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Do Cash Windfalls Affect Wages? Evidence from R&D Grants to Small Firms

Sabrina T. Howell
NYU Stern and NBER, USA

J. David Brown
U.S. Census Bureau and IZA, USA

This paper examines how employee earnings respond to a one-time cash flow shock in the form of a government R&D grant. In a regression discontinuity design, we find that the grant immediately increases average annual employee-level earnings by 2.9%. This benefit accrues only to incumbent employees and rises with job tenure. The grant also affects firm growth, but the initial wage patterns do not appear to reflect growth or productivity. Instead, the evidence supports implicit equity financing within the firm, where employees initially accept lower wages from financially constrained firms and earn more when the firm has ability to pay. (*JEL* G32, G35, J31, J41)

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Much evidence indicates that small firms face financial constraints (Kerr and Nanda 2009; Ferreira, Manso, and Silva 2014; Bellon et al. 2021). This could lead to benefits from delaying employee compensation until there is more ability to pay. In this paper, we assess how small, private, high-tech firms share a positive, one-time cash flow shock with employees. We find evidence

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consistent with employees providing implicit equity to the firm through lower initial wages, which are repaid after the firm experiences the cash flow shock.

Existing literature on rent-sharing with employees has focused on whether productivity shocks affect wages using proxies for productivity-induced surplus, such as value-added, profits, sales, and patent grants.[1] Thus far, it has been difficult to disentangle the effects on wages of productivity growth from profit-sharing (Card et al. 2018) or to test for evidence of backloaded wage contracts. The ideal experiment would randomly assign cash to firms and observe subsequent wages. We approximate this by evaluating the effects of a government R&D grant program on employee earnings using a regression discontinuity design that compares grant awardees with unsuccessful applicants. The grant can be considered a cash flow shock because there are no restrictions on how it is spent.

We use applications between 1995 and 2013 to U.S. Department of Energy (DOE) Small Business Innovation Research (SBIR) grants. Private ranking data permit a regression discontinuity design. The grant amount is uniform within a given year, at $150,000 in recent years, or about $22,000 per employee as of the year before the award. Awardees are not required to use the money as outlined in their applications, nor are their expenditures monitored ex post. We link applicants to U.S. Census Bureau data on the firms and their employees, including employee-level Internal Revenue Service (IRS) W-2 annual earnings data. Note that the term "earnings" in this paper refers to worker, not firm, earnings. As these firms appear to primarily employ full-time workers (discussed further below), we sometimes use the term "wages" instead of "earnings," following convention in the literature.[2] A benefit of these data is that they provide a well-defined and fairly homogenous sample of small, high-tech U.S. firms. It is important to understand how such firms set wages, because this type of firm is crucial to new job creation.[3]

We first examine the effects on average earnings. At the employee level with employee fixed effects, the grant increases earnings by 2.9%. To benchmark the effect on wages against the literature, we show that the implied rent-sharing elasticity from the employee-level estimate is 0.07, which is smaller than the seminal estimate in Van Reenen (1996) but similar to recent estimates with employee-level data, such as Card, Devicienti, and Maida (2014). The effect at the firm level is larger, at 11%, because it weights smaller firms more heavily and they experience larger effects. The positive impact of the grant begins in the quarter following the grant award, is larger in the first 2 years, and endures with statistical significance for at least 5 years. The effect is similar using the award

1 In addition to work cited below, this literature includes Blanchflower, Oswald, and Sanfey (1996), Abowd, Kramarz, and Margolis (1999), Card, Devicienti, and Maida (2014), and Mogstad et al. (2017).

2 We do not observe equity compensation, though exercised options and bonuses are included. (However, the vast majority of private firms – even high-tech, young ones – do not grant stock to nonowner employees.)

3 See Decker et al. (2014).

amount per employee rather than an award indicator and does not appear to reflect more hours worked. For the average firm, increased wages account for the entire grant amount about 15 years after the award. These results indicate that in the short run, the firms share some of the cash flow shock with workers.

Next, we show that these wage increases accrue to incumbent employees only. New employees, hired in or after the year of the award, do not benefit and the difference between incumbents and new hires is statistically significant. Despite wide variation in preexisting incumbent wages, the effect among incumbents is similar across their wage distribution. Among incumbent employees, the only large and robust source of heterogeneity is in years of tenure at the firm (see Figure 3). The grant effect strongly and linearly increases in a worker's tenure, a relationship that is not driven by owners and does not attenuate with controls for observed skill proxies, including wage, age, total career experience, and education.

Instead, the evidence is most consistent with employment relationships compensating for financial frictions. While we would not expect pass-through to wages among large, unconstrained firms (Azariadis 1988; Dharmapala, Foley, and Forbes 2011), the firms in our setting are financially constrained and we observe larger effects among those that are particularly constrained. For example, we find that the effect is much larger among firms that report not having access to expansion capital. This unusually direct measure of constraints comes from matching our data to the U.S. Census Bureau Survey of Business Owners, which asks about sources of financing. We also find larger effects among firms that are younger, smaller, and not VC-backed, which we expect to be more constrained.

If the firm is financially constrained but can commit to long term contracts, employees can offer financing to the firm. The employee initially agrees to be underpaid relative to some benchmark (such as his outside option) in exchange for a higher wage later when the firm's situation improves. Our findings that wage increases accrue only to incumbent workers and increase in job tenure are consistent with the firm paying back the worker after a windfall. Two further predictions of this model are satisfied: The effect is larger among firms that paid below-market wages and grew faster before the grant application, as well as for those employees who started when the firm was relatively small. Also, long-tenure incumbent workers appear to pay a "constrained employer" penalty when they start at the firm relative to the pay at their previous job, consistent with having accepted a backloaded contract.

We consider whether the implicit financing contract we observe best reflects equity or debt. The existing literature on financing within the firm, notably Guiso, Pistaferri, and Schivardi (2013), has focused on implicit debt contracts within mature, steady-state firms. Repayment is on a fixed schedule independent of firm performance, consistent with conventional debt contracts with external lenders. The contingent nature of repayment that we observe, where there is repayment after a cash flow shock, indicates some risk sharing.

This is much better described by an equity contract, which is to our knowledge is a novel insight and is appropriate to our sample of high-tech, small firms that typically primarily receive external finance in the form of equity rather than debt.

Through an equity lens, when the cash flow shock increases the value of the firm, equity-holders should benefit in proportion to their equity. Long-tenured workers have the largest claims and thus should experience the largest effects. Consistent with this, we observe that the increase in earnings rises with tenure in a linear fashion, with the largest increase among workers who have been with the firm for more than 10 years. In contrast, any repayment that might occur in a debt model should be concave in tenure, with intermediate tenures having the largest claims.

In a back-of-the-envelope calculation, we show that 2 years after the grant, long-tenure incumbent workers earn a premium for having accepted the backloaded contract, assuming a 5% opportunity cost of capital. This is most consistent with an equity interpretation where employees accrue rights to future cash flows, helping to explain why the one-time shock might have at least medium-term effects on wages for long-tenured incumbent employees.

Why doesn't the firm renege on backloaded wage contracts? There are two potential enforcement mechanisms: retention and reputation. If the incumbent worker can threaten to quit, her firm-specific capital operates as implicit collateral (Michelacci and Quadrini 2009). Consistent with this mechanism, we show that incumbents who receive higher wage gains are more likely to stay with the firm. Second, firms may seek to maintain credibility and reputation, which have long-run benefits (Lazear 1989; Kahneman, Knetsch, and Thaler 1986). This mechanism predicts that firms which do not fulfill old equity contracts will find it more difficult to finance themselves from new hires. Indeed, we find that firms that shared less of the award with incumbents are less likely to borrow from new workers.

To assess whether this mechanism is used in practice among firms in the data, we conduct a survey of DOE SBIR grantee principal investigators, who are almost always company CEOs. The survey asks whether the firm had used backloaded wage contracts because of financial constraints. The results indicate that the mechanism is used in practice, with 55.6% of respondents replying yes, 21.2% no, and 23.2% not explicitly answering the question.

We find that the grant positively affects employment and revenue. While these gains might help sustain higher wages in the longer term, it does not appear that higher growth or productivity explain the immediate pattern of wage effects. First, there is no effect on labor productivity. Second, subsequent revenue, employment, and productivity growth are uncorrelated with the wage effects. Third, the full earnings effects appear within two quarters, while only part of the long-term revenue effect occurs during the first 2 years. Fourth, a productivity channel would predict effects among new employees, especially after they have been with the firm for several years, since there is strong evidence of firm fixed effects in wage-setting that are driven by overall firm productivity (Barth et al. 2016; Card, Cardoso, and Kline 2016; Song et al. 2018). In sum, while the grant may have other effects on the firm, including on its long-term productivity, changes to productivity alone do not seem to explain the initial effects on wage patterns.

We consider other channels that might explain why wages increase after a cash flow shock: bargaining power, incentive contracting, agency frictions, match quality, and efficiency wages. The evidence is inconsistent with any of them playing a primary role, though we do not rule out that they may be present in a secondary capacity, and clearly they play a role in wage-setting in general.

Our paper builds primarily on two previous literatures: financing within the firm and rent-sharing with employees. Theoretical work shows how constrained firms may borrow from employees by delaying wage payments (Garmaise 2007; Michelacci and Quadrini 2009; Sun and Xiaolan 2019). Guiso, Pistaferri, and Schivardi (2013) show that in Italian provinces with less developed credit markets at the time of hiring, wages increase with tenure more than in provinces with more developed credit markets. They argue that the average wage patterns reflect borrowing, where postponed wages substitute for bank debt and are repaid on a fixed repayment schedule over time regardless of firm performance. In contrast, we study how wage profiles dynamically react to a cash-flow shock. The payouts from a cash inflow that we see are more consistent with an equity-like contract.

Our interpretation of the wage increase as compensation for holding implicit equity departs from the large rent-sharing literature on how productivity and profits pass through to worker pay, including Van Reenen (1996), Black and Strahan (2001), Fuest, Peichl, and Siegloch (2018), and Kline et al. (2019).[4] Kline et al. (2019) find that a patent award leads to higher wages, reflecting superior innovation and thus higher productivity (they do not instrument for getting a patent). They explain the results with a bargaining model that relates productivity with wages in a static way. The main overlap with our findings is that their effects are concentrated among incumbent workers; however, their effects exist only in the top half of the wage distribution, consistent with these workers having more bargaining power. In contrast, we observe effects across the wage distribution. We focus on worker tenure and its relationship to financial constraints, neither of which appear in Kline et al. (2019). This enables us to present the novel insight that observations of "profit-sharing" may in part reflect the dynamic nature of backloaded wage contracts. One other advantage of our setting is a particularly clean cash flow shock; while the shock may have permanent effects, it offers new insights relative to the existing work on productivity increases.

[4] See also Toivanen and Väänänen (2012), Macis and Schivardi (2016), Bergman, Iyer, and Thakor (2017), Friedrich et al. (2019), Lamadon, Mogstad, and Setzler (2019), Saez, Schoefer, and Seim (2019), and Ku, Schoenberg, and Schreiner (2020).

More broadly, this paper contributes to work on the relationship between finance and labor, particular under conditions of financial constraints (Matsa 2010; Benmelech, Bergman, and Seru 2011; Pagano and Pica 2012; Ellul and Pagano 2019). Our paper also joins studies of how firms spend cash in the presence of frictions (e.g., Hennessy and Whited 2007; Erel, Jang, and Weisbach 2015). Starting with Fazzari, Hubbard, and Petersen (1988) and Hoshi, Kashyap, and Scharfstein (1991), the literature has focused on investment (see also Faulkender and Petersen 2012; Gilje and Taillard 2016; Cespedes, Huang, and Parra 2019). This paper examines the labor side.

