Do Cash Windfalls Affect Wages?

Evidence from R&D Grants to Small Firms

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Abstract

This paper examines how wages in financially constrained firms respond to a cash flow shock. The shock is a government R&D grant awarded to small, high-tech U.S. firms. The firms are linked to IRS W2 earnings ("wages") and other U.S. Census Bureau datasets. In a regression discontinuity design based on private ranking data, we find that the grant increases average wages with a rent-sharing elasticity of about 0.21. The effect on wages 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 receive wages beneath the firm average. The grant also increases employment and revenue, but a growth channel cannot fully explain the effect on wages. We discuss a channel that appears consistent with all the results, which is that financially constrained firms "borrow" from employees through backloaded wage contracts. In these contracts, employees initially accept low wages but are paid back when cash is available.

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1 Introduction

How do small, financially constrained firms set wages? These firms are crucial to economic growth and encompass about half of U.S. jobs.[1] If they must make tradeoffs between spending on optimal wages or net present value positive investment, 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. This paper studies firms that apply to a government grant program. They are high-tech, involved in energy innovation, young, private, and small, all characteristics likely associated with financial constraints. In a regression discontinuity design comparing grant awardees with unsuccessful applicants, we show positive effects of the cash flow shock on wages, and also on within-firm wage inequality and firm growth. Since there are no restrictions on how the grant is spent, it can be considered a cash flow shock. The evidence is most consistent with the positive effect of the cash flow shock in part reflecting a backloaded wage contract in which employees initially accept a lower wage with the expectation of a payout when the firm does well, as predicted by Michelacci & Quadrini (2009) and Guiso, Pistaferri & Schivardi (2013). Our 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 (Barth et al. 2016, Card, Cardoso & Kline 2016, Song et al. 2018).

The literature on rent sharing has focused on the pass-through of productivity shocks to wages using proxies for productivity-induced surplus that include 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). The ideal experiment would observe wage effects of randomly assigning cash to firms.

To approximate this experiment, we examine the effect of a U.S. Department of Energy (DOE) Small Business Innovation Research (SBIR) grant to high-tech, small firms. We use application and award data between 1995 and 2013, which we link to U.S. Census data on firms and their employees. Specifically, we link firms to the firm-level Longitudinal Business Database (LBD), the employee-level Longitudinal Employer-Household Dynamics (LEHD)

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

[2] In 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).

data, and the employee-level IRS W-2 earnings data. As these firms appear to primarily employ full-time workers, we follow convention in the literature and term these annual W-2 earnings "wages." Private ranking data permit a regression discontinuity (RD) design. The ranks are assigned by program officials to all applicant firms within narrow sector-specific competitions. The RD design assumes that after controlling for rank, firms around the cutoff for the award are ex-ante similar. The grant amount is uniform within a given year, at $150,000 in recent years. Awardees are not required to use the money as outlined in their applications, nor are their expenditures monitored ex-post. A benefit of these data is that they provide a well-defined and fairly homogenous sample of small, young, high-tech U.S. firms. A downside of the setting, of course, is that we cannot necessarily extrapolate the results to large or non-high tech firms.

Using firm-level regressions, we find that receiving a grant leads to a nine percent increase in wages. 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. The effect does not appear to reflect more hours worked. At the employee level with employee fixed effects, we find effects of three to four percent. With both firm and employee effects (thus identifying off switchers), the effect is about eight 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 a number of more recent estimates.

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 is no effect for new hires (those hired in the year of the award or after) at entry or in the longer term. 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, suggesting that market conditions do not affect wage sensitivity to changes in recent profits. 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. This 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. Similarly, Howell (2017) finds that these SBIR grants positively affect subsequent innovation, with larger effects for smaller and younger firms.

Several tests show that a firm growth channel cannot fully explain the effects on incumbent wages. For example, the entire wage effects are observable within two quarters, while only half the long term revenue effect exists within the first two years. A revenue decomposition, in which we "instrument" for revenue growth with winning an award, finds a much smaller and weaker effect on wages than the main effect of the award. More generally, if the worker's marginal product increased after the grant, for example because she has learned to work with new equipment, there is no reason that this would not apply to the long term wages of new hires as well.

Heterogeneity in the wage effects leads to higher within-firm inequality. For example, a grant increases the growth of the log wage difference between the 90th percentile and the 10th percentile by 24 percent, an effect that is larger within the first two years of the grant. 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*b*, Mueller, Ouimet & Simintzi 2017*a*, Song et al. 2018).

Wages might increase after a cash flow shock for a number of reasons besides a growth channel. We use cross-sectional variation to distinguish which appears most likely. The evidence has inconsistencies with benchmark employee bargaining power, incentive contracting, efficiency wages, and match quality models. It is more consistent with the possibility that employment relationships compensate for financial frictions. Azariadis (1988), Burdett & Coles (2003), Michelacci & Quadrini (2009), and Bernhardt & Timmis (1990) theoretically show how employees can borrow from or lend to firms in settings where firms can commit to long term contracts. If workers are liquidity constrained but the firm has perfect access to capital markets, the wage contract is flat and increases only when the employee's outside option increases. This predicts no effect of the grant. Michelacci & Quadrini (2009) and Guiso, Pistaferri & Schivardi (2013) suggest the opposite situation,

where the firm is financially constrained and workers are willing to "lend" to the firm. Then the contract may backload the wage, which increases when the firm's constraints ease. That is, the worker initially agrees to be underpaid relative to his outside option in exchange for a higher wage later. This steeper wage profile is consistent with workers lending implicitly to their employers.

In our data, the facts that new hires are unaffected and incumbent worker benefit increases with job tenure are consistent with financially constrained firms offering backloaded wage contracts that are at least partially repaid after a windfall. A number of additional predictions of this model are satisfied. The effect is larger among firms that we expect to be more constrained, that initially paid below-market wages, and that grew faster before the grant application. If incumbent workers accept a backloaded contract, their initial wage should reflect a "constrained employer" penalty. Indeed, the percent raise in the first year at the SBIR applicant firm relative to the previous job is decreasing in worker tenure. More broadly, the median worker at the likely constrained firms in our sample accepts a lower wage when he joins than he earned at his previous firm. A back-of-the-envelope calculation suggests that after a grant, incumbent workers earn a premium for having accepted the backloaded contract. We do not find evidence that there is a significant change in the overall wage-tenure profile after the grant, suggesting that the firm remains constrained and engages in similarly backloaded contracts with new hires. 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 in engaging in new backloaded wage contracts.

This paper is closely related to the literature on the relation between 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 deserve further discussion. First, Aghion, Akcigit, Hyytinen & Toivanen (2018) find that after Finnish firms patent, all employees benefit, with the owners and inventors benefiting the most. A patent grant represents a productivity shock, or the expectation of a future stream of cash flows from monopolization of an innovation. Rather than a shock to future expected cash flows, we focus on the effects of an actual cash flow shock. Second, Kline, Petkova, Williams & Zidar (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. Most importantly, receiving a high-value patent is a productivity shock, not a certain one-time cash flow shock. Second, they assume that patents are granted or rejected at random conditional on firm fixed effects.[3] We use a regression discontinuity design with a quasi-experimental interpretation. Third, most of the patent values in Kline et al. (Forthcoming) are extrapolated from the relationship between stock prices and six characteristics for public firm patents.[4] In contrast, our source of variation is whether an applicant firm wins a grant or not. Despite different methodologies, the conclusions are quite consistent. Kline et al. (Forthcoming) show that patent-instrumented surplus leads to higher wages among incumbent top earners and especially inventors. 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 cash flow shock rather than a patent.

