

Do Cash Windfalls Affect Wages?

Evidence from R&D Grants to Small Firms

Sabrina T. Howell & J. David Brown*

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Abstract

This paper examines how employee earnings at small firms respond to a cash flow shock, which takes the form of a government R&D grant. Applicant firms are linked to IRS W2 earnings and other U.S. Census Bureau datasets. In a regression discontinuity design based on private ranking data, we find that the grant increases average earnings with a rent-sharing elasticity of 0.09 (0.21) at the employee (firm) level. The effect is only observed for incumbent employees who were present at the firm before the award. Among incumbent employees, the effect is strongly increasing in worker tenure. The grant increases within-firm wage inequality, in part because new hires earn less than the firm average. The grant also leads to higher employment and revenue, but a growth channel cannot fully explain the effect on earnings. The data and a grantee survey are consistent with a backloaded wage contract channel, in which employees of a financially constrained firm initially accept low wages and are paid more when cash is available. JEL G32, G35, J31, J41

*NYU Stern & NBER, U.S. Census Bureau.

Emails: Sabrina.howell@nyu.edu, J.David.Brown@census.gov

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1 Introduction

How do small, high-tech, financially constrained firms set wages? These firms are crucial to economic growth, and more generally small businesses encompass about half of U.S. jobs, so it is important to understand how they grow and the role of wage contracts in that growth.¹ This paper evaluates the effects of a government R&D grant program on employee earnings using a regression discontinuity design that compares grant awardees with unsuccessful applicants. The grant can be considered a cash flow shock because there are no restrictions on how it is spent. We show positive effects of the cash flow shock on earnings, and also on within-firm earnings inequality and firm growth. 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).

To assess the pass-through of productivity shocks to wages, the literature on rent sharing has used proxies for productivity-induced surplus such as value-added, profits, sales, and patent grants.² Two challenges have been that it is difficult to find exogenous sources of productivity variation and even exogenous productivity shifts may be 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 use applications for U.S. Department of Energy (DOE) Small Business Innovation Research (SBIR) grants. Private ranking data spanning 1995 to 2013 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 applications, nor are their expenditures monitored ex-post. We link applicants to U.S. Census Bureau data on the firms and their employees, including the employee-level IRS W-2 earnings data.³ A benefit of these data is that they provide a well-

¹See Decker, Haltiwanger, Jarmin & Miranda (2014) and <https://www.sba.gov/sites/default/files/advocacy/2018-Small-Business-Profiles-US.pdf>.

²In addition to work cited below, this literature includes Abowd & Lemieux (1993), Blanchflower et al. (1996), Card, Devicienti & Maida (2014), Card et al. (2016), Carlsson et al. (2016), Mogstad et al. (2017), Goldschmidt & Schmieder (2017), and Helpman et al. (2017).

³As these firms appear to primarily employ full-time workers, we follow convention in the literature and sometimes term annual W-2 earnings “wages.” 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 –

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.

In firm-level regressions, we find that receiving a grant leads to a nine percent increase in earnings.⁴ The positive impact of the grant begins in the quarter immediately following the grant award and endures with statistical significance for at least five years. For the average firm, increased wages account for the entire grant amount between years two and three after the grant. At the employee level with employee fixed effects, we find effects of three to four percent. Smaller employee-level effects reflect large firms being more heavily weighted and experiencing smaller effects. The implied rent-sharing elasticity from the firm-level estimate is 0.21, which is slightly smaller than the seminal estimate in Van Reenen (1996), but larger than some more recent estimates. At the employee level it is .09. The effect does not appear to reflect more hours worked.

The positive effect on wages exists only among employees hired before the application year. These incumbent employees receive a 16 percent increase, which is consistent across the wage distribution. There are no effects for new hires (hired in or after the year of the award). The difference between incumbent employees and new hires is statistically significant. Within incumbent employees, by far the largest and most robust source of heterogeneity is tenure, 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, Schoefer, Young & Zweimüller (2018) in suggesting that outside options are not especially important sources of wage variation.

The grant also increases growth, measured using employment and revenue. While growth appears to explain the persistence of the positive effect on wages over time, several tests show that a firm growth channel cannot fully explain the immediate effects on incumbent earnings. For example, the entire earnings effects are observable within two quarters, while only half the long term revenue effect exists within the first two years. A revenue decomposition, in

even high-tech, young ones – do not grant stock to non-owner employees.)

⁴We use an indicator for award rather than the amount per employee to avoid endogeneity issues, as employment is affected by the grant. The results are robust, however, to this alternative approach.

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 about 17 percent of the total effect on earnings can be explained through a revenue channel. We find no effects on firm or employee exits.

Heterogeneity in the earnings effects leads to higher within-firm wage inequality. This reflects the combination of lower average wages among new hires across all firms, no grant effect for new hires, and higher grant effects on levels of wages 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 (Edmans & Gabaix 2016, Mueller, Ouimet & Simintzi 2017, Song et al. 2018).

We consider seven channels besides growth 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. The positive effect of the grant on growth implies that the grant is partially used for investment, and thus that recipients were financially constrained. Consistent with this, the grant has larger effects among smaller and younger firms at both the firm and employee level. A smaller effect among the larger firms is to be expected mechanically, as the grant is the same size for all firms. The effect is about two-thirds larger for firms with below-median employment in the year before the grant. 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 his outside option in exchange for a higher wage later. This gives rise to backloaded wage contracts, in which the wage increases 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. Several further predictions of this model are satisfied: The effect is larger among firms that (a) we expect to be more constrained, (b) initially paid below-market wages, and (c) grew faster before the grant application. Also, if incumbent workers accept a backloaded contract, their initial earnings should reflect a “constrained employer” penalty. Indeed, the percent raise between an employee’s previous job and his first year is decreasing in worker tenure.

To directly assess whether firms in the data are using backloaded wage contracts as a result of financial constraints, we conducted an email survey of DOE SBIR grantee principal investigators, who are almost always company CEOs. The survey asked directly about use of backloaded wages contracts as a result of the firm having been financially constrained. We received 97 responses, representing a response rate of 19.5 percent. The results offer strong support for the mechanism, with 55.7 percent of respondents replying yes, 21.6 percent no, and 22.7 percent not explicitly answering the question.

We do not find that awards lead to flatter wage-tenure profiles. This suggests that the firm may engage in similarly backloaded contracts with new hires if it remains constrained. 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.

A back-of-the-envelope calculation suggests that after a grant, incumbent workers earn a premium for having accepted the backloaded contract. This suggests that rather than characterizing the backloaded contract in our setting as implicit debt, as the existing theoretical literature has done, an equity lens may be more appropriate. We would not expect a debt contract to yield persistent effects or gains from the grant in excess of the previously foregone wage, funded in part through future growth. Instead, this evidence is more consistent with incumbent employees having rights to future cash flows. That is, the firm may implicitly raise equity through its wage contracts.

The evidence is much less consistent with six other mechanisms: dividends through wages, match quality models, efficiency wages, bargaining power, incentive contracting, and agency frictions. The latter three are the most plausible. The main argument against a bargaining model is that the cash windfall does not affect the worker’s productivity (note that essentially all rent-sharing models focus on the share a worker gets of TFP or revenue 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 or firm financial constraints.

