical strategy for identifying causal impacts of local sales and show the main results. In Section 5, I present discussions about the explanations and robustness checks. Section 6 provide concluding remarks.

## 2. Data

### 2.1 Institutional Background

I focus on the spending behaviours of local governments in England. Under the Parliament and Government of the United Kingdom, there are principal councils responsible for providing public goods and services to local areas. Two patterns of local government are in use. Some areas are governed by two levels of local governments. The county council is responsible for services such as education, waste management and strategic planning within a county, with several non-metropolitan district councils responsible for services such as housing, waste collection and local planning. In other areas, only one level of local government which are called unitary authorities. London Principal councils are elected in separate elections. For the convenience of the empirical design to follow, I exclude the county councils in the double-tier areas such that all the councils in our sample are geographically exclusive.

Councils in England are generally constrained by funding sources but flexible in spending decisions. In 2018/19, local authorities in England received 31% of their funding from government grants, 52% from council tax, and 17% from retained business rates – revenue from business rates that they do not send to the Treasury.Unlike central government, local authorities cannot borrow to finance day-to-day spending, and so they must either run balanced budgets or draw down reserves – money built up by underspending in earlier years – to ensure that their annual spending does not exceed their annual revenue. But reserves can only be used once. Once reserves are spent, they cannot be spent again.Only about 12% of the UKs taxes were collected, or intended to be collected, locally in 2014.

In addition, the public is made accessible to information about the councils' spending by legislative transparency codes. Initiated by the Localism Act 2011, the local government transparency code is issued to meet the government's desire to place more power into citizens' hands to increase democratic accountability. Among the several data items, "How money is spent" is the most salient information required to be disclosed. All spending transactions over £500, and contracts valued over £5,000 [5] are published in monthly frequency and independently by each individual council. To ensure enforcement,the Department for Communities and Local Government (DCLG) is to withhold funding from a district council in response to alleged transparency failings.

## 2.2 Data Source

There are three major data sets used in this paper: (1) monthly transactions between councils and suppliers;(2) contract characteristics;(3) firm financial information. First, I scraped monthly expenditure details from each single local authority's website or by FOIA request. I end up with results of 308 geographical exclusive local authorities which almost covers the whole England from 2011 to 2020. [6]. The common set of information available from different local authorities includes the name of the supplier,the invoice date [7] and the amount of payment [8]. Some local governments also disclose the registration number and postcodes of the suppliers; some local governments publish expenditure for capital account (which the purpose of the expenditure is for investment) and I excluded them. The supplier's identity is mainly determined by its name. I adopt fuzzy match technique to find the unique registration number using the names filed with Companies House.The match results are modified by postcode and disclosed registration number if the information is available. Each council-supplier-month observation

[5] The threshold was updated to £250 from 2014. The adoption of the threshold in practice varies across different local authorities. See Appendix for detailed description of the coverage of the data.

[6] I exclude the 30 upper-tier county councils as they provides part of the public services and geographically overlap with lower-tier councils.In addition, there are a few cases of consolidation and division of local governments during the sample period. See Appendix for the discussion of these issues.

[7] The gap between invoice data and transaction date is small. According to the Prompt Payment Policy, the government commits to pay 90% of undisputed and valid invoices from SMEs within 5 days and 100% of all undisputed and valid invoices to be paid within 30 days.

[8] Through out the paper, I use the amount net of VAT which is also the common format for information disclosure by local governments. Where only gross amount including VAT is available, I assume the standard VAT rate 20% is adopted.

is regarded as one transaction between a council and a supplier.

The data set is of great novelty which complements the increasingly popular transaction-level database to answer economic and financial questions [9]. Most of them are transactions between household customers and consumer-facing firms. Usually only a small number of the suppliers in their setting can be matched with other firm characteristics. The transaction database I constructed contains information 308 government bodies as customers and more than 384,000 suppliers of disperse sizes and mostly private. The suppliers in my database are limited to certain sectors,such as construction, health, administration and professional services (See Table B7) which is complementary to the coverage of household transactions database. Another feature of my transactions database is it keeps a more comprehensive record of the customers' history of consumption as apposed to household consumption recorded by debit/credit card spending.

The second data set is a sample of procurement contracts disclosed by local authorities on their own websites or centralised platforms such as Contracts Finder. The disclosure of procurement contracts is of poorer quality than the transaction information due to slack requirement for disclosing contracts, especially those with small values [10]. The raw information extracted from all available sources are standardized such that I end up with a sample of 289,489 contracts with information about the contract value,start and end dates, classification (CPVs), the awardees (suppliers) and government bodies offering the contracts. In general, the contracts data set is a subset of expenditure of local governments but it complement the transactions data set by offering more "real" aspects of the cash flows, for example ,the nature of the services and/or goods provided (CPVs).

The third source of information is Companies House, which provides financial information about the firms.Firms incorporated in England are required to disclose their key financial information regardless if the firm size, including industry, address, directors' information, incorporation date, total asset and asset's composition, and capital structure. Depending on the size of the firms,some may also disclose information on the sales and profits.

Other information used in the paper includes the geo-political information at local level. Local elections results by candidates are from Local Elections Archive Project [11]; Business Rates

[9] Papers using financial transaction data include Bachas et al. (2021), Agarwal and Qian (2014), Agarwal et al. (2017), Medina (2021), Aydin (2021), Baker et al. (2020), Olafsson and Pagel (2018), Ganong and Noel (2019) and etc.

[10] For example, contracts with value over £10,000 are required to be disclosed and advertised on the centralised national platform Contracts Finder. Contracts with higher values are also advertised on regional platforms sponsored by several local councils.

[11] http://www.andrewteale.me.uk/leap/

from the Valuation Office Agency [12]; and local authorities' boundaries from Office for National Statistics.

### 2.3 Sample Size

As a preliminary characterization of the data sets, I will summarize the sample size in different dimensions.The most granular observation among the data sets are at $supplier \times council \times month$ level (or $supplier \times council \times contract$ level for procurement data). Each observation can be regarded as an occurrence of cash flows for each $supplier \times council$ pair. There are a total of 15,597,722 observations from the cash flows information and 294,235 observations from the procurement information. I count the number of observations for each $supplier \times council$ pair and each $supplier$. It's helpful to generally picture the frequency of cash flows and contracts at both supplier-council and supplier levels.

In Table 1, its shows the number of observations for each supplier-council pair and each supplier respectively in two panels. From Panel A, we know there are 1,739,964 supplier-council pairs with an average of 9 observations of monthly cash flows; for procurement contracts, there are 102,634 supplier-council pairs with about 2.9 contracts on average.For these pairs, local pairs have more occurrences of cash flows and contracts.

In Panel B, I show the count of observations for each of 384,322 suppliers in the cash flows data and 30,091 suppliers in the contracts data. On average, a supplier has 40.6 observations of monthly cash flows and 9.8 contracts. Out of them, cash flows from the local customer has about 10.5% and contracts from local customer is about 14%. These numbers suggest local customer is dominating the customer pool (The benchmark for irrelevance of locality is each customer has a share of 1/308).

Another thing to highlight is the skewness in the distribution of sample size per supplier-council pair or each supplier. For a median supplier-council pair, there are only three times of cash flows and one contract documented over the period from 2011 to 2020; for a median supplier, there are only four times of cash flows and 2 contracts documented. It means for the majority of suppliers in our sample, their transaction history with the councils are short-lived. Therefore, the interpretation of the results are not only related to intensive margin but also extensive margin variations in sales to local governments.

[12] https://voaratinglists.blob.core.windows.net/html/rlidata.htm

## 2.4 Validity of the Data Sources

In this section, I will study the relationship between three data sets:(1) collected granular spending data set;(2) the procurement contracts data; (3) the aggregate expenditure data (both budgeted and actual). I aim to show that the two collected data sets are representative samples of the universe of all expenditure of local governments. In addition, I will show some general macro patterns about the fiscal expenditure of local governments in UK.

### 2.4.1 Relationship between Monthly Transactions and Aggregate Expenditure

The collected granular transactions data set may not be representative for the universe of all fiscal expenditure for two reasons. First, the disclosure policy of local government spending is adopted by different local authorities in different years and the threshold of qualified spending is varying across different local authorities. Second, the providers of goods and services to local governments are not limited to for-profit corporations. The aggregate expenditure includes internal transfers, transactions with non-corporate entities (government bodies and NGOs). Figure B1(a) plots the average cumulative growth of aggregate fiscal expenditure for all councils. It has been declining since 2011. Also there is a gap between actual spending and planned spending. However, as shown in sub-figure B1(b), the total value of all transactions from collected data has been increasing which is due to more and more transparent information disclosure and staggered adoption of the transparency policy.

Another way to validate the collected data is to study the sensitivity of growth in monthly spending (aggregated using transactions data) to the growth in annual aggregate expenditure (reported data). I study this sensitivity from two perspectives. First, by each month within one fiscal year, I show that the sensitivity of monthly spending growth to aggregate expenditure varies across months in Figure B2 (a). In the first six months (from April to September), the monthly spending growth is positively correlated with annual budgeted expenditure growth.For the discretionary growth (the difference between growth in actual annual expenditure and growth in budgeted annual expenditure), only the growth of November expenditure responds to it. These patterns suggest that the collected monthly data does capture a representative proportion of overall fiscal expenditure, as the variation in expected growth is reflected in the variation in the monthly growth, especially in the first half year. The declining sensitivity from April to March is due to discretionary expenditure comes later in the second half of the fiscal year. There is a spike in the sensitivity to discretionary growth in November. It corresponds to the fact that the central government's budget for next year is usually published at the end of October, which in turn induces local government's response by altering its original spending plan. Figure B2 (b) plots the sensitivity at industry level. There is heterogeneous response to the budgeted aggregate expenditure growth across sectors. For example, there are several sectors that are insensitive to the fluctuations in aggregate expenditure: Public Administration,Mining ,Arts, Financial Services and Water.These sectors contains suppliers that provide compulsory and routine services to the public.

### 2.4.2 Match Contracts with Monthly Cash Flows

Each procurement contract is expected be followed by a stream of cash flow in the collected granular expenditure data set. If we can observe salient cash flows following the inception of procurement contract, it's a cross-validation of both data sets' quality.I plot the cash flows following contract start date in Figure B3. There is indeed a jump of cash flows in the month of contracts' starting date and followed by persistent cash flow patterns. On average, 1% of the contract value is paid out in the first month. There is a clear distinction on the cash flow patterns between contracts to local suppliers and contracts to non-local suppliers, from the monthly cash flows plot, it's obvious that contracts to local suppliers persist longer than contracts to non-local suppliers.The patterns might be confounded by different contract lengths. To make it clearer, I plot the cumulative cash flow in contrast to the expected evolution of cash flows assuming the cash flows are evenly distributed over the contract horizon. The results are shown in Figure B3. It's evident that for contracts awarded to local suppliers, cash flows grow faster than those to non-local suppliers. Contracts to local suppliers are expected to be paid out after 36 months, but in 24 months, total cash flows paid out has exceed the contract value. On the other hand, the non-local suppliers' contracts' cash flows evolve very closely to the pattern generated by even payment over contract horizon. The two types of contracts are not so different in their expected cash flows path. The faster payment and more persistent cash flows of local contracts can arise from two possible channels: (1) faster payment within contract period; (2) more likely to have follow-up contracts.

## 2.5 Measurement and Summary Statistics

In this section, I will introduce the construction of key variables and summary statistics of them. Through out the summary, we can observe some preliminary patterns on the distinction between local and non-local council-supplier pairs. The key variables can be divided into three categories:(1) firm-year level characteristics of the sales to councils; (2) firm-year level financial information;(3) contract level characterization.

### 2.5.1 Concentration and Churn

I collapse the monthly transaction data into two data sets:(1) customer $\times$ supplier $\times$ year level; (2) supplier $\times$ year level. Each observation in the first data set is denoted as $S_{i,c,t}$, representing the monthly cash flow from customer $c$ to supplier $i$ in month $t$. From the cash flow data, we can tell the footprints of the development of each supplier's markets expansion and composition (at least in the public sector market). We expect to see that the evolution of suppliers' markets are dependent on the local share of previous transactions. To characterize the development of markets using thee cash flows, I creates two variables that capture the dynamics of suppliers' markets. One that can tell the concentration of supplier's customer pool; the other capture the supplier's rate of customer churn ( Baker et al. (2020)).

I construct the following measures. Using the weight of each customer in a firm's total sales, we can measure how concentrated the *portfolio* of sales are and how frequently this portfolio is adjusted. I focus on the patterns of cash flows within a period of $s$ years. The first measure $Concentration_{i,t}$ is the concentration ratio based on all sales from $t+1$ to $t+T$.$Concentration_{i,t} = 1$ implies that firm $i$ has only one customer during that period. The smaller this measure is , the more disperse the firm's pool of customers is. On the other hand captures, we are interested in the the dynamics of the pool of customers.I use $Churn_{i,t}$ to measure the difference in the customers pool between period $t$ and $t+s$. $Churn_{i,t} = 0$ is the case when the customer pool (and the weights) from $t+1$ to $t+s$ is the sames as the customer pool observed in $t$. $Churn_{i,t} = 1$ mean that none of the customers observed in $t$ appears from $t+1$ to $t+s$.

$$\begin{aligned}
Concentration_{i,t} &= \sqrt{\sum_c (w_{i,c,t+1\to t+s})^2} \\
Churn_{i,t} &= \sum_c |w_{i,c,t+1\to t+s} - w_{i,c,t-s+1\to t}|/2 \\
\text{where} \quad w_{i,c,t_1\to t_2} &= \frac{\sum_{\tau=t_1}^{t_2} S_{i,c,\tau}}{\sum_c \sum_{\tau=t_1}^{t_2} S_{i,c,\tau}}
\end{aligned}$$

As shown in the equations above, we measure concentration using $Concentration$ and market expansion using $Churn$. The building block for the two measures is $w_{i,c,t_1\to t_2}$ which is the share of transaction value between firm $i$ and council $c$ in all the transactions of $i$ during the period between $t_1$ and $t_2$. They measures how specialized a supplier is in a particular council-supplier sales relationship from a static and dynamic perspective respectively.We expect these two measures to be conditional the locality of current composition of suppliers' sales.

The summary statistics are shown in Panel A of Table 2. Observations are separated by *Locality*. There is clearly a distinction between observations with $Locality > 0.5$ and observations with $Locality \leq 0.5$. Conditional positive sales in year $t$, in the period over next three years, the concentration of customer pool is much higher and market expansion is much slower for suppliers following local sales compared to non-local sales. The average Concentration after local sales is close to 1 while it's 0.37 after non-local sales. Following local sales, the percentage of sales that comes from new councils is only 6.2% in the next three years, while for non-local sales, it's 20.1%. That means locality is correlated with higher concentration in customer pool and less likely to build new sales relationships with other councils.

These patterns might correlate with the current year's composition of sales. In the current year, the majority of observations with $Locality > 0.5$ has only one council customer and more than 25% of the $Locality \leq 0.5$ observations have over 1 council customer. If each council-supplier sales relationship persists equally well, it's not surprising to see higher $Concentration$ following local-sales. However, for the measure of customer churn $Churn$, the patterns can not be simply explained by the number of customers in the current year. Higher customer churn means either that current sales relationships are hard to maintain or that these suppliers tend to actively expand to new markets.Simply from the number of observations, the first channel is supported. In current year $t$, observations with $Locality > 0.5$ occupy 29.5% and the fraction increases to 35.8% in the next three years. Both Gourio and Rudanko (2014) and Baker et al. (2020) have argued that lower churn measures a form of intangible capital ("customer" capital) valuable in the presence of product market frictions and imposing an additional adjustment cost on firm expansion that dampen their response to shocks. I take similar stand but in our setting such intangible capital does not require additional investment to obtain for between local council-supplier pairs. Moreover,the difference in customers' concentration and churn reflects that the political authority maybe one form of market friction.

### 2.5.2 Firm Financials

Using the financial information registered with Companies House, I construct measures to characterize the investment and financial decision of firms. Due to the transparency legislation, firms are required to disclose different information. The common financial report required to file is the balance sheet. For profit related information, firms qualified as "small" or "micro" entities[13] can prepare and submit the "profit and loss account" (equivalent to "income" state-

[13]For the qualification conditions of firm types based on firm size, please refer to "Accounts Guidance":https://www.gov.uk/government/publications/life-of-a-company-annual-requirements/life-of-a-company-part-1-accounts

ment) on a voluntary basis. Since most firms as councils' suppliers are small firms, to avoid contamination of results by disclosure bias, I focus on information with the largest coverage in the balance sheet.

I characterize firms' behaviours from two aspects. First, focusing the asset's side, I construct the asset's growth, intangibility and investment ratio (equivalent to CAPEX). Asset growth is the annual log growth rate of firms' total asset. Intangibility is measured as the ratio of firm's intangible assets to total asset to represents intellectual capital. Investment ratio is the ratio of investment in fixed asset to total asset, which is equivalent to the term "Capex" under other accounting standards. To be more specific on these two measures, in the guidance set by Companies House [14], intangible assets include long term resources, not cash or held for conversion into cash that do not have a physical presence e.g. brand, reputation, goodwill, supplier relationships; investment in fixed assets are resources held by the company for investment rather than trading purposes. Intangible assets, tangible assets and fixed asset investments together constitute the book value of total fixed assets. On the financial side, I construct the leverage which is the ratio of total debt to total assets, and long term debt ratio which is the share of long term debt in the total debt. In addition, I also calculate the asset turnover which is the ratio of total sales ("turnover" in the profit & loss account) to average total asset (the average of current and previous year) to how effectively companies are using their assets to generate sales.

