IPOs, Human Capital, and Labor Reallocation*

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ABSTRACT

How does access to public equity markets affect real outcomes? We examine the human capital of IPO-filing firms and how going public affects their labor force. 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 earnings growth of pre-IPO workers. Instead, post-IPO hires receive larger earnings increases upon joining. Overall, going public has significant implications for the workers, the firm, and labor reallocation across firms.

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Understanding how access to public equity markets affects real outcomes 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 are unclear. Theoretically, going public can resolve financial constraints, allowing the firm to increase employment and wages. These firms may also be able to to attract new talent and decrease departures of workers with valuable ideas (Rajan, 2012; Babina, Forthcoming). However, the transition to public ownership could also exacerbate agency problems, leading to empire-building, value-destroying diversification, short-term focus on safer projects and reduced experimentation. This may trigger more departures of creative and entrepreneurial-minded workers (Williamson, 1964; Jensen, 1986; Manso, 2016).

The empirical evidence so far is inconclusive on the real effects of IPOs. For example, patent-based metrics show mixed effects of IPOs on innovation (Atanassov, Nanda, and Seru, 2007; Bernstein, 2015; Acharya and Xu, 2017; Cong and Howell, 2018). Similarly, while employment at the firm going public increases (Kenney, Patton, and Ritter, 2012; Borisov, Ellul, and Sevilir, 2015), it is not clear that going public creates new jobs, 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 on the real effects of IPOs by creating a unique dataset 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 IPO filing. Using an instrumental variables (IV) approach, we then estimate the causal impact of the IPO on the firm's employment, wages as well as the inflows and outflows of human-capital following the IPO. These findings provide 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 mean annual earnings, measured a quarter before the IPO-filing, are over $82,000, compared

to the national average of $37,000, indicating that IPO-filing firms employ primarily high-skill workers. In contrast, following an IPO filing, the mean new hire earnings are only $57,000. Second, we document minimal industrial and geographic diversification of the business establishments and labor force of IPO-filing firms. Third, consistent with anecdotal evidence, we find that workers at firms that file for an IPO tend to be young, male and white. Fourth, on average, 2.3% of employees leave an IPO-filing firm in the three 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 (Forthcoming), 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 into being public (Maksimovic, Phillips, and Yang, 2020), 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 firms that succeed in going public to firms that withdrew their IPO. Furthermore, to control for unobservable differences between firms with successful and withdrawn IPOs, we use an IV approach from Bernstein (2015). Specificially, we use NASDAQ returns in the 60-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.

We show that overall firm employment increases following IPOs. Post-IPO firm employment increases by over 22% annually over each of the three years following the IPO filing, on average. These results are consistent with Borisov, Ellul, and Sevilir (2015), who document IPOs cause higher employment using NETs data. These results support a financial constraints mechanism but are also evidence of empire-building, given the decline in high-powered incentives following the transition to less-concentrated public ownership. To shed light on the debate about whether IPOs

[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, Benveniste, and Guo (2001), Benveniste, Ljungqvist, Wilhelm Jr, and Yu (2003), Edelen and Kadlec (2005), Dunbar and Foerster (2008), and Bernstein (2015).

create new jobs or just add jobs through M&As, we examine if this increased employment growth is driven purely through the additions of new establishments which would be added through M&As. Surprisingly, we do not find a significant increase in the number of establishments following the IPO, potentially suggesting firms grow at least some employment organically.

To further differentiate between the two channels that could be driving higher employement after IPO, we examine how firms grow their labor force after an IPO. A financial constraints channel would predict an increase in geographic expansion, since companies often grow by using IPO proceeds to invest and expand into new geographic markets (or "scale"; Pride, Hughes, and Kapoor 2016). However, agency theories would predict the expansion into new industries and increased industrial diversification since diversification decreases uncertainty and benefits risk-averse mangers (Amihud and Lev, 1981; Denis, Denis, and Sarin, 1997). Our findings best support the agency mechanism as employment growth is more pronounced in non-core industries, defined as employment outside of the four-digit SIC industry with the greatest employment. These results suggest that the IPO not only facilitates growth but industrial diversification as well, which is consistent with the general ramifications of a decline in concentrated ownership.[2] Moreover, we do not find a significant increase in geographic expansion into new markets as a result of an IPO, which does not support the financial constraints channel.[3]

Using our IV approach, we document statistically insignificant wage decreases 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). Likewise, workers hired post-IPO are likely to fetch a wage premium as recently public firms rush to hire to exploit valuable growth opportunities unlocked by the relaxation of financial constraints. In contrast, the agency channel has no clear prediction on wage growth post-IPO. We also find increasing wages post-IPO among new hires when comparing

[2]Note this result is different from Arikan and Stulz (2016) who look at the life-cycle firm effects, while we are interested in the causal effect of going public. Also note, it is possible that firms diversify efficiently (Maksimovic and Phillips, 2002). However, since high-Q firms would diversify in this channel, the IV strategy controls for this selection issue.

[3]These findings are consistent with Cornaggia, Gustafson, Kotter, and Pisciotta (2019) who show in a sample of all U.S. IPOs that there is no significant geographic expansion following going public. However, Cornaggia, Gustafson, Kotter, and Pisciotta (2019) also show that in the sample of the largest IPO-filing firms, those least likely to suffer from financial constraints, geographic expansion increases as the result of an IPO.

wages at their previous 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 the agency channel.

In further support of the argument 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. Following a successful IPO, the rate at which employees leave to take key positions at new firms (aged three years or younger) grows by over five 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-going firms to firms that patent for the first time, which is a proxy but not a direct measurement of new firms.

We further show that the departure of existing employees to startups following an IPO is strongest for firms in the high-tech sectors. This supports our agency mechanism given that pre-IPO, 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. 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 also show that the 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 (Acemoglu, Akcigit, and Celik, 2014). Finally, we do not find significant changes in the inflow of employees from startups post-IPO, suggesting that the newly-public firms do not compensate for the loss of human capital departing to startups.

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—results that rely on the

employee-employer matched LEHD data—can be estimated only on 31 states. Specifically, while our data on employment, number of establishments, geographic and industrial diversification cover 100% of IPOs, our data on wage and employee reallocation cover 71% of IPOs. We show that summary statistics for the firms in our 31 LEHD states match those of the full sample. Likewise, we show that the baseline results, which can be estimated for all states, are robust to being estimated on our 31 LEHD states.

