JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 60, No. 6, Sep. 2025, pp. 2584–2614

doi:10.1017/S0022109024000772

IPOs, Human Capital, and Labor Reallocation

Tania Babina
University of Maryland Smith School of Business

Paige Ouimet
University of North Carolina at Chapel Hill Kenan-Flagler School of Business

Rebecca Zarutskie
Federal Reserve Bank of Dallas
rebecca.zarutskie@dal.frb.org (corresponding author)

Abstract

How does access to public equity markets affect the human capital of IPO filing firms? While IPO filing firms have high average wages and limited industrial diversification, a successful IPO increases departures of high-wage employees to startups and triggers industrial diversification through employment growth in non-core industries. Surprisingly, IPOs do not significantly affect the earnings growth of pre-IPO workers. Instead, post-IPO hires receive larger earnings increases upon joining. Overall, going public has a significant effect on a firm's workforce and labor reallocation across firms.

I. Introduction

Understanding how a firm's access to public equity markets affects its human capital and the reallocation of labor across firms is important. Conventional wisdom says that capital raised via an initial public offering (IPO) allows firms to create jobs and accumulate more human capital. This motivates policies across the globe to make it easier for firms to go public. However, the effects of IPOs on labor markets

We thank Mara Faccio (the editor), Meghana Ayyagari, Shai Bernstein, Sen Chai, Thomas J. Chemmanur, Mike Ewens, Laura Lindsey, Elena Loutskina, Debarshi Nandy, Merih Sevilir, Mindy Zhang, seminar participants at Columbia University, HBS-Entrepreneurship, North Carolina State, the University of Minnesota, the University of Naples Federico II, the SEC, the University of Virginia and conference participants at the AFA, Darden and Cambridge Judge Entrepreneurship and Innovation Research Conference, Cavalcade, Changing Role of Stock Markets in Capital Formation-NYU, Midwest Finance Association, NBER Entrepreneurship Working Group, SMLR Fellowship Program Workshop, the Society of Labor Economists (SOLE) annual conference, Texas Finance Festival, and the Tuck Private Equity and Entrepreneurship Research Conference. We thank Belinda Chen for outstanding research assistance. We thank Shirley Liu, Danielle Sandler, and Emily Wisniewski for their diligent assistance with the data and clearance requests. The views and conclusions are those of the authors and do not necessarily indicate concurrence by the Federal Reserve Board, other members of its staff, or other members of the Federal Reserve System. The research in this paper was conducted while the authors were Special Sworn researchers of the U.S. Census Bureau at the Triangle and Baruch Census Research Data Center. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 881 (CBDRB-FY24-0105).

are unclear. Theoretically, going public can resolve financial constraints, allowing the firm to increase wages and employment. These firms may also be able to attract new talent and decrease the departures of workers with valuable ideas as in Rajan (2012) and Babina (2020). However, the transition to public ownership could also exacerbate agency and career concerns, leading to empire-building, diversification, a short-term focus on safer projects, and reduced experimentation. This may trigger more departures of creative and entrepreneurial-minded workers (Williamson (1964), Jensen (1986), and Manso (2016)).

More broadly, IPOs have been shown to impact the real economy, although unresolved questions remain. For example, patent-based metrics show mixed effects of IPOs on innovation (Atanassov, Nanda, and Seru (2007), (Bernstein (2015), Acharya and Xu (2017), and Cong and Howell (2021)). Additionally, IPOs can have negative spillover effects on employment growth in the county of an IPO firm (Cornaggia, Gustafson, Kotter, and Pisciotta (2024)) and positive spillover effects on the local economy nearest the IPO firm headquarters, leading to higher real estate prices and more consumer spending (Butler, Fauver, and Spyridopoulos (2019)). Similarly, while employment at the firm going public increases (Kenney, Patton, and Ritter (2012), Borisov, Ellul, and Sevilir (2021)), it is not clear that going public creates jobs organically, since firms tend to be active acquirers immediately after their IPOs (Celikyurt, Sevilir, and Shivdasani (2010), Brau and Fawcett (2006)).

In this paper, we bring new evidence of the real effects of IPOs by creating a unique data set that combines data on U.S. IPO filing firms with establishment and firm worker matched data from the U.S. Census. These data allow us to document a number of stylized facts about IPO filing firms' labor forces before and after an IPO filing. Using an instrumental variables (IVs) approach, we estimate the causal impact of IPOs on firms' employment, wages, and the inflows and outflows of human capital. These findings provide novel evidence on the real consequences of a firm's transition from private to public ownership, an especially important topic given the active debate about the recently documented decline in public firms and IPOs in the U.S. (Doidge, Karolyi, and Stulz (2017), Kahle and Stulz (2017)) and the importance of labor as the key input for firms (Zingales (2000)).

We first highlight several novel facts about an IPO filing firm's workforce. First, we document that the mean annual earnings of all employees, measured a quarter before the IPO filing, are over $83,000, compared to the national average of $37,000, as measured over the same period, indicating that IPO filing firms employ primarily high-skill workers. While the high level of wages might be surprising, this evidence is consistent with the recent data in Ewens, Nanda, and Stanton (2020) that the cash compensation of founders of VC-backed startups increases substantially well before the liquidity event that usually occurs at the IPO or a sale of a company. In contrast, following an IPO filing, new hire earnings are only $57,000, on average. Second, consistent with anecdotal evidence, we find that workers at firms filing for an IPO tend to be young, white, and male. Third, on average, 2.3% of employees leave an IPO filing firm in the 3 years post-IPO filing for key positions at startups. This departure rate is large compared to the mean rate of 1.5% at all public firms, as measured in Babina (2020), suggesting IPO filing firms are important sources of entrepreneurs.

We next examine how the IPO causally impacts a firm's human capital. Given the inherent endogeneity in the IPO decision, important issues with the selection of firms that go public (Maksimovic, Phillips, and Yang (2023)), and the life-cycle effects associated with going public (Arikan and Stulz (2016)), we use a sample of firms that filed for an IPO and compare those that succeed in going public to those that withdraw their IPO. Furthermore, to control for unobservable differences between firms with successful and withdrawn IPOs, we use an IV approach following Bernstein (2015). Specifically, we use Nasdaq returns in the 40-trading-day window immediately following the IPO filing to instrument for IPO completion, as lower Nasdaq returns are associated with a significant decrease in the probability of IPO completion.[1] While market returns can predict firm growth and, consequently, labor market outcomes due to the correlation between market returns and investment opportunities, we control for this correlation with year fixed effects, and our instrument relies only on variation in returns over a short post-IPO filing window.

Using this IV approach, we document several important facts regarding the labor outcomes at IPO firms. First, we show that overall firm employment increases following IPOs. Post-IPO firm employment increases by nearly 20% annually over each of the 3 years following the IPO filing, on average. These results are consistent with Borisov et al. (2021), who document that IPOs cause higher employment using National Establishment Time-Series (NETS) data. These results support a financial constraints mechanism in which access to public markets allows a firm to grow more rapidly but could also be consistent with agency theories, such as empire building, given the possible decline in high-powered incentives following the transition to less-concentrated public ownership. To shed light on the debate about whether IPOs create jobs organically or just add jobs through mergers and acquisitions (M&As), we examine whether the increased employment is driven purely through the additions of new establishments that would be added through M&As. We do not find a significant increase in the number of establishments following the IPO, suggesting that firms grow at least some employment organically and expand their employment by using larger offices. Moreover, this employment growth is more pronounced in non-core industries, defined as employment outside the 4-digit SIC industry with the greatest ex ante employment. These results suggest that the IPO not only facilitates growth but also industrial diversification as well, consistent with predictions of agency theories.

We next show that IPOs lead to statistically insignificant wage changes for pre-IPO workers. By itself, this result does not support the existence of financial constraints at the IPO filing firm. A financial constraints channel would predict rising wages post-IPO, as shown theoretically in Michelacci and Quadrini (2009). While firms with successful IPOs tend to hire lower-wage workers following an IPO, as compared to their pre-IPO workforce, we document a significant wage premium paid post-IPO to new hires when comparing wages at their previous

[1]The probability of withdrawing an IPO depends directly on the overall performance of the market during the book building period as seen in Busaba et al. (2001)), Benveniste et al. (2003), Edelen and Kadlec (2005), and Dunbar and Foerster (2008).

employer to their starting wage at the IPO filing firm. This increase in the new hire wage premium may indicate fewer incentives to keep wages low following the IPO or evidence of a firm's rapid growth in employment while facing an upward-sloping labor supply curve. As the post-IPO firm hires more workers, the marginal worker now needs to receive a higher premium to be willing to join the firm. Overall, the wage evidence is not entirely consistent with the financial constraints channel, while it does not contradict an agency channel.

In further support of the notion that the change in ownership has important implications for the firm's labor force, we find differences in post-IPO employee turnover. Consistent with a reduction in experimentation and a decrease in investments in risky ideas, we observe an increased rate of departure of entrepreneurial-minded employees. This result is consistent with a career concerns mechanism. Public ownership will entail less close monitoring. As such, managers may reduce investments in innovation out of fear that investing in innovation will expose them to undue termination risk, as the market may penalize them for failures, even if the failure was due to idiosyncratic reasons. Following a successful IPO, the rate at which employees leave to take key positions at new firms (presumably those employees most likely to be engaged in innovative activities) grows by over 5 percentage points, when accounting for the endogeneity of the choice to go public. This evidence complements Bernstein (2015), who examines the mobility of inventors cited in the patents of IPO filing firms to firms that patent for the first time. Our results expand his findings to non-patenting IPO filing firms and to a broader class of workers besides employees with patents.

We further document that the departure of existing employees to startups following an IPO is strongest for firms in high-tech sectors. This supports the career concerns mechanism, as these firms are most likely to have been focused on high-risk, high-growth projects associated with experimentation, where entrepreneurial-minded employees would have been relatively more important (Manso (2016)). As incentives for experimentation decline due to short-term profit pressures post-IPO, these firms now require fewer entrepreneurial-minded employees, leading to higher turnover of these employees. We document that the turnover results are stronger among higher-paid and younger employees. Higher-wage employees are more likely to be high-skilled, and younger employees are associated with more creative innovation (Liang, Wang, and Lazear (2018), Ouimet and Zarutskie (2014)) and greater career concerns.

Finally, we confirm the robustness of our main results by showing placebo results for our IV approach. We also address potential concerns that some of our results rely on the employee–employer matched Longitudinal Employer–Household Dynamics (LEHD) data. Specifically, while our data on employment, number of establishments, geographic and industrial diversification, and establishment mean wages cover all domestic employment at IPO firms, our data on individual wages and employee reallocation cover employment only in our 31 LEHD states. We show that summary statistics for firms headquartered in our 31 LEHD states are similar to those of firms headquartered outside of our 31 LEHD states. Importantly, we also show that our baseline results are not statistically different when estimated on firms with headquarters outside our 31 LEHD states.

Our paper adds to the empirical literature on the role of human capital in IPO firms. Previous literature has considered the role of managers' human capital (Chemmanur and Paeglis (2005), Kaplan, Sensoy, and Strömberg (2009)), underwriters (Carter and Manaster (1990), Carter, Dark, and Singh (1998)), and venture capitalists (Megginson and Weiss (1991), Hellmann and Puri (2002)).[2] We extend this earlier literature by considering all employees at IPO firms. The overall labor force at the firm is an important driver of firm success and has been underresearched in this setting. Although some employees, such as the founder, have outsized roles in young firms, the success of these firms critically depends on the firm's overall human capital.

