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Do Temporary Demand Shocks Have Long-Term Effects for Startups?

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Using procurement auctions and register data, we find that temporary demand shocks have long-term effects for startups. Startups that win a procurement auction have 20% higher sales and employment and are more profitable than startups that narrowly lose an auction, even several years after the contract work has ended. There are no such effects for mature firms. The effects for startups are large: about 50% of the contract value is transmitted into long-term sales. Our analysis suggests learning-by-doing as a plausible mechanism. Overall, our results point to the importance of path dependence in shaping the long-term outcomes of startups. (*JEL* D24, G39, L11, L25, L26)

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Startups are important for job creation and innovation and there is wide interest in understanding why some young firms become successful and others do not. In this paper, we investigate the effect of temporary demand shocks on long-term startup outcomes. The empirical opportunity comes from procurement auctions in Norway. We compare the long-term sales and employment of startups that win an auction with the long-term sales and employment of runner-up startups. That is, we compare long-term outcomes for startups that received a temporary demand shock with startups that almost did.

To motivate why temporary demand shocks could have long-term effects for young firms, consider the story of Microsoft. In 1975, the year when Microsoft

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was founded, Paul Allen read an article about the Altair 8800 microcomputer and suggested to Bill Gates that they could program a BASIC interpreter for the device. Gates called Altair and claimed to have a working interpreter, something they did not have at that time. Over the next few weeks, Allen and Gates worked frenetically to produce an interpreter, achieved it, and to their surprise, it worked flawlessly when they demonstrated it to Altair, which bought it. This early demand shock has often been viewed as instrumental to the subsequent phenomenal growth of Microsoft.[1]

We use extraordinarily detailed data from Norway. Register data provide detailed accounting, ownership, and employment information on all Norwegian firms at the yearly level. These data allow us to identify startups, as opposed to spinoffs from established companies, based on firm age and subsidiary status. The Norwegian Public Roads Administration (hereafter Public Roads), the government agency responsible for road construction in Norway, provided us with data on all procurement auctions run between 2003 and 2015. The typical auction involves a road-related procurement, but the auctions may concern a variety of other needs, from consulting reports to office supplies. For each auction, the Public Roads data describe the product or service procured, the contract duration (typically less than a year), and, for each bid, the identity of the bidder and the bid size. The average bid by a startup is about 25% of sales in the prior year.

Our empirical strategy is to compare long-term outcomes for startups that win a procurement auction to long-term outcomes for startups that narrowly lose, using a difference-in-differences setup. The main result can be summarized as follows: even several years after the contract work has been completed, the startups that won are substantially larger in terms of sales and employment, and are more profitable, than the startups that came second. The effects are large; about 50% of the contract value is transmitted into an increase in long-term sales.

A main threat to identification could be differences between winner and runner-up startups not captured by the difference-in-differences model. It is possible that winners would follow a different trend than runners-up after the auction, even if they had not won. That winners and runners-up are balanced on observable characteristics before the auction alleviates some concern, and to accommodate unobserved heterogeneity, we include firm fixed effects. We also perform two placebo tests. First, we compare the trends of winners and runners-up before the auction and show that they are the same. Second, we compare runners-up with startups that end up in *third* place. Post-auction differences between these two groups cannot be due to winning an auction, as neither win. We show that these two groups of startups are balanced on trends after the auction, which suggests that the difference in bids between

[1] The story of Microsoft's beginnings has been told and retold many times. See, for example, Isaachson (2013).

these two groups do not translate into noticeable post-auction differences. To further explore the role of unobserved differences between winners and runners-up, in Section 5 we engage in several additional analyses, including a "timing-of-event specification" (e.g., Abraham and Sun 2021) that compares current startup winners to startups that win in some future year; a within-auction specification that compares winners and losers from the exact same auction; and a regression discontinuity design which compares winners and losers in narrowly-won auctions. These additional analyses yield very similar results as the main specification, and suggest that unobserved differences between winners and runners-up do not drive the results.

