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ABSTRACT

This paper examines how employee earnings at small firms respond to a cash flow shock in the form of a government R&D grant. We use ranking data on applicant firms, which we link to IRS W2 earnings and other U.S. Census Bureau datasets. In a regression discontinuity design, we find that the grant increases average earnings with a rent-sharing elasticity of 0.07 (0.21) at the employee (firm) level. The beneficiaries are incumbent employees who were present at the firm before the award. Among incumbent employees, the effect increases with worker tenure. The grant also leads to higher employment and revenue, but productivity growth cannot fully explain the immediate effect on earnings. Instead, the data and a grantee survey are consistent with a backloaded wage contract channel, in which employees of financially constrained firms initially accept relatively low wages and are paid more when cash is available.

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1 Introduction

To what extent do firms share rents with employees? Existing literature has focused on whether productivity shocks affect wages using proxies for productivity-induced surplus, such as value-added, profits, sales, and patent grants.[1] Firms may also share with workers rents that are unrelated to labor productivity. Thus far, it has been difficult to disentangle the effects of productivity growth from pure rent-sharing, as even exogenous productivity shifts are intertwined with changing marginal products of employment relationships (Card, Cardoso, Heining & Kline 2018). To examine how firms share rents, the ideal experiment would randomly assign cash to firms and observe wage effects.

To approximate this experiment, we evaluate 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. In the short term, the effect of winning a grant offers a clean experiment in which firm productivity and growth are fixed, yet there are new rents to potentially be shared. We find that incumbent workers benefit immediately, but there are no short- or long-term effects for workers hired after the grant award year.

The firms in our data are small, private, and high-tech, a type of firm that is crucial to economic growth.[2] It is important to understand how these firms set wages. There is much evidence that this type of firm faces financial constraints (Kerr & Nanda 2009, Howell 2017), leading to potential benefit from delaying employee compensation until there is more ability to pay. Indeed, a backloaded wage contract mechanism best explains the short-term effect of the grant on wages. Here, the rent sharing represents implicit debt incurred by foregone earnings that the firm owes the employee (Michelacci & Quadrini 2009, Guiso, Pistaferri & Schivardi 2013).

Our data consist of 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, which follows Howell (2017). 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

[1] The rich literature on this subject includes Abowd & Lemieux (1993), Blanchflower, Oswald & Sanfey (1996), Abowd, Kramarz & Margolis (1999), Bell & Van Reenen (2011), Card, Devicienti & Maida (2014), Card et al. (2016), Carlsson et al. (2016), Mogstad et al. (2017), Goldschmidt & Schmieder (2017), and Helpman et al. (2017).

[2] See Decker, Haltiwanger, Jarmin & Miranda (2014).

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 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, we sometimes use the term "wages" instead of "earnings," following convention in the literature.[3] A benefit of these data is that they provide a well-defined and fairly homogenous sample of small, high-tech U.S. firms. Of course, the specific sample limits the extent to which we can extrapolate our results to other types of firms. For example, as the application process is quite onerous, applicant firms may be especially in need of funds.

We first examine the effects on average earnings and calculate rent-sharing elasticities. In firm-level regressions, we find that receiving a grant leads to a nine percent immediate increase in employee earnings. The positive impact of the grant begins in the quarter following the grant award and endures with statistical significance for at least five years. At the employee level with employee fixed effects, we find effects of three to four percent. The employee-level effect is smaller because larger firms naturally experience smaller effects. The effect is similar using the award amount per employee as of the year before the award, rather than an indicator for an award. This per-worker estimate is larger for smaller firms but does not reject the hypothesis that rent sharing has constant elasticity across firm sizes. The implied rent-sharing elasticity from the firm-level estimate is 0.21, which is smaller than the seminal estimate in Van Reenen (1996). The elasticity from the employee-level estimate is .07, which is similar to recent estimates with employee-level data, such as Card, Devicienti & Maida (2014). For the average firm, increased wages account for the entire grant amount about four (nine) years after the grant using the employee-level (firm-level) estimate. These results indicate that in the short run, the firms share some of the rent with workers. The effect does not appear to reflect more hours worked.

We next examine which employees benefit. Incumbent employees, hired before the application year, receive a 16 percent increase, which is consistent across the wage distribution. New hires, hired in or after the year of the award, do not benefit at all. The difference between incumbent employees and new hires is statistically significant. Among incumbent employees, by far the largest and most robust source of heterogeneity is tenure,

[3]We do not observe equity compensation such as stock option grants, though exercised options and bonuses are included. (However, the vast majority of private firms – even high-tech, young ones – do not grant stock to non-owner employees.)

or the number of years an employee has been with the firm (see Figure 3). The relationship between tenure and the grant effect is strong and linear; that is, longer-tenure workers benefit more. This is not driven by owners. We find no effects of interactions between the grant and relevant measures of labor market tightness. These results join Jäger et al. (2018) in suggesting that outside options are not especially important sources of wage variation.

We consider eight channels that might explain why wages increase after a cash flow shock. The evidence is most consistent with employment relationships compensating for financial frictions. The starting point for this mechanism is that applicant firms appear to be financially constrained. They are high-tech, involved in energy innovation, young, private, and small, all characteristics likely associated with financial constraints. Consistent with this, the grant has larger effects among firms that are younger and smaller, which we expect to be more constrained. Similarly, Howell (2017) finds that the grants positively affect subsequent innovation, with larger effects for smaller and younger firms. Existing literature suggests that we would not observe pass-through to wages among large, unconstrained firms that are relatively risk-neutral (Azariadis 1988, Dharmapala, Foley & Forbes 2011).

If the firm is financially constrained but can commit to long term contracts, employees can offer financing to the firm. In this case, the worker 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. Michelacci & Quadrini (2009) and Guiso, Pistaferri & Schivardi (2013) add financial constraints to the Harris & Holmstrom (1982) model to show how workers may lend to financially constrained firms. Our findings that new hires are unaffected and that the incumbent worker benefit increases with 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 initially paid below-market wages and that grew faster before the grant application. Also, long-tenure incumbent workers appear to pay a "constrained employer" penalty when they start at the firm, consistent with having accepted a backloaded contract. Finally, the effect of the grant is decreasing in the percent raise that the incumbents received when they joined.

To assess whether this mechanism is used in practice among firms in the data, we conducted a survey of DOE SBIR grantee principal investigators, who are almost always company CEOs. The survey asked directly about use of backloaded wage contracts as a result of the firm having been financially constrained. We received 99 responses, representing a response rate of 20 percent. The results indicate that the mechanism is used in practice, with 55.6 percent of respondents replying yes, 21.2 percent no, and 23.2 percent not explicitly answering the question.

If the effect for incumbent workers reflects backloaded wage contracts, two questions arise. First, do incumbent workers receive a premium for having accepted the risky contract? A back-of-the-envelope calculation suggests that they do. Second, do new workers receive flat or backloaded contracts? We do not find that awards lead to flatter wage-tenure profiles. This suggests that the firm may remain constrained and engage in similarly backloaded contracts with new hires. In this case, the effect on incumbent workers reflects a need to use an observable windfall to pay back employees with the most unvested human capital. This gives the firm credibility to engage in new backloaded wage contracts.

The evidence is much less consistent with seven other mechanisms: productivity growth or expected productivity growth, bargaining power, incentive contracting, agency frictions, dividends through wages, match quality models, and efficiency wages. The first four are the most plausible. First, the grant increases firm employment and revenue, which may be linked to productivity growth and could help explain the persistence of the effect on wages over time (note there are no effects on firm death or employee departures). However, productivity growth does not fully explain the immediate effects on incumbent earnings. For example, the entire earnings effects are observable within two quarters, while only part of the long-term revenue effect exists within the first two years. Productivity measured as revenue per employee does not increase substantially in the two years immediately after the grant. A revenue decomposition, in which we "instrument" for revenue growth with winning an award, finds a much smaller and weaker effect on earnings than the main effect of the award; only 16.9 percent of the total effect on earnings can be explained though a revenue channel.

The main argument against a bargaining model is that the cash windfall does not affect the worker's productivity. The firm may have more ability to pay after the grant, but this does not affect its cost of hiring a replacement worker, and thus does not change a worker's bargaining power. Six specific findings are inconsistent with either bargaining or incentive contracting: Immediate effects, persistent effects, no effects for new workers, no variation in proxies for skill, and no variation with measures of labor market tightness.

The third plausible alternative is that employees accrue agency power and become more entrenched over time. Such agency rents should cease when the free cash flow is exhausted, but instead the effect persists over time. More importantly, an agency story is observationally equivalent to the backloaded wage explanation. It requires us to ask why the employee's agency power didn't allow him to previously receive a higher wage. The answer must be that the firm faced financial constraints. Therefore, both models predict that after a cash flow shock, constrained firms increase wages based on incumbent tenure. The difference between the two models is the source for the wages implicitly owed to the employee; in the agency interpretation, the source is perhaps "friendship" with the owner. This is irrelevant to the key components of the backloaded wage mechanism, which are that (a) the constrained firm owes wages to employees and (b) this unvested human capital is increasing in tenure, leading a cash windfall to be shared proportionally with tenure.

The results shed light on how wages are set across firm and worker lifecycles, helping to explain why wages differ systematically across firms in ways that help shape inequality (Card, Heining & Kline 2013, Barth et al. 2016, Song et al. 2018, Bonhomme, Lamadon & Manresa 2019). We show that the cash windfall causes higher within-firm wage inequality. This reflects the combination of lower average earnings among new hires across all firms, no effect for new hires, and higher effects on levels of earnings among high-earning incumbents. The inequality results are consistent with the hypothesis 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 et al. 2017*a*, Mueller et al. 2017*b*, Song et al. 2018). The results suggest that inequality within the firm can increase while all incumbent employees arguably receive a "fair share" of rents, and new workers are treated equally as a group (see Edmans 2019). There is evidence from the psychology and behavioral economics literature that people dislike unfairness but not inequality (Starmans, Sheskin & Bloom 2017).

This paper contributes to studies of rent sharing and pass-through, including Black & Strahan (2001), Card, Devicienti & Maida (2014), Macis & Schivardi (2016), Bergman et al. (2017), Fuest, Peichl & Siegloch (2018), Friedrich et al. (2019), Garin & Silvério (2019), and Lamadon, Mogstad & Setzler (2019). These papers primarily study shocks that likely affect the employer-employee surplus, such as demand shocks or geographic variation, or they take a structural approach. In Sweden and Norway, respectively, Saez, Schoefer & Seim (2019) and Ku, Schoenberg & Schreiner (2020) find evidence that permanent tax policy changes affect firm rent sharing with workers.

The literature on innovation and wages is also related to this paper. It has found that inventor wages, average firm wages, and firm productivity increase after patent grants (Van Reenen 1996, Balasubramanian & Sivadasan 2011, Toivanen & Väänänen 2012, Bell et al. 2017, Aghion et al. 2018, Kogan et al. 2019). Kline et al. (Forthcoming) find that patent-instrumented surplus leads to higher wages among incumbent top earners but not among new hires, similar to our results. Their approach differs in several ways from ours, including the empirical design, source of variation, and mechanism (they argue that patents increase wages because some workers' marginal productivity changes). Relative to this literature, this paper has two innovations. One is the regression discontinuity design, permitting clear causal effects. The other is the use of a one-time cash flow shock rather than a patent, which represents a potential productivity increase and expectations of future cash flows. The one-time cash flow shock also has the benefit of permitting a precise calculation of rent sharing, as opposed to using an imputed patent value.

We further contribute to the literature on how firms spend cash in the presence of frictions (e.g. Hennessy & Whited 2007, Erel, Jang & Weisbach 2015). Starting with Fazzari, Hubbard & Petersen (1988) and Hoshi, Kashyap & Scharfstein (1991), the literature has focused on investment (see also Faulkender & Petersen 2012, Gilje & Taillard 2016, and Cespedes et al. 2019). This paper examines the labor side, joining recent work such as Schoefer (2015). Finally, an additional contribution is to provide the first causal evaluation of how R&D grants affect firm revenue, employment, and wages. Previous literature including Einiö (2014), Bronzini & Iachini (2014), Jaffe & Le (2015), and Howell (2017), focuses primarily on subsequent patenting and investment.[4]

2 Empirical setting

This section first describes the setting (Section 2.1), then explains the data sources (Section 2.2), and finally describes summary statistics (Section 2.3).

2.1 Institutional context

This paper uses data on applications and awards from the U.S. Department of Energy'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

[4]Using data on Finland and an IV strategy based on geographic variation, Einiö (2014) studies sales and employment effects but not wages. There are also structural approaches, including Takalo, Tanayama & Toivanen (2013). Also related is Lokshin & Mohnen (2013).

requires 11 federal agencies to allocate 3.2 percent 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 nine months of proof-of-concept work (the amount increased in two steps from $50,000 in 1983). Phase 2 grants of $1 million, awarded about two years after Phase 1, aim to fund later stage demonstrations. For both phases, 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 for both phases is onerous, taking a full-time employee one to two 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. However, to apply for Phase 2 a firm must (i) have spent the Phase 1 money as outlined in the application; (ii) demonstrate progress on the Phase 1 project; and (iii) not be more than 50 percent owned by outside private equity investors. Howell (2017) finds that these requirements lead to adverse selection in Phase 2 application, and 40 percent of winners do not apply to Phase 2. Consistent with Howell (2017), we find no effects of the Phase 2 grant (results are available upon request).

Each year, DOE officials in technology-specific programs (e.g., Solar) announce competitions in granular sub-sectors. The officials then rank applicants within each competition based on written expert reviews and their own discretion, according to three criteria: (i) strength of the scientific/technical approach; (ii) ability to carry out the project in a cost effective manner; and (iii) 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.[6]

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,

[5] Applicants must describe the project and firm in detail and provide an itemized budget for the proposed work. There are over 100 pages of instructions on DOE's SBIR Phase 1 application website. Interviews with grantees confirmed the 1–2 month time-frame.

