Working Paper 25-040

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Funding for this research was provided in part by Harvard Business School, the Omidyar Foundation, and the Private Capital Research Institute.

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First draft: November 2023; This version: February 2025

Abstract

Impact investors claim to distinguish themselves from traditional venture capital and growth equity investors by also pursuing ESG objectives. Whether they successfully do so in practice is unclear. We use confidential Census Bureau microdata to assess worker outcomes across portfolio companies. Consistent with earlier studies, impact investors are more likely than other private equity firms to fund businesses in economically disadvantaged areas, and the performance of these companies lags behind those held by traditional private investors. We show that post-funding impact-backed firms are more likely to hire minorities, unskilled workers, and individuals with lower historical earnings, perhaps reflecting the higher representation of minorities in top positions. They also allocate wage increases more favorably to minorities and rank-and-file workers than VC-backed firms. Our results are consistent with impact investors and their portfolio companies acting according to non-pecuniary social goals.

Keywords: ESG, private equity, venture capital, wages.

JEL classification: G20.

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1 Introduction

The rapid growth of funds with environmental, social, and governance (ESG) objectives has attracted increasing scrutiny in the finance literature. Much of this literature has examined the realized and/or expected returns of publicly traded stocks and bonds with favorable ESG characteristics (examples include Pastor et al. (2021); Pedersen et al. (2021); Pastor et al. (2022); Hong and Shore (2023); Giglio et al. (2023); Cao et al. (2023)). Increasingly, though, scholars are focusing on understanding the real effects of these investment choices on the broader economy and environment. Two of the most influential of these studies have examined the relative innovative efficiency of more or less ESG-compliant firms in clean technologies (Cohen et al. (2020)) and the environmental consequences of changes in the cost of capital associated with widely disseminated ESG rankings (Hartzmark and Shue (2023)). Both of these papers suggest that a focus on ESG investment may have unexpected, and indeed counterproductive, effects.

ESG-oriented funds focused on the private markets, frequently dubbed impact investors, have also experienced a rapid rise in activity.[1]. Impact investors include affiliates of large private capital groups such as Bain, KKR, and TPG and (typically smaller) groups solely dedicated to such transactions. Much of the academic work to date, in contrast to the literature on ESG and public securities, has focused on their financial performance: examples include Kovner and Lerner (2015); Cole et al. (2020); Barber et al. (2021); Boulongne et al. (2024); Jeffers et al. (2024).[2] This relative neglect of the ex post real outcomes of such investments is surprising, given the increasing activity and the intense controversy they have generated, as seen in efforts in numerous Republican-dominated states in the U.S. to curb their pensions from such investing.

In this paper, we seek to assess the consequence of the provision of capital by U.S. impact investors for firms and workers. This dimension is, of course, only one along which impact investors seek to have consequences. Some funds, for instance, emphasize the development of alternative

[1]Precisely sizing this market is challenging. Many estimates do not carefully distinguish between private and public market investors. Nonetheless, they suggest very substantial growth: e.g., the Global Impact Investing Network (GIIN) estimated the capital under management by impact funds has increased more than thirty-fold from the amount a decade years before (Hand et al. (2024))

[2]Exceptions include the finding in Boulongne et al. (2024) that loans to entrepreneurs in disadvantaged areas yield more job growth than ones to their counterparts elsewhere in France, and the results in Cole et al. (2023) suggesting that impact-backed firms are in sectors with greater social returns.

energy technology or the provision of educational services, to name two popular areas. However, in general, employment considerations are central to impact investors. This assertion can be illustrated, for instance, by the Joint Impact Indicators, launched in 2021 by the World Bank's International Finance Corporation, the GIIN, and a group of leading impact investors.[3] These indicators, which try to standardize the goals of impact investors, have three major categories: climate, gender (much of which relates to that of the management and employees), and jobs. The tabulations and illustrative illustrations in Appendix A.1 also underscore the importance of such objectives.

Our underlying conceptual foundation is based on investors in impact funds having preferences over employee attributes, akin to the framework in Green and Roth (2024). These might include offering opportunities for individuals in disadvantaged groups or preferences for reducing unemployment. These preferences are in addition to focusing on firm profitability and financial returns. Thus, the objective function of the firms in which impact funds invest is multi-dimensional, such as maximizing profitability subject to increasing the number of workers hired from disadvantaged groups. The exact weights on these criteria for the impact-orientated funds and the firms they choose to invest in are not known. Thus, our empirical analysis will provide initial evidence on what ex post changes are associated with impact investments. Note that throughout, we do not claim that the impact investing causes any of the ex post changes we document, as it may be that the impact funds invest in firms already planning on hiring workers from disadvantaged groups and/or operating in underprivileged areas. Regardless of whether the results are driven by selection or treatment, our results reveal an intention to invest in firms whose employment goals are consistent with the investors' goals.

Ultimately, much of the controversy surrounding impact investing is whether the underlying firms selected will implement these stated goals and preferences. In the environmental space, there has been criticism that the firms and funds engage in "greenwashing" to appeal to investors but do not actually improve the environment (Lam and Wurgler (2024)); more generally, this phenomenon is called "impact washing." We directly examine whether impact-orientated funds invest in firms

[3]These are documented at *https://indicators.ifipartnership.org/indicators/joint-impact-indicators-jii*.

located in areas with higher unemployment and higher concentration of workers in disadvantaged groups. In addition, we investigate the types of workers hired and changes in the wages of workers after the impact fund invests.

Our paper focuses on the count, gender, and ethnicity of workers, as well as their wages, a set of outcomes where the data allow precise measurement and whose importance in impact investment goals is widely agreed upon. We seek to understand how well employees, particularly disadvantaged ones, do relative to those in control firms. By focusing on these performance measures, we are not seeking to minimize the importance of other industry-specific indicators, such as greater consumer choice, better educational outcomes, or the decarbonization of the economy. (Indeed, a number of evaluations of the social impact of traditional (non-impact) private investors, particularly those focused on customer welfare (e.g., Bernstein and Sheen (2016); Eaton et al. (2020); Fracassi et al. (2022)) have used such metrics.)

We begin with the sample of portfolio companies of impact investors identified by Cole et al. (2023) and described at length in Burton et al. (2021). We focus on the subset of U.S.-headquartered firms and match them to confidential U.S. Census microdata, in particular, the Longitudinal Business Database (LBD) and the Longitudinal Employer-Household Dynamics (LEHD) database. We use the LBD, which tracks the universe of U.S. establishments over time, to investigate how impact investors select targets for investing and how these firms develop after receiving funding. The LEHD allows us to evaluate worker-level outcomes following impact investments, for both incumbent workers and new hires at portfolio firms.

After identifying impact portfolio companies in the Census data, to benchmark our findings, we construct a matching sample of two different sets of firms: (1) firms backed by traditional venture capital and growth equity investors (henceforth referred to as VC- or venture-backed firms for simplicity) and (2) control firms without backing by a private equity investor. Doing so allows us to simultaneously compare the outcomes associated with impact investments to those of these two different sets of firms where the explicit goals are not impact-oriented. We then evaluate how impact-backed firms differ and evolve compared to these alternative sets of firms. We match using a series of sequentially expanding calipers of firm and deal characteristics to prioritize the best matches. We also, importantly, attempt to capture variation in unobservables by emphasizing matches at the highly granular NAICS-5 industry level.[4] Carefully doing so is essential since, as highlighted in Cole et al. (2023), impact investors tend to invest in industries that are not, on average, representative of traditional venture and growth investors.

We highlight three sets of facts that emerge from the analysis.

First, impact-funded firms are more likely to be located in zip codes with more Black, poor, and unemployed residents than non-impact venture-funded ones or the other matching firms. This pattern holds even after controlling for a rich set of firm- and region-specific variables. Within these areas, impact investors are more likely than venture investors to fund Black- or Hispanic-founded firms. Similarly, impact-funded firms are more likely to have workforces that are more Black, Hispanic, female, and without a college education. These findings are consistent with the industry's own narratives (Zhang (2023)). Thus, firms funded by impact funds do, in fact, fulfill investor preferences to invest areas that are neglected by traditional investors.

Second, impact-backed firms have better post-funding outcomes than matched small firms for workers on employment, payroll, and salaries, as well as for firm revenue and productivity. They also increase the fraction of employees that are Black, Hispanic, and female more than control firms. Relative to similar firms backed by non-impact venture investors, however, impact-funded firms generally lag on employment and payroll, though not on other measures. Once controlling for this overall gap in performance, there is no discernible difference between the impact and traditional venture companies when it comes to firms based in disadvantaged areas. As numerous studies that have documented better performance along some dimensions among firms funded by traditional private investors have acknowledged (for example, see Kortum and Lerner (2000); Boucly et al. (2011); Puri and Zarutskie (2012); Davis et al. (2014)), these patterns are likely to be driven by a combination of a selection and treatment effects.

Finally, the earnings of workers at impact-backed firms in the four years that follow the transactions increase more substantially than at the control firms but less rapidly than at the venture-

[4]For example, new car dealers (NAICS 44111) are differentiated from used car dealers (NAICS 44112). NAICS industries help us capture unobservables since they are defined at the establishment level and are based on the specific processes used to produce goods or services.

backed firms. The relative wage performance of Black, Hispanic, and rank-and-file workers, however, is substantially greater at the impact-backed firms, driven in large part by those who remain at the impact-funded firm for an extended period. Impact investors are also more likely to hire Black, Hispanic, female, and less educated workers.

In short, the results paint a largely positive, if limited, picture of the consequences of impact investing. The disparities with the literature on ESG investing in public markets, where studies have suggested unintended social effects of these investments, should not be too surprising. The ability of investors to monitor and influence the behavior of companies is typically much greater in private markets (Kaplan and Strömberg, 2003; Bernstein et al., 2016; Gompers et al., 2020), and employment-based characteristics are relatively easy to monitor. Indeed, impact investors frequently include terms in contracts with the companies in their portfolios that reflect social objectives, as Geczy et al. (2021) shows.

The rest of the paper is organized as follows. Section 2 describes the data used and our empirical design. Section 3 presents the results, and Section 4 concludes the paper.

2 Sample Construction

We construct our sample in several steps. First, we identify portfolio companies (PCs) of impact investors and venture capital (VC) firms in US Census Bureau data. Second, we match the sample of impact PCs to similar VC PCs and control firms without VC funding. Third, we define economically disadvantaged areas using zip-code-level demographic information. Fourth and final, we filter the firm-level sample and collect relevant worker-level information.

2.1 Identifying Impact and VC Portfolio Companies

We begin by assembling a list of PCs that receive funding from impact investors. We source data on impact PCs from the Project on Impact Investments' Impact Investment Database, detailed in Burton et al. (2021). This dataset combines information from Capital IQ, Crunchbase, PitchBook, Preqin, VentureXpert, and manual web searches. It contains over 4,000 portfolio companies and represents the most comprehensive impact investing database assembled to date.

For the purposes of this paper, given our reliance on US Census data, we require PCs to be located in the US. We also exclude firms funded exclusively by non-US impact investors, assuming (after an examination of a sample of such cases) that investors with missing country information are US-based. PCs receiving both US and foreign impact funding are kept in the sample. We also condition on knowing when a PC first receives impact funding. We define this as the treatment year y for firm-level analyses or treatment quarter q for worker-level analyses, as described later. As in Davis et al. (2014), we define years as centered around March 12, the start of each Census year. That is, for any dates in October–December of the calendar year, we increase the year by one to align it with the years used by the Census.

Next, we link impact PC samples to US Census Bureau data. We merge the PCs with the Longitudinal Business Database (LBD), which contains annual establishment-level information on industry, employment, payroll, and revenue for the universe of firms operating in the US. We do so by linking to the LBD via the County Business Patterns Business Registry (CBPBR), which records the names of US establishments. We use CBPBR name and location (in order of preference: zip code, city, state, or none) to identify sampled impact PCs in the LBD. For linked firms, we require positive employment in event years $y-1$ and y in the LBD. This requirement lets us compare the firm's performance before and after funding and observe firm characteristics before treatment. We also drop firms in NAICS sector 92 (public administration and government).[5]

We seek to compare the impact investments to those by financially-oriented investors. To do so, we create a matching set of investments by venture capital and growth equity funds (henceforth called VC-backed or venture-backed for expositional convenience.) We do not include investments by buyout groups in the analysis as the portfolio companies of the impact firms tend to be small and generally young firms, and quite different from the companies traditionally backed by buyout groups (Davis et al., 2014).