Overall, our paper provides new evidence to the active debate on the trade-off between public listing and staying private and its inuence on a rm's real activities—the debate invigorated by the influential papers showing the decline of public firms (Doidge, Karolyi, and Stulz, 2017; Kahle and Stulz, 2017). Access to public markets offers the benets of cheaper capital, allowing a public firm to conduct more mergers and acquisitions (Maksimovic, Phillips, and Yang, 2013), to grow employment (Borisov, Ellul, and Sevilir, 2015), and improve 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; Maksimovic, Phillips, and Yang, 2020). Alternatively, the agency conicts resulting from divergent incentives between investors and managers at public rms impair firm investments (Asker, Farre-Mensa, and Ljungqvist, 2015) and innovation (Bernstein, 2015). Our ndings show that IPOs have real effects beyond the firm itself and affect multiple dimensions of real outcomes, including organic job growth, wages, and labor reallocation. Our results also highlight the importance of agency-driven implications. Moreover, our results complement the existing literature using patents. While patents represent an important measure of firm outcomes, patents have at least three important limitations. First, patenting is an endogenous decision by the firm that depends on the firm's intellectual property (IP) strategy, which can change when the firm goes public (Sampat, 2018). Second, patents capture only the subset of innovation outputs that are contractible, which also varies drastically by industry (Garcia-Macia, Hsieh, and Klenow, 2019). Third, patents can themselves affect inventor mobility, since they can serve as a signal of worker quality and affect bargaining.

Our paper also 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), inventors (Bernstein, 2015), venture capitalists (Megginson and Weiss, 1991; Hellmann and Puri, 2002), and total employment (Borisov, Ellul, and Sevilir, 2015).[4] We extend this 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. We provide three main contributions to this literature. To the best of our knowledge, we are the first to use US Census micro-level data and to document some basic facts about a firm's labor force before and after IPO filing. We also provide the first evidence that successful IPOs cause an increase in industrial diversification and the outflow of high-human-capital workers to startups, with no compensating increase in inflow from startups. Interestingly, while we document that 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 contribute to the growing literature focusing on the intersections between labor and finance, which study how corporate decisions such as corporate governance, M&A, diversification, leverage, and private equity affect workers and labor reallocation (e.g. Atanassov and Kim (2009); Maksimovic, Phillips, and Prabhala (2011); Simintzi, Vig, and Volpin (2015); Tate and Yang (2015); Agrawal and Matsa (2013); Agrawal and Tambe (2016)). We contribute by documenting that IPOs trigger important labor-force changes inside and across firms.

1 Hypotheses and Tests

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

[4]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), operating income (Jain and Kini, 1994), stock returns (Ritter 1991; Loughran and Ritter 1995), insider ownership (Mikkelson, Partch, and Shah, 1997) and acquisitions (Celikyurt, Sevilir, and Shivdasani, 2010).

1.1 Financial Constraints

Stock markets can provide various benets as a source of external capital by reducing asymmetric information, lowering the cost of capital, and enabling the development of growth opportunities in rms (Rajan, 2012). As investors' portfolios become more liquid and diversied, stock market listing lowers the cost of capital (Pagano, Panetta, and Zingales, 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, Ellul, and Sevilir, 2015). Moreover, since companies often grow by using IPO proceeds to expand into new geographic markets (or "scale"; Pride, Hughes, and Kapoor 2016), IPO proceeds might relax the financial constraints 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 is primarily motivated to resolve financial constraints, then we should observe particular wage patterns. (Michelacci and Quadrini, 2009) develop a labour 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 going firm has valuable growth opportunities that can be competed away, the firm might be willing to pay a wage premium to be able to hire workers quickly. Hence, the financial constraints channel predicts high wage growth of both pre-IPO and new workers following the IPO.

Finally, the relaxation of financial constraints of the IPO-going firm also implies larger retention of employees who work on the development and the implementation of new ideas, products and services (Grossman and Hart, 1986). The infusion of cash from an IPO means that some of the employees would now stay and develop their ideas internally—the ideas that would have been rejected by the firm due to constraints (Babina, Forthcoming). 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. Moreover,

the firm might also need to hire additional employees for the R&D and experimentation associated with developing new products and services. In sum, the easing of financial constraints is likely to be associated with higher retention of the existing employees and increase in new hires. Since entrepreneurship is often associated with experimentation and the development of new ideas (Manso, 2016), a financial constraints mechanism predicts that after an IPO the turnover to startups would decrease and the hiring of workers from startups would potentially increase.

1.2 Agency Theories

The transition from private to public ownership leads to less concentrated ownership and the weakening of managerial incentives, potentially resulting in agency costs. First, since larger firms tend to offer management more perks and higher compensation, managers may engage in empire building (Williamson, 1964; Jensen, 1986). This is likely to result in higher overall firm employment. Second, managers might seek to diversify into different industries since diversification decreases uncertainty and benefits risk-averse mangers (Amihud and Lev, 1981; Denis, Denis, and Sarin, 1997). Hence, the agency channel predicts faster employment growth and the increase in industrial diversification following the IPO.

Agency theories do not have clear predictions about post-IPO wage growth of existing and newly-hired employees. Managers might seek a "quiet life" where minimal effort negotiating with workers leads to higher wages for existing and new employees (Bertrand and Mullainathan, 2003). On the other hand, it is not entirely obvious why in competitive labor markets entrenched managers need to pay a wage premium to their workers.

Agency theories also predict lower retention of human capital involved in developing new ideas, products, or services following IPOs. Public owners have a more short-term focus and will, therefore, prefer safer projects to those associated with greater experimentation and long-term horizons. Stein (1989) shows that stock markets tend to target short-term earnings and such myopia could induce public rms to invest sub-optimally. With their compensation linked to stock performance, the managers of public rms have incentives to sacrice long-term investments to boost short-term stock returns. Indeed, recent empirical evidence is consistent with this view. Terry, Whited, and

Zakolyukina (2018) find that managers cut R&D around periods in which they have to restate their books. 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 these models is that stock markets hinder firms from investing in high-risk, high growth projects and projects that require experimentation and long-term horizon.