[6] 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 indicated that 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.

and they must be relatively small (less than 500 employees). 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. 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.

2.2 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 $884 million (in 2012 U.S. dollars) 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. 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 non-farm sector with at least one employee, by EIN (when available) or probabilistic and then clerical matching on name, address, and zip code. About 70 percent 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 are able to link the firms to other Census Bureau datasets. 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 jobs. While bonuses or stock exercises would appear in W2 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 IPO and do not grant stock to non-owner employees.[7]

We also link to the Longitudinal Business Database (LBD), which begins in 1976 and ends in 2015. The LBD is the universe of non-farm, non-public administration business establishments with paid employees. We use three outcome variables from the LBD. The first is employment, which is observed quarterly after 2004 (before 2004, it is observed once per year, in the pay period that includes March 12). The second is payroll, which is observed quarterly throughout. The third is revenue, which is observed annually starting in 1996. We also use information from the Individual Characteristics File about employees of all firms in the Longitudinal Employee Household Dynamics dataset, which has similar coverage to W-2s These data provide demographic information about the employees. The sample sizes differ across outcomes, because data are not available for all firms for all outcomes. In particular, variables based on W2 data have considerably smaller samples. A disadvantage of our data is we lack information about occupation. Instead, we use proxies for skill that include education and pre-existing wage.

2.3 Summary statistics

The main summary statistics are presented in Table 1. 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, for all firms in the U.S. in 2012, the average number of employees is 20, and within establishments with 20-99 employees, the average is 39.[8] Average revenue in the sample is $4.8 million; though the distribution is highly right-skewed. 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 is high, at about 60 percent 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

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

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

is 8.3 years.

The primary measure of within-firm wage inequality is the 90/10 ratio, or the log wage difference between the 90th percentile and the 10th percentile. This is standard in the literature, including Goldin & Katz (2008), Van Reenen (2011), and Abowd et al. (2018). We also use the 99/50 ratio as a proxy for upper-tail inequality, and the standard deviation. The 90/10 ratio is preferred to the standard deviation in part because we expect the latter to mechanically increase if all employees' wages increase by the same percentage. In unreported results, we found generally similar effects using the interquartile range. Some of our analysis uses logged growth measures, defined as the log difference of an outcome in a given year relative to the year before application ($t = -1$): $Growth_{i,t} = \ln\left(\frac{Y_{i,t}}{Y_{i,t=-1}}\right)$. Table 1 Panel B shows that on average, these measures are small but negative, implying that they tend to be larger in the year before application compared to other years. This is because firms grow over time, with some attrition due to exits. Therefore, the outcome measures are on average lower in the years before the application than in the pre-application year, and there are more observations in this pre-application period. Note that the number of observations reflect data availability. Some statistics require W2 data, which are only available after 2005. Revenue is available in the LBD only for a subset of firms.

Employee-level statistics are in Table 1 Panel C. The average earnings among all employees at applicant firms is $63,500 (in 2010 dollars). Tenure averages 3.85 years. Consistent with existing work, tenure is correlated with wages; the correlation coefficient is 0.33. The subsequent rows in the table compare incumbent and new employees. The average firm has almost seven incumbent and four new employees by the second year after the award year (note 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. Panel D shows that incumbent workers are more educated, older, and have much higher earnings than new employees. However, they receive a smaller average wage increase relative to their previous jobs. The wage distribution among incumbent workers is more positively skewed but is significantly higher than new workers throughout the distribution.

Additional firm and worker characteristics are in Appendix Table A.1. As we might expect for applicants to an R&D grant program, the most common NAICS 3-digit industry is Professional, Scientific, and Technical Services, at 62 percent of firms.[9] The next most

[9]Industry is a firm-year variable because industry assignations may change over time within a firm.

common is Computer and Electronic Product Manufacturing, at 7.9 percent. The table shows an additional seven industries. The average worker is 43 years old. Just 22 percent of employees are female. There are also disparities relative to the population in ethnic makeup and country of origin; only 2.7 percent of employees are Black, for example, and just 71 percent are U.S.-born.

3 Estimation approaches

The ideal experiment would randomly allocate cash to a subset of firms, enabling us to examine the effect of the exogenous cash flow shock on firm outcomes. Following Howell (2017), we approximate this experiment 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 on average the differences in the true distance between ranks should be the same. That is, errors in differences on either side of the cutoff in any given competition should average zero. The primary concern is whether firm ranks are manipulated around the cutoff. The cutoff in a valid RD design must be exogenous to rank (Lee & Lemieux 2010). Howell (2017) provides five tests for manipulation, a discussion 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 non-awardees have similar observable characteristics, such as moments of the wage distribution, wage, and employee education.

The primary specification for evaluating the effect of a grant award is shown in Equation 1.[10] Here and below, i denotes a firm, k denotes an employee, j denotes a competition, and

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.

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

t denotes a year.

$$\begin{aligned} W_{i/k,t} = & \beta PostAward_{i,j,t} + \gamma Award_{i,j} + \delta Post_{i,j,t} \\ & + \eta_1 Rank_{i,j} + \eta_2 Rank_{i,j}^2 + \eta_3 Age_i + \eta_4 Age_i^2 \\ & + \lambda_{j/i/k} + \tau_t + \varepsilon_{i,j,t} \end{aligned} \tag{1}$$

We have chosen to use a panel setting, where each observation is a firm-year. This offers several advantages. First, while Howell (2017) provides extensive 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 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 et al. 2005 and Cardoso & Portela 2009). Finally, the panel permits a larger sample and thus more subsamples to be disclosed without reaching Census restrictions. We find similar results in a non-panel setting where each observation is an application.

A firm that ever wins a grant is assigned the non-time varying indicator $Award_{i,j} = 1$. 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. The main model uses an indicator for winning a grant, not the award amount per employee. This is because employment is also an outcome variable, creating potential concerns about endogeneity. However, we use award per employee in robustness tests at both the firm and employee level, and also use this to explore whether the effect on a per-worker basis exhibits constant elasticity across firm sizes.

The primary specification controls for rank within the competition quadratically, as shown in Equation 1. We do not use higher order polynomials, following Gelman & Imbens (2018). We also show the results controlling for rank separately among winners and non-winners. Since the number of applicants and awards varies across competitions, ranks are centered around zero. The lowest-ranked winner i in competition j has centered rank

$Rank_{i,j}$ =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 essentially the same point estimates. Due to disclosure limitations, we do not report specifications with narrow bandwidths around the cutoff, but the results are all qualitatively robust to those specifications.[11]

The dependent variable in Equation 1 is either a levels measure, such as the average wage of firm i or employee k in year t ($W_{i,t}$ or $W_{k,t}$), or a growth measure, such as $\ln\left(\frac{W_{i,t}}{W_{i,t=-1}}\right)$, where $W_{i,t=-1}$ is the firm's average wage in the year before the grant award year. The grant award year for rejected applicants is the year they applied and failed to win a grant. The growth specification ensures that unobserved time-invariant characteristics are controlled for, which is a conservative approach since we do not report specifications with narrow bandwidths. Levels outcomes are used to compare effects on new and incumbent employees, as "change" is undefined within the firm for new employees. The primary model includes competition fixed effects (λ_j) and calendar year fixed effects (τ_t). Other controls include the firm's age and age squared. In alternative approaches, we use firm-application fixed effects (λ_i), which subsume rank, award, and competition controls; the goal is to control more completely for pre-treatment differences, including all the characteristics of the application. Analysis at the employee level includes employee fixed effects (λ_k).

We graphically present results from two additional specifications. First, we show the effects by rank around the cutoff for the award using Equation 2.

$$\begin{aligned} Y_{i,t} = & \sum_{x=-6}^{x=3} \beta_x \left(PostAward_{i,j}\right)\left(Rank_{i,j} = x\right) \\ & + \eta_1 Age_i + \eta_2 Age_i^2 + \tau_t + \lambda_j + \varepsilon_{i,j,t} \end{aligned} \tag{2}$$

Outcomes are in levels (e.g. log employment), though the effects are similar when growth outcomes are used in Equation 2 instead. Second, we show the effects by quarter around the award quarter using Equation 3, where q denotes the quarter.

[11] In the remainder of this paper, there are numerous results discussed but not reported to limit disclosure burdens. While the samples underlying some results are simply too small to ever disclose, future drafts can report additional results as desired by readers.

$$Y_{i,q} = \sum_{x=-13}^{x=13+} \left[\beta_x \left(Award_{i,j} = 1\right)(q = x) + \delta_x (q = x)\right] + \tau_q + \lambda_i + \varepsilon_{i,j,q} \tag{3}$$

The coefficients of interest, β_x, are on the quarter indicators interacted with the award dummy, and these are shown in the graph. We include firm-application fixed effects. This specification is most stringent, as it controls for all possible application and firm characteristics. Again, outcomes are in levels. We find similar effects using competition fixed effects or growth outcomes. In estimating Equations 1, 2 and 3, standard errors are clustered by competition for firm-level analysis and employee for employee-level analysis, though the main effects are robust to a variety of error assumptions.

4 Grant effect on earnings

This section presents the main results. First, Section 4.1 describes the effect of a grant on earnings at the firm and employee levels. The result is decomposed across incumbent employees and new hires in Section 4.2. Employee characteristics, especially tenure, are considered in Section 4.3. Finally, Section 4.4 examines the effect of the grant on within-firm wage inequality.

4.1 Average earnings

Table 2 shows the grant effect on levels and growth of earnings at the firm level, using variations of Equation 1. The coefficient on $PostAward_{i,j,t}$ is the average effect of winning in years after the application year, controlling for whether the firm is a winning firm and whether the year is after the application year. The preferred main estimate in Column 1 finds an effect of nine percent, which translates to an increase of $5,773 at the mean wage. Column 2 includes fixed effects for the firm's state and an indicator for whether the firm is located in one of five top MSAs.[12] Appendix Figure A.2 A shows the effect on levels of log earnings by rank around the cutoff, using Equation 2.

The effect on earnings growth, the ratio of earnings in the current year to the base year,

[12] This is an indicator for being located in the MSAs of San Francisco, New York, Los Angeles, Texas triangle (Dallas-Fortworth, Austin-San Marcos, San Antonio and Houston), Boston, and Washington, D.C.

which is the year before the award, is considered in columns 5-8.[13] The main model in column 5 finds an effect of about 13 percent. In columns 6 and 7, we present two robustness tests. First, the effect is similar when rank is controlled for separately on either side of the cutoff. Second, the effect is also robust to including firm-application fixed effects (column 7), which absorb controls for rank and competition. The effect occurs quickly, with almost the entire effect observed within a two-year window of the application year (Table 2 column 8). Figure 1 demonstrates the effect on earnings by quarter around the award quarter, using Equation 3. The figure shows that the effect is immediate but persists over time, consistent with the long-term regression coefficients being very similar to the two-year coefficients. In Section 5, we consider the implications of persistence and the possibility that firm growth may fund wages in the longer term.

We next turn to employee-level analysis, in which we use log earnings as the dependent variable and include employee fixed effects. The results are in Table 3. For all specifications except column 4, the employee fixed effects absorb firm fixed effects, as each individual is observed only while employed at the applicant firm. The main estimates in columns 1-3 find effects of three to four percent; an estimate of 0.032 translates to a $2,032 increase in the mean wage. These are smaller than the firm-level estimates because larger firms are more heavily weighted than smaller firms at the employee level, and as we will see below, the effects are larger among smaller firms. The effect is somewhat larger within two years (column 3). Column 4 uses switchers to identify the effect by including employee-years before and after an employee worked at the SBIR applicant firm. This permits both firm and employee fixed effects. The estimate is higher, at 7.6 percent. We find no effect of winning on employee departures from the firm. The effect is also persistent over time at the employee level (not reported). Using the employee(firm)-level estimate of three(nine) percent, the grant can be "accounted for" entirely through wage increases after ten(four) years.

To situate the findings so far within the rent-sharing literature, we can approximate a firm rent-sharing elasticity. To motivate this measure, consider the relationship between rents per worker and wages posited by Card et al. (2018). This is motivated by a standard bargaining model, in which the wage reflects the reservation wage and a share of the joint surplus from the worker's employment relationship with the firm (Stole & Zwiebel 1996).

[13] The coefficient gives the percentage change in $\frac{Y_{i,t}}{Y_{i,t=-1}}$ associated with being an award recipient relative to a non-winner. The exact effect is $100 * (e^{\beta} - 1)$. Note it is relative to the year before the application (that is, the effect is not an absolute increase).

When the worker has more bargaining power, there is greater weight on the latter term, which is tied to firm productivity. We denote by w 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:

$$w = o + \gamma \frac{G}{N}. \tag{4}$$

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

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

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 (Card et al. 2018).[14] The parallel in our context is a calculation of the wage elasticity to the grant in the year following the award. The effect of the grant on levels of earnings is about nine percent in the first year (this can also be seen by quarter in Figure 1 Panel C). The average grant per employee, using employment in the year before the award year, is \$21,880, or 43 percent of the median wage. This implies a rent sharing elasticity ξ of 0.21 (.09/.43). In turn, we can use Equation 5 to approximate a rent-sharing parameter γ of 0.56.[15] At the employee level, the coefficient of 3.2 yields a rent-sharing elasticity of 0.07.

These elasticities are in the same general range as previous findings. In a seminal study, Van Reenen (1996) instruments for rents with innovation and finds a similar wage elasticity of about 0.25. Kline et al. (Forthcoming) 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 (Fakhfakh & FitzRoy 2004, Card et al. 2014, Card et al. 2016). Estimates using individual data are smaller and closer to our employee-level estimate, including Margolis & Salvanes 2001, Arai 2003, Martins 2009, Gürtzgen 2009, Carlsson et al. (2016), and Bagger et al. (2014). The literature has found larger rent-sharing effects when firm value added or

[14] 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 $\frac{\frac{G}{N}}{w}$.