Table 2 presents the summary of the one-year lead values of these financial variables for each firm $\times$ year observation. There is a clear distinction between observations with $Locality > 0.5$ and $Locality \leq 0.5$. First, in general, local suppliers are smaller and younger, not only by the means but also by the medians and grows faster. However, they have lower level of intangibility and investment in fixed assets. Besides, the leverage is lower and long term debt ratio is slightly higher. There is also a small gap in terms of asset's turnover. Yet, we can't conclude a causal effect by local sales on these ratios. The selection of supplier's characteristics may be in the same direction of the patterns observed, then the estimates maybe biased without controlling selection bias.

### 2.5.3 Contract Characteristics

From all available data sources, I collect information on a sample of procurement contracts awarded by local authorities.The contract characteristics include contract value, start and ending dates, title, short description and CPVs(Common public Procurement Vocabulary) that can classify contracts into categories based on the content of the goods and services provided. Since

[14] Abbreviated Accounts Balance Sheet: https://ewf.companieshouse.gov.uk/help/en/stdwf/accountsHelp.html

the various information sources disclose the procurement data at different aggregation level, I define a contract by: customer (local authority), start date, end date, title and/or description of the contract. This means one contract have multiple awardees.

In particular, I focus on if a contract have multiple CPVs and multiple awardees. These two indicators imply the **ex-ante** complexity of the contracts. Contracts with multiple CPVs are believed to be more complicated. In addition, I count the number of awardees for each contract, the more awardees, the more specialized the task awarded for each awarded supplier will be. Contracts with single CPV and are awarded to many suppliers are believed to be customary tasks that can easily be extended and renewed without much adjustment cost for the supplier which is cost-effective for both the supplier and the customer. Adjustment cost has important implications for firms' investment (For example, Cooper and Haltiwanger (2006)).

In addition to the characteristics explicitly specified ex ante, I match the procurement contracts with the cash flows data and construct **ex-post** measures to characterize the cash flows generated after the inception of the contracts. Given that it's possible that each supplier-customer pair can have multiple contracts and these contracts' periods overlap with each other. To tackle with these issues, I use the following procedure to back out the cash flows linked to each contract.

$$ln(1 + S_{i,c,t}) = \sum_{\tau=-12}^{T+12} r_{i,c,\tau} ln(1 + TV_{i,c,t-\tau}) + e_{i,c,t} \tag{1}$$

Where $TV_{i,c,t-\tau}$ is the total value of contracts initiated in month $t$. I examine the cash flows from $t-12$ to $t+T$ where $T$ is the longest term (in months) of the contract procured from $c$ to $i$ during sample period. Therefore, $r_{i,c,s}$ measures the cash flow in month $t$ as a fraction of the value of contracts started $s$ months ago. It is assumed that cash flow patterns for a customer-supplier pair is the same for any contracts signed between them. Then I add up these monthly shares over different time windows to evaluate the dynamics of cash flows:

$$TCF_{i,c}(s_1 \rightarrow s_2) = \sum_{s=s_1}^{s_2} r_{i,c,s}$$

The total cash flow generated from $s_1$ to $s_2$ months after the start of contract is define above.I consider three cases about the time window for evaluation: (1) $s_1 = t$ and $s_2 = t + 11$ which is the first year after the contract starts ($TCF(1st\, Yr.)$); (2) $s_1 = t$ and $s_2 = t + T/2 - 1$ which is the first half of the duration of contract period ($TCF(1st\, Half Period)$); (3) $s_1 = t + T + 1$ and $s_2 = t + T + 12$ which is the year after the contract terminates ($TCF(1st\, Yr.\ After\ Contract)$). The three measures tells the ex-post cash flows from different dimensions. While the values of these measures are all unknown *ex ante*, the first and third measures (might) contain cash flows beyond the time length of the contract which is the cash flows due to extension or renewal of contracts. The second measure captures how fast the supplier can reap the contract's value. Just as in Barrot and Nanda (2020), the "faster payment" to small firms can relax the financial constraints.

The ex-ante contract terms might contribute to the differences in ex-post cash flow patterns. More complex and less customary contracts are different from other contracts in two ways. First, they are in general more difficult to implement, so the cash flows are more likely delayed if the payment is based on the progress. Second, these contracts involve irregular needs from the council, so suppliers have lower chances to have the contracts extended or renewed and that means lower cash flows generated from this customer-supplier relationship beyond the contract's length.

In the summary statistics shown in Table 3. local contracts do differ from non-local contracts in many aspects. Local contract is slight longer by 1 month and indifferent in the contract size. The average number of suppliers in local contract is higher even though the difference from the top 25% contracts. Local contracts are 9% less likely to have multiple CPVs, which is significant magnitude compared to the average likelihood of all local contracts (16%). As for the cash flows patterns, they are in general consistent with the contract terms. In the first half of the contract's length, 75% of the contract value is paid for local contracts while only 62% are paid for non-local contracts. As for cash flows after the contract ends, there is a sharp difference between local and non local contracts. For the mean value there is a 35% difference and at the median, the difference is 43%.

It should be noted that the analysis for contracts is at customer-supplier level.So the interpretation of the difference in cash flows patterns is limited to each customer-supplier pair. It's very likely that suppliers awarded with non-local contracts have sales relationships with more councils and the aggregate cash flows patterns might be different. However, it adds no value to aggregate the cash flows from contracts and analyze at firm level for two reasons. First, the data set coverage is less comprehensive about procurement contracts soc aggregation leads to data bias. Second, customer-supplier level cash flows patterns provide better insights into the origins of customer churn, adjustment cost and firms' response.

### 2.5.4 Comparison with Other Settings

For the convenience of the interpretation of the results and external validity, I can conduct a cross-sectional comparison with other similar settings of suppliers to governments ( Ferraz et al. (2015) and Hvide and Meling (2020) ). In terms of contract terms, Ferraz et al. (2015) uses a sample of over 4,000,000 auctions and the average contract size is about £26,000[15] in Brazil. The top categories of spending are "Medical Veterinary Equip", "IT","Subsistence (Food)" and "Laboratory Equipment" . Hvide and Meling (2020)' setting in Norway has a sample of 4,083 public roads procurement contracts with median contract value about £560,000 and median duration of 7 months. Compared to our setting, the contract value is in between the two settings (median around £65,000 and mean around £550,000). However, compared to Hvide and Meling (2020)'s sample, the contracts in my setting are much longer as the median duration is 36 months. As for firm financials, only Hvide and Meling (2020)'s paper has comparable financial information of suppliers. The median company in their sample is 12-year old and has total assets of £1.14 million. While the age is similar to my paper, the size of company is bigger (in my paper, the median total asset value is £0.42 million).

Overall, compared to the settings used by these two papers, the suppliers in my database are smaller and the contracts' values are within a reasonable interval. In addition, my database has a more comprehensive coverage of sectors and firms' sizes. Such differences should be taken into account for the interpretation of the results in this paper.

## 3. Local Preferential Treatment

How local suppliers are treated differently from non-local suppliers? In this section, I am going to characterize purchases patterns of councils and show that they indeed treat local and non-local suppliers differently in at least two aspects: contract terms and the customer relationship. Unlike consumer goods, the consumption of governments are mostly for customized services, so they take an active role in determining the contract terms and targeted potential suppliers ( Bajari and Lewis (2009)). In practice, even if the procuring process is held in competitive auctions, there are different eligibility requirements for bidders. The "buy-local" policies may induce councils to offer contract terms that are more favourable to the provision of goods and services and development of the firm. With a political connection view, the favouritism via contract terms might be beneficial to the firm in short-horizon rather than long-horizon. For example, contracts awarded to local suppliers may be of greater value and short length of

[15] Their original currencies are converted into GBP for comparison

time to finish, to provide greater and quicker cash flows to the firm. In another dimension, the customer-supplier relationship with local suppliers are more likely to be extended after the current contracts end because of the greater incentives to build political connection with local suppliers. In a friction-less market, councils shouldn't differentiate suppliers based on their locality to the council.

It's challenging to disentangle *locality* from other possible factors that lead to heterogeneous treatments to suppliers. Local suppliers are mechanically more distant than non-local suppliers. This introduces several confounders. First, the trade between local suppliers and the council may have lower transaction costs. It makes councils to allocate to local suppliers more services contracts that can be delivered without transportation cost. Second, due to local information advantage, it's easier to local suppliers to build a more persistent relationship with the council. This is unnecessarily induced by political connection incentives. Third, due to heterogeneous distribution of external factors (for example, supply side shocks, and demand shocks from private sector) across different regions, for a particular council, its differentiated treatment to local suppliers might be driven by other factors that are specific to that region. With the setting, we are able to control for these factors by comparing suppliers within narrow neighbourhood crossing the administrative boundaries of councils. Meanwhile, there is a discontinuous variation in the $local$ status of a supplier-council pair when crossing the boundary. However, its not enough to utilize this discontinuity design because it's single-dimension while the spatial distribution of suppliers surrounding the council is two-dimension. To tackle with that, I add spatial fixed effects tot the regression discontinuity design[16]. I include spatial fixed effects that assign a unique spatial group for suppliers located within a 3Km $\times$ 3Km grid based on UTM coordinates. Therefore, combining the two elements, the identification does exploit discontinuous variation in $local$ status of the supplier among a set of suppliers located in very close neighbourhood that shares similar distance to a given council customer, supply side shocks and local economic conditions. The running variable for the spatial regression design is the distance from the firm's postcode location to the nearest border of the customer council. The regression specification is as follow:

$$\begin{aligned} X_{i,c,t} &= \beta Local_{i,c} + F(Dist_{i,c}) + K_{g,t} + a_c + b_t + \epsilon_{i,c,t} \\ Local_{i,c} &= \begin{cases} 1 & \text{if} \quad Dist_{i,c} \leq 0 \\ 0 & \text{if} \quad Dist_{i,c} > 0 \end{cases} \end{aligned} \tag{2}$$

[16]Examples of papers using Spatial Fixed Effects Estimator are Magruder (2012), Conley and Udry (2010) and Goldman et al. (2013)

In the equation above, $Dist_{i,c}$ is the normalized distance from firm $i$'s location to the closest point of customer $c$'s boundary lines (coastal lines excluded). The distance between the local government and the supplier is defined by the distance from supplier's postcode location to the nearest border of the local authority. It's then normalized by the maximum distance between local suppliers and the local government. The normalization helps to align local authorities of different sizes [17]. $Local_{i,c}$ is the indicator for local supplier. $F(Dist_{i,c})$ is a polynomial function of the distance. Spatial fixed effects are included and interacted with time as $K_{g,t}$, so in a given period, suppliers in the same grid are compared to yield the results. With such identification strategy, only a fraction of the observations are the effective sample. For grids specified to be 3 Km $\times$ 3 Km, about 33.5% of the observations are included.

There are two categories of outcome variables $X_{i,c,t}$ in the regression. The first set of variables are the contract characteristics I have discusses in the previous section: contract size, length of time, the cash flows patterns within and beyond the contract's horizon. The second set uses the monthly purchases data and include: the contemporaneous year's total value of sales, and the next year's total values of sales relative to contemporaneous year's total value of sales.

In our context, the assumptions for the identification strategy is the normal distribution of potential suppliers (including those firms that are not government suppliers) and the continuity of all other possible covariates of them around the threshold ( Lee and Lemieux (2010)). These assumptions are mild for our setting. First, the continuity of the distribution of firms around the boundaries can be tested (Appendix). For those unobservable covariates (transaction costs for example), the possibility of unconditional non-discontinuity is minimum because the unconditional distributions of any covariates are symmetric. It's because in out setting with many councils neighbouring to each other, the local status of suppliers is also symmetrically defined.

Before showing the regression results, it's more straightforward to show the results graphically. The Figure 3 plots the differentiated treatment to suppliers that fall within short distances to the border of the councils. From the figure, on average 15% of each local governments' expenditure flows to local suppliers (normalized distance $\leq 0$). Another 15% of the expenditure are with suppliers that are located within 5 times the distance of furthest local supplier. On the border, within the same distance to border (0.1 unit of normalized distance), the local governments allocate 1.6% to local suppliers compared to 0.3% to non-local suppliers [18]. In terms of customer relationship, there is a 10.5% gap in the persistence of customer relationship,as measured by the next year's total values of sales relative to contemporaneous year's total value of sales ($ln(1 + \frac{S_{i,c,t+1}}{S_{i,c,t}})$), which is about 1/6 of the persistence of non-local suppliers' next year's

[17] In the appendix, I also show results are robust when I use absolute distance for these regressions

[18] I exclude the coastal boundaries of local authorities as there are no potential suppliers on the other side of the border by default.

relationship with the council.

## 4. Empirical Framework

In this section, I set up the empirical framework to build a causal relationship between local sales and firms behaviours. As I have shown in the previous section, councils treat local suppliers and non-local suppliers differently. Such local preferential treatment can have important impacts on the investment and financing behaviours of the suppliers. However, it's empirically challenging. Ideally, we expect to compare the behaviours of two identical suppliers that are randomly allocated with procurement contracts from some council that is only local to one of the two suppliers [19]. Yet, it's difficult to ensure the randomness of matching between councils and suppliers. The selection of local and non-local suppliers are based on ex ante characteristics of the firms and the selection rules for the two types of suppliers can be very different. I am going to find an instrument to only consider those suppliers picked with randomness.

### 4.1 Baseline Specification

Before explaining that instrument, we need to set up the baseline specification for the identification. The key independent variable in the regression is the share of sales that is to the local council in each year for each individual supplier. It tells the relative importance of the local council in all the sales to councils.

$$\textit{LocalShare}_{i,t} = S_{i,c_i,t} / \sum_c S_{i,c,t}$$
$$S_{i,t} = \sum_c S_{i,c,t}$$

Where $S_{i,c,t}$ is the value of sales from firm $i$ to council $c$ in time period $t.c_i$ refers to the council that firm $i$ is located. The higher the ratio, the more dependent the firm is on local council as customer relative to other councils. The values of $LocalShare_{i,t}$ range from 0 to 1. When the measure takes value of 1, it means the local council is the only council customer.When the

[19]It's not ideal to compare one council's different responses to exogenous demand from the local and one non-local council because the effects of government purchases on firms can be long-run. Then the effects from the two councils can overlap with each other and hard to be disentangled. In addition, the two types of sales can be endogenously correlated.

measure takes value of 0, it means the only non-local councils purchase from the firm. Figure 5 shows the distribution of this measure. It's highly skewed to the two sides of the domain. It's suggesting that sales to the local and non-local councils are exclusive. It's also a *conditional* measure, namely only takes values for supplier-period observations with non-zero total sales to councils ($S_{i,t} > 0$) because we focus on the different behaviours of suppliers to different councils rather than the difference between firms that are and are not suppliers to councils.

The main argument in this paper is that suppliers with greater exposure to the local council behave differently from other suppliers in years following the sales. Then we have the following identification:

$$\Delta ln(Y_{i,t+s}) = \gamma \textbf{\textit{LocalShare}}_{i,t} + K_{g,t} + Controls_{i,t} + \epsilon_{i,t} \tag{3}$$

The main specification above regress firm level outcome variables on the local share of sales to councils at firm $\times$year level for all suppliers. The main dependent variables I am interested in are log growth of level variables of firm: sales, asset, fixed asset, total debt and long-term debt. I also considers some financial ratios: the leverage, long-term debt' share in total debt, and the composition of total firm sales. For these ratio variables, I focus on the absolute growth. Our specification in first-difference eliminates any bias that would be generated by a correlation between non time-varying firm characteristics (likely to affect current and future firm behaviours) and the level of the sales. To check how persistent are the results, I also consider dependent variables over longer periods, from $t + 1$ to $t + 3$. $K_{g,t}$ is grid$\times$time fixed effects. The inclusion of these time-varying spatial fixed effects allow me to compare suppliers located within small neighbourhoods. This controls for any confounders related to the location of supplier. Moreover I include firm characteristics $Controls_{i,t}$, for example the age and total asset level.

The specification takes advantages of variations in the $LocalShare_{i,t}$ for suppliers located within a grid regardless of customer's identity. However, it does not control for endogenous matching between councils and suppliers. The firm characteristics $X_{i,t}$ only captures part of the endogenous selection. The unobservable factors that govern the council-supplier selection can bias the results. For example, the local council's choice of local suppliers might be based on their private information about the quality of the firms' products and services. The solution is to find an instrument that can extract the proportion of variation in $LocalShare_{i,t}$ that are independent of the possible endogenous selection.

### 4.2 Demand Shock as Instrument

I exploit the demand shocks of council as the instrument for endogenous concerns mentioned above. Conceptually, the endogeneity of the matching between councils and suppliers will be minor in times of unexpected demand from the council. From the suppliers' perspective, they can't endogenous adjust their behaviour in anticipation of the council's purchases. From the councils' perspectives, in time of "surprise" of demand for public goods and services, they are unlikely to endogenously choose suppliers at their own will due to the constraint of firms to expand their capacity in a short period of time. If we focus on suppliers on the two sides of the boundary of councils, the demand shock from the council on one side of the boundary will increase its purchases from suppliers from both sides of the boundary. This constitutes the ideal experiment I suggest at the beginning. Moreover, in the baseline specification, demand shocks will mechanically creates a gap in $LocalShare_{i,t}$ for suppliers on the two sides of the boundary.

Essentially, it is a *quasi* difference-in-difference design. In the cross section, suppliers on the two sides of the boundaries are treatment and control groups respectively. In the time series, demand shock from one side of the boundary can be regarded as an exogenous event. In this particular setting, selection on firms' observable characteristics is allowed as long as the selection bias remains constant without the shock, which can be tested by checking parallel pretrend. The setting addresses the concern for endogenous matching based on anticipation of suppliers' future behaviours.