More specifically, the sample of VC-backed firms is sourced from PitchBook. This sample is mutually exclusive from the impact PC sample: any firm that receives impact funding is counted

[5]For additional details on our PC-LBD linking, refer to Appendix A.2.

as an impact PC regardless of whether it also receives VC funding separately. For VC PCs, we only consider funding of deal types "PE growth/expansion," "Seed round," "Platform creation," and any variant of "VC." All other steps are the same as for impact PCs, except we do not define a treatment year until after matching, described next.

2.2 Matching Impact PCs to VC PCs and Control Firms

In the next step, we limit the sample by matching impact PCs to two sets of firms: (1) the VC-backed firms identified in the LBD above and (2) a sample of LBD firms receiving neither impact nor VC funding, which we refer to as the control firms. We match up to five VC PCs and five control firms for each impact PC using a matching procedure that is designed to be flexible while prioritizing matches that are similar along as many measurable dimensions as possible. Flexibility is important since the pool of VC PCs is much smaller than that of control firms. Similarity along multiple dimensions is also important since impact PCs are often small firms that can be highly heterogeneous (for example, two firms may have the same number of employees, but one may have triple the revenue). Our matching approach identifies the most similar firms while also ensuring the sample is sufficiently large.[6]

To start, we pool all firms meeting a baseline set of matching criteria. For each impact PC, we collect VC PCs and control firms that have the same NAICS-2 industry and multi-unit status (if the firm operates one or multiple establishments) and employment and age within +/-50% of the impact PC's. Employment is recorded in year $y-1$ for the impact PC and control firms (where $y-1$ is the year before the impact PC is funded). For VC PCs, we allow for more temporal flexibility by measuring employment one year before the matched funding round (see below) instead of strictly at $y-1$.

We impose additional restrictions that are specific to the matching between VC PCs and control firms. For impact PCs that receive impact funding in their first financing round, we only match to VC PC 1st rounds. For impact PCs getting impact funding for the first time in the second financing round or higher (i.e., that start off with a traditional VC investment before getting any

[6] The procedure, described below, is also fully illustrated in Appendix A.2.

impact funding), we only match to VC PCs in their second round or higher. Within this group, the VC PC matched funding round number needs to be within +/-50% of the round number of the impact PC's first impact funding (rounded down for lower bounds at -50% and up for upper bounds at +50%) and occur within +/-5 years of the impact PC's year y. For example, an impact PC getting impact funding in its first round in 2010 is matched to VC PCs who had their first rounds between 2005 and 2015. Meanwhile, an impact PC not getting impact funding until the second (third) round is matched to VC second and third (second through fourth) rounds. For control firms, which have no funding rounds but are far more numerous than VC firms, we do not impose round requirements but instead match on year $y-1$ as described above.

Next, for each impact PC, we rank these potential VC PC and control firm matches and select the five closest in each category. To rank matches, we apply nearest-neighbor matching (NNM) within a gradual sequence of expanding calipers on size and growth and loosening industry requirements. Starting with pairs matched on NAICS-5 (the closest industry definition), we define a caliper cutoff of +/-5% employment, payroll, and revenue and +/-10% growth in employment, payroll, and revenue. Growth is the annual average in the preceding two years. As in Davis et al. (2014), growth is measured as $(X_t–X_{t-1})/(0.5*(X_t+X_{t-1}))$ each year and is naturally bounded to be between -200% and +200%. This prevents the outsized influence of extreme growth rates, often observed among small firms, on estimates. It is also symmetric in increases and decreases (Davis et al., 1996).[7] The caliper is then sequentially expanded in five and ten percentage point increments for the size and growth variables, respectively, until a maximum caliper of +/-50% in employment, payroll, and revenue and +/-100% in growth employment, payroll, and revenue.[8] If the impact PC has a missing value for any of these variables, we exclude it when ranking matches. We then repeat this process for NAICS-4 pairs, NAICS-3 pairs, and finally, NAICS-2 pairs. Since revenue is only available in specific years, we repeat the above process without conditioning on revenue variables.

[7]For example, a firm that halves in size grows by -67% while one that doubles in size grows by +67%.

[8]To illustrate, consider an impact PC with 20 employees and $1 million in payroll and revenue, with employment, payroll, and revenue growth at 20%. We first look for matching firms within the 1st caliper of 19–21 employees, $0.95–1.05m payroll and revenue, and 10–30% growth. The 2nd caliper expands these criteria to 18–22 employees, $0.9–1.1m payroll and revenue, and 0–40% growth. This repeats until the largest possible caliper of 10–30 employees, $0.5–1.5m in payroll and revenue, and -80–120% growth.

At this stage, some impact PCs may still have fewer than five VC PC and control firm matches if these fail to meet the NNM calipers defined above. For these, we find further matches by applying Mahalanobis distance to find the closest control firms without imposing a caliper cutoff. We first choose the closest matches using employment, payroll, and revenue by NAICS-5, then NAICS-4, NAICS-3, and NAICS-2. We then repeat this process using only employment and payroll as matching variables. At the end of the procedure, each impact PC will ideally have ten matched firms (five VC PCs and five control firms), although this is not necessary for it remain in the sample. Matched VC PCs are assigned the counterfactual treatment year y corresponding to the round of the matching impact PCs' first impact funding (so that 1st-round impact PCs are compared to 1st-round VC PCs and later-round to later-round). Matched control firms are assigned the same counterfactual treatment year y as the impact PCs they are matched to.

2.3 Defining Economically Disadvantaged Areas

One of the main elements in our analysis is the role of operating in economically disadvantaged areas. To identify these areas, we download data from the IPUMS National Historical Geographic Information System (NHGIS), which aggregates publicly reported US Census Bureau data. We identify four types of economically disadvantaged areas as those in the top 10% of zip codes by (1) fraction of the population that is Black, (2) fraction of the population without any college education, (3) poverty rate, or (4) unemployment rate. We also create a generic disadvantaged area definition if any of the four previous criteria are met (corresponding to roughly 20% of US zip codes). In choosing to focus on zip codes that were the top decile of the Black share, we are motivated by the (negative) association of these areas with measures of economic opportunity; Economic mobility data published by Opportunity Insights shows that nearly two-thirds of the top 10% of counties by black population are also in the bottom 10% by mobility. Meanwhile, this same number is near-zero for Hispanic-dominated counties, which are frequently in the Southwest and have more robust economic growth and social mobility.[9]

[9]Source: The Opportunity Atlas (*https://www.opportunityatlas.org*) and IPUMS. The Opportunity Atlas does not report zip-code-level data, but these figures are also similar at the tract level (which are more granular than zip codes).

2.4 Finalizing the Sample

From this dataset, we finalize the three primary samples that form the core of our analysis. The first is a sample of firms. To evaluate firm performance, we first require firms to have non-missing and non-zero revenue. The LBD records employment and payroll from 1976 to 2020, but revenue is only available for 1997 through 2018. Moreover, we also require firms to have sufficiently long time-series after including lagged employment growth as a control variable (where growth is the average from the two preceding years). In implementation, this means that firms must receive impact funding (or the matched counterfactual) in their third year of operation or later. The final sample comprises 7,300 firms: 700 impact PCs, 2,000 VC PCs, and 4,600 control firms.[10]

The second sample is worker-level data for individuals employed at sample firms just before treatment (receiving impact funding) or the matched equivalent. We identify these individuals, whom we call incumbent workers, in the Longitudinal Employer-Household Dynamics (LEHD). The LEHD, which the US Census Bureau maintains, records a quarterly panel of earnings for all worker-employer pairings in the US. After linking LBD sample firms to employers in the LEHD, we select the workers employed at these firms in quarter $q-1$. For these individuals, we (1) pull all earnings information +/-16 quarters around the event date q, (2) drop any jobs paying less than $1,000 quarterly, (3) drop worker-years with more than ten different jobs, and (4) keep only one primary job per worker-quarter (the highest-paying job). After doing so, we are left with a quarterly worker panel with up to 33 quarters per worker. We identify the following worker characteristics in the LEHD: age, years of education, sex, race, and ethnicity. Finally, we drop observations where workers are younger than 18 (if not college-educated) or 22 (if college-educated) and where workers are older than 65. Since the worker-level analyses do not require the same control variables as the firm-level analyses do, workers are identified before filtering out firms with missing revenue or lagged firm growth. This keeps the sample as comprehensive as possible, while also extending the panel to include observations from 1992 to 2021. The incumbent worker sample consists of 86,500 employees of impact PCs, 102,000 of VC PCs, and 369,000 of control firms.

[10] Given this sample construction approach, the probability that a firm is impact- or venture-backed is considerably greater in the sample than in the economy as a whole.

Third and finally, we identify workers who start working at sample firms after treatment (the year of the first impact investment) or the matched equivalent. We call these workers new hires and apply the same procedure as for incumbent workers. The only difference is that instead of identifying workers employed at sample firms at $q-1$, we identify workers who begin working at sample firms in the first year after the firm's treatment ($q+1$ through $q+4$). We identify 197,000 new hires: 34,500 recruited by impact PCs, 59,500 by VC PCs, and 103,000 by control firms.

3 Results

3.1 Variable Definitions and Summary Statistics

Table 1 contains variable definitions for the variables we use in our analysis. Our key variables include detailed firm-level data from the Longitudinal Business Database (LBD) and employee-level data from the Longitudinal Employment and Household Dynamics (LEHD) database at the Census Bureau. These variables are described in Table 1. We have both quarterly wages and demographic and education characteristics from the decennial census.

Insert Table 1 here

Table 2 presents summary statistics for the firms in our sample. Statistics are presented for impact investment portfolio companies (PCs), venture capital portfolio companies, and matched control firms. For continuous variables, we report pseudo-medians (the mean of observations within the 45th to 55th percentile) to comply with Census disclosure requirements. For dummy variables, we instead report means since medians are uninformative. Panel A describes the cross-section of firms in the year prior to the company receiving impact or venture funding, along with characteristics of the matched control firms.

Matched control firms are matched in that year using the procedure described in the data section. In brief, firms are matched by industry, sales, payroll, the number of employees, age, and the number of establishments. The firms matched to the impact firms as control firms in column 3 are similar in the matching variables. There are significant differences in unmatched characteristics such as revenue over payroll (profitability). We control for these characteristics in later tables.

As the table shows in columns 1 and 2, firms with impact investment are larger and older than the median VC-backed firms in our sample. Lastly, the impact-funded firms survive at higher rates than the control firms but at a lower rate than the VC-backed firms. The largest difference economically for the impact-funded firms versus either VC-backed firms or control firms is that they are significantly more likely to invest in firms that operate in disadvantaged areas, with 31% of impact firms operating in disadvantaged areas. Only 18% of venture firms and 23% of control firms operate in these areas.

On other characteristics, such as the percentage of workers without a college education and the characteristics of incumbent and newly hired workers, the impact-funded firms are typically higher than VC-backed firms but lower than control firms. For example, 27% of new hires at impact-funded firms are Black or Hispanic, while VC firm hires are only 19%, and control firm hires 30% Black or Hispanic. Incumbent workers at VC-backed firms are less frequently minorities and more often have college degrees than workers than impact-funded firms.

Insert Table 2 here

3.2 Firm Funding Likelihood

One of the goals of impact funding is to address socially disadvantaged groups or areas. We test whether it, in fact, fulfills this aim or mission by examining whether areas that have high unemployment, high poverty, high Black populations, or few college graduates are more likely to get impact funding versus venture capital funding. We compare areas using zip codes and examine the top 10% of disadvantaged areas/groups.

In Table 3, we run the multinomial logistic regression model on a cross-sectional sample of firms:

$$FundingType_{i,n,s,t} = \beta_1 Disadvantaged_i + \lambda X_i + \pi_n + \sigma_s + \tau_t + \epsilon_{i,n,s,t} \quad (1)$$

where $FundingType_{i,n,s,t}$ is a set of outcome variables that represent if firm i in industry n and state s receives impact funding, VC funding, or neither in year t. $Disadvantaged_i$ is a dummy equal to one if the firm operates in a disadvantaged area prior to funding (measured with four different proxies, as described above). X_i is a vector of explanatory variables, recorded one year before the event, that may influence a firm's likelihood of receiving impact or VC funding: size (employment), profitability, growth, and firm age. π_n, σ_s, and τ_t are dummies for industry, state, and year.