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 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.

This paper is related to the literature on innovation and wages, which 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). Three recent papers are especially relevant. First, Kline et al. (Forthcoming) regress firm outcomes on an indicator for obtaining a high-value, initially allowed patent, relative to a low-value and initially rejected patent. Their approach differs in several ways from ours, including the empirical design, source of variation, and mechanism. Some of our conclusions are similar to theirs; for example, they find that patent-instrumented surplus leads to higher wages among incumbent top earners but not among new hires. Second, Aghion et al. (2018) find that after Finnish firms patent, all employees benefit, with the owners and inventors benefiting the most. Third, Kogan et al. (2019) study how wages and employee mobility change after public firms receive a valuable patent. They find that patents are associated with wage increases among high earners. They also show that competitor innovation is associated with more exits from employment. This paper's main contribution relative to existing literature is to assess the effect of a one-time cash flow shock rather than a patent, which represents a productivity shock, or the expectation of a future stream of cash flows from monopolization of an innovation.

This paper most directly 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, Laun, Meghir & Pistaferri (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 geographically-based variation, or they take a structural approach. In contrast, we study a pure firm-level cash flow shock. We also 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.⁵

2 Empirical Setting

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, law 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. The application process for both phases is onerous, taking a full-time employee one to two months.⁶

⁵Using 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).

⁶Applicants 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

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) demonstrate progress on the Phase 1 project; and (ii) not be more than 50 percent owned by outside private equity investors. 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. 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.⁷

By virtue of their status as applicants to DOE’s SBIR program, at the time they apply the firms in the sample are engaged in some sort of innovation activity related to energy, and they must be relatively small (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

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 grantees confirmed the 1–2 month time-frame).

⁷The 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.

2012 U.S. dollars) in SBIR grants between 1983 and 2013. In the data used in this paper, there are about 270 competitions. 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 beyond those provided here, see Howell (2017).

The application data were 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 were matched successfully. We erred 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 was no clear bias in matching, and match rates are similar by rank around the cutoff.

Once a link to a Business Register record was established, we were able to link the firm 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. Also, 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.⁸

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 in the pay period that includes March 12 until 2005, when we observe employment for all four quarters of each year. The second is payroll, which is observed quarterly throughout. The third is revenue, which is observed annually starting in 1996. 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

⁸For example, Robb & Robinson (2014) show that just 4 percent of young firms receive outside equity in a large, representative survey of U.S. firms started in 2004 that over-samples high-tech firms. In fact, Coleman & Robb (2011) use the same survey to show that high tech firms have lower rates of outside equity than low tech firms.

samples. A disadvantage of our data is we lack information about occupation. In its stead, 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. 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 39.⁹ 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 is 8.3 years (with a standard deviation of 6.4).

In some models the outcome variables are 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 true for two reasons. First, 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. Second, there are more observations in this pre-application period.

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), Mueller et al. (2017), and Abowd et al. (2018). We also examine the 99/50 ratio as a proxy for upper-tail inequality. Finally, we use the standard deviation of wages. The 90/10 ratio is preferred to the standard deviation in part because we expect the latter to mechanically increase if all employees' wages

⁹<https://www.census.gov/data/tables/2012/econ/susb/2012-susb-annual.html>

increase by the same percent. (In unreported results, we found generally similar effects using the interquartile range.) 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 received a smaller average wage increase relative to their previous job. 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.¹⁰ The next most 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

¹⁰Industry is a firm-year variable because industry assignments may change over time within a firm. Industry is based on six-digit NAICS codes. Where a firm has multiple units, and therefore potentially multiple industries, we use the NAICS associated with the firm’s largest employment share.

estimates a local average treatment effect around a cutoff in a rating variable. A valid RD design requires that treatment not cause rank. This 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 1.¹¹ Here and below, i denotes a firm, k denotes an employee, j denotes a competition, and t denotes a year.

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

Before describing each variable in Equation 1, note that 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) prevents affirmatively establishing 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.¹² We can use fine controls and growth specifications, lending additional validity to the empirical design. The panel setting also follows related

¹¹Our 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.

¹²Also note that a challenge of patent outcomes with change models is that they may confound zero patenting with a zero difference between patents before and after the application.

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. We do not use award per employee as this might generate concerns about endogeneity, as employment is also an outcome variable. As mentioned below, the results are robust to this alternative independent variable.

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). 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.¹³

The dependent variable in Equation 1 is either a levels measure, such as the average wage in 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 exists for rejected applicants as well, representing the year they applied and failed to win a grant. Note that it is necessary to use levels outcomes when we compare effects on new and incumbent employees, as “change” is undefined within the firm for new employees. 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.

¹³In 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.

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. We also present two other specifications. One controls for rank separately among winners and non-winners. A second includes 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. The third type of model is at the employee level and includes employee fixed effects (λ_k). Errors are clustered by competition, though the main effects are robust to a variety of error assumptions.

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

$$Y_{i,t} = \sum_{x=-6}^{x=3} \beta_x (PostAward_{i,j}) (Rank_{i,j} = x) + \eta_1 Age_i + \eta_2 Age_i^2 + \tau_t + \lambda_j + \varepsilon_{i,j,t} \quad (2)$$

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.

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

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 2 and 3, standard errors are clustered by competition.

4 Grant effect on earnings

4.1 Average earnings

Table 2 shows the grant effect on earnings growth 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 coefficients on quadratic rank are included in column 1 and on either side of the cutoff in column 2. Firm-application fixed effects are included in column 3, which absorb controls for rank and competition. The coefficient on $PostAward_{i,j,t}$ in the most stringent model indicates that a grant award increases earnings growth (the ratio of earnings in the current year to the base year) by about nine percent (column 3).¹⁴ Roughly the same nine percent effect is found when the dependent variable is levels of earnings in column 5. Figure A.1 A shows the effect on levels of log earnings by rank around the cutoff, using Equation 2.

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 employee is observed only at the applicant firm. The main estimates in columns 1-3 find effects of three to four percent. 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 substantially larger among smaller firms. The effect is somewhat larger within two years (Column 3). Previous estimates with individual data are similar, at 0.01-0.06 (Margolis & Salvanes 2001, Arai 2003, Martins 2009, and Gürtzgen 2009). 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 effects described thus far are average effects measured across all years in which we observe the firm or employee. We are also interested in the dynamics over time. The effect occurs quickly, with almost the entire effect observed within a two year window of the application year (Table 2 column 4). Figure 2 A demonstrates the effect on levels of log

¹⁴The 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).

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. The effect is also persistent over time at the employee level (not reported). The implications of persistence and the possibility that firm growth may fund higher wages and wages for new employees in the longer term are discussed further below.