However, given the nature of demand shocks, it's very likely that the treatment is serially correlated. It's because even though the volume of procurement contracts might be exogenous, the cash flow following them are extended for longer periods. This can bias the standard errors of the estimation and may reject the "parallel trend" assumption wrongly. To tackle with the issue, I explicitly get rid of the serial correlation of demand shocks using AR models. In addition, following Bertrand, Duflo and Mullainathan (2004)'s non-parametric method, I simply collapses the relevant time series variables into a "pre"- and "post"-period. So in the regression results, I construct the dependent variable as the average over the 3-year period following the sales (from $t$ to $t+2$).

Now, let me discuss about the detailed steps to construct the instrument using demand shocks. The instrument constructed is similar to shift share instrument (For example, Bartik (1991), Borusyak et al. (2018) and Aghion et al. (2018)) and is adapted to several features of our setting.

First, I decompose the demand shocks into different components and analysis the exogeneity of them and relevance for the identification for this study.UK local governments make annual budget for the next fiscal year based on the category of expenditure and announce it usually at the end of October. The budget covers expenditure for the next fiscal year from April to next March. Results in Table B7 shows that the budgeted growth is indeed correlated with the growth using collected data. The actual aggregate expenditure is not used for this decomposition. The reasons for that the actual aggregate expenditure fails to capture the variations in monthly granular spending has been articulated in the Data Section. The unexpected components of councils' expenditure can be detected by decomposing the actual year-over-year monthly expenditure growth:

$$d_{c,t,m}^{actual} = a_{t,m} + b_c + d_{c,t}^{Budgeted} + d_{c,m}^{Unexpected} \tag{4}$$

The actual monthly expenditure growth of council $c$ in month $m$ is $d_{c,t,m}^{actual}$ which is calculated as the year-over-year growth of monthly total expenditure $ln(E_{c,t,m}/E_{c,t-1,m})$ where $E_{c,t,m}$ is the total expenditure of council $c$ in the $m$-th month in fiscal year $t$. $d_{c,t}^{Budgeted}$ is the expected annual growth using aggregate expenditure data reported by MHCLG. $a_{t,m}$ is month fixed effects that captures trends of the growth of expenditure that is common to all councils. $b_c$ are council fixed effects that captures the time-invariant cross-sectional heterogeneity. $d_{c,t,m}^{Unexpected}$ is the residuals from the regressions above. It measures how unexpected the expenditure of a council in a given month as the growth is beyond the council's planning. Moreover, the residual is month specific for each council, so it tells exactly the months within a fiscal year that the council purchases due to shocks. Then suppliers located in the council with positive $d_{c,t,m}^{Unexpected}$ in these months are potentially "treated" by the demand shock from the local council.

For each supplier, the intensity of local demand shock it is exposed within a fiscal year is:

$$\Delta D_{i,t}^{Local} = \sum_{m=1}^{12} w_{i,t,m} d_{c_i,t,m}^{unexpected} 1(d_{c_i,t,m}^{unexpected} > 0)$$

where $c_i$ refers to the council where supplier $i$ is located within. (For each calculation of the council expenditure's growth, purchases from supplier $i$ is excluded) [20]. The monthly shocks are weighted by $w_{i,t,m}$ which represents the fraction of a supplier's sales in month $t$ within year $t$. I construct demand shocks at year level because the outcome variables are mostly at year level. Due to the weighting, only suppliers with sales in months of unexpected expenditure are regarded treated by the demand shocks. The weighting method levers up the intensity of

[20]Another potential source for spurious results is if $i$ have a dominating position in $c$'s expenditure. So I exclude the supplier if it consists more than 10% of the government's expenditure, which is very rare.

the treatment. It is consistent with the idea that exogeneity is sourced from the firms' inability to increase production capacity within a short period of time. So it's important to narrow the time window of the occurrences of shock events. Moreover, I include the dummy variable $1(d_{c_i,t,m}^{unexpected} > 0)$ because I only consider positive shocks [21].

Even though the instrument, $\Delta D_{i,t}^{Local}$ is defined to be supplier-year specific. The majority of the variation in this instrument is at council-year level (88%). It implies that with the instrument, I am able to exploit the discontinuous variation of $LocalShare_{i,t}$ induced by local council's demand shock. There rest part of the variation arises from the "leave-one-out" principle in the calculation of unexpected monthly expenditure growth and the firm specific weighting. Due to concerns that this small fraction of variation which does constitute the ideal experimentation, I show in appendix the results for the sample that only includes suppliers in geographical grids that cross the boundaries of councils.

The uniqueness of our setting highlights the exogeneity of the unexpected growth in expenditure. First, unlike other settings that government fiscal shocks affect the taxation and credit markets ( Berndt et al. (2012)), demand shocks for councils in England have limited channels to affect other macroeconomic factors because councils have limited fundraising power. The council specific shocks are mostly funded via internal reserve funds, namely internal smoothing. The redistribution of expenditure over time following a demand shock may induce a chain of shocks. But in the earlier discussion, the unexpected growth of expenditure used in the construction of instrument gets rid of the serial correlation. Second, the major sources of unexpected expenditure are mostly driven by natural disasters and pandemics. For example, the outbreak of COVID-19 in 2020 in England increases the expenditure of all councils across England, especially those councils with more aged population. Another example in 2018, the Anticyclone Hartmut, a cold wave that hit South West England increases the affected councils' annual expenditure by about 30%. The heterogeneous geographical and demographic attributes enable us to observed heterogeneous growth in council's expenditure. However, even if these attributes for two nearby councils are on average different, they are not discontinuously distributed on the boundary. This validates the identification strategy in this paper because the sources of demand shocks do not necessarily overlap with the location of suppliers affected by the demand shocks.

In summary, I manage to instrument the key regressor $LocalShare_{i,t}$ by the local demand shock. The instrument induces great differences in $LocalShare_{i,t}$ for suppliers located on the two sides of council's boundaries. These variations correspond to demand shocks events when the matching between councils and suppliers is believed to be independent of the *locality* of the supplier,

[21] In the appendix, I also show results if negative shocks are used in the construction of the instrument.

at least to the extent that it is exogenous to any possible anticipation of the future behaviours of the firms. The relevance of the instrument can be easily verified. For the exclusion restriction of the instrument, we need to rule out the possibility that the demand shock affects suppliers' behaviours from other channels instead of more purchases. Those other possible factors channeling the relationship between demand shock and suppliers' behaviours must bear the following characteristics: (1) induced by the local demand shock; (2) distributed discontinuously over the council's boundary; (3) not restricted to the suppliers to the councils. For example, local demand shock increases the purchases of goods and services that allow the council to provide better support to the development of businesses. This channel satisfies both conditions (1) and (2) but not necessarily condition (3). However, it's testable by studying if local demand shocks also have an impact on firms that are not suppliers to any councils.

# 5. Main Results

In this section, I will present the main results about the causal relationship between local sales and suppliers' behaviours in sales, investment and financing. This section is divided into three sections. First, I show first stage regression results that validate the instrument. Second, I focus on the impacts of local share of sales to council on suppliers' sales patterns : the level, uncertainty and composition of customers. Third, I focus on the impacts on investment and financing variables of the suppliers.

## 5.1 Validity of the Instrument

It's important to first ensure that the instrument local demand shock is a strong instrument and it bears with randomness as we expect. Table 4 shows about the property of the instrument. First, it has a strong correlation with the *LocalShare*. An unexpected demand shock of 100% on average increases the local share of sales to councils by 0.15 relatively. The magnitude is not trivial. The average value of *LocalShare* is 0.25. That means in a grid crossing the council's border with 200 suppliers, without demand shock, 50 of them are suppliers to their local councils (25 for each of the two councils, $LocalShare = 1$). When one of the council doubles its expenditure in a given month (total purchases increase from 100 to 200), there will be (25+7.5)=32.5 suppliers with $LocalShare = 1$ in the shocked council, and (25-7.5)=17.5 suppliers with $LocalShare = 1$ in the other council near the boundary. 85 of the incremental purchases go to suppliers in other councils further away. [22] The intensity of treatment on

[22] In this illustrative example, I am assuming all the suppliers are identical and will be offered one unit of sale.

*LocalShare* actually depends on what fraction of the additional purchases is to be allocated to local suppliers. With th example, we can infer that about 7.5% of the additional purchases are allocated to local suppliers. This is smaller than the unconditional mean of *LocalShare*. This is consistent with the exogeneity of demand shocks: councils are unable to stick their allocation rule in normal times when facing demand shocks.

In addition, I check if the intensity of demand shocks is evenly distributed over suppliers with different characteristics. In Table ?, the first stage regression results are not heterogeneous across firms' characteristics. This supports the external validity of the IV regression results in the main specification. The interpretation of the results can be based on the *average* supplier rather than suppliers with particular characteristics. Moreover, there are some special characterization of the suppliers that receive higher intensity of treatment. There are more new suppliers (the share of first-time sales to councils[23]) and more new contracts (the starting of contracts in that month). Demand shocks initiate more new council-supplier relationships and new contracts than other times. This backups the exogeneity of the instrument: in times of demand shocks, councils are less likely to rely on existing suppliers because of limited capacity of suppliers and the demand shocks are not capturing systematic cash flows windfalls of existing contracts. The patterns in the distribution of demand shocks also imply that the results are more likely driven by new suppliers and new contracts. In unreported regressions, for the sub-sample including observations of new suppliers or new contracts,I find results similar to the sub-sample including observations without neither new sales relationship or new contracts.

### 5.2 Customers and Sales

I start with reporting results of the impacts on the patterns of sales and customers. The first-order effects of demand shocks should be manifested on the sales and composition of customers. In theory, the effects of demand shocks from a particular customer on other customers can be either complementary or substitution. There can be intra-firm positive spillovers due to the scalability and mutability of production inputs and knowledge (for example, Ding (2020), Jovanovic and Gilbert (1993), Dhingra (2013), Eckel and Neary (2010) and Bernard et al. (2019)). On the other hand, the internal spillover within firm can be negative if the production of goods/services to different customers involve very different skills and inputs. The second effect of substitution between different types of customers can be exaggerated if the firm has limited capacity to expand production or financially constrained. Given that the suppliers in our sample are small and the demand of councils can be rather customized, it's likely the sec-

[23]Given that the sample period starts from 2010, for each supplier-council pair, the first observed sales after 2012 is regarded as a first-time sales.

ond effect will dominate the overall effects from local demand shocks.

I then divide the customers of councils' suppliers into several categories and compare the effects of *LocalShare* on them. It spell out if both complementary and substitution effects of one customer's demand shock exist. Combining the Companies House data and the hand-collected purchases data, I manage to decompose the firm's total sales into three parts: the local council, other councils, and customers in the private sector [24]. In addition, I focus the concentration and churn rate of each supplier's pool of council customers. The patterns in these two variables also provide insights on the intra-firm spillover effects. Higher churn and smaller concentration is consistent with a positive internal spillover channel.

Beyond the composition of sales, I focus on the aggregate effects. Specifically, I am interested in two variables: the growth of total sales growth (the total firm sales an the total sales to councils depending on the availability of data) and the volatility of total sales growth. To measure the volatility of sales growth, I extract the residual growth of sales for each supplier:

$$\Delta ln(TS_i, t) = a_i + b_t + \epsilon_{i,t}$$

where the growth in total sales $\Delta ln(TS_i, t)$ is decomposed into a firm specific component ($a_i$), year component ($b_t$) and the idiosyncratic component ($\epsilon_{i,t}$). In the regression above, I include observations for each supplier from 2010 to 2020. The absolute values of the idiosyncratic growth are then used as the dependent variable in the main specification and the coefficient before *LocalShare* imply the additional variability in sales growth following sales that is to the local council. The absolute value of $\epsilon_{i,t}$ is not a supplier-specific measure of ex ante perception of uncertainty, but the cross-sectional average of this measure can be an ex post proxy for uncertainty on the aggregate. This is essentially in line with the empirical literature that uses the cross-sectional dispersion as the measure of uncertainty (for example, Bloom et al. (2018)).

The results are shown in Table ?. First, on the composition of sales, the impact of *LocalShare* is positive on the growth of sales to the local council, negative on the growth of sales to other councils, and insignificant on the growth of sales to private sector. For local council, the positive effects are significant up to at least three years. It implies that exogenous purchases from the local council induces very persistent relationship with the supplier in the long-run which is consistent with the stylized facts documented in the section of local preferential treatment. In terms of magnitude, the persistence documented here is smaller by 2% (8% versus 10% in

[24] For the majority of observations, I am able construct two categories of sales: sales to the local council and non-local councils. For 10% of the observations that have total firm sales data, I am able to decompose the total firm sales into three parts as stated.

Figure 3.) because endogeneity problem is resolved in the main identification strategy. For sales to non-local councils, they are crowded out by sales to the local council which implies a negative intra-firm spillover. There is no significant effect on sales to private sector which might be due to data limitation as the total firm sales is only available to 10% of the observations which are mostly larger suppliers. Furthermore, *LocalShare* causes the supplier to have a more concentrated and sticky customer pool, namely suppliers to the local council become more and more specialized with the local market.

On aggregate, the results show that sales to the local council doesn't increase the level of sales growth but reduce its uncertainty. As implied in results about the composition of sales from different groups of customers, the local council's purchases result in persistent sales relationship but also crowds out the suppliers' sales to other councils. The aggregate effect on total sales is ambiguous. In Table ?, the growth in total sales to councils is affected by *LocalShare* from year $t$ to $t+3$. The result is similar if I focus on the growth of total firm sales. The persistence in sales relationship with the local council is offset by the crowding-out effects. However, the two forces combined together lead to lower uncertainty of the growth in total sales. In Table ?, the local council's purchase on average reduces the volatility of annual growth in total sales to councils by 3.05%, which is economically large, as the unconditional volatility is just 5.72% (measured as the average of absolute values of idiosyncratic growth, which can be interpreted as the cross-sectional dispersion).

Overall, the results on suppliers' sales picture a distinguished role of purchases from the local council. It reshapes the composition of the customers and therefore reduces the uncertainty of cash flows to the supplier. Part of this is in line with Gourio and Rudanko (2014) and Baker et al. (2020) who emphasizes the role of customer relationship as a form of intangible capital. But they may neglect the exclusivity of customer capital that can crowds out the demand from competing customers. In the context of intra-firm spillover, this suggests that the scalibility and mutuality of production inputs and skills are weakened by obtaining a procurement contract from the local council. This has already been implied by the results in the section of local preferential treatment that contracts from the local council are more specialized in terms the singularity of products/services and the similarity of co-awardees. In columns (10) and (11), I show that the patterns in contracts' characteristics also hold at the supplier level following exogenous local sales. Local council's purchases result in the contracts awarded to suppliers in the next three years to be more specialized. They are 21% more likely to contain single category of products/services, and 36% more likely to be collaborated with other similar suppliers (in the same sector and location). The specificity of the products/services limits the firm to scale the production factors to the provision of goods/services to other potential customers. The collaboration with only similar firms also prohibits the "learning-by-doing" of suppliers from other firms (for examples of "learning-by-doing", see Conley and Udry (2010) and Foster et al. (2016)). For small businesses, the specialization of contracts can have a more pronounced effects because of higher adjustment cost of investment. The sluggish adjustment of investment and the specificity of the investment give rise to the path dependency of the supplier's production skill and customer base.

### 5.3 Investment and Capital Structure

Eventually, my goal is to explore the causal impacts of the local council's purchases on firm's investment and financing. As we from the analysis above about sales and customers, suppliers after exogenous purchases from the local council are featured with less volatile cash flows. The relationship between uncertainty and investment is in generally believed to be negative and more pronounced when the irreversibility of assets is high ( Bloom et al. (2007), Bloom (2009), and Gilchrist et al. (2014)). The connection between uncertainty and capital structure, is however, rather ambiguous and delicate. Due to agency problems between debt holders and equity holders ( Jensen and Meckling (1976)), traditionally people hold the view that higher risk reduces leverage. On the other hand, aversion to ambiguity may lead firms to increase leverage since equity value is more sensitive to the ambiguity ( Lambrecht and Myers (2017) and Izhakian et al. (2021)). In addition, I am interested in the share of long-term debt in total debt. Essentially, the choice of debt maturity relies on the trade-off between the costs of underinvestment and mispricing of long-term debt against the liquidity/refinancing risk and monitoring effect of short-term debt. Lower volatility in cash flows can reduce the cost of accessing debt market ( Minton and Schrand (1999)). Long-term debt can be the most sensitive fund providers to the volatility in cash flows since cash flows is a common type of collateral for long-term debt in England.

In addition, I can imagine the possibility of other factors stimulated by the local council's purchases that can also affect firm's investment and capital structure. For example, the specilization of the contracts may have a direct implication on the firm's investment decision. It may lever up the capital adjustment costs and slows down the investment of firms.The paper is limited in disentangling uncertainty from other channels in explaining the results. But I believe uncertainty plays a first-order role in driving patterns in the investment and capital structure. In the next section, I will provide additional robustness checks to support uncertainty as the main explanation.

I construct two sets of variables to measure the patterns of suppliers' investment and capital structure decisions. First, for investment, I focus on the physical capital investment. In theory, capital investment will be the change in PPE(Property, Plant, and Equipment) ($\Delta PP\&E$) plus the depreciation.The administrative records of firms' financials in England have limited information of depreciation. However, it's a reasonable assumption that depreciation is moving slowly from year to year. With this assumption, the growth in capital expenditure will be minimally biased by ignoring depreciation in the calculation of physical capital expenditure [25]. I also calculate the growth in firm's total asset as it might captures the increment in intangible capital investment [26]. Second, for the capital structure, I focus both the levels of growth in total debt and long-term debt and the changes in leverage and share of long-term debt in total debt. These variables can inform us the effects of *LocalShare* on the leverage and debt maturity choice of suppliers to councils.