This paper is also closely related to work on rent sharing, including Black & Strahan (2001), Macis & Schivardi (2016), and Bergman et al. (2017). Consistent with our results, Fonseca & Doornik (2019) find that relaxing credit constraints increases wage inequality, with the wage adjustment primarily among incumbent workers. This paper also contributes to the literature on how firms spend cash in the presence of frictions. Starting with Fazzari, Hubbard & Petersen (1988) and Hoshi, Kashyap & Scharfstein (1991), the literature has focused on investment (see also Hennessy & Whited 2007 and Gilje & Taillard 2016). Also related is Cespedes, Huang & Parra (2019), who study how local retailers who sell lottery tickets respond to a cash windfall from a lottery winning customer. This paper examines the labor side. Finally, an additional contribution is to provide the first causal evaluation of how R&D grants affect firm revenue, employment, and wages; previous literature has focused primarily on subsequent patenting and investment (Einiö 2014, Bronzini & Iachini

[3]They base this assumption on evidence that five firm variables do not predict the allowance decision. The variables are employees, revenue, value added, wage bill, and EBITD. Based on this evidence, the authors "proceed by assuming that any remaining selection is on time-invariant firm characteristics that can be captured by firm fixed effects." The implication is that patent examiners randomly allow patents within a given firm, which is surprising given the resources associated with the patent examination process.

[4]This exercise uses the patent values in Kogan et al. (2017), which are available only for granted patents among publicly traded firms. Nearly all firms in Kline et al. (Forthcoming) are private, and 92 percent of patent applications are rejected. The six characteristics are the patent family size, patent number of claims, firm revenue, employees, and the application and decision years. Art unit (a USPTO patent classification scheme) effects are included, but due to data constraints the authors treat these as *iid* normal draws rather than estimate them.

2014, Jaffe & Le 2015, Howell 2017).[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 eleven 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]

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, US-based, and majority US-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)

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

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

when she conducts the ranking. She submits ordered lists to a central DOE SBIR office, which determines the cutoff.[7]

The firms in our data tend to be focused on a specific technology, rather than being diversified. 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). Many of the firms can be described as high-tech startups. A drawback of our data is that the sample of firms 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 $884 million (in 2012 US$) in SBIR grants between 1983 and 2013. The data include 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.

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

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

each employee. These data begin in 2005 and end in 2013. We observe only employee wages, not capital gains or other types of income. We do not observe hourly wages. Instead, the wage should be thought of as salary income, as most of the jobs in this sample are full-time jobs. 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 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. There are 2,100 unique applicant firms in 270 competitions. The average number of employees is 35, though it is about seven in the year before the award year. For all firms in the U.S. in 2012, the average is 20 employees, and within establishments with 20-99 employees, the average number of employees is 39.[8] Average revenue is \$4.8 million; though the distribution is highly right-skewed. The average is also well-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 wages are also higher, at \$64,150 relative to \$40,417 across all U.S. firms with 20-99 employees in 2012. For additional details and summary statistics about the application and award data beyond those provided here, see Howell (2017).

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 near zero but negative. That is, size measures are larger in the year before application compared to other years, reflecting wages growing on average and being lower in the years before the

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

application, with more observations before.

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*a*), and Abowd et al. (2018). We also examine upper-tail inequality, the 99/50 ratio, and 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 increase by the same percent. (In unreported results, we found generally similar effects using the interquartile range.)

Employee-level statistics are in Panel C. The average wage 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). Panel D shows that incumbent workers are more highly educated, older, and have much higher wages that 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.[9] 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, while in the application year the average founder is 51 years old. Just 22 percent of employees are female, and only 11.5 percent of founders are female. There are also disparities relative to the population in ethnic makeup; only 2.7 percent of employees are Black, for example. Seventy-one percent of employees s are U.S.-born.

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

3 Estimation Approaches

The ideal experiment would randomly allocate cash to a subset of firms, enabling us to examine the effect of the exogenous cash flow shock on firm outcomes. Following Howell (2017), we approximate this experiment using a regression discontinuity (RD) design, which estimates a local average treatment effect around a cutoff in a rating variable. 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 $Rank_{i,j} = -1$. 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. We find very similar results in a non-panel setting, like that in Howell (2017), where each observation is an application rather than a firm-year. The panel approach follows Guiso et al. (2005) and Cardoso & Portela (2009*a*). It exploits the richness of the U.S. Census data, permitting finer controls.

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

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

The dependent variable is either a levels measure, such as the average wage in firm i in year $t(W_{i,t})$, 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 around the cutoff due

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

to disclosure limitations. However, all of the results are qualitatively robust to using narrow bandwidths around the cutoff, and as shown below, rank continues to be uninformative about outcomes, as in Howell (2017).

The primary specification controls for rank within the competition quadratically, as shown in Equation 1. We do not show higher order polynomials, following Gelman & Imbens (2018). 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.

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 around the discreteness of the rating variable, and extensive evidence of continuity of observable baseline covariates around the cutoff.

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

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

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.

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

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, which are the most stringent specification possible, as they control 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 both Equations 2 and 3, standard errors are clustered by competition.

4 Grant Effect on Wages

4.1 Average wages

Table 2 shows the grant effect on wage growth, 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 wage growth (the ratio of wages in the current year to the base year) by about nine percent (column 3).[11] Roughly the same nine percent effect is found when the dependent variable is levels of wages in column 5. 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 wages by quarter around the award quarter, using Equation 3. Figure A.1 A shows the effect on levels of log wages by rank around the cutoff, using Equation 2.

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

The effects are robust to a number of unreported approaches. They are similar with a bandwidth of one firm around the cutoff. 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. We cannot rule out that the effect on wages 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 wages 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, the effects differ dramatically between new and incumbent employees, shown below, which would be unlikely if the average effect reflected higher hours.

To situate the effect in the rent-sharing literature, we can approximate a rent-sharing elasticity at the firm level. To motivate this measure, consider the following standard relationship between rents per worker and wages, following Card et al. (2018). We denote by w the wage, o the worker's outside option, $\gamma \in [0, 1]$ a rent-sharing parameter (assumed to derive from a bargaining process between workers as a group and the firm), G the rent (here, the grant), and N the number of employees:

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

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

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

To arrive at an estimate of ξ, the literature typically relates a measure of quasi-rents, often value-added per worker, to wages on an annual basis (Card et al. 2018).[12] The parallel in our context is a calculation of the wage elasticity to the grant in the year following the award. The effect of the grant on levels of wages 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

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

rent-sharing parameter γ of 0.56.[13]

An elasticity of 0.21 is similar to previous findings, if on the larger end. In a seminal study, Van Reenen (1996) instruments for rents with innovation and finds a very 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 the relationship between patents and wages and the relationship between patents and profits. 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, Du Caju et al. 2011, Card et al. 2014, Card et al. 2016). Estimates using revenue per worker are similar or slightly smaller, including Barth et al. (2016), Carlsson et al. (2016), and Bagger et al. (2014).