Insert Table 3 here

Inspection of the results in Table 3 shows that, indeed, companies that receive impact funding are in areas of higher poverty, higher unemployment, and with higher Black populations. Firms that receive venture capital funding are not statistically different from firms not receiving financing, except that they are significantly less likely to invest in areas with the lowest decile of college graduates. In contrast, firms that receive impact funding are not different in terms of the likelihood of being in the top 10% of zip codes with the least college graduates. Across the board, firms operating in disadvantaged areas are consistently more likely to receive impact funding than VC funding: this difference is significant at the one percent confidence level in all four area types. Expressed as marginal effects, the magnitude of this differential ranges from 5.2 percentage points in high-unemployment areas to 11.5 points in areas with least college graduates.[11] This is substantial relative to the baseline chance that a random firm in our sample receives impact funding (10%) or VC funding (27%). Note that these baseline probabilities are, by construction (due to the matching process employed), not representative of the funding likelihood of for the full US firm population.

An additional potential goal of impact funds is to invest in firms with management teams and workers that come from underrepresented groups, even after controlling for firm location. Table 4 uses a similar multinomial logit specification as Eq. 1, but with an additional control variable

[11] These estimates are calculated by subtracting the estimated marginal effects of $Disadvantaged_i$, displayed in brackets, for VC funding from impact funding. For disadvantaged areas based on unemployment (columns 7 and 8), the differential is [0.023] – [-0.029] = 5.2pp. For areas with least college graduates (columns 3 and 4), it is [0.013] – [-0.102] = 11.5 pp.

$FirmDemographics_i$:

$$FundingType_{i,n,s,t} = \beta_1 FirmDemographics_i + \lambda X_i + \pi_n + \sigma_s + \tau_t + \epsilon_{i,n,s,t} \qquad (2)$$

where $FirmDemographics_i$ represents a different dimension of firm demographics in each of Table 4 Panels A, B, and C. In Panel A, $FirmDemographics_i$ is a dummy equal to one if one of the founders (defined as the three highest-paid employees in the year of the firm formation) are from a disadvantaged group: Black or Hispanic, female, or without any college education. In Panel B, $FirmDemographics_i$ is instead one if the top team in the year before the treatment (defined similarly) is in these categories. In Panel C, $FirmDemographics_i$ captures the percentage of the workforce in these categories. $Disadvantaged_i$ is now included in control vector X_i as an indicator variable that equals one if the firm is in any one of the disadvantaged area types as defined in Table 3. The remaining controls are the same as in the previous table.

Insert Table 4 here

Inspection of the results in Table 4 shows the following findings: (1.) Impact-funded companies do not differ significantly from the control companies in the likelihood that their founding top team contains Black, Hispanic, or female individuals. (2.) Venture capital funded companies are, relative to the control firms, less likely to have founding top team members that are Black or Hispanic and female. (3.) Both VC-funded and (less consistently) impact companies are less likely to have founding and top teams that include college-educated members relative to the controls. (4.) Both impact and VC firms are less likely to invest in firms with disadvantaged employees than the control firms. (5.) Impact investors are significantly more likely than VCs to invest in firms with Black or Hispanic founders (by 5.7 percentage points at the margin (=1.5 – -4.2, columns 7 and 8)) and with a disadvantaged workforce. More specifically, a 10 percentage point increase in the fraction of workers that are not college educated corresponds to a 1.8 percentage point lower chance of getting VC funding relative to impact funding (=(-0.9 – -18.6)/10, columns 7 and 8). Similarly, the differentials are 1.1 and 0.7 percentage points for minorities and women, respectively. These results are consistent with impact investors caring more about who is running and working at the firm, as well as the literature documenting significant disadvantages for minorities and women in accessing venture finance (e.g., Calder-Wang et al. (2021); Cook et al. (2022)).

3.3 Impact Funding and Firm Outcomes

We now examine the ex post growth, productivity, profitability, and employment of firms that receive impact versus venture capital funding. We estimate these post-funding differences using matched difference-in-difference regressions with firm and year fixed effects. We include multiple years of post-funding for each firm, including the impact and venture-funded firms for all years from 1997 to 2018, to have comprehensive coverage. We also include as control firms the firms not receiving funding that are matched to impact-funded firms. For all firms, we restrict data to a nine-year event window (+/-4 years).

Table 5 tabulates estimates from the following regression model:

$$Y_{i,t} = \alpha + \beta_1 Impact_i \times Post_{i,t} + \beta_2 VC_i \times Post_{i,t} + \beta_3 Post_{i,t} + \lambda X_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \qquad (3)$$

where $Impact_i \times Post_{i,t}$ is an indicator that firm i in year t has received impact funding, and $VC_i \times Post_{i,t}$ for VC funding. $Post_{i,t}$ indicates that the firm has received impact funding (for impact PCs), VC financing (for VC PCs), or is in or after the corresponding matched year (for control firms). $X_{i,t}$ is a vector of controls for age, lagged employment growth, and lagged profitability. μ_i and τ_t are firm and year fixed effects. The outcome variable $Y_{i,t}$ is one of nine different variables, each corresponding to a column in Table 5: (1) employment, (2) payroll, (3) revenue, (4) average salary, (5) productivity, (6) profitability, and (7)–(9) the share of the workforce that is disadvantaged, measures as in Table 4. Columns (4)–(5) and column (6) include $ln(1/Emp)$ and $ln(1/Pay)$, respectively, as additional controls in $X_{i,t}$ to account for any mechanical correlation caused by the scaling factor (denominator) applied to the dependent variable (see Chaney et al. (2020)).

Table 5 shows the results from our difference-in-difference regressions. We can see that both impact funding and venture funding are associated with improved growth (columns 1–3) and performance (columns 4–6) relative to unfunded control firms. However, firms receiving VC funding have higher employment and payroll growth than impact-funded firms. Specifically, the estimates associate impact investing with an 18.3–20.8% increase in scale compared to 22.1–27.8% for VC funding. Interestingly, the average salary appears to increase to a similar extent in impact-backed and VC-backed portfolio companies, suggesting that the payroll effects are driven by the number of workers, not their compensation. These effects may reflect the differing size of these investments: if we compare the size of all first-round rounds of US firms and with at least one US investor between 1997 and 2018 in PitchBook, expressing the investment values in 2019 US dollars, we see substantial disparity. The average size of such financings with only traditional VCs is $5.4 million, as opposed to $2.8 million for the impact investor-present financings. (The median sizes are much closer.)

It should be noted that since the performance metrics in columns 4–6 are ratios, they can be fairly noisy, especially for small, entrepreneurial firms. In Section 3.4, we investigate salary developments for workers at the granular worker level. Finally, we see in the final columns that impact firms increase the share of Black or Hispanic and female workers after the investment, relative to both VC-backed or control firms. The magnitude of this effect is a 0.7 percentage point increase for Black or Hispanic workers (compared to a sample median of roughly 9% before funding) and 1.2 percentage points for women (median of 32% before funding).

Insert Table 5 here

We present in Figures 1, 2, and 3 graphs associated with the nine columns of Table 5. In each case, we estimate coefficients for the impact firms relative to the control firms, from four years before to four years after the transaction. In Figure 1, we see a sharp immediate effect of the transaction on hiring and payroll, with a lagged impact on revenue (which seems sensible). Similarly, in Figure 2, productivity and profitability only respond to the transaction with a lag. We see a gradual upward trend in the share of workers who are disadvantaged in Figure 3. With the exception of Figure 3, Panel (c), we do not see much evidence of pre-trends.

Insert Figure 1, 2, and 3 here

Table 6 repeats the analysis in Table 5 but now includes a dummy variable and interaction terms equal to one if the firm operates in an economically disadvantaged area, as defined above. We seek to determine whether impact funds are better at fostering growth for firms in these areas. Formally, Eq. 4 below expands on the specification in Eq. 3 as follows (shortening *Disadvantaged* to *Disadv*):

$$Y_{i,t} = \alpha + \beta_1 Impact_i \times Disadv_{i,t} \times Post_{i,t} + \beta_2 VC_i \times Disadv_{i,t} \times Post_{i,t} + \Lambda Z_{i,t} + \lambda X_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \quad (4)$$

where $Y_{i,t}$, $X_{i,t}$, μ_i, and τ_t are the same as in Eq. 3. $Impact_i \times Disadvantaged_{i,t} \times Post_{i,t}$ and $VC_i \times Disadvantaged_{i,t} \times Post_{i,t}$ are indicators for impact PCs and VC PCs, respectively, operating in disadvantaged areas after receiving funding. $Z_{i,t}$ is the vector of remaining interaction terms of $Disadvantaged_{i,t}$ and $Post_{i,t}$ with $Impact_i$ and VC_i (not listed here, for brevity, but displayed in Table 6). Note that $Disadvantaged_{i,t}$ is not absorbed by firm fixed effects since, in rare instances, firms may move into or out of disadvantaged areas during the event window.

As the results in Table 6 show, impact investors do not appear to be more skilled at promoting growth for firms in disadvantaged areas than either VC-backed firms or control firms.

Insert Table 6 here

In the final analysis in this section, in Table 7 we examine whether the firm ceases operations in the years after the financing using the linear probability model:

$$Death_{i,n,s,t+x} = \alpha + \beta_1 Impact_i + \beta_2 VC_i + \lambda X_i + \pi_n + \sigma_s + \tau_t + \epsilon_{i,n,s,t} \quad (5)$$

where the dependent variable $Death_{i,n,s,t+x}$ indicates whether the firm ceases operations in one of the four years x after the treatment year t. x varies by column to compare short- and long-run effects. We include the same control variables in vector X_i as before in Eq. 1 (size, profitability, growth, and age), as well as dummy variables for industry, state, and year (π_n, σ_s, and τ_t: we have one observation for each firm so cannot use firm fixed effects). We exclude cases where a firm is acquired from the analysis due to the ambiguities of interpretation here: for instance, a facility may be shut after an acquisition to consolidate operations in a single facility, even if sales and profits are sharply increasing.

When we compare the dummies for impact- and VC-backed firms, we see no significant differences from each other. Nor do they differ from the control firms in any sort of consistent manner. There is little suggestion that these firms are either more or less likely to survive. An important takeaway here is that our results are unlikely to be biased by differences in survivorship by firm type ("differential attrition"). For example, if underperforming VC-backed firms were more likely to shut down than impact-backed firms, this could drive the difference in our results. In our setting, this does not appear to be the case.

Insert Table 7 here

As we saw earlier, impact investors are more likely to invest in firms with disadvantaged areas and work forces. Despite this tendency, we see higher growth for both impact and venture-funded firms. To be sure, impact-funded firms have less growth in employment than venture-backed firms. The results are consistent with impact-funded firms being selected to fulfill other objectives than just growth, which may be stronger motivations for firms funded by venture capitalists.

3.4 Employees of Impact Investing Firms

Next, we examine the earnings for workers who work at either impact or venture-funded firms and compare them to workers at our control sets of firms. We examine worker earnings for existing incumbent workers (workers that are already employed at sample firms one quarter before the funding quarter), newly hired workers, and workers that separate from the firm. We examine the earnings of non-disadvantaged workers and those workers in disadvantaged groups. The event window is limited to +/-16 quarters around the funding date (or matched counterfactual). We include worker, year-quarter, industry, and state fixed effects, and control for worker age.

Our first look is at worker compensation at the treated and control firms. Table 8 presents estimates from matched difference-in-difference regressions using our quarterly panel of workers employed at sample firms that receive impact funding, VC funding, or neither, with the following specification:

$$
\begin{aligned}
Y_{j,n,s,t} = \alpha + \beta_1 Impact_j \times Post_{j,t} + \beta_2 VC_j \times Post_{j,t} + \beta_3 Post_{j,t} + \beta_4 Impact_j \times WorkerType_j \times Post_{j,t} \\
+ \beta_5 VC_j \times WorkerType_j \times Post_{j,t} + \beta_6 WorkerType_j \times Post_{j,t} + \lambda X_{j,t} + \pi_n + \sigma_s + \gamma_j + \tau_t + \epsilon_{j,n,s,t} \quad (6)
\end{aligned}
$$

where $Y_{j,n,s,t}$ is the logarithm of worker earnings for worker j in industry n, state s, and year-quarter t. $Impact_j$, VC_j, and $Post_{j,t}$ is defined as in Eq. 3 and elsewhere in the paper. $WorkerType_j$ is an indicator for worker characteristics that varies by column in Table 8: Black or Hispanic (2), female (3), without college education (4), or rank and file (5), which are those workers that are not on the top team (the top three earners at the firm one quarter before treatment). Column (1) excludes the $WorkerType_j$ variable. The vector $X_{j,t}$ consists of a worker age control. π_n, σ_s, γ_j, and τ_t are industry, state, worker, and year-quarter fixed effects.