Our focus on the overall labor force is most similar to two more recent papers, Borisov et al. (2021), which focuses on the effect of going public on firm employment, and Bernstein (2015), which focuses on the effect of going public on innovation and finds that key inventors are more likely to leave post-IPO. We complement and extend the results in Borisov et al. (2021) by using U.S. Census microdata to document additional and important characteristics of the firm's labor force pre- and post-IPO. For example, we document that while IPOs have no significant effect on the earnings growth of pre-IPO workers, an IPO does lead to an increase in the wage premium offered to new hires. We also provide the first evidence that successful IPOs cause an increase in industrial diversification. We complement and extend the results in Bernstein (2015) by showing that the outflow of talent post-IPO to startups occurs in a sample broader than the key inventors explored in this earlier paper. Moreover, our results are an important complement to the existing literature looking at employee turnover post-IPO using patents.

Overall, our paper provides new evidence to the active debate on the trade-off between listing as a public company and staying private and its influence on a firm's real activities, the debate invigorated by influential papers showing the decline of public firms (Doidge et al. (2017), Kahle and Stulz (2017)). Access to public markets offers the benefits of cheaper capital (Pagano, Panetta, and Zingales (1998)), allowing a public firm to conduct more M&As (Celikyurt et al. (2010)), encourages public firms to be active buyers and sellers of assets in merger waves (Maksimovic, Phillips, and Yang (2013)), helps grow employment (Borisov et al. (2021)), expands into new geographic markets (Cornaggia, Gustafson, Kotter, and Pisciotta (2021)), and improves innovation (Acharya and Xu (2017)). Public firms are also more responsive to changes in investment opportunities than their private counterparts (Mortal and Reisel (2013), Phillips and Sertsios (2014), Gilje and Taillard (2016), and Maksimovic et al. (2023)) and invest more, as compared to their private counterparts, as shown in Feldman, Kawano, Patel, Rao, Stevens, and Edgerton (2021)).[3] Alternatively, the agency conflicts resulting from divergent incentives between investors and managers at public firms can impair firm investments (Asker, Farre-Mensa, and

[2]There is also broader literature that looks at changes to firm characteristics following IPOs, including the impact on real investments and productivity (Chemmanur, He, and Nandy (2010)), innovation (Bernstein (2015)), operating income (Jain and Kini (1994)), stock returns (Ritter (1991)), Loughran and Ritter (1995)), insider ownership (Mikkelson, Partch, and Shah (1997)), and acquisitions (Celikyurt et al. (2010)).

[3]In contrast, using a sample of commodity chemical producers, Sheen (2020) finds that private firms are more responsive to demand shocks.

Ljungqvist (2015)). We document that IPOs have real effects with implications for wages and the reallocation of labor across firms.

II. Hypotheses and Tests

In this section, we present our main hypotheses. We focus on three broad sets of theories on the impact of IPOs on firms' human capital: financial constraints, agency theories, and career concerns. We review these theories and draw out testable implications.

A. Financial Constraints

Stock markets can provide various benefits as a source of external capital by reducing asymmetric information, lowering the cost of capital, and enabling the development of growth opportunities in firms (Rajan (2012)). As investors' portfolios become more liquid and diversified, stock market listing lowers the cost of capital (Pagano et al. (1998)). Going public also helps to lower borrowing costs because of the reduced asymmetry of information. These benefits of going public are likely to result in the relaxation of financial constraints, leading to an increase in firm investments and employment growth (Borisov et al. (2021)). Moreover, since companies often grow by using IPO proceeds to expand into new geographic markets (Cornaggia et al. (2021)), IPO proceeds might relax the financial constraints needed to enter new geographic markets. Hence, the financial constraints channel predicts faster employment growth and the expansion into new geographic markets following the IPO.

If a public offering resolves financial constraints, then we should observe particular wage patterns. Michelacci and Quadrini (2009) develop a labor market equilibrium model in which firms sign optimal long-term contracts with workers. Firms that are financially constrained offer an increasing wage profile: they pay lower wages today in exchange for higher future wages once they become unconstrained. Hence, if IPOs resolve financial constraints, we should observe rising wages of existing workers following the IPO. Moreover, if an IPO-filing firm has valuable growth opportunities that can be competed away, the firm might be willing to pay a wage premium that would enable it to hire workers quickly. Hence, the financial constraints channel predicts high wage growth of both pre-IPO and new workers, following the IPO.

Moreover, the relaxation of financial constraints of the IPO filing firm also implies greater retention of employees who work on the development and implementation of new ideas, products, and services (Grossman and Hart (1986)). The infusion of cash from an IPO means that some of the employees may now stay and develop their ideas internally, ideas that may not have been funded by the firm when it was private due to financial constraints (Babina (2020)). This increased retention of talent is likely to be higher in industries with high-growth opportunities (Maksimovic and Phillips (2002)) and among high-skilled workers who have domain area expertise to develop the projects. In sum, the easing of financial constraints is likely to be associated with higher retention of existing employees and an increase in new hires.

Moreover, we expect all the predictions of the financial constraints mechanism to be particularly salient at small firms, as in Hadlock and Pierce (2010).

B. Agency Theories

The transition from private to public ownership leads to less concentrated ownership and a possible weakening of managerial incentives, potentially resulting in agency costs. First, since larger firms may offer management more perks and higher compensation, managers may engage in empire building (Williamson (1964), Jensen (1986)). This may result in higher overall firm employment. Second, managers might seek to diversify into different industries, since diversification decreases uncertainty and benefits risk-averse managers; hence, greater diversification can indicate greater agency conflicts (Amihud and Lev (1981), Denis, Denis, and Sarin (1997)).[4] The agency channel, thus, predicts faster employment growth and an increase in industrial diversification following the IPO. Moreover, with weaker managerial incentives post-IPO, managers may have less incentive to keep wages low, predicting higher post-IPO wage growth.

C. Career Concerns

Career concern theories can also predict lower retention of human capital involved in developing new ideas, products, or services following IPOs. Investing in innovation is inherently risky, and failure does not necessarily reflect managerial talent. However, managers anticipate that if they invest in innovation and the innovation fails, even if due to idiosyncratic reasons, investors may attribute this failure to their abilities (Holmstrom (1989)). This career concerns argument is particularly salient at public firms, where a diverse shareholder base cannot closely monitor the CEO and thus is less able to identify whether an innovation failure is due to poor managerial skill or bad luck (Holmstrom (1989)). Moreover, Stein (1989) shows that stock markets tend to target short-term earnings, and such myopia could induce public firms to invest suboptimally. With their compensation and job security linked to stock performance, the managers of public firms have incentives to sacrifice long-term investments to boost short-term stock returns. Likewise, Ferreira, Manso, and Silva (2014) introduce a model to demonstrate that managers of public companies are biased against innovative projects, which typically have a higher failure rate and prefer safer projects. An implication of their models is that stock markets hinder firms from investing in high-risk, high-growth projects, and projects that require experimentation and long-term horizons. Recent empirical evidence is consistent with this view. Terry, Whited, and Zakolyukina (2023) find that managers cut R&D around periods in which they have to restate their books. Bernstein (2015) finds that the novelty of innovation reduces after a firm goes public.

Since entrepreneurship is often associated with high-risk, high-growth projects as well as experimentation and a long-term horizon (Kerr, Nanda, and Rhodes-Kropf (2014), Manso (2016)), a career concerns mechanism predicts that following IPOs, managers will prefer to invest in more routine projects with quicker payoffs

[4]While managers may be motivated to diversify their firms for personal gain, this does not require that diversification is necessarily value-destroying for shareholders.

that require fewer entrepreneurial-minded workers. Moreover, this career concerns channel has three cross-sectional predictions. First, managers in industries with more high-risk, high-growth opportunities are more likely to benefit from reduced investments in risky opportunities and, therefore, have fewer incentives to retain employees with experimental-type preferences following an IPO. We thus predict that increased departures to startups post-IPO are likely to be relatively larger in high-tech industries. Second, since the development of new projects requires high-skilled labor, the departures post-IPO are likely to be relatively higher among high-skilled and high-wage talent. Third, given that career concerns about having innovation failure incorrectly attributed to worker skill due to insufficient monitoring at public firms should be most relevant for young workers, we predict that innovative younger workers are relatively more likely to depart following IPOs.

D. Testable Implications

We sum up the predictions drawn above for our three non-mutually exclusive mechanisms.

1. Employment. Both the financial constraints and the agency channels predict increased post-IPO employment growth. However, the two channels disagree on how the employment would grow: the financial constraints channel predicts geographic expansion, while agency theories predict expansion into new industries and increased industrial diversification.
2. Wages. The financial constraints channel makes a clear prediction that we should observe rising wages of existing workers following the IPO. Moreover, workers hired post-IPO are likely to fetch a wage premium.
3. Retention and hiring of talent. The financial constraints and career concerns channels disagree in their predictions on the turnover and hiring of talent involved in developing new ideas, products, or services. The financial constraints channel predicts lower turnover to startups following IPOs, with increased retention in high-tech industries and among high-wage workers. In contrast, the career concerns channel predicts heightened turnover to startups post-IPO, particularly in high-tech industries, and among high-wage and younger workers.

III. Measuring Employment, Wages, Turnover, and Hiring

Key to our analysis is our ability to measure employment, wages, employee outflows, and inflows across time. In the following section, we review the multiple databases used to measure our variables of interest and to create our sample. We also provide summary statistics of the firms included in our sample and discuss the calculations of key variables.

A. Data Sources

We combine databases from the following sources to form our estimation sample: the U.S. Census Bureau; Thompson Reuter's SDC, VentureXpert, VentureSource, and "Carter-Manaster" underwriter reputation from Loughran and Ritter (2004).

1. Establishment-Level Data

We start with establishment-level information from the Longitudinal Business Database (LBD), a database maintained by the U.S. Census Bureau. The LBD is a panel data set that tracks all U.S. business establishments.[5] An establishment is any separate physical location operated by a firm with at least 1 paid employee. The LBD contains information on the number of employees working at the establishment, total annual establishment payroll, and the industry and physical location of each establishment. In addition, the LBD contains a unique firm-level identifier, *firmid*, which longitudinally links establishments that are part of the same firm. We observe the LBD for all 50 states and the District of Columbia, which allows us to measure IPO filing firms' age, total employment, number of establishments, and average firm wages across all 50 states, along with the industrial and geographic diversification of each firm.

2. Matched Employer–Employee Data

We add worker-level data using the LEHD data, also maintained by the U.S. Census Bureau. This database tracks employers, employees, and their earnings on a quarterly basis. The LEHD data also allow us to observe the age, gender, race, and place of birth of each employee. We link the LEHD to firm identifiers in the LBD using the employer identification number (EIN). The LEHD data are collected from the unemployment insurance records of states participating in the program.[6] Data start in 1990 for several states, and the number of states included increases over time. The data coverage ends in 2008. Our project has access to data from 31 states: Arkansas, Colorado, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Louisiana, Maine, Maryland, Minnesota, Missouri, Montana, Nevada, New Jersey, New Mexico, North Carolina, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Utah, Vermont, Virginia, Washington, and Wisconsin. While we do not observe data for all states, we observe almost 100% of private employment (Jarmin and Miranda (2002)) for any state in the program. We map states available in our LEHD data in Figure 1.

Given that the high-tech sector is an important one among IPO filing firms, a concern may arise that our LEHD data do not cover key states, such as California and Massachusetts. In Subsection VI.B., we validate the LEHD sample by showing that results using the LBD data, which are available for all 50 states, are similar whether estimated over the LEHD or non-LEHD states, thereby mitigating any concerns of bias due to incomplete LEHD coverage. We also show that 49% of firms in our LEHD sample are high-tech firms, compared to 50% for all IPO filing firms, mitigating concerns that we undersample high-tech firms.