We next turn to employee-level analysis, where we use log wages as the dependent variable and include employee fixed effects, which absorb firm fixed effects as each employee is observed only at the applicant firm. The main estimates in columns 1-3 of Table 3 find effects of three to four percent. An estimate of four percent yields a rent-sharing elasticity of 0.09. 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 slightly stronger within two years (Column 3). Previous estimates that use individual data are similar, at 0.01-.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.

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*b*) and Guiso, Pistaferri & Schivardi

[13]To proxy for the outside wage we use the median wage among firms that did not win an award in the year before the award year, as these are arguably the most similar firms to the winning firms, which is $47,010.

(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.

4.2 Incumbent vs. new employees

We next examine how the grant effect on wages is distributed across new and pre-existing (incumbent) employees.[14] 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). An award increases the difference between incumbent and new hire wages 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 7). The result is consistent with Kline et al. (Forthcoming), who find that patent grants do not lead to higher wages for new employees.

Table 1 Panel D compares new and incumbent workers. The first set of statistics show that incumbent workers are more educated, older, and have higher average wages. 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 wages. 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. We find no effect of winning on employee departures from the firm.

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

4.3 Tenure and other employee characteristics

To explore what may explain the large effect of the grant on incumbent wages, we interact winning an award with various employee characteristics among incumbent employees. By far the largest and most robust source of heterogeneity is tenure, or the number of years an incumbent employee has been with the firm. 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 $\text{PostAward}_{i,j,t} \cdot \text{Tenure}^2{}_{k,t}$ 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 very 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 wage percentiles, where wages 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. In sum, while the effect of the cash flow shock on wages does increase with measures of employee skill, and especially for top earners, the effect of tenure is by far the largest economically, even after controlling for 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). Figure A.1 Panel B demonstrates the effect on the 90/10 ratio by rank around the cutoff.[15] 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 wage percentiles are in Table A.2 Panel 2. Columns 1-4 use wage growth outcomes, and columns 5-8 use wage level percentiles.[16] We see the same patterns for both outcomes; 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 wages drives the effect on inequality. Table 7 columns 6 and 7 show that there is no effect of winning on inequality within incumbents or new hires, consistent with Table 4. New hires induced by the grant do not receive an

[15] 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.

[16] Note 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.

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, such as though 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 after the application year relative to the base year is shown as the coefficient on $PostAward_{i,j,t}$ in columns 1-3. 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.[17]

[17]The coefficient gives the percentage change in $\frac{Y_{i,t}}{Y_{i,t=-1}}$ associated with being an award recipient relative

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 wages, 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.[18] 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.[19]

To explore whether the effect on wages 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 wages. 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 wages by about 8 percent. Therefore, while the grant's effect on revenue is passed to wages, a maximum of about 60 percent of the total effect on wages can be explained though a revenue channel.

The second test shows that the effect of winning is not higher among firms with higher growth or innovation after the grant. Columns 2-4 of Table 9 conducts heterogeneity analyses, all of which restrict the sample to the two years after the grant application year. First, we interact winning with revenue growth (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

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).

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

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

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 wages is if anything smaller among firms that grow more after award. This suggests that the subset of firms that invest the grant in order to grow pass less of the grant to wages. There may be substitution between spending the grant on wages and investment to further innovation. Regardless of these more speculative possibilities, it is clear that the pass-through to wages does not entirely reflect a productivity-related channel.

6 Exploring the Mechanism

This section provides six hypotheses for how a cash flow shock might plausibly affect wages, and in the process discusses relevant literature, in particular important work not mentioned in the introduction. We consider each hypothesis in relation to the evidence presented above, and conduct additional analysis to shed light on which channels best explain the results. In the discussion below, we rely primarily on cross-sectional evidence, which is more descriptive than the causal analysis that establishes the main effect of the grant on wages presented above. We are not able to precisely pin down a single mechanism – and indeed it is likely that multiple are at play – but instead discuss which channel the evidence best supports.

7 Theoretical Context & Hypotheses

7.1 Standard Model

With perfect capital markets, a neoclassical model predicts that the money goes to shareholders because (a) workers are paid their marginal product and (b) the firm is financially unconstrained, so the grant should not affect investment. Like much of the existing empirical literature, the results in the preceding sections are clearly at odds with this model, as it predicts no effect of a cash flow shock on wages.

The grant could be paid to shareholders via wages to owner-employees, who likely have

the longest tenures. However, in this case we would not expect a linear effect of tenure, as shown in Figure 3. We would expect a convex relationship, but instead we observe a slightly concave relationship. Further, it ought to be tax disadvantaged to pay dividends via wages. Finally, we show in Table 5 column 5 that the effect of tenure interacted with winning persists among incumbents hired at least three years after the firm is first observed, who are not plausibly owners.

7.2 Bargaining Power

Many wage-setting models center around a bargaining parameter that weighs employee productivity and the outside option (Brown & Ashenfelter 1986, Abowd & Lemieux 1993, Stole & Zwiebel 1996, Hall & Milgrom 2008). In the simplest static model similar to what we use to compare our rent-sharing finding to the literature (Equation 4), the wage can be written as:

$$w_{i,k} = (1-\gamma)\, o_i + \gamma \theta_{i,k}. \tag{6}$$
$$\theta_{i,k} = f\left(\text{productivity}_{i,k}, \text{rents}_k\right)$$

where the outside option is denoted o_i. Employees with low bargaining power (low γ), who also likely have low wages, should have wages that move closely with the outside option. If the grant permits investment that leads to growth, this growth could be associated with training or other effects that increase an employee's productivity. Note that the grant does not directly affect productivity; this would be an indirect effect via a growth channel.

The baseline employee wage in our sample may be determined primarily via a bargaining model like that in Equation 6. More generally, it is difficult to succinctly assess bargaining power as theoretical models of how it might manifest are so diverse. However, our results regarding the effect of a cash windfall on wages are inconsistent with a benchmark, static bargaining model. One reason is that new hires do not benefit at all. In a bargaining model, they should benefit just as much as incumbents in an estimate that includes employee fixed effects. Relatedly, we expect both o_i and $\theta_{i,k}$ to be at least as relevant for new hires. Information about the grant is public, so it is not the case that insiders know more about the windfall.

Also, it is not the case that wages among employees with low wages, who likely have low

bargaining power, move more closely with the outside option. To assess whether retention channels are important, we interact the effect of the award with measures of labor market tightness, specifically annual state and industry unemployment. The grant effects do not vary with these measures. This null interaction persists throughout the wage distribution, and it also persists when the sample is restricted to new hires, for whom the outside option should be more immediately available as they are likely more actively searching for a new job. Also, the effect does not robustly increase with proxies for skill; while it increases with pre-existing wage and education, the effects are economically small.

Models in which bargaining occurs over marginal product seem unlikely to explain the grant effect because the grant does not affect productivity (i.e., the employment relationship surplus), so an effect would have to occur through a growth channel. We observe the entire effect on wages within the second quarter after the grant, while the long term effect on revenue is halved when we limit observation to the first two years after the grant. Finally, we present evidence below that the effect is larger among more financially constrained firms, which we would not expect if employee bargaining power primarily explained the positive average effect. That is, if an unconstrained firm receives a cash windfall, a bargaining power story should enable workers at that firm to benefit at least as much as workers at a constrained firm.