Since entrepreneurship is often associated with high-risk, high growth projects as well as experimentation and a long-term horizon (Manso, 2016; Kerr, Nanda, and Rhodes-Kropf, 2014), an agency mechanism predicts that following IPOs, firms will employ fewer entrepreneurial-minded workers. The agency channel also has three cross-sectional predictions. First, conditional on an increase in agency costs, mangers of firms with high-risk, high growth opportunities are more likely to benefit from the reduction of investments in risky opportunities and, hence, have lesser incentives to retain employees with experimental-type preferences following an IPO. Hence, the increased departures to startups post-IPO are likely to be larger in high-tech industries. Second, since the development of new projects requires high-skilled labor, the departures post-IPO are likely higher among high-skilled and high-wage talent. Third, given that Acemoglu, Akcigit, and Celik (2014) find that younger workers are associated with more experimentation and more creative innovation, the agency mechanism will predict that younger workers are relatively more likely to depart following IPOs. Moreover, managers will not have an incentive to compensate for this turnover by hiring new experimental-minded workers.

1.3 Summing-up Testable Implications

We next sum up the predictions drawn above.

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.

Wages. The financial constraints channel has 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. The agency channel has no clear prediction on wage growth post-IPO.

Retention and hiring of talent. The two channels disagree in their predictions on the turnover and hiring of talent that is involved in developing new ideas, products, or services. The financial constraints channel predicts lower turnover to startups following IPOs, with the increased retention in high-tech industries and among high-wage workers. In contrast, the agency channel predicts heightened turnover to startups post-IPO, particularly in high-tech industries, and among high-wage and younger workers. The agency channel also predicts no changes in hiring from startups following the IPO.

2 Measuring Employment, Wages, Turnover and Hiring

Key to our analysis is being able 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.

2.1 Data Sources

We combine databases from the following sources to form our estimation sample: the US Census Bureau; Thompson Reuter's Securities Data Corporation (SDC) and VentureXpert.

2.1.1 Establishment-Level Data

We start with establishment-level information from the Longitudinal Business Database (LBD), a database maintained by the US Census Bureau. The LBD is a panel dataset that tracks all US business establishments.[5] An establishment is any separate physical location operated by a firm with at least one paid employee. The LBD contains information on the number of employees working for an establishment, total annual establishment payroll, and the industry and physical

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

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, as well as to measure the industrial and geographic diversification of each firm.

2.1.2 Matched Employer-Employee Data

We add worker-level data using the Longitudinal Employer-Household Dynamics (LEHD) data, also maintained by the US 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, Iowa, Idaho, Illinois, Indiana, Louisiana, Maryland, Maine, Minnesota, Missouri, Montana, North Carolina, New Jersey, New Mexico, Nevada, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Utah, Virginia, Vermont, 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 the high-tech sector is an important one among IPO-filing firms, the concern may arise that our LEHD data do not cover key states, such as California and Massachusetts. However, we can observe all 50 states when using the LBD. In Section 3, we show 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.

[6] See Abowd, Stevens, Vilhuber, Andersson, McKinney, Roemer, and Woodock (2006) for a more detailed description of the program and the underlying datasets that it generates.

2.1.3 Data on IPOs and Other Financial Variables

We use Thomson's 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 Census Bureau sources and allow for a three-year, post-event window to measure post-IPO-filing labor-related outcomes. Following the literature, we exclude the IPO filings of financial firms (SIC codes between 6000 and 6999), non-US based firms, unit offers, closed-end funds (including REITs), ADRs, limited partnerships, special acquisition vehicles, spin-offs, and issues of non-common shares.[7]

Using these restrictions, we identify in the SDC data 4,900 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 a) we are able to match to the LBD, using all 50 states; and b) have all control variables used in regression analysis. We find that 77% of the firms in our final sample successfully completed their IPOs. Since employee-level data are only available for 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 mergers and acquisitions (M&As) of firms in our IPO-filing firm sample—we control for mergers in all regressions. 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 venture capital (VC) funding using both SDC Thomson (VentureXpert) and Dow Jones (VentureSource) data sources. We employ a crosswalk developed by Puri and Zarutskie (2012) that

[7] We also exclude industries in SIC1 (mining and construction) as these industries have an especially low correlation with NASDAQ. The low correlations with NASDAQ returns prevent us from using our IV approach for these industries.

[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 that are 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.

uses a name and address-matching algorithm to link to firms in the LBD. Specifically, we identify firms filing an IPO as VC-financed if they can be matched to firms that are reported as having received VC financing in the VentureXpert or VentureSource databases.

2.2 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 three years after the IPO filing event. Our three-year window ensures that sufficient time has passed for IPO-filing firms to complete or withdraw their IPO filing, that 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 at the quarterly level. Thus, we estimate worker-level characteristics in the LEHD using the quarter that strictly precedes the IPO filing date and again estimate ex-post values using a snapshot occurring three years after the IPO filing. We aggregate across all workers observed in our data to calculate a firm-level estimate.

2.3 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 US 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.[10] These firms also tend to be

[10]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.

small, with an average of 467 employees spread over nine distinct establishments, and have minimal industrial diversification. On average, 93% of employment is assigned to the firm's primary 4-digit SIC. This high industrial concentration is unique. For example, Babina (Forthcoming) 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 three 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.[11] Just under half of our firms have headquarters in our sample of LEHD states. Across all these variables, we report economically similar estimates for the set of firms which withdrew their IPO relative to the set of firms which successfully completed their IPO. Bernstein (2015) makes the same conclusion when comparing firms which withdrew versus completed their IPO along different dimensions, including innovation and accounting performance.

We measure NASDAQ market returns both before (90 days) and after (60 days) the IPO filing. Average 60-day post-issuance NASDAQ returns at firms that complete their IPO are 2.3%, compared to -2.5% for firms that withdraw their IPO. 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 Jr, 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 a 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

[11] 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, 5065, or 5063. A firm is in the "Computers" industry if its primary SIC code is 5370–5379, 5044, 5045, 5734 or 7370–7379.

37 years. Consistent with anecdotal evidence, our sample is disproportionately male and white. Interestingly, less 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 over $82,000, compared to the national average of $37,000 over this period.[12] Earnings include all forms of immediately taxable compensation, such as salaries, wages, commissions, bonuses, and exercised stock options. While there might be concerns that these earnings do not include non-taxable compensation, it is not entirely clear why firms with withdrawn and completed IPOs would have differential non-taxable compensation. If firms are financially constrained before the IPO filing, high mean earnings might seem somewhat surprising. These high mean earning are even more surprising given they do not capture non-taxable compensation. 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 firm, 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 three 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 three 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

[12]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.