1. Empirical Setting

In this section, we explain the setting (Section 1.1) and data sources (Section 1.2). We then describe summary statistics (Section 1.3).

1.1 Institutional context

This paper uses data on applications and awards from the U.S. DOE SBIR grant program. Congress first authorized the SBIR program in 1982 to strengthen the U.S. high technology sector and support small firms. Today, the law requires 11 federal agencies to allocate 3.2% of their extramural R&D budgets to the SBIR program. The law also stipulates that the SBIR program has two phases. Phase 1 grants of $150,000 are supposed to fund 9 months of proof-of-concept work. Eligible firms are for-profit, U.S. based, and majority U.S. owned. There is no required private cost sharing, and the government takes no equity and demands no rights to IP. The application process is onerous, taking a full-time employee 1 to 2 months.[5] The firm proposes to use the grant for R&D in its application, but there is no monitoring or enforcement once the firm receives the lump sum.[6]

Each year, DOE officials in technology-specific programs (e.g., Solar) announce competitions in granular subsectors. The officials then rank applicants within each competition based on written expert reviews and their own discretion, according to three criteria: (a) strength of the scientific/technical approach; (b) ability to carry out the project in a cost effective manner; and (c) commercialization impact (Oliver 2012). The program official does not know the award cutoff (the number of grants in a competition) when she conducts the ranking. She submits ordered lists to a central DOE SBIR office, which determines the cutoff.[7]

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5 Applicants must describe the project and firm in detail and submit an itemized budget for the proposed work. The DOE's SBIR Phase 1 application website contains over 100 pages of instructions. Interviews with grantees confirmed the 1- to 2-month time-frame.

6 Phase 2 grants of $1 million, awarded about 2 years after Phase 1, aim to fund later-stage demonstrations. There is adverse selection in Phase 2 application, and 40% of winners do not apply to Phase 2 (Howell 2017). Consistent with Howell (2017), we find no effects of the Phase 2 grant (results are available on request).

7 The cutoff in a competition is based on budget constraints. Ranking occurs before the SBIR office determines how many awards to allocate to each program and competition. Interviews with DOE officials have indicated that

By virtue of their status as applicants to DOE's SBIR program, at the time they apply the firms in the sample are engaged in some sort of innovation activity related to energy, and they must be relatively small. They tend to be focused on a specific technology, rather than being diversified. Many can be described as high-tech startups. A drawback is that the sample is not representative of all U.S. firms (we discuss representativeness below). However, there are two important benefits. First, these firms are of a type that is an important engine of economic growth. Second, their common characteristics make them more comparable, which is helpful for our identification strategy.

1.2 Data sources

We use complete data from the two main applied offices at the DOE: Fossil Energy (FE) and Energy Efficiency and Renewable Energy (EERE). Together, they awarded US$(2012)884 million in SBIR grants between 1983 and 2013. In the data used in this paper, there are about 270 competitions (all reported counts are rounded to comply with Census disclosure requirements). Each competition has on average about 16 applicants and three winners.[8] We observe the applicant's company name, address, funded status, and award notice date. While awards are public information, the ranks and losing applicant identities are indefinitely secret. Ranking data exist from 1995, so analysis begins then. For additional details and summary statistics about the application process and data, see Howell (2017).

The application data are matched to the U.S. Census Bureau's Business Register, which contains all business establishments in the U.S. private nonfarm sector with at least one employee, by EIN (when available) or probabilistic and then clerical matching on name, address, and zip code. About 70% of firms are matched successfully. We err on the side of including only matches that we were confident are correct, to avoid an excess of false positives. Based on observable characteristics in the DOE data, there is no clear bias in matching, and match rates are similar by rank around the cutoff.

Once a link to a Business Register record is established, we connect the firms to other Census Bureau data sets. One is IRS W-2 data, which contain annual earnings for each employee. These data begin in 2005 and end in 2013. We observe only earnings, not hourly wages. The earnings should be thought of as salary income, as most of the jobs in this sample appear to be full-time

the cutoff decision is exogenous to the ranking process. Some ranking data provided in the form of e-mails from program officials to the SBIR office also support exogeneity. Observable variables do not predict competition cutoffs. Average award numbers do not vary systematically by office or competition subsector. The budget for each contest is set at the beginning of the year based on the budget for the program office (e.g., Solar), which overwhelmingly goes to other line items, like the national labs.

8 Our main analysis focuses on Phase 1 grants. As in Howell (2017), we find no effects of Phase 2, and the sample is much smaller.

jobs.[9] While bonuses or stock exercises would appear in W-2 earnings, we do not observe equity compensation. However, the vast majority of private firms – even high-tech, young ones – have no expectation of a liquidity event, such as an acquisition or initial public offering (IPO) and do not grant stock to nonowner employees.[10]

We connect the data to three further Census Bureau data sets. The first is the Longitudinal Business Database (LBD), which spans 1976 to 2015 and includes the universe of nonfarm, nonpublic administration business establishments with paid employees. We use three outcome variables from the LBD. The first is employment, observed quarterly after 2004 (before 2004, it is observed once per year). The second is payroll, observed quarterly throughout. The third is revenue, observed annually starting in 1996. Second, we use demographic information from the Individual Characteristics File. This describes employees of all firms in the Longitudinal Employee Household Dynamics data set, which has similar coverage to W-2s. Third, we use data from the Survey of Business Owners (SBO), which includes all nonfarm tax-paying businesses. This provides information about sources of startup or acquisition capital as well as sources of expansion capital in the year of the survey. We consider the 2002, 2007, and 2012 surveys, employing the closest survey year to the award year for matched firms.[11]

1.3 Summary statistics

Table 1 presents the main summary statistics. We first compare key statistics about our firms to those of the U.S. population, which sheds light on representativeness. Among the 2,100 unique applicant firms, the average number of employees across all firm-years is 35, and 6.8 in the year before the award year. In comparison, the average U.S. firm in 2012 had 20 employees.[12] The firms in our data are small using the Small Business Administration's definition (independent, privately owned, with fewer than 500 employees). Small businesses account for over 99% of all U.S. employer firms, half of total U.S. employment, and 64% of net new private-sector jobs.[13] Among these,

[9] This assumption is based on the relatively high average wages, and the fact that the vast majority of jobs in the applicant firm sectors are full-time. According to the CPS, the share of workers who are full-time in Information, Professional and Business Services, and Durable Goods Manufacturing are 82%, 84%, and 90%, respectively. See https://www.bls.gov/cps/cpsaat21.htm.

[10] For example, Robb and Robinson (2014) show that just 4% percent of young firms receive outside equity in a large, representative survey of U.S. firms started in 2004 that oversampled high-tech firms. Coleman and Robb (2011) use the same survey to show that high-tech firms have lower rates of outside equity than do low-tech firms.

[11] Aggregate data and information on the SBO are available here: https://www.census.gov/programs-surveys/sbo/data.html.

[12] https://www.census.gov/data/tables/2012/econ/susb/2012-susb-annual.html

[13] https://www.sba.gov/sites/default/files/FAQ_Sept_2012.pdf

Table 1
Summary statistics

A. SBIR Phase 1 competition data (counts)

	N
Unique applicant firms	2,100
Applications	4,300
Grant award winners	800
Grant award non-winners	3,600
Competitions	270

B. Firm-year variables

Levels statistics	N	Mean	SD	Median
Payroll (millions 2010 $)	30,500	2.5	6.1	0.7
Employment	30,500	36	72	12
Employment$_{t=-1}$	30,500	6.9	4.5	17
Award amt/emp$_{t=-1}$ (thousands 2010 $)	30,500	22	34	9.1
Average earnings (thousands 2010 $)	30,500	64	39	58
90/10 log earnings differential	9,600	1.8	1.1	
99/50 log earnings differential	9,600	.95	.7	
SD of *log(earnings)*	9,600	0.86	.33	
Revenue (millions 2010 $)	13,000	4.8	11	
Firm age	30,500	12	8.5	
Subsequent patent citations (3-year window)	30,500	2.1	11	
Never previously won an award	30,500	.57		

log(Growth statistics) (base is $t=-1$)	N	Mean	SD	Median
Payroll	30,500	−.11	1.2	−.0015
Employment	30,500	−.082	1	0
Earnings	30,500	−.023	.83	0
Revenue	13,000	−.048	1.1	
Productivity	13,000	.0097	.85	0
90/10 differential	7,500	−.0015	.98	
99/50 differential	7,500	.0028	.6	
Standard deviation	7,500	.0048	.33	

These panels show summary statistics about the SBIR data that were matched to U.S. Census data. Growth measures use the year before the application year as the base year ($t=-1$). Application year is first application year if the firm never won a grant, and first winning year if it ever won. Median is calculated as the average of the 49th and 51st percentiles, as statistics associated with a specific firm or individual may not be disclosed. It was not disclosed for all variables. The numbers of observations are rounded to meet Census disclosure requirements. This table reports results from disclosures CMS request 7276 and CBDRB-FY19-452. (*Continued*)

high-tech and potentially high-growth small firms are especially important for employment growth (Decker et al. 2014).

The average firm revenue in the sample is $4.8 million, with a right-skewed distribution (all dollar amounts are in 2010 dollars). This is reasonably aligned with U.S. averages, which are $779,000 for firms with less than 20 employees, and $7.9 million for firms with 20-99 employees. Average payroll in our data is higher than the average for U.S. firms with 20-99 employees, at $2.5 million relative to $1.6 million. Average earnings are also higher, at $64,150 relative to $40,417 across all U.S. firms with 20-99 employees in 2012. The within-firm standard deviation about 60% of the mean. These differences indicate that the firms in the data have relatively high-skill employees. Average firm age is 12 years, but in the year before the application, it is 8.3 years. As we might expect

Table 1
(Continued)

C. Employee variables (SBIR applicant firms)

	N	Mean	SD	Median	Level of observation
# unique individuals in sample	73,000				Person
Earnings at SBIR firm (thousands 2010 $)	257,000	64	86	50	Person-year
Earnings all jobs (thousands 2010 $)	909,000	59	84	44	Person-year
Earnings preaward (incumbents only, thousands 2010 $)	41,000	50	80	39	Person-year
Tenure at SBIR firm (years)	257,000	3.9	3.1	3	Person-year
Share of workers with ≥ 5 years tenure	38,000	.39			Person
Total tenure at SBIR firm (years)	38,000	4.8	3.9	4	Person
Percent raise	62,000	0.24	1.3	−.061	Person
As of 2nd year after award, firm # of:					
Incumbent employees	2,300	6.7	12	5	Firm
New2 employees	2,300	4	24	0	Firm

D. Employee characteristics by incumbent or new hire status

	Incumbent workers		New hires		
	N	Mean	N	Mean	p-value for diff of means
Employee-level within 2 yrs of award yr					
HighEduc (BA or above)	49,500	.45	11,500	.36	.00
Age (years)	49,500	43	11,500	36.99	.00
Earnings (thousands 2010 $)	49,500	69	11,500	40	.00
Percent raise (thousands 2010 $)	49,500	0.224	11,500	0.243	.09
Firm-level, all years					
10th pctile earnings (thousands 2010 $)	8,200	19	3,200	13	.00
50th pctile earnings (thousands 2010 $)	8,200	41	3,200	23	.00
90th pctile earnings (thousands 2010 $)	8,200	77	3,200	45	.00
99th pctile earnings (thousands 2010 $)	8,200	95	3,200	51	.00

These panels show summary statistics about employees at SBIR applicant firms. Incumbent employees are those present at the firm in the year of grant application. New employees are those hired after the year of grant application. Percentage raise is the change in earnings between the last year of the previous job and the first year at SBIR firm. Growth measures use the year before the application year as the base year (base is $t=-1$). Application year is first application year if the firm never won a grant, and first winning year if it ever won. Median is calculated as the average of the 49th and 51st percentiles, as statistics associated with a specific firm or individual may not be disclosed. The statistics for tenure are very similar when restricted to the award year and thus only to incumbent workers. The numbers of observations are rounded to meet Census disclosure requirements. This table reports results from disclosures CMS request 7276, CBDRB-FY19-452, CBDRB-FY2020-CES005-023, CBDRB-FY19-452, and CBDRB-FY2022-CES005-012.

for applicants to an R&D grant program, the most common NAICS three-digit industry is Professional, Scientific, and Technical Services, at 62% of firms.[14] The next most common is Computer and Electronic Product Manufacturing, at 7.9%. Overall, the firms in our data ought to be roughly representative of small, high-tech, financially constrained firms.