Insert Table 8 here

The results in column 1 show that both impact and venture-funded firms pay their workers more overall after getting funding than workers at our control set of matched firms. Workers at VC portfolio companies see an estimated 3.4% increase in earnings compared to those at control firms, while this increase is slightly lower at 3.1% for workers at impact-backed firms. These results are consistent with pay being correlated with productivity increases, consistent with the evidence of greater labor productivity gains at VC- and impact-backed firms in Table 5. Impact-funded firms pay more than our control firms for all types of workers but pay less than VC-funded firms.

Figure 4 looks at the estimated wage premium associated with impact firms relative to controls, estimated from an analysis of incumbent workers (as in Table 8). The estimates are on a quarterly basis from four years before to four years after the transaction. We see the absence of pre-trends and the strong positive increase in wages around the time of the transaction.

Insert Figure 4 here

While overall wage increases are higher at VC firms' portfolio companies and those of impact investors, how these wage increases are distributed among workers can differ significantly. In each of columns 2–5 of Table 8, we separately introduce and interact with an indicator variable indicating if the worker belongs to a group of a particular interest: in column 2, Black or Hispanic, in 3, female, in column 4, not college-educated, and finally, whether the workers are rank-and-file as opposed to management. These regressions are triple difference-in-difference ones, where the interaction of $Impact_j \times WorkerType_j \times Post_{j,t}$ captures the earnings effect associated with impact investing for minority or disadvantaged workers specifically (and the equivalent for VC).

The results show that Black and Hispanic workers, as well as rank-and-file ones, do better in relation to their peers at impact-funded firms versus VC-funded firms. Economically, Table 8 estimates that earnings increases for workers that are Black or Hispanic are 2.2% lower at VC PCs, versus 1.0% lower at impact PCs. Interestingly, we see much less of a differential across gender. This non-result is consistent with the finding in Fang et al. (2023) that private capital investment is associated with an alleviation of gender gaps in compensation within firms.[12]

In the last column, we observe the largest differences for rank-and-file workers. Managers at impact-backed firms see pay increases of 3.0% versus 6.7% for managers at VC-backed firms. However, for rank-and-file workers, the relative earnings increase is 0.1% higher at impact PCs and 3.5% lower at VC PCs. In other words, practically all of the wage increase differential between VC- and impact-backed firms is concentrated among the top earners relative to rank-and-file workers. Our findings show that, while earnings increases are higher overall for employees at firms receiving VC funding than impact funding, how these two types of portfolio companies choose to distribute higher wages differs significantly.

We next examine new hires - workers that join sample firms during the year after treatment (or matched counterfactual), more specifically, during quarters t+1 through t+4. We examine the hiring patterns of firms after they receive impact and VC funding versus the matched control firms. We use the entire pool of new workers hired by the three sets of firms as observations and examine whether worker characteristics are associated with a higher or lower probability of being hired by impact-backed firms or VC-backed firms relative to the unfunded control firms.

[12] Interestingly, their effect seems largely driven by the replacement of high-wage workers, who tend to be male and older, with younger ones who are more likely to be female, while we are looking at compensation changes of individual workers.

Table 9 presents results from the following multinomial logistic regression:

$$HiringFirmType_{j,n,s,t} = \beta_1 BlackHispanic_j + \beta_2 Female_j + \beta_3 NoCollege_j + \lambda X_j + \pi_n + \sigma_s + \tau_t + \epsilon_{j,n,s,t} \tag{7}$$

where $HiringFirmType_{j,n,s,t}$ is a set of outcome variables representing if worker j's hiring firm is an impact PC, VC PC, or matched control firm. $BlackHispanic_j$, $Female_j$, and $NoCollege_j$ are indicator variables that the worker is Black or Hispanic, female, or without any college education, respectively. X_j is a vector of explanatory variables describing worker j: indicator variables for if their previous job was in a disadvantaged area or in the same industry as the hiring firm, their average prior earnings four years before being hired, and their age. As elsewhere, π_n, σ_s, and τ_t are dummies for industry (of the hiring firm), state, and year, respectively.

Insert Table 9 here

Table 9 shows that new workers at impact-funded firms are significantly more likely to be Black or Hispanic, female, and without a college education than those at VC-backed firms. More specifically, the results for impact-funded firms are similar to workers at the control set of firms, while the results for VC-funded firms show a significantly lower likelihood of hiring disadvantaged workers. Minorities, women, and workers without a college education are respectively 7.4, 2.5, and 2.6 percentage points less likely to be hired by VC PC than impact PCs at the margin (respectively: =1.3 – -6.1; =0.7 – -1.8; =0.1 – -2.5, columns 7 and 8). These results are consistent with the firm-level results presented earlier and show that the results extend to new hires and also to the individual worker level. We also see that for both impact and VC-funded firms, workers hired tend to have higher average prior earnings and to be younger than at control firms.[13] However, prior earnings are significantly higher for new hires at VC PCs than impact PCs.

In the context of our previous results, this implies that not only are impact investors more likely than VC investors to allocate capital at the fund level to minority-run businesses and firms

[13] $PriorEarnings_j$ is the natural log of worker j's average quarterly earnings for the period $q = -16$ through $q = -1$. Some of these quarters may be periods of unemployment, defined as those with earnings under \$1,000. As such, $PriorEarnings_j$ is a holistic measure of the worker's earnings, including lost income due to unemployment. In contrast, the panel regressions of Table 8 and, later, 10, measure changes in wages, conditional on employment; Unemployment quarters are dropped because they take a value of 0 and the dependent earnings variable is a logarithm.

in economically disadvantaged areas (Tables 3 and 4), but their portfolio companies also choose to internally allocate capital at the firm level differently. For impact firms, pay raises and subsequent wage growth are distributed more equitably among minorities (Table 8) and minorities and economically disadvantaged workers are more likely to be hired (Table 9). All of these results are consistent with impact investors themselves *and* the companies they invest in fulfilling the objectives of achieving employment-related social benefit goals.

Finally, Table 10 looks at wages for three subgroups of workers.

Panel A resembles Table 8 with the same specification in Eq. 6, but here we narrow the focus onto "continuers": workers that do not move to a different firm in the four years after the event.

In Panel B, we assess whether "switchers"—workers that change jobs in the four years after the event—transition to higher-paying firms after their employer receives impact funding, VC funding, or neither. To do so, we replace the dependent variable of Eq. 6 with firm-specific wage premiums, as proposed by Abowd et al. (1999). To estimate these firm-specific wage premiums, we take the following steps using a similar methodology to Lachowska et al. (2020) and Arnold et al. (2023). First, we start by regressing logarithmic worker earnings on firm, state, worker, and year-quarter fixed effects. This model is formally specified as:

$$Y_{i,j,s,t} = \alpha + \mu_i + \sigma_s + \gamma_j + \tau_t + \epsilon_{i,j,s,t} \tag{8}$$

where $Y_{i,j,s,t}$ is logarithmic worker earnings for worker j at firm i in state s and year-quarter t. μ_i, σ_s, γ_j, and τ_t are firm, state, worker, and year-quarter fixed effects. Industry fixed effects are unnecessary due to firm fixed effects. Next, we save the estimated firm fixed effects and winsorize them at 5% tails by year-quarter to reduce the effect of outliers. These firm fixed effects are the firm-specific wage premium, which is then used (in logarithmic form) as the dependent variable in a regression otherwise identical to Eq. 6.

Panel C analyzes our sample of 197,000 new hires in event time. The regression specification is the same as Eq. 6, with one difference: $Post_{j,t}$ now takes a value of one when worker j joins the sample firm, not when the sample firm received funding (or the matched counterfactual). By construction, these can diverge from each other by no more than one year (see above).

The results suggest a variety of patterns. First, as with the workers as a whole, we see that the continuers at impact-backed firms see greater wage growth relative to matching firms but lesser increases than those at firms backed by traditional VCs. The wage growth of minorities, females, and the rank-and-file are less disfavorable at impact firms as well.

Turning to Panel B, we see that switchers who move to another firm transition to firms that offer higher-paying jobs. These effects are greater for those from impact-backed firms, suggesting the investments led to greater additions to the workers' human capital. However, the effects for different subsets of workers are more mixed and less conclusive.

We know from Table 9 that impact-funded firms hire more minorities and other disadvantaged groups after getting funding. Panel C checks whether these minorities also receive more favorable pay raises, conditional on being hired. The results show that conditional on being hired, new workers receive overall higher wage increases at impact portfolio companies (2.8% vs. 2.1%). This result is not so surprising since we know from the prior table that they also earn less prior to joining the firm. Second, it actually appears that minorities that are hired by VC PCs see more wage growth than those hired by impact portfolio companies relative to the baseline: for Black and Hispanic workers, the increase is 1.9% (0.030-0.011). The magnitude for VC-backed firms is similar (0.016+0.003=1.9% growth). In other words, minorities are much more likely to be hired by an impact portfolio company than a VC PC, but conditional on actually getting the job, they are no better off at the impact portfolio company than the VC PC, relative to earnings before changing jobs.

4 Conclusions

In this paper, we seek to explore the real effects of impact investors to understand their effectiveness in accomplishing social (as opposed to financial) returns. We focus on the hiring patterns and wages of firms, looking at metrics such as the count, gender, ethnicity, and background of the workers, as well as their wages. Given the central nature of employment considerations to impact investors, we ask how well employees, particularly disadvantaged ones, do at these firms in comparison to those elsewhere. We use comprehensive Census microdata that covers the entire population of U.S. firms to examine these investments. We compare hiring and employee outcomes of impact-funded firms to firms funded by venture capital and growth equity investors, as well as other matched small firms that do not have professional investors backing them.

We begin by looking at the firm level. We investigate which firms get impact funding. We find that impact-funded firms are more likely to operate in zip codes with a larger fraction of Black, poor, and unemployed residents than other venture capital portfolio companies and firms without backing from private investors. Within these areas, impact investors are also more likely than venture investors to fund firms with Black and Hispanic founders, as well as workforces that have more Black and Hispanic, female, and non-college educated workers. Thus, impact funds do, in fact, fulfill investor preferences to invest in areas that are underrepresented by traditional venture investors.

We then show that impact-backed firms have better post-funding outcomes than matched small firms in terms of increases in employment, payroll, salaries, and improved performance (firm revenue and productivity), as well as the fraction of minority and female employees. Relative to similar firms backed by non-impact venture investors, however, impact-funded firms lag on employment and payroll growth. Once controlling for this overall gap in performance, there is no discernible difference between the impact and traditional venture companies when it comes to firms based in disadvantaged areas. These patterns are likely to be driven by a combination of selection and treatment effects.

We evaluate worker-level outcomes and find that the earnings of workers at impact-backed firms grow more substantially than at the control firms but less so than at the venture-backed firms. There are substantial differences in how this wage growth is allocated across worker characteristics. Black, Hispanic, and rank-and-file workers, all of whom experience slower wage growth than their counterparts (non-Black, non-Hispanic, and top earners) at VC-backed firms, do relatively better when employed by an impact-backed firm. Finally, we observe significant differences in hiring patterns. Impact portfolio companies are more likely to hire minorities, women, less educated, and lower-earning workers than venture-backed firms.

Overall, our results are consistent with impact investors and their portfolio companies acting in pursuit to non-pecuniary social goals. These findings paint a largely positive, if limited, picture of the consequences of impact investing. The disparities with the environmentally focused ESG literature, where studies have suggested unintended and often detrimental effects of these investments, should not be too surprising, given that these employment characteristics are more easily able to be measured. The ability of private market investors to monitor and influence the behavior of companies is also typically much greater than funds active in the public markets.

Finally, our findings suggest the desirability of expanding the scope of evaluation of the consequences of impact investments. It would be natural to examine whether the differences in the performance of the companies in impact investors portfolios extends, for instance, to pollution and workplace safety. While the traditional public sector databases used by academics to assess such behavior have, in many cases, very limited coverage of the very small firms typically funded by impact investors, we hope that creative researchers will be able to make progress on these important questions.