[15] To proxy for the outside wage we use the median wage among firms that did not win an award in the year before the award year, as these are arguably the most similar firms to the winning firms.

profits are instrumented with a variable correlated with systematic or permanent changes in rents.[16] Cardoso & Portela (2009) and Guiso, Pistaferri & Schivardi (2005) find zero elasticities to transitory changes in value added or sales. The finding in this paper of a positive elasticity for a one-time cash flow shock (i.e. the immediate effect within the first few quarters) is, to our knowledge, new to the literature not only because previous work has focused on shocks directly associated with productivity or permanent rent changes, but also because it differs from previous studies of transitory shocks.

We also estimate the effect of log award per employee in Appendix Table A2.[17] The effect is shown at both the employee (columns 1-3) and firm (columns 4-5) 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 rents 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 award.[18] 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 is the best-fit across the coefficients. Finally, the red solid horizontal line shows 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 to some degree decreases in firm size. However, the result is not inconsistent with rent 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, when we split the sample by time period, for example around 2005 or 2008, we find similar effects on either side. The magnitude of the effect is somewhat larger in the earlier periods, but not statistically significantly so. Fourth, the effect is not driven by the first year after the award. When we omit the first year, the

[16]In addition to works cited above, this includes Abowd & Lemieux (1993), Guiso et al. (2005), and Arai & Heyman (2009).

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

[18]The quintiles are less than 6, 6-11, 12-23, 24-69, and more than 69 employees.

coefficient is similar and of equal significance.[19] Fifth, the effect is similar when multiple-time grant winners are excluded from the sample; that is, the result does not reflect future grants.

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, as pointed out by Kline et al. (Forthcoming). Second, more hours worked should affect both incumbent and new employees; as we show below, there is no effect among new employees.

4.2 Incumbent vs. new employees

We next examine how the positive grant effect on earnings established in the previous section is distributed across new and pre-existing (incumbent) employees.[20] Table 2 columns 3 and 4 and Table 3 columns 5 and 6 restrict the sample to either incumbent or new employees. Both tables strongly suggest that incumbent employees drive the average effect. Consistent with this, Table 3 column 7, at the employee level and using firm fixed effects, shows that an interaction between $\text{PostAward}_{i,j,t}$ and being an incumbent employee is .096 and highly significant. That is, an award increases the difference between incumbent and new hire earnings by about 10 percent. When employee controls for tenure, age, education, and wage in the year before the application year are added, the interaction coefficient increases to 0.15 (column 8). This does not reflect partial earnings in a new employee's first year. When the first year of work is omitted, the coefficient for the new worker sample is larger, but still significantly lower than the effect for incumbents and not significantly different from zero. Appendix Table A2 Column 2 shows that the effect remains significantly larger for incumbent employees using the award amount per employee as the independent variable.

Table 1 Panel D compares new and incumbent workers. The first set of statistics shows that incumbent workers are more educated, older, and have higher average earnings. The second set shows that the large difference is roughly consistent across the wage distribution. Despite these differences, the specification with controls suggests the incumbent-new differential is unlikely to be fully explained by skill. Also consistent with

[19] This creates especially small implicit samples with other samples, and cannot be reported.

[20] We exclusively use level outcomes because there are no new employees in year $t = -1$ with which to construct growth measures.

this, and perhaps counterintuitively, the large positive effect for incumbents persists at all points in the wage distribution, which is shown in Table 4. Here, the dependent variables are the within-firm 10th, 50th, 90th, or 99th percentile earnings. Chetverikov, Larsen & Palmer (2016) explain how this type of quantile regression panel estimator is consistent and asymptotically normal. The effect is the same, at about 15 percentage points, at the 10th and the 90th percentiles. This consistency across the wage distribution is not driven by very small firms where all employees might plausibly be a narrow group of co-founders. When we eliminate firms below the 25th percentile of employment from the sample, we continue to find consistent effects across the wage distribution, though they are slightly smaller at the higher end.

The difference in the wage effect between new and incumbent workers could reflect a compositional effect. That is, perhaps the selection of new hires is different at winning firms than at non-winning firms. For example, it may be that winning firms hire lower skill workers on average but pay them relatively more. However, new workers have similar education, age, earnings in their previous job, and percent raise when they arrive at rejected and winning firms, suggesting that different selection is not an especially important factor.

4.3 Tenure and other employee characteristics

To explore what may explain the large effect of the grant on incumbent earnings, we interact winning an award with various employee characteristics within the sample of incumbent employees.[21] 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. Table 5 column 1 shows that an additional year of tenure increases the effect of winning on wage by 1.2 percent, which is about 25 percent of the average employee-level effect (mean tenure is 3.8 years). To assess whether this is an artifact of skill or employee age, we add controls for employee age, education, and pre-existing wage percentile (column 2) or pre-existing linear wage (column 3). The effect persists with essentially the same magnitude as in column 1. Appendix Table A2 Column 3 shows that the effect continues to increase in tenure with the award amount per employee as the independent variable.

The effect is markedly linear in tenure. Figure 3 shows coefficients from a regression with

[21] For wage, inequality, and growth outcomes, we examined heterogeneity in a wide array of firm, employee, and location characteristics. We found no significant and robust interactions besides those described here. There is no effect of heterogeneity in the share of employees of a certain gender, age bin, or race/ethnicity.

separate dummies for years of tenure interacted with winning, among incumbent employees. The effect increases linearly through ten years (subsequent years are similar but very noisy). While the effects at two and three years are negative, they are not significantly different from the effects at one year. A quadratic specification in Table 5 column 4 confirms the relationship. The coefficient on $\text{PostAward}_{i,j,t} \cdot {\text{Tenure}^2}_{k,t}$ is negative and significant, albeit economically small, so the effect on earnings is somewhat concave in tenure. The tenure effect does not appear to reflect firm owners. Column 5 restricts the sample to incumbent employees hired at least three years after the first year the firm is observed, who are not plausibly owners. The result is similar to Column 1. There is no measurable effect of the award on the firm's wage-tenure profile. As has been shown in the overall universe of firms (e.g. Brown 1989), there is a positive relationship for both awardees and non-awardees, and the difference between them is not statistically different.

Other characteristics, again among incumbent employees, are considered in Table 6. For parsimony, we show only the main interaction of interest. Columns 1 and 2 show that while there is a positive association between employee age and benefit from the award, this disappears with other employee controls. We do find persistent positive effects in education and wage (columns 3-7), but they are all small in magnitude. The effect of having at least a BA is about three percent, relative to mean of 46 percent. Interacting with four parts of the pre-existing wage percentiles, where earnings less than the 10th percentile are the omitted group, we find that the effect is largest for the top 10 percentiles. The linear effect of interacting winning with log pre-existing wage is three percent, significant only at the .1 level. Note that despite the firms being small with just seven employees on average in the year before the award, there is substantial variation in pre-existing wages, with the standard deviation being about 60 percent of the mean in the year before the award. In sum, while the effect of the cash flow shock on earnings does increase with measures of employee skill, and especially for top earners, the effect of tenure is by far the largest economically, even after conditioning on the employee's wage.

4.4 Wage inequality

The heterogeneity established above suggests that the cash flow shock may affect within-firm inequality. We find large and robust positive effects on the three inequality measures in Table 7. Columns 1-4 use growth outcomes, and columns 5-10 use levels outcomes. A grant increases the growth of the 90/10 ratio by 24 percent (column 1), and the effect is in fact slightly larger when only the first two years after the application are included (column 2).[22] The effect on upper-tail inequality growth (the 99/50 ratio), shown in column 3, is smaller, at about eight percentage points. Note that when there are 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 substantial, even when the firms have few employees.

The inequality effects contrast with the positive effect among incumbents at all points in the wage distribution (Table 4), which is something of a puzzle. The answer is that the difference between new hire and incumbent earnings drives the effect on inequality. Table 7 columns 6 and 7 show that there is no effect of winning on inequality within incumbents or new hires, consistent with Table 4. New hires induced by the grant do not receive an above-market wage and 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. Since incumbent high-wage employees receive a large bump and there are few new high-wage employees, the average effect on inequality comes from the top of the distribution.

Our results highlight how a windfall is different from making incentives more high-powered. Bandiera, Barankay & Rasul (2007) show that the introduction of managerial incentives leads to higher within-firm wage inequality. They find that this is driven by managers targeting their effort towards making the most productive workers even more productive. In our case, the increase in wage dispersion comes in part from the extensive margin, where new and relatively lower wage workers are hired. These results shed light on both within- and across-firm wage inequality, helping to explain why workers with similar skills are paid different amounts depending on where they work, and why it may be profitable for firms to outsource low-skill services as they grow (Goldschmidt & Schmieder 2017). Within our sample of small, high-tech firms, within-firm inequality appears to increase with growth as the firm "fleshes out", hiring more relatively lower skilled workers.

[22] Figure A.2 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 W2 data used to construct inequality measures are annual.

5 Effects on firm growth

An immediate effect of the grant on growth might explain the immediate effect on wages we observe. We cannot observe profits or productivity, but we can observe revenue and total employment. This section shows that there are positive effects of the grant on these growth measures in the longer term, but they do not fully explain the immediate effect on wages.

The effects of the grant award on these growth measures are presented in Table 8. The effect of winning a grant on log employment relative to the base year is the coefficient on $PostAward_{i,j,t}$ in columns 1-4. The coefficients on quadratic rank (column 1) and on either side of the cutoff (column 2) are also shown. Firm-application fixed effects are included in column 3, which soak up the controls besides age and year. The coefficient of 0.27 means that a grant award increases employment growth (the ratio of employment in the current year to the base year) by about 30 percent.[23] Evaluated at the means, this indicates that winners have about 19 percent more employees than losers, or on average 6.7 more employees, relative to the year before application. Column 4 shows that about half the effect on employment occurs within two years of the grant application. Appendix Figure A.2 Panel C demonstrates the effect on levels of log employment by rank around the cutoff.

A grant award increases revenue growth by about 20 percent, or 15 percent more revenue than in the pre-application year (Table 8 columns 5-8). Again, just over half the effect on revenue occurs within two years of the grant application.[24] Appendix Figure A.2 Panel D demonstrates the effect on levels of log revenue by rank around the cutoff. The effect on employment within two years is statistically indistinguishable from the effect on revenue within two years (columns 4 and 8). Indeed, productivity measured as revenue per employee does not increase substantially in the period immediately after the grant. We also examined firm exit in the forms of firm acquisition and death but found no measurable effects on these outcomes.[25]

To explore whether the effect on employee earnings shown above is primarily a function of increased revenue or profitability, we conduct two tests. The first decomposes the effect

[23]The coefficient gives the percentage change in $\frac{Y_{i,t}}{Y_{i,t=-1}}$ associated with being an award recipient relative to a non-winner. The exact effect is $100 * (e^{\beta} - 1)$. Note it is relative to the year before the application (that is, the effect is not an absolute increase).

[24] There is no quarterly graph because Census does not have quarterly revenue data.

[25]We define exit via acquisition as an instance in which the last establishment year is later than the last firm year. This indicates that the establishment continues but the firm dies. We define failure as establishment and firm exit from the panel.

into that which goes through revenue and that which goes straight to earnings. We do this by instrumenting for revenue growth with the grant. The first stage regresses revenue growth on the grant, and the second stage regresses wage growth on the revenue growth predicted by the grant. We do not report the first stage to minimize disclosure requirements. The Cragg-Donald F-statistic is 249. Table 9 column 1 reports the coefficient on the second stage, which is 0.08, significant at the .1 level. Both revenue growth and wage growth are logged, so the interpretation is an elasticity; a 100 percent increase in instrumented revenue increases earnings by about 8 percent. Since the effect on revenue is about 20 percent, this implies that revenue instrumented with the grant increases wages by about 1.6 percent, which is 16.9 percent of the main effect of the grant on wages (from Table 2 column 3). That is, while some of the grant's effect on revenue is passed to earnings, a maximum of about 17 percent of the total effect on earnings can be explained though a revenue channel.

The second test shows that the immediate effect of winning is not higher among firms with higher growth or innovation in the two years after the grant. First, we interact winning with revenue growth, and find no statistically significantly effect (Table 9 column 2). The same is true for employment growth (column 3). Third, we interact the number of cite-weighted patents that the firm applies for and is ultimately granted during the two years after the application year, a measure of innovation quality. The coefficient on the interaction is negative and significant (column 4). Results are similar when longer time frames are used. These results demonstrate that the effect on earnings is not larger among firms that are able to grow more in the immediate years after the award. In sum, it is clear that the pass-through to wages does not entirely reflect a productivity-related channel, and in the short term is quite independent from growth.

6 Financial constraints as an explanatory mechanism

The results thus far have found that on average the grants immediately and persistently affect wages for incumbent but not new hires. They also affect growth within a few years of the grant, but a growth channel does not fully explain the immediate wage effects. These results are somewhat puzzling; in particular, it is not intuitive that a one-time cash flow shock would yield permanent effects on wages for a subset of employees.

This section describes an economic mechanism that is particularly consistent with the data: Early employees implicitly finance the firm through backloaded wage contracts. We first provide theoretical background in Section 6.1. Empirical support for the mechanism, including additional analysis and a survey, is in Sections 6.2-6.4. Section 6.5 discusses how the implicit contract may be enforced.