The results are shown in Table ?. For capital investment, there is indeed a significant positive impacts. On average, the local council's purchase result in 9.5% higher growth in physical capital investment. This is economically large. To interpret the results, We need to pay attention to the skewness of the distribution of these variables. For a median supplier (3.5%), the local council's purchase can lift it to the 68th percentile of the distribution. Similarly, for the growth in total assets, the effect is 11.7% (from median to 70th percentile.). It's worthwhile to note that this is not a mechanical response to a shock in demand. First, as we see in the previous subsection, the local demand shock doesn't induce a difference in growth of total sales. So it's not merely a consequence of capital investment to tackle the contemporaneous surge in production of goods/services. Second, the positive effects on capital investment is going beyond the contemporaneous year. That means even if the nature of procurement tasks of local council requires more investment in the current year, the effect is long lasting.

On the financing side, the effect is insignificant for the total debt but positive for the long-term debt. Consequently, the effect on leverage is negative and on the debt maturity is positive. In Table ?, the local council's purchases increase long-term debt growth by 8.4% (from median to 71th percentile) annually from year t+1 to t+3. This increases the share of long-term debt in total debt by 1.5% (from median to 64th percentile). These results not provide evidence on the positive relationship between uncertainty and leverage,highlighting the ambiguity aversion. Moreover, the effects on long-term and short-term debts are opposite direction. This implies the reduction in uncertainty helps firms to have better access to long-term debt but

[25] For 12% of the observations that have data of depreciation, I find that the variability of depreciation from year to year is very small. The log growth of depreciation is on average 1.27% and the ratio of depreciation to $\Delta PP\&E$ is only 22.3% on average. These implies for each 10% in the growth in actual capital expenditure, there is a a maximum of 1.8% overestimate of it by ignoring depreciation. Moreover, as we are capturing the difference in two types of suppliers' growth of capital investment, the bias may be offset.

[26] I don't measure the intangible capital investment independently because the variable is not pervasive for observations and the book valuation of intangible asset is not reliable. See Eisfeldt et al. (2020) for a discussion of different approaches to the valuation of intangible value of firms.

doesn't increase its total demand for credit. It may also suggest that suppliers are not necessarily financially constrained, otherwise there won't be negative spillover effect on short-term borrowing.

# 6. Robustness Checks

In this section, I am going to run several sets of robustness tests that can shed lights on the mechanisms. First, I will study heterogeneous results based on the attitudes towards uncertainty of directors of firms. Second, I will use the political turnover to test the political incentives behind the local preferential treatment and suppliers' expectation-based decision-making. Third, I run test on firms that are not suppliers to any councils to validate the exclusion restriction of the instrument.

## 6.1 Attitudes towards Uncertainty

To ensure that differentiated uncertainty in cash flows is the main channel that explains the effects, I provide additional suggestive evidence that the effects are stronger for suppliers that are more averse to uncertainty. So I study if there is heterogeneous results across directors with different levels of risk-aversion. Directors are the decision-makers of investment and capital structure policies. Moreover, gender and age are found be factors that affect the risk attitudes of people. For example, in Kerr et al. (2017) provide a summary of evidence that females fall short in entrepreneurship and risk attitude is one of the key factor. Females have higher degree of risk-aversion. In Eckel and Grossman (2008) that compares men and women systematically in many contexts, women are found to be more in general averse to risk than men. Dohmen et al. (2017) find that willingness to take risks decreases over the life course.

Given that directors may have different risk attitudes, their sensitivities to the reduction in uncertainty are different. So we expect to see the effects of the local council's purchases to be heterogeneous depending on the characteristics of directors. To identify that heterogeneity, I modify the main regression model in the following way:

$$\Delta ln(Y_{i,t+s}) = \gamma_0 \textit{LocalShare}_{i,t} + \gamma_1 \textit{LocalShare}_{i,t} \times Z_{i,t} + K_{g,t} + Controls_{i,t} + \epsilon_{i,t}$$

which adds the interaction between *LocalShare* and characteristics of the directors $\textit{LocalShare}_{i,t} \times Z_{i,t}$. $Z_{i,t}$ can be the gender and log of the age of directors. When there are multiple directors, I take the average values across all directors existing in the current year of sales $t$. So $\gamma_1$ can capture the heterogeneous effects.

In the sample, 23% of the directors are female, and the median age of them is 37. Directors of small businesses are on average younger and there are more females than directors of an average firm. In Table ?, I present the results of main dependent variables. First, the effects on sales growth and uncertainty are not conditional on the characteristics of directors. It confirms the exogeneity of the demand shocks. Second, for the capital investment and financing, I do find results that are consistent with the general perception that females and older people are more risk averse. From the table, for suppliers with females directors, the effects are larger: 2.3% higher growth in physical capital investment and 1.8% higher growth in long-term debt. Similarly, for suppliers with elder directors (greater than the median age of 37), the effects are larger: 3.7% higher growth in physical capital investment and 2.9% higher growth in long-term debt. The results provide additional support that the reduction in uncertainty of cash flows is indeed an explanation of the effects on suppliers' investment and capital structure.

### 6.2 The Impacts of Political Turnovers

In this part, I will investigate the impacts of political turnovers on the results documented on local preferential treatment and effects on suppliers' sales, investment, and capital structure. First, if the local preferential treatment is based on the political incentives to favour local voters and rent-seeking, political turnover in the local council will act as a negative shock to the political incentive for each supplier-council pair, while the political turnover in other councils won't affect the supplier-council relationship. Second, the shock to the supplier-council relationship is anticipated by the suppliers and they will adjust their investment and capital structure before the customer-supplier relationship with the local council actually breaks up.

Therefore, political turnovers of the councils serve to be an experiment for me to test two things. First, the local preferential treatment is motivated by political incentives. Second, it provides an empirical design of anticipated negative shock to council-supplier relationship. It complements to the argument that suppliers are making decisions based on their expectation of future sales' composition. The chain of effects can be the reverse: suppliers increase the capital investment which in turn changes composition of sales. Political turnover provides a setting which alleviates the concern of reverse causality.Suppliers with ongoing contracts expect the relationship with the local council to end in the future when the results of local elections turn out to change the ruling party. But the sales to local council is unlikely affected and therefore the contemporaneous composition of sales is also untouched. Any responses observed in the contemporaneous investment growth and capital structure can be attributed to the expectation of break-up of relationship with the local council.

As an overview of the local elections in England, political turnovers are not frequent. In this paper, the political turnover is defined as the change of ruling party, the party that has the largest fraction of councilors in that council. Two factors are driving the low probability of political turnover. First, the turnout rate (the share of registered voters who actually vote in the elections) in local elections is very low. In 2019, the England average is only 35%. Second, in most cases, only half or one third of the councillors are elected which makes the switching or ruling party very sluggish. In my paper,for all council-year observations that have local elections, only 9.2% of them result in political turnover.

### 6.2.1 Local Preferential Treatment

I first focus on the impacts of political turnover on the preferential treatment given by the local council. Based on the baseline regression model (2), I estimate the impacts of political turnover using the following regression:

$$X_{i,c,t} = \beta_0 Local_{i,c} + \beta_1 Local_{i,c} \times Turnover_{c,t^*} + \beta_2 Turnover_{c,t^*} + F(Dist_{i,c}) + K_{g,t} + a_c + b_t + \epsilon_{i,c,t}$$

$Turnover_{c,t^*}$ is a dummy that equals 1 if there is a political turnover in council $c$ in year $t^*$ where $t < t^* \leq t+3$ [27]. The restriction on $t^*$ specifies a political turnover that occurs within three years in the future. $\beta_1$ captures the impacts of political turnover of the local council. In addition, $\beta_2$ is expected to be zero or negative. It depends on if the change of controlling party also induces reshuffle of the non-local suppliers.

The results are shown in Panel A of Table ?, the political turnover results in 13.6% lower persistence in the sales relationship with the local council. Other contract terms including the value, time length, and specialization of contracts are not correlated with the political turnover. This implies that contemporaneous characteristics don't predict the likelihood of political turnover. Meanwhile, $\beta_2$ is insignificant in all specifications, suggesting political turnovers of councils don't lead to reshuffling the non-local suppliers. Overall political turnovers indeed reduces the persistence of customer-supplier relationship with the local council. It implies that political incentive is a key driver of the local preferential treatment to suppliers. Political turnover is a negative shock to existing council-supplier relationships which are sustained by political incentives. Following political turnovers of the local council, existing suppliers are abandoned

[27] If there are multiple years with political turnover,I consider the earliest one

and new suppliers are selected with political incentives.Effectively, political turnovers result in reshuffling of local suppliers and exert no impacts on non-local suppliers.

### 6.2.2 Sales, Investment and Capital Structure

Then I examine how suppliers respond to political turnovers of the local council in terms of capital investment and capital structure. I modify the regression model (3) in the following way:

$$\Delta ln(Y_{i,t+s}) = \gamma_0 \textit{LocalShare}_{i,t} + \gamma_1 \textit{LocalShare}_{i,t} \times Turnover_{i,t^*} + \gamma_2 Turnover_{i,t^*} + K_{g,t} + Controls_{i,t} + \epsilon_{i,t}$$

where $Turnover_{i,t^*}$ is equal to 1 if supplier $i$ is exposed to a political turnover shock in year $t^*$. Given my main identification strategy (3) focus on the comparison between suppliers on the two sides of councils' boundaries, the exposure to political turnover shock is defined to be 1 if there is occurrence of political turnover in any nearby councils near supplier's location I consider the council that the supplier belongs to and the non-local council closest to the supplier. Similarly, $t^*$ satisfies that $t < t^* \leq t + 3$, specifying that I only focus the first political turnover within three years in the future.

In addition, I pay particular attention to the years after before the termination of current procurement contracts (when there are multiple contracts, the earliest date). It enables to be observe suppliers' response in capital investment and borrowing while keeping the sales unaffected. So the effects on investment and financing can attributed to expected loss of relationship with the local council instead of confounded effects by the reverse causality between investment and sales.

In Panel B of Table ?, I manage to show that suppliers retrieve the investment and long-term borrowing when they anticipate the loss of the relationship with the local council due to political turnover. The annual growth in physical capital investment and long-term debt decreases by 5.6% and 8.7% respectively before the contract terminates. Even though, suppliers have slow down their investment and long-term borrowing, it's not fatal to them. Given that they have time to manage the expected political risk, the survival rate and bankruptcy rate is unchanged.

Overall,the effects of purchases from local council is undermined by the possibility of turnover. However, the frequency of political turnovers is infrequent, so it won't absorb the positive effects brought by the local council's purchases.

### 6.3 Exclusion Restriction

One doubt about the instrument is the exclusion restriction. For the exclusion restriction of the instrument, we need to rule out the possibility that the demand shock affects suppliers' behaviours from other channels instead of more purchases. Those other possible factors channeling the relationship between demand shock and suppliers' behaviours must bear the following characteristics: (1) induced by the local demand shock; (2) distributed discontinuously over the council's boundary; (3) not restricted to the suppliers to the councils. For example, local demand shock increases the purchases of goods and services that allow the council to provide better support to the development of businesses. This channel satisfies both conditions (1) and (2) but not necessarily condition (3). However, it's testable by studying if local demand shocks also have an impact on firms that are not suppliers to any councils.

Therefore, I run the following reduced form regression for firms that are never suppliers to any councils during the sample period:

$$\Delta ln(Y_{i,t+s}) = \phi \Delta D_{i,t}^{Local} + K_{g,t} + Controls_{i,t} + \epsilon_{i,t}$$

The effects of demand shock from the local council [28] are expected to be zero for firms that are not government suppliers. In each geographical grid that has a positive number of suppliers to councils, I randomly draw a set of firms that have no sales to councils and it contains the equal number of suppliers to councils. In Table ?, the results show no significant relationship between any of the outcome variables and the local demand shock. On one hand, this does alleviate the concern for exclusion restriction of the instrument. On the other hand, it indicates that the causal effects of local government's purchases I have been trying to build is not spread to other firms in the private sector. However, this is not saying the fiscal multiplier on the private sector is only limited to the direct suppliers to the local governments. The local governments' purchases can have external effects on broader set of firms, but is not in a discontinuous way as I documented in this paper. Namely, a council's purchases have equal effects on firms located on the two sides of the council's boundary.

[28] The construction is slightly different from the one for suppliers with positive sales to councils. I extract a annual component of unexpected growth in expenditure: $d_{c,t,m}^{actual} = a_{t,m} + b_c + d_{c,t}^{Budgeted} + d_{c,t}^{Unexpected} + \epsilon_{c,t,m}$. Then the annual unexpected component $d_{c,t}^{Unexpected}$ is a measure of local demand shock.

## 7. Conclusion

In this paper, motivated by the fact that local governments give preferential treatment to local suppliers throughout their purchases,I try to answer the question that if such buy-local policies have positive impacts of the suppliers' investment. In particular, I examine if sales to the local government result in more physical capital investment than sales to nonlocal governments. I do find significant effects of the purchases from the local council relative to other councils' purchases. Moreover, the effects are not driven by the level of sales but the volatility of sales instead. This is a combined result on the persistence and exclusivity of customer-supplier relationship with the local council. I find more micro explanations on the crowding-out effects of purchases from the local council: contracts from the local council tend to be single category of products/services and are collaborated with similar firms.

I contribute to the understanding of government's behaviours using micro data set and granular identification strategy. I construct a novel and granular data set of the purchases of 308 councils in England with corporate suppliers in monthly frequency from 2011 to 2020. First, I document that compared to non-local boards, suppliers receive more specialised contracts from the local council and maintain a more persistent customer-supplier relationship with it. Next, to identify the causal relationship between local sales and suppliers' outcomes, I exploit exogenous demand shocks with spatial fixed effects on the councils' boundaries.

In general,the paper shed light on the novelty of government's purchases patterns and the special patterns in the demand-side factors do spur the capital investment of suppliers. It further our understanding of government's purchases on the economic activities in the private sector beyond taking them as demand shocks to the suppliers. They can reshape the behaviours of firms in the private sector through more profound channels. The policy implications from the study is that local governments can spur the survival,growth and investment of small suppliers by reducing the uncertainty in cash sales. However, the results found on the differential treatment in the contract terms offered to local suppliers may inspire policy makers to ponder on the design of procurement contracts that can foster positive intra-firm spillovers instead of crowding out demand from other councils and/or private sector customers.

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# 9. Main Figures and Tables

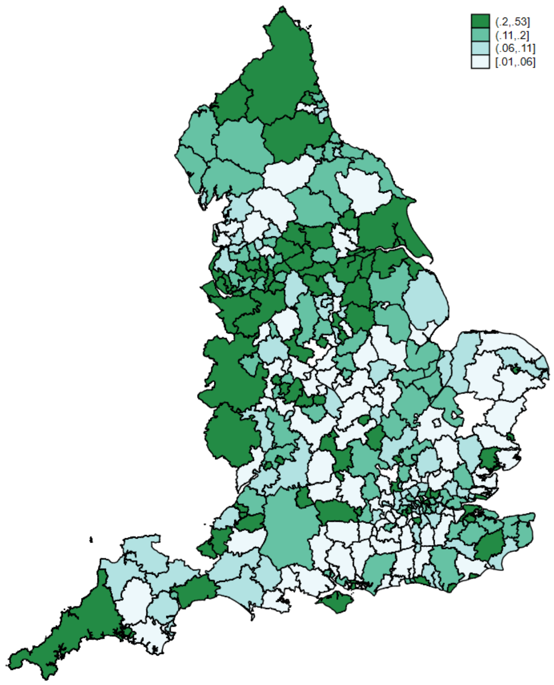



Figure 1: Share of Local Expenditure For Each Council

This map plots the share of local expenditure for each council in England. Boundary lines of councils are in black. Local expenditure includes purchases from suppliers located within the council's authority. The calculation uses all the transactions between 2011 and 2020. In the map, I include only geographically exclusive councils (See Appendix A).

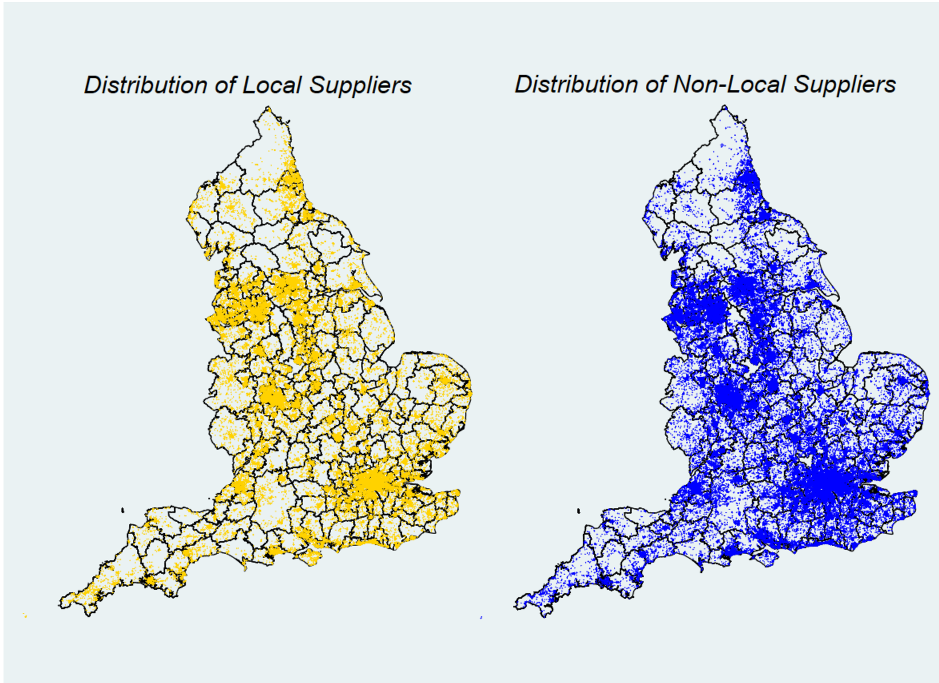



Figure 2: Distribution of Suppliers

The maps plot the distribution of suppliers to local governments in England based on their political locality with regard to the council. Each circle represents a supplier with positive sales between 2011 and 2020. The left plots the suppliers with sales to local council and the right plots the suppliers with sales to non-local councils. The location of suppliers is based on the headquarter's postcode.