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 IPO.

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, wages decline by 3.6%. This pattern is more pronounced for firms that withdrew their IPO. 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 three years immediately following the IPO. By definition, this variable can only be estimated for those workers who were 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 wage difference between first full quarter wages at the new firm, compared to the last full quarter wage at their 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 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 three years later, have left the firm and are now employed at a startup (firms younger than or equal to three years old) and where they are one of the top five 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 Babina (Forthcoming) and Azoulay, Jones, Kim, and Miranda (2018).[13] On average, 2.3% of employees leave an IPO filing firm in the three 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 (Forthcoming), suggesting IPO-filing firms are important sources of entrepreneurs.

3 Results

In this section, we report the results of testing the hypotheses developed in Section 1 related to changes in employment, wages, turnover, and hiring following a completed IPO. To allow for casual inference, we use an instrumental variables approach. We start by validating 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.

3.1 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, Benveniste, and Guo (2001); Benveniste, Ljungqvist, Wilhelm Jr, and Yu (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 60 days following the IPO filing, an approach based on Bernstein (2015).[14]

To validate that NASDAQ returns during the 60-day window following an IPO filing predict successful IPO completions in our sample, we estimate the following regression:

$$IPO_i = \beta_1 \text{NASDAQ Return 60 Days After}_i + X_i^{'}\delta_i + \mu_t + \vartheta_k + \varepsilon_i \quad (1)$$

[13] For example, Azoulay, Jones, Kim, and Miranda (2018) find that a firm's top three initial earners usually include the firm's owners. This is because the W-2 data that is 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.

[14] OLS regressions are reported in the Online Appendix. The IV approach is quite helpful since 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 the stock market tanks. Or, it may be that firms with strongest growth prospects are less likely to withdraw because investors' demand to its shares will be more inelastic.

where NASDAQ Return 60 Days After$_i$ is the cumulative NASDAQ returns during a 60-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 robust standard errors are reported.

Table 3 reports the results. In column 1, we include year fixed effects. There is a strong and positive relationship between 60-day NASDAQ returns and whether the IPO was successfully completed. A decline of one standard deviation in NASDAQ returns translates into a 4.4% decline in the probability of a successful IPO. In our sample of IPO filing firms, 76.6% of firms complete their IPO. Moreover, the F-statistic of 26.5 exceeds the conventional threshold of F = 10 and suggests that the instrument is strong and 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, 90-day NASDAQ return prior to filing, and a dummy variable that identifies IPO filers that are acquired during the three-year window post-IPO filing.[15] Column 3 adds (SIC1) industry and year fixed effects and Column 4 includes industry and year fixed effects as well as firm-level controls.[16] The strong and positive relationship between 60-day NASDAQ returns and whether the IPO was successfully completed is robust across all four specifications.

In order to be a valid instrument, the IV must also meet the exclusion restriction condition. In the case of our specification, we must argue that 60-day NASDAQ returns do not directly impact future firm characteristics except through the IPO-completion channel. It is important here 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.

[15]Due to US Census restrictions on the number of reported estimates, coefficient estimates on control variables are not reported.

[16]Results are qualitatively similar in terms of economic and statistical significance if we instead use SIC2 industry fixed effects. Results are available upon request.

It is unlikely that returns during this small window will directly predict growth opportunities except through the channel of reflecting broader macroeconomic trends, 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. Moreover, we further validate this assumption in Section 3 by using a placebo test. We show an IV using NASDAQ returns during a narrow window of time, but a window that strictly precedes the IPO filing, yields insignificant results.

3.2 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 $\varepsilon_i$is the error term. Observations are measured at the firm level.

We report results of the causal impact of IPO completion on firm employment, number of establishments, geographic and industrial diversification, and wages in Table 4. Controls are included for firm size (the natural logarithm of the count of total domestic employment in the year of the IPO filing), average wages (the natural logarithm of annualized firm average wages), firm age, 90-day NASDAQ return prior to filing, and a dummy variable that identifies IPO filers that are acquired during the three-year post-IPO filing window, as well as year and industry fixed effects. Estimates without controls are similar in terms of economic and statistical significance for all our dependent variables and are reported in Online Appendix in Table 2.

In column 1, we show that a successful IPO completion leads to a positive and statistically significant increase in employment. Our estimate shows that following a successful IPO, firms increase employment annually by 23% over the next three years (or 70% over the next three years),

compared to firms with a withdrawn IPO. This increased employment growth after IPO is consistent with both the financial constraints and the agency mechanisms. However, it is important to emphasize that the instrumental variable analysis approach is identified only for the firms sensitive to NASDAQ value declines. As such, the IV estimates measure local average treatment effects. Those firms whose IPO decisions are least sensitive to market fluctuations may experience different post-IPO trends.[17]

Since firms tend to be active acquirers immediately after their IPOs (Celikyurt, Sevilir, and Shivdasani, 2010; Brau and Fawcett, 2006), it is not clear that going public creates new jobs organically. To shed new light on the debate about whether IPOs create new jobs or just add jobs through M&As, we examine if this increased employment growth is driven purely through the additions of new establishments which would 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, suggesting modest post-IPO increases in new plants and physical properties and potentially suggesting that firms grow at least some employment organically post IPO.

In Section 1, we discuss that 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. To start to distinguish between the channels, in column 3, we show an insignificant increase in the geographic footprint following an IPO, which is inconsistent with the financial constraints channel. Alternatively, column 4 shows 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 3.8% each year, as measured over the three-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 IPO filing, firms in our sample are highly concentrated, with only 7% of their employment in industries outside of their main four-digit SIC code.

[17] In the OLS specification, the parallel estimation yields an economic significance of 14%, suggesting a more muted response when considering the full sample of IPO filing firms. OLS regressions are reported in the Online Appendix in Table 3.

Taken together, these results show that a successful IPO leads to employment growth. 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. However, the fact that employment growth does not come from geographic expansion, but instead occurs disproportionately outside of the firm's main industry also suggests increasing agency problems. For example, Denis, Denis, and Sarin (1997) find that lower managerial and blockholder ownership is associated with value-destroying diversification.