Section 6.6 briefly discusses key points about other plausible mechanisms. In Appendix B we examine these in greater detail, separately considering the evidence for and against six alternative hypotheses: 1) A standard neoclassical model, or payment of the grant as a dividend to owners via wages; 2) Match quality revealed over time (Jovanovic 1979); 3) Efficiency wages (Akerlof & Yellen 1988); 4) Incentive contracting (Lazear 1981); 5) Benchmark employee bargaining power (Stole & Zwiebel 1996); 6) Agency frictions (i.e., entrenchment; Berk, Stanton & Zechner 2010). The evidence either contradicts or is not fully consistent with the main predictions of these models.

6.1 Lending within the firm

The financial mechanism of within-firm lending begins with wage-tenure profiles. An initial literature, including Azariadis (1975) and Bernhardt & Timmis (1990), argues that wage-tenure dynamics are flatter than they would be in the absence of financial frictions because relatively more risk-neutral firms insure relatively more risk-averse workers. Dating back to Harris & Holmstrom (1982), the flat wage contract provides optimal risk sharing by enabling workers to smooth consumption, which they cannot achieve by borrowing in outside financial markets.

Later work takes note of the stylized fact that wages tend to correlate strongly with tenure, especially in small firms. Michelacci & Quadrini (2009) model how a financially constrained firm may optimally pay workers lower wages initially, implicitly borrowing from them. This enables the firm to grow faster than it would otherwise. Their theory reconciles several stylized facts: larger (but not older) firms pay higher wages, firms growing faster pay lower wages, and firms with more financial pressure pay lower wages. Guiso et al. (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.[26] A similar prediction is in the model of Garmaise (2007), where workers agree to employment at risky, financially constrained firms without compensation for the extra risk because they have the

[26] A key assumption in their work is that better workers do not sort differently across provinces.

option to quit. In his model, financially constrained firms share a larger portion of future profits with workers. Finally, a broader view on these relationships can be found in models of insurance within the firm, including Guiso et al. (2005) and Cardoso & Portela (2009).

Next we turn to testing predictions of models in which financially constrained firms offer backloaded wage contracts, where the wage rises after a windfall. If the firm uses the grant to repay implicit financing from employees, a number of predictions arise: The effect should be larger among firms that are more constrained, and as a result initially paid below-market wages. The effect should also be larger among firms that grew faster before the grant application. Clearly, only incumbent employees should be affected, and importantly, their "unvested human capital" should increase with job tenure. In the following sections, we consider how the evidence supports these predictions, and then discuss enforcement.[27] Importantly, we are agnostic about the source of foregone wages. The counterfactual higher wage without constraints might reflect any number of wage-setting forces, such as the outside option, bargaining power, or agency rents.

6.2 Financial constraints

In the absence of financial frictions, firms should make all positive NPV investments. In contrast to a productivity or future cash flow shock, unconstrained firms should not respond to a cash flow shock by growing. The fact that they do points to financial constraints. Note that applicant firms have undergone an onerous application process that is not only time intensive but requires substantial disclosure to the government and some public disclosure if a grant is awarded. We should expect that managers believe their firm needs the grant else they would not apply. A similar cash windfall at a random firm of the same size and industry would likely have a smaller effect. For perspective, it is useful to consider publicly traded firms. There is evidence that public firms spend tax holiday-induced cash windfalls from repatriation primarily on dividends, not wages (Dharmapala, Foley & Forbes 2011).[28] This is more consistent with the flat wage-tenure profiles theorized in Azariadis (1988), where risk-neutral firms insure risk-averse workers. Large publicly traded firms with significant

[27] If the data do not support these predictions, it does not mean that the mechanism is not at play. Even if the grant reduces financial constraints, the firm may remain constrained, and even if implicit lending within the firm is occurring, the firm might spend the grant only on other things.

[28] Relatedly, Blanchard, Lopez-de Silanes & Shleifer (1994) ask what public firms do with a cash windfall. Using a sample of 11 firms that won lawsuits, they find that managerial cash compensation rises 84 percent after an award, which they conclude best reflects severe agency problems between managers and shareholders.

overseas cash holdings likely have good access to capital markets, unlike the small, young, private firms in our data. The differing responses to a cash windfall may reflect this disparity. The Azariadis (1988) model can help explain the lack of pass through among large public companies, while the Michelacci & Quadrini (2009) model can help explain the large pass-through and steep wage-tenure profile observed here.

With this background in mind, within our sample the backloaded wage contract predicts larger effects among firms that we expect to be more constrained. We find at the employee level in Table 10 that the grant is more useful for smaller and younger firms. Columns 1 and 2 show the effect of winning interacted with indicators for top quartile employment and age in the year before the grant award year.[29] In both cases, the coefficient is large and negative. The result in Column 1 is to some degree mechanical, because the grant is the same size for all firms. However, the results are supported by the finding in Howell (2017) that winning has a larger effect on innovation and VC among smaller and younger firms, and imply that the results are likely driven by more constrained firms.

Four additional pieces of evidence are consistent with financially constrained firms offering a backloaded wage contract. First, these contracts are most useful when the firm needs to grow fast, so Michelacci & Quadrini (2009) predict that firms growing faster should initially pay lower wages. Consistent with this, we find that firms growing faster before the application year experience larger effects. Specifically, in Table 10 column 3, we interact winning with revenue growth between three and one years before the grant application year. The coefficient is strongly positive, consistent with the effect stemming from fast-growing firms that substitute other investments for wage payments.

Second, firms that tended to pay more before the grant are less likely to be using these backloaded wages contracts. Indeed, firms that paid above-median wages in the year before the application year tend to experience a smaller effect of the grant on wages, shown in Table 10 column 4. Third, the finding that there is, if anything, some substitution in the years after the grant between wage increases and investment (Table 9 columns 2-4) is consistent with those firms that remain constrained using less of the grant to repay existing backloaded wage contracts. Finally, we expect that wages will increase as profits rise if they are initially pushed down by firm financial constraints. Consistent with this hypothesis, we find that on average as revenue increases, wages rise more for workers with high tenure (we do not

[29] We use indicator variables here because at the employee level, these variables are quite skewed. These relationships persist at the firm level.

observe profits).[30]

6.3 Incumbent status

If the grant is used to pay out existing backloaded wage contracts, only incumbent employees should be affected. Indeed, this is what we find. We would also expect that the firm "owes" the most to 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.

If incumbent workers accept a backloaded contract, their initial wage should reflect a "constrained employer" penalty, and their benefit should be increasing in this penalty. Table 10 shows evidence consistent with both of these predictions. First, column 6 shows that the percent 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. Second, column 5 shows how the effect varies with the employee's percent raise when he was hired relative to his previous job. The interaction term indicates that the effect on earnings decreases in the percent raise. In other words, workers who accepted higher wage penalties when they joined the firm receive a larger benefit because of the grant award.

Several descriptive facts about the percent raise are worth reporting, as this is seldom examined in the literature. The median worker at the firms in our sample accepts a lower wage when he joins than he earned at his previous firm. This median pay penalty is about 6 percent (Table 1 Panel C). However, there is substantial skewness. Table 1 Panel D shows that the average percent raise is positive and significantly larger for new employees.[31] There is no difference in the percent raise among new hires across award status, consistent with the absence of an effect among awardees generally. Also, the percent raise is smaller among firms we expect to be more constrained (Table 10 columns 7-8).[32] The award does not affect the percent raise for new hires, consistent with there being no different composition of new hires across firm types.

[30]We did not disclose this result as revenue is not observable for some observations and so a new sample is created that led to excessively small implicit samples with the samples of other disclosed results.

[31]Note the 24 percent average raise across the whole distribution is on the high side but not dissimilar to data on pay raises in general for highly educated individuals. E.g., see for data scientists: https://www.burtchworks.com/2019/05/13/2019-update-analytics-salary-increases-when-changing-jobs/

[32]Note we do not conduct this exercise comparing across awardees and non-awardees because it is irrelevant, as there is no effect for new hires, and incumbents' raise is a pre-application event.

We do not find evidence that there is a significant change in the overall wage-tenure profile after the grant, suggesting that the firm may remain constrained and engage in similarly backloaded contracts with new hires.[33] The grant does not leave the firm unconstrained – in fact, to the degree the firm uses the grant to fund growth, it may engage in contracts that are even more backloaded. The effect on incumbent workers could reflect a need to use part of an observable windfall to "pay back" employees with the most unvested human capital. This gives the firm credibility in engaging in new backloaded wage contracts.

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 using the main results and descriptive statistics suggest a substantial premium for workers with seven years of tenure at the time of the grant (seven 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 three years. Within seven years, the additional income will further compensate for a reasonable assumption about lower wage growth.[34] 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.

6.4 Survey evidence

Thus far, we have provided evidence for the mechanism that is cross-sectional and therefore inherently more descriptive than the causal analysis that establishes the main effect of the grant on wages. The ideal test would observe whether firms in the data are actually using

[33]Using a within-firm annual measure of the correlation between tenure and wage, we found no difference between winners and non-winners post-award decision. Among new employees, the tenure-wage profile is also not statistically different across winners and non-winners.

[34]The closest measure we have to the average unconstrained wage bump is the bump for new hires among awardees, which is 24 percent. The percent increase is decreasing by .025 on average per year of tenure (Table 10 column 7). A worker with seven years of tenure (about one standard deviation above the mean) therefore "missed out" on about 4.2 percent of wage gain when hired. The average wage in the last year of the previous job is $47,570, implying that he missed out on $1,997 per year. The increase in wages due to the grant is about nine percent. 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 percent, and reinvesting the income on it, the foregone earnings total $17,776. It therefore takes between two and three years after the grant to make up for this lost income.

backloaded wage contracts as a result of financial constraints. To assess whether this mechanism is used in practice, we conducted an email survey of DOE SBIR grantee principal investigators, who are almost always company CEOs.[35] 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.[36] Among these, 88 addresses bounced. We received 99 responses, representing a response rate of 19.9 percent. The full text of the email is shown in Appendix Figure A.3, which also includes an actual response (with permission from the responder).[37] Across the 99 respondents, 55.6 percent replied yes, 21.2 percent no, and 23.2 percent did not explicitly answer the question. The sample response in Appendix Figure A.3 is representative of the fact that most responders directly answered the question while also generously providing qualitative color. Three additional examples are as follows (also with permission). First, 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."

Second, Ron Sinton, Founder and President of Sinton Instruments, wrote:

[35] 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, so did not require IRB approval.

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

[37] The SBIR grant for this responder is publicly filed under the firm name "ProjectEconomics," available at https://www.sbir.gov/sbirsearch/detail/880883.

> "I would say "yes". I effectively do this by supplementing salaries with discretionary year-end bonuses, proportionate to base salary for each employee....that a bonus depends on the vagaries of profits rather than effort does not always sit well with some employees...I try to emphasize that I hand out cash rather than stock because stock value is an optimistic scenario that may not materialize."

Third, Tom Heiser, President and 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."

These quotes highlight the positive answers regarding the mechanism. They also join many other responses in emphasizing the non-pecuniary amenities of working at a small, high-tech firm. Motivating employees to feel that they are part of a larger, important mission seems integral to the incentive compatibility of these implicit labor contracts and seems a fruitful avenue for future research.

It is important to caveat the results: There are no doubt biases in both the subset of all grantees that we reached and in the decision to respond. Nonetheless, the results offer strong support for the mechanism. The survey responses indicate that grantees have often used backloaded wages contracts as a result of having been financially constrained and share windfalls with workers as a way to repay these contracts.

6.5 Enforcement mechanism

Why doesn't the firm renege on backloaded wage contracts? The evidence suggests that firms remain constrained after they receive the grant. Yet they increase wages permanently, raising their expenses and thus potentially becoming more constrained. One explanation is that higher pay was formerly positive net present value but the firm wasn't able to take on this "project." The combination of the grant and subsequent growth allow it to pay the optimal wage. Employees expected part of their compensation to be in the form of higher wages when the firm succeeds, leading to an implicit financing contract. The question remains, however, how the firm is able to commit to this *ex-ante*. In Michelacci & Quadrini (2009), 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. In this way, the enforcement mechanism in Michelacci & Quadrini (2009) is a type of bargaining power. However, this holdup problem should be double-sided. If the human capital of the employee is to some degree firm-specific, the firm should in theory be able to hold up the employee just as well as the reverse. Also, new hires should have the most bargaining power as they are actively choosing between firms and have no firm-specific capital. Yet we find no effect among new hires.

An alternative enforcement mechanism is concern for fairness or reputation. Sharing rents in a manner deemed fair by employees could benefit the firm in the long run (Lazear 1989, Kahneman, Knetsch & Thaler 1986). In an implicit contract, worker loyalty yields more productivity, and in exchange employees are guaranteed a share of firm rents (Howell & Wolff 1991). Indeed, establishing a good reputation and building trust with employees appear to play a role in real world wage bargaining outcomes (Blanchard & Philippon 2006). There is abundant evidence that fairness – especially relating to relative pay – shapes employee wage perceptions. This literature includes Falk, Fehr & Zehnder (2006), Card, Mas, Moretti & Saez (2012), Breza, Kaur & Shamdasani (2017), and Dube, Giuliano & Leonard (2019).

The results suggest that inequality within the firm can increase while all incumbent employees receive a "fair share" of rents. There is evidence from the psychology and behavioral economics literature that people dislike unfairness but not inequality (Starmans, Sheskin & Bloom 2017). Edmans (2019) suggests that penalties for high within-firm inequality, exemplified by taxes or divestment campaigns targeting companies with high pay ratios, may be misplaced if the pie grows for all employees even as inequality increases.

6.6 Other mechanisms

We have already shown in Section 5 that an increase in firm growth does not explain the effect on earnings. Appendix B contains detailed consideration of alternative mechanisms. Here we discuss a few key points relating to the three most plausible possibilities, which are bargaining, incentive contracting, and agency models.