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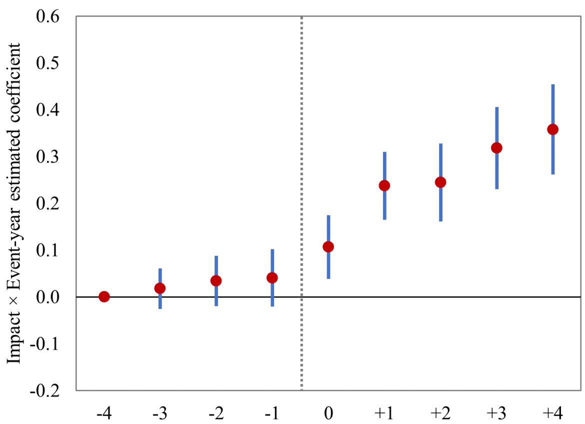
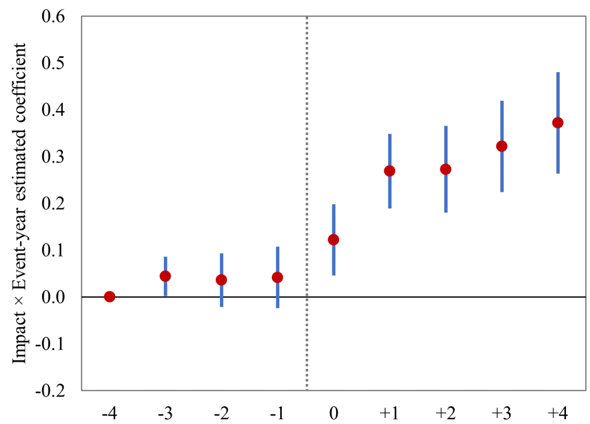
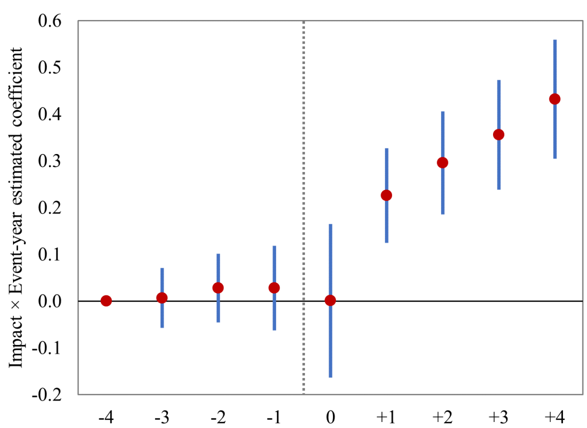
Figure 1: **Funding and firm size.** These plots show estimations from an event study on a sample of 7,300 impact portfolio companies (PCs), venture capital (VC) PCs, and matched control firms without either type of funding. The event window is nine years (-4, +4), as displayed on the x-axis. Red dots indicate estimated β_y coefficients from the following regression model: $Y_{i,t,y} = \alpha + \sum_{y=-3}^{4} \beta_y Impact_i \times EventYear_y + \mu_i + \tau_t + \delta_y + \chi_y + \lambda X_{i,t} + \epsilon_{i,t,y}$, where $Impact_i \times EventYear_y$ is a dummy equal to one if firm i has or will receive impact funding and the event year is y (with -4 as the reference year). y is shown on the horizontal axis and represents the years relative to the firm's first impact funding (for impact PCs), VC financing (for VC PCs), or corresponding matched year (for control firms). The dependent variable $Y_{i,t,y}$ is a logarithmic size indicator that varies by panel. μ_i, τ_t, and δ_y are firm, calendar year, and event year fixed effects. χ_y represents fixed effects from the interaction of event year and a VC-backed dummy. $X_{i,t}$ is a vector of controls for age, lagged employment growth, and lagged profitability. α is a constant. Blue bars plot 90% confidence intervals. Standard errors are clustered by firm. The sample period is 1997–2018.



(a) Employment

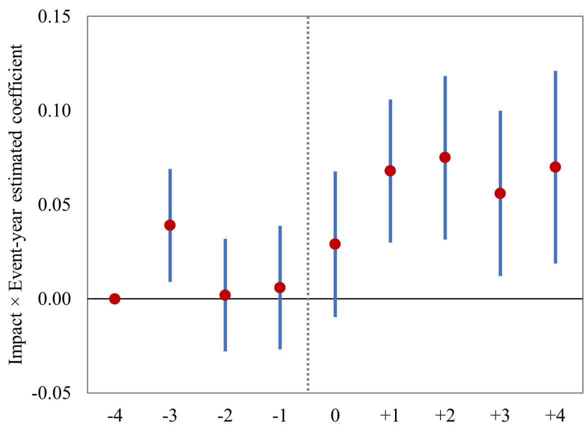


(b) Payroll

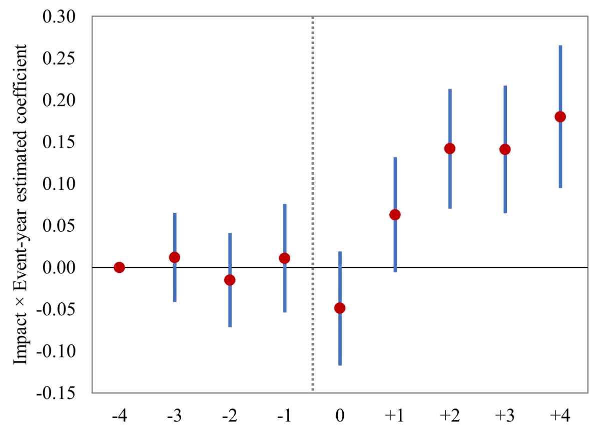


(c) Revenue

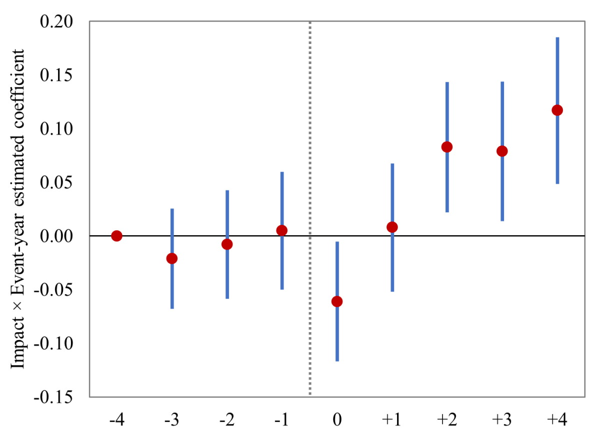
Figure 2: **Funding and firm performance.** These plots are the same as those in Figure 1, but with size-scaled performance measures as the dependent variable (varying by panel). In these regressions, the vector of control variables $X_{i,t}$ also includes $ln(1/Emp)$ in Panels A and B and $ln(1/Pay)$ in Panel C to account for any mechanical correlation caused by the scaling factor (see Chaney et al. (2020)). All other details are as in Figure 1.



(a) Average salary (=Pay/Emp)

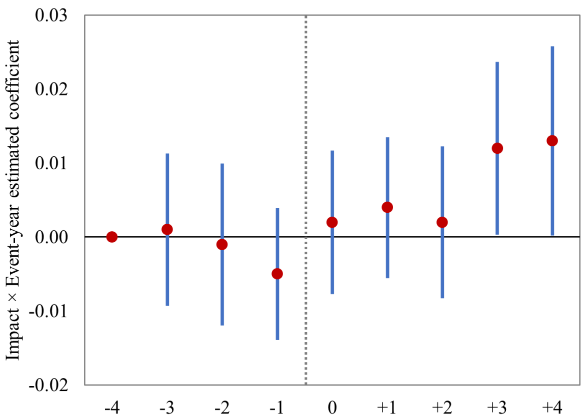


(b) Productivity (=Rev/Emp)

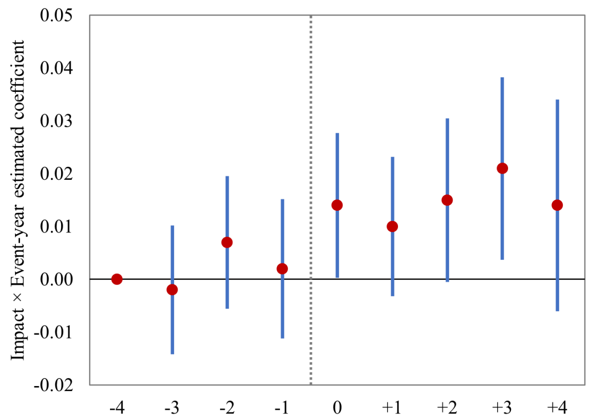


(c) Profitability (=Rev/Pay)

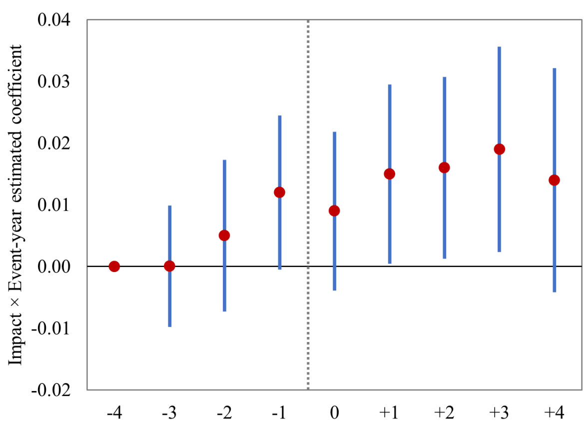
Figure 3: **Funding and firm workforce demographics.** These plots are the same as those in Figure 1, but with a different set of dependent variables. Specifically, the dependent variable $Y_{i,t,y}$ records the fraction of firm i's employees in year t that are Black or Hispanic (Panel A), are female (Panel B), or do not have any college education (Panel C). All other details are as in Figure 1.



(a) % of workers Black/Hispanic



(b) % of workers female



(c) % of workers without college education

Figure 4: **Funding and worker earnings.** This figure shows estimations from an event study on a sample of 557,500 incumbent workers at impact PCs, VC PCs, and control firms. The event period is four years of quarterly data before and after the treatment (-16, +16), as displayed on the x-axis, where treatment occurs when a worker's employer first gets funding (impact or VC PCs) or the matched counterfactual year (control firms). Red dots indicate estimated β_y coefficients from the following event study regression: $Y_{j,n,s,t,q} = \alpha + \sum_{q=-15}^{16} \beta_q Impact_j \times EventTime_q + \gamma j + \tau_t + \pi_n + \sigma_s + \delta_q + \chi_q + \lambda X_{i,t} + \epsilon_{j,n,s,t,q}$, where $Impact_j \times EventTime_q$ is a dummy equal to one if the individual works at an impact PC when that firm receives funding and the event year-quarter is q (with -16 as the reference point). The dependent variable $Y_{i,t}$ is the worker's logarithmic earnings (in 2019 USDk). γ_j, τ_t, π_n, σ_s, and δ_q are worker, industry (NAICS-3), state, calendar year-quarter, and event year-quarter fixed effects. $\chi_{j,q}$ represents fixed effects from the interaction of event year-quarters and a VC-backed-employer dummy. $X_{j,t}$ controls for worker age (expressed as a logarithm). α is a constant. Blue bars plot 90% confidence intervals. Standard errors are clustered at the worker-level. Incumbent workers are defined as individuals that are at minimum employed at a sample firm in $y = -1$ and $y = 0$. The sample period is 1992–2021.