Several theoretical perspectives are consistent with long-term effects of auction wins; learning-by-doing, sunk costs, financial constraints, and reputational effects. Auxiliary evidence suggests that learning-by-doing (e.g., Arrow 1962; Thompson 2012) plays an important role. First, winners expand "vertically" by becoming more likely than runners-up to participate in subsequent larger auctions and against larger competitors. In addition, winners expand "horizontally" by increasing their participation in auctions for new products. They also hire managers of higher quality. Second, the learning-by-doing hypothesis, as formulated by Arrow (1962), posits that learning-by-doing has decreasing returns. In other words, the more accumulated experience, the less learning at the margin. Consistent with this idea, we find no long-run effects of auction wins for mature firms. Perhaps most compellingly, analyzing all the auction bids in our data, we find that startups bid more aggressively than mature firms, even when controlling for auction fixed effects. This suggests that startups are aware of the future learning benefits of winning and adjust their bids accordingly.

Other explanations for the long-run effects than learning-by-doing appear to receive less support by the data. One explanation is that winners make contract period (sunk) investments that permanently reduce their marginal cost. We find that winners increase their tangible assets only *after* the contract work ends, which suggests that investments are a consequence, rather than a cause, of growth. Still, we acknowledge that the apparent lack of investment response during the contract period could be because investment is difficult to measure with accounting data. Another explanation is that the contract work was delayed and spilled into the post-contract period, contaminating the long-run estimates. In a secondary analysis, we collect and analyze data on actual time use for a large subset of the procurement contracts. Although contract delays are not uncommon, the mean and median delays are only 1.5 and 0 months, respectively. This is much shorter than would be required to affect the long-run estimates. Section 7 discusses mechanisms further.

A central question in corporate finance is to what extent financial constraints impede the growth of firms. Recent work by Howell (2017) finds positive effects of receiving a government grant on technology startups' propensity to patent and become venture capital financed, which suggests that the startups were initially financially constrained (see also Schmalz, Sraer, and Thesmar 2017). In our setting, the role of financial constraints appears more nuanced. On the one hand, we find that winning startups do not increase their levels of tangible assets during the contract period; instead, they increase their use of intermediate inputs (i.e., purchases from suppliers) to ramp up production, consistent with short-run financial constraints. On the other hand, we find that the magnitude of the long-run effects of auction wins on firm size do not vary with conventional measures of financial constraints, such as the cash-to-assets ratio, prior dividend payments, or firm size.[2] In other words, while financial constraints may impede short-term investment following a demand shock, they do not appear to impede longer-term firm growth.

The main empirical results suggest that temporary demand shocks have long-term effects for startups. The results inform a large entrepreneurial finance literature that explores factors (e.g., founder ability, access to finance, legislative framework) that explain why some startups succeed and others do not.[3] In contrast to much of the existing literature, our statements are causal, in that we exploit exogenous variation in temporary demand at the firm level. Farre-Mensa, Hedge, and Ljunqvist (2020) find that, for a sample of technology startups, obtaining an early-life patent positively affects a startup's long-term sales and employment growth. A patent award could be interpreted as a long-term shock to demand, as patents partially or fully protect inventions from competition for up to 20 years. In contrast, we study the long-run effects of a temporary demand shock (the typical procurement contract in our setting lasts less than 12 months).[4]

More broadly, our work contributes to empirical research that explores path dependence in a variety of settings. For example, Aghion et al. (2016) demonstrate path dependence in the type of research that R&D-intensive firms pursue. Other contexts include economic geography (Bleakley and Lin 2012; Glaeser, Kerr, and Kerr 2015), industrial organization (David 1985), corporate governance (Bebchuk and Roe 1999), labor markets (Oyer 2008; Schoar and Zuo 2017), financial decisions (Anagol, Balasubramaniam, and Ramadorai 2021; Malmendier and Nagel 2011), and central banker decisions

[2] As pointed out by Farre-Mensa and Ljungqvist (2015), these and other measures of financial constraints may poorly reflect true financial constraints.

[3] This literature is too large to list exhaustively. A few recent examples are Foster, Haltiwanger, and Syverson (2016) on demand accumulation, Gompers et al. (2010) on ability differences and performance persistence for serial entrepreneurs, Hall and Woodward (2010) on venture capital funding, Hvide and Jones (2018) on legislative framework, Hvide and Oyer (2018) on within-family transfers of human capital, Kerr and Nanda (2009) on financial constraints, Kerr, Lerner, and Schoar (2014) on business angels, Lerner and Malmendier (2013) on peer learning, and Levine and Rubinstein (2016) on founder characteristics.

[4] A related body of academic work analyzes the effects of various exogenous shocks on the propensity to start a business. For example, Adelino, Ma, and Robinson (2017) use regional variation in exposure to nationwide manufacturing shocks; Bernstein et al. (2022) use regional variation based on commodity price shocks; and Hombert et al. (2019) use the rollout of insurance for self-employed people in France. In contrast, we study how existing firms respond to demand shocks and show that temporary shocks have long-term effects on firm outcomes.