3. Data on IPOs and Other Financial Variables

We use SDC to identify firms filing for IPOs from 1992 through 2006 and to determine whether the IPO was completed or withdrawn. We start in 1992 and end in the first quarter of 2006 in order to match the time series of available data from the

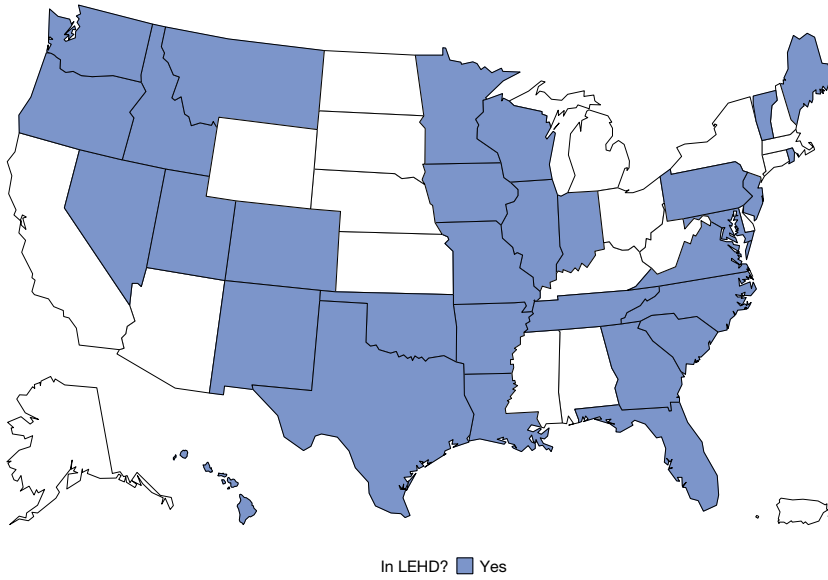
[5]See Jarmin and Miranda (2002) for more information.

[6]See Abowd, Stevens, Vilhuber, Andersson, McKinney, Roemer, and Woodock (2009) for a more detailed description of the program and the underlying data sets that it generates.

FIGURE 1

Map of 31 U.S. States Available in LEHD Database

Figure 1 shows which states are included in the LEHD data base. Included states are shown in blue.



Census Bureau sources and allow for a 3-year, post-event window to measure post-IPO filing labor-related outcomes. We exclude the IPO filings of firms in agriculture, mining, and construction (SIC codes 1000–1999), financial firms (SIC codes 6000 and 6999), non-U.S.-based firms, unit offers, closed-end funds (including Real Estate Investment Trusts (REITs)), American Depositary Receipts (ADRs), limited partnerships, special acquisition vehicles, spin-offs, and issues of non-common shares.[7]

Using these restrictions, we identify in the SDC data 4900 firms with IPO filings during our time period.[8] We link our IPO filing sample to the Census data in the year the firm first filed for an IPO. Because the SDC does not provide EINs for some firms, we fill in missing EIN information by obtaining the EIN from the underlying SEC filing, typically a Form S-1 or a Form S-1/A, when possible. This leaves us with a sample of 4,700 IPO filings with non-missing EINs that we attempt to match to Census data.[9] The final sample used in the analysis is 3,400 IPO filing firms that i) we are able to match to the LBD, using all 50 states; and ii) have all control variables used in our regression analysis.[10] We find that 77% of the firms in

[7]We exclude firms in industries with (1-digit) SIC codes of 1 (agriculture, mining, and construction), as the (within-sample) correlation of IPO completion with Nasdaq returns is weaker for SIC1 = 1 than it is for other industries in our sample.

[8]The number of observations is rounded to the nearest 100 due to Census Bureau disclosure policies.

[9]We cannot obtain EINs for all IPOs, given that some IPO filings reported in SDC as being issued by domestic companies are actually issued by foreign companies, which we exclude. Additionally, a few small issuers do not file a Form S-1.

[10]Our match rate is comparable to other papers matching IPO data to Census data using EINs, such as Maksimovic et al. (2023).

our final sample successfully completed their IPOs. Since employee-level data are available for only 31 states, the worker-level LEHD sample consists of 2,400 unique firms that have employees in the covered states.

We use a separate database provided by SDC to identify all M&As of firms in our IPO filing firm sample. We match across databases using the CUSIP of the issuing firm. We define a successful acquisition when the deal is completed and a firm's post-acquisition ownership percentage exceeds 50%. We identify firms that received VC funding using both SDC Thomson (VentureXpert) and Dow Jones (VentureSource) data sources. We employ a crosswalk developed by Puri and Zarutskie (2012) that uses a name- and address-matching algorithm to link firms in VentureSource and VentureXpert to the LBD. We use this crosswalk to identify which of the IPO filing firms we match to the LBD are VC-financed. We measure "Carter-Manaster" underwriter quality (based on Carter and Manaster (1990)) from Loughran and Ritter (2004). Other deal characteristics are from SDC.

B. Variable Construction

To measure firm and employee characteristics of IPO filing firms using the LBD, we measure establishment-level values the year of the IPO filing. To measure flows, we compare these ex ante estimates to ex post estimates, observed 3 years after the IPO filing event. Our 3-year window ensures that sufficient time has passed for IPO filing firms to complete or withdraw their IPO filing, that the lock-up windows have expired, and that new funds have been invested. We then calculate firm-level estimates by aggregating establishment-level data to firm-level data. Alternatively, the worker-level LEHD data are observed quarterly. Thus, we estimate worker-level characteristics in the LEHD using the quarter that precedes the IPO filing date and again estimate ex post values using a snapshot occurring 3 years after the IPO filing. We aggregate across all workers observed in our data to calculate a firm-level estimate.

C. Summary Statistics

Table 1 reports summary statistics for our primary samples, as measured at the time of the IPO filing. In the first column, we report the mean and standard deviation (in parentheses) of firm-level characteristics for all firms in our sample. In the second column, we limit the sample to firms that withdrew their IPOs. In the third column, we limit the sample to firms that successfully completed their IPOs. In Panel A, we start with our main sample of 3,400 firms. This sample is constructed using LBD data (available for all 50 U.S. states and the District of Columbia) and is measured as of the year of the IPO filing.

Firms filing for an IPO are typically young, averaging 8.8 years.[11] These firms also tend to be small, with an average of 467 employees spread over nine distinct establishments, and have minimal industrial diversification. On average, 93% of

[11] Firm age is equal to the age of the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda (2013)). This definition of firm age will not misclassify an establishment that changes ownership through M&As as a firm birth, since a firm is defined as a new firm only when all the firm establishments are new establishments, and establishment age should remain the same in the LBD regardless of ultimate ownership.

TABLE 1

Summary Statistics on IPO Filing Firms as of IPO Filing

Table 1 describes firm-level characteristics of IPO filing firms. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. In Panel A, characteristics are measured as of the year of the IPO filing. In Panel B, characteristics are measured as of the quarter immediately preceding the IPO filing. The table reports means for all variables and standard deviations, in parentheses, for continuous variables. The unit of observation is a complete firm (Panel A; 3,400 observations) or a firm-level aggregation of workers available in our 31 LEHD states (Panel B; 2,400 observations). Column 1 reports the values for the full sample. Columns 2 and 3 report the values for the sample of firms that withdrew (completed) their IPO filing. Firm Age_t is calculated as the age of the oldest establishment owned by the firm in the first year it appears in the LBD. $Employment_t$ is total firm employment calculated as the sum of employment of all the firm's establishments in the LBD (50 U.S. states and the District of Columbia). Number of $Establishments_t$ is the number of all the firm's establishments in the LBD. Number of $States_t$ is the number of states in which the firm has establishments in the LBD. Industrial $Concentration_t$ is the fraction of the firm's employment in the LBD that is in the firm's biggest employment-wise SIC4 industry. Nasdaq Return 40 Trading Days After is the Nasdaq return in the 40-trading-day window following the IPO filing. Nasdaq Return 60 Trading Days Before is the Nasdaq return in the 60-trading-day window prior to the IPO filing. Syndicate Size is the number of underwriters of the IPO. Underwriter Reputation measures the "Carter-Manaster" underwriter quality. We obtain these data from Jay Ritter's website. Log Filing Amount is in millions of dollars. VC-backed is 1 if the firm received VC investment prior to the IPO filing, and 0 otherwise. High-tech is 1 for firms in the computer, bio-tech, or electronics sectors, and 0 otherwise. HQ State not in LEHD is 1 for firms with headquarters outside of the LEHD coverage, and 0 otherwise. Average Worker Age_t is the firm-level average of employee age. Percent $Female_t$ is the percentage of the firm's workforce that is female. Percent $White_t$ is the percentage of the firm's workforce that is white. Percent Foreign-$born_t$ is the percentage of the firm's workforce that was born outside of the U.S. Average $Wages_t$is the average annualized quarterly earnings of the firm's workers (in thousands and in 2014 real dollars). Per Census Bureau disclosure rules, observations, and estimates are rounded.

	Full Sample	IPO Withdrawn	IPO Completed
Panel A. LBD Sample (3,400 observations)			
Firm age_t	8.84	8.34	8.99
	(7.05)	(7.09)	(7.03)
$Employment_t$	467	491	460
	(865)	(940)	(841)
Number of $establishments_t$	9.00	9.00	9.01
	(19.91)	(20.11)	(19.85)
Number of $states_t$	3.10	3.06	3.12
	(4.60)	(4.65)	(4.58)
Industrial $concentration_t$	0.925	0.919	0.927
	(0.165)	(0.178)	(0.160)
NASDAQ Return 40 Trading Days After	0.011	−0.025	0.022
	(0.100)	(0.123)	(0.089)
NASDAQ Return 60 Trading Days Before	0.060	0.058	0.060
	(0.121)	(0.148)	(0.111)
Syndicate Size	2.59	2.67	2.57
	(1.58)	(1.24)	(1.67)
Underwriter Reputation	7.06	7.46	6.94
	(2.22)	(1.99)	(2.27)
Log Filing Amount ($ millions)	3.5	3.66	3.46
	(1.02)	(0.972)	(1.02)
VC-Backed	0.489	0.541	0.473
High-Tech	0.503	0.495	0.506
HQ State not in LEHD	0.527	0.533	0.525
IPO Completed	0.77	0	1
Panel B. LEHD Sample (2,400 observations)			
Average Worker Age_t	37.36	37.56	37.30
	(4.77)	(4.68)	(4.80)
Percent $Female_t$	0.34	0.35	0.33
	(0.25)	(0.24)	(0.25)
Percent $White_t$	0.83	0.82	0.83
	(0.17)	(0.16)	(0.17)
Percent Foreign-$Born_t$	0.07	0.07	0.07
	(0.12)	(0.11)	(0.12)
Average $Wages_t$ ($ thousands)	82.67	85.46	81.76
	(47.50)	(46.23)	(47.88)

employment is assigned to the firm's primary 4-digit SIC. This high industrial concentration is unique. For example, Babina (2020) finds that, on average, 61% of public firms' employment are in their top industry. Moreover, these firms tend to have modest geographic distribution, with physical locations in 3 states, on average. In the full sample, 49% of the firms are identified as being VC-backed, and 50% are in the high-tech sector, which includes the biotech, electronics, and computer industries.[12] Just under half of our firms have headquarters in our sample of LEHD states. On average, there are 2.6 underwriters in each syndicate with an average "Carter-Manaster" underwriter reputation of 7.0 as measured in Loughran and Ritter (2004) and based on Carter and Manaster (1990).[13,14] The average IPO in our sample files for just over $33 million in total proceeds, for a log value of 3.5. Across all these variables, we report economically similar estimates for the set of firms that withdrew their IPOs relative to the set of firms that successfully completed their IPOs. Bernstein (2015) makes the same conclusion when comparing firms that withdrew versus those that completed their IPOs along different dimensions, including innovation and accounting performance.