First, in bargaining models the wage is based on employee productivity and the outside option (Stole & Zwiebel 1996, Hall & Milgrom 2008). When a cash windfall occurs, the worker's productivity has not changed, quite unlike the theoretical model in Kline et al. (Forthcoming), where wage effects come from the changes to marginal productivity that happen after a patent grant. Thus, bargaining models predict no immediate effect of the cash windfall on wages, because the firm's greater ability to pay does not affect its cost of hiring a replacement worker, and thus does not change a worker's bargaining power. It is therefore inconsistent with bargaining to observe the entire effect on wages within the second quarter after the grant. If immediate gains reflected bargaining over expected future productivity growth, these gains should be proportional to the benefit that the employee will provide and should also accrue to new workers. Yet we find no variation with proxies for skill, such as pre-existing wage or education, and no effects for new workers.

Relatedly, in a bargaining model, we expect workers with less power to have wages that move more closely with their outside option. To test for this, we interact the effect of the award with measures of labor market tightness but find no interaction effects at any point in the wage distribution. Finally, the larger effect among more financially constrained firms would not be expected in a bargaining model unless the worker had foregone previous wages, which is observationally equivalent to the backloaded wage contract mechanism.

A second plausible mechanism is incentive contracting. This should yield a "bonus" type payout that would be temporary. Instead, we observe permanent increases. 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. It seems unlikely that low wage workers, such as administrative assistants, would have been pivotal to receiving an R&D grant. Finally, there is no reason an incentive contracting mechanism would reflect measures of financial constraints.

A third alternative is that employees accrue agency power and become more entrenched over time (Berk et al. 2010). One challenge to an agency frictions channel is that the effect persists over time. We would normally expect agency rents to cease when the free cash flow is exhausted. More importantly, an agency model is fundamentally observationally equivalent to the backloaded wage explanation. To illustrate this, suppose an employee is not paid his reservation wage, and there are two possible explanations: (1) He has implicitly agreed to a backloaded wage and knows that when a cash windfall occurs he will be compensated for foregone wages; (2) He knows that the employer will treat him "fairly" by sharing with him in proportion to his tenure at the firm. The second model – the agency story – requires us to ask why his agency power didn't allow him to previously receive a higher wage. The answer must be that the firm faced financial constraints, which prevented him from extracting more agency rents. Therefore, both models predict that after a cash flow shock, constrained firms increase wages based on incumbent tenure. The difference between the two models is the source for the wages implicitly owed to the employee. In a more classical interpretation, they stem from the employee's outside option or, in a bargaining model, his productivity. In the agency interpretation, the source is perhaps the employee being "friends" with the owner. The source is orthogonal to the key components of the backloaded wage mechanism, that (a) the constrained firms owes wages to employees and (b) this unvested human capital is increasing in tenure, leading a cash windfall to be shared proportionally with tenure.

7 Conclusion

A firm might spend a cash flow shock on dividends (i.e. transfer it to owners or shareholders), wages, or investment in physical or human capital (i.e. new hires). 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, likely financially constrained firms. In addition to being economically important, small firms are particularly interesting because their employment and wage structures are especially dynamic. 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 rents.

We show that the cash flow shock significantly increases wages only among incumbent employees who are present at the time of the grant application. The effect on incumbents increases essentially linearly in worker tenure. The grant also increases within-firm wage inequality, employment, and revenue. However, a growth channel does not fully explain the effects on wages. The results are most consistent with the firm sharing rents with employees to pay out backloaded wage contracts, a form of implicit financing that the employee provides to the firm. 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 & Williams 1994, Carpenter 2007, Elfenbein et al. 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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Table 1: Summary Statistics

A. SBIR Phase 1 competition data (counts)

	N
Unique applicant firms	2100
Applications	4300
Grant award winners	800
Grant award non-winners	3600
Competitions	270

B. Firm-level outcome and control variables (firm-year)

<u>Levels statistics</u>

	N	Mean	Std Dev	Median†
Payroll ('000 2010 \$)	30500	2546	6141	689.5
Employment	30500	35.36	72.17	11.51
Employment$_{t=-1}$	30500	6.86	4.45	16.9
Award amount/employment$_{t=-1}$	30500	21880	33690	9106
Average earnings ('000 2010 \$)	30500	64.15	38.55	57.85
90/10 log earnings differential	9600	1.809	1.053	
99/50 log earnings differential	9600	0.951	0.702	
Standard deviation of log earnings	9600	0.861	0.325	
Revenue ('000 2010 \$)	13000	4834	11410	
Firm age	30500	12.38	8.539	
Subsequent patent citations (3 year window)	30500	2.071	10.81	
Never previously won an award	30500	0.57		

<u>Log growth statistics (base is $t = -1$)</u>

	N	Mean	Std Dev	Median†
Payroll	30500	-0.105	1.245	-0.0015
Employment	30500	-0.082	1.008	0
Earnings	30500	-0.023	0.825	0
Revenue	13000	-0.048	1.078	
90/10 differential	7500	-0.0015	0.983	
99/50 differential	7500	0.0028	0.599	
Standard deviation	7500	0.0048	0.334	

Note: 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.

C. Employee variables (SBIR applicant firms)

	N	Mean	Std Dev	Median†	Level of observation
# unique individuals in sample	73000				Person
Earnings at SBIR firm ('000 2010 $)	257000	63.50	86.54	49.99	Person-year
Earnings all jobs ('000 2010 $)	909000	58.92	84.39	44.45	Person-year
Tenure at SBIR firm (years)*	257000	3.85	3.11	3	Person-year
Percent raise from last year of previous job to first year at SBIR firm	62000	0.24	1.32	-0.061	Person
As of 2nd year after award, firm # of:					
Incumbent employees	2300	6.689	12.38	5	Firm
New employees	2300	4.036	24.32	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_k (BA or above)	49500	0.45	11500	0.358	0.00
$\text{Age}_{k,t}$ (years)	49500	43.11	11500	36.99	0.00
$\text{Earnings}_{k,t}$ ('000 2010 $)	49500	68.98	11500	39.70	0.00
Percent $\text{raise}_{k,t}$ ('000 2010 $)	49500	0.224	49500	0.243	0.09
Firm-level, all years					
10th pctile $\text{earnings}_{i,t}$ ('000 2010 $)	8200	19.34	3200	12.46	0.00
50th pctile $\text{earnings}_{i,t}$ ('000 2010 $)	8200	40.54	3200	22.93	0.00
90th pctile $\text{earnings}_{i,t}$ ('000 2010 $)	8200	76.88	3200	44.80	0.00
99th pctile $\text{earnings}_{i,t}$ ('000 2010 $)	8200	94.85	3200	50.51	0.00

Note: 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. 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, and CBDRB-FY19-452.

Table 2: Grant Effect on Earnings (Firm-Level)

Dependent variable:	Log earnings				Log earnings growth			
Sample:			Incumbent	New				2 year window
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}$	.0931**	.0751**	.137***	0.0607	.134***	.133***	.0946**	.126***
	(.0387)	(.0373)	(0.0482)	(0.0753)	(0.048)	(0.0481)	(0.0391)	(0.0406)
Controls								
$\text{Award}_{i,j}$	Y	Y	Y	Y	Y	Y	N	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y	Y	N	N	Y
Rank | $\text{win/lose}_{i,j}$	N	N	N	N	N	Y	N	N
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	Y	Y	Y	Y	N	Y
State, Top MSA FE	N	Y	N	N	N	N	N	N
$\text{Firm-app}_{i,j}$ FE	N	N	N	N	N	N	Y	N
N	30500	30500	8200	3200	30500	30500	30500	20000
R^2	0.0924	.119	0.142	0.135	0.0988	0.099	0.449	0.0738

Note: This table shows the effect of the grant on earnings levels (columns 1-4) and earnings growth (columns 5-8), using Equation 1. The base year for growth measures is $t = -1$, the year before the application year. Rank is controlled for quadratically, on either side of the cutoff, or through firm-application fixed effects ($\text{Firm-app}_{i,j}$ FE, which also absorb award and competition). Column 8 restricts the post sample to the two years after the grant application year (this includes the application year). Control coefficients are not reported to minimize disclosure requirements. Data are observed at the firm-year level. Standard errors are clustered by competition. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosures CMS request 7276 and CBDRB-FY19-369.

Table 3: Grant Effect on Earnings (Employee-Level)

Dependent variable: Log earnings

Sample:			2 year window		Incumbent	New		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}$	.032**	.029**	.042**	.076***	.038***	-.044	-.126***	-.125***
	(.014)	(.014)	(.021)	(.012)	(.013)	(.193)	(.033)	(.0321)
$\text{PostAward}_{i,j,t} \cdot \text{Incumbent}_k$							.096***	.153***
							(.029)	(.029)
Incumbent_k							.584***	.113***
							(.010)	(.016)
Controls								
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	N	Y	N	N	N	N	N	N
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	N	Y	N	N	N	N	N	N
$\text{Post}_{i,j,t} \cdot \text{Incumbent}_k$	N	N	N	N	N	N	Y	Y
Employee $\text{controls}_{k,t=-1}$	N	N	N	N	N	N	N	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Employee_k FE	Y	Y	Y	Y	Y	Y	N	N
Firm_i FE	N	N	N	Y	N	N	Y	Y
N	257000	257000	95000	909000	177000	80000	257000	257000
R^2	.762	.762	.819	.699	.745	.78	.187	.385

Note: This table shows the effect of the grant on employee earnings, using Equation 1. Column 3 restricts the post sample to the two years after the grant application year (this includes the application year). Column 4 identifies the effect off employees who switch jobs by including employee-years after and before an employee worked at the SBIR applicant firm, and including both firm and employee fixed effects. Columns 5 and 6 restrict the sample to incumbent and new employees, respectively. Columns 7 and 8 interact whether the firm wins 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 $\text{controls}_{k,t=-1}$ include tenure, age, high education (BA or above), and log wage in the year before the award year. Data are observed at the employee-year level. Standard errors are clustered by employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CBDRB-FY19-369.

Table 4: Grant Effect on Firm Earnings among Incumbent and New Employees

Dependent variable: Log earnings at the firm's:

	10th pctile		50th pctile		90th pctile		99th pctile	
Sample:	Incumbent	New	Incumbent	New	Incumbent	New	Incumbent	New
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}$	.15**	0.058	.121**	0.0587	.156***	0.0453	.146**	0.0362
	(0.0706)	(0.0755)	(0.0503)	(0.0871)	(0.0559)	(0.103)	(0.0655)	(0.117)
Controls								
$\text{Award}_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	Y	Y	Y	Y	Y	Y
N	8200	3200	8200	3200	8200	3200	8200	3200
R^2	0.17	0.103	0.129	0.14	0.13	0.137	0.183	0.148

Note: This table shows the effect of the grant on earnings percentiles by employee type using Equation 1. Incumbent employees are those who were present at the firm in the year before the grant award year. Control coefficients are not reported to minimize disclosure requirements. Data are observed at the firm-year level. Standard errors are clustered by competition. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CMS request 7276.

Table 5: Grant Effect on Earnings Among Incumbent Employees by Tenure (Employee-Level)

Dependent variable: Log earnings					
Sample:					Hired $\geq$ 3 yrs after firm first observed
	(1)	(2)	(3)	(4)	(5)
$\text{PostAward}_{i,j,t} \cdot \text{Tenure}_{k,t}$	.0119**	.0107**	.0114***	.0565***	.014**
	(.00465)	(.00417)	(.004)	(.016)	(.00625)
$\text{PostAward}_{i,j,t}$	-.0106	-.0276	-.0346	-.103***	-.0365
	(.0285)	(.0264)	(.0256)	(.0373)	(.0353)
$\text{Post}_{i,j,t} \cdot \text{Tenure}_{k,t}$	-.0213***	-.0148***	-.0147***	-.0637***	-.018***
	(.00389)	(.00333)	(.00318)	(.013)	(.00525)
$\text{Tenure}_{k,t}$	.129***	.0747***	.0569***	.208***	.127***
	(.00288)	(.00246)	(.00254)	(.0058)	(.00341)
$\text{Post}_{i,j,t}$	.0931***	.0641***	.0632***	.0969***	.0659***
	(.0193)	(.017)	(.0164)	(.0272)	(.0235)
$\text{PostAward}_{i,j,t} \cdot {\text{Tenure}^2}_{k,t}$				-.0058***	
				(.00143)	
$\text{Post}_{i,j,t} \cdot {\text{Tenure}^2}_{k,t}$				.00666***	
				(.00126)	
${\text{Tenure}^2}_{k,t}$				-.0129***	
				(.000426)	
Controls					
$\text{Age}_{k,t}$	N	Y	Y	Y	N
HighEduc_k	N	Y	Y	Y	N
$\text{WagePctiles}_{k,t=-1}$ FE	N	Y	N	N	N
$\text{Wage}_{k,t=-1}$	N	N	Y	Y	N
Year_t FE	Y	Y	Y	Y	Y
Firm_i FE	Y	Y	Y	Y	Y
N	177000	177000	177000	177000	133000
R^2	.241	.406	.44	.459	.236

Note: This table shows the effect of the grant on employee earnings, using Equation 1. The sample is restricted to incumbent workers who were at the firm before the application year. Column 5 further restricts the sample to include only those hired at least three 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 $Award_{i,j}$ 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 employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CBDRB-FY19-369.