14 Industry is a firm-year variable because industry assignations may change over time within a firm. Industry is based on six-digit NAICS codes. Where a firm has multiple units, and therefore potentially multiple industries, we use the NAICS associated with the firm's largest employment share.

Table 1, panel B, shows outcome and control variables. The primary measure of within-firm wage inequality is the 90/10 ratio, or the logarithm of the wage difference between the 90th percentile and the 10th percentile (see, e.g., Van Reenen 2011). We also use the 99/50 ratio as a proxy for upper-tail inequality, as well as the standard deviation. Logged growth measures are defined as the logarithm of the difference of an outcome in a given year relative to the year before application ($t=-1$): $Growth_{i,t} = \ln\left(\frac{Y_{i,t}}{Y_{i,t=-1}}\right)$. These measures have small, negative means, implying that they tend to be larger in the year before application compared to other years. This is because firms on average grow over time. Therefore, the outcome measures are on average lower in the years before the application than in the preapplication year, and there are more observations in this preapplication period. Note that the number of observations reflect data availability. Some statistics require W-2 data, which are only available after 2005. Revenue is available in the LBD only for a subset of firms.

Table 1, panel C, reports the employee-level statistics. The average earnings among all employees at SBIR applicant firms is \$63,500. It is somewhat smaller when all jobs are included beyond the SBIR firms. At the employee-year level, average job tenure is almost 4 years. Consistent with existing work, tenure is correlated with wages; the correlation coefficient is 0.33. By the second year after the award year, the average firm has almost seven incumbent employees–who were present at the firm before the award year–and four new employees. (The "award year" includes firms that did not win; it refers to the year the award decision was announced.) These statistics reflect a skewed distribution in which some firms grow fast while others exit, which is typical of young, high-tech firms.

Incumbent workers earn a lower wage than the overall sample in the year before the award year, at \$50,420. The standard deviation of wages is quite high preaward decision, at \$79,940, or 160% of the mean. Incumbent workers are more educated, older, and earn more than new employees. In the years after the award, incumbents earn a higher wage than the overall sample, at \$68,980. However, they received a smaller average wage increase relative to their previous jobs. Regarding demographics, the average worker is 43 years old, and 22% of employees are women (not tabulated).

Last, Table 2 presents the summary statistics about the SBO match. Sources of expansion capital are reported for the year of the survey, while startup capital is at business launch or acquisition. The left columns describe the SBIR firm data, including only financing sources with a large enough sample to disclose. The right column shows the all-U.S. figures.[15] Consistent with the firms in this sample being financially constrained, the SBIR firms are about twice as likely as all U.S. firms to report having no access to expansion capital, are more likely

[15] We report summary statistics at the employee-year level, which gives the variable used in the regressions. It is not possible to disclose these statistics at the one-per-firm level, but the means are quite similar. There are about 800 unique firms in the matched sample.

Table 2
Sources of startup and expansion capital

	Sources of expansion capital			Sources of startup capital		
	SBIR analysis sample		All U.S.	SBIR analysis sample		All U.S.
	N	Mean	Mean	N	Mean	Mean
No access	90,500	.032	.014			
Profits	90,500	.3	.082			
Credit card	90,500	.024	.107	90,500	.044	.097
Bank loan	90,500	.23	.076	90,500	.074	.099
Personal savings	90,500	.074	.258	90,500	.036	.575
Personal assets (Nonsavings)	90,500	.033	.039	90,500	.12	.075
None needed	90,500	.23	.54	90,500	.038	.245
Government-guaranteed loan	90,500	.032	.003			.007
Home equity loan			.031			.044
Direct government loan			.004			.006
Loan/investment from family/friends			.009			.023
VC			.002			.003
Grants			.003			.003
Other			.009			.021
Don't know			.063			.050

This table describes data from the Census Survey of Business Owners. The left columns contain statistics for firms appearing in the survey that matched to the SBIR applicant firms that we use in the analysis. We use the survey year closest to the award year. The right columns show the publicly available all-U.S. figures averaged across the three survey years (there are no major differences across years). Here, we show all the options from the survey, where the bottom section of variables are those not used in our analysis. We do not use a variable in the analysis when the matched set of firms was too small for a binary variable to be disclosed. Data are at the employee-year level, as in analysis. There are about 800 unique firms in the matched sample. Data and information on the SBO are available here: https://www.census.gov/programs-surveys/sbo/data.html. This table reports results from disclosure CBDRB-FY2022-CES005-012.

to use profits for expansion capital, and are less likely to need no capital. They are also less likely to use credit cards or personal savings, consistent with the businesses being relatively high-tech, risky, and capital intensive.

2. Estimation Approaches

The ideal experiment would randomly allocate cash to a subset of firms. We approximate this using a regression discontinuity (RD) design, which estimates a local average treatment effect around a cutoff in a running variable. A valid RD design requires that treatment not cause rank, which is not a problem here, as the award decision happens after ranking and previous winners are excluded. Ranks are ordinal, and since on average the differences in the true distance between ranks should be the same, errors in differences on either side of the cutoff should average zero. The primary concern is whether firm ranks are manipulated around the cutoff (Lee and Lemieux 2010). Howell (2017) provides five tests for manipulation, a discussion about and test of the discreteness of the rating variable, and extensive evidence of continuity of observable baseline covariates around the cutoff. In our setting, we confirm that before applying, the awardees and nonawardees have similar observable characteristics, such as moments of the wage distribution and employee education.

The primary employee-year-level specification for evaluating the effect of a grant award on earnings at the employee level is shown in Equation (1). Here and below, i denotes a firm, k an employee, j a competition, and t a year.

$$W_{i,k,t} = \beta PostAward_{i,j,t} + \delta Post_{j,t} + f\left(Rank_{i,j}\right) + \eta_1 Age_{i,t} + \eta_2 Age^2_{i,t} + \lambda_k + IndYear_{i,t} + MSAYear_{i,t} + \varepsilon_{i,j,t}. \tag{1}$$

A firm that ever wins a grant is assigned the non-time-varying indicator $Award_{i,j} = 1$.[16] The variable $Post_{i,j,t}$ is an indicator for the year being after the year the firm applied, and $PostAward_{i,j,t}$ is the interaction between $Post_{i,j,t}$ and $Award_{i,j}$. Some firms apply multiple times, and some of these firms become multiple-time grant winners. Our primary approach includes winning firms only once, for their first grant.[17] Award is not identified because it is defined at the firm level and employees appear only while at the SBIR applicant firms, so employee fixed effects soak it up. Across all analysis, standard errors are clustered by firm.

We center ranks around zero because the number of applicants and awards varies across competitions. The lowest-ranked winner has $Rank_{i,j} = 1$, and the highest-ranked loser has $Rank_{i,j} = -1$. Howell (2017) shows that rank is uninformative about outcomes, and this remains true in our setting. Therefore, bandwidths of one firm or all firms around the threshold yield very similar point estimates. We do not report models with narrow bandwidths around the cutoff due to disclosure limitations. We control for rank quadratically, linearly separately among winners and nonwinners or with a triangular kernel. The latter approach weights observations far from the cutoff less than those close to the cutoff, weakening the parallel trends assumption for winners and losers and thus validating the difference-in-differences design. Following DiNardo and Tobias (2001), we use the formula $Kernel_{i,j} = 1 - \frac{|Rank_{i,j}|}{\max_j |Rank_{i,j}| + .01}$.[18]

The primary model includes employee fixed effects (λ_k) as well as Industry-year and city-year fixed effects, where industry is defined at the three-digit NAICS level and city is defined as an MSA ($IndYear_{i,t}$ and $MSAYear_{i,t}$). These address the possibility that results reflect labor market dynamics at the industry or geographical levels. We also control for the firm's age ($Age_{i,t}$) and age

16 We use an indicator for winning a grant rather than the award amount per employee because employment is an outcome variable, creating potential concerns about endogeneity. However, we use award per employee in robustness tests and to explore whether the effect on a per-worker basis exhibits constant elasticity across firm sizes.

17 We do allow firms to appear multiple times when their additional applications are losing ones. That is why with employee fixed effects the firm's rank is identified.

18 We add 0.01 so that the observations with the max absolute rank don't end up with a weight of zero (which would cause them to drop out of the regression).

squared. Note that if the RDD intuition is correct, controls are not technically necessary; consistent with this, none of the controls affects the estimates much.

For some tests, we use firm-year panels and estimate Equation (2):

$$W_{i,t} = \beta PostAward_{i,j,t} + \gamma Award_{i,j} + \delta Post_{j,t} + f\left(Rank_{i,j}\right) \quad (2)$$
$$+ \eta_1 Age_{i,t} + \eta_2 Age_{i,t}^2 + CompYear_{j,t} + IndYear_{i,t} + MSAYear_{i,t} + \varepsilon_{i,j,t}.$$

Here, $W_{i,t}$ represents log average earnings at the firm. The remaining variables are as described above, except that instead of employee fixed effects we use competition-year fixed effects ($CompYear_{j,t}$). These soak up the variable $Post_{j,t}$, so it is not reported. In a robustness test, we use firm-application fixed effects ($\lambda_{i,j}$), which subsume rank, award, and competition controls. Following Lee and Lemieux (2010), we do not use firm fixed effects in the main models, although the results are generally robust to this approach.

We graphically present results from two additional specifications. First, we show the firm-level effects by rank around the cutoff for the award using Equation (3):

$$W_{i,t} = \sum_{x=-6}^{x=3} \beta_x \left(Post_{j,t}\right)\left(Rank_{i,j} = x\right) + \eta_1 Age_{i,t} + \eta_2 Age_{i,t}^2 + Year_t + \lambda_j + \varepsilon_{i,j,t}. \quad (3)$$

This model uses competition fixed effects (λ_j), to focus on highlighting within-competition variation in the effect. Second, we show the effects by quarter around the award quarter using Equation (4), where q denotes the quarter:

$$W_{i,q} = \sum_{x=-13}^{x=13+} \left[\beta_x \left(Award_{i,j} = 1\right)(q = x) + \delta_x (q = x)\right] + Quarter_q + \lambda_{i,j} + \varepsilon_{i,j,q}. \quad (4)$$

The coefficients of interest, β_x, are on the quarter indicators interacted with the award dummy, and these are shown in the graph. Here, we include firm-application fixed effects, which create more noise but offer the most stringent specification, as they control for all possible application and firm characteristics.