We measure Nasdaq market returns both before (60 trading days) and after (40 trading days) the IPO filing. Average 40-trading-day post-issuance Nasdaq returns at firms that complete their IPOs are 2.2%, compared to −2.5% for firms that withdraw their IPOs. These results are consistent with a number of earlier studies documenting the role of market returns in IPO success, such as Busaba, Benveniste, and Guo (2001), Benveniste, Ljungqvist, Wilhelm, and Yu (2003), Edelen and Kadlec (2005), Dunbar and Foerster (2008), and Bernstein (2015). Alternatively, we find an economically small difference of 0.2% in pre-filing returns between the two groups.

In Panel B, we report worker characteristics for the quarter before the IPO filing. Firm-level means are reported, using all employment available in our 31 LEHD states. The unit of observation is a complete firm for firms whose employment is located solely within our 31 states. Alternatively, the unit of observation is a partial firm, for firms with some employment outside of our 31 LEHD states. Our total sample size drops to 2,400 observations, reflecting the fact that some IPO filing firms have no employment within our 31 LEHD states.

We find that workers at firms that file for an IPO tend to be young, with an average age of 37 years. Consistent with anecdotal evidence, our sample is disproportionately white and male. Interestingly, fewer than 10% of workers are born outside the United States, a fraction much smaller than the share of immigrant founders among VC-backed firms. These workers are well compensated, with average annual earnings of approximately $83,000, compared to the national average of $37,000 over this period.[15] Earnings include all forms of immediately

[12]A firm is in the "Biotech" industry if its primary SIC code is 2830–2839, 3826, 3841–3851, 5047, 5048, 5122, 6324, 7352, 8000–8099, or 8730–8739, and excluding 8732. A firm is in the "Electronics" industry if its primary SIC code is 3600–3629, 3643, 3644, 3670–3699, 3825, 5063 or 5065. A firm is in the "Computers" industry if its primary SIC code is 5044, 5045, 5370–5379, 5734, or 7370–7379.

[13]Underwriter reputation data are available from: https://site.warrington.ufl.edu/ritter/ipo-data/.

[14]When missing, we replace the value with the sample median to avoid generating a sliver sample.

[15]U.S. Bureau of the Census's "Real Mean Personal Income in the United States" retrieved from FRED, Federal Reserve Bank of St. Louis, and expressed in 2014 real dollars.

taxable compensation, such as salaries, wages, commissions, bonuses, and exercised stock options. We acknowledge that one limitation of our data is that these earnings do not include non-taxable compensation. It is not entirely clear how the absence of unexercised stock grants will impact our analysis. If firms are financially constrained before the IPO filing, they may rely more on stock-based compensation. Alternatively, following an IPO and the creation of a publicly traded stock price, performance pay should become less risky, and the optimal compensation contract might now include more stock-based compensation. These high pre-IPO earnings of $83,000 can be contrasted with the much lower mean earnings of $34,000 at all young firms reported in Babina, Ma, Moser, Ouimet, and Zarutskie (2019), indicating that IPO filing firms employ a different types of worker than a typical young firm. Finally, firms with withdrawn and completed IPOs look economically similar across these different employee characteristics.

Table 2 documents statistics of firm and employee characteristics following the IPO filing. As in the previous table, we report the mean and standard deviation (in parentheses) of firm-level characteristics and present statistics for all firms, withdrawn-IPO firms, and completed-IPO firms. In Panel A, we start with our main sample of 3,400 IPO filing firms and document statistics on annualized changes in employment size, wages, and diversification over the 3 years immediately following the IPO filing. For the full sample, we show a large average increase in employment post-IPO filing. However, this average masks striking differences between firms with successful and unsuccessful IPOs. Firms with successful IPOs experience an average annual increase in employment of 23% during the subsequent 3 years. Alternatively, firms that later withdraw their IPO filings instead have an average annual employment growth rate of 7%. Coincident with the growth in total employment, we also document an increase in the number of establishments per year post-IPO. On average, firms with successful IPOs increase their number of establishments by 7.2% per year, compared to a 4.5% increase at firms with withdrawn IPOs.

Industry concentration declines post-IPO filing and is especially pronounced at firms with successful IPOs. We also report a similar pattern with geographic concentration. Firms with successful IPOs are associated with an average annual growth of 5.1% in the number of states in which they have a physical presence, compared to an average 3.2% growth for firms that withdrew their IPOs.

Finally, we measure changes in firm-level wages, where wages are measured as salaries, tips, bonuses, and commissions. Firm-level wages can vary over time due to time series variation in the wages of employees who remain at the firm (i.e., pay raises for continuers) as well as from changes in the composition of workers over time (i.e., different wage-level workers get hired over time). On average, for firms in our sample, firm-level mean wages decline by 3.6%. This pattern is more pronounced for firms that withdrew their IPOs. However, the difference is not statistically significant.

In Panel B, we report firm-level aggregates of worker-level data from our LEHD states. In the first row, we measure the average annualized change in employee earnings, measured over the 3 years immediately following the IPO. By definition, this variable can be estimated only for those workers who were

TABLE 2

Summary Statistics on IPO-Filing Firms Following IPO Filing

Table 2 describes changes in firm-level characteristics following a firm's IPO filing. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The table reports means for all variables and standard deviations, in parentheses, for continuous variables. The unit of observation is a complete firm (Panel A; 3,400 observations) and a firm-level aggregation of workers available in our 31 LEHD states (Panel B; 2,400 observations). Column 1 reports the values for the full sample. Columns 2 and 3 report the values for the sample of firms that withdrew (completed) their IPO filing. Annualized Growth in Employment$_{t,t+3}$ is the annualized employment growth over the 3 years following an IPO filing relative to the year of the IPO filing, calculated using LBD data for all 50 U.S. states and the District of Columbia. Employment growth is calculated as the log difference between the future and the IPO filing years' employments. If the 3-year future employment is missing, we use future annualized 2-year employment; if both 3- and 2-year future employments are missing, we use 1-year future employment. All variables in Panel A are calculated similarly to Annualized Growth in Employment$_{t,t+3}$. Annualized Growth in Average Firm Wages$_{t,t+3}$ is calculated over 3 years following an IPO filing relative to the year of the IPO filing. Average Firm Wages are total firm payroll divided by total firm employees. This measure captures two things: post-IPO wage changes of remaining employees and changes in wage composition due to new hires. Annualized Growth in # of Establishments$_{t,t+3}$ is the annualized growth in the number of a firm's establishments over the 3 years following an IPO filing relative to the year of the IPO filing. Annualized Growth in # of States$_{t,t+3}$ is the annualized growth in the number of states in which a firm operates over the 3 years following an IPO filing relative to the year of the IPO filing. Annualized Growth in Industrial Concentration$_{t,t+3}$ is calculated over the 3 years following an IPO filing relative to the year of the IPO filing. The 3-year Growth in Employee Wages$_{t,t+3}$ is measured using the sample of workers 1 quarter before IPO filing who are observed 3 years later, and defined as differences in log wages. New Hire Wage Premium is measured using the sample of workers hired between the quarter of IPO filing and 3 years after the filing, and is defined as log differences in wages earned during the first quarter at the IPO firm relative to wages at the employer just prior to the IPO filing employer. Fraction of Entrepreneurial Departures$_{t,t+3}$ is the fraction of workers 1 quarter before IPO filing who are observed 3 years later at a firm no more than 3 years old and who are among the top 5 earners at that firm. Per Census Bureau disclosure rules, observations, and estimates are rounded.

	Full Sample	IPO Withdrawn	IPO Completed
Panel A. LBD Sample (3,400 observations)			
Annualized Growth in Employment$_{t,t+3}$	0.194	0.074	0.230
	(0.347)	(0.369)	(0.332)
Annualized Growth in Average Firm Wages$_{t,t+3}$	−0.036	−0.089	−0.020
	(0.220)	(0.266)	(0.201)
Annualized Growth in # of Establishments$_{t,t+3}$	0.066	0.045	0.072
	(0.212)	(0.214)	(0.211)
Annualized Growth in # of States$_{t,t+3}$	0.046	0.032	0.051
	(0.147)	(0.139)	(0.149)
Annualized Growth in Industrial Concentration$_{t,t+3}$	−0.006	−0.003	−0.006
	(0.041)	(0.038)	(0.042)
Panel B. LEHD Sample (2,400 observations)			
3-Year Growth in Employee Wages$_{t,t+3}$	0.139	0.096	0.153
	(0.331)	(0.338)	(0.327)
New Hire Wage Premium	0.357	0.355	0.358
	(0.139)	(0.125)	(0.143)
Departure Rate to Entrepreneurship$_{t,t+3}$	0.023	0.022	0.024
	(0.065)	(0.042)	(0.071)

observed pre-IPO. For successful IPO firms, and to a lesser extent unsuccessful IPO firms, we document rising earnings. Reconciling the mean positive earnings changes observed using a constant set of pre-IPO workers with declining firm-level average earnings suggests that new hires are joining the firm at lower wages relative to the workers at the firm prior to the IPO filing. In fact, new hires added after the IPO filing receive an average earnings of $57,000, relative to the mean pre-IPO filing earnings of $83,000. We also report the average new hire wage premium, measured as the difference between the first full quarter wage at the new firm and the last full quarter wage at the previous employer. On average, our sample of IPO filing firms offers a sizable new hire wage premium of 36%. Firms with completed and withdrawn IPOs experience similar growth in wages for these new workers.

Finally, we show mean rates of departure to entrepreneurship, which is a measure of the worker flow to startups. Specifically, the departure rate to entrepreneurship is defined as the fraction of the employees at the firm prior to the IPO filing who, as of 3 years later, have left the firm and are now employed at a startup (firms younger than or equal to 3 years old) and where they are one of the top 5 earners in the new firm. We measure this departure rate using only top employees at the new firm to better capture founders and other key employees at the new firm, as in Azoulay, Jones, Kim, and Miranda (2020)) and Babina (2020).[16] On average, 2.3% of employees leave an IPO filing firm in the 3 years post-IPO filing to join a startup, where they hold key positions. This is large compared to the mean rate of 1.5% at all public firms, as measured in Babina (2020), suggesting IPO filing firms are important sources of entrepreneurs.

IV. Results

In this section, we report the results of testing the hypotheses developed in Section II related to changes in employment, wages, turnover, and hiring following a completed IPO. To allow for causal inference, we use an IVs approach. We start by discussing our IV approach. We then follow by showing second-stage results using the LBD data for all 50 states. Finally, we show second-stage results using the employee–employer matched LEHD data.

A. Validating the Instrument Variable

The successful completion of an IPO depends on market conditions during the book-building period as well as on firm-specific characteristics (Busaba et al. (2001), Benveniste et al. (2003), Edelen and Kadlec (2005), and Dunbar and Foerster (2008)). To allow for a clean inference of the causal impact of an IPO completion on employee outcomes, we instrument for IPO completion using Nasdaq returns in the 40 trading days following the IPO filing, an approach based on Bernstein (2015). The IV approach is quite helpful, as it is not possible to determine the sign of a potential OLS bias ex ante. It could be that stronger firms have better financing prospects in non-public markets and are more likely to withdraw if they believe the IPO valuation is too low. Alternatively, omitted variables, such as firm quality, may be correlated with both IPO completion rates (due to strong investor demand) and post-IPO firm outcomes.