We can use the results to roughly calculate how much of the grant on average is accounted for with earnings growth. The most straightforward calculation uses the employee-level results for incumbent workers (Table 3 column 5). (Section 4.2 will discuss in detail incumbent relative to new hire effects.) The main model estimate of 3.8 percent implies that the firm’s wage bill increases by about \$16,400 per year, such that the grant would take about 9 years to be exhausted if paid out only in wages.¹⁵ If we alternatively use the higher average firm-level estimates of about 9 percent from Table 2, the grant amount can be accounted for in wages by the fourth year. The differences across levels of analysis are explained by smaller firms being weighed more heavily in the firm-level analysis.

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). 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:

$$w = o + \gamma \frac{G}{N}. \quad (4)$$

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

$$\xi = \frac{\gamma \frac{G}{N}}{o + \gamma \frac{G}{N}}. \quad (5)$$

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).¹⁶ The parallel in our context is a calculation of the wage elasticity to the grant in the year following the

¹⁵The inputs to this calculation are a 3.8 percent effect for the on average 6.8 employees earning \$64,150 per year.

¹⁶The 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 $\frac{\frac{G}{N}}{w}$.

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 2 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 (9/43). In turn, we can use Equation 5 to approximate a rent-sharing parameter γ of 0.56.¹⁷ At the employee level, the coefficient of a four percent increase yields a rent-sharing elasticity of 0.09.

An elasticity of 0.21 is similar to 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 revenue per worker are smaller and closer to our employee-level estimate, including Barth et al. (2016), Carlsson et al. (2016), and Bagger et al. (2014).

The grant is a one-time, transitory cash flow shock. It is transmitted to wages quickly, yet also endures over time. One reason that the effect may endure is that the grant may lead to innovation, higher revenue, and long-term increases in rents, which is considered in Section 5. The literature has generally found larger rent-sharing effects when firm value added or profits are instrumented with a variable correlated with systematic or permanent changes in rents (in addition to works cited above, this includes Abowd & Lemieux 1993, Guiso et al. 2005, and Arai & Heyman 2009). Cardoso & Portela (2009) and Guiso, Pistaferri & Schivardi (2005) find zero elasticities to transitory changes in value added or sales, which contrast to positive elasticities for permanent shocks. 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.

The effects are robust to a number of unreported approaches. First, they are similar

¹⁷To 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.

with state fixed effects and location in a major MSA.¹⁸ Second, they are similar with a bandwidth of one firm around the cutoff. Third, when we split the sample by time period, for example around 2005 and 2008, we find similar effects on either side. The magnitude of the effect is somewhat larger in the early period, 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 coefficient is similar and of equal significance.¹⁹ Fifth, the result is very similar when multiple-time grant winners are excluded from the sample; that is, the result does not reflect future grants. Finally, using the award per employee rather than the award indicator, we continue to find robust positive results for all our main findings, including heterogeneity in worker tenure (discussed below). The effect is smaller in magnitude as the average effect is larger for smaller firms.

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 the higher earnings reflect 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 6 and 7 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, at the employee level an interaction between $\text{PostAward}_{i,j,t}$ and being an incumbent employee is .096 and highly significant, using firm fixed effects (Table 3 column 7). 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

¹⁸This is an indicator 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.

¹⁹This creates especially small implicit samples with other samples, and so definitely cannot be reported.

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

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. This result is consistent with Kline et al. (Forthcoming), who find that patent grants do not lead to higher earnings for new employees.

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 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 as at 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 the SBIR applicant firm. These facts suggest 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 among incumbent employees.²¹ By

²¹For 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.

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 (note mean tenure is 3.8 years). To assess whether the effect 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.

The effect is markedly linear in tenure. Figure 3 shows coefficients from a regression with separate dummies for years of tenure interacted with winning, among incumbent employees. The omitted group is those with one year of tenure, and more than ten years are excluded (the coefficients are noisier). The result indicates that starting with five years of tenure, there is a positive effect that increases linearly through ten years. 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 linear relationship. The coefficient on $\text{PostAward}_{i,j,t} \cdot \text{Tenure}_{k,t}$ increases, while the coefficient on $\text{PostAward}_{i,j,t} \cdot \text{Tenure}_{k,t}^2$ is negative and significant, albeit economically small. Therefore, the effect of the award is somewhat concave in tenure.

The tenure effect does not appear to reflect firm owners. Column 5 shows the main effect of tenure among incumbent employees hired at least three years after the first year the firm is observed, who are not plausibly owners. It finds a similar result 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 within 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. In Table 7 within-firm inequality growth measures are used in columns 1-4, and levels in columns 5-7 (we find similar effects in levels using the sample for which we observe growth, available upon request). We find large and robust positive effects on the three inequality measures. A grant increases the growth of the 90/10 ratio by 24 percent (Table 7 column 1), and the effect is in fact slightly larger when only the first two years after the application are included (column 2).²² Note that when there are fewer than 10 employees, the algorithm assigns the 90th and 10th percentiles to the extreme observations. Despite having only about seven employees on average in the year before the award, the within-firm standard deviation of wages is substantial.

The large effect on inequality is driven by effects at the top of the distribution. The effect on upper-tail inequality growth (the 99/50 ratio), shown in column 3, is smaller, at about eight percentage points. Regression estimates of the effects of the grant on earnings percentiles are in Table A.2. Columns 1-4 use growth outcomes, and columns 5-8 use level outcomes.²³ We see the same patterns for both; at the bottom of the wage distribution, there is no effect (columns 1 and 5). At the median, there are positive but insignificant coefficients. At the 90th and 99th percentiles, there are large and robust effects.

The inequality effects contrast with the positive effect within 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

²²Figure A.1 Panel B demonstrates the effect on the 90/10 ratio by rank around the cutoff. We only report two positive ranks for inequality, because the smaller sample led to a very large confidence interval for the firm three ranks away from the cutoff. We cannot create the quarterly figure as the W2 data used to construct inequality measures are annual.

²³Note that the sample size is larger with levels, because we do not observe all firm-years both for the pre-application year and every subsequent year, as the W2 data begin in 2005. In unreported results, we find similar effects on levels in the sample for which we observe growth.

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.

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. While hiring new, more able workers increases average productivity in their data, this selection mechanism does not have any effect on wage dispersion. Our results highlight how a windfall is different from making incentives more high-powered. 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 (see Goldschmidt & Schmieder 2017 on this last point). 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.

5 Effects on Firm Growth

If the grant causes growth, this could in turn affect wages, for example through higher labor productivity if the worker has bargaining power. We cannot observe profits or productivity, but we can observe revenue and total employment. The effects of the grant award on firm growth are presented in Table 8.

The effect of winning a grant on log employment relative to the base year is shown as 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 most of the controls used elsewhere. 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.²⁴ 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. The effect on employment, like that on earnings, occurs quickly. Figure 2 Panel B demonstrates the effect on levels of log employment by quarter around the award quarter. Figure A.1 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 5-7). Again, just over half the effect on revenue occurs within two years of the grant application.²⁵ Figure A.1 Panel D demonstrates the effect on levels of log revenue by rank around the cutoff. We also examined firm exit in the forms of acquisition and death, but found no measurable effects on these outcomes.²⁶

To explore whether the effect on earnings is primarily a function of increased revenue or profitability, we conduct two tests. The first decomposes the effect 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 that is 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. Since both revenue growth and wage growth are logged, 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 17 percent of the main effect of the grant on wages (from Table 2 column 5). 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 through a revenue channel.