All the models described above use panel data, which offer several advantages. First, while Howell (2017) provides evidence of continuity around the threshold for winning, the discreteness of the running variable (a firm's rank in a competition) means that we cannot affirmatively establish local continuity. Frandsen (2014) shows how a panel setting can add a differences-in-differences aspect to the RD design, enabling the much weaker condition of local continuity in differences, and local continuity conditional on characteristics. While the data in Howell (2017) did not permit a panel approach, the richness of the U.S. Census Bureau data does. We can use fine controls and growth specifications, lending additional validity to the empirical design. The panel setting also follows related wage literature more closely (e.g., Guiso, Pistaferri, and Schivardi 2005; Cardoso and Portela 2009). For

Table 3
Grant effect on earnings (employee level)

Dependent variable: *log(earnings)*

Sample:	2-year window				
	(1)	(2)	(3)	(4)	(5)
PostAward	.029**	.032*	.028**	.041**	.026
	(.015)	(.015)	(0.016)	(.024)	(0.017)
Post	.00092	.0095	.0011	.0039	.0044
	(.007)	(.007)	(.0069)	(.0098)	(.007)
PostAward · YearsFromAward					−0.00073
					(0.0037)
YearsFromAward					.0063***
					(0.002)
Rank, Rank^2	Y	N	N	N	N
Kernel weight by rank	N	N	Y	N	N
Age, Age^2	Y	N	Y	N	N
Employee FE	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y
MSA-year FE	Y	Y	Y	Y	Y
Firm-application FE	N	Y	N	N	N
N	257,000	257,000	257,000	95,000	257,000
R^2	.772	.77	0.77	.87	.77

This table shows the effect of the grant on employee earnings, using Equation (1). Column 2 uses firm-application fixed effects, which absorbs rank and competition. Column 3 uses a kernel weight by rank. Column 4 restricts the post sample to the 2 years after the grant application year (this includes the application year). Column 5 interacts the effect with years from the award. Note that $Award$ is defined at the firm level, so is absorbed by employee fixed effects. Control coefficients are not reported to minimize disclosure requirements. Data are observed at the employee-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. *p < .1; **p < .05; ***p < .01.

both firm- and employee-level analyses, the panel is unbalanced, so that individuals and firms that exit disappear from the sample. We find no effects of the grant on firm exit, employee retention, or employee departure from the sample (most likely to unemployment), so the unbalanced panel should not bias the results. We also find similar results in a nonpanel setting in which each observation is an application (not reported).

3. How the Grant Affects Earnings

In this section, we first present the effect of a grant on earnings, then decompose it across incumbent employees and new hires. Next, we consider employee characteristics, especially tenure, and last examine the effect of the grant on within-firm wage inequality.

3.1 Average earnings

We report estimates of Equation (1) in Table 3. These models use employee-level data and include employee fixed effects. Our preferred estimate using the standard quadratic rank control is in column 1. It shows an effect of 2.9%, which translates to a \$2,032 increase in the mean wage over the whole sample or a \$1,462 increase in the mean wage of incumbents before the award. Using the latter interpretation, the grant can be "accounted for" entirely through wage increases after 15 years. We adjust controls in the two following columns. Column 2 includes firm-application fixed effects, which absorb controls for rank and competition. Column 3 controls for rank with a kernel weighting.

The effect occurs quickly and is a bit higher in the short term, at 4.1% within a 2-year window (Table 3, column 4). Yet the effect also persists over time. In column 5, we interact winning with the number of years since the award. The interaction is precisely zero. The independent effect of PostAward is 2.6% and just barely statistically insignificant, reflecting the difficulty of identifying an effect with both a years-from-award control and granular time-varying fixed effects. This model shows that relative to nonwinning firms, wages at winning firms jump up and persist at the higher level, rather than experiencing an increase that either attenuates or grows over time. Since wages increase over time on average, the coefficient for years from award is positive.

Table A1 in the Internet Appendix reports firm-level regression results using Equation (2). The estimates are larger, for example 11% in our preferred model (column 1) because small and large firms are equally weighted and the effect is larger among smaller firms, a finding discussed below (this does not reflect Jensen's inequality). The remaining columns present robustness tests, including using earnings growth relative to the award decision year as the dependent variable (columns 5-9). Figure 1, panel A shows the effect by rank around the cutoff, using Equation (3). Panel B shows the effect on earnings by quarter around the award quarter, using Equation (4), and confirms that the effect is immediate but persists over time.

To benchmark these findings against the rent-sharing literature, we approximate a firm rent-sharing elasticity, where rents are profits in excess of returns to factors of production. To motivate this measure, consider the relationship between profits per worker and wages in a standard bargaining model, where the wage reflects the reservation wage and a share of profits (Card et al. (2018)). When the worker has more bargaining power, there is greater weight on profits. w denotes the wage, o the worker's outside option, $\gamma \in [0,1]$ a rent-sharing parameter, G the rent (here, the grant), and N the number of employees:

$$w = o + \gamma \frac{G}{N}. \tag{5}$$

The elasticity of wages with respect to the profits-per-worker is

$$\xi = \frac{\gamma \frac{G}{N}}{o + \gamma \frac{G}{N}}. \tag{6}$$

To arrive at an estimate of ξ, the literature typically relates a measure of quasi-rents, such as value-added per worker, to wages on an annual basis

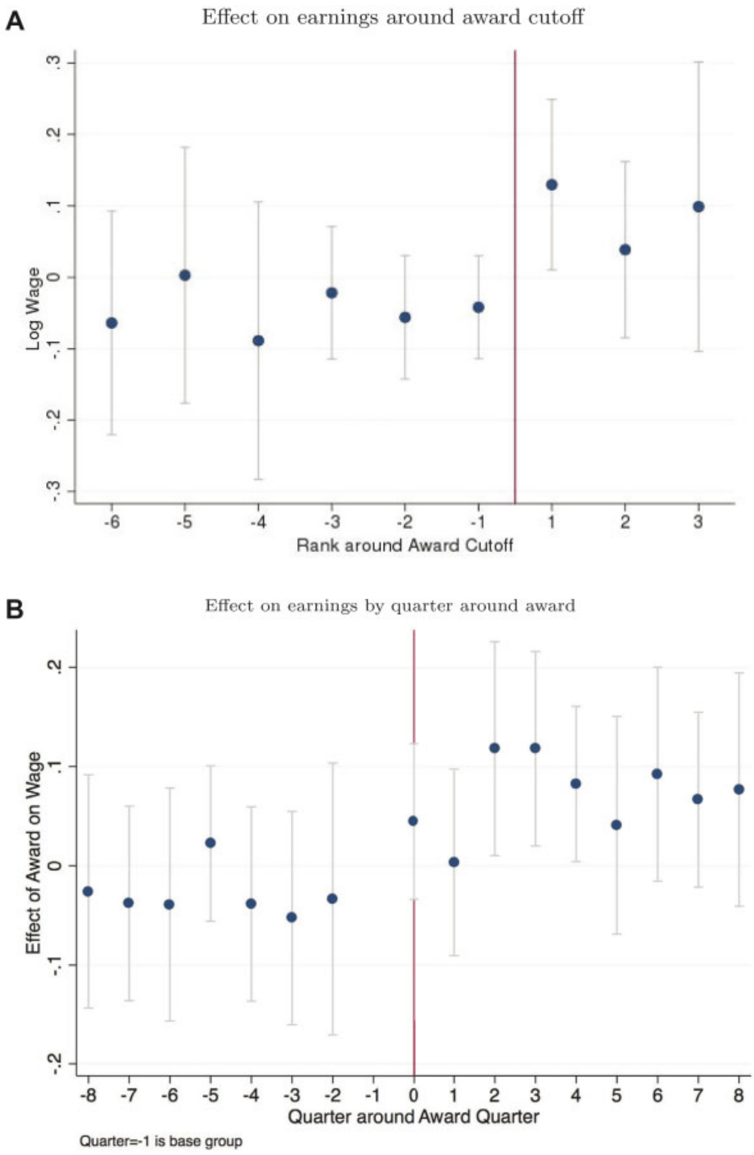

Figure 1
Graphical effects on earnings
Panel A shows the results from estimating Equation (3), where the dependent variable is the logarithm of earnings at the firm-year level. Each point is a coefficient for a specific DOE-assigned rank around the award cutoff, where winning applicants have positive ranks, and non-winning applicants have negative ranks. Panel B shows the results from estimating Equation (4) on quarterly levels of the logarithm of firm-year earnings. Each point is a coefficient for a quarter around the award quarter interacted with winning an award. The base quarter is -1 (immediately before the quarter of award). Ninety-five percent confidence intervals are shown. This figure reports results from disclosure DRB-B0086-CDAR-20180607.

(Card et al. 2018).[19] The parallel in our context is the wage elasticity with respect to the grant in the year following the award. Using the employee-level estimate of 2.9%, and the fact that the average grant per employee in the year before the award year is $21,880, or 43% of the mean earnings among incumbents preaward (Table 1, panel C), we calculate an elasticity ξ of 0.066 (.029/.43). In turn, we can use Equation (6) and the median wage among firms that did not win an award in the year before the award year to proxy for the outside option o to calculate a rent-sharing parameter γ of 0.15.[20]

These elasticities are smaller than estimates using patent-based productivity shocks, likely because the cash flow shock we study is not linked to a jump in productivity. In a seminal study Van Reenen (1996) instruments for profits with innovation and finds a firm-level wage elasticity of about 0.25. Kline et al. (2019) estimate the effect of patent-instrumented surplus on the average wage, and find an an elasticity of 0.35. Kogan et al. (2019) find an elasticity of 0.19 by taking the ratio of patent-wage and patent-profits relationships. Other existing work at the firm level has employed measures of value added per worker, profit per worker, or output/revenue per worker. Estimates based on value-added are roughly one-fifth of our estimate (Card, Devicienti, and Maida 2014; Card, Cardoso, and Kline 2016). Estimates using individual data are closer to our result, including Margolis and Salvanes 2001, Arai 2003, Martins 2009, Gürtzgen 2009, Carlsson, Messina, and Skans (2016), and Bagger, Christensen, and Mortensen (2014). Cardoso and Portela (2009) and Guiso, Pistaferri, and Schivardi (2005) find zero elasticities to changes in value added or sales. The finding in this paper of a positive, immediate elasticity for a one-time cash flow shock is, to our knowledge, new to the literature.

We also estimate the effect of the logarithm of the award per employee in Table A2 in the Internet Appendix.[21] The effect is shown at both employee (columns 1-3) and firm (columns 4 and 5) levels. The coefficients imply roughly the magnitude of the employee-level result in Table 3, because the average award amount per employee of $21,880 is about one-third of the average wage. This approach permits us to ask whether it is appropriate to assume that firms of different sizes share equally on a per-worker basis. In Figure 2, we show the effect of the award per employee for each quintile of firm size, measured as the number of employees in the year before the

19 The above equations assume that $\frac{G}{N}$ is exogenous to the level of wages, which is true when bargaining jointly determines capital and labor. The elasticity is arrived at by differentiating wages with respect to $\frac{G}{N}$, which yields γ, and multiplying by $\frac{\frac{G}{N}}{w}$.

20 To proxy for the outside wage o, we use the mean wage among firms that did not win an award in the year before the award year, as these firms are the best available counterfactual to the winning firms. This mean is extremely similar to the mean among winning firms preaward, consistent with the parallel trends assumption.