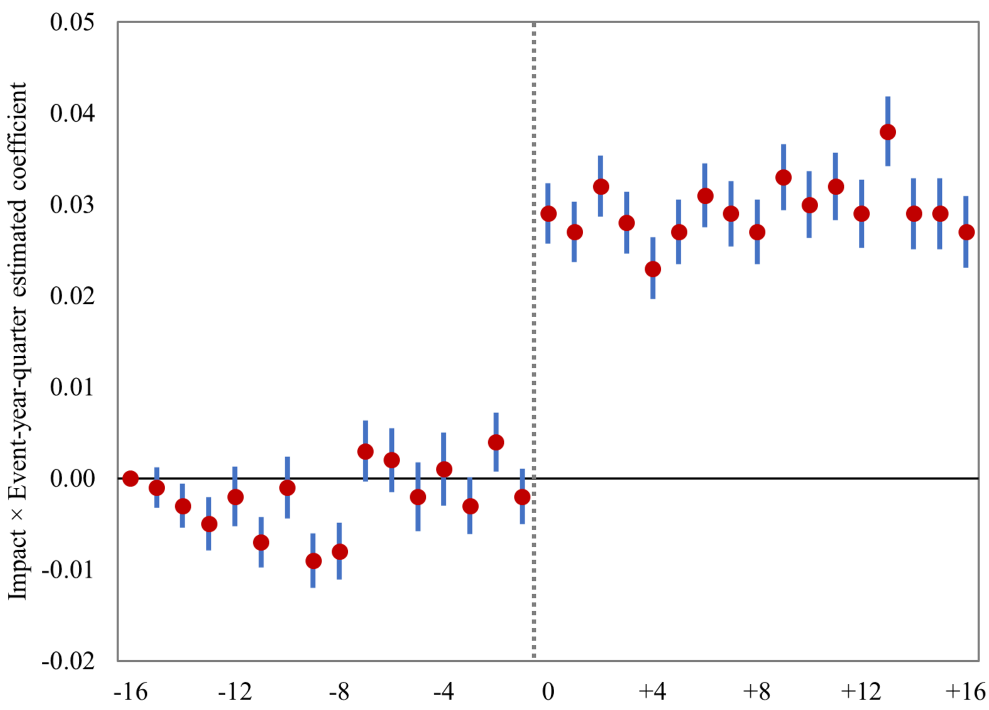


Table 1: **Key Variables.** This table provides detailed definitions of the key variables used in this article. All continuous variables except for "% workforce ..." are expressed as $ln(x)$ in regression tables.

Variable	**Definition**
Panel A: Firm-level variables	
Employment/Size	Number of individuals employed.
Payroll	Annual employee payroll (2019 USDk).
Revenue	Annual revenue (2019 USDk).
Salary	Average worker salary (=Payroll/Employment).
Productivity	Revenue per worker (=Revenue/Employment).
Profitability	Revenue over payroll (=Revenue/Payroll).
Impact	Dummy =1 if firm is or becomes impact portfolio company (PC).
VC	Dummy =1 if firm is or becomes venture capital PC.
Post	Dummy =1 if the observation is on or after a sample firm has received funding (or the matched counterfactual year for control firms).
Disadvantaged area	Dummy =1 if the firm operates in an economically disadvantaged zip code. The classification of disadvantaged zip codes varies by table and is specified in the captions.
Growth	Average annual arc growth in employment over the two preceding years, calculated as $(Emp_{it} - Emp_{it-1})/(0.5 * (Emp_{it} + Emp_{it-1}))$, bounding growth between -200% and +200%. See e.g., Davis et al. (1996).
Firm age	Age in years since the firm's first recorded employment.
Top team Black/Hispanic	Dummy =1 if any of 3 top-paid employees are Black or Hispanic.
Top team female	Dummy =1 if any of 3 top-paid employees are female.
Top team without college	Dummy =1 if any of 3 top-paid employees have no college education.
Founder Black/Hispanic	Dummy =1 if top team has Black/Hispanic members in firm's first year.
Founder female	Dummy =1 if top team has female members in firm's first year.
Founder without college	Dummy =1 if top team has members without college in firm's first year.
% workforce Black/Hispanic	Fraction of all employees that are Black or Hispanic.
% workforce female	Fraction of all employees that are female.
% workforce without college	Fraction of all employees that have no college education.
Panel B: Worker-level variables	
Earnings	Quarterly wages (2019 USDk). For workers with more than one job, only the highest-paying one is considered. Quarterly wages under $1,000 are recorded as 0 and dropped (unemployed).
Impact	Dummy =1 if employer is or becomes impact PC.
VC	Dummy =1 if employer is or becomes venture capital PC.
Post	For incumbent workers, this dummy =1 if the observation is on or after a sample firm has received funding (or the matched counterfactual year for control firms). For newly hired workers, the variable is instead defined relative to the worker's starting employment quarter.
Treated	Dummy =1 if worker belongs to minority or disadvantaged group as defined by column (Black/Hispanic, female, or without college).
Black/Hispanic	Dummy =1 if the worker is Black or Hispanic.
Female	Dummy =1 if the worker is female.
No college	Dummy =1 if the worker has no college education.
Worked in disadv area	Dummy =1 if worker's previous job in disadvantaged area (any type).
Worked in same industry	Dummy =1 if worker's previous job was in same industry (NAICS-3).
Average prior earnings	Average quarterly earnings in the last four years, including 0 for quarters of unemployment.
Worker age	Worker's age since birth.
Firm-specific wage premium	As proposed by Abowd, Kramarz, and Margolis (1999). See caption.

Table 2: **Summary statistics.** This table presents summary statistics for three different samples analyzed in this paper. Continuous variables summarized here are not log-adjusted as in subsequent regression tables, but are still winsorized at 5% tails by year. Panel A describes the cross-section of firms one year before they receive impact funding (or the corresponding matched year for VC portfolio companies and control firms without funding). Panel B details incumbent worker characteristics one quarter before their employer receives impact funding (or corresponding match). Panel C describes newly hired workers that join one of the sample firms in the year after the firm receives impact funding (or corresponding match) by tabulating the cross-section of characteristics one quarter before the workers start their new job. Statistics in columns (1)–(3) are pseudo-medians, except for dummy variable indicated by †, in which case they are means (since medians are not informative). To comply with Census disclosure requirements, pseudo-medians are calculated as the mean of observations between the 45th and 55th percentiles. Percents above 10% are rounded to the nearest integer. Column (4) tests for differences in medians (means if †) between impact and VC PCs using equality-of-medians χ^2 tests (equality-of-means t-tests if †), and Column (5) between impact PCs and control firms.

				Difference tests	
Firm type subsample:	Impact	VC	Control	I-V	I-C
	(1)	(2)	(3)	(4)	(5)
Panel A: Firm-level variables (in year before firm receives funding)					
	[N=700]	[N=2,000]	[N=4,600]		
Employees	20	17	18	*	.
Payroll (2019 USDm)	1.6	1.7	1.3	.	**
Revenue (2019 USDm)	3.0	2.3	3.3	**	.
Salary (2019 USDk per worker)	76	95	68	***	***
Productivity (2019 USDk revenue per worker)	149	153	175	.	***
Profitability (revenue over payroll)	2.2	1.8	2.6	***	***
Firm age	6.1	4.6	5.6	***	.
Establishment count	1	1	1	***	.
Year of first impact/matched funding	2013	2013	2013	.	*
Round of first impact/matched funding	1	1		.	
% Black or Hispanic workers	7.6%	7.5%	9.9%	***	***
% female workers	32%	32%	36%	*	***
% no college workers	31%	23%	34%	**	***
Firm operates in disadvantaged area (any type)†	31%	18%	23%	***	***
Dies (t+1 to t+4), annual rate†	5.4%	6.0%	4.3%	.	***
Acquired (t+1 to t+4), annual rate†	0.9%	0.8%	1.2%	.	*
Panel B: Incumbent worker-level variables (in quarter before firm receives funding)					
	[N=86,500]	[N=102,000]	[N=369,000]		
Average ann earnings (2019 USDk), prev 4 years	46.2	47.1	45.3	***	***
Age	39	37	41	***	***
Black or Hispanic†	22%	20%	22%	***	.
Female†	39%	36%	40%	***	***
No college education†	35%	33%	36%	***	***
Panel C: Newly hired worker-level variables (in quarter before taking new job)					
	[N=34,500]	[N=59,500]	[N=103,000]		
Average ann earnings (2019 USDk), prev 4 years	27.7	34.3	20.9	***	***
Age	32	32	32	.	.
Black or Hispanic†	27%	19%	30%	***	***
Female†	41%	38%	43%	***	***
No college education†	38%	31%	42%	***	***

Table 3: **Likelihood of funding if operating in disadvantaged areas.** This table presents estimations from firm-level multinomial logit regressions where the dependent variable is a categorical variable if the firm receives impact funding (odd-numbered columns), VC funding (even-numbered columns), or neither (the base category or reference group). Marginal effects are shown in brackets and robust z-statistics in parentheses. The main control variable of interest is whether the firm operates in a disadvantaged area, the definition of which varies by column, as indicated in the top row. Specifically, a disadvantaged area falls in the top 10% of zip codes by: (1)-(2) fraction of population that is Black, (3)-(4) fraction of population without any college education, (5)-(6) poverty rate, or (7)-(8) unemployment rate. All characteristics are recorded one year prior to the firms' treatment year. Industry dummies are at the NAICS-3 level. Continuous variables are winsorized at the 5% tails by year. Standard errors are clustered at the industry level. The sample is 7,300 firms, 1997–2018. See Table 1 for further variable definitions. The last row of the table shows the significance values of cross-equation chi-squared tests for whether the estimated disadvantaged area coefficients are equal for impact PCs and VC PCs. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Observation counts are rounded according to Census disclosure requirements.

Disadvantaged area type:	Top 10% Black		Top 10% without college		Top 10% poverty		Top 10% unemployment	
Funding type outcome:	Impact (1)	VC (2)	Impact (3)	VC (4)	Impact (5)	VC (6)	Impact (7)	VC (8)
Disadvantaged area	0.434***	-0.118	-0.025	-0.584***	0.464***	-0.088	0.243*	-0.13
(type varies by column)	[0.038**]	[-0.032]	[0.013]	[-0.102**]	[0.040***]	[-0.027]	[0.023*]	[-0.029*]
	(3.19)	(1.15)	(0.15)	(3.13)	(5.55)	(1.06)	(1.71)	(1.57)
Size	-0.024	-0.054	-0.006	-0.049	-0.03	-0.055	-0.017	-0.054
	[-0.001]	[-0.009]	[0.001]	[-0.008]	[-0.001]	[-0.009]	[-0.000]	[-0.009]
	(0.62)	(1.53)	(0.18)	(1.39)	(0.82)	(1.50)	(0.46)	(1.48)
Profitability	-0.396***	-0.494***	-0.397***	-0.490***	-0.399***	-0.494***	-0.401***	-0.493***
	[-0.020***]	[-0.077***]	[-0.020***]	[-0.076***]	[-0.020***]	[-0.077***]	[-0.020***]	[-0.077***]
	(6.76)	(10.58)	(6.82)	(10.36)	(6.85)	(10.51)	(7.05)	(10.54)
Growth	1.113***	0.976***	1.089***	0.967***	1.124***	0.976***	1.104***	0.975***
	[0.066***]	[0.144***]	[0.064***]	[0.143***]	[0.067***]	[0.144***]	[0.065***]	[0.144***]
	(3.78)	(4.52)	(3.89)	(4.46)	(3.88)	(4.48)	(3.81)	(4.49)
Firm age	0.335***	0.192***	0.343***	0.198***	0.343***	0.191***	0.340***	0.194***
	[0.022***]	[0.025**]	[0.023***]	[0.026**]	[0.023***]	[0.025**]	[0.023***]	[0.026**]
	(8.85)	(3.71)	(9.33)	(3.80)	(9.11)	(3.70)	(8.94)	(3.75)
Industry dummies	Y		Y		Y		Y	
State dummies	Y		Y		Y		Y	
Year dummies	Y		Y		Y		Y	
Observations	7,300		7,300		7,300		7,300	
Pseudo R^2	0.100		0.100		0.100		0.099	
Disadv. area, Impact=VC	***		***		***		***	

Table 4: **Likelihood of funding based on firm demographics.** This table presents estimations from multinomial logit regressions similar to those in Table 3, but with two changes in the control variables. First, the disadvantaged area dummy (included but not shown) now takes a value of one for firms operating in any of the four previously defined types of disadvantaged zip codes. The same controls for size, profitability, growth, firm age, industry, state, and year are also included but not shown. Second, the specification adds three different sets of independent dummy variables identifying employee characteristics, as indicated by panel. In Panel A, dummies indicate if any of the firm's founding (first recorded year) top team are Black/Hispanic, female, or without college education, where the top team is the firm's three highest-paid employees. Panel B adds similar indicators for the firm's top team membership in the year before treatment (or matched counterfactual). Panel C adds continuous control variables for the fraction of the firm's employees that are Black/Hispanic, female, or without college education. All other details are the same as in Table 3.