The second test shows that the immediate effect of winning is not higher among firms with higher growth or innovation after the grant. For this, we restrict the sample to the

²⁴The 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).

²⁵ There is no quarterly graph because Census does not have quarterly revenue data.

²⁶We 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.

two years after the grant application year. First, we interact winning with revenue growth (Table 9 column 2), and find that the effect is not statistically significantly larger when revenue growth is higher than average in the first two years after the grant. The same is true for employment growth (column 3). Last, 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.

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 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.

In 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 and 6.3. Section 6.5 discusses how the implicit contract may be enforced.

Section 6.5 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); 7) 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.²⁷ 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 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 loans to 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.²⁸

²⁷A key assumption in their work is that better workers do not sort differently across provinces.

²⁸If 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.

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. We and Howell (2017) show that the grant causes growth and innovation investment, indicating that firms were constrained. These effects almost certainly reflect the sample: 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.

More concretely, it is useful to compare our firms with publicly traded ones. There is evidence that public firms spend tax holiday-induced cash windfalls from repatriation primarily on dividends, not wages (Dharmapala, Foley & Forbes 2011).²⁹ 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 overseas cash holdings likely have good access to capital markets, while the small, young, private firms in my data are likely much more constrained. 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. Indeed, 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.³⁰ 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.

²⁹Relatedly, 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.

³⁰ We use indicator variables here because at the employee level, these variables are quite skewed. These relationships persist at the firm level.

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 observe profits).³¹

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. As mentioned above, the effect cannot be fully explained 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. Consistent with backloaded wage contracts, 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 (Table 10 column 5). Our data

³¹We 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.

provide further interesting descriptive facts. First, 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 statistically significantly larger for new employees.³² 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 6-7).³³ The award does not affect the percent raise for new hires, consistent with there being no different composition of new hires across firm types.

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.³⁴ 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 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 (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.³⁵ While the exact number is of course sensitive to assumptions, it is clear that

³²Note 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/>

³³Note 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.

³⁴Using 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.

³⁵The 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)

an incumbent worker with long tenure who is at a firm that wins the grant is handsomely compensated. This suggests that rather than characterizing the backloaded contract in our setting as implicit debt, as the existing theoretical literature has done, an equity lens may be more appropriate. We would not expect a debt contract to yield persistent effects or gains from the grant in excess of the previously foregone wage, funded in part through future growth. Instead, this evidence is more consistent with incumbent employees having rights to future cash flows. That is, the firm may implicitly raise equity through its wage contracts.

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 backloaded wage contracts as a result of financial constraints. To assess whether this mechanism passes a “smell test” among the firms, we conducted an email survey of DOE SBIR grantee principal investigators, who are almost always company CEOs.³⁶ 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.”

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 in the firm of \$63,500, this is \$5,715. This implies that the pay bump is more than twice the penalty at hiring, suggestive of a substantial premium. Making the extremely conservative assumption that the employee would have invested this income at 5 percent (a commonly assumed long term equity risk premium), and reinvesting the income on it, the foregone earnings total \$17,776. It therefore takes between two and three years after the grant to fully make up for this lost income.

³⁶The survey was conducted using emails sent by 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. As the survey targeted firms, it does not require IRB approval.

We sent the same email to 585 individuals for whom we were able to find email addresses.³⁷ Among these, 88 addresses bounced. We received 97 responses, representing a response rate of 19.5 percent. The full text of the email is shown in Figure 4, which also includes an actual response (with permission from the responder).³⁸ Across the 97 respondents, 55.7 percent replied yes, 21.6 percent no, and 22.7 percent did not explicitly answer the question. The sample response in Figure 4 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:

“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.

³⁷We started with the sample of all Phase 1 grantees. The emails were sent October 31 and November 1 2019. All tabulated responses were collected by November 6.

³⁸The SBIR grant for this responder is publicly filed under the firm name “ProjectEconomics,” available at <https://www.sbir.gov/sbirsearch/detail/880883>.

In sum, 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. 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.

6.5 Enforcement mechanism

Why doesn't the firm renege on a backloaded wage contract? 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 NPV 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 contract. 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.³⁹ If the labor market is not very competitive, 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 through a fairness or reputation channel. Sharing rents in a manner deemed fair by employees could benefit the firm in the long run (Lazear 1989). 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 &

³⁹The authors thank Holger Mueller for this suggestion.

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 receiving 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

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 following 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. Note that in this example, administrative assistants may have helped with the paperwork. However, the difference in quality of the administrative assistants between winners and losers is likely to be less than the difference in quality of the principal investigators who wrote the applications. Finally, there is no reason an incentive contracting mechanism would reflect measures of financial constraints.

Finally, a third alternative is that employees accrue agency power and become more entrenched over time (Berk et al. 2010). 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, 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.

As mentioned above in the context of bargaining, 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 outside option or, in a bargaining model, his productivity. In the agency interpretation, the source is something like the employee being "friends" with the owner. The source is orthogonal to the key components of the backloaded wage mechanism, which is 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. The setting is government R&D grants to small, likely financially constrained firms. In addition to being economically important yet relatively understudied, 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)

Levels statistics

	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		

Log growth statistics (base is $t = -1$)

	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 in Panel B 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.

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		P-value for diff of means
	N	Mean	N	Mean	
Employee-level within 2 yrs of award yr					
HighEduc _k (BA or above)	49500	0.45	11500	0.358	0.00
Age _{k,t} (years)	49500	43.11	11500	36.99	0.00
Earnings _{k,t} ('000 2010 \$)	49500	68.98	11500	39.70	0.00
Percent raise _{k,t} ('000 2010 \$)	49500	0.224	49500	0.243	0.09
Firm-level, all years					
10th pctile earnings _{i,t} ('000 2010 \$)	8200	19.34	3200	12.46	0.00
50th pctile earnings _{i,t} ('000 2010 \$)	8200	40.54	3200	22.93	0.00
90th pctile earnings _{i,t} ('000 2010 \$)	8200	76.88	3200	44.80	0.00
99th pctile earnings _{i,t} ('000 2010 \$)	8200	94.85	3200	50.51	0.00

Note: This panel contains summary statistics, all of which refer to SBIR applicant firms unless otherwise specified. 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.