(Malmendier, Nagel, and Yan 2021). To our knowledge, we are the first to document path dependence for startups in a setting with exogenous variation in initial economic conditions.

Ferraz et al. (2016) and Lee (2017) also use procurement data to study firm outcomes following an auction win. Neither focuses on startups, and neither has access to information on subsidiary status, which is needed to identify startups as opposed to spinoffs. Using data from Brazil, Ferraz et al. (2016) document positive effects of auction wins on firm growth, which are temporary and die out after 2 to 3 years (their figure 4). Compared to the contracts in our paper, which on average are worth NOK 11 million, Ferraz et al. (2016) analyze much smaller contracts; their table 1 reports an average contract value of 10,314 Brazilian real ($\approx$ NOK 22,500), which could explain why the effects in their paper are transitory. For the smallest contracts, we do not detect long-run effects (Figure 3). Lee (2017) considers procurement auctions in South Korea with the unusual feature that the winning firm is the lowest bid subject to bidding above a random cutoff that is unknown to bidders ex ante. Lee (2017) compares the auction winner to a broad sample of nonwinners and finds long-run positive effects on revenue growth and, in some specifications, also on employment growth. Lee (2017) does not present evidence on the growth rates of winners and losers in the years before auctions, which makes it difficult to assess the validity of the empirical design.[5]

Our work also relates to the macroeconomic literature. Haltiwanger, Jarmin, and Miranda (2013) show that young firms have a disproportionate share in job creation. Moreira (2016) and Sedlacek and Sterk (2017) show that firms born in cohorts with weak job creation are persistently smaller on average, even when the aggregate economy recovers. As both demand-side and supply-side factors vary with the business cycle, it is difficult to establish what drives these cohort effects based only on aggregate data. With the important caveat that extrapolating from micro to macro is difficult due to general equilibrium effects, our findings suggest that the cohort effects in startup job creation documented in the macroeconomic literature could be due to the demand component of business cycle variations.

Finally, our work may interest policy makers. Many governments, including in the United States and in the United Kingdom, employ policies that facilitate the participation of startups and small firms in procurement auctions as a means to enhance startup creation and growth. For example, the U.S. government limits competition for certain procurement contracts (e.g., those with an estimated value below \$150,000) to small businesses, a policy intended to help small

[5] Lee (2017) does not analyze the long-run effects of auction wins on startup outcomes. Table VIII in Lee (2017) interacts an auction win dummy with a linear firm age term and, using data from 1 year after the auction, documents a negative correlation between firm age and the short-run effect of auction wins.

businesses compete for and win federal contracts.[6] The empirical results of our paper provide a possible rationale for expanding such policies: in terms of job creation and sales growth, winning a procurement auction seems to have much larger effects for young firms than for mature firms.

1. Institutional Background

Here we provide context on Public Roads procurement auctions. In Section 1.1, we provide a brief overview of Norway and its government procurement. In Section 1.2, we provide institutional details on Public Roads and its procurement process.

1.1 Norway

Norway is an industrialized country with a population of approximately five million. In 2017, Norway ranked among the ten richest countries in the world, as measured by purchasing power adjusted GDP per capita (World Bank 2017). Norway has a well-educated population with a large middle class, low income inequality, and low wealth inequality. The country has a large public sector that is primarily financed by high levels of taxation and oil revenue. Norway ranks 8 of 190 on the World Bank ease-of-doing business index. The public sector in Norway consistently ranks among the ten least-corrupt in the world, according to the Corruption Perception Index published by Transparency International.

Government procurement accounted for about 15% of GDP in Norway in 2015 (Statistics Norway 2015). This is close to the OECD average of 12% (OECD 2017). Norwegian procurement is regulated by the Public Procurement Act, which aims to promote transparency and efficiency. The Public Procurement Act provides instructions on, among other things, whether, how, and when government procurers should announce their demands to the public; how to organize procurement auctions; and which restrictions should be imposed on suppliers in terms of minimum safety requirements, environmental impact, and fair payment to subcontractors. In Internet Appendix Section A, we describe these rules and regulations in more detail.