To validate that Nasdaq returns during the 40-trading-day window following an IPO filing predict successful IPO completions in our sample, we estimate the following regression:

$$(1) \quad IPO_i = \beta_1 \text{ Nasdaq Return 40 Trading Days After}_i + X_i'\delta_i + \mu_t + \vartheta_k + \varepsilon_i,$$

[16]For example, Azoulay et al. (2020) find that a firm's top 3 initial earners usually include the firm's owners. This is because the W-2 data that form the basis for the LEHD must be filed for all employees, including owners who actively manage the business and are required by law to pay themselves reasonable wage compensation.

where Nasdaq Return 40 Trading Days After$_i$ is the cumulative Nasdaq returns during a 40-trading-day window, starting on the day of the IPO filing; IPO_i is a dummy variable that equals 1 if the IPO was successfully completed; $X_{i'}$ is a vector of control variables, μ_t are year fixed effects, ϑ_k are industry fixed effects, and ε_i is the error term. Observations are measured at the firm level and standard errors are clustered at the 2-digit SIC code level when using LBD data (available at the annual level), and double clustered at the 2-digit SIC code level and by year-quarter when using LEHD data (available at the year-quarter level).

Table 3 reports the results, with Panel A reporting results estimated on the full LBD sample and Panel B reporting results on the LEHD sample. In column 1, we include year fixed effects. There is a strong and positive relationship between 40-trading-day Nasdaq returns and whether the IPO was successfully completed. A decline of 1 standard deviation in Nasdaq returns translates into a 4.5% decline in the probability of a successful IPO. In our sample of IPO filing firms, 76.6% of firms complete their IPOs. Moreover, in Panel A (B), the *F*-statistic of 54.6 (40.9) exceeds the conventional threshold of F = 10 and suggests that the instrument is strong and

TABLE 3

IV Regressions: First-Stage

Table 3 reports first-stage results of the IV regressions. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The unit of observation is a complete firm. The dependent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. The IV, Nasdaq Return 40 Trading Days After, is the Nasdaq return in the 40-trading-day window following the IPO filing. The control variables include the log of firm employment and average firm wages in the year of the IPO filing, firm age, Nasdaq return in the 60-trading-day window prior to the IPO filing, and an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount. Average firm wages are calculated as the natural logarithm of the ratio of firm payroll normalized by its employment in the year of the IPO filing, calculated using LBD data for all 50 U.S. states and the District of Columbia. The parameter estimates for the control variables are not reported due to U.S. Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations, and estimates are rounded. Standard errors are reported in parentheses and are clustered by 2-digit SIC code in Panel A and by year-quarter and 2-digit SIC code in Panel B. *** denote significance at the 1% level.

	Dependent Variable: IPO			
	1	2	3	4
Panel A. LBD Sample (3,400 observations)				
Nasdaq Return 40 Trading Days After	0.448***	0.506***	0.447***	0.506***
	(0.061)	(0.065)	(0.059)	(0.064)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
No. of obs.	3,400	3,400	3,400	3,400
Adjusted R^2	0.112	0.128	0.117	0.153
F-statistic	54.6	60.5	58.0	63.5
Panel B. LEHD Sample (2,400 observations)				
Nasdaq return 40 Trading Days After	0.478***	0.525***	0.478***	0.525***
	(0.073)	(0.072)	(0.071)	(0.070)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
No. of obs.	2,400	2,400	2,400	2,400
Adjusted R^2	0.116	0.128	0.118	0.130
F-statistic	40.9	25.4	43.6	26.0

unlikely to be biased toward the OLS estimates (Bound, Jaeger, and Baker (1995), Staiger and Stock (1997)).

Column 2 adds additional controls for firm size (the natural logarithm of the count of total domestic employment in the year of the IPO filing), average firm wages (the natural logarithm of firm total annual payroll divided by total employment), firm age, 60-trading-day Nasdaq return prior to filing, an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount.[17] Column 3 includes (SIC 1-digit) industry and year fixed effects, and column 4 includes industry and year fixed effects as well as firm-level controls. The strong and positive relationship between 40-trading-day Nasdaq returns and whether the IPO was successfully completed is robust across all four specifications and both samples.

In order to be a valid instrument, the IV must also meet the exclusion restriction condition. To do so, we argue that 40-trading-day Nasdaq returns, measured in the window after the IPO filing, do not directly impact future firm characteristics except through the IPO completion channel. It is important to note that while Nasdaq returns may predict future growth opportunities, by including year fixed effects, we are controlling for macroeconomic trends. Our identification rests on the fact that returns measured only during a specific and small window immediately following the IPO filing predict IPO success. It is unlikely that returns during this small window will directly predict growth opportunities except through the channel of reflecting broader macroeconomic trends, which are controlled for in our analysis with year fixed effects. The identification comes from comparing firms that file in the same year. Some firms have a bad draw in terms of short-term market returns, while other firms do not.

For our IV approach to be valid, we must assume that the set of firms whose IPO decisions are sensitive to post-filing stock market returns are similar in all respects except that some firms were exposed to a positive shock and some were exposed to a negative shock. While it is not possible to document this unequivocally, we show additional evidence consistent with this argument in Section VI A. In this section, we show additional robustness tests, including reduced form regressions that regress the IPO outcomes directly on the IV and on placebos using returns before and after the book building phase.

B. Causal Effects of an IPO on Firm Labor Characteristics

Having validated our instrument, we now proceed with the second stage of our IV estimate. We run the following baseline regression:

$$Y_i = \beta_2 \widehat{IPO}_i + X_i' \delta_i + \mu_t + \vartheta_k + \varepsilon_i, \tag{2}$$

where Y_i measures the outcome variable of interest. $\widehat{IPO}_i$ is estimated in the first stage (equation (1)), X_i' is a vector of control variables, μ_t are year fixed effects, ϑ_k are industry fixed effects, and ε_i is the error term. Controls are included for firm size (the natural logarithm of the count of total domestic employment in the year

[17]Due to U.S. Census restrictions on the number of reported estimates, coefficient estimates on control variables are not reported.

TABLE 4

Impact of Successful IPOs on Firm Employment, Scale, Diversification, and Wages

Table 4 reports second-stage results of IV regressions and shows how a successful IPO affects a firm's ex post characteristics. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The unit of observation is a firm-level aggregation of establishment-level LBD data for all 50 U.S. states and the District of Columbia. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. IPO is instrumented with the Nasdaq return in the 40-trading-day window following the initial IPO filing. All dependent variables are measured over 3 years, starting the year of the IPO. These variables are then transformed into an annualized number. In column 1, the dependent variable is the annualized growth in employment. In column 2, the dependent variable is the annualized growth in the number of establishments. In column 3, the dependent variable is the annualized growth in the number of states where a given firm has a physical presence. In column 4, the dependent variable is the annualized growth in industrial concentration. In column 5, the dependent variable is the annualized growth in average firm wages. The control variables include the log of firm employment and average firm wages in the year of the IPO filing, firm age, Nasdaq return in the 60-trading-day window prior to the IPO filing, and an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount. The parameter estimates for the control variables are not reported due to the U.S. Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations, and estimates are rounded. Standard errors are reported in parentheses and are clustered by 2-digit SIC code. ** denote significance at the 5% level.

	Dependent Variable				
	Annualized Growth in Employment$_{t,t+3}$	Annualized Growth in # of Establishments$_{t,t+3}$	Annualized Growth in # of States$_{t,t+3}$	Annualized Growth in Industrial Concentration$_{t,t+3}$	Annualized Growth in Average Firm Wages$_{t,t+3}$
	1	2	3	4	5
IPO	0.199**	0.095	0.036	−0.044**	0.023
	(0.081)	(0.084)	(0.059)	(0.020)	(0.074)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
No. of obs.	3,400	3,400	3,400	3,400	3,400
F-statistic	63.5	63.5	63.5	63.5	63.5

of the IPO filing), average wages (the natural logarithm of annualized firm average wages), firm age, 60-trading-day Nasdaq return prior to filing, an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount, as well as year and industry fixed effects. Observations are measured at the firm level, and standard errors are clustered at the 2-digit SIC code level when using LBD data (available at the annual level) and double clustered at the 2-digit SIC code level and by year-quarter when using LEHD data (available at the year-quarter level).

In Table 4, we report our second stage IV results, which show the causal impact of IPO completion on the growth in firm employment, number of establishments, geographic and industrial diversification, and average wages. In column 1, we show that a successful IPO completion leads to a positive and statistically significant increase in employment. Following a successful IPO, firms increase employment annually by nearly 20% over the next 3 years (or 60% over the next 3 years), compared to firms with a withdrawn IPO. This result is consistent with Borisov et al. (2021), which finds 37% higher employment in the year after an IPO. Increased employment growth post-IPO is consistent with both the financial constraints and the agency mechanisms.

Looking at the change in employment, we find a larger treatment effect using our IV specification as compared to the OLS specification, which is reported in Supplementary Material Table IA1. This is consistent with the fact that our IV is providing a local average treatment effect (LATE), compared to the OLS, which is

providing an average treatment effect (ATE). Our IV estimates are of the effect of treatment on firms whose IPO completion is sensitive to Nasdaq returns. These firms are likely to be more responsive to the impact of the IPO completion compared to the average firm, as they are likely to have fewer alternatives to financing.

Since firms tend to be active acquirers immediately after their IPOs (Brau and Fawcett (2006), Celikyurt et al. (2010), Cornaggia et al. (2021)), it is not clear that going public creates new jobs organically. To shed new light on the debate about whether IPOs create jobs organically or just add jobs through M&As, we examine if this increased employment growth is driven purely through the additions of new establishments that could be added through M&As. In column 2, we measure growth in the number of establishments and document a positive but statistically insignificant point estimate, using our IV approach, suggesting that firms grow at least some employment organically post-IPO.

In Section II, we discuss the way in which financing constraints and agency channels disagree on how the employment would grow: the financial constraints channel predicts geographic expansion, while agency theories predict expansion into new industries and increased industrial diversification. To start to distinguish between these channels, in column 3, we show an insignificant increase in the geographic footprint following an IPO, which is inconsistent with the financial constraints channel.[18] Alternatively, column 4 shows that a completed IPO leads to a statistically significant and economically meaningful decrease in industrial concentration or, equivalently, an increase in industrial diversification, supporting the agency channel. On average, firms with successful IPOs increase industrial diversification by 4.4% each year, as measured over the 3-year post-IPO window, compared to firms that later withdrew their IPOs. This difference is economically important relative to the ex ante mean level of industrial diversification. On average, before the IPO filing, firms in our sample are highly concentrated, with only 7% of their employment in industries outside of their main 4-digit SIC code.

Finally, in column 5 of Table 4, we consider changes in average firm wages. By using average firm wages, we are able to observe a given firm's entire domestic workforce. However, we are unable to separate wage changes due to changes in the population of employees from changes in wages for a stable set of employees, which we can do in our later tests using the LEHD. Using this measure, we document a modest but statistically insignificant increase in wages following an IPO.

We next turn to testing how wage growth changes following the IPO. In Section II, we hypothesize that the financial constraints channel predicts positive wage growth of pre-IPO workers and positive new hire wage premium following the IPO, while the agency channel does not offer a clear prediction on wage growth. To document these wage dynamics, we turn to regressions using the LEHD sample. With the LEHD data, we can observe employer–employee data across time,

[18]Our insignificant findings differ from Cornaggia et al. (2021) who document a statistically significant geographic expansion of U.S. firms following an IPO completion. Cornaggia et al. (2021) show that successful IPO filing firms add relatively fewer establishments in their home county, compared to establishments outside of their home county, looking at a sample of the largest IPO filing firms.