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

Dependent variable:	Log earnings growth				Log earnings levels			Log payroll
					2 year window	Incumbent	New	Within 2 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostAward $_{i,j,t}$.134*** (0.048)	.133*** (0.0481)	.0946** (0.0391)	.126*** (0.0406)	.0931** (.0387)	.137*** (0.0482)	0.0607 (0.0753)	.148* (.0776)
<u>Controls</u>								
Award $_{i,j}$	Y	Y	N	Y	Y	Y	Y	Y
Post $_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Rank $_{i,j}$, Rank $^2_{i,j}$	Y	N	N	Y	Y	Y	Y	Y
Rank win/lose $_{i,j}$	N	Y	N	N	N	N	N	N
Age $_{i,t}$, 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	Y	Y
Firm-app $_{i,j}$ FE	N	N	Y	N	N	N	N	N
N	30500	30500	30500	20000	30500	8200	3200	20000
R 2	0.0988	0.099	0.449	0.0738	0.0924	0.142	0.135	0.276

Note: This panel shows the effect of the grant on earnings growth, using Equation 1. The base year is $t = -1$, the year before the application year. Columns 1-3 replicate the three main specifications from Table 8, with rank controlled for quadratically, on either side of the cutoff, or through firm-application fixed effects (Firm-app $_{i,j}$ FE, which also absorb award and competition). Column 6 restricts the sample to the two years after the grant application year (including the application year). Control coefficients are not reported to minimize disclosure requirements. Data are observed at the firm-year level. Except in column 6, wage is the computed as the average wage within the firm-year. Standard errors are clustered by competition. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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

Dependent variable: Log earnings								
	2 year window			Incumbent Employees	New Employees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostAward _{<i>i,j,t</i>}	.032**	.029**	.042**	.076***	.038***	-.044	-.126***	-.125***
	(.014)	(.014)	(.021)	(.012)	(.013)	(.193)	(.033)	(.0321)
PostAward _{<i>i,j,t</i>} · Incumbent _{<i>k</i>}							.096***	.153***
							(.029)	(.029)
Incumbent _{<i>k</i>}							.584***	.113***
							(.010)	(.016)
<u>Controls</u>								
Post _{<i>i,j,t</i>}	Y	Y	Y	Y	Y	Y	Y	Y
Rank _{<i>i,j</i>} , Rank _{<i>i,j</i>} ²	N	Y	N	N	N	N	N	N
Age _{<i>i,t</i>} , Age _{<i>i,t</i>} ²	N	Y	N	N	N	N	N	N
Post _{<i>i,j,t</i>} · Incumbent _{<i>k</i>}	N	N	N	N	N	N	Y	Y
Employee controls _{<i>k,t=-1</i>}	N	N	N	N	N	N	N	Y
Year _{<i>t</i>} FE	Y	Y	Y	Y	Y	Y	Y	Y
Employee _{<i>k</i>} FE	Y	Y	Y	Y	Y	Y	N	N
Firm _{<i>i</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 panel shows the effect of the grant on employee log earnings, using Equation 1. Column 3 restricts the sample to two years on either side of application year. Column 4 uses switchers to identify the effect 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. Control coefficients are not reported to minimize disclosure requirements. Employee controls_{*k,t=-1*} include tenure, age, high education (BA or above), and log wage in the year before the award year. Note that *Award_{*i,j*}* is defined at the firm level, so is absorbed by either employee or 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.

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	
Employee type:	Incumbent	New	Incumbent	New	Incumbent	New	Incumbent	New	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
PostAward $_{i,j,t}$.15** (0.0706)	0.058 (0.0755)	.121** (0.0503)	0.0587 (0.0871)	.156*** (0.0559)	0.0453 (0.103)	.146** (0.0655)	0.0362 (0.117)	
<u>Controls</u>									
Award $_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y	
Post $_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y	
Rank $_{i,j}$, Rank $^2_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y	
Age $_{i,t}$, 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.

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

Dependent variable: Log earnings		Hired \geq 3 yrs after firm first observed				
	(1)	(2)	(3)	(4)	(5)	
PostAward $_{i,j,t}$ · Tenure $_{k,t}$.0119** (.00465)	.0107** (.00417)	.0114*** (.004)	.0565*** (.016)	.014** (.00625)	
PostAward $_{i,j,t}$	-.0106 (.0285)	-.0276 (.0264)	-.0346 (.0256)	-.103*** (.0373)	-.0365 (.0353)	
Post $_{i,j,t}$ · Tenure $_{k,t}$	-.0213*** (.00389)	-.0148*** (.00333)	-.0147*** (.00318)	-.0637*** (.013)	-.018*** (.00525)	
Tenure $_{k,t}$.129*** (.00288)	.0747*** (.00246)	.0569*** (.00254)	.208*** (.0058)	.127*** (.00341)	
Post $_{i,j,t}$.0931*** (.0193)	.0641*** (.017)	.0632*** (.0164)	.0969*** (.0272)	.0659*** (.0235)	
PostAward $_{i,j,t}$ · Tenure $^2_{k,t}$				-.0058*** (.00143)		
Post $_{i,j,t}$ · Tenure $^2_{k,t}$.00666*** (.00126)		
Tenure $^2_{k,t}$				-.0129*** (.000426)		
<u>Controls</u>						
Age $_{k,t}$	N	Y	Y	Y	N	
HighEduc $_k$	N	Y	Y	Y	N	
WagePctiles $_{k,t=-1}$ FE	N	Y	N	N	N	
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 panel shows the effect of the grant on employee log earnings, using Equation 1. The sample is restricted to incumbent workers, those 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 likely drive the effect of 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.

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)
PostAward _{<i>i,j,t</i>}							
·Age _{<i>k,t</i>}	.00278*** (.000869)	.00108 (.000761)					
·HighEduc _{<i>k</i>}			.0715*** (.0179)	.0344** (.015)			
·Wage ∈ 10, 50 _{<i>k,t=-1</i>}					.115** (.0573)	.0916* (.0553)	
·Wage ∈ 50, 90 _{<i>k,t=-1</i>}					.0935 (.0623)	.0709 (.0584)	
·Wage ∈ > 90 _{<i>k,t=-1</i>}					.235*** (.0648)	.178*** (.062)	
Wage _{<i>k,t=-1</i>}							.0333* (.0176)
<u>Controls</u>							
PostAward _{<i>i,j,t</i>}	Y	Y	Y	Y	Y	Y	Y
Post _{<i>i,j,t</i>}	Y	Y	Y	Y	Y	Y	Y
Post _{<i>i,j,t</i>} · X [†]	Y	Y	Y	Y	Y	Y	Y
Tenure _{<i>k,t</i>}	N	Y	N	Y	N	Y	N
Age _{<i>k,t</i>}	Y	Y	N	Y	N	Y	N
HighEduc _{<i>k</i>}	N	Y	Y	Y	N	Y	N
WagePctiles _{<i>k,t=-1</i>} FE	N	N	N	N	Y	Y	N
Wage _{<i>k,t=-1</i>}	N	Y	N	Y	N	N	Y
Year _{<i>t</i>} FE	Y	Y	Y	Y	Y	Y	Y
Firm _{<i>i</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 panel shows the effect of the grant on employee log 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 < 10pct_{*k,t=-1*}. Control coefficients are omitted for space considerations, but are available upon request. [†]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.