1.2 Public Roads

Public Roads (*Statens Vegvesen* in Norwegian) is a government agency established in 1884 to build and maintain roads all over the country. Its current assignments include, among others, the planning, construction, and operation of road networks; driver training and licensing; vehicle inspection; and subsidies to car ferries. Public Roads is led by the Directorate of Public Roads, a subsidiary

[6] See U.S. Small Business Administration (2020). In the United Kingdom, a major policy reform facilitated small firm participation in government procurement contracts; measures taken included abolishing prequalification for all small contracts (see, e.g., Young 2015). The Australian government recently capped the maximum value of IT contracts to facilitate participation by small firms, see Bailey (2017).

of the Ministry of Transport and Communications. Administratively, Public Roads is divided into five regions — Northern, Central, Western, Southern, and Eastern — and 30 districts. In 2015, Public Roads had 7,585 employees and, with a procurement volume exceeding NOK 40 billion (US$4.5 billion), accounted for about 10% of all government procurement in Norway. Internet Appendix Section A.2 provides more details on the Public Roads procurement process.

2. Data

Here we provide details on the data sources used in the empirical analysis.

2.1 Register data

We use an extended version of the register data in Hvide and Jones (2018), who provide detail on the data. We use accounting information from the Dun and Bradstreet database, which contains annual financial statements submitted to the Norwegian tax authorities. These data include variables such as five-digit industry code, sales, assets, number of employees, and firm profits for the years 1992–2016.[7] In Norway, the fiscal year coincides with the calendar year. Accordingly, all firm outcomes are measured at end-of-year. We supplement these data with incorporation documents submitted by new firms to the government agency Brønnøysundregisteret. This register includes the startup year, capitalization, and the personal identification number and ownership share of all initial owners with at least 10% ownership stake.

We also use data on individuals from 1993 to 2015, prepared by Statistics Norway. These records are based on government register data and tax statements, and include anonymized personal identification numbers and yearly sociodemographic variables, such as gender, age, education in years, taxable wealth, and income. The data contain all Norwegian individuals, not a sample, as in the Panel Study of Income Dynamics (PSID) or the Survey of Consumer Finance (SCF). As with the PSID and the SCF, the data are anonymized (i.e., they contain no names of individuals). Individuals in these data can be linked to their employers via firm identifiers, which allows us to identify all the workers of the startups in the sample.

Based on the register data described above, we define a startup as a firm that is not a subsidiary at birth and is younger than 10 years. The definition of a startup as being younger than 10 years old is to a large extent arbitrary. While a 4-year-old firm is clearly still a startup, a 9-year-old firm is likely to have reached a learning curve plateau. To deal with this somewhat arbitrary definition of a startup, in Section 4, we present results for a narrower definition

[7] The Dun and Bradstreet database contains yearly information on all Norwegian incorporated limited liability companies, not just a sample (as in the U.S. equivalent). Incorporated companies are required by law to have an external auditor who certifies the accounting statements in the annual reports.

of startups; those that are less than 5 years old. To exclude a relatively small number of very large young firms from the startup sample, we define startups as having less than NOK 16 million ($\approx$ US$2 million) in total assets in the first and second years of operation. Our results are not sensitive to using a higher total asset threshold, such as NOK 30 million.

2.2 Procurement-level data

We obtained access from Public Roads to transcripts of all their procurement auctions in the 2003–2015 period. For each auction, the transcripts provide the names of all bidders and the size of their bids, as well as the name of the winning firm. The transcripts also provide details on whether the auction was decided by price only or included other winning criteria, such as perceived product quality, and whether or not Public Roads limited competition by restricting the set of eligible bidders. Internet Appendix Section A.2 gives summary statistics for the transcripts.

We are primarily interested in competitive procurement auctions where at least one startup participates, and where price is the only winning criterion. Moreover, we focus on auctions that were open to all interested bidders and had more than one bidder, and exclude auctions that do not provide data on the bids and names of all bidders. We are left with about 1,200 auctions, as described in more detail in Internet Appendix Section A.2. The main estimation sample is based on the subset of these 1,200 auctions where a startup either wins for the first time (treated group) or places a runner-up bid without ever before having won an auction (control group), in total about 400 auctions. In placebo tests and extensions of the main analysis, we construct broader estimation samples. For example, in Figure 4, we estimate the effects of auction wins for both startups and mature firms using data from about 4,000 auctions.