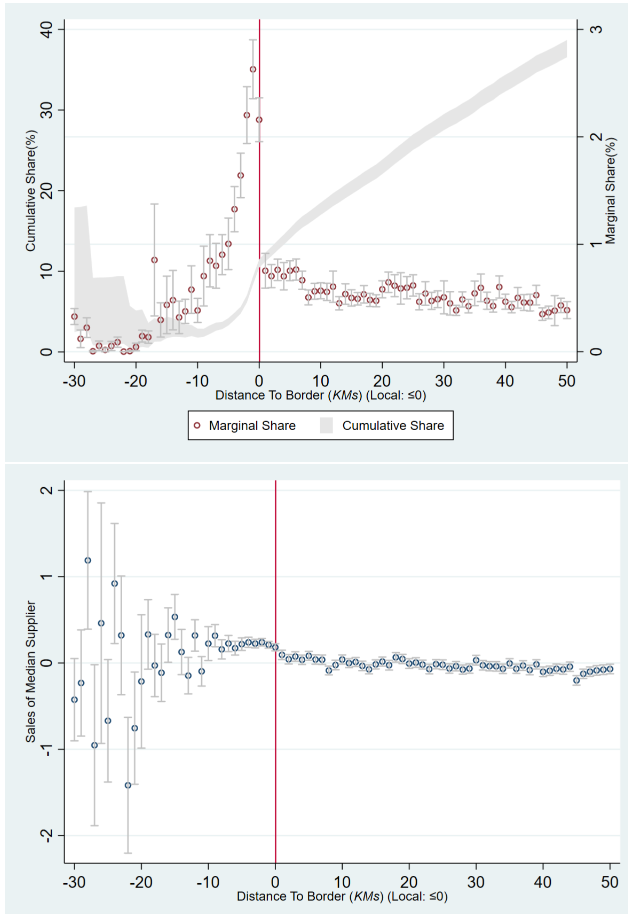



Figure 3: Distribution of Suppliers Near the Boundary

Note: The figure plots the distribution of suppliers near the border of the council from the perspective of the customer (council). The horizontal axis is the distance between the supplier to the nearest border of the council customer in kilometer bins. The distance of politically local suppliers are taken as negative. The vertical axis plots the marginal share and cumulative (from left to right) share of total expenditure in each distance bins. Location of supplier is determined by the post code level address of headquarter office (registered with Companies House) of the supplier.

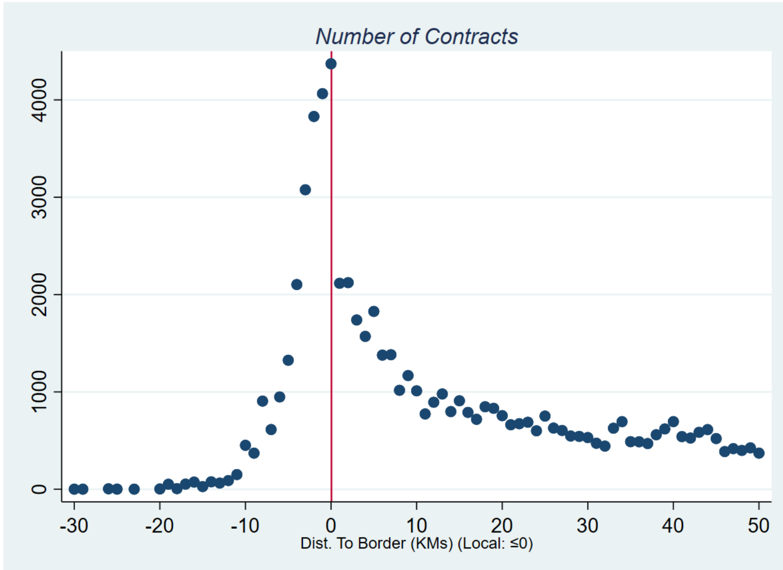



Figure 4: Distribution of Contracts Awardees Near the Boundary

Note: The figure plots the distribution of awardees of procurement contracts near the border of the council from the perspective of the buyer (council). The horizontal axis is the distance between the supplier to the nearest border of the council customer in kilometer bins. The distance of politically local suppliers are taken as negative. The vertical axis plots the marginal share and cumulative (from left to right) share of total expenditure in each distance bins. Location of supplier is determined by the post code level address of headquarter office (registered with Companies House) of the supplier.

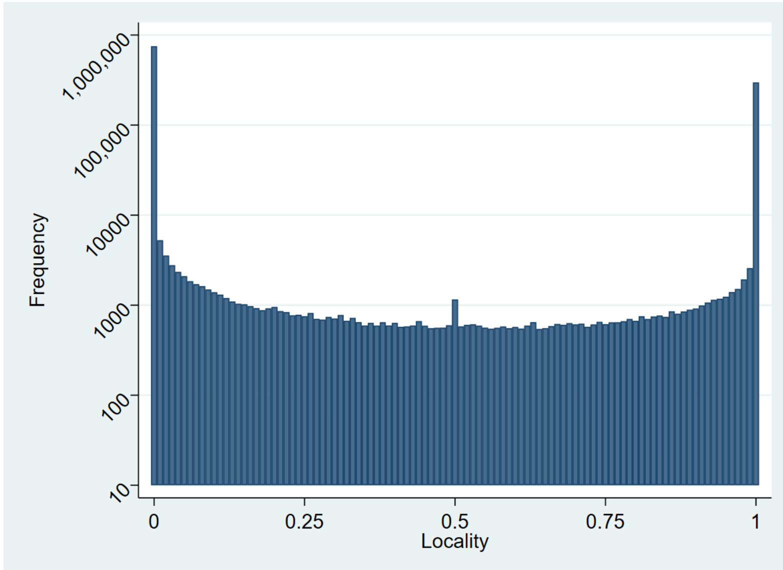



Figure 5: Distribution of Suppliers' *Locality*

Note: The figure plots the distribution of suppliers' *locality* of sales. The observations are at supplier-year level. *Locality* is the share of sales to local council in all sales to councils. The fraction of observations with $Locality = 1$ is 25.2%; the fraction of observations with $Locality = 0$ is 65.3%.

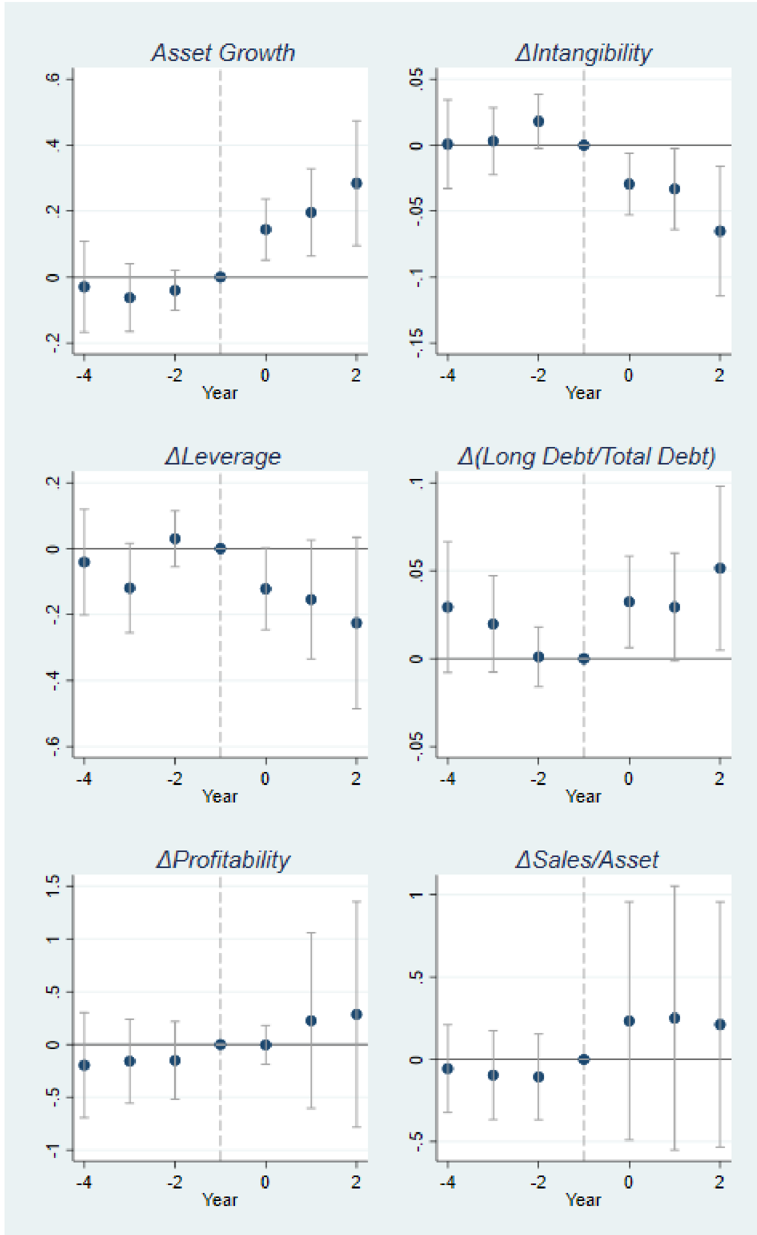



Figure 6: Pre-Trend and Cumulative Effects

Note: The figures graphically show the pre-trend test and how persistent the effects are. These coefficients are obtained replacing the dependent variables by the difference between lead and lag terms of the interested financial variables and the value as of year $t-1$ in the baseline specification (Equation 6.3): $\Delta Y_{t+s} = Y_{t+s} - Y_{t-1}$ where $s$ is how many years after the observation of the sales. Asset growth is also defined in similar way: the cumulative growth from year $t-1$ to year $t-s$. $t$ is the fiscal year of the observation of the sales. See Appendix !!! for detailed explanations on these financial variables.

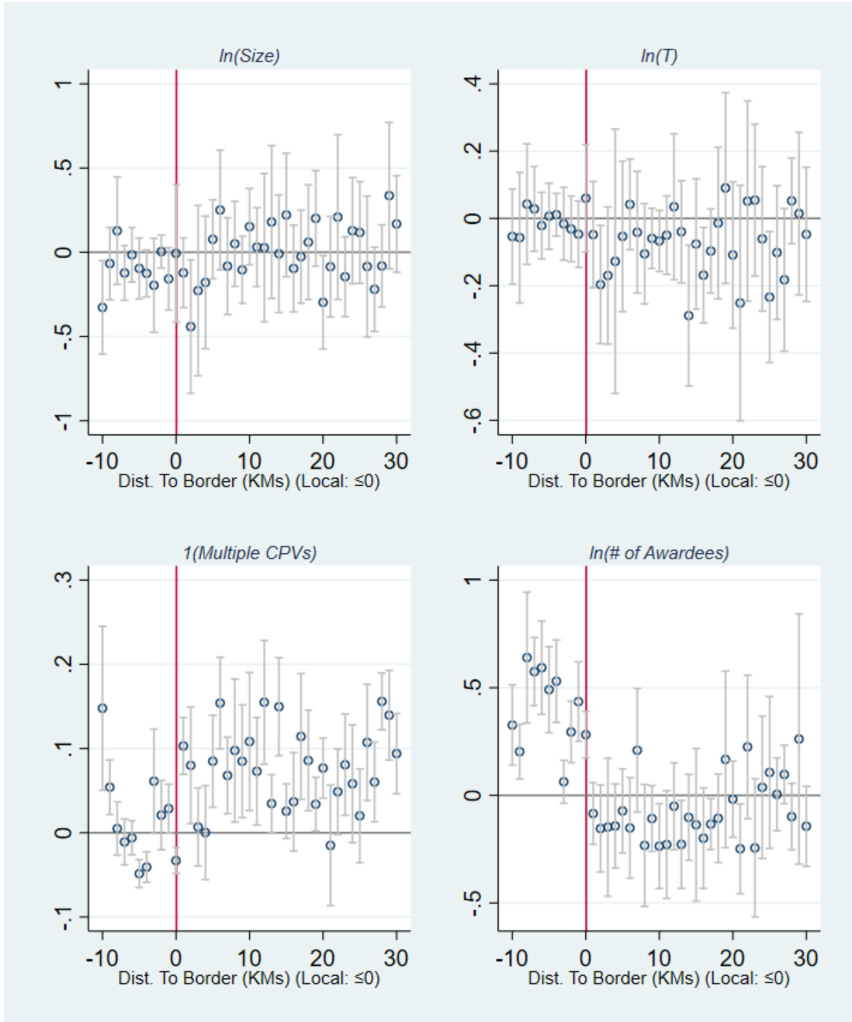



Figure 7: Geographic Differentiation in Demand Characteristics: Contract Terms

Note: The figures plot the procurement contracts' characteristics of suppliers located in different distance bins from the council. The horizontal axis is the distance between the supplier to the nearest border of the council customer in kilometer bins. There are four characteristics considered: the log of contract's value ($ln(Size)$), the log of the duration ($ln(T)$), indicator if the contract has multiple CPvs ($1(Multiple\ CPvs)$), and the log of number of awardees in the same contract ($ln(Number\ of\ Awardees)$). Then these variables are normalized by subtracting the average of all contracts awarded by the council in that year.

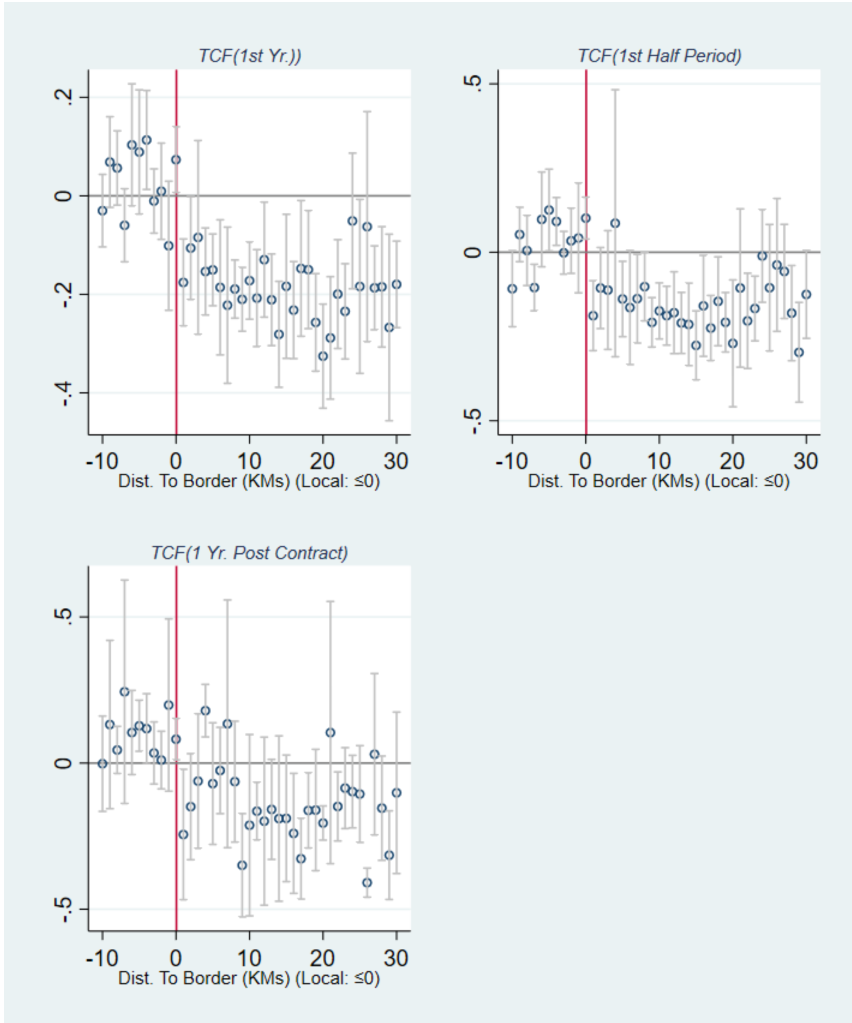



Figure 8: Geographic Differentiation in Demand Characteristics: Ex-Post Cash Flows Patterns

Note: The figures plot the cash flows patterns linked with contracts of suppliers located in different distance bins from the council. The horizontal axis is the distance between the supplier to the nearest border of the council customer in kilometer bins. There are three measures of the cash flows considered: the fraction of cash flows within 1 year after the contract starts ($TCF(1st\,Yr.)$),the fraction of cash flows within the first half of the contract's horizon ($TCF(1st\ Half\ Period)$), and the total value of cash flows within 1 year after the contract ends as a fraction of the contract's total value ($TCF(1\,Yr.\ Post\ Contract)$). Then these variables are normalized by subtracting the average of all contracts awarded by the council in that year.