Table 7: Grant Effect on Within-Firm Inequality

Dependent variable: Inequality growth				
	90/10		99/50	Std Dev
	within 2 yrs			
	(1)	(2)	(3)	(4)
PostAward _{<i>i,j,t</i>}	.236*** (0.0822)	.265*** (0.0866)	.0791* (0.0458)	.0727*** (0.0268)
<u>Controls</u>				
Post _{<i>i,j,t</i>}	Y	Y	Y	Y
Rank _{<i>i,j</i>} , Rank ² _{<i>i,j</i>}	Y	Y	Y	Y
Age _{<i>i,t</i>} , Age ² _{<i>i,t</i>}	Y	Y	Y	Y
Year _{<i>t</i>} FE	Y	Y	Y	Y
Competition _{<i>j</i>} 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	
	Incumbent	New			
	(5)	(6)	(7)	(8)	
PostAward _{<i>i,j,t</i>}	.151** (0.0683)	0.00531 (0.0769)	-0.0127 (0.115)	.116** (0.0539)	.0556** (0.0217)
<u>Controls</u>					
Post _{<i>i,j,t</i>}	Y	Y	Y	Y	Y
Rank _{<i>i,j</i>} , Rank ² _{<i>i,j</i>}	Y	Y	Y	Y	Y
Age _{<i>i,t</i>} , Age ² _{<i>i,t</i>}	Y	Y	Y	Y	Y
Year _{<i>t</i>} FE	Y	Y	Y	Y	Y
Competition _{<i>j</i>} 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 sample to the two years after the grant application year (including 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.

Table 8: Grant Effect on Firm Growth Outcomes

Dependent variable:	Employment growth				Revenue growth			
			2-year window				2-year window	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostAward $_{i,j,t}$.271*** (0.0984)	.262*** (0.0968)	.27*** (0.0795)	.142** (0.0573)	.19*** (0.0614)	.183*** (0.0613)	.273*** (0.0707)	.159** (0.0624)
Award $_{i,j}$	-0.073 (0.0752)	0.0348 (0.0889)	0.019 (0.0333)					
Post $_{i,j,t}$	-0.0333 (0.0374)	-0.0321 (0.0375)						
Rank $_{i,j}$	0.00352 (0.00773)							
Rank $^2_{i,j}$	0.000107 (0.000189)							
Rank win $_{i,j}$		-0.0584 (0.036)						
Rank lose $_{i,j}$		0.000996 (0.0024)						
<u>Controls</u>								
Award $_{i,j}$	-	-	-	Y	Y	Y	N	Y
Post $_{i,j,t}$	-	-	N	Y	Y	Y	Y	Y
Rank $_{i,j}$, Rank $^2_{i,j}$	-	N	N	Y	Y	N	N	Y
Rank win/lose $_{i,j}$	N	-	N	N	N	Y	N	N
Age $_{i,t}$, 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
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 panel 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 show the effect in the two years after the grant application year (including the application year). We show control coefficients for employment and no other outcomes to minimize disclosure requirements. None of these variables have any significance in subsequent columns. Data are observed at the firm-year level. Standard errors are clustered by competition. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 9: Relationship between Growth and Earnings Effects

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

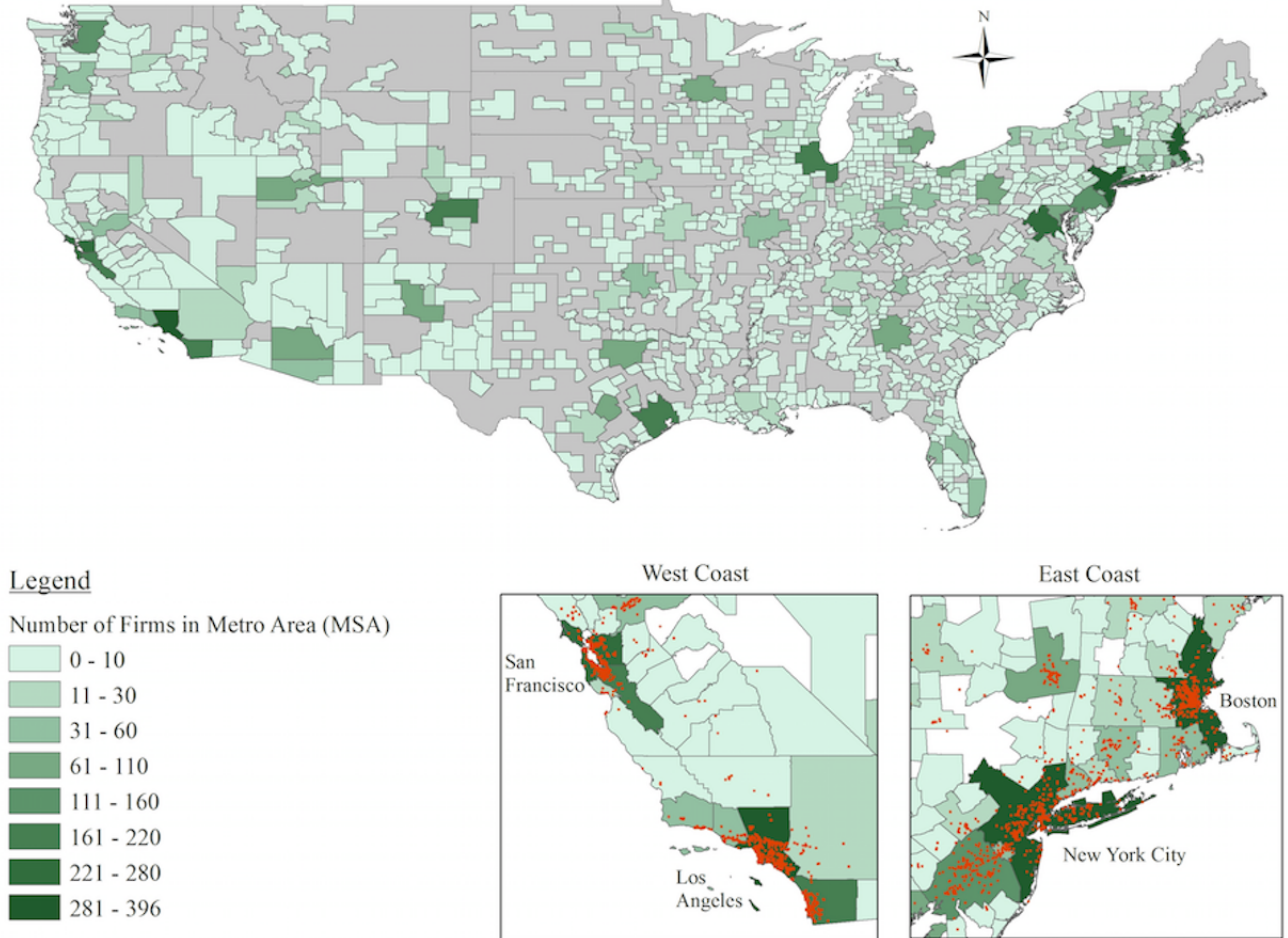
Note: Column 1 decomposes the effect of the award on earnings through revenue by instrumenting for revenue with the award. The Cragg-Donald F-statistic is 249 on the first stage, where revenue is regressed on the award. Columns 2-4 are restricted to the two years after the grant application year (including the application year). They interact the effect of the grant on log earnings growth with three characteristics, using Equation 1. The base year for the dependent variable is $t = -1$, the year before the application year. The characteristics are revenue growth, employment growth, and citations to granted patents applied for in the two years following the grant. Data are observed at the firm-year level. Standard errors are clustered by competition. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