7.3 Incentive contracting

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

Incentive contracting is probably occurring to some degree; it is likely that the longest-tenure, highest-earning individuals would have been most responsible for the grant application. However, several results are at odds with incentive contracting as a primary explanation. First, there is no reason the effect would reflect measures of financial constraints. Second, we would expect that more direct measures of employee skill (proxies

for being the scientists and managers at a small firm who would have applied for the grant) would offer the strongest cross-sectional sources of heterogeneity. Instead, heterogeneity is strongest in tenure, and *all* incumbent employees, including low wage workers, benefit. It seems unlikely that low wage workers, such as administrative assistants, would have contributed to receiving an R&D grant.

7.4 Efficiency wages

It is possible that the grant enables a formerly constrained firm to pay a wage that exceeds market-clearing level to maximize labor productivity, often called an "efficiency wage." There are four varieties of efficiency wage models. First, efficiency wages may be paid to reduce turnover (Salop 1979, Becker 1964). Second, efficiency wages may deter shirking if there is a cost to losing the job, which would not exist at the market-clearing wage (Shapiro & Stiglitz 1984). Third, higher wages may attract higher quality applicants, which may be valuable to a firm that cannot perfectly observe applicant quality (Weiss 1980). Fourth, efficiency wages may reflect fairness considerations (Solow 1980, Kahneman, Knetsch & Thaler 1986, Fehr & Schmidt 1999).[20] Here, employee effort is a function of the wage relative to the perceived fair wage, which the employee arrives at by comparing pay with coworkers at the same firm (Akerlof & Yellen 1988, Akerlof & Yellen 1990) or with similar workers at other firms (Summers 1988). An efficiency wage channel predicts a symmetrical effect among new hires. The absence of any benefit among new hires is inconsistent with a purely efficiency wage channel. However, below we will employ the notion that fairness concerns are important in labor contracts (Akerlof & Yellen 1990, Summers 1988).

7.5 Match quality or search frictions

Several theories seek to explain the strong relationship between wages and tenure. In light of the strong interaction between the grant and tenure, these theories deserve particular attention. In an influential model, Jovanovic (1979) theorizes that the wage may reflect expected productivity, but this is subject to imperfect information about the quality of the match between the firm and the employee. It is possible that after the award, expected

[20] This is distinct from shirking models such as Shapiro & Stiglitz (1984) because it does not reflect the costs of losing a job, and because it relies on "gift relationships" as described in Akerlof (1980) and Akerlof (1982).

productivity or information about match quality changes in such a way as to cause the firm to pay wages that reflect what has been learned over time about the quality of the match. An alternative is that upward-sloping wage-tenure profiles reflect frictions in the search process (Burdett & Coles 2003). Shi (2009) models a scenario in which firms commit to labor contracts but workers, who are risk-averse, cannot commit not to quit. In the model, wages strictly increase with tenure to prevent employees from quitting; the mechanism is that the chances of a better outside offer fall as wages rise. This relationship depends on the worker being risk-averse. Stevens (2004) treats workers as risk neutral, and finds it optimal to backload wages but in a non-linear way.

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

7.6 Lending within the Firm

The final hypothesis also relies on wage-tenure profiles, but suggests the financial mechanism of within-firm lending to explain them. On one hand, Azariadis (1975), Azariadis (1988), and Bernhardt & Timmis (1990) argue that wage-tenure dynamics are flatter than they would be in the absence of financial frictions because more risk-neutral firms insure more risk-averse workers. The flat wage contract enables workers to smooth consumption, which they cannot achieve by borrowing in outside financial markets.

On the other hand, Michelacci & Quadrini (2009) and Guiso et al. (2013) argue that financial constraints lead the wage-tenure correlation to be higher than it otherwise would be. Michelacci & Quadrini (2009) model how a financially constrained firm may optimally pay workers lower wages initially, implicitly borrowing from them. 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.[21] In this way, the firm can grow faster than it would otherwise. Relatedly, Cardoso & Portela (2009*a*) model insurance at the firm level. In our case, note that even if the grant reduces financial constraints, the firm may remain very constrained, and even if implicit lending within the firm is occurring, the firm might spend the grant only on other things.

The results thus far are quite consistent with the hypothesis that financially constrained firms offer backloaded wage contracts in which the wage rises constraints slacken. If the firm does use 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.

7.6.1 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).[22] 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

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

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

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 are 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 above-median employment and age in the year before the grant application. In both cases, the coefficient is large and negative, implying that the effect of winning is about 18 percent smaller for smaller or younger firms. We use indicator variables here because at the employee level, these variables are quite skewed. These relationships persist at the firm level. This is consistent with Howell (2017)'s finding that winning has a larger effect on innovation and VC among smaller and younger firms.[23]

Michelacci & Quadrini (2009) predict that firms growing faster should initially pay lower wages, which they find to be the case on average in Finnish data. Consistent with this, we find that firms growing faster before the application year experience larger effects. This is shown in Table 10 column 3, where 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. Also, 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. Finally, 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 very constrained using less of the grant to repay existing backloaded wage contracts. Relatedly, we expect that wages will increase as profits rise if they are initially pushed down by firm financial constraints. Unfortunately, we do not observe profits. But consistent with this hypothesis, we do find that on average as revenue increases, wages rise more for workers with high tenure.[24]

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

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

7.6.2 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. Consistent with this, 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, which are consistent with backloaded wage contracts being used across all employees. 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.[25] 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 even more backloaded contracts. The effect on incumbent workers reflects a need to credibly 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.

If incumbent workers accept a backloaded contract, their initial wage should reflect a "constrained employer" penalty. Most importantly, 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 also provide some 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. However, there is substantial skewness: Figure 4 shows percentiles separately for incumbents and new hires. Among both groups, the 10th percentile of workers experienced a large pay penalty, the median a small penalty, and the 90th percentile experienced a large wage increase. The distributions are visually quite similar across the two groups of workers, but Table 8 shows that the average percent raise is statistically significantly larger for new employees. (Note the average of 24 percent raise across the whole distribution is on the high side but not dissimilar to data on pay raises in general for highly educated individuals.[26]) There is no difference in the percent raise among new hires across award status, consistent with the

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

[26] E.g., see for data scientists: https://www.burtchworks.com/2019/05/13/2019-update-analytics-salary-increases-when-changing-jobs/

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).[27] There is no effect of an award on the percent raise for new hires vs. incumbents because the award only affects incumbents. The absence of an effect confirms that the lack of an effect on new hires is not driven by different composition of new hires across firm types.

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 worker 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.[28] While the exact number is of course sensitive to assumptions, it is clear that an incumbent worker with long tenure who is at a firm that wins the grant is handsomely compensated.

7.6.3 Enforcement mechanism

Why doesn't the firm renege on a backloaded wage 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.

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

[28] The closest measure we have to the average unconstrained wage bump is the bump for new hires among awardees, which is 24 percent. The percent increase is decreasing by .025 on average per year of tenure (Table 10 column 7). A worker with seven years of tenure (about one standard deviation above the mean) therefore "missed out" on about 4.2 percent of wage gain when hired. The average wage in the last year of the previous job is $47,570, implying that he missed out on $1,997 per year. The increase in wages due to the grant is about nine percent. Relative to the average incumbent wage 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.