The Public Roads transcripts provide a detailed description of the exact product or service being procured (e.g., "Building a 200-meter bicycle path in Oslo"), which we use to classify all procurements into 54 categories. Figure 1 summarizes the 25 most-procured categories for the 1,200 auctions where at least one startup participates (the summary statistics are very similar when requiring that a startup is either the winner or runner-up; these results are not reported). The figure shows considerable variation in procurement categories, ranging from road work, to consulting jobs, to high-skilled civil engineering jobs, such as geotechnical surveying. Internet Appendix Section A.2 presents alternative breakdowns of the data.

We merge the procurement data and register data by firm name, a variable that is present in both data sets. Internet Appendix Section A.2 provides details on the name matching procedure. We find register data matches for about 90% of the bids in the procurement data.

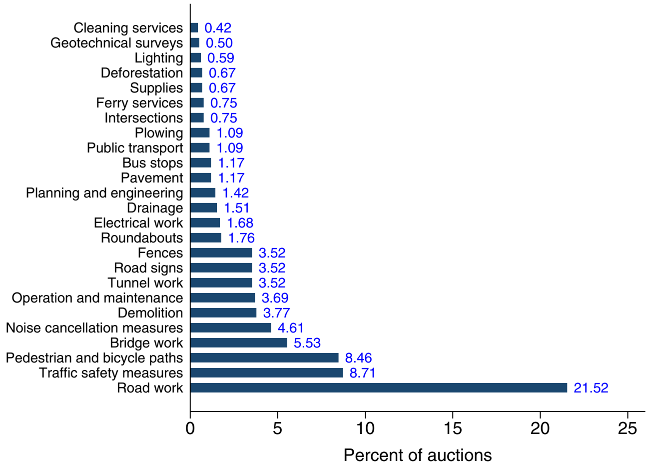

Figure 1
Public Roads' most-procured products and services
The figure summarizes Public Roads' top-25 most-procured products and services. The statistics are calculated based on the sample of 1,207 price-only procurement auctions in which at least one startup places a bid (see Section 2.2 for details on this sample).

3. Empirical Methodology

We aim to estimate the causal long-run effect of temporary demand shocks on startup outcomes. The identification strategy involves comparing startups that win a procurement auction with startups that place bids but come second. The unit of analysis is startup-by-auction, indexed by j. For example, a startup that participates in auctions in 2003 and 2009 is represented in the sample with two time-series, one centered on 2003 and one on 2009, each time assigned a different j. We estimate the following difference-in-differences model:

$$y_{je} = \theta b_{je} + \kappa_t + \alpha_e + \lambda_j + \varepsilon_{je}, \tag{1}$$

where y_{je} is the outcome (e.g., log sales) for startup-by-auction j in event-time e centered on the auction year, and κ_t, α_e, and λ_j are calendar year, event-time, and firm-by-auction fixed effects, respectively. The treatment indicator, b_{je}, is defined by the interaction $Treated_j \times Post_e$, where $Treated_j = 1$ for startups that win an auction and $Treated_j = 0$ for startups that do not win; $Post_e = 1$ in all years after the auction and zero before. Throughout the analysis, we cluster standard errors at the firm level, and estimate Equation (1) using 5 years of data on each side of the auction year. The results are robust to using both shorter and longer estimation windows. The parameter of interest is θ, which captures the within-startup change in y_{je} from before to after an auction win, net of the time trend for startups that placed bids but did not win. The parameter θ has a causal interpretation if winners and runners-up would follow the same trend in y_{je} absent the auction participation — that is, the common trends assumption.[8]

To increase the plausibility of the common trends assumption, we make two sample restrictions. First, we include only runner-up startups in the control group. The idea is that unobserved quality differences are likely to be smaller — and counterfactual trends more comparable — between winner and runner-up startups than between winners and all auction losers. Second, we restrict the treated group to first-time winners, and the control group to runners-up that have never before won an auction. For example, if a startup places a runner-up bid in 2004 and winner bids in both 2007 and 2009, we include two time-series for this startup; one centered on 2004 (control group) and one centered on 2007 (treated group), each time assigning a different startup-by-auction identifier, j. This restriction addresses a perhaps more subtle concern: if there are gradual effects of auction wins, previous winners would follow different trends than their nonwinner controls, thus violating the common trends assumption.[9]

In panel A of Figure 2, we assess the plausibility of the common trends assumption by comparing trends and levels of key firm characteristics between winner and runner-up startups in the years before their focal auction. We find no statistically significant differences in trends between the treated and control group before the auction. While the identifying assumption of the difference-in-differences model only requires the balance of trends between treated and control, we find it reassuring that also the levels of firm characteristics are balanced between winner and runner-up startups. Panels C and D of Figure 2 present difference-in-differences effects of auction wins on log value added and employment, estimated before the focal auction (the figure also presents post-auction estimates, which we discuss in Section 4). We find no placebo effects of auction wins on value added and employment before the auction.