Funding type outcome:	Impact (1)	VC (2)	Impact (3)	VC (4)	Impact (5)	VC (6)	Impact (7)	VC (8)
Panel A: Founders								
Founder Black/Hispanic	0.075	-0.294***					0.112	-0.225***
	[0.014]	[-0.054***]					[0.015]	[-0.042**]
	(0.78)	(3.57)					(1.14)	(2.73)
Founder female			-0.099	-0.233***			-0.097	-0.202***
			[-0.002]	[-0.039***]			[-0.003]	[-0.033**]
			(1.23)	(3.93)			(1.23)	(3.18)
Founder without college					-0.109	-0.257***	-0.116	-0.211***
					[-0.002]	[-0.043***]	[-0.004]	[-0.034**]
					(1.00)	(3.67)	(1.05)	(2.85)
Controls, dummies	Y		Y		Y		Y	
Observations	7,300		7,300		7,300		7,300	
Pseudo R^2	0.101		0.101		0.101		0.103	
Black/Hispanic, Impact=VC	***						***	
Female, Impact=VC			.				.	
Without college, Impact=VC					.		.	
Panel B: Top team members								
Top team Black/Hispanic	-0.241*	-0.371***					-0.191	-0.291***
	[-0.010]	[-0.059***]					[-0.008]	[-0.046***]
	(1.65)	(4.61)					(1.31)	(3.68)
Top team female			-0.176	-0.298***			-0.153	-0.264***
			[-0.007]	[-0.048***]			[-0.006]	[-0.042***]
			(1.61)	(4.30)			(1.43)	(3.92)
Top team without college					-0.221***	-0.329***	-0.193***	-0.284***
					[-0.010]	[-0.052***]	[-0.008]	[-0.045***]
					(3.37)	(5.74)	(2.85)	(5.09)
Controls, dummies	Y		Y		Y		Y	
Observations	7,300		7,300		7,300		7,300	
Pseudo R^2	0.101		0.102		0.102		0.105	
Black/Hispanic, Impact=VC	.						.	
Female, Impact=VC			.				.	
Without college, Impact=VC					.		.	
Panel C: Workforce composition								
% workforce Black/Hispanic	-0.623**	-1.303***					-0.421	-0.794***
	[-0.018]	[-0.212***]					[-0.014]	[-0.127***]
	(2.42)	(6.72)					(1.52)	(4.19)
% workforce female			-0.352**	-0.663***			-0.278	-0.482***
			[-0.012]	[-0.107***]			[-0.010]	[-0.077**]
			(2.08)	(4.02)			(1.58)	(2.82)
% workforce without college					-0.629***	-1.444***	-0.467*	-1.137***
					[-0.014]	[-0.236***]	[-0.009]	[-0.186***]
					(2.58)	(7.02)	(1.87)	(5.63)
Controls, dummies	Y		Y		Y		Y	
Observations	7,300		7,300		7,300		7,300	
Pseudo R^2	0.104		0.102		0.106		0.109	
Black/Hispanic, Impact=VC	**						.	
Female, Impact=VC			**				.	
Without college, Impact=VC					***		***	

Table 5: **Funding, firm growth, and employment.** This table presents estimations from diff-in-diff regressions on an annual panel of impact, VC, and control firms. The dependent variable varies by column: (1)-(3) measures size, (4)-(6) performance, and (7)-(9) firm demographics. Size and performance variables are logarithmic. *Post* is a dummy equal to one if the observation is on or after the firm has received funding (or the matched counterfactual). Columns (4)-(5) and column (6) include $ln(1/Emp)$ and $ln(1/Pay)$, respectively, to account for any mechanical correlation caused by the scaling factor (see Chaney et al. (2020)). Refer to Table 1 for detailed variable definitions of remaining control variables. The last row shows the significance of Wald tests that the coefficient estimates of Impact×Post and VC×Post are statistically indistinguishable. 7,300 firms are sampled, 1997–2018. A constant is included but not displayed. Continuous variables are winsorized at the 5% tails by year. T-statistics are in parentheses and standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Observation counts are rounded according to Census disclosure requirements.

Dependent variable:	Employment	Payroll	Revenue	Salary (=Pay/Emp)	Productivity (=Rev/Emp)	Profitability (=Rev/Pay)	% Black or Hispanic	% female	% without college
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Impact×Post	0.194***	0.208***	0.183***	0.045***	0.056**	0.021	0.007*	0.012**	0.007
	(7.05)	(6.89)	(4.93)	(3.35)	(2.33)	(1.05)	(1.84)	(2.32)	(1.48)
VC×Post	0.256***	0.278***	0.221***	0.054***	0.039**	0.000	-0.001	0.002	0.004
	(14.22)	(13.68)	(9.13)	(5.67)	(2.44)	(0.02)	(0.31)	(0.57)	(1.35)
Post	-0.058***	-0.087***	-0.098***	-0.036***	-0.050***	-0.022***	-0.002	-0.002	-0.004*
	(7.22)	(9.63)	(8.71)	(6.79)	(5.91)	(3.20)	(1.35)	(0.92)	(1.86)
Firm age	0.709***	0.565***	0.716***	-0.060***	0.178***	0.221***	0.011***	0.031***	0.030***
	(26.69)	(19.46)	(21.14)	(4.02)	(7.82)	(11.69)	(2.66)	(5.23)	(5.29)
Lag growth	0.568***	0.511***	0.473***	0.009	0.040***	0.052***	0.008***	0.008**	0.020***
	(33.67)	(27.33)	(21.08)	(1.00)	(2.77)	(4.15)	(2.90)	(2.00)	(5.58)
Lag profitability	-0.033***	-0.029**	0.263***	0.006	0.273***	0.262***	0.000	0.006**	0.002
	(3.14)	(2.56)	(16.04)	(1.11)	(22.19)	(23.64)	(0.00)	(2.50)	(1.06)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Scaling factor control				Y	Y	Y			
Observations	40,000	40,000	40,000	40,000	40,000	40,000	32,000	32,000	32,000
Adjusted R^2	0.928	0.925	0.897	0.819	0.779	0.819	0.901	0.865	0.825
Impact×Post=VC×Post	**	**	.	.	.	.	*	*	.

Table 6: **Funding, firm growth, and employment in disadvantaged areas.** This table presents estimations from regressions similar to those in Table 5, but with the addition of control and interaction variables taking a value of one if a firm operates in an economically disadvantaged area. We define a disadvantaged area as one that falls in the top 10% of zip codes by: fraction of population that is Black, fraction of population without any college education, poverty rate, or unemployment rate. The last row shows the significance of Wald tests that the coefficient estimates of Impact×Disadvantaged×Post and VC×Disadvantaged×Post are statistically indistinguishable. Other controls which are included but not shown include firm age, lag growth, and lag profitability. All other details are the same as in Table 5.

Dependent variable:	Employment (1)	Payroll (2)	Revenue (3)	Salary (=Pay/Emp) (4)	Productivity (=Rev/Emp) (5)	Profitability (=Rev/Pay) (6)	% Black or Hispanic (7)	% female (8)	% without college (9)
Impact×Disadvantaged×Post	-0.013	-0.003	-0.08	-0.018	-0.095**	-0.086**	0.001	0.007	-0.002
	(0.25)	(0.05)	(1.18)	(0.71)	(2.02)	(2.17)	(0.14)	(0.69)	(0.15)
VC×Disadvantaged×Post	-0.033	-0.039	-0.100*	-0.02	-0.098**	-0.060*	0.005	-0.007	-0.005
	(0.78)	(0.81)	(1.68)	(0.84)	(2.31)	(1.68)	(0.77)	(0.80)	(0.62)
Impact×Disadvantaged	0.119*	0.09	0.116	0.021	0.056	0.016	0.001	-0.007	0.006
	(1.77)	(1.33)	(1.24)	(0.69)	(0.91)	(0.32)	(0.14)	(0.54)	(0.49)
Impact×Post	0.200***	0.209***	0.209***	0.050***	0.086***	0.048*	0.006	0.01	0.008
	(5.53)	(5.22)	(4.31)	(2.93)	(2.80)	(1.95)	(1.46)	(1.39)	(1.24)
VC×Disadvantaged	0.094*	0.094	0.117	0.005	0.045	0.015	-0.002	-0.012	0.017
	(1.65)	(1.44)	(1.53)	(0.16)	(0.80)	(0.36)	(0.19)	(1.07)	(1.56)
VC×Post	0.264***	0.288***	0.242***	0.059***	0.059***	0.012	-0.002	0.003	0.006
	(13.26)	(12.84)	(9.14)	(5.58)	(3.31)	(0.79)	(0.55)	(0.72)	(1.54)
Disadvantaged×Post	0.001	0.017	0.028	0.016*	0.029**	0.016	0.002	-0.001	0.001
	(0.05)	(0.95)	(1.42)	(1.82)	(2.15)	(1.48)	(0.94)	(0.28)	(0.17)
Disadvantaged	-0.03	-0.04	-0.019	-0.024**	-0.005	0.014	0.000	0.000	-0.002
	(1.33)	(1.60)	(0.63)	(2.08)	(0.28)	(0.91)	(0.07)	(0.02)	(0.50)
Post	-0.059***	-0.092***	-0.106***	-0.041***	-0.058***	-0.026***	-0.002	-0.002	-0.004*
	(6.43)	(8.88)	(8.37)	(6.78)	(6.21)	(3.47)	(1.60)	(0.67)	(1.72)
Other controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Scaling factor control				Y	Y	Y			
Observations	40,000	40,000	40,000	40,000	40,000	40,000	32,000	32,000	32,000
Adjusted R^2	0.928	0.925	0.897	0.819	0.779	0.819	0.901	0.865	0.825
Impact×Dis×Post=VC×Dis×Post	.	.	.	.	.	.	.	.	.

Table 7: **Funding and firm survival.** This table presents estimations from linear probability models predicting a firm's likelihood of "death" (ceasing operation) in each of the four years after that firm receives funding (if impact- or VC-backed) or the corresponding counterfactual (if a matched control firm). Acquired firms are excluded from the analysis since changes in ownership are not deaths. Control variables are defined in Table 1 and recorded one year prior to treatment (year t-1). The last row shows the significance of Wald tests that the coefficient estimates of Impact and VC are statistically indistinguishable. A constant is included but not shown.

Dependent variable:	Death t+1	Death t+2	Death t+3	Death t+4
	(1)	(2)	(3)	(4)
Impact	0.000	-0.003	0.003	0.012
	(0.04)	(0.34)	(0.31)	(1.27)
VC	-0.011*	0.010*	0.01	0.010*
	(1.93)	(1.97)	(1.46)	(1.67)
Size	-0.005***	0.000	0.005***	-0.004*
	(2.67)	(0.12)	(2.78)	(1.82)
Profitability	-0.012**	-0.022***	-0.021***	-0.009**
	(2.52)	(6.54)	(4.92)	(2.16)
Growth	-0.030**	-0.007	-0.013	-0.005
	(2.16)	(0.73)	(1.31)	(0.54)
Firm Age	-0.014*	-0.016***	-0.020***	-0.007**
	(1.99)	(3.44)	(4.98)	(2.11)
Industry FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	5,300	5,300	5,300	5,300
Adjusted R^2	0.016	0.009	0.012	0.004
Impact=VC	.	.	.	.

Table 8: **Funding and worker earnings.** This table presents estimations from diff-in-diff regressions on a quarterly panel of workers in event time. The sample consists of 557,500 incumbent workers at firms that receive impact funding, VC funding, or neither, 1992-2021.The event window is +/-4 years around the treatment quarter, which is when the firm first receives impact funding (or matched equivalent). Incumbent workers are defined as individuals that are at minimum employed at a sample firm in $y = -1$ and $y = 0$. The dependent variable (quarterly worker earnings) is scaled as $ln(Earnings)$, where $Earnings$ is expressed in thousands of USD (inflation-adjusted to 2019 levels). A dummy variable indicating if the worker belongs to a minority or economically disadvantaged group is included and varies by column, as indicated in the first row. A control variable for worker age and constant are included but not displayed. Continuous variables are winsorized at the 5% tails by year. T-statistics are in parentheses and standard errors are clustered by worker. The last two rows show the significance of Wald tests that the coefficient estimates of Impact×Post and VC×Post as well as Impact×WorkerType×Post and VC×WorkerType×Post are statistically indistinguishable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Observation counts are rounded according to Census disclosure requirements.