We next next turn to testing how wage growth changes following the IPO. In Section 1, 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 have a clear prediction on wage growth. 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. Using this measure we document a modest but statistically insignificant increase in wages following an IPO.

To better understand these wage dynamics, we next turn to regressions using the LEHD sample. With the LEHD data, we can observe employer-employee data across time, 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 same IV specification as in Table 4.[18]

In column 1, we report the effect of IPO completion on the three-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. In this 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 who leave, we consider subsamples in columns 2 and 3. In column 2, we limit the sample to those workers who are observed at the firm in the pre-IPO filing period and at the firm in the three-year ex-post period. In column 3, we limit the sample

[18]The first-stage IV regressions for the LEHD sample are reported in the Online Appendix in Table 1.

to those workers who are at the firm in the pre-IPO filing period but who leave this firm over the next three years. Interestingly, we find negative, but insignificant coefficient estimates on IPO completion in both samples: while, as expected, the coefficient for the stayers is less negative, than for leavers, the negative and insignificant growth for the stayers is quite surprising. One caveat about these null results in the IV setting is that IV methods generally have low power and large variances of estimates, potentially suggesting differential effects across the levels of distribution of worker wages. More research is needed on whether IPOs have a significant causal impact on worker earnings. With that caveat in mind, 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 IPO but then offer higher pay growth following cash infusion through a successful IPO, as in the model of firm financial constraints and wages of 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. We show that the new hire wage premium increases by nearly 8.7% 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.

Moving to the prediction related to changes in investor and management incentives post-IPO, we examine turnover to startups and hiring of workers from startups. In Section 1, the two channels disagree in their predictions on the turnover and hiring of talent from startups. The financial con-

straints channel predicts lower turnover to startups and potentially increased hiring from startups following the IPO. While the agency channel predicts heightened turnover to startups post-IPO and no changes in hiring from startups following the IPO. We next consider the departure rate to entrepreneurship, which is a measure of the impact of an IPO on worker flow to startups. Results are reported in Table 6 using the same empirical specification as in the two previous tables. 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 three years to take top-five roles at startups. For firms that complete their IPO, this jumps by 5.3%, implying that the average rate of worker exits to startups doubles for a marginal firm that completes an IPO filing. These results support the agency channel.[19]

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 halfs of the firm's wage distribution, respectively. Again, these results support the agency channel and are inconsistent with the financial constraints channel.

Given that Acemoglu, Akcigit, and Celik (2014) find that younger workers are associated with more experimentation and more creative innovation, age may also be important. In column 4 (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 agency channel as discussed in Section 1. Finally, in column 6, we explore the differential treatment 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

[19]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 instrumental variables approach.

short-term focus (Manso, 2016). As predicted by the agency 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 funds would help to fund.

Finally, when we look at the hiring rates post-IPO of employees from startups, we do not find any significant changes (unreported), consistent with the agency story and potentially inconsistent with the financial constraints channels. This result is different from Bernstein (2015) who finds that the IPOs causes an increased rate of hiring of outside inventors.

Overall, these results about labor flow from/to startups suggest that the change in ownership that occurs following an IPO has important implications. One consequence of public ownership is increased emphasis on short-term goals. This, in turn, encourages managers to prefer more-conventional projects over experimentation, as in the model of Ferreira, Manso, and Silva (2014), and is consistent with our finding of increased departure rates of entrepreneurial-minded employees.

4 Discussion

We next discuss the implications of our findings, before discussing additional robustness tests in the next section. Our results provide new insights on the real consequences of a firm's transition from private to public ownership—especially important topic given the active debate about the recently documented decline of public firms and IPOs in the U.S. (Doidge, Karolyi, and Stulz, 2017; Kahle and Stulz, 2017). Our findings of increased industrial diversification and the loss of high-skilled and creative talent is most consistent with theories that predict an increase in agency costs following the transition to public ownership.

The real nature of our outcome variables and the importance of labor as the key input for firms (Zingales, 2000) provide further evidence on the important real effects of IPOs, complementing evidence in the existing work using patents. We also show that the employment growth caused by the IPOs documented in Borisov, Ellul, and Sevilir (2015) is likely not entirely driven by M&A, suggesting some jobs are created organically. Additionally, vibrant public markets have always been

thought to be fundamentally important for encouraging entrepreneurship through reallocation of high-skill workers from IPO-bound to new firms (Michelacci and Suarez, 2004). We provide the first direct empirical evidence of this importance by using data on IPO-going firms, new firms, and their labor-forces.

Finally, our results speak to an important question about how going public affects workers. From a worker perspective, our results are likely positive. First, we show that IPOs create jobs. We also show significant wage increases for workers hired by the newly-public firms—both in the cross-section and causally through the IPO effect. Moreover, the IPO causes an economically large increase high-wage workers in high-tech sectors transitioning to entrepreneurship, suggesting that going public affects worker careers.

5 Robustness

In the following section we address several key robustness tests. First, 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 only available for 31 states. We show that results using only these 31 states appear similar to results estimated using the full sample. Second, we consider a placebo test to validate our instrumental variables approach.

5.1 Validating the LEHD Sample

As a first step in validating the LEHD sample, we report summary statistics for firms in the 31 LEHD states. We also report the same statistics for the full set of firms, thereby allowing for easy comparison. The results are reported in Table 7; the full sample is in column 1 and the LEHD sample is in column 2. For both samples, we aggregate establishment-level data to create firm level averages. For the full sample, we report the results for all firms. For the LEHD sample, we report results using only those firms with at least some employment in our 31 LEHD states. For completeness, we replicate all summary statistics from Tables 1 and 2 that can be generated for

the full set of states.

Panel A reports pre-IPO summary statistics for the full and the LEHD samples. Not surprisingly, firms that are observed, at least partially, in the 31 LEHD states tend to be larger in terms of total employment, number of establishments, or physical presence across states. These firms are also more likely to have their headquarters in one of the 31 LEHD states. 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.

Panel B reports post-IPO changes for the full and LEHD samples. 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. Overall, firms with employment in our 31 LEHD states demonstrate no strong differences, compared to the full sample.