If the human capital of the employee is firm-specific, the firm should in theory be able to hold up the employee just as well as the reverse.[29] 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 & 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.

8 Conclusion

This paper offers the first evaluation of how a cash flow shock affects firm wages and other outcomes. The setting is a government R&D grant 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 (Haltiwanger, Scarpetta & Schweiger 2014). Generally, the firm might spend the grant on dividends (i.e. transfer it to owners or shareholders), wages, or investment in physical or human capital (i.e. new hires). 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

[29] The authors thank Holger Mueller for this suggestion.

grant also increases within-firm wage inequality, as well as employment and revenue. However, a growth channel does not fully explain effects on wages.

The effect on wages is larger among firms that are likely more financially constrained and that previously paid below-market wages. The absence of a standard flat contract in which the firm insures the employee seems to reflect the setting of financially constrained (small, young, private) firms. The results are most consistent with the firm sharing rents with employees to repay an implicit loan established through a backloaded wage contract.

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, Troske 1999, Carpenter 2007, Elfenbein et al. 2010). Indeed, models of rent-sharing via bargaining assume the firm is small enough for the employee to have the ability to bargain directly (Stole & Zwiebel 1996). However, we cannot necessarily extrapolate our results to large or non-high tech firms.

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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 $)	30500	2546	6141	689.5
Employment	30500	35.36	72.17	11.51
Award amount/employment$_{t=-1}$	30500	21880	33690	9106
Average wage ('000 2010 $)	30500	64.15	38.55	57.85
90/10 log wage differential	9600	1.809	1.053	
99/50 log wage differential	9600	0.951	0.702	
Standard deviation of log wages	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
Wage	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
$\text{Wage}_{k,t}$ at SBIR firm ('000 2010 $)	257000	63.50	86.54	49.99	Person-year
$\text{Wage}_{k,t}$ all jobs ('000 2010 $)	909000	58.92	84.39	44.45	Person-year
$\text{Tenure}_{k,t}$ at SBIR firm (years)*	257000	3.85	3.11	3	Person-year
Percent raise from last year of previous job to first year at SBIR firm	62000	0.24	1.32	-0.061	Person
As of 2nd year after award, firm # of:					
Incumbent employees	2300	6.689	12.38	5	Firm
New employees	2300	4.036	24.32	0	Firm

D. Employee characteristics by incumbent or new hire status

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

Note: This panel contains summary statistics about the wage distribution. All statistics 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 Wages (Firm-Level)

Dependent variable:	Log wage growth				Log wage levels			Log payroll
				2 year window		Incumbent	New	Within 2 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{PostAward}_{i,j,t}$	.134***	.133***	.0946**	.126***	.0931**	.137***	0.0607	.148*
	(0.048)	(0.0481)	(0.0391)	(0.0406)	(.0387)	(0.0482)	(0.0753)	(.0776)
Controls								
$\text{Award}_{i,j}$	Y	Y	N	Y	Y	Y	Y	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	N	N	Y	Y	Y	Y	Y
Rank | $\text{win/lose}_{i,j}$	N	Y	N	N	N	N	N	N
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	N	Y	Y	Y	Y	Y
$\text{Firm-app}_{i,j}$ 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 wage 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 ($\text{Firm-app}_{i,j}$ 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). Column 7 examines the effect of the grant on the wage of the firm founder. 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 Wages (Employee-Level)

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

Note: This panel shows the effect of the grant on employee log wages, 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 $\text{controls}_{k,t=-1}$ 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 Wages among Incumbent and New Employees

Dependent variable: Log wage 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)
$\text{PostAward}_{i,j,t}$	.15**	0.058	.121**	0.0587	.156***	0.0453	.146**	0.0362
	(0.0706)	(0.0755)	(0.0503)	(0.0871)	(0.0559)	(0.103)	(0.0655)	(0.117)
Controls								
$\text{Award}_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	Y	Y	Y	Y	Y	Y	Y	Y
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	Y	Y	Y	Y	Y	Y
N	8200	3200	8200	3200	8200	3200	8200	3200
R^2	0.17	0.103	0.129	0.14	0.13	0.137	0.183	0.148

Note: This table shows the effect of the grant on wage 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 Wages Among Incumbent Employees by Tenure (Employee-Level)

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

Note: This panel shows the effect of the grant on employee log wages, 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 Wages Among Incumbent Employees by Employee Age, Education, and Wage (Employee-Level)

Dependent variable: Log wage

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

Note: This panel shows the effect of the grant on employee log wages, 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 wage group is Wage $< 10\text{pct}_{k,t=-1}$. Control coefficients are omitted for space considerations, but are available upon request. $^{\dagger}Post_{i,j,t}$ is interacted with characteristic of interest (e.g. $Age_{k,t}$ in column 1). Note that $Award_{i,j}$ is defined at the firm level, so is absorbed by firm fixed effects. Data are observed at the employee-year level. Standard errors are clustered by employee. $*$, $**$, and $***$ denote significance at the 10%, 5%, and 1% levels.

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$_{i,j,t}$	.236***	.265***	.0791*	.0727***
	(0.0822)	(0.0866)	(0.0458)	(0.0268)
Controls				
Post$_{i,j,t}$	Y	Y	Y	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	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)	(9)
PostAward$_{i,j,t}$	.151**	0.00531	-0.0127	.116**	.0556**
	(0.0683)	(0.0769)	(0.115)	(0.0539)	(0.0217)
Controls					
Post$_{i,j,t}$	Y	Y	Y	Y	Y
Rank$_{i,j}$, Rank$^2_{i,j}$	Y	Y	Y	Y	Y
Age$_{i,t}$, Age$^2_{i,t}$	Y	Y	Y	Y	Y
Year$_t$ FE	Y	Y	Y	Y	Y
Competition$_j$ FE	Y	Y	Y	Y	Y
N	9600	8200	3200	9600	9600
R^2	0.1	0.174	0.12	0.136	0.0441

Note: This table shows the effect of the grant on inequality measures using Equation 1. Column 2 restricts the 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***	.262***	.27***	.142**	.19***	.183***	.273***	.159**
	(0.0984)	(0.0968)	(0.0795)	(0.0573)	(0.0614)	(0.0613)	(0.0707)	(0.0624)
$\text{Award}_{i,j}$	-0.073	0.0348	0.019					
	(0.0752)	(0.0889)	(0.0333)					
$\text{Post}_{i,j,t}$	-0.0333	-0.0321						
	(0.0374)	(0.0375)						
$\text{Rank}_{i,j}$	0.00352							
	(0.00773)							
$\text{Rank}^2_{i,j}$	0.000107							
	(0.000189)							
$\text{Rank} \mid \text{win}_{i,j}$		-0.0584						
		(0.036)						
$\text{Rank} \mid \text{lose}_{i,j}$		0.000996						
		(0.0024)						
Controls								
$\text{Award}_{i,j}$	-	-	-	Y	Y	Y	N	Y
$\text{Post}_{i,j,t}$	-	-	N	Y	Y	Y	Y	Y
$\text{Rank}_{i,j}$, $\text{Rank}^2_{i,j}$	-	N	N	Y	Y	N	N	Y
$\text{Rank} \mid \text{win/lose}_{i,j}$	N	-	N	N	N	Y	N	N
$\text{Age}_{i,t}$, $\text{Age}^2_{i,t}$	Y	Y	Y	Y	Y	Y	Y	Y
Year_t FE	Y	Y	Y	Y	Y	Y	Y	Y
Competition_j FE	Y	Y	N	Y	Y	Y	N	Y
Firm-$\text{app}_{i,j}$ 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 Wage Effects