Table 6: Grant Effect on Earnings Among Incumbent Employees by Employee Age, Education, and Preexisting Earnings (Employee-Level)

Dependent variable: Log earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{PostAward}_{i,j,t}$							
$\cdot\text{Age}_{k,t}$	.00278***	.00108					
	(.000869)	(.000761)					
$\cdot\text{HighEduc}_k$			.0715***	.0344**			
			(.0179)	(.015)			
$\cdot\text{Wage} \in 10, 50_{k,t=-1}$					.115**	.0916*	
					(.0573)	(.0553)	
$\cdot\text{Wage} \in 50, 90_{k,t=-1}$					.0935	.0709	
					(.0623)	(.0584)	
$\cdot\text{Wage} \in> 90_{k,t=-1}$					.235***	.178***	
					(.0648)	(.062)	
$\text{Wage}_{k,t=-1}$							.0333*
							(.0176)
Controls							
$\text{PostAward}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y
$\text{Post}_{i,j,t} \cdot X^{\dagger}$	Y	Y	Y	Y	Y	Y	Y
$\text{Tenure}_{k,t}$	N	Y	N	Y	N	Y	N
$\text{Age}_{k,t}$	Y	Y	N	Y	N	Y	N
HighEduc_k	N	Y	Y	Y	N	Y	N
$\text{WagePctiles}_{k,t=-1}$ FE	N	N	N	N	Y	Y	N
$\text{Wage}_{k,t=-1}$	N	Y	N	Y	N	N	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y
Firm_i FE	Y	Y	Y	Y	Y	Y	Y
N	177000	177000	177000	177000	177000	177000	177000
R^2	.222	.439	.213	.439	.357	.406	.439

Note: This table shows the effect of the grant on employee earnings, using Equation 1. The sample is restricted to incumbent workers, those at the firm before the application year. In columns 5-6, the omitted percentile earnings group is Wage $< 10\text{pct}_{k,t=-1}$. Control coefficients are omitted for space considerations, but are available upon request. $^{\dagger}Post_{i,j,t}$ is interacted with characteristic of interest (e.g. $Age_{k,t}$ in column 1). Note that $Award_{i,j}$ 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 employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CBDRB-FY19-369.

Table 7: Grant Effect on Within-Firm Inequality

Dependent variable: Inequality growth

	90/10		99/50	Std Dev
Sample:		within 2 yrs		
	(1)	(2)	(3)	(4)
$\text{PostAward}_{i,j,t}$	.236***	.265***	.0791*	.0727***
	(0.0822)	(0.0866)	(0.0458)	(0.0268)
Controls				
$\text{Post}_{i,j,t}$	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y
Competition_j FE	Y	Y	Y	Y
N	7500	6000	7500	7500
R^2	0.0615	0.0571	0.0703	0.0469

Dependent variable: Inequality levels

	90/10			99/50	Std Dev
Sample:		Incumbent	New		
	(5)	(6)	(7)	(8)	(9)
$\text{PostAward}_{i,j,t}$	.151**	0.00531	-0.0127	.116**	.0556**
	(0.0683)	(0.0769)	(0.115)	(0.0539)	(0.0217)
Controls					
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	Y	Y	Y
N	9600	8200	3200	9600	9600
R^2	0.1	0.174	0.12	0.136	0.0441

Note: This table shows the effect of the grant on inequality measures using Equation 1. Column 2 restricts the post sample to the two years after the grant application year (this includes the application year). Control coefficients are not reported to minimize disclosure requirements. Data are observed at the firm-year level. Standard errors are clustered by competition. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CMS request 7276.

Table 8: Grant Effect on Firm Growth Outcomes

Dependent variable:	Employment growth				Revenue growth			
Sample:				2-year window				2-year window
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}$	.271***	.262***	.27***	.142**	.19***	.183***	.273***	.159**
	(0.0984)	(0.0968)	(0.0795)	(0.0573)	(0.0614)	(0.0613)	(0.0707)	(0.0624)
$\text{Award}_{i,j}$	-0.073	0.0348	0.019					
	(0.0752)	(0.0889)	(0.0333)					
$\text{Post}_{i,j,t}$	-0.0333	-0.0321						
	(0.0374)	(0.0375)						
$\text{Rank}_{i,j}$	0.00352							
	(0.00773)							
$\text{Rank}^2_{i,j}$	0.000107							
	(0.000189)							
$\text{Rank} \mid \text{win}_{i,j}$		-0.0584						
		(0.036)						
$\text{Rank} \mid \text{lose}_{i,j}$		0.000996						
		(0.0024)						
Controls								
$\text{Award}_{i,j}$	-	-	-	Y	Y	Y	N	Y
$\text{Post}_{i,j,t}$	-	-	N	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	-	N	N	Y	Y	N	N	Y
$\text{Rank} \mid \text{win/lose}_{i,j}$	N	-	N	N	N	Y	N	N
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	N	Y	Y	Y	N	Y
$\text{Firm-app}_{i,j}$ FE	N	N	Y	N	N	N	Y	N
N	30500	30500	30500	20000	13000	13000	13000	9500
R^2	0.21	0.21	0.532	0.244	0.143	0.141	0.528	0.426

Note: This table shows the effect of the grant on log growth outcomes, using Equation 1. The base year for the dependent variables is $t = -1$, the year before the application year. Columns 4 and 8 restrict the post sample to the two years after the grant application year (this includes the application year). We show control coefficients only in columns 1-3 to minimize disclosure requirements. Data are observed at the firm-year level. Standard errors are clustered by competition. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CMS request 7276.

Table 9: Relationship between Growth and Earnings Effects

Dependent variable: Earnings growth				
Sample:		within 2 years		
	(1)	(2)	(3)	(4)
Revenue instr w/ $\text{PostAward}_{i,j,t}$	.0813*			
	(0.0473)			
$\text{PostAward}_{i,j,t} \cdot \text{RevGrowth}_{i,t\in 0,2}$		-0.0988		
		(0.0716)		
$\text{PostAward}_{i,j,t} \cdot \text{EmpGrowth}_{i,t\in 0,2}$			-0.0704	
			(.0752)	
$\text{PostAward}_{i,j,t} \cdot \text{PatentCites}_{i,t\in 0,2}$				-.0162***
				(0.00511)
$\text{PostAward}_{i,j,t}$		.118***	.18***	.168***
		(0.0424)	(.0453)	(0.0412)
$\text{RevGrowth}_{i,t\in 0,2}$		.111***		
		(0.019)		
$\text{EmpGrowth}_{i,t\in 0,2}$			-.105***	
			(.022)	
$\text{PatentCites}_{i,t\in 0,2}$				-0.000514
				(0.00161)
Controls				
$\text{Award}_{i,j,t}$	Y	Y	Y	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y
$\text{Award}_{i,j,t} \cdot \text{2yrRevGrowth}_{i,t}$	N	Y	N	N
$\text{Post}_{i,j,t} \cdot \text{2yrRevGrowth}_{i,t}$	N	Y	N	N
$\text{Award}_{i,j,t} \cdot \text{2yrEmpGrowth}_{i,t}$	N	N	Y	N
$\text{Post}_{i,j,t} \cdot \text{2yrEmpGrowth}_{i,t}$	N	N	Y	N
$\text{Award}_{i,j,t} \cdot \text{2yrPatentCites}_{i,t}$	N	N	N	Y
$\text{Post}_{i,j,t} \cdot \text{2yrPatentCites}_{i,t}$	N	N	N	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y
Year_tFE	Y	Y	Y	Y
$\text{Competition}_j\text{FE}$	Y	Y	Y	Y
N	13000	20000	20000	20000
R^2	0.143	0.0802	0.0763	0.0827

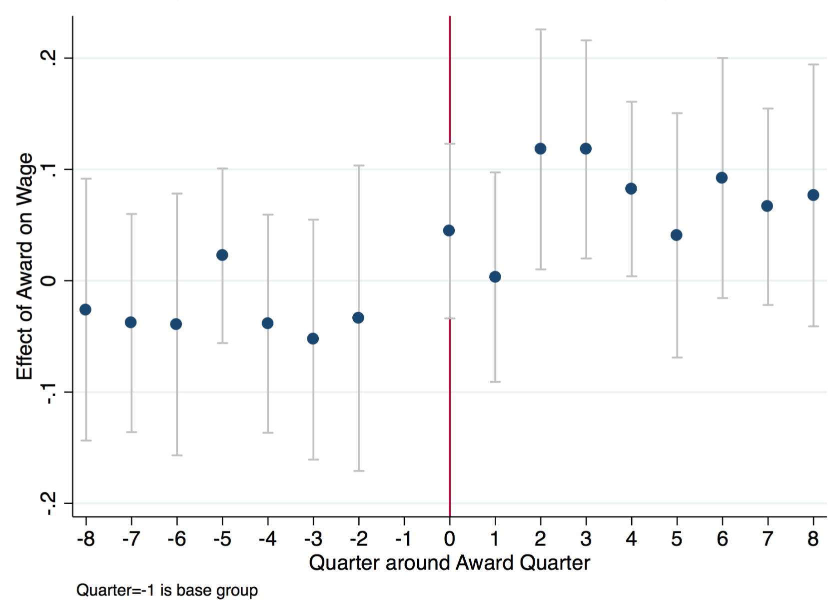
Note: This table shows how effects on earnings growth vary by measures of subsequent growth. Column 1 shows how much of the effect may come through revenue by instrumenting for revenue with the award. Columns 2-4 restrict the post sample to the two years after the grant application year and interact the effect of the grant with three characteristics, using Equation 1: revenue growth, employment growth, and citations to granted patents applied for in the two years following the grant. Data are at the firm-year level. Standard errors are clustered by competition. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosures CMS request 7276 and CBDRB-FY19-452.

Table 10: Grant Effect on Earnings Among Incumbent Employees by Firm Size, Age, Growth

Dependent variable:	Log earnings					Percent raise in first year of job relative to last year of previous job		
Sample:	Incumbents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}\cdot$								
$\text{Large}_{i,t=-1}$	-.178**							
	(.083)							
$\text{Old}_{i,t=-1}$		-.176***						
		(.032)						
$\text{Growth}_{i,t\in-3,-1}$			.104***					
			(.0242)					
$\text{HighPay}_{i,t=-1}$				-.25***				
				(.071)				
Pct Raise_k					-.0257**			
					(.0128)			
$\text{PostAward}_{i,j,t}$	.21**	.177***	.0363***	.279***	.0436***			
	(.0823)	(.028)	(.013)	(.069)	(.0134)			
$\text{Tenure}_{k,t=-1}$						-.025***		
						(.005)		
$\text{Large}_{i,t=-1}$							.062***	
							(.010)	
$\text{Old}_{i,t=-1}$								.023*
								(.012)
Controls								
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	N	N	N
$\text{Post}_{i,j,t}\cdot X^{\dagger}$	Y	Y	Y	Y	Y	N	N	N
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Employee_i FE	Y	Y	Y	Y	Y	N	N	N
N	177000	177000	177000	177000	177000	21500	62000	62000
R^2	.743	.743	.759	.743	.743	.002	.0008	.0007

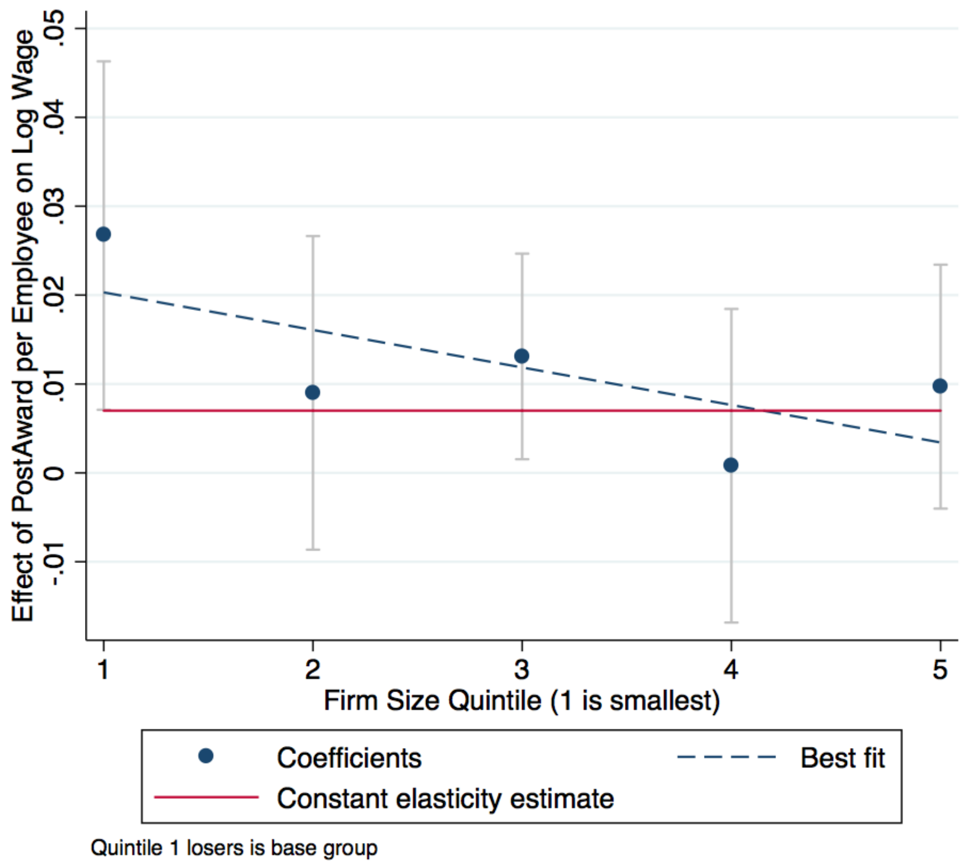
Note: This table shows the effect of the grant on employee earnings, using Equation 1. Large, Old and HighPay are indicators for top quartile employment, age, and average wage in the year before the award. Growth is the revenue growth in the three years before the award. $PctRaise_k$ is employee k's percent raise in the first year of his job at the SBIR applicant firm relative to earnings in the last year of the previous job † Note that $Award_{i,j}$ is defined at the firm level, so is absorbed by firm fixed effects. Data are at the employee-year level in columns 1-5 and at the employee level in columns 6-8. "Year FE" in columns 6-8 control for the year before the award year ($t = -1$). Standard errors are clustered by employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosures CBDRB-FY19-369, CBDRB-FY19-452, and CBDRB-FY2020-CES005-010.