That first-time winners and runners-up are balanced in terms of pre-auction trends does not guarantee the validity of the empirical design; being a winner could be correlated with post-auction shocks to firm size, unrelated to the auction. In Section 5, we consider various extensions to address unobserved heterogeneity between winners and runners-up. For example, we estimate a "timing-of-event" model that compares winners to later-winners; a within-auction model that compares winners to runners-up from the same auction; and a regression discontinuity that compares winners to losers in narrowly won

[8] A small number of startups participate in multiple auctions within the same calendar year. For startups that participate in multiple auctions within the same year, we include a single time-series surrounding the auction year, and set $Treated_j = 1$ for the startups that win at least one auction that year, and $Treated_j = 0$ for the startups that come in second at least once but win zero auctions.

[9] Suppose that a firm wins one auction in 2002 and another in 2004, and that the effect on sales of each win is positive and increasing over time. In this simple scenario, the winner in 2004 would be on a positive sales trend due to the 2002 win, while the control group, comprising runners-up without prior wins, would experience no such trends. This violates the common trends assumption of the difference-in-differences model.

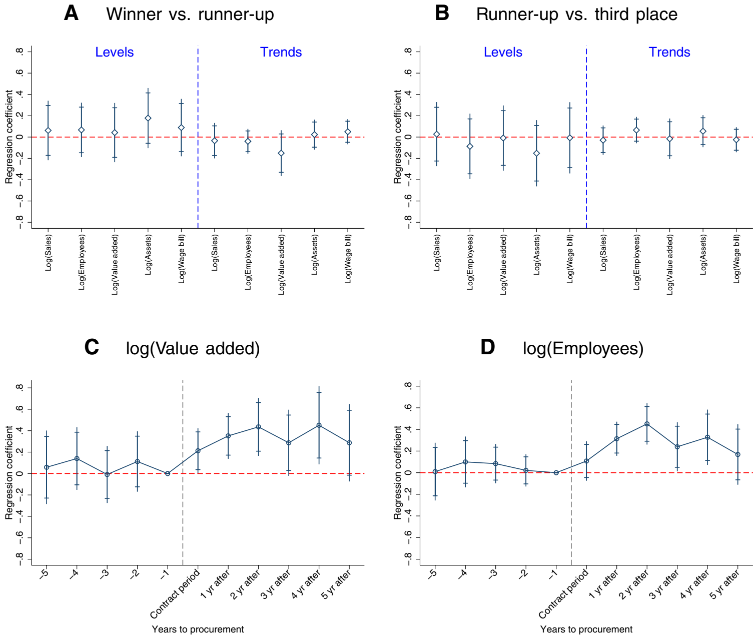

Figure 2
Assessing the identifying assumption
Panel A depicts balance tests between auction winners and runners-up (i.e., the main treated and control groups defined in Section 3), and panel B depicts balance tests between runners-up and third-placed startups. The balance tests are based on estimates from separate regressions where a preauction firm characteristic, measured in either levels or trends, is regressed on a dummy for whether the firm wins an auction in event-time zero (the focal auction year). When the outcome is measured in levels, we consider all firm observations from event-time -5 to -1, and we include event-time and calendar time fixed effects in the regressions. Trends are measured as the change in a given firm characteristic between event-time -2 and -1. Panels C and D report event-time specific difference-in-differences estimates of the effect of auction wins on log value added and employment. Event-time takes on negative values in years before the auction and, in the post-auction period, event-time is measured relative to the end of the contract period. For all control group firms, we impute a contract duration of 12 months, and we include firm age fixed effects in the difference-in-differences estimation. In all panels, standard errors are clustered at the firm level, and we include 95% (outer notches) and 90% (inner notches) confidence intervals around the coefficient estimates.

auctions. The estimates from these models are very similar to those obtained from the main specification in Equation (1).