21 Specifically, the independent variable of interest is post interacted with the logarithm of the award amount ($150,000) divided by the number of employees at the firm in year $t=-1$.

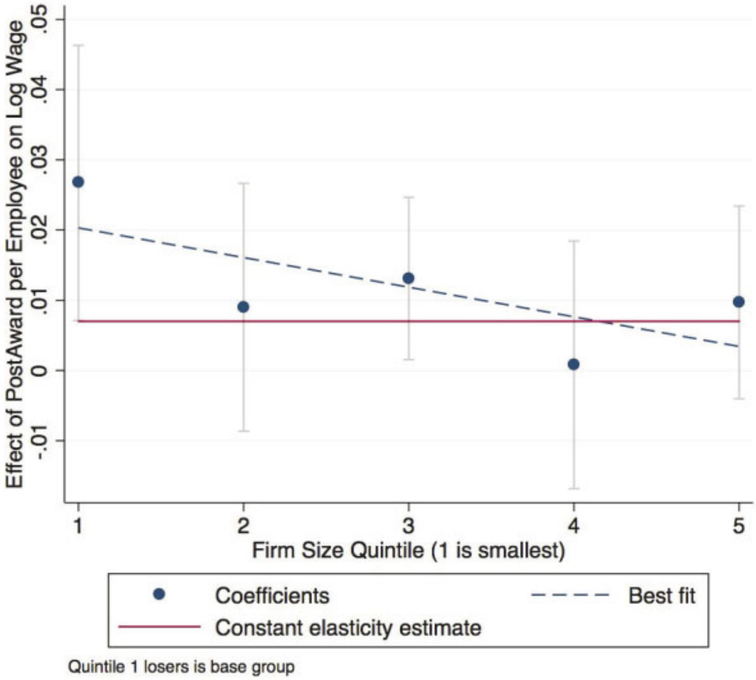

Figure 2
Effect of award per employee by firm size bin
This figure shows the effects of winning on the logarithm of the earnings per employee within five firm size bins, corresponding to the quintiles of firm size, measured as the number of employees in the year before the award. Each point represents a coefficient from a regression with separate independent variables for each bin of firm size, using a variant of Equation (1), giving the effect of winning in award dollars per employee conditional on being within a given quintile of firm size. The omitted group is firms that failed to win an award and that were in the bottom size quintile. The blue dashed line represents the best-fit line between the coefficients. The red solid horizontal line represents the effect of grant per worker estimated on the full sample, which is the prediction from assuming constant elasticity with respect to grant per worker. Ninety-five percent confidence intervals are shown for the quintile coefficients. This figure reports results from disclosure CBDRB-FY2020-CES005-010.

award.[22] Each point is a coefficient showing the effect of award dollars per employee conditional on being within a given quintile of firm size. The omitted group is composed of rejected applicants in the bottom size quintile. The blue dashed line represents the best-fit across the coefficients. Finally, the red solid horizontal line represents the effect of grant per worker estimated on full sample, which is the prediction from assuming constant elasticity with respect to grant per worker. The effect is largest for the smallest quintile, but otherwise is similar across the size distribution, and is not statistically significantly different from the estimate in the whole population (the red line) for any quintile. In sum, it appears that the effect decreases somewhat in firm

22 The quintiles are fewer than 6, 6-11, 12-23, 24-69, and more than 69 employees.

Table 4
Grant effect on earnings by incumbent status (employee level)

Dependent variable: *log(earnings)*

Sample:	Incumbent employees only (1)	(2)	(3)	2-year window (4)
PostAward	.041***	−.14***	−.12***	−.21***
	(.015)	(.03)	(.029)	(.049)
PostAward·Incumbent		.15***	.15***	.22***
		(.028)	(.027)	(.048)
Post	.0034	.003	.046	.135
	(.0066)	(.0023)	(.062)	(.42)
Incumbent		.44***	.11***	.59***
		(.009)	(.01)	(.014)
Post·Incumbent	N	Y	Y	Y
Employee controls$_{t=-1}$	N	N	Y	N
Employee FE	Y	N	N	N
Firm FE	N	Y	Y	Y
Industry-year FE	Y	Y	Y	Y
MSA-year FE	Y	Y	Y	Y
N	177,000	257,000	257,000	95,000
R^2	.78	.24	.39	.3

This table shows the effect of the grant on employee earnings by incumbent status, using Equation (1). Incumbent employees are those who were present at the firm in the year before the grant award year. Column 1 restricts the sample to incumbent employees. Columns 2-4 interact winning a grant with being an incumbent employee. Note that $Award_{i,j}$ is defined at the firm level, so is absorbed by either employee or firm fixed effects. Control coefficients are not reported to minimize disclosure requirements. Employee controls$_{k,t=-1}$ include tenure, age, high education (BA or above), and the logarithm of the wage in the year before the award year. Data are observed at the employee-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. *$p<.1$; **$p<.05$; ***$p<.01$.

size but is not inconsistent with profit-sharing having constant elasticity across firm sizes.

The effects are robust to a number of unreported approaches. First, they are similar with a bandwidth of one firm around the cutoff. Second, splitting the sample by time period around 2005 or 2008 yields similar effects on either side. Third, the effect is not driven by the first year after the award. When we omit the first year, the coefficient is similar and of equal significance. Fourth, the effect is similar when multiple-time grant winners are excluded from the sample; that is, the result does not reflect future grants. Fifth, the effects are robust to using to using alternative clusters, such as by employee. We cannot rule out that the effect on earnings reflects more hours worked, as we do not observe the hourly wage. However, this seems unlikely for two reasons. First, the effects endure over time. If higher earnings reflected more hours worked, the effect should decline over time as the firm hires new workers and reaches a new target size. Second, more hours worked should affect both incumbent and new employees; as we show below, there is no effect among new employees.

3.2 Incumbent versus new employees

Next, we decompose the positive effect on earnings across new and incumbent employees. Table 4, column 1 restricts the sample to incumbent employees and finds a larger effect than in the overall sample, at 4.1%. Column 2 shows that an interaction between PostAward and being an incumbent employee is .15 and highly significant; after exponentiating (because the outcome is logged), this means that the award increases the difference between incumbent and new hire earnings by 16%. Therefore, the overall positive effects of the award on wages stem from incumbents, who are the majority of employees. Further, the negative coefficient for PostAward implies that winning reduces earnings among the minority of employees who are new hires.

This difference is highly robust. It persists with controls for employee tenure, age, education, and wage in the year before the application year (column 3). It is larger when the sample is restricted to a 2-year window after the grant award decision (column 4). The result is also similar at the firm level, where new hire wages can be analyzed separately (Table A1, columns 3 and 4, in the Internet Appendix). The difference does not reflect partial earnings in a new employee's first year, as partial earners are omitted, and the results are the same when the first year after the award is excluded. Last, the difference is robust to using the logarithm of the award amount per employee as the independent variable (Table A2, column 2, in the Internet Appendix).

Next, we examine whether this difference seems to primarily reflect skill by asking whether it persists across the wage distribution. There is wide variation in incumbent preaward wages, pointing to different levels of preexisting skill (Table 1, panel C). The results are in Table 5, where we employ Equation (2) with firm-level data. Each column considers the average firm earnings for a percentile within the firm's wage distribution. Chetverikov, Larsen, and Palmer (2016) explain how this type of quantile regression panel estimator is consistent and asymptotically normal. The effect is 11-14 percentage points and highly significant across all percentiles for incumbents, and is small and insignificant for new hires.[23] The third column in each group stacks the two samples and shows that the difference between incumbents and new hires is significant at the 10th and 50th percentiles, but not the 90th or 99th, because of large standard errors for the new hire coefficients. In sum, these results suggest that the incumbent effect does not reflect different new-hire skill and is consistent with a firm factor.

3.3 The role of employee tenure

To explore reasons for the large effect of the grant on incumbent earnings, we interact winning an award with various employee characteristics within the sample of incumbent employees. By far the largest and most robust source of heterogeneity is tenure, or the number of years an incumbent employee has been with the firm. In unreported analysis, we do not find significant effects

[23] This consistency across the wage distribution is not driven by very small firms where all employees might plausibly be a narrow group of cofounders. When we eliminate firms below the 25th percentile of employment from the sample, we continue to find consistent effects across the wage distribution.

Table 5
Grant effect on firm earnings among incumbent and new employees (firm level)

Dependent variable: *log(earnings)* at the firm's:

	10th pctile			50th pctile			90th pctile			99th pctile		
Sample:	Incumbent (1)	New (2)	All (3)	Incumbent (4)	New (5)	All (6)	Incumbent (7)	New (8)	All (9)	Incumbent (10)	New (11)	All (12)
PostAward	.13**	.052	−.019	.11**	.053	.016	.14***	.043	.066	.14**	0.036	.062
	(.062)	(.066)	(.064)	(.051)	(.058)	(.054)	(.055)	(.063)	(.057)	(.065)	(.071)	(.069)
PostAward·Incumbent			.15**			.093**			.068			.06
			(.059)			(.043)			(.048)			(.054)
Incumbent			.9***			.75***			.6***			.66***
			(.027)			(.02)			(.022)			(.024)
Award	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Rank, $Rank^2$	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Age, Age^2	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Competition-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	8,200	3,200	11,500	8,200	3,200	11,500	8,200	3,200	11,500	8,200	3,200	11,500
R^2	0.25	0.19	.24	.2	.23	.25	.2	.22	.22	.24	.23	.24

This table shows the effect of the grant on earnings percentiles by employee type at the firm level using Equation (2). Note that competition-year fixed effects absorb *Post.* Data are observed at the firm-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. $*p<.1; **p<.05; ***p<.01$.

Table 6
Grant effect on earnings among incumbent employees by tenure (employee level)

Dependent variable: *log(earnings)*

Sample:						No owners	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PostAward·Tenure	.011*	.0096**	.0089**	.0096***	.045***	.011**	.037**
	(.0038)	(0.0037)	(.0032)	(.004)	(.012)	(.0065)	(.016)
PostAward	−.025	−.018	−.027	−.032*	−.087***	−.044	−.077**
	(.022)	(.022)	(.02)	(.019)	(.029)	(.027)	(.034)
Post·Tenure	−.019***	−.019***	−.013***	−.014***	−.058***	−.015***	−.048***
	(.0033)	(.0033)	(.0028)	(.0027)	(.011)	(.0041)	(.014)
Tenure	.12***	.11***	.073***	.056***	.19***	.12***	.21***
	(.0019)	(.0018)	(.0015)	(.0015)	(.0035)	(.0023)	(.004)
Post	.084***	.083***	.059***	.059***	.09***	.061***	.058**
	(.017)	(.017)	(.015)	(.014)	(.025)	(.02)	(.023)
Experience		.054***					
		(.0032)					
PostAward·Tenure^2					−.0049***		0.004***
					(.0011)		(.0015)
Post·Tenure^2					.006***		.0055***
					(.0011)		(.0013)
Tenure^2					−.012***		−.013***
					(.00027)		(.00034)
Age, HighEduc	N	N	Y	Y	Y	N	Y
WagePctiles$_{t=-1}$ FE	N	N	Y	N	N	N	N
Wage$_{t=-1}$	N	N	N	Y	Y	N	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y
MSA-year FE	Y	Y	Y	Y	Y	Y	Y
N	177,000	177,000	177,000	177,000	177,000	133,000	133,000
R^2	.3	.3	.45	.47	.49	.31	.5

This table shows the effect of the grant interacted with employee tenure on employee earnings, using Equation (1). The sample is restricted to incumbent workers who were at the firm before the application year. Columns 6 and 7 further restrict the sample to include only those hired at least 3 years after the firm is first observed, to test whether owners drive the interaction effect with tenure. Control coefficients are not reported to minimize disclosure requirements. Note that $Award$ is defined at the firm level, so is absorbed by firm fixed effects. Data are observed at the employee-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. $*p<.1; **p<.05; ***p<.01$.

in other variables such as age, gender, race/ethnicity, location, education, or preexisting wage.