To provide further evidence of no systematic bias in the LEHD sample, we repeat the regressions presented in Table 4 using first the full sample and then the LEHD-State sample, or the set of firms with employment in our 31 LEHD states. Table 8 presents the results. The unit of observation is a firm-level aggregate using all domestic establishments. Panel A presents results for the full sample (a replication of the results in Table 4, presented again here for ease of comparison.) Panel B presents results using only those firms with at least some employment in our 31 LEHD states. In both panels, we instrument the IPO-completion indicator and the interaction of the IPO and high-tech firm indicators with: 1) the NASDAQ return in the 60-day window following the initial IPO filing, and 2) the interaction of high-tech indicator with the NASDAQ return. We also include the full set of controls, as well as year and industry fixed effects.

Overall, the coefficients are 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-State 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 establishments, number of states, or average wages in either sample. In sum, these results provide further support for our argument that there is no systematic bias in our 31 LEHD states.

In a related approach, we next consider 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. We address this concern directly in Table 9 by repeating the analysis in Table 4 but interacting IPO completion with a dummy variable that takes the value of one if the firm is headquartered outside of our 31 LEHD states. As in the previous tables, we instrument for IPO using 60 day NASDAQ returns following the IPO filing and include the full set of controls as well as year and industry fixed effects. We continue to identify statistically significant relations between IPO completion and employment growth and change in industrial concentration. However, across all five regressions, the interaction between IPO completion and the indicator variable for a headquarters outside of our 31 LEHD states is always insignificant.

5.2 Placebo Test

To validate that the exclusion restriction is met, we consider the following placebo test. We look at the relationship between firm characteristics and NASDAQ returns using the three-month window prior to the IPO filing on the same sample of firms. We argue that returns prior to the IPO filing should have no impact on our outcome variables of interest, after controlling for annual returns using year fixed effects. Indeed, this is confirmed in Table 10 using the four outcome variables for which we found significant coefficients in the earlier tests. As in the previous tables, we include the full set of controls as well as year and industry fixed effects.

Column 1 reports the first-stage results using the LBD sample. There is no significant relationship between returns prior to the IPO filing and IPO success. Column 2 reports second-stage results where the dependent variable is the annualized growth in employment. Column 3 reports second-stage results where the dependent variable is the annualized change in industrial concentration. After instrumenting for IPO success with NASDAQ returns prior to the IPO filing, we find no statistical relationship between IPO success and either post-IPO employment growth or industrial diversification.

Column 4 reports the first-stage results using the LEHD sample. There is again no significant relationship between returns prior to the IPO filing and IPO success. Column 5 reports second-stage results where the dependent variable is the new hire wage premium. Column 6 reports second-stage results where the dependent variable is the departure rate to entrepreneurship. As with the LBD sample, after instrumenting for IPO success with NASDAQ returns prior to the IPO filing, we find no statistical relationship between IPO success and either new hire wage premiums or future rates of departures to entrepreneurship. These results give additional validity to our assumption that there is no direct relationship between post-filing IPO returns and labor outcomes after controlling for annual returns.

6 Conclusion

In this paper, we focus on the role of the IPO market and the firm's choice to go public on the firms's labor force. Using micro data from the US 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 are also able to 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 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, Manso, and Silva (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 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 document a large ex-post increase in employment, 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. However, given limitations of some of these results, we think more research is needed to understand the effects of IPOs on workers across differed thresholds of the distribution of wages and wage inequality.

While we provide new evidence on how IPOs affect labor markets, there is still a lot 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&A exits instead? Moreover, it would be interesting to dig into whether the long-run IPO under-performance (Ritter, 1991) is related to the deterioration of human capital post-IPO. Additionally, more work is needed to understand whether IPOs create jobs, or just increase employment through M&As. Finally, an important question is whether going from public to private reverses the patterns.

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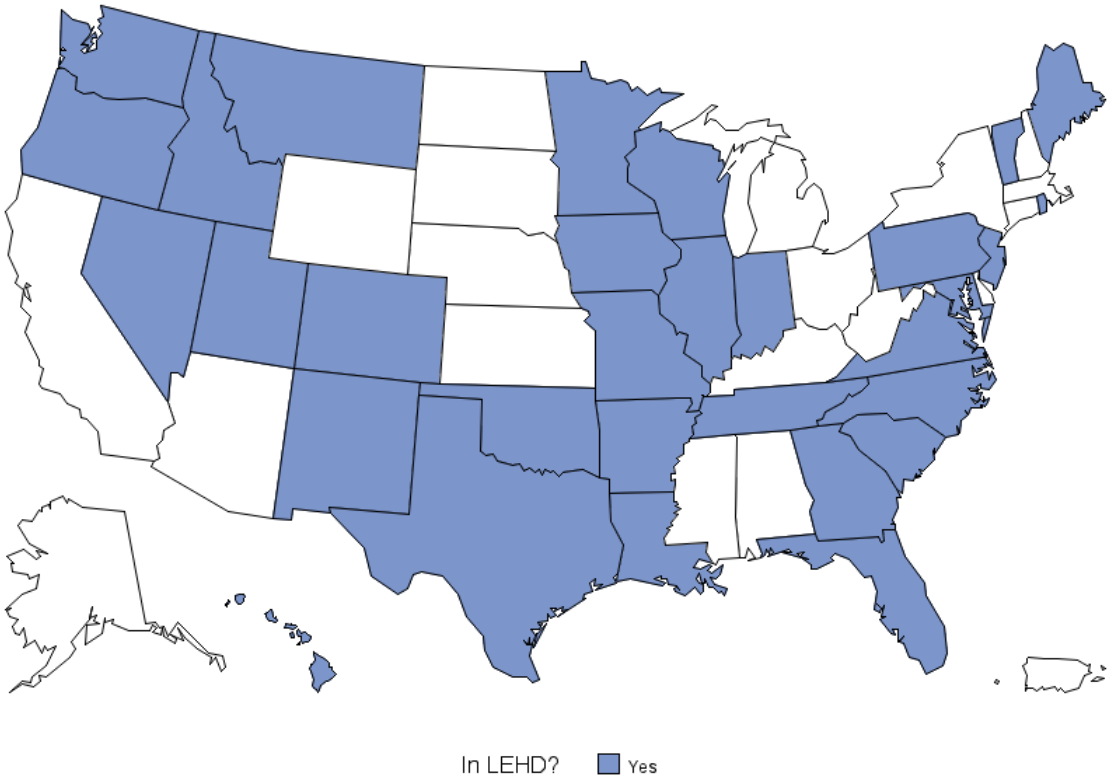