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

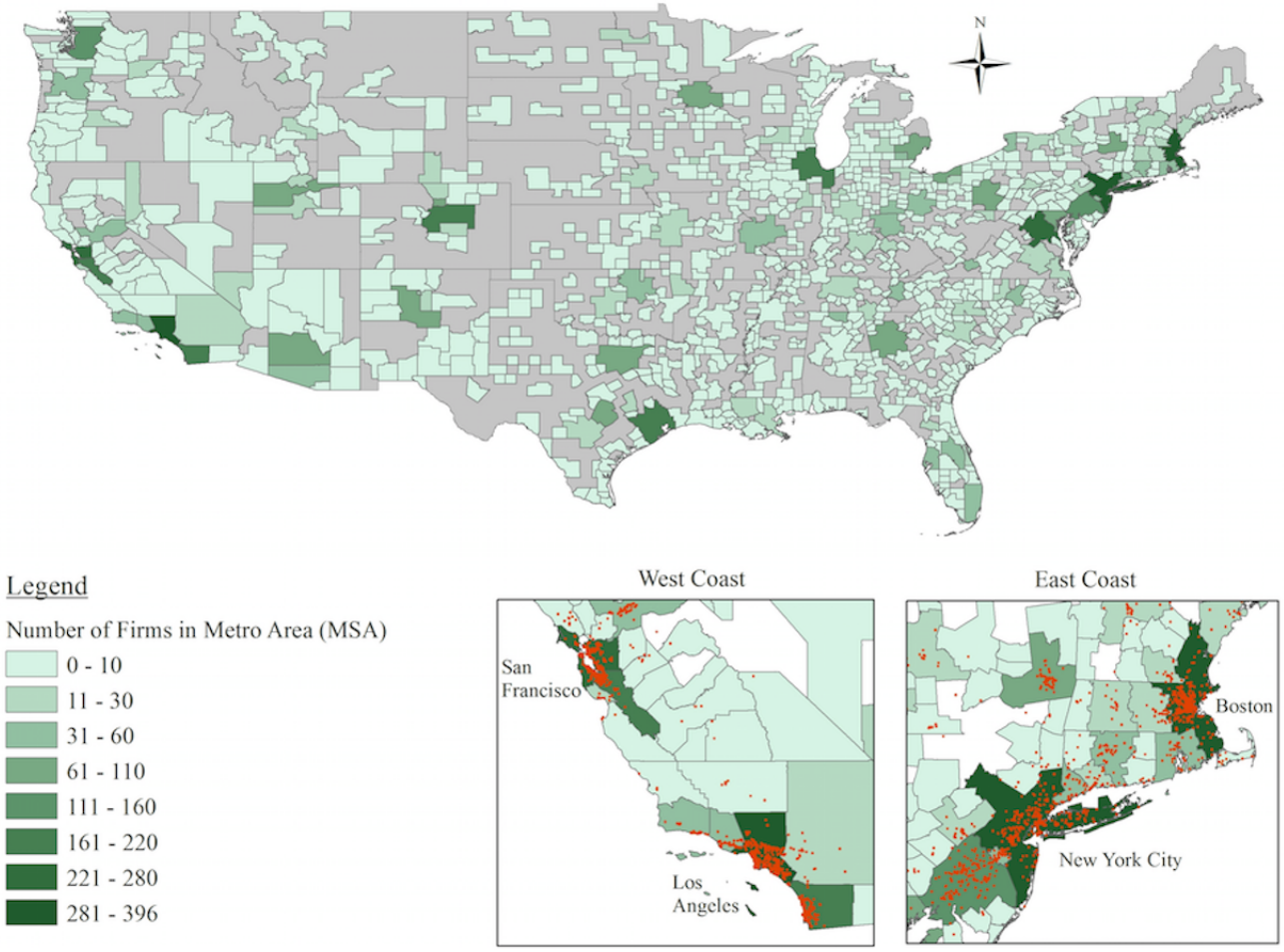
Note: Column 1 decomposes the effect of the award on wages through revenue by instrumenting for revenue with the award. The Cragg-Donald F-statistic is 249 on the first stage, where revenue is regressed on the award. Columns 1-3 are restricted to the two years after the grant application year (including the application year). They interact the effect of the grant with three characteristics on log wage growth, using Equation 1. The base year for the dependent variables 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 Wages Among Incumbent Employees by Firm Size, Age, Growth

Dependent variable:	Log wage				Percent raise in first year of job relative to last year of previous job		
					Incumbent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{PostAward}_{i,j,t} \cdot \text{HighEmp}_{i,t=-1}$	-.178**						
	(.083)						
$\text{PostAward}_{i,j,t} \cdot \text{HighAge}_{i,t=-1}$		-.176***					
		(.032)					
$\text{PostAward}_{i,j,t} \cdot \text{RevGrowth}_{i,t\in-3,-1}$			.104***				
			(.0242)				
$\text{PostAward}_{i,j,t} \cdot \text{HighWage}_{i,t=-1}$				-.25***			
				(.071)			
$\text{PostAward}_{i,j,t}$	.21**	.177***	.0363***	.279***			
	(.0823)	(.028)	(.013)	(.069)			
$\text{Tenure}_{k,t=-1}$					-.025***		
					(.005)		
$\text{HighEmp}_{i,t=-1}$						.062***	
						(.010)	
$\text{HighAge}_{i,t=-1}$							.023*
							(.012)
Controls							
$\text{Post}_{i,j,t}$	Y	Y	Y	Y	N	N	N
$\text{Post}_{i,j,t} \cdot X^{\dagger}$	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