Figure 1: Effects on Quarterly Earnings



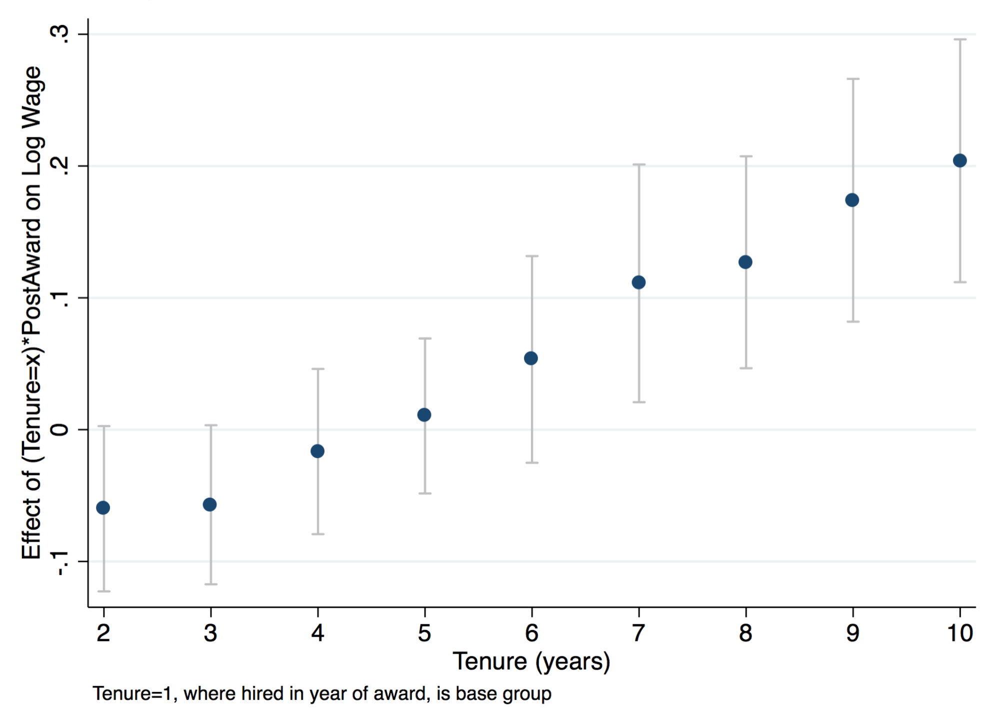
Note: This figure shows the results from estimating Equation 3 on quarterly levels of log firm-year earnings. Each point is a coefficient on a quarter around the award quarter interacted with winning an award. The base quarter is -1 (immediately before the quarter of award). 95% confidence intervals are shown. This figure reports results from disclosure DRB-B0086-CDAR-20180607.

Figure 2: Effect of Award per Employee by Firm Size Bin



Note: This figure shows the effects of winning on log 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 is a coefficient from a regression with separate independent variables for each bin of firm size, using a variant of Equation 1. The coefficient shows 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 is the best-fit line between the coefficients. The red solid horizontal line shows 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. 95% confidence intervals are shown for the quintile coefficients. This figure reports results from disclosure CBDRB-FY2020-CES005-010.

Figure 3: Incumbent Employee-Level Effects by Tenure



Note: This figure shows the effects of winning on log 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 one year of tenure, and more than ten years are excluded (the coefficients are noisier). 95% confidence intervals are shown. This figure reports results from disclosure CBDRB-FY19-369.

Appendix A

(For Online Publication)

Table A.1: Additional Summary Statistics of Firm-Year Data

Probability in industry (most common 3 digit NAICS)	N	Mean	
Administrative and Support Services	30500	0.013	
Chemical Manufacturing	30500	0.0167	
Computer and Electronic Product Manufacturing	30500	0.079	
Electrical Equipment, Appliance, and Component Manufacturing	30500	0.0324	
Fabricated Metal Product Manufacturing	30500	0.0241	
Machinery Manufacturing	30500	0.0495	
Merchant Wholesalers, Durable Goods	30500	0.0257	
Professional, Scientific, and Technical Services	30500	0.622	
Other firm and employee statistics	**N**	**Mean**	**Std Dev**
Firm age in application year	2000	8.309	6.378
Worker age	9600	43.1	8.398
Share employees who are female	9600	0.223	
Share employees who are Asian	9600	0.173	
Share employees who are Black	9600	0.0273	
Share employees who are Hispanic	9600	0.0406	
Share employees who are White	9600	0.737	
Share employees with BA/advanced degree	9600	0.515	
Share employees with some college	9600	0.250	
Share employees with high school degree	9600	0.169	
Share employees with no high school degree	9600	0.0642	
Share employees who are U.S. born	9600	0.714	

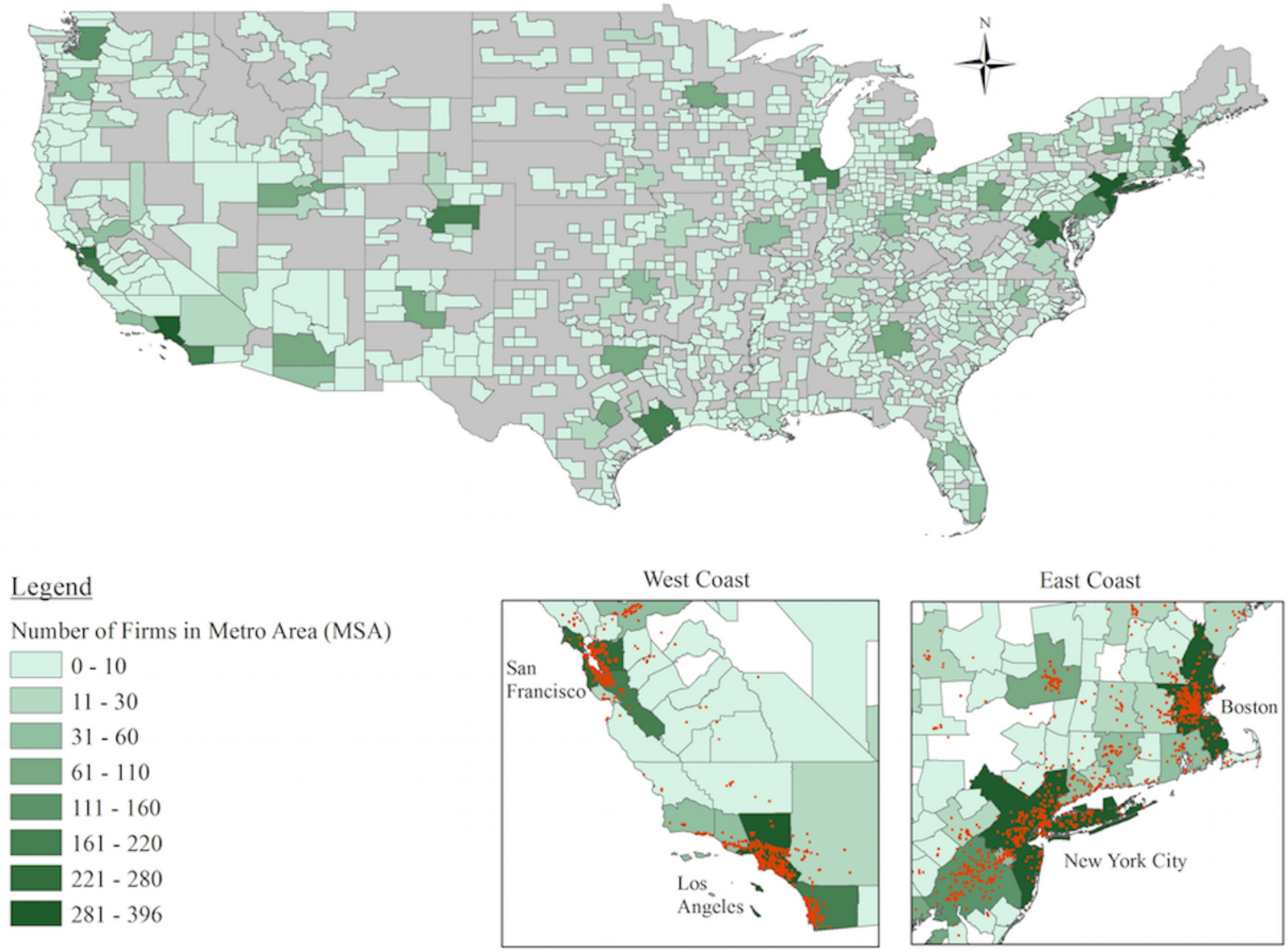
Note: This table shows summary statistics about the SBIR data that were matched to U.S. Census data. The share of firms in the most common eight 3-digit NAICS codes are shown (there are a total of 99 3-digit NAICS). Firms may change NAICS codes across years. Worker-related variables are from linked W-2-Individual Characteristics File data. "White" indicates non-Hispanic White. The number of observations rounded to meet Census disclosure requirements. This table reports results from disclosure CMS request 7276.

Table A.2: Grant Amount Per Employee Effect on Earnings (Firm & Employee-Level)

Dependent variable:	Log earnings				Log earnings growth
Level of analysis:	Employee			Firm	
	(1)	(2)	(3)	(4)	(5)
$\text{PostAwardAmtPerEmp}_{i,j,t}$	.00423***	-.00754**	-.309***	.00953**	.0142***
	(.00151)	(.0034)	(.0249)	(.00418)	(.00518)
$\text{PostAwardAmtPerEmp}_{i,j,t} \cdot \text{Incumbent}_k$		.00703**			
		(.00315)			
$\text{PostAwardAmtPerEmp}_{i,j,t} \cdot \text{Tenure}_k$			.28***		
			(.0221)		
Incumbent_k		.585***			
		(.0102)			
Tenure_k			.123***		
			(.00276)		
$\text{AwardAmtPerEmp}_{i,j,t}$				.00722	.0134***
				(.00489)	(.00474)
Controls					
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	N	N	N	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	N	N	N	Y	Y
$\text{Post}_{i,j,t} \cdot \text{Incumbent}_k$	N	Y	N	N	N
$\text{Post}_{i,j,t} \cdot \text{Tenure}_k$	N	N	Y	N	N
Year_t FE	Y	Y	Y	Y	Y
Competition_j FE	N	N	N	Y	Y
Employee_k FE	Y	N	N	Y	Y
Firm_i FE	N	Y	Y	N	N
N	257000	257000	177000	30500	30500
R^2	.762	.187	.245	.0915	.0955

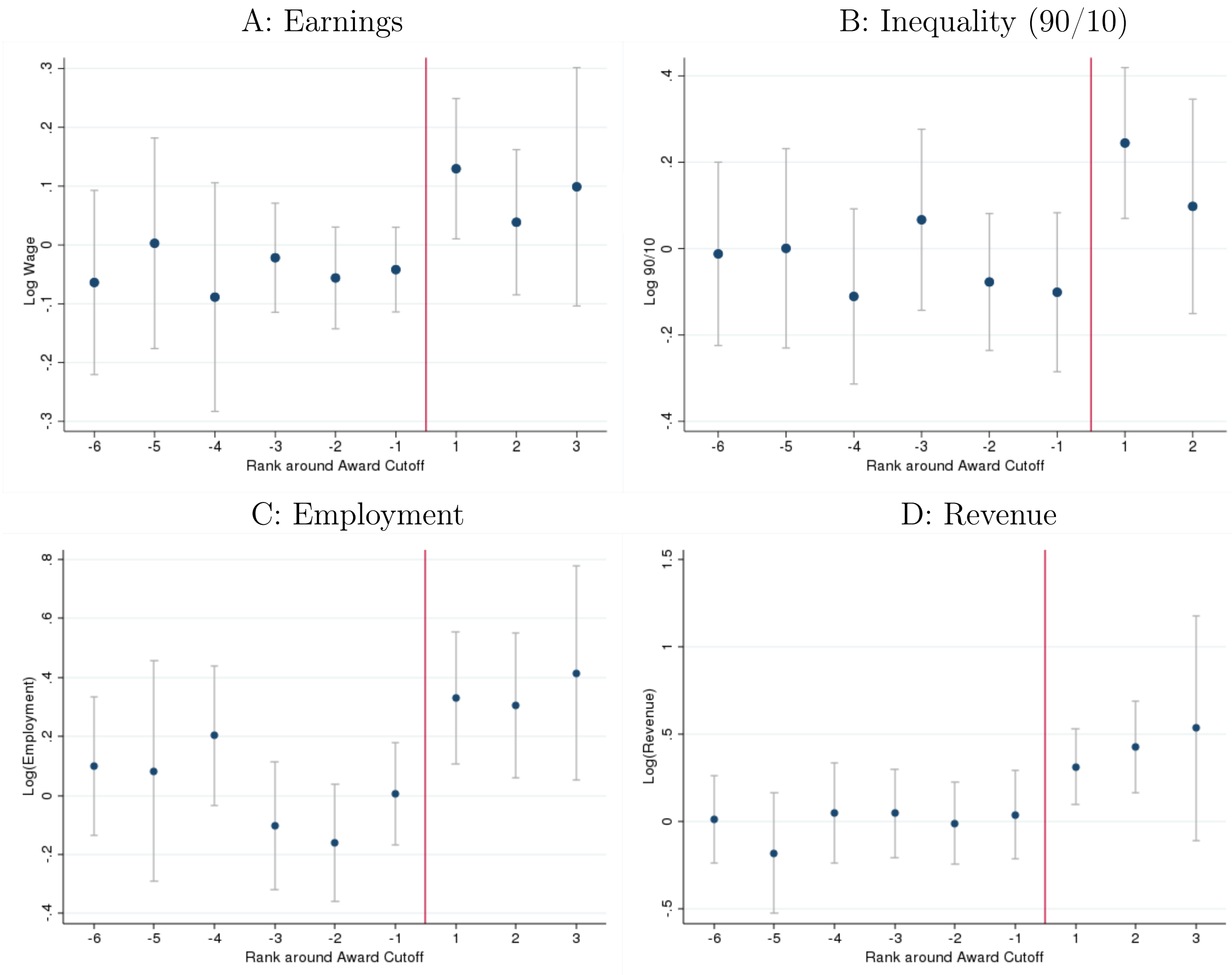
Note: This table shows the effect of the grant amount per employee on earnings, using Equation 1. Columns 1-3 use employee-level data. Column 2 interacts the award amount per employee with an indicator for the employee being an incumbent worker. Column 2 interacts the award amount per employee with the employee's tenure at the firm. Note that $\text{AwardAmtPerEmp}_{i,j,t}$ is defined at the firm level, so is absorbed by either employee or firm fixed effects in columns 1-3. Columns 4-5 use firm-level data, and show the effects on levels and growth of earnings. Standard errors are clustered by employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels. This table reports results from disclosure CBDRB-FY2020-CES005-010.