Table 6, column 1 shows that an additional year of tenure increases the effect of winning on wage by about 1%, or 36% of the average employee-level effect. In these models, we do not include employee fixed effects because the event of an award occurs only once for each employee, at which point they have a unique amount of tenure. Having established robust causal effects above, we include firm fixed effects here to address concerns that that firm-specific trends, such as particularly long-tenured workers at some firms, might explain the relationship.

We conduct several tests for whether this is an artifact of skill. First, we control for years of total career job experience in column 2. We then consider alternative proxies for skill, expecting that preexisting wage should map closely to skill. We add controls for employee age, education, and preexisting wage

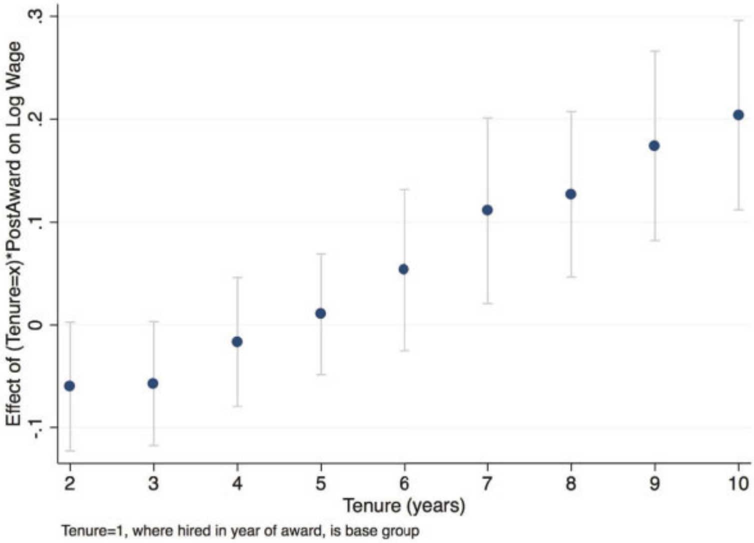

Figure 3
Incumbent employee-level effects by tenure
This figure shows the effects of winning on the logarithm of the earnings by years of tenure, among incumbent employees. Each point is a coefficient from a regression with separate dummies for years of tenure interacted with winning, using a variant of Equation 1. The omitted group is those with 1 year of tenure, and more than 10 years are excluded (the coefficients are noisier). Ninety-five percent confidence intervals are shown. This figure reports results from disclosure CBDRB-FY19-369.

percentile (column 3) or preexisting linear wage (column 4). The effect persists with essentially the same magnitude as in column 1.

The effect is markedly linear in tenure. Figure 3 shows coefficients from a regression with separate dummies for years of tenure interacted with winning, among incumbent employees. The effect increases linearly through 10-plus years. A quadratic specification in Table 6, column 5 confirms this relationship, indicating a very slight concavity in tenure. There is no measurable effect of the award on the firm's wage-tenure profile.

The tenure effect does not appear to reflect firm owners. Columns 6 and 7 restrict the sample to incumbent employees hired at least 3 years after the first year the firm is observed, who are not plausibly owners, and finds a larger result than in parallel full-sample specifications of columns 1 and 5. Last, Table A2, column 3, in the Internet Appendix shows that the effect continues to increase in tenure with the award amount per employee as the independent variable.[24]

24 Note that the interaction with tenure changes the scale of the coefficients. Also, the coefficient for the logarithm of the amount per employee interacted with award (first row) is the effect when tenure is zero, which is never the case in the data (it starts at one when the person is first observed at the firm).

3.4 Wage inequality

The striking difference between incumbents and new hires suggests a possible effect on within-firm inequality. We examine this in Table A3 in the Internet Appendix, using Equation (2). Columns 1-4 use growth outcomes, and columns 5-9 use levels outcomes. A grant increases the growth of the 90/10 ratio by 32% (column 1). The effect is slightly larger when only the first 2 years after the application are included (column 2).[25] The effect on upper-tail inequality growth (the 99/50 ratio), shown in column 3, is smaller.[26]

These inequality effects are in puzzling contrast to the positive effect among incumbents at all points in the wage distribution (Table 5). The explanation is that the difference between new hires and incumbents leads to higher inequality. Table A3, columns 6 and 7, in the Internet Appendix show no effect of winning on inequality within incumbents or new hires, consistent with Table 5. New hires tend to be at the lower end of the firm's wage distribution. This "weighs against" the bump that incumbent low earners receive, which is in percentage terms about the same as for incumbent high earners. These results shed light on both within- and across-firm wage inequalities, helping to explain why workers with similar skills are paid different amounts depending on where they work.

The inequality results align with evidence that the value of higher, but not lower, skill labor increases with firm scale, helping to explain why larger firms have more within-firm inequality (Mueller, Ouimet, and Simintzi 2017; Song et al. 2018). They also suggest that inequality within the firm can increase while all incumbent employees receive a "fair share" of rents and new workers are treated equally as a group, which is related to the idea that people dislike unfairness but not inequality (Starmans, Sheskin, and Bloom 2017; Edmans 2019).

4. Effects on Firm Growth and Productivity

Next, we explore whether the wage effects above stem from firm growth or increases in productivity. We cannot observe profits, but we do observe revenue and total employment. In Table 7, we show effects on growth and labor productivity, again using Equation (2). Relative to the year before the award, the grant increases employment growth by about 30% (columns 1 and 2). Evaluated at the means, this indicates that winners have about 19% more employees than losers relative to the year before application. A bit more than half the effect on employment occurs within 2 years of the grant

[25] Figure A.1, Panel B, demonstrates the effect on the 90/10 ratio by rank around the cutoff. We only report two positive ranks for inequality, because the smaller sample led to a very large confidence interval for the firm three ranks away from the cutoff. We cannot create the quarterly figure as the W-2 data used to construct inequality measures are annual.

[26] For fewer than 10 employees, the algorithm assigns the 90th and 10th percentiles to the extreme observations. As mentioned earlier, the within-firm standard deviation of wages is large even when the firms have few employees.

Table 7
Grant effect on firm growth outcomes

Dependent Variable:	*Employment growth*				*Revenue growth*			*Productivity*		
	(1)	(2)	2-year window (3)	Incumbent Less New Hires (4)	(5)	(6)	2-year window (7)	(8)	(9)	2-year window (10)
PostAward	.29***	.24***	.18**	0.061	.2***	.17***	.15***	−.048	0.0078	−.055
	(0.045)	(0.037)	(.046)	(.056)	(0.042)	(.044)	(.06)	(.043)	(.042)	(.067)
Award	Y	Y	N	Y	Y	Y	N	Y	Y	N
Rank, $Rank^2$	Y	Y	N	Y	Y	Y	N	Y	Y	N
Age, Age^2	Y	Y	N	Y	Y	Y	N	Y	Y	N
Competition-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-application FE	N	Y	N	N	N	Y	N	N	Y	N
N	30,500	30,500	20,000	8,200	13,000	13,000	9,600	13,000	13,000	9,600
R^2	.41	.69	.47	.27	.59	.78	57	.36	.68	.35

This table shows the effect of the grant on the logarithm of firm growth and productivity, using Equation (2). The base year for the dependent variables is $t=-1$, the year before the application year. Columns 3, 7, and 10 restrict the post sample to the two years after the grant application year (this includes the application year). The dependent variable in column 4 is the number of incumbent employees at the firm less the number of new employees in that year, and we take the logarithm of the difference plus one. Productivity is the revenue productivity of labor, measured as revenue divided by employment. Data are observed at the firm-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. $*p<.1; **p<.05; ***p<.01$.

application (column 3). We assess whether the effect stems primarily from retaining incumbents or hiring new workers by using the number of incumbent minus new employees as a dependent variable. The result, in column 4, shows that growth appears to come from both groups. However, the coefficient being positive and large points to a possible increase in incumbent retention.

We similarly see an effect of about 22% on revenue (Table 7, columns 5 and 6). This represents 15% more revenue than in the preaward year. Figure A1 in the Internet Appendix reports the effect on levels of the logarithm of the employment and revenue by rank around the cutoff.

There is no significant effect of the grant on productivity measured as revenue divided by employment. If anything, the effect is negative, shown in Table 7, columns 8-10. Despite growth in both employment and revenue, the employment growth is even with or exceeds the revenue growth. Note that the post-grant period is ever-after within the span of our sample, and thus includes many years after the grant. If the channel for immediate wage gains is expected future increases in productivity, there might not be an immediate effect on revenues or productivity, but we should expect an effect in the longer term.

To examine whether the effect on earnings occurs at the same firms that experience a strong growth effect, we interact winning with growth over the 2 years after the award year in productivity, revenue, and employment. The interaction coefficients, reported in Table A4, columns 1-3 in the Internet Appendix, are uniformly negative and statistically insignificant, in contrast with the strong positive effect of winning. We also interact with the number of cite-weighted patents that the firm applies for and is ultimately granted during the 2 years after the application year, a measure of innovation quality. The coefficient for the interaction is negative and significant (column 4). We find similar results with longer time frames.

In sum, these results demonstrate that the effect on earnings seems unrelated to realized or correctly expected productivity or growth. If anything, they are substitutes because the firms that experience initial growth are not the ones providing the largest earnings increases. Overall, while the grant positively affects growth and clearly has permanent effects on the firm, the wage patterns do not seem to reflect an observable productivity channel.

5. Equity Financing within the Firm

Thus far, we have seen that a one-time positive cash flow shock immediately raises wages for incumbent employees, an effect that increases in tenure. Puzzlingly, the wage effect does not appear to reflect proxies for skill and is independent of subsequent firm growth and productivity. In this section, we propose a mechanism to explain the results: Through backloaded wage contracts, the financially constrained firm funds its growth by providing implicit equity to employees, initially underpaying them and then paying them more when the firm's value and ability to pay improve. We cannot rule out the presence of other channels, but as we discuss below, no other channel enjoys strong support from the data.

The financial mechanism of within-firm financing begins with wage-tenure profiles. Early literature theorized that a flat wage contract provides optimal risk sharing, where firms insure workers (Azariadis 1975; Harris and Holmstrom 1982; Bernhardt and Timmis 1990). The fact that wages exhibit a strong correlation with tenure, especially in small firms, led later research to take a different approach. Michelacci and Quadrini (2009) model how a financially constrained firm optimally pays workers lower wages initially, implicitly borrowing from them in order to grow faster than it would otherwise.[27] Guiso, Pistaferri, and Schivardi (2013) offer evidence for borrowing within the firm.