TABLE 5

Impact of Successful IPOs on Wage Growth

Table 5 reports second-stage results of IV regressions and shows how a successful IPO affects growth in wages for different types of employees. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The unit of observation is a firm-level aggregation of workers available in our 31 LEHD states. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. IPO is instrumented with the Nasdaq return in the 40-trading-day window following the initial IPO filing. In columns 1–3, the dependent variable is the 3-year growth in employee wages. In column 1, the wage growth calculation includes all workers observed at the firm 1 quarter before the IPO filing. In column 2, the wage growth calculation includes all workers observed at the firm 1 quarter before the IPO filing who remain at the firm 3 years later. In column 3, the wage growth calculation includes all workers observed at the firm 1 quarter before the IPO filing who are no longer at the firm 3 years later. In column 4, the dependent variable is the new hire wage premium, defined as the difference in log wages between the first full quarter wage at the IPO filing firm and the last full quarter of wages at the previous employer. The control variables include the log of firm employment and average firm wages in the year of the IPO filing, firm age, Nasdaq return in the 60-trading-day window prior to the IPO filing, and an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount. The parameter estimates for the control variables are not reported due to the U.S. Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations, and estimates are rounded. Standard errors are reported in parentheses and are clustered by year-quarter and 2-digit SIC code. *** denote significance at the 1% level.

	Dependent Variable			
	3-Year Growth in Employee Wages$_{t,t+3}$			New Hire Wage Premium
	Type of workers			
	Pre-IPO	Pre-IPO & Stay at $t+3$	Pre-IPO & Leave by $t+3$	Post-IPO Hires
	1	2	3	4
IPO	−0.187	−0.184	−0.204	0.078***
	(0.273)	(0.238)	(0.263)	(0.027)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	2,400	2,400	2,400	2,400
F-statistic	26.0	26.0	26.0	26.0

allowing us to estimate wage changes while holding the composition of workers fixed. We aggregate worker-level results to the firm level and report these results in Table 5, using the baseline IV specification.[19]

In column 1, we report the effect of IPO completion on the 3-year wage change, using only those workers observed at the IPO filing firm in the quarter immediately preceding the filing date. Surprisingly, we report a negative and statistically insignificant earnings change, using our IV. At risk of over interpreting insignificant results, these results could suggest a transition of compensation from wages to stock options post-IPO for existing workers. As discussed in Section III, we can observe only taxable compensation and will underreport compensation in the form of unexercised stock options.

In the above test, we include all workers observed in the data in the post-period, regardless of whether they remain employed at the original IPO filing firm. To better identify whether these results are driven by workers who remain employed at the firm or workers who leave, we consider subsamples in columns 2 and 3. In column 2, we limit the sample to those workers observed at the firm in the pre-IPO filing period and at the firm in the 3-year ex post period. In column 3, we limit the sample to those workers at the firm in the pre-IPO filing period but who leave the firm over the next 3 years. Interestingly, we find negative but insignificant coefficient estimates on IPO completion in both samples: while, as

[19]OLS version of this specification is available in Supplementary Material Table IA2.

expected, the coefficient for the stayers is less negative than for leavers, the negative and insignificant growth for the stayers is surprising. One caveat about these null results in the IV setting is that IV methods generally have low power and large variances of estimates. With that caveat in mind, and the possibility that post-IPO workers receive greater stock-based compensation that we do not observe, it is worth noting that, in general, the lack of a significant effect of the IPO on earnings is difficult to reconcile with the existence of large financial constraints being present at the IPO filing firm. If firms were sufficiently constrained pre-IPO, then they would be expected to pay low wages before the IPO but then offer higher pay growth following a cash infusion through a successful IPO, as in the model of firm financial constraints and wages in Michelacci and Quadrini (2009). We do not observe such a pattern in the data.

Finally, in column 4, we consider an alternative measure of wage changes for the workers hired post-IPO: the new hire wage premium. We estimate the new hire wage premium as the difference in the logarithm of the first full quarter earnings at the IPO filing firm, compared to the logarithm of the terminal full quarter earnings at the employee's previous firm. This is a measure of whether IPOs cause firms to pay higher wages to attract new employees, which are proxied by employees' wages in the previous place of employment. We show that the new hire wage premium increases by nearly 7.8% following a successful IPO. This result is consistent with multiple interpretations. Due to increased capital and greater agency frictions post-IPO, firms may be less incentivized to minimize new hire wages in pursuit of managerial empire building. Alternatively, this could reflect an upward-sloping labor supply curve. Firms, which have valuable growth opportunities that can be competed away and which are expanding rapidly, may face limits in the number of employees interested in working at their firm and be required to raise wages to fill all open vacancies. Overall, while a bit mixed, the wage results are not supportive of the financial constraints channel, and they are not at odds with the agency channel.

We next examine turnover to top-five roles at startups, our proxy for departures of entrepreneurial-minded employees. In Section II, the financial constraints and career concerns channels disagree in their predictions about the turnover of talent to startups. The financial constraints channel predicts lower turnover to startups, while the career concerns channel predicts higher turnover to startups post-IPO. In Table 6, we report results from a two-stage least squares (2SLS) regression of an IPO on the departure rate to startups.[20] Column 1 reports a positive and statistically significant coefficient on instrumented IPO completion, indicating a causal relationship between IPO completion and employee departures to key positions at startups. Our estimates show not only a statistically significant and causal relationship between IPO completion and employee departures to startups but also an economically significant relationship. On average, for our sample of firms, 2.3% of pre-IPO employees depart over 3 years to take top-five roles at startups. For firms that complete their IPOs, this jumps by 5.4 percentage points, implying that the average rate of worker exits to startups

[20]OLS version of this specification is available in Supplementary Material Table IA3.

TABLE 6

Impact of Successful IPOs on Employee Departures to Entrepreneurship

Table 6 reports second-stage results of IV regressions and shows how a successful IPO affects departure rates to entrepreneurship for different types of employees. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The unit of observation is a firm-level aggregation of workers available in our 31 LEHD states. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. IPO is instrumented with the Nasdaq return in the 40-trading-day window following the initial IPO filing. The dependent variable is the fraction of entrepreneurial departures, defined as the fraction of workers 1 quarter before IPO filing who are observed 3 years later at a firm no more than 3 years old and who are among the top 5 earners at that firm. In columns 1 and 6, the dependent variable includes all workers at the firm 1 quarter before the IPO filing. In columns 2 and 3, the dependent variable includes workers at the firm 1 quarter before the IPO filing whose wage is above (equals or is below) the median worker wage. In column 4 and 5, the dependent variable includes workers at the firm 1 quarter before the IPO filing whose age is above (equals or is below) the median worker age. In column 6, the IPO indicator and the interaction of IPO and High-tech firm indicators are instrumented in the first stage with: i) the Nasdaq return in the 40-trading-day window following the initial IPO filing and ii) the interaction of High-tech indicator with the Nasdaq return. High-tech is 1 for firms in the computer, bio-tech, or electronics sectors, and 0 otherwise. The control variables include the log of firm employment and average firm wages in the year of the IPO filing, firm age, Nasdaq return in the 60-trading-day window prior to the IPO filing, an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount. The parameter estimates for the control variables are not reported due to the U.S. Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Standard errors are reported in parentheses and are clustered by year-quarter and 2-digit SIC code. ** and *** denote significance at the 5% and 1% levels, respectively.

	Dependent Variable: Departure Rate to Entrepreneurship$_{t,t+3}$					
	Type of Workers					
	All	High Wage	Low Wage	High Age	Low Age	All
	1	2	3	4	5	6
IPO	0.054**	0.090***	0.033	0.016	0.088***	0.002
	(0.012)	(0.008)	(0.023)	(0.020)	(0.019)	(0.012)
IPO X High-tech						0.093***
						(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	2,400	2,400	2,400	2,400	2,400	2,400
F-statistic	26.0	26.0	26.0	26.0	26.0	13.0

doubles for a marginal firm that completes an IPO filing. These results support the career concerns channel.[21]

Moreover, this increase in employee departures to startups following an IPO is concentrated among high-wage workers, that is, those workers who are most likely to be working in developing, managing, and commercializing new projects. This is shown in columns 2 and 3, where we estimate the departure rate to entrepreneurship using only those workers in the top and bottom halves of the firm's wage distribution, respectively. Again, these results support the career concerns channel and are inconsistent with the financial constraints channel.

Given that Acemoglu, Akcigit, and Celik (2014) and Liang et al. (2018) find that younger workers are associated with more experimentation and more creative innovation and have greater career concerns, age may also be important. In columns 4 and 5, we estimate the departure rate to entrepreneurship using only those workers in the top (bottom) half of the firm's age distribution. The treatment effect is concentrated among younger workers, consistent with the career concerns channel, as discussed in Section II. Finally, in column 6, we explore the differential treatment

[21] These findings of increased employee turnover following the IPO are unique to workers leaving for startups. We find no change in employee departures to established firms following the IPO when using our IV approach.

TABLE 7

Cross-Sectional Variance Following IPO Filing Analysis: Firm Size

Table 7 reports second-stage results of IV regressions and shows that post IPO-filing growth in a firm's employment, industrial concentration, new hire wages, and employee departure rate to entrepreneurship for two broad samples of firms: i) all firms in the LBD (columns 1 and 2) or LEHD (columns 3 and 4) samples and ii) firms that are above median in the employment size distribution of IPO filing firms. The sample includes U.S. firms that filed for an IPO from 1992 through the first quarter of 2006. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. Large Firm is a dummy variable equal to 1 if a firm is above the median employment size of the sample of IPO filing firms. The IPO indicator and the interaction of IPO and Large firm indicators are instrumented in the first stage with: i) the Nasdaq return in the 40-trading-day window following the initial IPO filing and ii) the interaction of the Large Firm dummy variable with the Nasdaq return. All dependent variables are measured over 3 years, starting the year of the IPO. The control variables include the log of firm employment and average firm wages in the year of the IPO filing, firm age, Nasdaq return in the 60-trading -day window prior to the IPO filing, and an indicator for whether a firm is VC-backed, syndicate size, underwriter reputation, and log filing amount, as well as the Large Firm dummy. Standard errors are clustered by 2-digit SIC code in columns 1 and 2 and by year-quarter and 2-digit SIC code in columns 3 and 4. *,**, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
	Annualized Growth in Employment$_{t,t+3}$	Annualized Growth in Industrial Concentration$_{t,t+3}$	New Hire Wage Premium	Departure Rate to Entrepreneurship$_{t,t+3}$
	1	2	3	4
IPO	0.340***	−0.037*	0.069***	0.056**
	(0.090)	(0.022)	(0.025)	(0.025)
IPO X Large Firm	−0.225***	−0.011	0.015	−0.004
	(0.070)	(0.009)	(0.018)	(0.018)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
No. of obs.	3,400	3,400	2,400	2,400
F-statistic	33.2	33.2	12.7	12.7

effect at high-tech firms. High-tech firms tend to develop high-risk, high-growth ideas that are associated with experimentation and are more likely to be cut due to short-term focus (Manso (2016)). As predicted by the career concerns channel, we document a significantly and economically larger treatment effect in these especially dynamic industries. This result is also inconsistent with the financial constraints channel, since high-tech industries are more likely to have promising growth options that the IPO proceeds would help to fund.

Finally, as one further test of the financial constraints mechanism, we divide our sample of firms by those with total employment above the median of the distribution of total employment of our sample of IPO firms at the time they file for their IPOs. We estimate regressions with interactions between the IPO dummy and a large firm dummy, which equals 1 if a firm is above the median total employment size, and 0 otherwise, for each of the four outcome variables where we observed significant changes post-IPO: employment growth, change in industrial concentration, new hire wage premium, and departure rate to entrepreneurship. The estimated coefficients are reported in Table 7. We find that the statistically significant employment growth following an IPO is driven by the smaller firms in our sample, those below the median size, which supports a financial constraints hypothesis. On the other hand, we find statistically similar changes in industrial diversification following IPO completion between small and large firms. This result is not consistent with an argument that financial constraints drive the increase in industrial diversification following IPO

completion but instead suggests that changes in firm strategy and agency theories may play a role. Likewise, we find no difference in the new hire wage premium or the departure rate of employees to startups in our sample of large and small firms, which suggests that these results are not driven by financially constrained smaller firms.