Table 1 provides summary statistics from the main estimation sample. The observations in Table 1 are at the startup-by-auction level, and startup characteristics are measured in the year before the startup wins an auction for the first time (treated) or comes second without previously having won (control). The estimation sample has 361 startup-by-auction units, corresponding to 313 unique startups.[10] The average startup is 3.87 years old, and the oldest is 8. The median startup has 7 employees; the average startup has 17 employees. On

[10] In the year of their focal auction, the 361 startup-by-auction units in Table 1 become winners or runners-up a total of 410 times. In other words, some treated units win multiple auctions within the focal auction year, and some

Table 1
Summary statistics: Main estimation sample

	Mean	SD	Min.	p(25)	Median	p(75)	Max.	*N*
A: Firm characteristics in year before auction								
Firm age	3.87	2.79	0.00	1.00	4.00	6.00	8.00	361
Employees	17.22	47.17	0.00	3.00	7.00	18.00	669.00	361
Sales	24.18	37.80	0.00	5.30	13.32	27.19	394.04	361
Total wage bill	7.28	18.19	0.00	1.17	3.60	7.47	175.14	361
Intermediate inputs	11.84	21.38	0.00	1.15	4.67	14.07	207.78	361
Long-term tangible assets	4.02	37.67	0.00	0.24	0.84	2.40	713.66	361
Total assets	13.42	46.19	0.00	2.70	6.29	12.58	814.56	361
B: Auction characteristics								
Focal auction year	2,009.50	3.18	2,003.00	2,008.00	2,009.00	2,012.00	2,015.00	361
Estimated contract value	12.20	48.66	0.10	1.91	4.97	10.49	815.90	307
Number of bidders	3.89	1.81	2.00	3.00	3.00	5.00	16.00	361
Bid size	9.56	39.62	0.00	1.54	3.67	7.91	718.88	361
Bid size, if winner	10.99	51.72	0.00	1.25	3.53	7.86	718.88	206
Bid/Estimated contract value	0.97	0.40	0.00	0.77	0.92	1.09	3.15	307
Winner	0.57	0.50	0.00	0.00	1.00	1.00	1.00	361
Contract duration, if winner	14.25	16.12	0.00	5.00	10.00	13.20	74.00	206
Auctions in focal auction year	1.31	0.79	1.00	1.00	1.00	1.00	7.00	361

The table reports summary statistics from the main estimation sample, using data from the year before the focal auction. For firms that start up in the year of the focal auction, we use data from the focal auction year. The data are at the startup-in-auction level, and there are 361 startup-in-auction units. Panel A summarizes firm characteristics. Sales, wage bill, intermediate inputs, long-term tangible assets, and total assets are all expressed in millions of NOK. Panel B summarizes the focal auctions. All measures of estimated contract values and bid sizes are expressed in millions of NOK, and the contract duration is expressed in months. The estimated contract value is Public Road's preauction valuation of the contract (see Internet Appendix Section A for details on this variable). If a startup competes in multiple auctions within the focal auction year, all auction characteristics except the Winner indicator are calculated as the average across the startup's auctions. For a given startup-in-auction, the Winner indicator equals one if the startup wins at least one auction in the focal auction year and zero if the startup places at least one runner-up bid but wins zero auctions.

average, startups sell for NOK 24 million and have NOK 13.4 million in assets, with median sales and assets of NOK 13.3 million and 6.3 million. Startups participate on average in 1.3 auctions in the focal auction year and win an average of 0.57 contracts. The average winning bid size is NOK 11 million; the median is NOK 3.63 million.

4. Main Results

Table 2 presents the estimated effects of procurement auction wins on startup outcomes, both during the contract period and after the contract period has ended. In the contract period, we find statistically and economically significant effects on sales, value added, employment, and intermediate inputs (which includes raw materials and subcontracting costs). These results are, to a large extent, mechanical and not surprising. The main results are presented in the second row of Table 2; the post-contract coefficients suggest that winners

control units come second multiple times. For these units, the auction characteristics in Table 1 are calculated as the average across the unit's auctions. The 361 startup-by-auction year units occupy *both* the winner and runner-up placements in 18 of these 410 auctions. In the "within-auction" model in Internet Appendix Section H.2, we include a separate time-series surrounding each auction a startup participates in.