Table 1: Number of Transactions/Contracts at Different Levels

| | Panel A: Supplier-Council Level Summary Statistics | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Num. of Supplier-Council Pairs | Mean | SD | Min | P25 | P50 | P75 | Max |
| | Transactions | | | | | | | |
| *All* | 1,739,964 | 9.0 | 15.1 | 1 | 1 | 3 | 9 | 129 |
| *Local* | 137,344 | 12.2 | 20.7 | 1 | 1 | 3 | 12 | 129 |
| *Non-Local* | 1,602,620 | 8.7 | 14.5 | 1 | 1 | 3 | 9 | 129 |
| | Contracts | | | | | | | |
| *All* | 102,634 | 2.9 | 17.3 | 1 | 1 | 1 | 2 | 147 |
| *Local* | 9,059 | 4.7 | 32.0 | 1 | 1 | 2 | 3 | 147 |
| *Non-Local* | 93,575 | 2.7 | 15.1 | 1 | 1 | 1 | 2 | 109 |
| | Panel B: Supplier Level Summary Statistics | | | | | | | |
| | Num. of Suppliers | Mean | SD | Min | P25 | P50 | P75 | Max |
| | Transactions | | | | | | | |
| *All* | 384,322 | 40.6 | 210.2 | 1 | 1 | 4 | 19 | 1227 |
| *Local (Conditional)* | 137,344 | 12.2 | 20.7 | 1 | 1 | 3 | 12 | 129 |
| *Non-Local (Conditional)* | 291,674 | 47.8 | 237.9 | 1 | 1 | 5 | 21 | 1221 |
| *Local (Unconditional)* | 384,322 | 4.3 | 13.7 | 0 | 0 | 0 | 1 | 129 |
| *Non-Local (Unconditional)* | 384,322 | 36.2 | 208.3 | 0 | 1 | 2 | 12 | 1221 |
| | Contracts | | | | | | | |
| *All* | 30,091 | 9.8 | 70.7 | 1 | 1 | 2 | 6 | 691 |
| *Local (Conditional)* | 9,059 | 4.7 | 32.0 | 1 | 1 | 2 | 3 | 147 |
| *Non-Local (Conditional)* | 23,015 | 10.9 | 78.1 | 1 | 1 | 2 | 7 | 690 |
| *Local (Unconditional)* | 30,091 | 1.4 | 17.7 | 0 | 0 | 0 | 1 | 147 |
| *Non-Local (Unconditional)* | 30,091 | 8.4 | 68.5 | 0 | 1 | 1 | 5 | 690 |

Note: This table summarize the number of observations for each supplier-council pair or each supplier. It also shows the summary statistics by the type of the political locality relationship between the supplier and the council (local versus non-local). In Panel B that show summary statistics at supplier level. I consider both conditional and unconditional statistics. The unconditional statistics include the suppliers with zero observations of that type and conditional statistics exclude those suppliers.In each panel, there are two data sets summarized: the number of transactions and contracts. For cash flows, the observations counted are at supplier-council-month level; for contracts, the observations are counted at supplier-council-contract level. Statistics shown include mean, standard deviation, minimum, maximum and 25th, 50th and 75th percentiles.

Table 4: Validity of Local Demand Shock as Instrument

| | $X$: Firm Charact. of Previous Year | | | | | $X$: New Purchases | |
|---|---|---|---|---|---|---|---|
| | $ln(Age)$ | $ln(Asset)$ | $Fixed\ Asset\ Ratio$ | $Leverage$ | $Long\ Debt\ Ratio$ | New Supplier | New Contracts |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $D_{i,t}^{Local}$ | 0.139*** | 0.143*** | 0.122*** | 0.154*** | 0.127*** | 0.125*** | 0.119*** |
| | (0.010) | (0.011) | (0.010) | (0.012) | (0.011) | (0.011) | (0.010) |
| $D_{i,t}^{Local} \times X_{i,t}$ | 0.004 | 0.001 | 0.046 | -0.009 | 0.050 | 0.080*** | 0.062*** |
| | (0.014) | (0.014) | (0.044) | (0.014) | (0.039) | (0.014) | (0.030) |
| Year*Spatial FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*Sector FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 759,111 | 714,976 | 703,494 | 660,303 | 660,303 | 759,111 | 759,111 |
| R-sq | 30.97% | 30.97% | 30.97% | 30.97% | 30.97% | 30.97% | 30.97% |
| F Statistics | 193.21 | 169.00 | 148.84 | 164.69 | 133.30 | 129.13 | 141.61 |

Note: This table shows the validity of unexpected demand shocks as instruments. I consider different components of council level demand shocks (See the decomposition in Equation 4) to construct the excess local demand shock as instrument: budgeted, transitory and unexpected. I regress the two main variables on the instruments constructed: locality (the share of local sales for a supplier in a given month), and $ln(Sales/Assets)$ (the total sales to councils in that month normalized by its asset value in the previous year.) Both regression results with and without the inclusion of spatial fixed effects are considered. Robust standard errors are presented in parentheses in regressions (1), (3), and (5); Standard errors are clustered at the spatial fixed effect (3 Km $\times$ 3 Km grids) level and presented in parentheses in regressions (2), (4), and (6). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Summary of Firm Level Variables

Panel A: Annual Sales

| Variables | Locality $> 0.5$ | | | | | Locality $\leq 0.5$ | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Obs(1000s)* | *Local %* | *Diff* |
| | (1) | | | | | (2) | | | | | | | (1)-(2) |
| No. of council Customers | 1.25 | 1.36 | 1.00 | 1.00 | 1.00 | 3.94 | 7.74 | 1.00 | 1.00 | 4.00 | 1,143 | 29.5% | -2.49*** |
| Total Sales (1000s) | 133.64 | 1897.53 | 1.40 | 7.49 | 34.13 | 251.17 | 3208.15 | 1.48 | 6.72 | 21.49 | 1,143 | 29.5% | -117.52*** |
| Concentration(Next 3 Yrs.) | 0.905 | 0.195 | 0.017 | 1.000 | 1.000 | 0.606 | 0.365 | 0.006 | 0.608 | 1.000 | 422 | 35.8% | 0.299*** |
| NEW(Next 3 Yrs.) | 0.062 | 0.152 | 0.000 | 0.000 | 1.000 | 0.133 | 0.201 | 0.000 | 0.045 | 1.000 | 422 | 35.8% | -0.071*** |

Panel B: Firm Financials

| Variables | Locality $> 0.5$ | | | | | Locality $\leq 0.5$ | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Obs(1000s)* | *Local %* | *Diff* |
| | (1) | | | | | (2) | | | | | | | (1)-(2) |
| Asset (£Million) | 12.15 | 177.22 | 0.07 | 0.25 | 1.02 | 73.90 | 2740.68 | 0.08 | 0.56 | 3.27 | 1,091 | *22.1%* | -61.755*** |
| Age | 14.82 | 13.47 | 6.00 | 11.00 | 19.00 | 17.26 | 15.76 | 7.00 | 13.00 | 22.00 | 1,109 | *22.0%* | -2.437*** |
| Intangibility | 0.10 | 0.25 | 0.00 | 0.00 | 0.05 | 0.18 | 0.32 | 0.00 | 0.00 | 0.19 | 833 | *23.7%* | -0.073*** |
| Capex | 0.02 | 0.22 | -0.04 | 0.02 | 0.08 | 0.05 | 0.28 | 0.00 | 0.03 | 0.10 | 886 | *23.6%* | -0.027*** |
| Asset Growth | 0.11 | 0.50 | -0.07 | 0.04 | 0.23 | 0.09 | 0.50 | -0.05 | 0.03 | 0.20 | 1,060 | *22.1%* | 0.023*** |
| Leverage | 0.74 | 1.18 | 0.26 | 0.54 | 0.87 | 0.87 | 0.37 | 0.00 | 0.64 | 0.91 | 1,002 | *23.0%* | -0.130*** |
| Long Debt Ratio | 0.19 | 0.28 | 0.00 | 0.02 | 0.30 | 0.18 | 0.28 | 0.00 | 0.02 | 0.26 | 997 | *23.0%* | 0.012*** |
| Sales/Asset | 0.99 | 0.59 | 0.52 | 0.94 | 1.33 | 0.93 | 0.56 | 0.51 | 0.89 | 1.24 | 281 | *23.5%* | 0.069*** |

Note: This table presents the summary of key variables. Key variables are constructed using two sources of information (See Section 2.5): the monthly transactions and firm financial information. Panel A presents of characterization for annual sales. Panel B presents the financial information in the year after. The observations are at supplier-year level. The sample are divided into two parts in each panel based on if the local sales exceed 50% of total sales to councils in that fiscal year. For each sub sample, I present the mean, standard deviation, median, minimum and maximum. Then I show the sample size and the fraction of $Local$ observation. In the last column, I show the difference between the two sub-samples' mean and test the significance of the difference. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Summary of Contract Level Variables

| Variables | Local | | | | | Non-Local | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Mean* | *SD* | *25th pctl.* | *Median* | *75th pctl.* | *Obs(1000s)* | *Local %* | *Diff* |
| | (1) | | | | | (2) | | | | | | | (1)-(2) |
| Contract Size (£Million) | 5.66 | 38.68 | 0.03 | 0.06 | 0.38 | 5.38 | 58.48 | 0.03 | 0.08 | 0.34 | *289* | *7.9%* | 0.28 |
| Contract Length (Months) | 40.53 | 38.84 | 12.00 | 36.00 | 48.00 | 39.52 | 35.73 | 12.00 | 36.00 | 49.00 | *289* | *7.9%* | 1.01* |
| Number of Awardees | 10.39 | 28.29 | 1 | 1 | 2 | 9.58 | 29.01 | 1 | 1 | 4 | *289* | *7.9%* | 0.81* |
| Multiple CPVs | 0.16 | 0.41 | | | | 0.25 | 0.41 | | | | *208* | *7.0%* | -0.09*** |
| TCF(1 Yr.) | 0.55 | 0.94 | 0.00 | 0.40 | 1.00 | 0.56 | 0.82 | 0.00 | 0.48 | 1.00 | *281* | *7.9%* | -0.01 |
| TCF(First Half Period) | 0.75 | 1.22 | 0.00 | 0.77 | 1.00 | 0.62 | 0.94 | 0.01 | 0.68 | 1.00 | *289* | *7.9%* | 0.13*** |
| TCF(1 Yr Post Period) | 0.87 | 1.43 | 0.00 | 0.88 | 1.00 | 0.52 | 0.91 | 0.00 | 0.45 | 1.00 | *205* | *8.9%* | 0.35*** |

Note: This table presents the summary of key variables at contract level. $Local$ refers to contracts awarded by local council.For each sub sample, I present the mean, standard deviation, median, minimum and maximum. Then I show the sample size and the fraction of $Local$ observation. In the last column, I show the difference between the two sub-samples' mean and test the significance of the difference. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Baseline Regressions: The Impacts of *LocalShare* on Firm Behaviours

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $AssetGrowth$ | $\Delta Leverage$ | $\Delta LongDebtRatio$ | $\Delta Intangibility$ | $\Delta Capex$ | $\Delta Sales/Asset$ |
| | Panel A: OLS | | | | | |
| $Locality \times ln(S/A)$ | 0.015*** | -0.012*** | 0.004*** | -0.003*** | -0.013*** | -0.001 |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.001) | (0.001) |
| $ln(S/A)$ | 0.021*** | -0.009*** | 0.001*** | -0.000*** | 0.002*** | -0.002*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| R-sq | 0.0286 | 0.0018 | 0.0009 | 0.0002 | 0.0014 | 0.0011 |
| | Panel B: Spatial FE | | | | | |
| $Locality \times ln(S/A)$ | 0.015*** | -0.014*** | 0.003*** | -0.001 | -0.008*** | 0.004 |
| | (0.002) | (0.003) | (0.000) | (0.001) | (0.002) | (0.003) |
| $ln(S/A)$ | 0.022*** | -0.010*** | 0.001*** | -0.000*** | 0.002*** | -0.003*** |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) |
| R-sq | 0.2513 | 0.2243 | 0.2566 | 0.2610 | 0.3834 | 0.4180 |
| | Panel C: Firm Controls | | | | | |
| $Locality \times ln(S/A)$ | 0.021*** | -0.022*** | 0.007*** | -0.010*** | -0.014*** | -0.010*** |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) | (0.001) |
| $ln(S/A)$ | 0.012*** | 0.001*** | -0.002*** | -0.002*** | -0.000* | 0.003*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| R-sq | 0.0511 | 0.1140 | 0.0947 | 0.1309 | 0.3017 | 0.0669 |
| | Panel D: Spatial FE + Firm Controls | | | | | |
| $Locality \times ln(S/A)$ | 0.020*** | -0.020*** | 0.005*** | -0.006*** | -0.010*** | -0.004 |
| | (0.002) | (0.003) | (0.001) | (0.001) | (0.002) | (0.003) |
| $ln(S/A)$ | 0.011*** | 0.001 | -0.002*** | -0.002*** | -0.000 | 0.003*** |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) |
| R-sq | 0.2695 | 0.3087 | 0.3307 | 0.3589 | 0.5641 | 0.4600 |
| N | 1,059,450 | 1,002,065 | 997,297 | 832,894 | 885,905 | 280,451 |

Note: This table presents the summary of baseline regression results of the regression model 6.3 about the impacts of local sales on suppliers' behaviours. With various specifications, the results are present in four panels. Panel A is OLS regression. Panel B includes spatial fixed effects (3 Km $\times$ 3 Km grids by month). Panel C includes firm controls(log of firm asset value lastly observable, log of age, sector fixed effects). Panel D has both spatial fixed effects and firm controls. The main independent variables $Locality \times Ln(Sales/Assets)$ and $Ln(Sales/Assets)$ are supplier-month level. The dependent variables are the annual log growth in asset, annual changes in leverage (debt/asset), long debt ratio (long-term debt/total debt), intangibility (intangible asset/asset), Capex (fixed asset investment/asset) and assets turnover ratio (total sales/asset). These ratios are valued as the average of three years' values following the government sales. Robust standard errors are presented in parentheses in Panel A and C; Standard errors are clustered at the spatial fixed effect (3 Km $\times$ 3 Km grids) level and presented in parentheses in Panel B and D. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: IV Results: The Impacts of *LocalShare* on Sales and Customers

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Growth of Sales | | | | | Uncertainty in Growth of Sales | | | |
| | Local Council | NonLocal Councils | All Councils | Total Firm Sales | Private Sector | All Councils | Total Firm Sales | Concentration | Churn |
| LocalShare | 0.131*** | -0.048** | 0.037 | 0.011 | 0.007 | 0.031*** | 0.012* | 0.465*** | -0.043*** |
| | (0.014) | (0.020) | (0.041) | (0.035) | (0.036) | (0.006) | (0.006) | (0.026) | (0.001) |
| N | 759,111 | 759,111 | 759,111 | 105,110 | 105,110 | 759,111 | 105,110 | 400,100 | 400,100 |
| First Stage | 0.137*** | 0.137*** | 0.137*** | 0.083*** | 0.083*** | 0.137*** | 0.083*** | 0.133*** | 0.133*** |
| | (0.012) | (0.012) | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) | (0.010) | (0.010) |
| | 134.16 | 134.16 | 134.16 | 57.3 | 57.3 | 134.16 | 57.3 | 176.89 | 176.89 |

Note: This table presents the summary of IV regression results about the impacts of local sales on suppliers' behaviours. In the regression, both spatial fixed effects and firm controls are included. The instrument is the *Local Demand Shock* constructed for each supplier in Section **??**. The main independent variables $Locality$ and $Ln(Sales/Assets)$ are supplier-month level. In the lower panel of the table, I show the first stage results. The dependent variables are the annual log growth in asset, annual changes in leverage (debt/asset), long debt ratio (long-term debt/total debt), intangibility (intangible asset/asset), Capex (fixed asset investment/asset) and assets turnover ratio (total sales/asset). These ratios are valued as the average of three years' values following the government sales. Standard errors are clustered at the spatial fixed effect (3 Km $\times$ 3 Km grids) level and presented in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: IV Results: The Impacts of *LocalShare* on Investment and Capital Structure

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Capital Investment | | | Capital Structure | | | | | | |
| | Growth of Fixed Assets | Growth of Current Assets | Growth of Total Assets | Growth of LT Debt | Growth of ST Debt | Growth of Total Debt | Leverage | LT Debt/Total Debt | Survival | Bankruptcy |
| LocalShare | 0.053*** | 0.016 | 0.030*** | 0.084** | -0.046* | -0.013 | -0.077*** | 0.015*** | 0.046* | -0.007 |
| | (0.012) | (0.047) | (0.010) | (0.030) | (0.023) | (0.041) | (0.008) | (0.002) | (0.023) | (0.010) |
| N | 703,976 | 703,976 | 759,111 | 660,303 | 660,303 | 660,303 | 660,303 | 660,303 | 759,111 | 759,111 |
| First Stage | 0.136*** | 0.136*** | 0.137*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.141*** | 0.141*** |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | 130.76 | 130.76 | 134.16 | 126.7 | 126.7 | 126.7 | 126.7 | 126.7 | 151.26 | 151.26 |

Table 8: Directors' Risk Attitudes and Heterogeneous Effects of *LocalShare*

Panel A: The Impacts on Sales and Customers

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Growth of Sales | | | | | Uncertainty in Growth of Sales | | | |
| | Local Council | Non-Local Councils | All Councils | Total Firm Sales | Private Sector | All Councils | Total Firm Sales | Concentration | Churn |
| LocalShare | 0.102*** | -0.045** | 0.035 | 0.009 | 0.011 | 0.028*** | 0.012* | 0.465*** | -0.043*** |
| | (0.014) | (0.016) | (0.041) | (0.034) | (0.033) | (0.006) | (0.006) | (0.026) | (0.001) |
| LocalShare*Female | 0.021 | -0.005 | 0.018 | 0.03 | 0.007 | 0.012 | 0.001 | 0.118 | 0.002 |
| | (0.011) | (0.010) | (0.013) | (0.046) | (0.010) | (0.010) | (0.010) | (0.110) | (0.010) |
| LocalShare*1(Director Age¿37) | 0.011 | -0.013 | -0.019* | -0.011* | 0.001 | -0.003 | 0.002 | -0.013 | -0.007 |
| | (0.010) | (0.017) | (0.009) | (0.005) | (0.009) | (0.007) | (0.007) | (0.023) | (0.007) |
| N | 759,111 | 759,111 | 759,111 | 105,110 | 105,110 | 759,111 | 105,110 | 400,100 | 400,100 |
| First Stage | 0.137*** | 0.137*** | 0.137*** | 0.083*** | 0.083*** | 0.137*** | 0.083*** | 0.133*** | 0.133*** |
| | (0.012) | (0.012) | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) | (0.010) | (0.010) |
| | 134.16 | 134.16 | 134.16 | 57.3 | 57.3 | 134.16 | 57.3 | 176.89 | 176.89 |