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		
	Incumbent workers only						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$PostAward_{i,j,t} \cdot HighEmp_{i,t=-1}$	-.178** (.083)						
$PostAward_{i,j,t} \cdot HighAge_{i,t=-1}$		-.176*** (.032)					
$PostAward_{i,j,t} \cdot RevGrowth_{i,t \in -3,-1}$.104*** (.0242)				
$PostAward_{i,j,t} \cdot HighWage_{i,t=-1}$				-.25*** (.071)			
$PostAward_{i,j,t}$.21** (.0823)	.177*** (.028)	.0363*** (.013)	.279*** (.069)			
$Tenure_{k,t=-1}$					-.025*** (.005)		
$HighEmp_{i,t=-1}$.062*** (.010)	
$HighAge_{i,t=-1}$.023* (.012)
<u>Controls</u>							
$Post_{i,j,t}$	Y	Y	Y	Y	N	N	N
$Post_{i,j,t} \cdot X^\dagger$	Y	Y	Y	Y	N	N	N
Year _t FE	Y	Y	Y	Y	Y	Y	Y
Employee _i FE	Y	Y	Y	Y	N	N	N
N	177000	177000	177000	177000	21500	62000	62000
R^2	.743	.743	.759	.743	.002	.0008	.0007

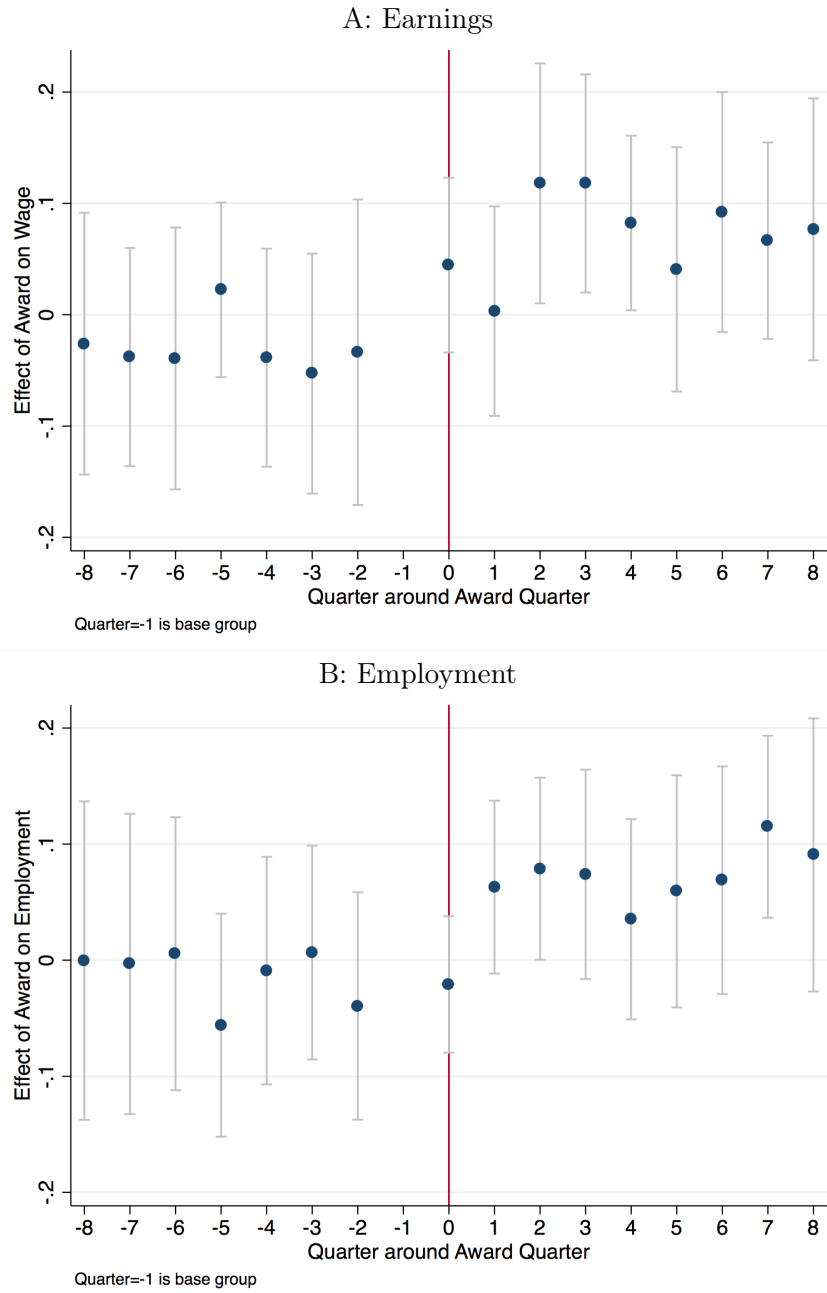
Note: This panel shows the effect of the grant on employee log earnings, using Equation 1. The sample is restricted to incumbent workers in columns 1-5 (those at the firm before the award year). Control coefficients are omitted for space considerations, but are available upon request. $^\dagger Post_{i,j,t}$ is interacted with characteristic of interest (e.g. $HighEmp_{i,t=-1}$ 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 in columns 1-4 and at the employee level in columns 5-7. “Year FE” in columns 5-7 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.

Figure 1: Applicant Firm Locations



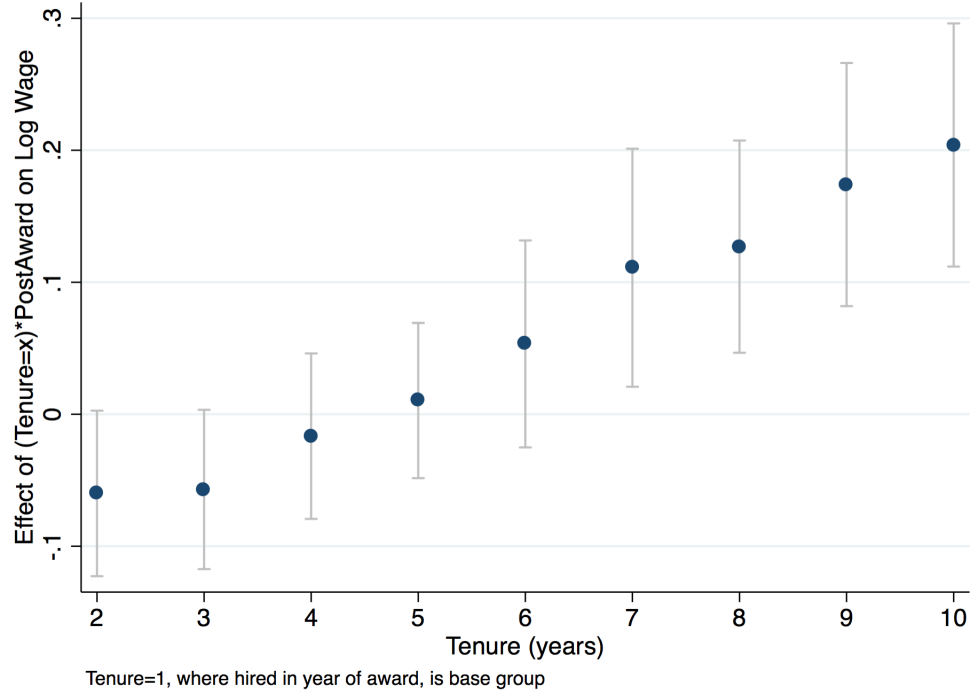
Note: This figure shows the location of all applicant firms in the data. In the main figure, a darker color for a metropolitan statistical area (MSA) indicates higher firm density. In the insets, actual firm locations are overlaid as orange dots.