Figure 1
Map of 31 US States Available in LEHD Database

Table 1
Summary Statistics on IPO-Filing Firms as of IPO Filing

The table describes firm-level characteristics of IPO-filing firms. The sample includes US 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. Column 2 (3) reports 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 US states and DC). 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 60 Days After is the NASDAQ return in the 60-day window following the IPO filing. NASDAQ Return 90 Days Before is the NASDAQ return in the 90-day window prior to the IPO filing. 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 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 (calculated with the SDC data). 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 US. Average Wages$_t$, in $000, 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 Successful
Panel A. Full 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 60 Days After	0.012	-0.025	0.023
	(0.101)	(0.123)	(0.091)
NASDAQ Return 90 Days Before	0.063	0.061	0.063
	(0.124)	(0.152)	(0.114)
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$_t$, in '$000	82.67	85.46	81.76
	(47.50)	(46.23)	(47.88)

Table 2
Summary Statistics on IPO-Filing Firms Following IPO Filing

This table describes changes in firm-level characteristics following a firm's IPO filing. The sample includes US 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. Column 2 (3) reports 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 three years following an IPO filing relative to the year of the IPO filing, calculated using LBD data in 50 US states and DC. The growth is calculated as log difference between the future and the IPO filing years' employments. Note, if the three-year future employment is missing, we use future annualized two-year employment; if both three- and two-year future employments are missing, we use one-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 three 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 three years following an IPO filing relative 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 three 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 three 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 one quarter before IPO filing who are observed three 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 three years after the filing, and 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 one quarter before IPO filing who are observed three years later at a firm no more than three years old and are among the top five earners at that firm. Per Census Bureau disclosure rules, observations and estimates are rounded.

	Full Sample	IPO Withdrawn	IPO Successful
Panel A. Full 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)

Table 3
IV Regressions: First-Stage

The table reports first-stage results of the instrumental variables (IV) regressions. The sample includes US 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 instrumental variable, NASDAQ Return 60 Days After, is the NASDAQ return in the 60-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 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over the course of three years after the IPO filing. 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 50 US states and DC. The parameter estimates for the control variables are not reported due to US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Dependent Variable:	IPO			
	[1]	[2]	[3]	[4]
NASDAQ Return 60 Days After	0.440***	0.487***	0.441***	0.489***
	(0.085)	(0.087)	(0.085)	(0.086)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Number of Observations	3,400	3,400	3,400	3,400
F-Statistic	26.5	31.5	26.9	32.03

Table 4
Impact of Successful IPOs on Firm Employment, Scale, Diversification, and Wages

The table reports second-stage results of IV regressions and shows how a successful IPO affects a firm's growth in employment, scale, diversification, and wages. The sample includes US 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 50 US states and DC. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. IPO is instrumented with the NASDAQ return in the 60-day window following the initial IPO filing. All dependent variables are measured over three 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 are the same as in the first stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The estimates without controls are similar in terms of statistical and economic significance and are presented in the Online Appendix. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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	0.225*	0.058	0.017	-0.038**	0.010
	(0.133)	(0.084)	(0.057)	(0.017)	(0.092)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Number of Observations	3,400	3,400	3,400	3,400	3,400

Table 5
Impact of Successful IPOs on Wage Growth

The table 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 US 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 60-day window following the initial IPO filing. In columns 1–3, the dependent variable is the three-year growth in employee wages. In column 1, the wage growth calculation includes all workers observed at the firm one quarter before the IPO filing. In column 2, the wage growth calculation includes all workers observed at the firm one quarter before the IPO filing who remain at the firm three years later. In column 3, the wage growth calculation includes all workers observed at the firm one quarter before the IPO filing who are no longer at the firm three 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 are the same as in the first-stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The estimates without controls are similar in terms of statistical and economic significance and are presented in the Online Appendix. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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	-0.206	-0.164	-0.227	0.087**
	(0.219)	(0.202)	(0.221)	(0.044)
Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Number of Observations	2,400	2,400	2,400	2,400

Table 6
Impact of Successful IPOs on Employee Departures to Entrepreneurship

The table reports second-stage results of IV regressions and shows how a successful IPO affects employee departures to entrepreneurship. The sample includes US 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 60-day window following the initial IPO filing. In columns 1–6, the dependent variable is the fraction of entrepreneurial departures, defined as the fraction of workers one quarter before IPO filing who are observed three years later at a firm no more than three years old and who are among the top five earners at that firm. In columns 1 and 6, the dependent variable includes all workers at the firm one quarter before the IPO filing. In column 2 (3), the dependent variable includes workers at the firm one quarter before the IPO filing whose wage equals or is above (equals or is below) the median firm's worker wage. In column 4 (5), the dependent variable includes workers at the firm one quarter before the IPO filing whose age equals or is above (equals or is below) the median firm's 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: 1) the NASDAQ return in the 60-day window following the initial IPO filing, and 2) the interaction of High-tech indicator with the NASDAQ return. High-tech is 1 for firms in computer, bio-tech or electronics sectors, and 0 otherwise. The control variables are the same as in the first-stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The estimates without controls are similar in terms of statistical and economic significance and are presented in the Online Appendix. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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	0.053*	0.090**	0.033	0.012	0.091**	0.004
	(0.032)	(0.040)	(0.038)	(0.037)	(0.043)	(0.034)
IPO X High-tech						0.091***
						(0.033)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,400	2,400	2,400	2,400	2,400	2,400

Table 7
Robustness Tests Comparing Full Sample vs. LEHD Sample: Summary Statistics

The table shows that firm-level characteristics of IPO-filing firms are similar for two samples: 1) all firms matched to the LBD (in column 1), and 2) the LBD-matched firms that also have employees in our 31 LEHD states (in column 2). The sample includes US 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 (column "Full Sample"; 3,400 observations), and a firm-level aggregation of workers available in our 31 LEHD states (column "LEHD Sample"; 2,400 observations). Both panels contain variables that are available for both samples to allow the comparison. Statistics in column "Full Sample" in Panel A (Panel B) are identical to the results in column "Full Sample" of Table 1 Panel A (Table 2 Panel A) but repeated here for comparison. Panel A shows firm-level characteristics as of their IPO filing, and Panel B shows changes in firm-level characteristics following the firm's IPO filing. Variable definitions are available in Table 1 for Panel A and in Table 2 for Panel B. Per Census Bureau disclosure rules, observations and estimates are rounded.