V. Discussion

We next summarize the implications of our findings before discussing additional robustness tests in the next section. Our results provide new insights into the real consequences of a firm's transition from private to public ownership, an especially important topic given the active debate about the recently documented decline of public firms and IPOs in the U.S. (Doidge et al. (2017), Kahle and Stulz (2017)).

Our results show that a successful IPO leads to employment growth. We also show that the employment growth caused by the IPOs documented in Borisov et al. (2021) is likely not entirely driven by M&As, suggesting that some jobs are created organically. These results are consistent with the existence of ex ante financial constraints that limited the IPO filing firm's ability to hire workers before the capital infusion. This argument is further strengthened by the fact that the effect is concentrated in small firms. However, the absence of significant wage growth post-IPO and the increase in employee turnover to startups is not directly consistent with a financial constraints mechanism.

Instead, the increase in departures to startups post-IPO is most consistent with a career concerns mechanism. Vibrant public markets have always been thought to be fundamentally important for encouraging entrepreneurship through the reallocation of high-skill workers from IPO-bound to new firms (Michelacci and Suarez (2004)). We provide the first non-patent empirical evidence of this importance by using data on IPO-filing firms, new firms, and their labor forces.

Finally, our results speak to an important question about how going public affects workers. From a worker's perspective, our results are generally positive. We show that IPOs create jobs. We also show significant wage increases for workers hired by the newly public firms. However, we do not find evidence of wage increases for existing workers who stay at the firm.

VI. Robustness

In this section, we address several key robustness tests. First, we conduct several more robustness tests related to our IV estimation approach. Second, we show that our results are economically unchanged if we drop all controls. Third, we show directionally consistent results using a difference-in-differences setting that uses a smaller sample of firms for which we have both pre- and post-IPO data. Fourth, we validate the generalizability of the LEHD sample. With the LEHD sample, we can track individual employees over time, which allows us to estimate more precise wage dynamics as well as measures of employee turnover. However, these data are available for only 31 states. We show that results, which can be

replicated for all 50 states appear similar when estimated just using these 31 states in our LEHD sample.

A. Further Validating our Baseline Specification

To further investigate the validity of our IV, we conduct a series of placebo tests in which we test whether 40-trading-day Nasdaq returns in the 10 months both before and after firms' IPO filings, but not directly following the filings, can predict the outcome variables we found to be significantly affected by IPO completion in our IV estimations. A potential vulnerability of the IV estimation strategy is that post-filing market returns might directly predict firms' post-IPO outcomes, for instance, through changes in investment opportunities. If our main outcome variables of interest can be shown to have an insignificant correlation with stock market returns in other similar pre- and post-IPO filing periods, but windows that do not overlap with the book building period, we can further discredit this potential vulnerability.

Specifically, we estimate OLS regressions of the following form using 40-trading-day Nasdaq returns during the 10 months both before and after firms' IPO filings. Detailed estimation results are reported in Section IA-I and in Figure IA1 of the Supplementary Material. Consistent with the baseline IV results, employment growth, the change in industrial concentration, the new hire wage premium, and employee departures to entrepreneurship are significantly correlated at the 5% level with Nasdaq returns measured over the 40 trading days following the IPO filing. Given that each graph in Figure IA1 details results from 16 unique regressions, 64 regressions in total, it is not surprising that we do find a statistically significant correlation with returns measured outside of the book building window in three instances, including one instance in which the sign of the correlation with the outcome variable is the same as during the 40 trading days following the IPO. The overall pattern, across all four outcome variables, is one where 40-trading-day returns outside of the book building period do not significantly impact our outcome variables of interest, supporting the validity of our chosen instrument.

In our baseline IV specification, we do not control for the pre-IPO filing characteristics of the firms beyond those in the year of IPO filing.[22] An alternative approach would be to use a difference-in-differences estimation approach in which we examine the change in our main outcome variables in the 3 years prior to the IPO filing, relative to the 3 years following the IPO filing. While this approach allows us to include additional controls (pre-IPO characteristics), it comes at a cost to sample size. We observe the required pre-IPO filing data for only one half of our sample. Nonetheless, in Table IA5, we estimate IV regressions in which we use pre- and post-IPO filing data. We use the 40-trading-day Nasdaq return following the IPO filing and its interaction with the "After" period dummy, which equals 1 in the period following the IPO filing, and 0 otherwise, to instrument for the IPO dummy and its interaction with the after dummy. Note that using the IV is still important even in a difference-in-differences framework, since such a setup does not eliminate the possible endogeneity of certain types of firms being more likely to complete

[22]Estimates without controls are similar in terms of economic magnitudes and are reported in Supplementary Material Table IA4.

their IPOs. The results are broadly consistent with those in the larger estimation sample, albeit with less statistical significance.

B. Validating the LEHD Sample

We next explore the robustness of our results to possible sample selection concerns related to the requirement that firms must be in the LEHD for our analysis of workers' wages and job changes. As a first step in validating the LEHD sample, we report summary statistics in Table IA6 for firms in the 31 LEHD states. Specifically, we report firm characteristics observed in the LBD, and hence available for all states, for the set of all firms, in column 1, and for the subset of firms that also have at least some employees in one of our LEHD states, in column 2.

Not surprisingly, firms that are observed, at least partially, in at least 1 of the 31 LEHD states tend to be larger in terms of total employment, number of establishments, and physical presence across states, on average. These firms are also more likely to have their headquarters in 1 of the 31 LEHD states. They also tend to be older. However, these firms are otherwise economically similar. For example, 51% of firms in the LEHD sample are VC-backed, compared to 49% in the full sample. Moreover, 49% of firms in the LEHD sample are high-tech, compared to 50% in the full sample. Firms in the full sample, which start with lower ex ante employment, grow employment modestly faster compared to firms in the LEHD sample. However, both sets of firms realize identical growth in average wages and industrial concentration to the third significant digit. Likewise, firms in both groups experience similar growth in the number of establishments and states with a physical presence.

To provide further evidence of no systematic bias in the LEHD sample, in Table IA7, we repeat the baseline regressions presented using LBD data, data available for all 50 states, but using only the subset of firms with employment in at least 1 of our LEHD states. In Panel A, we reproduce the baseline results using the full sample for ease of comparison. In Panel B, we repeat the same specifications but include only the set of firms with employment in our 31 LEHD states.

Overall, the coefficients are generally similar when we use the full sample (Panel A) or when we use only those firms with at least some employment in our 31 LEHD states (Panel B). With the full sample, we find a positive and statistically significant relationship between employment growth and IPO completion. With the LEHD sample, we also document a positive and statistically significant relationship between employment growth and IPO completion. Moreover, the two coefficient estimates are similar in economic magnitude. Likewise, we report negative and statistically significant coefficients of similar magnitude when measuring the causal impact of IPO completion on changes in industrial concentration using either the full or LEHD-state sample. We find no significant relation between IPO completion and the growth in the number of states or average wages in either sample. The one difference is we now report a significant increase in the number of establishments post-IPO in the LEHD sample; however, this result is significant at only the 10% level. In sum, these results further support our argument that there is no systematic bias in our 31 LEHD states.

We next consider a related but distinct point regarding the distribution of headquarters. While we have documented similar results for firms with any employment in our 31 LEHD states relative to the full sample, there may still be a concern that results for firms with headquarters outside our 31 LEHD states may be different. In Table IA8, we present summary statistics for firms in the LEHD sample with headquarters in LEHD states and for firms in the LEHD sample whose headquarters are in non-LEHD states. Over half of our sample of LEHD firms have headquarters located in LEHD states. Eighty-eight percent of employees and payroll are located in LEHD states for firms whose headquarters are also in LEHD states. By contrast, around 16% of employees and payroll are located in LEHD states for firms whose headquarters are not located in an LEHD state. The wages of employees at firms headquartered in LEHD states are higher on average than for firms headquartered outside those LEHD states, likely reflecting the fact that the highest-paid executives are in the sample for firms whose headquarters are included in the sample. We also show that the departures of workers to entrepreneurship are similar across samples.

It is possible that we may be missing some effects of IPOs for firms whose headquarters, and much of their employment, are not in our LEHD sample. As such, we examine whether our main results differ for firms whose headquarters are and are not located in LEHD states. In Supplementary Material Tables IA9 and IA10, we present estimates of our main IV regressions differentiating the effect of the IPO by firms whose headquarters are not based in LEHD states. We find that, in all but one case, our results are not significantly different when we break out the effects by firms whose headquarters are not in LEHD states. The one exception we find is a significantly greater decline in wages among pre-IPO employees who remain at the firm post-IPO at firms with headquarters outside our LEHD states. This suggests that employees outside of the headquarters, employees less likely to be executives, are relatively more likely to experience wage declines following an IPO. Otherwise, we find similar results regardless of whether the firms' headquarters are or are not included in our sample.

VII. Conclusion

In this paper, we focus on the role of the IPO market and the firm's choice to go public on the firm's labor force. Using micro data from the U.S. Census, we document a number of novel facts regarding ex ante characteristics of IPO filing firms as well as changes in employment, wages, and turnover following the event. Overall, our findings suggest that going public has significant implications for the firm's labor force, the firm itself, and labor reallocation across firms.

With these results, we add to the debate regarding the key drivers of change following the transition to public ownership. Our results point to important changes to incentives. With a reduction in ownership concentration, agency conflicts and distortions in investment due to career concerns may increase. Consistent with this argument, we observe that a successful IPO leads to an increase in firms' industrial diversification through employment growth in non-core industries. We also document an increase in the new hire wage premium. Moreover, as argued in Ferreira et al. (2014), we find that incentives to experiment decline, as evidenced by the increased turnover of employees to new firms following an IPO. Vibrant IPO markets have always been thought to be fundamentally important for encouraging

entrepreneurship through the reallocation of high-skill workers from IPO-bound to new firms. We provide the first direct empirical evidence of this importance. This churn of experimentation-minded employees might make firms stay private longer when they have better access to private markets.

We find more mixed evidence that the IPO resolves significant financial constraints at the IPO filing firm. We show a large ex post increase in employment, especially at smaller firms, which could be consistent with the resolution of financial constraints. However, we also document high pre-IPO mean wages and no evidence of higher rates of wage increases after the influx of capital with the IPO.

While we provide new evidence on how IPOs affect labor markets, there is still much we do not know. For example, given our surprising null effect on wage growth of pre-IPO workers, more work is needed to understand how going public affects workers and which workers benefit or suffer as a result of an IPO. Is the change good or bad for the firm? If bad, can this explain why firms may delay IPOs or prefer M&As exits instead? Moreover, it would be interesting to dig into whether the long-run IPO underperformance (Ritter (1991)) is related to the deterioration of human capital post-IPO. Finally, an important question is whether going from public to private reverses the patterns.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109024000772.

Funding statement

This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the National Science Foundation Grants SES-9978093, SES-0339191, and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation.

References

Abowd, J.; B. Stevens; L. Vilhuber; F. Andersson; K. McKinney; M. Roemer; and S. Woodock. "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators." In *Producer Dynamics: New Evidence from Micro Data*, T. Dunne, J. B. Jensen, and M. J. Roberts, eds. Chicago, IL: University of Chicago Press (2009).