Figure 2: Effects on quarterly outcomes



Note: These figures show the results from estimating Equation 3 on quarterly levels of log firm-year employment, payroll, and average 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). We do not show revenue to minimize disclosure requirements, and we cannot show inequality because the W2 data that permits the inequality measures are annual. 95% confidence intervals are shown.

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. The omitted group is those with one year of tenure, and more than ten years are excluded (the coefficients are noisier).

Figure 4: 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]



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 A

(For Online Publication)

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

<u>Probability in industry (most common 3 digit NAICS)</u>			
	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	
 <u>Other firm and employee statistics</u>			
	N	Mean	Std Dev
Firm employment in application year	2000	6.85	25.64
Firm age in application year	2000	8.309	6.378
Worker age	9600	43.1	8.398
Average worker tenure	9600	2.219	1.925
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	

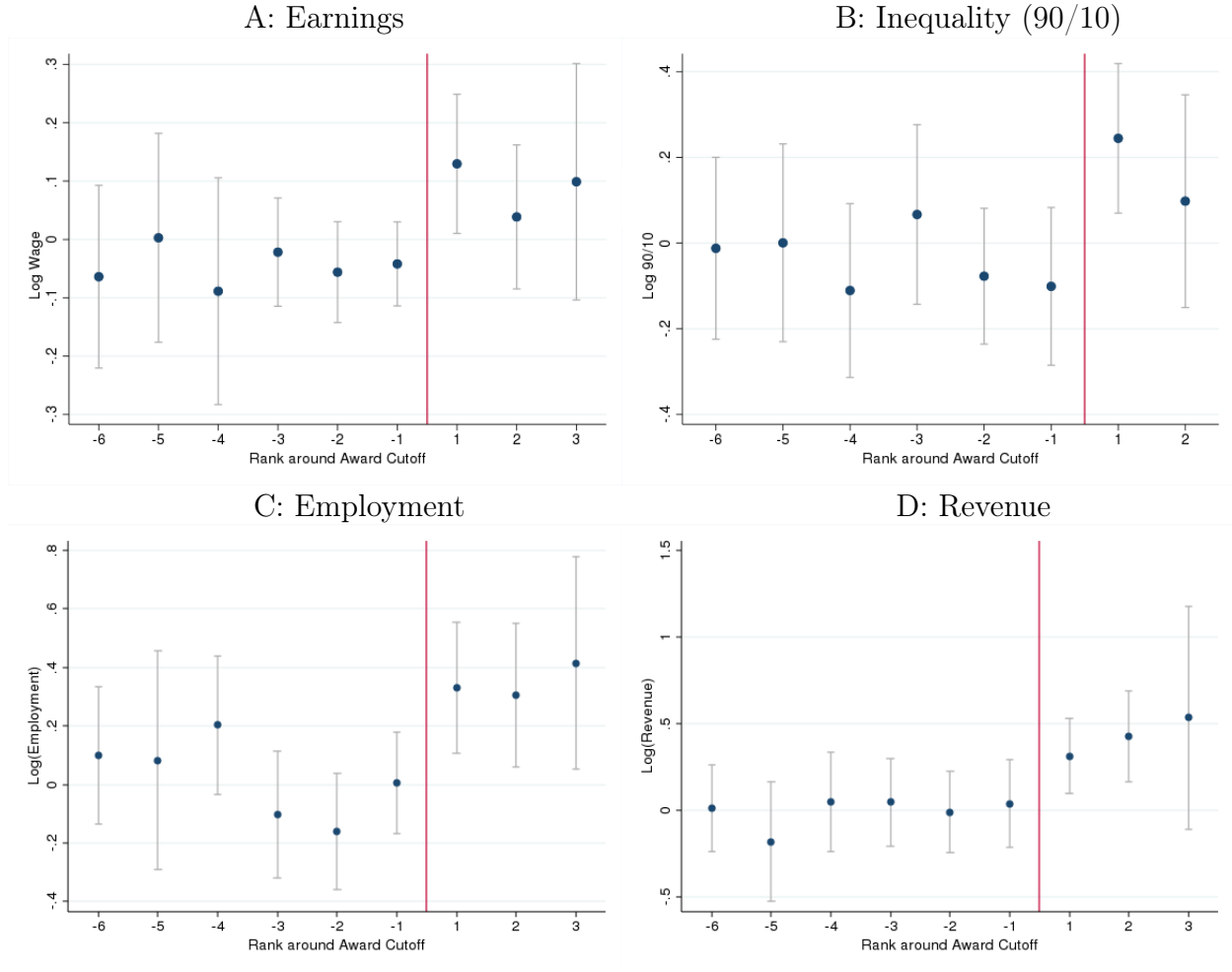
Note: This table shows summary statistics about the SBIR data that were matched to U.S. Census data. The share of firms in the most common eight 3-digit NAICS codes are shown (there are a total of 99 3-digit NAICS). Firms may change NAICS codes across years. Worker-related variables are from linked W-2-Individual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

Table A.2: Grant Effect on Percentiles of Earnings

Dependent variable:	Earnings growth percentile				Earnings level percentile			
	10	50	90	99	10	50	90	99
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PostAward_{i,j,t}$	-0.0941 (0.0722)	0.0589 (0.0603)	.142** (0.0618)	.151** (0.0733)	0.00296 (0.0625)	0.063 (0.052)	.154*** (0.0521)	.179*** (0.0674)
<u>Controls</u>								
$Post_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$Rank_{i,j}, Rank_{i,j}^2$	Y	Y	Y	Y	Y	Y	Y	Y
$Age_{i,t}, Age_{i,t}^2$	Y	Y	Y	Y	Y	Y	Y	Y
<u>Fixed effects</u>								
$Year_t$	Y	Y	Y	Y	Y	Y	Y	Y
$Competition_j$	Y	Y	Y	Y	Y	Y	Y	Y
N	7500	7500	7500	7500	9600	9600	9600	9600
R^2	0.0484	0.0747	0.097	0.0759	0.0521	0.107	0.133	0.194

Note: This panel shows the effect of the grant on earnings growth percentiles (columns 1-4) and percentiles of earnings levels (columns 5-8), using Equation ???. The base year is $t = -1$, the year before the application year. Control coefficients are not reported to minimize disclosure requirements. Data are observed at the firm-year level. Earnings is computed as the average earnings within the firm-year. Standard errors are clustered by competition. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Figure A.1: 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.