Several features of our data suggest implicit financing within the firm. If the grant is used to pay out existing backloaded wage contracts, only incumbent employees should be affected, which is what we find. The firm should also "owe" the most to the incumbent employees who have been at the firm the longest. Indeed, the effect increases in worker tenure, which is not driven by firm owners and is similar across the wage distribution, suggesting that backloaded wage contracts are used for all employees. In our data, workers stay at the firm long enough to reasonably benefit from the implicit financing contract; for example, the share of people with a job tenure of at least 5 years is 0.39, and across employees the average maximum tenure is about 5 years (Table 1, panel C).[28]

In a setting of mature, steady-state firms, Guiso, Pistaferri, and Schivardi (2013) find that implicit debt is repaid on a fixed schedule and does not depend on a good event; the firm would fail to repay on the schedule determined by worker tenure only in the extreme event of default. The contingent nature of repayment that we observe, where repayment occurs after a positive cash flow shock, indicates some risk sharing. This is better described by an equity contract, which to our knowledge is a novel insight. It is appropriate to this sample of high-tech, small firms that typically receive external finance primarily in the form of equity rather than debt.

The relationship between the effect on wages and worker tenure is informative about how fast firms "pay back" their workers. After the cash flow shock, the value of the firm increases, so equity-holders should benefit in proportion to their equity. Long-tenured workers have the largest claims and thus should experience the largest effects. Consistent with this, we observe that the increase in earnings rises with tenure in a linear fashion, with the largest increase among workers who have been with the firm for more than 10 years (Figure 3). In contrast, in a debt contract, any repayment should be concave in

[27] Related to this idea are models of insurance within the firm, including Guiso, Pistaferri, and Schivardi (2005), Cardoso and Portela (2009), and Ellul, Pagano, and Schivardi (2018).

[28] These statistics are calculated within the sample of firms with wages observed for at least 5 years.

tenure because those with intermediate tenures would have the highest claims; the longest tenure workers have already been paid back and have no current claims (see figure 1 in Guiso, Pistaferri, and Schivardi 2013).

In sum, the finding of immediately higher wages after a positive cash flow shock that increases linearly in tenure points to paying "dividends" via earnings to employees as "shareholders." Note that this mechanism does not require the shock to have no effects on productivity (or on other dimensions such as certification). Instead, it is important that any growth and productivity effects do not explain the immediate wage patterns, as shown in Section 4. While the new cash may enable investments that ultimately increase productivity and have long-term effects on wages, such a channel is different from settings in which the shock itself is to productivity, as, for example, with a new patent.

In the following subsections, we present further evidence to support this mechanism.

5.1 Financial constraints

Limited access to outside financing could explain why some firms use their employees as a financing source. Since such a practice places the firm at a disadvantage in hiring, we would not expect firms to tap employees unless they could not obtain sufficient financing elsewhere. Therefore, this mechanism predicts a larger effect among the more financially constrained firms in our sample. In this section, we conduct a series of heterogeneity tests providing strong support for this assumption.

For our first test, we obtain an unusually direct measure of financial constraints by linking the firms to the Census Bureau Survey of Business Owners, which includes questions about startup and expansion sources of capital. As mentioned above, the SBIR applicants appear constrained relative to the U.S. firm population (Table 2). For example, they have a 3.2% chance of reporting no access to expansion capital, which is more than twice the national mean. To test whether the effects are larger among those applicants that are especially constrained, we interact the reported sources of financing with winning an award in Table 8. Here and below, we control for labor productivity to ensure that it does not explain both financial constraints and a larger effect of the grant.

The results show that the effect of the grant is concentrated among the most financially constrained firms: The interaction between PostAward and No Access (to expansion capital) is .15, significant at the .05 level, indicating that the increase in earnings is about 16% larger at firms that report no access relative to those that do not (column 1). In contrast, the coefficient is negative and insignificant for the large share of firms with bank loans, consistent with them facing less need to replace wages with equity. We also find no significant interactions on financing out of credit cards, profits, personal and family savings and other assets, and with needing no financing. Table A5 in the Internet Appendix shows interactions with sources of startup capital and

Table 8
Grant effect on earnings among incumbent employees by firm sources of expansion capital

Dependent variable: *log(earnings)*

	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)
PostAward·								
No access	.15**							
	(.064)							
Bank loan		−.046						
		(.038)						
Gov't guaranteed loan			.17**					
			(.073)					
Credit card				-.05				
				(.23)				
Profits					.033			
					(.034)			
Personal savings						.019		
						(.069)		
Personal assets (nonsavings)							.08	
							(.19)	
None needed								.016
								(.027)
PostAward	.073**	.083**	.068***	.072**	.063**	.068**	.069***	.066***
	(.023)	(.025)	(.023)	(.022)	(.025)	(.023)	(.022)	(.023)
Post	−.0061	−.0055	−.0029	−.0046	−.0058	−.0026	−.006	−.0046
	(.011)	(.0093)	(.0087)	(.0086)	(.011)	(.0088)	(.086)	(.011)
Labor productivity	.014	.014	.014	.013	.014	.014	.013	.014
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
X, Post·X	Y	Y	Y	Y	Y	Y	Y	Y
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y
MSA-year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	95,000	95,000	95,000	95,000	95,000	95,000	95,000	95,000
R^2	.77	.77	.77	.77	.77	.77	.77	.77

This table shows the effect of the grant on employee earnings within the sample of incumbent employees, and within the sample of firms that match to the Survey of Business Owners (SBO). We use the SBO year closest to the year of award. Each interaction variable is an indicator for the firm reporting using the particular type of financing to expand its business in the year of the survey. "Personal" also includes family. Note that $Award_{i,j}$ is defined at the firm level, so is absorbed by employee fixed effects. Data are at the employee-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. $*p<.1$; $**p<.05$; $***p<.01$.

finds positive relationships for credit cards and personal savings, consistent with entrepreneurs being financially constrained and personally financing and borrowing to launch their firms, which we might expect if they were also receiving financing from their early employees. (No Access is not an option for the question on startup capital.)

We also examine standard proxies for financial constraints in Table 9. Columns 1 and 2 show that the grant is more useful for smaller and younger firms.[29] Firms with access to private equity financing are likely to be less constrained; consistent with this, column 3 shows that an interaction with

29 The variables are indicators for top quartile employment and age in the year before the grant award year. We use indicators because these variables are quite skewed. These relationships persist at the firm level.

Table 9
Grant effect on earnings among incumbent employees by firm size, age, and growth

Dependent variable: *log(earnings)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostAward·								
Large firm$_{t=-1}$	−.18**							
	(.068)							
Old firm$_{t=-1}$		−.17***						
		(.034)						
VC-backed$_{t=-1}$			−.064**					
			(.025)					
High pay firm$_{t=-1}$				−.26***				
				(.074)				
Firm growth$_{t\in-3,-1}$					.11***			
					(.023)			
Employee's % raise						−.026***		
						(.0094)		
High rev firm$_{t=In.}$							−.085***	
							(.028)	
High emp firm$_{t=In.}$								−.084***
								(.027)
PostAward	.21***	.18***	.05***	.29***	.037***	.044***	.071***	.069***
	(.068)	(.03)	(.015)	(.072)	(.013)	(.015)	(.018)	(.017)
Post	−.0016	.0024	.0035	.046	.0022	.0035	.011	.0072
	(.045)	(.011)	(.0073)	(.031)	(.0069)	(.067)	(.0093)	(.0093)
Labor productivity	.0094*	.0096*	.011**	.01*	.0097*	.0094*	.008	.0096*
	(.0054)	(.0054)	(.0054)	(.0053)	(.0054)	(.0054)	(.0052)	(.0054)
X, Post·X	Y	Y	Y	Y	Y	Y	Y	Y
Employee FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y
MSA-year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	177,000	177,000	177,000	177,000	177,000	177,000	177,000	177,000
R^2	.74	.74	.74	.74	.74	.74	.74	.74

This table shows the effect of the grant on employee earnings using Equation (1) and interacting PostAward and Post with a firm(employee)-level characteristic in columns 1-5 (6-8). Large, Old, and High pay are indicators for top quartile employment, age, and average wage in the year before the award. Previous VC is an indicator for the firm having VC investment before the award decision. Growth is the revenue growth in the 3 years before the award. Employee's % raise is the worker's earnings in the first year of his job at the SBIR applicant firm relative to the last year of the previous job. High rev(emp) firm$_{t=Initial}$ is an indicator for revenue (employment) being above median (across all initial employee observations) in the first year the employee joined. $Award_{i,j}$ is defined at the firm level, so is absorbed by employee fixed effects. Data are at the employee-year level. Standard errors are clustered by firm. This table reports results from disclosure CBDRB-FY2022-CES005-012. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

previous VC investment (anytime before the award decision) is negative.[30] Finally, less constrained firms would have been able to pay more before the grant. Indeed, firms that paid above-median wages in the year before the application year give smaller wage increases (column 4).

5.2 Dynamic evidence for implicit financing

In this section we present several tests for implicit equity financing across firm and employee lifecycles. First, backloaded wage contracts (whether equity or debt) should be most useful when the firm needs to grow fast, so

30 The mean of previous VC in this employee-level sample is 0.14.

Michelacci and Quadrini (2009) predict that firms growing faster should initially pay lower wages. In Table 9, column 5, we interact winning with revenue growth between 3 and 1 years before the grant application year. The coefficient is strongly positive, consistent with firms financing growth by paying lower wages.

Second, we expect that if incumbent workers accept a backloaded contract, their initial wage should reflect a "constrained employer" penalty, and their benefit from the cash flow shock should increase in this penalty. Table 9 presents evidence consistent with both of these predictions. Column 6 shows how the effect varies with the employee's percentage raise when he was hired relative to his previous job. The interaction term indicates that the effect on earnings decreases in the percentage raise. In other words, workers who accepted higher wage penalties when they joined the firm receive a larger benefit because of the grant award. We focus on the percentage raise in Table A6 in the Internet Appendix. This table shows correlations between firm and worker characteristics and the percentage raise. In column 1, we show that the percentage raise in the first year at the SBIR applicant firm relative to the previous job is decreasing in the tenure of the worker as of the year before the application. This is consistent with the grant's effect in tenure reflecting these high-tenure workers having paid a penalty when they initially joined. We also show that the larger, older firms for which there is a smaller grant effect gave incumbent workers larger raises when they first joined (columns 2 and 3). This again supports the connection between financial constraints and ability to pay workers.

Third, the effect should be larger for employees who joined when the firm was especially constrained and more likely to finance itself via backloaded wage contracts. As firm size is related to ability to pay employees (Brown and Medoff 1989; Gibson and Stillman 2009), we use indicators for employment and revenue being above-median in the first year the employee joined, where median is defined across all initial employee observations. Columns 7 and 8 in Table 9 show that the effect of the grant on wages is smaller when the employee joined the firm at a relatively larger size, consistent with higher ability to pay market wages initially, and thus smaller employee claims at the time of the grant.

5.3 Incumbent premium and new hire effects

Do incumbent workers earn a risk premium for having accepted the backloaded contract? Without observing the counterfactual unconstrained wage trajectory, we cannot fully answer this question. However, if we put aside counterfactual wage growth, we can assess whether the pay penalty at hiring is repaid after the grant, and if so with what premium or discount. A simple calculation suggests a substantial premium for workers with 7 years of tenure at the time of the grant (7 years is about one-standard-deviation above the mean). The annual increase is over twice the pay penalty for joining early, allowing the worker to "make up" for foregone income within 3 years.[31] While the exact number is of course sensitive to assumptions, an incumbent worker with long tenure who is at a winning firm appears to be well-compensated. This premium is consistent with incumbent employees having rights to future cash flows, through implicit equity. We would not expect a debt contract to yield such persistent gains. In this way, the equity interpretation is consistent with the one-time shock having permanent effects.