Panel B: The Impacts on Investment and Capital Structure

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Capital Investment | | | Capital Structure | | | | | | |
| | Growth of Fixed Assets | Growth of Current Assets | Growth of Total Assets | Growth of LT Debt | Growth of ST Debt | Growth of Total Debt | Leverage | LT Debt/Total Debt | Survival | Bankruptcy |
| LocalShare | 0.044*** | 0.015 | 0.036*** | 0.075** | -0.050* | -0.008 | -0.030*** | 0.009*** | 0.036* | -0.017 |
| | (0.013) | (0.037) | (0.009) | (0.030) | (0.024) | (0.041) | (0.008) | (0.002) | (0.018) | (0.010) |
| LocalShare*Female | 0.023** | 0.007 | 0.011** | 0.018** | -0.003 | 0.003*** | 0.005** | 0.007** | 0.001 | -0.011 |
| | (0.011) | (0.009) | (0.005) | (0.009) | (0.011) | (0.001) | (0.003) | (0.003) | (0.003) | (0.009) |
| LocalShare*1(Director Age¿37) | 0.037*** | 0.009 | 0.023* | 0.029* | 0.001* | 0.008 | 0.001 | 0.002* | 0.003 | 0.002 |
| | (0.012) | (0.011) | (0.012) | (0.015) | (0.000) | (0.023) | (0.032) | (0.001) | (0.005) | (0.029) |
| N | 703,976 | 703,976 | 759,111 | 660,303 | 660,303 | 660,303 | 660,303 | 660,303 | 759,111 | 759,111 |
| First Stage | 0.136*** | 0.136*** | 0.137*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.141*** | 0.141*** |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | 130.76 | 130.76 | 134.16 | 126.7 | 126.7 | 126.7 | 126.7 | 126.7 | 151.26 | 151.26 |

Table 9: Political Turnovers and Heterogeneous Effects of *LocalShare*

Panel A: The Impacts on Sales and Customers

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Growth of Sales | | | | | Uncertainty in Growth of Sales | | | |
| | Local Council | Non-Local Councils | All Councils | Total Firm Sales | Private Sector | All Councils | Total Firm Sales | Concentration | Churn |
| LocalShare | 0.130*** | -0.047** | 0.037 | 0.013 | 0.007 | 0.031*** | 0.012* | 0.580*** | -0.003*** |
| | (0.014) | (0.020) | (0.041) | (0.037) | (0.036) | (0.006) | (0.006) | (0.018) | (0.001) |
| LocalShare*Turnover | 0.013 | -0.015 | 0.011 | 0.001 | 0.011 | 0.011 | 0.001 | 0.002 | 0.017 |
| | (0.011) | (0.010) | (0.039) | (0.006) | (0.021) | (0.010) | (0.010) | (0.009) | (0.020) |
| Turnover | 0.001 | 0.003 | -0.001 | 0.012 | 0.004 | 0.013 | 0.002 | -0.003 | -0.001 |
| | (0.002) | (0.021) | (0.003) | (0.019) | (0.007) | (0.019) | (0.017) | (0.013) | (0.002) |
| N | 759,111 | 759,111 | 759,111 | 105,110 | 105,110 | 759,111 | 105,110 | 317,633 | 317,633 |
| First Stage | 0.137*** | 0.137*** | 0.137*** | 0.083*** | 0.083*** | 0.137*** | 0.083*** | 0.123*** | 0.123*** |
| | (0.012) | (0.012) | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) | (0.010) | (0.010) |
| | 134.16 | 134.16 | 134.16 | 57.3 | 57.3 | 134.16 | 57.3 | 186.778 | 186.778 |

Panel B: The Impacts on Investment and Capital Structure

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Capital Investment | | | Capital Structure | | | | | | |
| | Growth of Fixed Assets | Growth of Current Assets | Growth of Total Assets | Growth of LT Debt | Growth of ST Debt | Growth of Total Debt | Leverage | LT Debt/Total Debt | Survival | Bankruptcy |
| LocalShare | 0.054*** | 0.073* | 0.049*** | 0.075** | -0.035* | 0.007 | -0.144 | 0.017** | 0.068** | -0.013 |
| | (0.015) | (0.038) | (0.009) | (0.030) | (0.017) | (0.045) | (0.085) | (0.007) | (0.026) | (0.010) |
| LocalShare*Turnover | -0.056*** | -0.039 | -0.019* | -0.087*** | -0.053 | -0.030 | -0.004 | 0.012 | -0.057 | -0.002 |
| | (0.016) | (0.054) | (0.009) | (0.017) | (0.051) | (0.047) | (0.089) | (0.013) | (0.038) | (0.011) |
| Turnover | 0.002 | 0.001 | 0.003 | 0.021 | 0.001 | 0.002 | 0.001 | 0.002 | 0.013 | 0.001 |
| | (0.009) | (0.011) | (0.022) | (0.023) | (0.000) | (0.011) | (0.032) | (0.003) | (0.040) | (0.002) |
| N | 703,976 | 703,976 | 759,111 | 660,303 | 660,303 | 660,303 | 660,303 | 660,303 | 759,111 | 759,111 |
| First Stage | 0.136*** | 0.136*** | 0.137*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.141*** | 0.141*** |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | 130.76 | 130.76 | 134.16 | 126.7 | 126.7 | 126.7 | 126.7 | 126.7 | 151.26 | 151.26 |

Table 10: Exclusion Restriction: The Impacts of Local Demand shocks to Non-Supplier Firms

Panel A: The Impacts on Sales and Customers

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Growth of Sales | | | | | Uncertainty in Growth of Sales | | | |
| | Local Council | Non-Local Councils | All Councils | Total Firm Sales | Private Sector | All Councils | Total Firm Sales | Concentration | Churn |
| LocalShare | 0.130*** | -0.047** | 0.037 | 0.013 | 0.007 | 0.031*** | 0.012* | 0.580*** | -0.003*** |
| | (0.014) | (0.020) | (0.041) | (0.037) | (0.036) | (0.006) | (0.006) | (0.018) | (0.001) |
| LocalShare*Turnover | 0.013 | -0.015 | 0.011 | 0.001 | 0.011 | 0.011 | 0.001 | 0.002 | 0.017 |
| | (0.011) | (0.010) | (0.039) | (0.006) | (0.021) | (0.010) | (0.010) | (0.009) | (0.020) |
| Turnover | 0.001 | 0.003 | -0.001 | 0.012 | 0.004 | 0.013 | 0.002 | -0.003 | -0.001 |
| | (0.002) | (0.021) | (0.003) | (0.019) | (0.007) | (0.019) | (0.017) | (0.013) | (0.002) |
| N | 759,111 | 759,111 | 759,111 | 105,110 | 105,110 | 759,111 | 105,110 | 317,633 | 317,633 |
| First Stage | 0.137*** | 0.137*** | 0.137*** | 0.083*** | 0.083*** | 0.137*** | 0.083*** | 0.123*** | 0.123*** |
| | (0.012) | (0.012) | (0.012) | (0.011) | (0.011) | (0.012) | (0.011) | (0.010) | (0.010) |
| | 134.16 | 134.16 | 134.16 | 57.3 | 57.3 | 134.16 | 57.3 | 186.778 | 186.778 |

Panel B: The Impacts on Investment and Capital Structure

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Capital Investment | | | Capital Structure | | | | | | |
| | Growth of Fixed Assets | Growth of Current Assets | Growth of Total Assets | Growth of LT Debt | Growth of ST Debt | Growth of Total Debt | Leverage | LT Debt/Total Debt | Survival | Bankruptcy |
| LocalShare | 0.054*** | 0.073* | 0.049*** | 0.075** | -0.035* | 0.007 | -0.144 | 0.017** | 0.068** | -0.013 |
| | (0.015) | (0.038) | (0.009) | (0.030) | (0.017) | (0.045) | (0.085) | (0.007) | (0.026) | (0.010) |
| LocalShare*Turnover | -0.056*** | -0.039 | -0.019* | -0.087*** | -0.053 | -0.030 | -0.004 | 0.012 | -0.057 | -0.002 |
| | (0.016) | (0.054) | (0.009) | (0.017) | (0.051) | (0.047) | (0.089) | (0.013) | (0.038) | (0.011) |
| Turnover | 0.002 | 0.001 | 0.003 | 0.021 | 0.001 | 0.002 | 0.001 | 0.002 | 0.013 | 0.001 |
| | (0.009) | (0.011) | (0.022) | (0.023) | (0.000) | (0.011) | (0.032) | (0.003) | (0.040) | (0.002) |
| N | 703,976 | 703,976 | 759,111 | 660,303 | 660,303 | 660,303 | 660,303 | 660,303 | 759,111 | 759,111 |
| First Stage | 0.136*** | 0.136*** | 0.137*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.140*** | 0.141*** | 0.141*** |
| | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) |
| | 130.76 | 130.76 | 134.16 | 126.7 | 126.7 | 126.7 | 126.7 | 126.7 | 151.26 | 151.26 |

Table 11: Political Turnovers and Heterogeneous Effects of *LocalShare*

| Panel A: The Impacts on Sales and Customers | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | Growth of Sales | | | | | Uncertainty in Growth of Sales | | | |
| | Local Council | Non-Local Councils | All Councils | Total Firm Sales | Private Sector | All Councils | Total Firm Sales | Concentration | Churn |
| Local Demand Shock | 0.011 | -0.001 | 0.002 | -0.007 | 0.013 | 0.012 | 0.002 | | |
| | (0.201) | (0.109) | (0.141) | (0.145) | (0.136) | (0.379) | (0.388) | | |
| N | 759,111 | 759,111 | 759,111 | 105,110 | 105,110 | 759,111 | 105,110 | | |
| R2 | 0.233 | 0.132 | 0.127 | 0.222 | 0.311 | 0.173 | 0.183 | | |

| Panel B: The Impacts on Investment and Capital Structure | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | Capital Investment | | | Capital Structure | | | | | | |
| | Growth of Fixed Assets | Growth of Current Assets | Growth of Total Assets | Growth of LT Debt | Growth of ST Debt | Growth of Total Debt | Leverage | LT Debt/Total Debt | Survival | Bankruptcy |
| Local Demand Shock | 0.005 | -0.010 | -0.001 | -0.003 | 0.001 | 0.007 | 0.011 | 0.001 | -0.016 | -0.000 |
| | (0.105) | (0.239) | (0.109) | (0.381) | (0.318) | (0.201) | (0.119) | (0.102) | (0.123) | (0.108) |
| N | 703,976 | 703,976 | 759,111 | 660,303 | 660,303 | 660,303 | 660,303 | 660,303 | 759,111 | 759,111 |
| R2 | 0.178 | 0.292 | 0.121 | 0.318 | 0.328 | 0.299 | 0.283 | 0.347 | 0.175 | 0.128 |

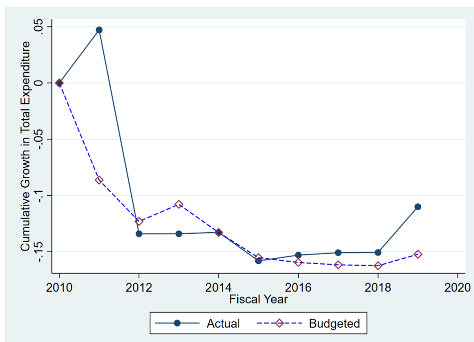
## Appendix A: Coverage of Councils

In the paper, I collect information about 308 councils that make are geographically exclusive councils. As of April 2019, there are 58 unitary authorities, 36 metropolitan boroughs, 32 London boroughs (single-tier authorities); and 181 non-metropolitan districts (the lower-tier under two-tier authorities). In addition, there are several special cases. First, there are consolidation of councils that became effective on 1st April 2019. The merger occurs among neighbouring councils that have close connections or has been under joint management for long time. The consolidation also affects the disclosure of spending information. Therefore, I treat some of the merged councils are single through out the sample period; for other merger cases that I can access information separately, I only keep data before the consolidation date and treat them as individual councils (See Table A0). Second, for Adur and Worthing that are legislatively two separate district councils but are jointly managed, I treat them as single council. Third, data of Dacorum (District) and South Derbyshire(District) can't be accessed.

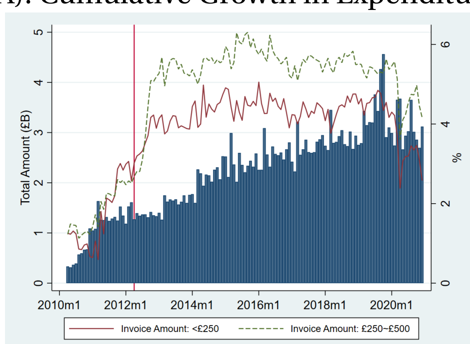
Table A0: Consolidation of Councils

| Effective Date | Before Consolidation | After Consolidation | Treatment |
|---|---|---|---|
| 01-Apr-19 | Forest Heath (District)<br>St Edmundsbury (District) | West Suffolk | As one single council from 2010m4 to 2020m3 |
| 01-Apr-19 | Suffolk Coastal (District)<br>Waveney (District) | East Suffolk | As one single council from 2010m4 to 2020m3 |
| 01-Apr-19 | Tauton Deane (District)<br>West Somerset (District) | Smerset West & Tauton | As one single council from 2010m4 to 2020m3 |
| 01-Apr-19 | Dorset (County)<br>Weymouth and Portland, West Dorset, North Dorset, Purbeck, and East Dorset (All are districts from Dorset County) | Abolished<br>Dorset (Unitary) | As different councils from 2010m4 to 2019m3 |
| | Christchurch (District)<br>Bournemouth (Unitary)<br>Poole (Unitary) | Bournemouth, Christchurch and Poole (Unitary) | As one single council |

# Appendix B:Other Figures and Tables



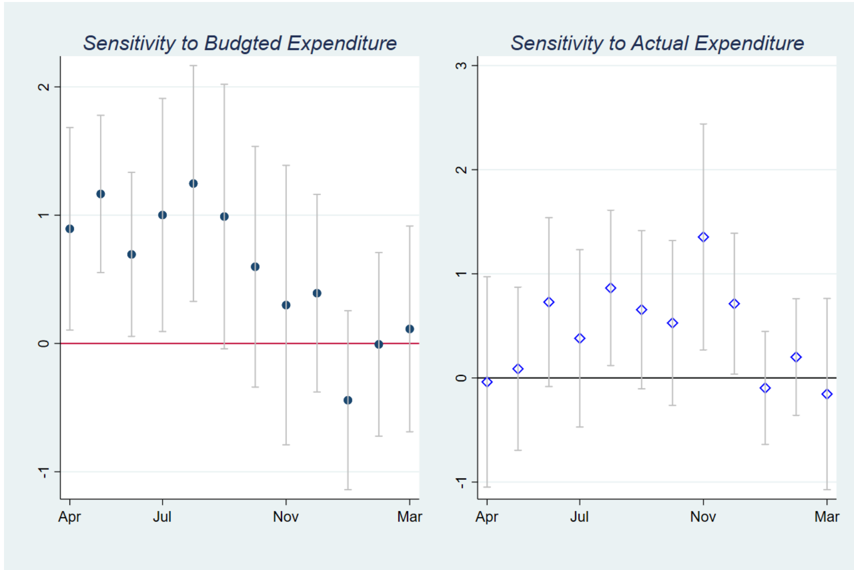

(A). Cumulative Growth in Expenditure




(b). Monthly Sales (Granular Sales Data)

Figure B1: Aggregate Expenditure and Disclosed Monthly Sales over Time

Note: The two figures plots the aggregate expenditure and disclosed spending over time. The upper plot uses aggregate expenditure published annually (both budgeted and actual[29]). It shows the average of cumulative growth rate of each council. The lower plot uses the data set "payment to suppliers" collected manually. I plot the total value of the cash flows in monthly basis. I also check the compliance to disclosure requirements by plotting the share of transactions (in numbers instead of values) that falls below £250 and in between £250 and £500. April 2012 marks the effective date of the Localism Act 2011.