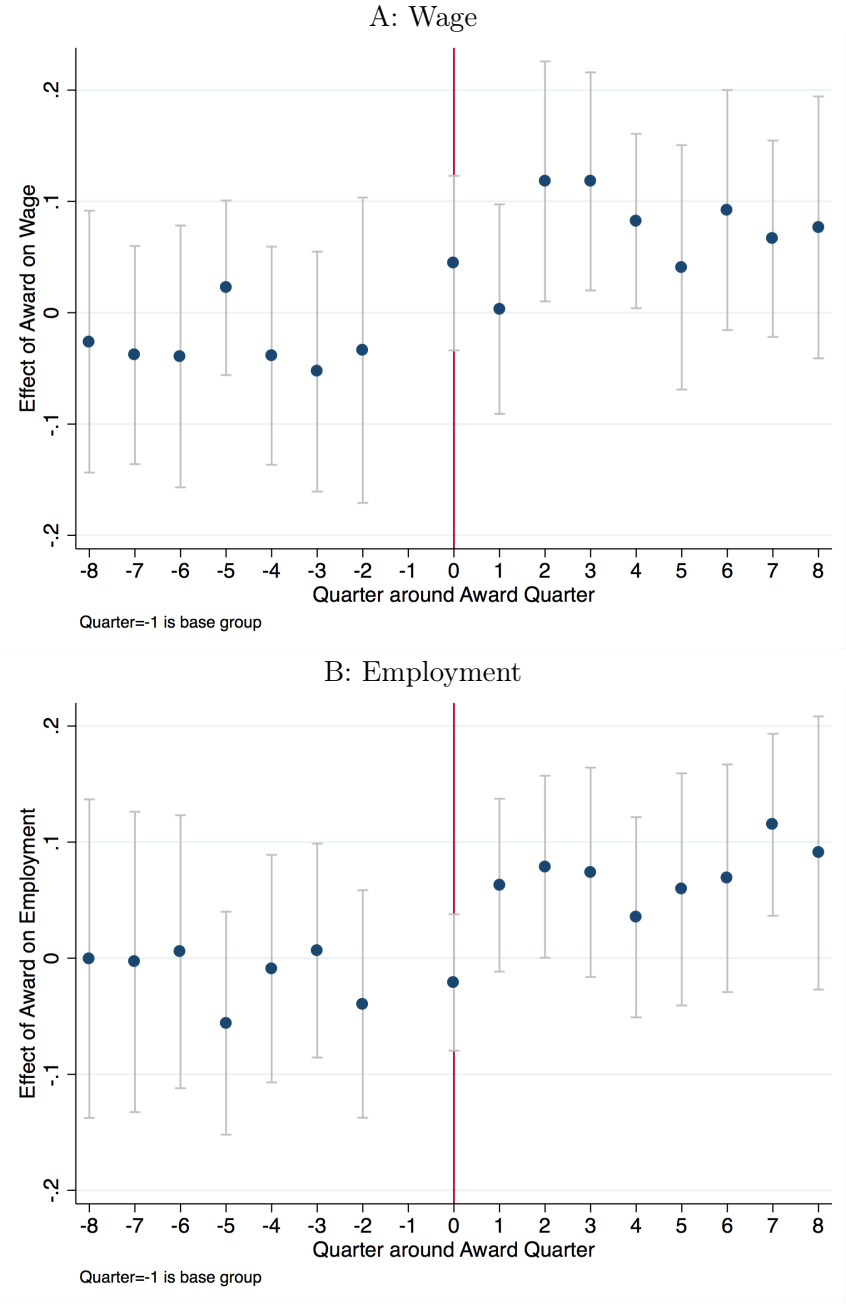
Note: This panel shows the effect of the grant on employee log wages, using Equation 1. The sample is restricted to incumbent workers in columns 1-4 and 7 (those at the firm before the award year). Control coefficients are omitted for space considerations, but are available upon request. $^{\dagger}Post_{i,j,t}$ is interacted with characteristic of interest (e.g. $HighEmp_{i,t=-1}$ in column 1). $PostAward_{i,j,t}$ is not reported in column 3 for disclosure reasons (see text). 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



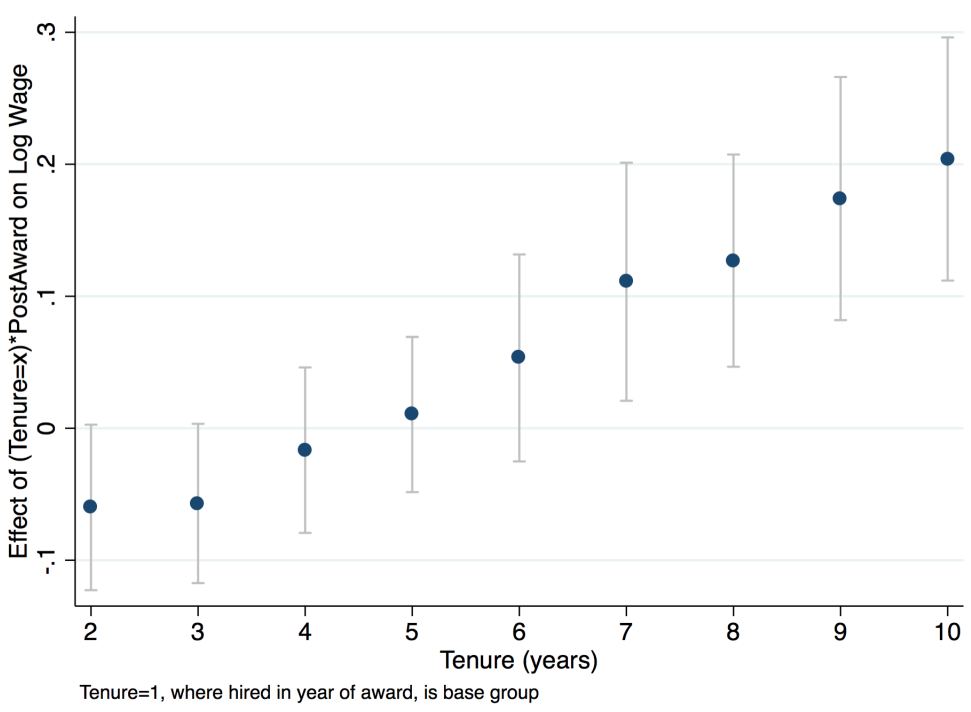
Note: This figure shows the location of all applicant firms in the data. In the main figure, a darker color for a metropolitan statistical area (MSA) indicates higher firm density. In the insets, actual firm locations are overlaid as orange dots.

Figure 2: Effects on quarterly outcomes



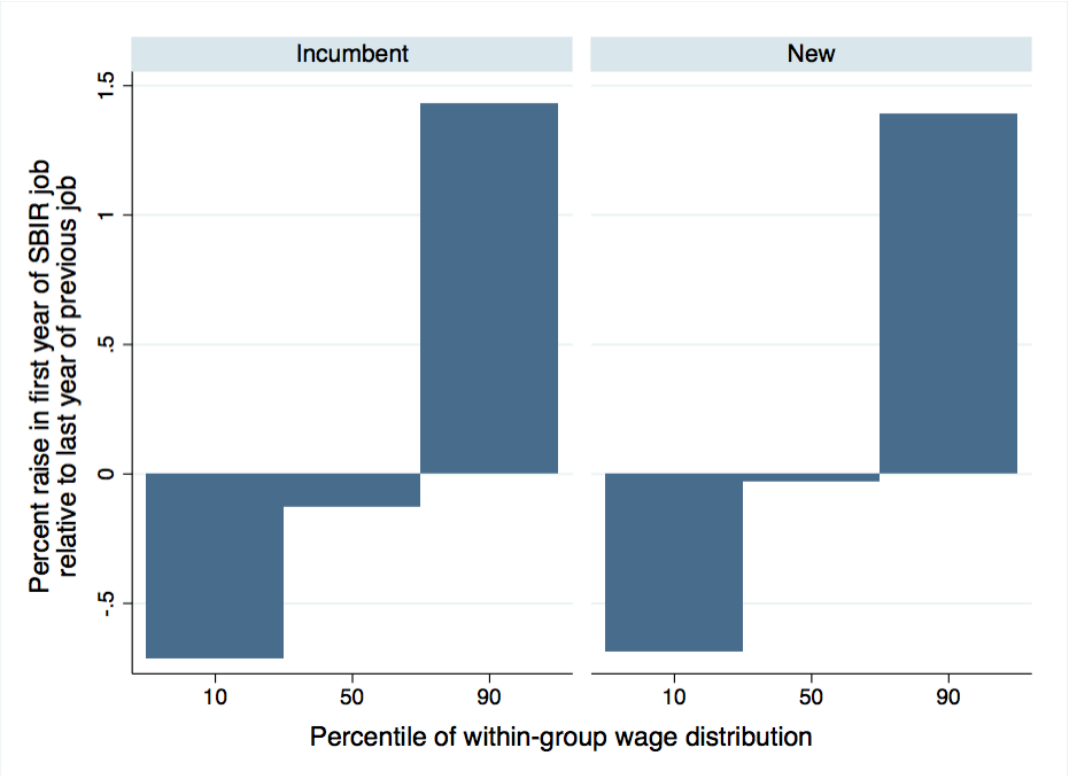
Note: These figures show the results from estimating Equation 3 on quarterly levels of log firm-year employment, payroll, and average wage. Each point is a coefficient on a quarter around the award quarter interacted with winning an award. The base quarter is -1 (immediately before the quarter of award). 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 wage 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: Percent raise between last year of previous job and first year of job at SBIR applicant firm