There is a negative effect of the grant on new hire earnings (Table 4) and no effect on the within-firm wage-tenure profile. One interpretation of these facts is that the firm engages in backloaded contracts with new hires, which is what occurs in the models of Michelacci and Quadrini (2009) and Guiso, Pistaferri, and Schivardi (2013): firms continuously borrow from new hires and pay back those with intermediate tenures. In our setting, while the grant relieves financing constraints, it is likely too small to eliminate them. The effect on incumbent workers could reflect a need to use part of an observable windfall to pay off employees with the most unvested human capital, creating credibility when engaging in new backloaded wage contracts (we discuss this further below in the context of enforcement). This helps to explain why new hires at winning firms would receive a lower wage than at losing firms.

The absence of a positive effect on new hires also supports an equity rather than a debt contract. In Michelacci and Quadrini (2009) and Guiso, Pistaferri, and Schivardi (2013), a cash windfall would flatten the wage profile in tenure. It might also lift it up, delivering gains for all tenure levels, depending on how the firm used the windfall. Both the flattening and, if it occurred, the lift predict an increase in new hire wages. The absence of such an effect is, therefore, one piece of evidence in favor of equity and against debt. An equity contract implies that the long-tenured worker's claim is to future cash flows; since recent hires lack such claims, it makes sense that they are not rewarded in the near-term.

5.4 Enforcement mechanism

Why doesn't the firm renege on backloaded wage contracts? There are two potential enforcement mechanisms: retention and reputation. The first is central to the Michelacci and Quadrini (2009) model, where the firm can commit to increase wages in the future because it invests in worker-specific capital. The loss of this capital should the worker quit operates as a form of implicit collateral for the employee. Similarly, in Sun and Xiaolan (2019), the intangible

[31] The closest measure we have to the average unconstrained wage bump is the bump for new hires among awardees, which is 24%. The percentage increase is decreasing by .025 on average per year of tenure (Table 9, column 7). A worker with 7 years of tenure (about one-standard-deviation above the mean) therefore "missed out" on about 4.2% of their wage gain when hired. The average wage in the last year of the previous job is $47,570, implying that they missed out on $1,997 per year. The increase in wages due to the grant is about 9%. Relative to the average incumbent wage of $63,500, this is $5,715. Thus, the pay bump is more than twice the penalty at hiring, suggesting a substantial premium. Making the conservative assumption that the employee would have invested this income at 5%, and reinvesting the income on it, the forgone earnings total $17,776. It therefore takes between 2 and 3 years after the grant to make up for this lost income.

capital of the employees serves as collateral. The second mechanism is concern for credibility or reputation. Making good on implicit contracts could benefit the firm in the long run (Lazear 1989; Kahneman, Knetsch, and Thaler 1986). In an implicit contract, worker loyalty yields more productivity, and in exchange employees are guaranteed a share of firm profits (Howell and Wolff 1991). Establishing a good reputation and building trust with employees appear to play a role in real world wage bargaining outcomes and in shaping employee wage perceptions (Blanchard and Philippon 2006; Falk, Fehr, and Zehnder 2006, Card et al. 2012; Breza, Kaur, and Shamdasani 2017; Dube, Giuliano, and Leonard 2019).

If enforcement is in part through reputation effects, firms that do not fulfill old equity contracts should find it more difficult to finance themselves from new hires. In practice, this predicts the incumbent wage growth after an award to be negatively correlated with the new hire percentage raise relative to their previous job. To test this, we construct two variables within the sample of winning firms. First, we calculate the growth in incumbent worker earnings in the year after award relative to the year of award. We then take the average across incumbent workers to create a one-per-firm variable called *Incumbent wage growth*. Second, we calculate the growth in wages between this job and the previous job for new hires (*New hire premium*).[32] We then assess the correlations between these two variables. Table A7 in the Internet Appendix presents the results. Panel A shows a correlation of −0.7, and panel B, column 2, shows that in a regression format with award year fixed effects and robust standard errors, the relationship is −0.038. Therefore, firms that shared less of the award with incumbents appear less likely to borrow from new workers.

Since on average firms do share with incumbents, this helps explain the negative effect for new hires. Winning firms have credible potential to pay higher wages in the future because after the publicly known grant event, they start to repay backloaded contracts with existing employees. New employees are willing to accept lower pay to work at a promising firm that does not renege on its implicit dynamic contracts with employees.

We test the retention mechanism by examining whether incumbents who receive higher wage gains are more likely to stay with the firm. To do this, we follow Baghai et al. (2021) in defining voluntary departure as an employee leaving the firm and appearing the next year at new firm. We correlate voluntary departure with the *Incumbent wage growth* measure from above.[33] This analysis is at the one-per-employee level since sharing may vary across individuals. The results, in Table A7 in the Internet Appendix, indicate a robust negative relationship between wage growth and voluntary departure.

[32] The mean of *Incumbent wage growth* is 0.06, and the mean of the *New hire premium* is 0.24. To calculate the latter, we impute the sample average for the few firms without any new hires.

[33] The mean *Incumbent wage growth* at the worker level is 0.01, and the mean voluntary departure rate is 0.27 within 3 years and 0.63 in the long run.

The correlation is much larger when we require voluntary departures to occur with 3 years of the award; for example, in a regression controlling for Industry-year and MSA-year fixed effects, the relationship is $-.13$ within 3 years and $-.05$ in the long term (panel B, columns 3 and 4). These results support sharing the award to retain incumbent workers. In sum, the data are consistent with both reputation and retention as a means for enforcing a backloaded wage contract.

5.5 Survey evidence

To explore whether backloaded wage contracts as a result of financial constraints are used in practice, we conducted an email survey of DOE SBIR grantee principal investigators, who are almost always company CEOs.[34] The survey asked the following question:

> "Have you ever paid employees less than you would optimally want to pay them because you were cash-constrained, and then been able to pay them more once you were doing well? That is, do employees sometimes accept lower pay initially so that the firm can grow faster, with the expectation that cash windfalls may be shared fairly with them in the future?
>
> You can simply reply "Yes" or "No" to this email, but if you have time it would be terrific if you can provide a bit of color or explanation as well."

We sent the same email to 585 individuals for whom we were able to find email addresses.[35] Among these, 88 addresses bounced. We received 99 responses, representing a response rate of 19.9%. The full text of the email is shown in Figure A2 in the Internet Appendix, which also includes an actual response.[36] Across the 99 respondents, 55.6% replied yes, 21.2% no, and 23.2% did not explicitly answer the question. The sample response in Figure A2 in the Internet Appendix is representative of the fact that most responders directly answered the question while also generously providing qualitative color.

Examples highlight the results and point to a further dimension for enforcing implicit contracts: the nonpecuniary benefit of mission-driven work at high-tech, small companies. Ron Sinton, Founder and President of Sinton

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[34] Emails sent from Sabrina Howell. Note that the grantee firm and individual principal investigator information used to develop the survey is public, available at www.sbir.gov, and makes no use of data from the U.S. DOE or the U.S. Census. The survey targeted firms and so did not require IRB approval.

[35] We started with the sample of all Phase 1 grantees. The emails were sent on October 31, 2019 and November 1, 2019. All tabulated responses were collected by November 6.

[36] The SBIR grant for this responder is under the firm name "ProjectEconomics," available at https://www.sbir.gov/sbirsearch/detail/880883. This and subsequent quotes are provided with permission from the individuals.

Instruments, wrote “I would say “yes.”I effectively do this by supplementing salaries with discretionary year-end bonuses, proportionate to base salary for each employee.” Tom Heiser, CEO of Ridgetop Group, said that “...in the past this was a very good strategy as long as the candidate could understand the vision and was willing to sacrifice short term for the long term.” Susan MacKay, CEO of Cerahelix, wrote: “Yes I have done that often...several times with a promise of higher salaries in the future (have also delivered on that promise). It’s not just a promise of higher salary in the future, I also told (and still do tell) my employees that the experience and level of responsibility, the learning curve and challenges that they will encounter, are more than they would ever experience at a larger, more mature company.” After explaining that “our company has not taken off yet, so we haven’t had an opportunity to pay more. I am hoping this would happen in the near future,” another CEO wrote that employees “understand that the company is developing an important technology…they share the mission of the company.” Motivating employees with a mission and learning opportunities may be integral to the incentive compatibility of these implicit labor contracts and a fruitful avenue for future research.

It is important to caveat the survey analysis by noting that there are no doubt biases in both the subset of grantees that we reached and in their decisions to respond. Nonetheless, the results offer strong support for the mechanism. The survey responses indicate that grantees have often used backloaded wages contracts because of financial constraints and share windfalls with workers as a way to repay these contracts.

5.6 Other mechanisms

Perhaps the most obvious alternative mechanism is a pure bargaining theory of wages. Bargaining power is required for enforcement of the implicit equity contract: If workers had no ability to quit or tarnish the firm’s reputation, then the contract would not be enforceable. The implicit equity mechanism is different, however, from standard bargaining models. In those models, the wage is based on employee productivity and the outside option (Stole and Zwiebel 1996; Hall and Milgrom 2008), creating, for example, the situation in Kline et al. (2019), where wage effects come from changes to employee marginal productivity after a patent grant.

These models are inconsistent with our data. First, they predict that any immediate effect of a cash windfall on wages should be related to increasing productivity or growth, which we do not find (Section 4). Second, if immediate wage gains reflected bargaining over expected future productivity growth, they should be proportional to the benefit that the employee will provide, thus varying more with skill than simply with tenure. Yet we find no variation with proxies for skill, such as preexisting wage or education. Third, bargaining power should vary with the outside option, but we find no interaction effects with measures of labor market tightness at any point in the wage distribution. Last, bargaining does not predict larger effects among more financially constrained firms. In sum, while bargaining plays an important role in wage setting, in its standard form this theory does not seem to fully explain the pattern of effects on wages that we see after the cash flow shock.

Another plausible mechanism is incentive contracting. This would most likely produce a temporary bonus payout, not the permanent increases we observe. Further, the benefit should be proportional to the individual's effort to get the grant, which should move more directly with proxies for skill than tenure. Also, we would not expect rewards for working on the grant to be persistent over multiple years. Finally, there is no reason an incentive contracting mechanism would yield heterogeneity in measures of financial constraints.

In Internet Appendix B, we consider bargaining power and incentive contracting in more detail along with search frictions/match quality, efficiency wages, and agency frictions. None of these mechanisms has strong support from the data. That said, it is important to emphasize the rich array of reasons we might observe wage increases after a grant, many of which we cannot rule out. In practice it is likely that multiple forces are at play.

6. Conclusion

This paper offers the first evaluation of how a cash flow shock affects firm wages, employment, and revenue, using government R&D grants to small, financially constrained firms. In addition to being economically important, small firms are interesting because they have especially dynamic employment and wage structures. If such firms must make tradeoffs between spending on optimal wages and other purposes, their wage-setting behavior may deviate from modern models focused on the interplay between a worker's bargaining power, her marginal product, and firm profits.

We show that the cash flow shock increases wages only among incumbent employees, and this effect increases linearly in job tenure. While the one-time shock may have permanent effects, growth and productivity increases do not fully explain the effects on wages. We propose a mechanism of implicit equity financing within the firm, where the worker's wage gains after the grant are a payoff for having previously invested in the firm by sacrificing wages. The firms in our data offer a good setting to test for implicit contracts governing rent sharing because small firms have less hierarchical structures, more employee autonomy, and more opportunity for monitoring and coordination (Isaac, Walker, and Williams 1994; Carpenter 2007; Elfenbein, Hamilton, and Zenger 2010). It seems likely that large, unconstrained firms would react quite differently to a cash windfall. Assessing heterogeneity effects across a representative population of firms is a fruitful avenue for future research.

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