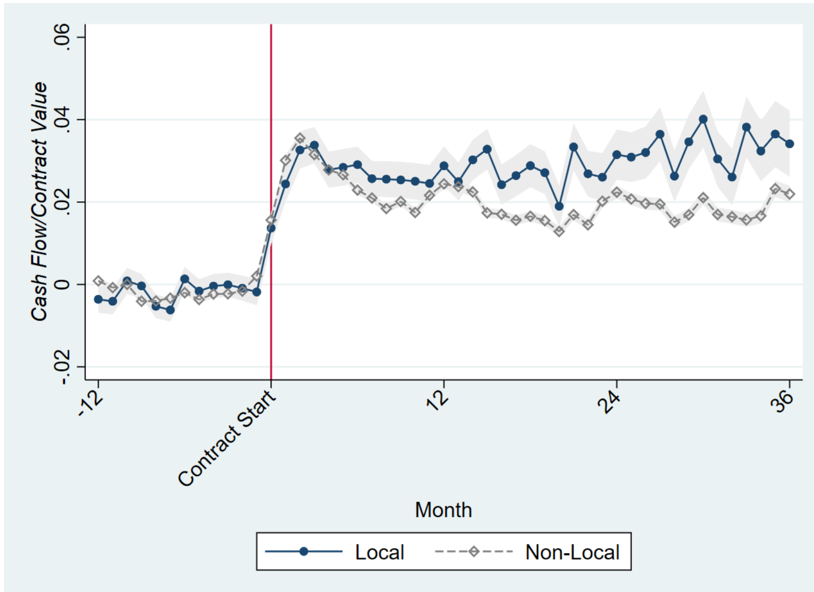



(A) By Month

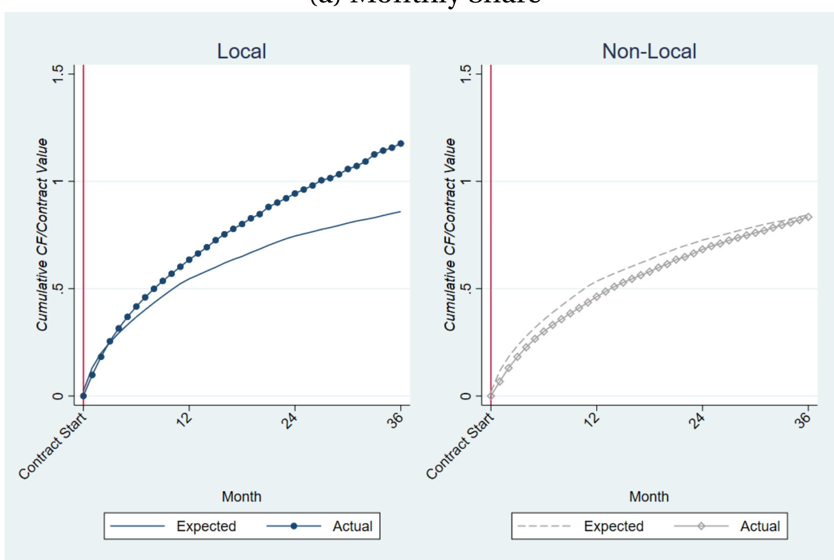

(B) By Industry

Figure B2: Sensitivity of Monthly Sales to Aggregate Annual Expenditure

Note: The two figures function as validation of the collected data on disclosed councils' spending. It plots the sensitivity of each council's year-over-year monthly growth rate in collected spending to each council's annual growth rate in published aggregate expenditure The upper figures plot these sensitivities by each calendar month within a fiscal year.The lower figures plot these sensitivities by sector (defines as the "Section" in UK Standard Industrial Classification (SIC) system.). The sectors are ranked from top to bottom by their share of total spending values.




(a) Monthly Share




(b) Cumulative Share

Figure B3: Cash flows Linked to Contracts

Note: The figures show the cash flows patterns of contracts matched with the "payment to suppliers" data set. The upper figure plots the monthly cash flows as a fraction of contract value in a 48 months period. The cash flows are then normalized by subtracting the average of the 12 months period before the contract starts (which is assumed to be the cash flows without the contract). In the lower figures, I plot the cumulative cash flows after the contract starts. The benchmark "expected" assumes even distribution of cash flows over the contract's period and no follow-up contracts; the "actual" uses the cash flows attributed to the contract. If there are multiple contracts initiated on the same date, they are treated as one single contract.

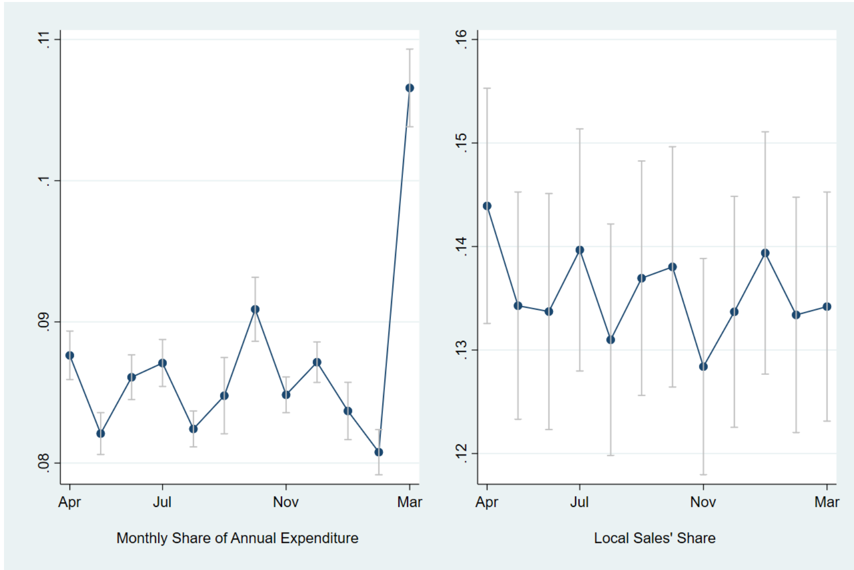



Figure B4: Monthly Distribution of Cash Flows within One Fiscal Year

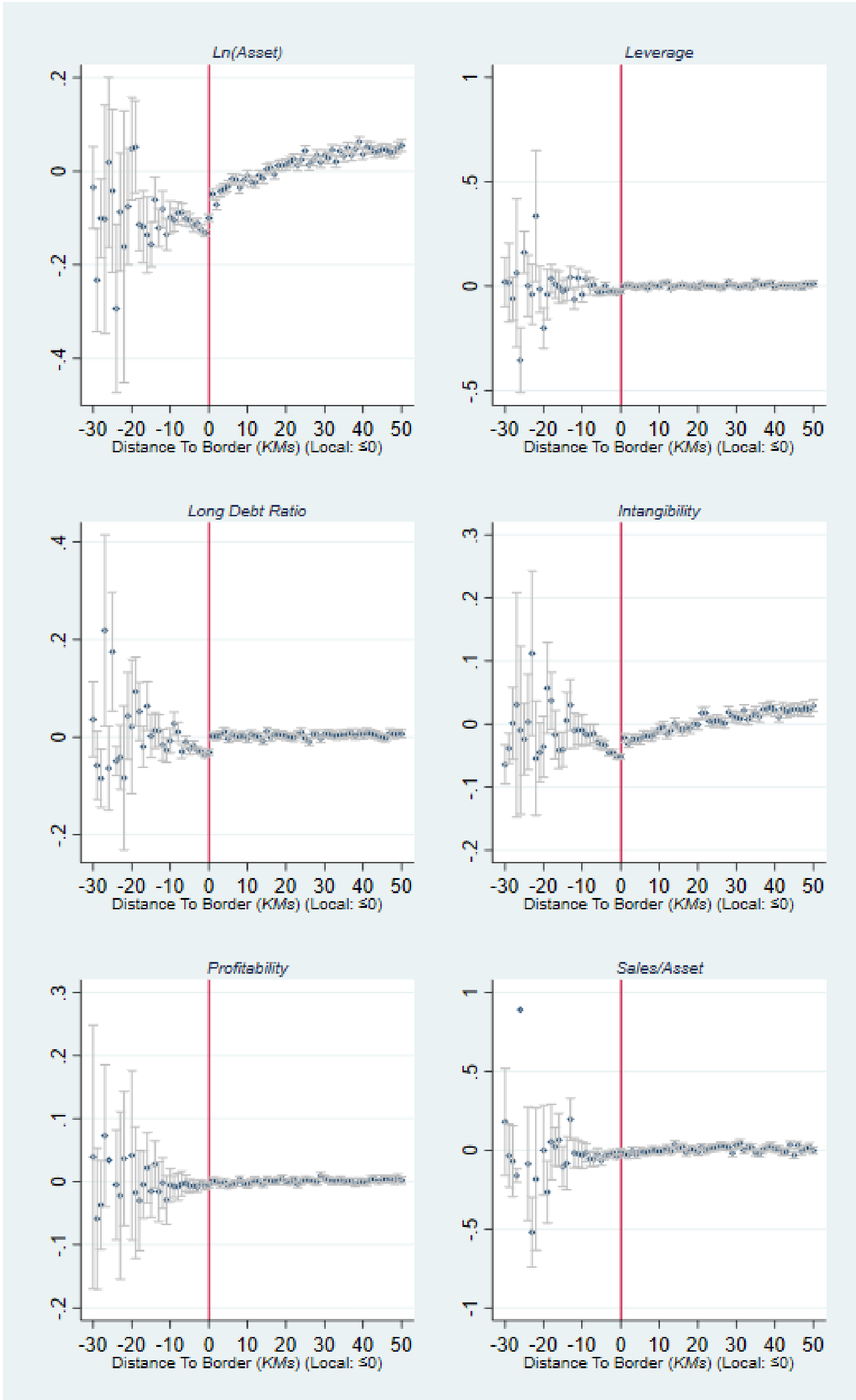



Figure B5: (Potential) Evidence of Selection in Suppliers' Characteristics

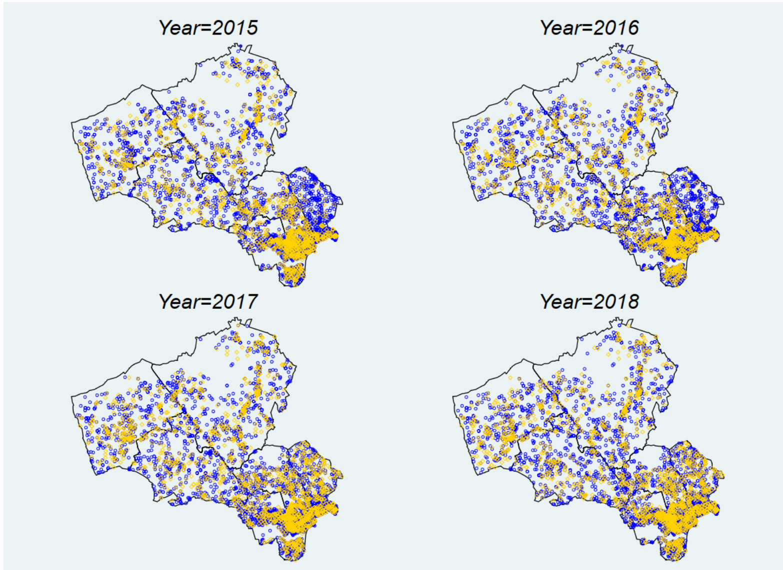



Figure B6: Illustration Example: Suppliers and Dynamics of their Locality

Blue dots represents suppliers if the share of local sales exceeds 50%; yellow represents suppliers if the share of local sales falls below 50%. The five local authorities in the map are Barnet, Islington,Camden, City of Westminster ,Brent and Harrow (Clockwise from the top).The four figures shows the evolution of localities of the suppliers located within these local authorities from 2015 to 2018.

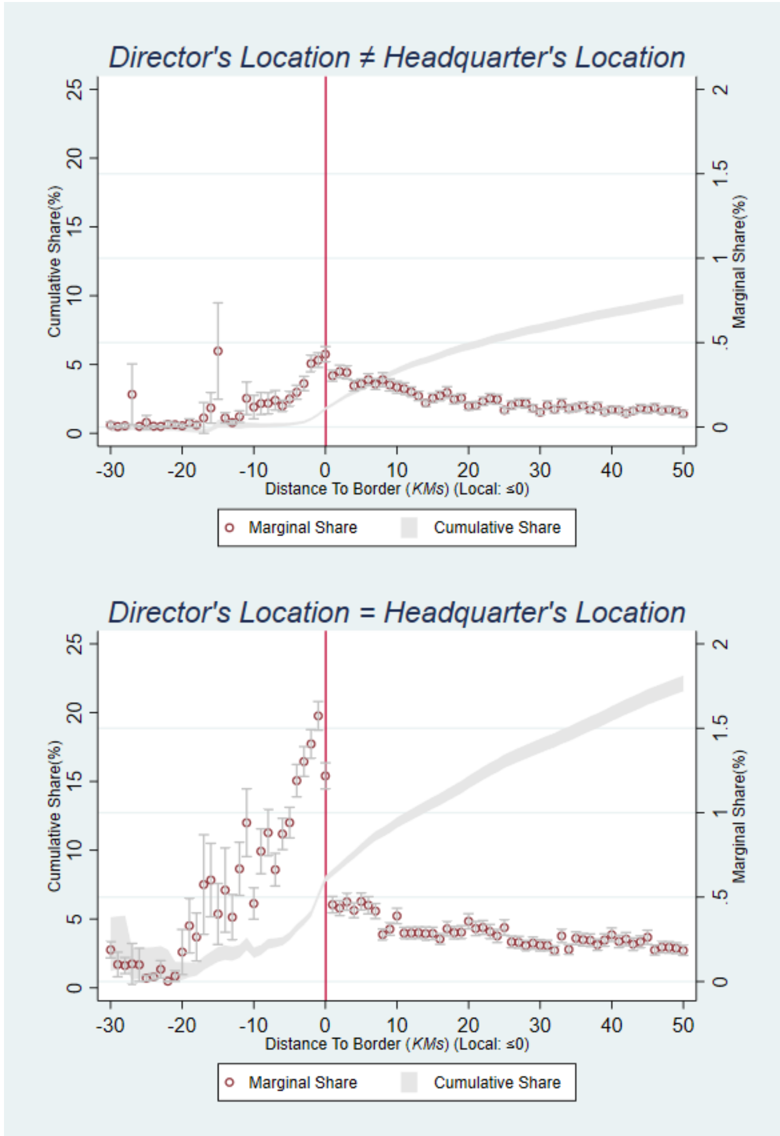



Figure B7: Distribution of Suppliers by Director's Location Near the Boundary

Note: The figure plots the distribution of suppliers for two different types of suppliers. The upper figure is the set of suppliers with directors located in different councils from the headquarter office of the firm; The upper figure is the set of suppliers with directors located in the same councils as the headquarter office of the firm. The horizontal axis is the distance between the supplier to the nearest border of the council customer in kilometer bins. The distance of politically local suppliers are taken as negative. The vertical axis plots the marginal share and cumulative (from left to right) share of total expenditure in each distance bins. Location of supplier is determined by the post code level address of **director** (registered with Companies House) of the supplier (when there are multiple active directors, choose the one closest to the customer council).

Table B7: Summary of Sales to Councils by Suppliers' Sector

| SIC Sector | Share of Council's Total Expenditure | Number of Suppliers | Total Government Sales (£Billion) | Local Share |
|---|---|---|---|---|
| Construction | 0.189 | 37905 | 46.509 | 0.121 |
| Health | 0.160 | 26794 | 43.429 | 0.231 |
| Administration | 0.113 | 37968 | 29.293 | 0.131 |
| Unspecified | 0.078 | 44821 | 18.426 | 0.124 |
| Professional | 0.076 | 40887 | 18.480 | 0.073 |
| Information | 0.048 | 23291 | 9.503 | 0.071 |
| Education | 0.041 | 19124 | 11.147 | 0.352 |
| Other Service | 0.040 | 18853 | 9.764 | 0.263 |
| Water | 0.039 | 2364 | 8.150 | 0.154 |
| Real estate | 0.038 | 17052 | 13.501 | 0.489 |
| Transportation | 0.034 | 9160 | 7.507 | 0.262 |
| Wholesale | 0.031 | 40051 | 6.651 | 0.127 |
| Manufacturing | 0.028 | 25547 | 5.236 | 0.079 |
| Financial | 0.027 | 4976 | 5.802 | 0.023 |
| Arts | 0.014 | 15172 | 2.888 | 0.538 |
| Energy | 0.013 | 656 | 2.609 | 0.063 |
| Accommodation | 0.012 | 15840 | 3.808 | 0.407 |
| Public | 0.009 | 842 | 3.031 | 0.569 |
| Mining | 0.006 | 342 | 1.311 | 0.015 |
| Agriculture | 0.003 | 2677 | 0.403 | 0.276 |

Table B7: Sensitivity of Monthly Sales Growth to Annual Aggregate Expenditure Growth

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| $g^{Budgeted}$ | 0.099 | 0.220* | 0.558*** | 0.632*** | | | | | -0.242* | -0.141 | 0.495*** | 0.570*** |
| | (0.095) | (0.110) | (0.099) | (0.110) | | | | | (0.101) | (0.125) | (0.109) | (0.119) |
| $g^{Actual}$ | | | | | 0.571*** | 0.645*** | 0.401*** | 0.432*** | 0.625*** | 0.672*** | 0.176 | 0.198 |
| | | | | | (0.060) | (0.081) | (0.104) | (0.114) | (0.064) | (0.090) | (0.116) | (0.124) |
| Constant | 0.208*** | 0.209*** | 0.211*** | 0.211*** | 0.213*** | 0.214*** | 0.211*** | 0.212*** | 0.212*** | 0.213*** | 0.212*** | 0.213*** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| Council FE | | Yes | | Yes | | Yes | | Yes | | Yes | | Yes |
| Month FE | | | Yes | Yes | | | Yes | Yes | | | Yes | Yes |
| N | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 | 24213 |
| R-sq | 0.0000 | 0.0753 | 0.0751 | 0.1474 | 0.0037 | 0.0795 | 0.0743 | 0.1466 | 0.0039 | 0.0795 | 0.0751 | 0.1475 |

Note: This table presents the sensitivities of monthly sales growth to the annual aggregate expenditure growth. In the regressions, the dependent variable is year-over-year monthly growth rate of total government sales at council level; and there are two independent variables the annual growth of budgeted expenditure and the annual growth of actual expenditure.In the regressions, I also include council fixed effects and month fixed effects. Standard errors are clustered at the council level and presented in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.