Worker type:	None	Black or Hispanic	Female	No college	Rank & file
Dependent variable:	Earnings (1)	Earnings (2)	Earnings (3)	Earnings (4)	Earnings (5)
Impact×Post	0.031***	0.033***	0.031***	0.032***	0.030***
	(28.79)	(26.72)	(22.40)	(23.45)	(5.54)
VC×Post	0.034***	0.038***	0.033***	0.034***	0.067***
	(31.77)	(31.11)	(24.87)	(25.60)	(16.78)
Post	0.008***	0.012***	0.009***	0.011***	-0.014***
	(17.30)	(23.15)	(15.78)	(18.96)	(6.49)
Impact×WorkerType×Post		-0.010***	0.001	-0.002	0.001
		(4.00)	(0.36)	(0.78)	(0.15)
VC×WorkerType×Post		-0.022***	0.001	0.000	-0.035***
		(8.81)	(0.60)	(0.09)	(8.58)
WorkerType×Post		-0.018***	-0.003***	-0.008***	0.023***
		(17.60)	(3.29)	(8.58)	(10.57)
Age control	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y	Y
Year-quarter FE	Y	Y	Y	Y	Y
Observations	15,240,000	15,240,000	15,240,000	15,240,000	15,240,000
Adjusted R^2	0.811	0.811	0.811	0.811	0.811
Impact×Post=VC×Post	***	***	.	.	***
Impact×WorkerType×Post=VC×WorkerType×Post		***	.	.	***

Table 9: **Probability for new hires to be recruited by firms receiving impact or VC funding.** This table presents estimations from worker-level multinomial logit regressions where the dependent variable is a categorical variable if the worker is hired by a firm that has received impact funding (odd-numbered columns), VC funding (even-numbered columns), or neither (the base category or reference group). The sample consists of workers that are newly hired by sample firms in the year after the firm receives funding (or corresponding counterfactual). The main control variables of interest are dummies for worker characteristics: Black/Hispanic, female, and without any college education. Average prior earnings are measured in the four years before the event and include values of zero for unemployed quarters. Included but not displayed is an indicator variable if the worker's previous job was in a disadvantaged area. See Table 1 for further variable definitions. A constant is included but not displayed. Industry dummies are at the NAICS-3 level. Continuous variables are winsorized at the 5% tails by year. T-statistics are in parentheses and standard errors are clustered at the firm level. The sample period is 1992-2021. The last three rows of the table show the significance values of cross-equation chi-squared tests for whether the estimated worker demographic coefficients are equal for impact PCs and VC PCs. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Observation counts are rounded according to Census disclosure requirements.

Hiring firm type outcome:	Impact (1)	VC (2)	Impact (3)	VC (4)	Impact (5)	VC (6)	Impact (7)	VC (8)
Black/Hispanic	-0.038	-0.369***					-0.031	-0.343***
	[0.013]	[-0.066***]					[0.013]	[-0.061***]
	(0.57)	(6.83)					(0.46)	(6.42)
Female			0.02	-0.096**			0.019	-0.095**
			[0.008]	[-0.019**]			[0.007]	[-0.018**]
			(0.37)	(2.50)			(0.35)	(2.49)
Without college					-0.054*	-0.187***	-0.049*	-0.148***
					[0.002]	[-0.032***]	[0.001]	[-0.025***]
					(1.94)	(7.93)	(1.93)	(6.74)
Average prior earnings	0.132***	0.254***	0.135***	0.271***	0.130***	0.260***	0.130***	0.238***
	[0.005]	[0.040***]	[0.005]	[0.043***]	[0.005]	[0.041***]	[0.006]	[0.037***]
	(4.28)	(11.89)	(4.22)	(12.09)	(4.16)	(12.08)	(4.23)	(11.42)
Prev worked in same industry	-0.125	-0.138**	-0.127	-0.153**	-0.126	-0.151**	-0.124	-0.138**
	[-0.010]	[-0.019]	[-0.010]	[-0.022]	[-0.010]	[-0.021]	[-0.010]	[-0.019]
	(1.60)	(2.16)	(1.62)	(2.41)	(1.61)	(2.37)	(1.59)	(2.15)
Age	-0.211**	-0.551***	-0.211**	-0.551***	-0.213**	-0.561***	-0.213**	-0.554***
	[-0.001]	[-0.090***]	[-0.001]	[-0.091***]	[-0.001]	[-0.092***]	[-0.001]	[-0.091***]
	(2.23)	(7.93)	(2.23)	(7.91)	(2.26)	(8.04)	(2.26)	(7.95)
Disadvantaged area control	Y		Y		Y		Y	
Industry, year, state dummies	Y		Y		Y		Y	
Observations	197,000		197,000		197,000		197,000	
Pseudo R^2	0.099		0.097		0.098		0.099	
Black/Hispanic, Impact=VC	***						***	
Female, Impact=VC			**				**	
Without college, Impact=VC					***		***	
Prior earnings, Impact=VC	***		***		***		***	

Table 10: **Funding and other worker outcomes.** This table presents estimations from diff-in-diff regressions similar to those in Table 8 and using the same control variables (including those not shown here). Panel A limits the sample to "continuers": workers that do not switch to a new employer in the four years after the event. Panel B looks at the complementary "switcher" worker sample with a new dependent variable: firm-specific wage premiums as proposed by Abowd, Kramarz, and Margolis (1999). These regressions assess whether workers transition to higher-paying firms after their previous employer receives impact funding, VC funding, or neither. To estimate firm-specific wage premiums, we take the following steps using a similar methodology to Lachowska, Mas, and Woodbury (2020) and Arnold, Milligan, Moon, and Tavakoli (2023): We regress $ln(earnings)$ on worker, year-quarter, state, and firm fixed effects, then save the estimated firm fixed effects. To reduce the influence of outliers, we winsorize these estimates at 5% tails by year-quarter. This estimated firm fixed effect is the firm-specific wage premium, which is then used as the dependent variable. Finally, Panel C presents regressions similar to those of Table 8 but on a sample of 197,000 newly hired workers. The treatment quarter (event quarter 0) corresponds to the point in time that the worker starts their new job rather than when the firm receives funding. Other details are as in Table 8. For visual purposes, standard errors are not shown below, but full versions of the tables below are provided in the internet appendix.

Treated worker type:	None	Black or Hispanic	Female	No college	Rank & file
Dependent variable:	See panel	See panel	See panel	See panel	See panel
	(1)	(2)	(3)	(4)	(5)
Panel A: Continuer earnings					
Impact×Post	0.027***	0.029***	0.026***	0.027***	0.045***
VC×Post	0.043***	0.047***	0.045***	0.042***	0.080***
Impact×WorkerType×Post		-0.010***	0.001	-0.002	-0.019***
VC×WorkerType×Post		-0.025***	-0.005*	0.003	-0.041***
Other controls, FEs	Y	Y	Y	Y	Y
Observations	8,238,000	8,238,000	8,238,000	8,238,000	8,238,000
Adjusted R^2	0.841	0.841	0.841	0.841	0.841
Impact×Post = VC×Post	***	***	***	***	***
Impact×WorkerType×Post = VC×WorkerType×Post		***	*	.	***
Panel B: Switcher firm-specific wage premiums					
Impact×Post	0.016***	0.017***	0.015***	0.016***	0.007
VC×Post	0.008***	0.010***	0.006***	0.008***	0.014***
Impact×WorkerType×Post		-0.006***	0.002	-0.001	0.009**
VC×WorkerType×Post		-0.008***	0.006***	0.000	-0.006*
Other controls, FEs	Y	Y	Y	Y	Y
Observations	6,917,000	6,917,000	6,917,000	6,917,000	6,917,000
Adjusted R^2	0.665	0.665	0.665	0.665	0.665
Impact×Post = VC×Post	***	***	***	***	.
Impact×WorkerType×Post = VC×WorkerType×Post		.	**	.	***
Panel C: New hire earnings					
Impact×Post	0.028***	0.030***	0.028***	0.026***	
VC×Post	0.021***	0.016***	0.017***	0.012***	
Impact×WorkerType×Post		-0.011*	0.000	0.007	
VC×WorkerType×Post		0.003	0.009*	0.029***	
Other controls, FEs	Y	Y	Y	Y	
Observations	4,766,000	4,766,000	4,766,000	4,766,000	
Adjusted R^2	0.735	0.735	0.735	0.735	
Impact×Post = VC×Post	**	***	***	***	
Impact×WorkerType×Post = VC×WorkerType×Post		*	.	***	

A Appendix

A.1 Examples of Employment-Related Goals among Impact Funds

To help ascertain the prevalence of employment-related objectives among US impact funds, we examined snippets of the mission statements from websites of 200 US-based impact investors that we collected in 2021 when undertaking our original screening for impact investors. 43 impact investors mentioned employment as among their goals. The excerpts below provide some examples of such discussions:

• "The [fund] is a developmental venture capital program designed to promote economic development and the creation of wealth and job opportunities in low-income geographic areas and among individuals living in such areas."

• "We provide a winning investment model to support small, women and minority led business, and maximize frequently overlooked human and investment potential. [We strive] to create wealth, high quality jobs and to open the doors of opportunity in LMI communities."

• "With growth, these businesses become engines of impact that can raise incomes and create jobs, empower women and young people, sustain peace, and preserve vulnerable ecosystems."

• "[We] promote the development of a regional entrepreneurial ecosystem in Northern Colorado. Accelerate job creation and entrepreneurship, and create long-term, regional economic impact by offering clients access to a broad network of specialized resources..."

• "Our primary impact goal of providing quality jobs and economic development for low-income communities"

• "[We] provide a return to investors and create good jobs in local communities."

• "The impact performance metrics – quality jobs, taxes paid, and revenue generated – represent key impact drivers..."

• "We help them maximize their business potential and develop win-win relationships with their employees through high-quality job creation."

A.2 Additional Sample Construction Description

A.2.1 Verifying and Extending Portfolio Company Links to the LBD

We extensively check all PC-LBD links by hand to ensure accuracy, comparing rough information on firm size (approximate number of employees) and industry recorded in the Impact Investment Database with that of the LBD. Moreover, in instances where the primary LBD firm identifier (lbdfid) changes even as the firm remains a standalone entity and its operations are unchanged (this may occur due to ownership or legal status changes, for example; see Davis et al. (2014) for discussion), we assign a consistent firm identifier. This ultimately affects very few sample firms. To do this, we manually check instances of firm identifier change and use rules of thumb to gauge if this change reflects an acquisition, in which case it should be kept as is, or not, in which case we extend the firm identifier for continuity. For example, suppose the lbdfid of a firm with only one establishment changes such that the old firm identifier ceases and a new firm identifier appears, but the establishment itself continues. In that case, the lbdfid change is unlikely to be an acquisition.

A.2.2 Detailed Illustration of the Matching Procedure

Below is a step-by-step breakdown of the entire matching procedure described in Section 2.2:

1. Set initial filters
   (a) Impact PCs: Require 1+ employees in year before and of first impact funding
   (b) Matchable firms: Require same NAICS-2, 1+ employees in year of matching, +/-50% of impact PC employment and age, and same single-unit or multi-unit status
      i. VC PCs: Require year of funding to be +/-5 relative impact PC first impact funding, funding round to be +/-50% relative to impact PC first impact round, require funding round to be 1 or 2+ if impact PC round is 1 or 2+, respectively.
      ii. b. Control firms: Require matching year to be the year before impact PC's first impact funding.
2. Rank matches using caliper nearest-neighbor matching (NNM).
   (a) Include revenue as matching criterion in level and growth variables.
      i. Same NAICS-5
         A. Define caliper of +/-5% level variables and +/-10% growth variables, then rank matches within caliper according to lowest average of deciles measuring the difference between impact PC and matched firm for each caliper level and growth variable.
         B. Define caliper of +/-10% level and +/-20% growth, then rank matches as above.
         C. Define caliper of +/-15% level and +/-30% growth, then rank matches as above.
         ...
         J. Define caliper of +/-50% level and +/-100% growth, then rank matches as above.
      ii. Same NAICS-4
         • Repeat the same steps as for NAICS-5.
      iii. Same NAICS-3
         • Repeat the same steps as for NAICS-5.
      iv. Same NAICS-2
         • Repeat the same steps as for NAICS-5.
   (b) If missing, exclude revenue as matching criterion in level and growth variables.
      • Repeat the same steps as in 2(a) above.
3. Rank remaining matches using Mahalanobis distance NNM.

(a) Rank according to distance in employment, payroll, and revenue for level variables only.

    i. Same NAICS-5

    ii. Same NAICS-4

    iii. Same NAICS-3

    iv. Same NAICS-2

(b) Rank according to distance in employment and payroll for level variables only (when revenue is missing).

    - Repeat the same steps as in 3(a) above.