Acemoglu, D.; U. Akcigit; and M. A. Celik. "Young, Restless and Creative: Openness to Disruption and Creative Innovations." NBER Working Paper No. 19894 (2014).

Acharya, V., and Z. Xu. "Financial Dependence and Innovation: The Case of Public Versus Private Firms." *Journal of Financial Economics*, 124 (2017), 223–243.

Amihud, Y., and B. Lev. "Risk Reduction as a Managerial Motive for Conglomerate Mergers." *Bell Journal of Economics*, 12 (1981), 605–617.

Arikan, A. M., and R. M. Stulz. "Corporate Acquisitions, Diversification, and the Firm's Life Cycle." *Journal of Finance*, 71 (2016), 139–194.

Asker, J.; J. Farre-Mensa; and A. Ljungqvist. "Corporate Investment and Stock Market Listing: A Puzzle?" *Review of Financial Studies*, 28 (2015), 342–390.

Atanassov, J.; V. K. Nanda; and A. Seru. "Finance and Innovation: The Case of Publicly Traded Firms." Ross School of Business Working Paper No. 970 (2007).

Azoulay, P.; B. F. Jones; J. D. Kim; and J. Miranda. "Age and High-Growth Entrepreneurship." *American Economic Review: Insights*, 2 (2020), 65–82.

Babina, T. "Destructive Creation at Work: How Financial Distress Spurs Entrepreneurship." *Review of Financial Studies*, 33 (2020), 4061–4101.
Babina, T.; W. Ma; C. Moser; P. Ouimet; and R. Zarutskie. "Pay, Employment, and Dynamics of Young Firms." Kenan Institute of Private Enterprise Research Paper No. 19–25 (2019).
Benveniste, L. M.; A. Ljungqvist; W. J. Wilhelm Jr; and X. Yu. "Evidence of Information Spillovers in the Production of Investment Banking Services." *Journal of Finance*, 58 (2003), 577–608.
Bernstein, S. "Does Going Public Affect Innovation?" *Journal of Finance*, 70 (2015), 1365–1403.
Borisov, A.; A. Ellul; and M. Sevilir. "Access to Public Capital Markets and Employment Growth." *Journal of Financial Economics*, 141 (2021), 896–918.
Bound, J.; D. A. Jaeger; and R. M. Baker. "Problems with Instrumental Variables Estimation when the Correlation Between the Instruments and the Endogenous Explanatory Variable Is Weak." *Journal of the American Statistical Association*, 90 (1995), 443–450.
Brau, J. C., and S. E. Fawcett. "Initial Public Offerings: An Analysis of Theory and Practice." *Journal of Finance*, 61 (2006), 399–436.
Busaba, W. Y.; L. M. Benveniste; and R.-J. Guo. "The Option to Withdraw IPOs During the Premarket: Empirical Analysis." *Journal of Financial Economics*, 60 (2001), 73–102.
Butler, A.; L. Fauver; and I. Spyridopoulos. "Local Economic Spillover Effects of Stock Market Listings." *Journal of Financial and Quantitative Analysis*, 54 (2019), 1025–1050.
Carter, R., and S. Manaster. "Initial Public Offerings and Underwriter Reputation." *Journal of Finance*, 45 (1990), 1045–1067.
Carter, R. B.; F. H. Dark; and A. K. Singh. "Underwriter Reputation, Initial Returns, and the Long-Run Performance of IPO Stocks." *Journal of Finance*, 53 (1998), 285–311.
Celikyurt, U.; M. Sevilir; and A. Shivdasani. "Going Public to Acquire? The Acquisition Motive in IPOs." *Journal of Financial Economics*, 96 (2010), 345–363.
Chemmanur, T. J.; S. He; and D. K. Nandy. "The Going-Public Decision and the Product Market." *Review of Financial Studies*, 23 (2010), 1855–1908.
Chemmanur, T. J., and I. Paeglis. "Management Quality, Certification, and Initial Public Offerings." *Journal of Financial Economics*, 76 (2005), 331–368.
Cong, L. W., and S. T. Howell. "Policy Uncertainty and Innovation: Evidence from Initial Public Offering Interventions in China." *Management Science*, 67 (2021), 7238–7261.
Cornaggia, J.; M. Gustafson; J. D. Kotter; and K. Pisciotta. "Does Being Private Constrain Geographic Expansion?" SSRN Working Paper No. 3756590 (2021).
Cornaggia, J.; M. Gustafson; J. D. Kotter; and K. Pisciotta. "Initial Public Offerings and the Local Economy." *Review of Finance*, 28 (2024), 1245–1273.
Denis, D. J.; D. K. Denis; and A. Sarin. "Agency Problems, Equity Ownership, and Corporate Diversification." *Journal of Finance*, 52 (1997), 135–160.
Doidge, C.; G. A. Karolyi; and R. M. Stulz. "The US Listing Gap." *Journal of Financial Economics*, 123 (2017), 464–487.
Dunbar, C. G., and S. R. Foerster. "Second Time Lucky? Withdrawn IPOs that Return to the Market." *Journal of Financial Economics*, 87 (2008), 610–635.
Edelen, R. M., and G. B. Kadlec. "Issuer Surplus and the Partial Adjustment of IPO Prices to Public Information." *Journal of Financial Economics*, 77 (2005), 347–373.
Ewens, M.; R. Nanda; and C. T. Stanton. "The Evolution of CEO Compensation in Venture Capital Backed Startups." NBER Working Paper No. 27296 (2020).
Feldman, N.; L. Kawano; E. Patel; N. Rao; M. Stevens; and J. Edgerton. "Investment Differences Between Public and Private Firms: Evidence from U.S. Tax Returns." *Journal of Public Economics*, 196 (2021), 104370.
Ferreira, D.; G. Manso; and A. C. Silva. "Incentives to Innovate and the Decision to Go Public or Private." *Review of Financial Studies*, 27 (2014), 256–300.
Gilje, E., and J. Taillard. "Do Private Firms Invest Differently than Public Firms? Taking Cues from the Natural Gas Industry." *Journal of Finance*, 71 (2016), 1733–1778.
Grossman, S. J., and O. D. Hart. "The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration." *Journal of Political Economy*, 94 (1986), 691–719.
Hadlock, C., and J. Pierce. "New Evidence on Measuring Financial Constraints: Moving Beyond the KZ Index." *Review of Financial Studies*, 23 (2010), 1909–1940.
Haltiwanger, J.; R. S. Jarmin; and J. Miranda. "Who Creates Jobs? Small Versus Large Versus Young." *Review of Economics and Statistics*, 95 (2013), 347–361.
Hellmann, T., and M. Puri. "Venture Capital and the Professionalization of Start-up Firms: Empirical Evidence." *Journal of Finance*, 57 (2002), 169–197.
Holmstrom, B. "Agency Costs and Innovation." *Journal of Economic Behavior & Organization*, 12 (1989), 305–327.
Jain, B. A., and O. Kini. "The Post-Issue Operating Performance of IPO Firms." *Journal of Finance*, 49 (1994), 1699–1726.

Jarmin, R. S., and J. Miranda. "The Longitudinal Business Database." Center for Economic Studies Working Paper No. CES-02-15 (2002).
Jensen, M. C. "Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers." *American Economic Review*, 76 (1986), 323–329.
Kahle, K. M., and R. M. Stulz. "Is the US Public Corporation in Trouble?" *Journal of Economic Perspectives*, 31 (2017), 67–88.
Kaplan, S. N.; B. A. Sensoy; and P. Strömberg. "Should Investors Bet on the Jockey or the Horse? Evidence from the Evolution of Firms from Early Business Plans to Public Companies." *Journal of Finance*, 64 (2009), 75–115.
Kenney, M.; D. Patton; and J. R. Ritter. "Post-IPO Employment and Revenue Growth for US IPOs, June 1996–2010." SSRN Working Paper No. 2063829 (2012).
Kerr, W. R.; R. Nanda; and M. Rhodes-Kropf. "Entrepreneurship as Experimentation." *Journal of Economic Perspectives*, 28 (2014), 25–48.
Liang, J.; H. Wang; and E. P. Lazear. "Demographics and Entrepreneurship." *Journal of Political Economy*, 126 (2018), S140–S196.
Loughran, T., and J. Ritter. "Why Has IPO Underpricing Changed Over Time?" *Financial Management*, 33 (2004), 5–37.
Loughran, T., and J. R. Ritter. "The New Issues Puzzle." *Journal of Finance*, 50 (1995), 23–51.
Maksimovic, V., and G. Phillips. "Do Conglomerate Firms Allocate Resources Inefficiently across Industries? Theory and Evidence." *Journal of Finance*, 57 (2002), 721–767.
Maksimovic, V.; G. Phillips; and L. Yang. "Private and Public Merger Waves." *Journal of Finance*, 68 (2013), 2177–2217.
Maksimovic, V.; G. Phillips; and L. Yang. "Do IPO Firms Become Myopic?" *Review of Finance*, 27 (2023), 765–807.
Manso, G. "Experimentation and the Returns to Entrepreneurship." *Review of Financial Studies*, 29 (2016), 2319–2340.
Megginson, W. L., and K. A. Weiss. "Venture Capitalist Certification in Initial Public Offerings." *Journal of Finance*, 46 (1991), 879–903.
Michelacci, C., and V. Quadrini. "Financial Markets and Wages." *Review of Economic Studies*, 76 (2009), 795–827.
Michelacci, C., and J. Suarez. "Business Creation and the Stock Market." *Review of Economic Studies*, 71 (2004), 459–481.
Mikkelson, W. H.; M. M. Partch; and K. Shah. "Ownership and Operating Performance of Companies that go Public." *Journal of Financial Economics*, 44 (1997), 281–307.
Mortal, S., and N. Reisel. "Capital Allocation by Public and Private Firms." *Journal of Financial and Quantitative Analysis*, 48 (2013), 77–103.
Ouimet, P., and R. Zarutskie. "Who Works for Startups? The Relation Between Firm Age, Employee Age and Growth." *Journal of Financial Economics*, 112 (2014), 386–407.
Pagano, M.; F. Panetta; and L. Zingales. "Why Do Companies Go Public? An Empirical Analysis." *Journal of Finance*, 53 (1998), 27–64.
Phillips, G., and G. Sertsios. "Financing Decisions and Product Introductions of Private and Publicly Traded Firms." NBER Working Paper No. 20578 (2014).
Puri, M., and R. Zarutskie. "On the Life Cycle Dynamics of Venture-Capital-and Non-Venture-Capital-Financed Firms." *Journal of Finance*, 67 (2012), 2247–2293.
Rajan, R. G. "Presidential Address: The Corporation in Finance." *Journal of Finance*, 67 (2012), 1173–1217.
Ritter, J. R. "The Long-Run Performance of Initial Public Offerings." *Journal of Finance*, 46 (1991), 3–27.
Sheen, A. "Do Public and Private Firms Behave Differently? An Examination of Investment in the Chemical Industry." *Journal of Financial and Quantitative Analysis*, 55 (2020), 2530–2554.
Staiger, D., and J. H. Stock. "Instrumental Variables Regression with Weak Instruments." *Econometrica*, 65 (1997), 557–586.
Stein, J. C. "Efficient Capital Markets, Inefficient Firms: A Model of Myopic Corporate Behavior." *Quarterly Journal of Economics*, 104 (1989), 655–669.
Terry, S.; T. M. Whited; and A. A. Zakolyukina. "Information Versus Investment." *Review of Financial Studies*, 26 (2023), 1148–1192.
Williamson, O. E. *The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm*. Englewood Cliffs, NJ: Prentice-Hall (1964).
Zingales, L. "In Search of New Foundations." *Journal of Finance*, 55 (2000), 1623–1653.