Note: This figure shows the average percent raise for a given percentile of the distribution of incumbent employees (left side) and new employees (right side). Incumbent employees were present at the firm in the year before the grant award year. The percent raise is calculated as the increase between the last year of the previous job and the first year of the SBIR job. The percentiles on the x-axis are within-group, so for example the first bar on the left shows that there is about a seven percent initial penalty for employees at the 10th percentile of the incumbent employee distribution.

Appendix

(for online publication)

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

Probability in industry (most common 3 digit NAICS)	N	Mean	
Administrative and Support Services	30500	0.013	
Chemical Manufacturing	30500	0.0167	
Computer and Electronic Product Manufacturing	30500	0.079	
Electrical Equipment, Appliance, and Component Manufacturing	30500	0.0324	
Fabricated Metal Product Manufacturing	30500	0.0241	
Machinery Manufacturing	30500	0.0495	
Merchant Wholesalers, Durable Goods	30500	0.0257	
Professional, Scientific, and Technical Services	30500	0.622	
Other firm and employee statistics	N	Mean	Std Dev
Firm 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	

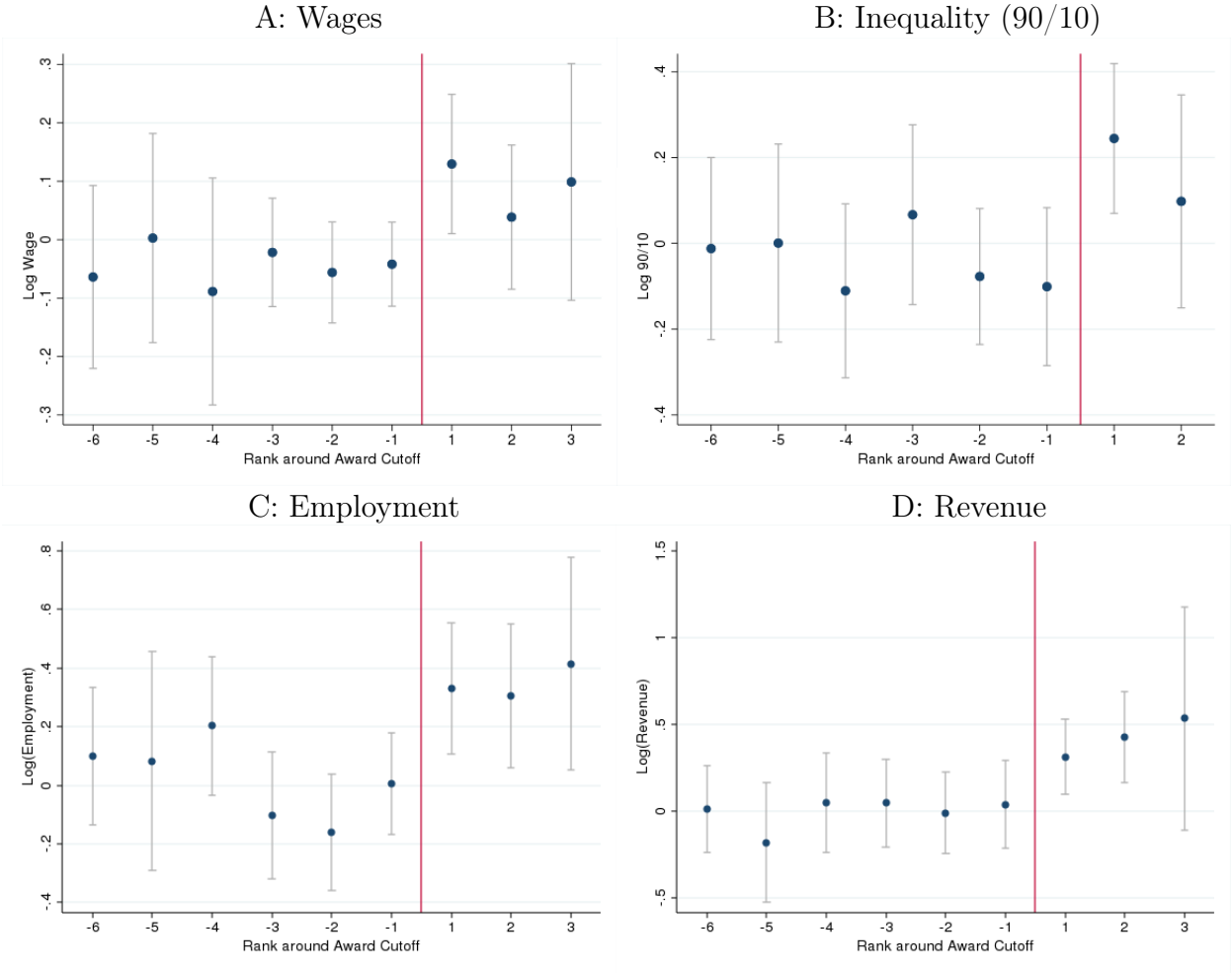
Note: This table shows summary statistics about the SBIR data that were matched to U.S. Census data. The share of firms in the most common eight 3-digit NAICS codes are shown (there are a total of 99 3-digit NAICS). Firms may change NAICS codes across years. Worker-related variables are from linked W-2-Individual 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 Wages

Dependent variable:	Wage growth percentile				Wage level percentile			
	10	50	90	99	10	50	90	99
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PostAward_{i,j,t}$	-0.0941	0.0589	.142**	.151**	0.00296	0.063	.154***	.179***
	(0.0722)	(0.0603)	(0.0618)	(0.0733)	(0.0625)	(0.052)	(0.0521)	(0.0674)
Controls								
$Post_{i,j,t}$	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
Fixed effects								
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 wage growth percentiles (columns 1-4) and percentiles of wage 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. 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.

Figure A.1: Effects around the award cutoff



Note: These figures show the results from estimating Equation 2 on levels of log firm-year wages, 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.