Figure A.1: Applicant Firm Locations



Note: This figure shows the location of all applicant firms in the data. In the main figure, a darker color for a metropolitan statistical area (MSA) indicates higher firm density. In the insets, actual firm locations are overlaid as orange dots.

Figure A.2: Effects around the award cutoff



Note: These figures show the results from estimating Equation 2 on levels of log firm-year earnings, employment, revenue and the 90/10 inequality measure.. Each point is a coefficient on a specific DOE-assigned rank around the award cutoff, where positive ranks are winning applicant firms, and negative ranks are non-winning applicant firms. 95% confidence intervals are shown. 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 (which exists in competitions with at least three winners). The coefficient magnitude is in line with the previous two. This table reports results from disclosure DRB-B0086-CDAR-20180607.

Figure A.3: Survey email and sample response

Re: Finance Professor Research Question

1 message

Eric Dahnke <[redacted]> Fri, Nov 1, 2019 at 7:23 PM

To: showell@stern.nyu.edu

Hi Sabrina,

Yes. In terms of providing a little more color, I would say a resounding yes. It was quite common during the first 1 to 3 years our company that I would "promise" or otherwise indicate that better pay was just around the corner and that employees would be rewarded with substantial upside, in terms of salary and bonuses, given that they were being paid below market rates, something both they and the company we're aware of. I will also add that we've kept good on those promises and are now paying those same employees above or well above market rates.

I can't imagine any start-up not employing that technique to be honest.

Eric Dahnke
Founder and CEO
solutions.powermarket.io
[redacted]

POWERMARKET

On Oct 31, 2019, at 4:34 PM, showell@stern.nyu.edu wrote:

Eric,

I'm a finance professor researching how high-tech firms pay employees. I'm hoping to get some insights from real-life small business company managers and owners about my theory to see if it may be true in reality. (We economists too rarely ask actual people about their activities.)

So, here is my 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.

Thanks a lot for your time,
Sabrina
P.S. I'm sending this email to a small group of SBIR awardees, which are a good way to identify high-tech small companies.

____

Sabrina T. Howell
Assistant Professor of Finance
NYU Stern School of Business
Phone: 212-998-0913
Email: sabrina.howell@nyu.edu
Website: www.sabrina-howell.com

Note: This figure shows the survey email with an actual sample response, provided with permission from the responder.

Appendix B

(For Online Publication)

This appendix considers six additional theoretical predictions for why a cash flow shock might affect wages, beyond our preferred mechanism in Section 6 of the main text.

B.1 Standard Model

With perfect capital markets, a neoclassical model predicts that the money goes to shareholders because (a) workers are paid their marginal product and (b) the firm is financially unconstrained, so the grant should not affect investment. Like much of the existing empirical literature, the results in the preceding sections are clearly at odds with this model, as it predicts no effect of a cash flow shock on wages. The grant could be paid to shareholders via wages to owner-employees, who likely have the longest tenures. However, in this case we would not expect a linear effect of tenure, as shown in Figure 3. We would expect a convex relationship, but instead we observe a slightly concave relationship. Furthermore, we show in Table 5 column 5 that the effect of tenure interacted with winning persists among incumbents hired at least three years after the firm is first observed, who are not plausibly owners.

B.2 Match quality or search frictions

Several theories seek to explain the strong relationship between wages and tenure. In light of the strong interaction between the grant and tenure, these theories deserve particular attention. In an influential model, Jovanovic (1979) theorizes that the wage may reflect expected productivity, but this is subject to imperfect information about the quality of the match between the firm and the employee. It is possible that after the award, expected productivity or information about match quality changes in such a way as to cause the firm to pay wages that reflect what has been learned over time about the quality of the match. An alternative is that upward-sloping wage-tenure profiles reflect frictions in the search process (Burdett & Coles 2003). Shi (2009) models a scenario in which firms commit to labor contracts but workers, who are risk-averse, cannot commit not to quit. In the model, wages strictly increase with tenure to prevent employees from quitting; the mechanism is that the chances of a better outside offer fall as wages rise. This relationship depends on the worker being risk-averse. Stevens (2004) treats workers as risk neutral, and finds it optimal to backload wages but in a non-linear way.

These models predict a wage-tenure relationship, but no effect of a cash flow shock. Also, in the Jovanovic (1979) model, the employee does not accept payment less than his expected productivity. Therefore, a financially constrained firm would have simply hired fewer workers. Similarly, a cash flow shock effect on wages, whether increasing with tenure or not, is at odds with the predictable increases in tenure to prevent departures modeled by Stevens (2004) and Shi (2009). If the original labor contract indicated that wages would increase with tenure to prevent quitting only in the event of rent increases, that would effectively be the same as employee lending to the firm.

B.3 Efficiency wages

It is possible that the grant enables a formerly constrained firm to pay a wage that exceeds market-clearing level to maximize labor productivity, often called an "efficiency wage." There are four varieties of efficiency wage models. First, efficiency wages may be paid to reduce turnover (Salop 1979, Becker 1964). Second, efficiency wages may deter shirking if there is a cost to losing the job, which would not exist at the market-clearing wage (Shapiro & Stiglitz 1984). Third, higher wages may attract higher quality applicants, which may be valuable to a firm that cannot perfectly observe applicant quality (Weiss 1980). Fourth, efficiency wages may reflect fairness considerations (Solow 1980, Kahneman, Knetsch & Thaler 1986, Fehr & Schmidt 1999).[1] Here, employee effort is a function of the wage relative to the perceived fair wage, which the employee arrives at by comparing pay with coworkers at the same firm (Akerlof & Yellen 1990) or with similar workers at other firms (Summers 1988). An efficiency wage channel predicts a symmetrical effect among new hires. The absence of any benefit among new hires is inconsistent with a purely efficiency wage channel. However, below we will employ the notion from this literature that fairness concerns are important in labor contracts.

[1]This is distinct from shirking models such as Shapiro & Stiglitz (1984) because it does not reflect the costs of losing a job, and because it relies on "gift relationships" as described in Akerlof (1982).

B.4 Incentive contracting

It may be that the result reflects deferred compensation in the form of implicit incentive contracting, which might help to retain employees (Lazear 1981). That is, the labor contract might be designed to maximize effort by rewarding it. This is one explanation for why firms provide broad-based employee stock option grants (Oyer & Schaefer 2005). Relatedly, Becker (1962) theorizes that wage increases with tenure reflect human capital accumulation. In such a case, we expect the firm to reward employees who contributed to the grant or, in the Becker (1962) case, who are more skilled.

A payout that reflected incentive contracting should be proportional to the individual's effort to get the grant. In this case, more direct measures of employee skill (proxies for being the scientists and managers at a small firm who would have applied for the grant) would offer the strongest cross-sectional sources of heterogeneity, or at least would interact significantly with tenure. However, they do not. Instead, heterogeneity is strongest in tenure, and *all* incumbent employees, including low wage workers, benefit. It seems unlikely that low wage workers, such as administrative assistants, would have contributed to receiving an R&D grant.

We also expect that this type of "bonus" should yield only temporary effects on pay, not permanent increases. Conversely, we observe long term increases in the wage for incumbent workers. Finally, there is no reason an incentive contracting mechanism would reflect measures of financial constraints.

B.5 Bargaining Power

Many wage-setting models center around a bargaining parameter that weighs employee productivity and the outside option (Brown & Ashenfelter 1986, Abowd & Lemieux 1993, Stole & Zwiebel 1996, Hall & Milgrom 2008). In the simplest static model similar to what we use to compare our rent-sharing finding to the literature (Equation 5 in the main text), the wage can be written as:

$$w_{i,k} = (1-\gamma)\,o_i + \gamma\theta_{i,k} \tag{1}$$
$$\theta_{i,k} = f\left(\text{productivity}_{i,k}\right)$$

where the outside option is denoted o_i. In general, the rent-sharing literature is interested in modeling how revenue productivity (or total factor productivity) passes through to wages (e.g. Card, Devicienti & Maida 2014 Kline et al. Forthcoming). Employees with low bargaining power (low γ), who also likely have low wages, should have wages that move closely with the outside option. If the employee has high bargaining power, he will be paid all of the surplus that he creates (exactly his productivity).

When a cash windfall occurs, the worker's productivity has not changed, quite unlike the theoretical model in Kline et al. (Forthcoming), where wage effects come from the changes to marginal productivity that happen following a patent grant. Thus, bargaining models predict no effect of the cash windfall on wages. The firm's greater ability to pay does not affect its cost of hiring a replacement worker, and thus does not change a worker's bargaining power. Our results regarding the effect of a cash windfall on wages are very inconsistent with a benchmark, static bargaining model. First, the immediate effects suggest rent-sharing that is not related to productivity. We observe the entire effect on wages within the second quarter after the grant, while the long term effect on revenue is halved when we limit observation to the first two years after the grant. A bargaining model predicts no immediate effect of the cash windfall on wages, but only effects that come through productivity.

A variant model would allow the worker to bargain over expected productivity growth, potentially helping to explain the immediate effect. The grant does appear to enable investment that leads to growth, which might be associated with training or other effects that increase an employee's productivity. In this case, the effect should be proportional to the benefit that the employee will provide. Higher wage and higher education individuals should be the largest future contributors to future productivity growth, and this should not vary across incumbent and new workers. However, we do not find economically meaningful variation in these variables, and new workers get no share of the windfall at all.

Indeed, it is inconsistent with a bargaining model that new hires do not benefit. In a bargaining model, they should benefit just as much as incumbents in an estimate that includes employee fixed effects. Relatedly, we expect both o_i and $\theta_{i,k}$ to be at least as relevant for new hires. Information about the grant is public, so it is not the case that insiders know more about the windfall.

A further variant focuses on replacement costs. The firm may pays their most important workers more to prevent them from leaving, and the longer tenure workers have more firm-specific human capital or are more productive. However, this faces the same challenge described above: after a cash flow shock, the worker's bargaining power based on his own productivity has not changed.

More generally, bargaining power is at odds with the grant having similar effects at all points in the incumbent wage distribution, as the most important employees are likely already relatively high earners. (As mentioned above, there is adequate within-firm wage variation in the years immediately around the award to reasonably expect previous wage heterogeneity tests to be useful.) The effect does not robustly increase with proxies for skill; while it increases with pre-existing wage and education, the effects are economically small.

Relatedly, in a bargaining model, we expect workers with less power to have wages that move more closely with their outside option. To assess whether retention channels are important, we interact the effect of the award with measures of labor market tightness, specifically annual state and industry unemployment. The grant effects do not vary with these measures. This null interaction persists throughout the wage distribution, and it also persists when the sample is restricted to new hires, for whom the outside option should be more immediately available as they are likely more actively searching for a new job.

Finally, the effect is larger among more financially constrained firms, which we would not expect if employee bargaining power primarily explained the positive average effect. This would only apply if the worker had foregone previous wages, which is observationally equivalent to the backloaded wage contract mechanism. That is, if an unconstrained firm receives a cash windfall, a bargaining power story should enable workers at that firm to benefit at least as much as workers at a constrained firm.

B.6 Agency frictions

An alternative to standard economic models is an agency story. Here, the employee may accrue agency power he becomes more entrenched. This is related to the idea that employees become entrenched and thus earn a higher wage over time, particularly in more distressed firms, as in Berk, Stanton & Zechner (2010). Since new employees have not had time to become entrenched and incumbent employees likely accrue more agency power over time as they become more entrenched, the findings that only incumbent employees benefit and their wage gain increases in tenure are consistent with an entrenchment hypothesis. However, one challenge to the agency frictions channel is that the effect persists over time. We would normally expect agency rents to cease when the free cash flow is exhausted.

More importantly, at a fundamental level descriptions based on agency or entrenchment are observationally equivalent to the backloaded wage explanation. To illustrate this, suppose an employee is not paid his reservation wage, and there are two possible explanations: (1) He has implicitly agreed to a backloaded wage and knows that when a cash windfall occurs he will be compensated for foregone wages; (2) He knows that the employer will treat him "fairly" by sharing with him in proportion to his tenure at the firm. In the second model, we must ask why his agency power didn't allow him to previously receive a higher wage. The answer must be that the firm faced financial constraints, and did allow him to extract more agency rents. Therefore, we have the same prediction from a cash flow shock in both models, which is that constrained firms pay out based on tenure at the firm. Importantly, the difference between the two models is the source for the wages implicitly owed to the employee. In a more classical interpretation, it relates to the employee's productivity and outside option. In the agency interpretation, it reflects something like the employee being "friends" with the owner. This is orthogonal to the key components of the model, which are that the constrained firms owes wages to employees and this unvested human capital is increasing in their tenure, leading a cash windfall to be shared proportionally with tenure.

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