	Full Sample	LEHD Sample
Panel A. Pre-IPO Characteristics		
Firm Age$_t$	8.84	9.76
	(7.05)	(7.32)
Employment$_t$	467	590
	(865)	(964)
Number of Establishments$_t$	9.00	11.58
	(19.91)	(22.52)
Number of States$_t$	3.10	3.83
	(4.60)	(5.19)
Industrial Concentration$_t$	0.925	0.906
	(0.165)	(0.182)
NASDAQ Return 60 Days After	0.012	0.012
	(0.101)	(0.106)
NASDAQ Return 90 Days Before	0.063	0.067
	(0.124)	(0.129)
VC-backed	0.489	0.506
High-tech	0.503	0.486
HQ State not in LEHD	0.527	0.445
Number of Observations	3,400	2,400
Panel B. Post-IPO Changes		
Annualized Growth in Employment$_{t,t+3}$	0.194	0.179
	(0.347)	(0.342)
Annualized Growth in Average Firm Wages$_{t,t+3}$	-0.036	-0.036
	(0.220)	(0.218)
Annualized Growth in # of Establishments$_{t,t+3}$	0.066	0.071
	(0.212)	(0.223)
Annualized Growth in # of States$_{t,t+3}$	0.046	0.050
	(0.147)	(0.152)
Annualized Growth in Industrial Concentration$_{t,t+3}$	-0.006	-0.006
	(0.041)	(0.044)
Number of Observations	3,400	2,400

Table 8
Robustness Tests Comparing Full Sample vs. LEHD Sample: Regression Analysis

This table reports second-stage results of IV regressions and shows that post IPO-filing growth in a firm's employment, scale, diversification, and wages are similar for two samples: 1) all firms matched to the LBD (Panel A), and 2) the LBD-matched firms that also have employees in our 31 LEHD states (Panel B). Panel A is identical to the results in Table 4 but repeated here for comparison. The sample includes US firms that filed for an IPO from 1992 through the first quarter of 2006. 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). All dependent variables are measured over three 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 are the same as in the first-stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A. Full Sample

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	0.225*	0.058	0.017	-0.038**	0.010
	(0.133)	(0.084)	(0.057)	(0.017)	(0.092)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Number of Observations	3,400	3,400	3,400	3,400	3,400

Panel B. LEHD Sample

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	0.237*	0.135	0.059	-0.045**	-0.057
	(0.142)	(0.097)	(0.065)	(0.020)	(0.098)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Number of Observations	2,400	2,400	2,400	2,400	2,400

Table 9
Robustness Tests Comparing Full Sample vs. LEHD Sample: Headquarter's Location

The table reports second-stage results of IV regressions and shows that post IPO-filing growth in a firm's employment, scale, diversification, and wages, is similar for two types of firms: 1) all firms matched to the LBD, and 2) the LBD-matched firms that also have their headquarters located outside of our 31 LEHD states coverage (indicator variable "HQ State Not in LEHD"). The sample includes US firms that filed for an IPO from 1992 through the first quarter of 2006. Unit of observation is a firm-level aggregation of the establishment-level LBD data in 50 US states and DC. The independent variable, IPO, equals 1 if a firm completed its IPO, and 0 otherwise. IPO is interacted with an indicator HQ State not in LEHD. The IPO indicator and the interaction of IPO and HQ State not in LEHD indicators are instrumented in the first stage with: 1) the NASDAQ return in the 60-day window following the initial IPO filing, and 2) the interaction of HQ State not in LEHD indicator with the NASDAQ return. All dependent variables are measured over three 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 are the same as in the first-stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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	0.294*	0.082	0.015	-0.038*	0.030
	(0.156)	(0.101)	(0.068)	(0.021)	(0.103)
IPO X HQ State Not in LEHD	-0.138	-0.043	0.010	-0.001	-0.059
	(0.139)	(0.083)	(0.057)	(0.018)	(0.093)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Number of Observations	3,400	3,400	3,400	3,400	3,400

Table 10
Placebo: Instrument IPO with Short-Term NASDAQ Returns Prior to IPO Filing

The table reports the results of IV regressions and shows the null results from a placebo test. In this test, IPO completion is instrumented with the NASDAQ returns over the 90-day window *prior* to the IPO filing. The sample includes US firms that filed for an IPO from 1992 through the first quarter of 2006. Unit of observation is a complete firm (columns 1–3), and a firm-level aggregation of workers available in our 31 LEHD states (columns 4–6). In columns 1 and 4, the dependent variable IPO, equals 1 if a firm completed its IPO, and 0 otherwise. In column 2, the dependent variable is the annualized growth in employment, measured over three years following the IPO filing. In column 3, the dependent variable is the annualized growth in industrial concentration, measured over three years following the IPO filing. In column 5, 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. In column 6, the dependent variable is the fraction of entrepreneurial departures, defined as the fraction of workers one quarter before IPO filing who are observed three years later at a firm no more than three years old and who are among the top five earners at that firm. The control variables are the same as in the first-stage IV regressions in Table 3 and include the log of firm employment and average firm wages in the year of the IPO filing, firm age, NASDAQ return in the 90-day window measured prior to the measurement of the placebo 90-day NASDAQ return prior to the IPO filing, and an indicator for whether a firm was acquired over three years following the IPO filing. The parameter estimates for the control variables are not reported due to the US Census restrictions on the number of exported estimates. Per Census Bureau disclosure rules, observations and estimates are rounded. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Full Sample			LEHD Sample		
	1st Stage	2nd Stage		1st Stage	2nd Stage	
Dependent Variable:	IPO	Annualized Growth in Employment$_{t,t+3}$	Annualized Growth in Industrial Concentration$_{t,t+3}$	IPO	New Hire Wage Premium	Departure Rate to Entrepreneurship$_{t,t+3}$
	[1]	[2]	[3]	[4]	[5]	[6]
NASDAQ Return 90 Days Before	0.116			0.094		
	(0.072)			(0.085)		
IPO		-0.576	-0.001		-0.087	-0.052
		(0.645)	(0.054)		(0.131)	(0.138)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	3,400	3,400	3,400	2,400	2,400	2,400
F-statistic	2.586	NA	NA	1.234	NA	NA
p-value of F-statistic	0.108	NA	NA	0.267	NA	NA