Table 2
Main results

	log(Sales)	log(VA)	log(Emp.)	Profits	Active	log(WB)	log(I)	log(TA)
θ (Contract)	0.25***	0.19**	0.14*	582.85**	−0.00	0.14*	0.34***	0.13
	(2.81)	(2.11)	(1.95)	(2.03)	(−0.05)	(1.88)	(3.04)	(1.12)
θ (Post-contract)	0.24**	0.19*	0.22***	813.44**	−0.01	0.23***	0.22**	0.38***
	(2.34)	(1.95)	(2.99)	(2.22)	(−0.18)	(2.91)	(2.01)	(2.74)
Adj R^2	.76	.77	.82	.63	.34	.81	.79	.74
N	2,945	2,926	2,776	2,946	3,761	2,843	2,848	2,783

The table reports estimates of θ from the regression: $y_{je} = \theta b_{je} + \kappa_t + \alpha_e + \lambda_j + \varepsilon_{je}$, where y_{je} is the outcome for firm-by-auction unit j in event-time e centered on its auction; b_{je} equals one for auction winners in the post-auction period, and zero otherwise; and κ_t, α_e, and λ_j are calendar-time, event-time, and firm-by-auction fixed effects, respectively. The estimation sample is defined in Section 3. The coefficient θ is allowed to differ in the contract and post-contract periods. VA, Emp., WB, I, and TA are value added, employment, wage bill, intermediate inputs, and long-term tangible assets, respectively. Active is an indicator for whether the firm is active. Profits are winsorized at the top and bottom 2.5%. Standard errors are clustered at the firm level. t-statistics in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

have 24% higher sales and 22% more employees than runners-up, even after the contract work has ended. We also find an increase in profitability that is statistically significant at the 5% level. In column 5, we find no difference in the post-contract period survival rates of winners and runners-up.[11] In column 6, we show that the percentage increase in the total wage bill (which includes executive compensation) is of similar magnitude to the increase in employment. In column 7, we find a post-contract period increase in intermediate inputs of 22%. Finally, in column 8, we find a post-contract increase in long-term tangible assets (e.g., machinery) of 38%.[12]

The effects in Table 2 are economically highly significant. For example, the elasticity of the long-run increase in sales to the contract period increase in sales is about 0.96, which suggests that almost the entire temporary effect remains in the long run. Another way to assess economic magnitudes is to compare the long-run increase in sales to the NOK value of the procurement contract. A back-of-the-envelope calculation shows that about 50% of the contract value is transmitted into a long-run effect on sales.[13]

Table 2 reports average effects over the post-contract period, which could disguise long-run convergence between winners and runners-up. In Internet Appendix Figure IA.4, we plot raw means of log value added and employment

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11 A firm is inactive in a given year if it is not reported in the Dun and Bradstreet database of accounting figures (see Section 2 for a description of this database). Also, a firm is inactive if both (1) sales are zero and (2) sales remain zero until the firm drops out of the Dun and Bradstreet database. In the latter case, firms are defined as inactive from the first year with zero sales.

12 By long-term tangible assets we mean the balance sheet item property, plant, and equipment (PP&E). Typical assets included in this balance sheet item are land, buildings, machinery, equipment, vehicles, office equipment, etc., which the business uses for production and that have a life of more than 3 years. Financial assets, such as stocks, bonds, and investments in other companies, are not included.

13 The calculation is based on the long-term effect of the auction win on log sales (24%, Table 2), the preauction mean sales millions (NOK 24 million, Table 1), and the mean size of the procurement contract (NOK 11 million, Table 1). A 24% long-term increase in sales from a base of 24 million corresponds to an increase of 5.76 million, or about 50% of the initial contract value.

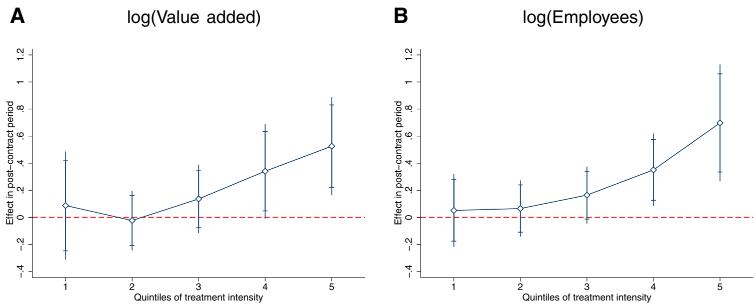

Figure 3
Effect on value added and employment by treatment intensity
The figure reports estimates of the post-contract treatment effect of procurement auction wins on log(Value added) and log(Employees). The post-contract treatment effect is estimated for quintiles of treatment intensity using the following regression specification: $y_{je} = \theta b_{je} \times \text{Quint}_j + \kappa_t + \alpha_e + \lambda_j + \varepsilon_{je}$, where y_{je} is the outcome for firm-by-auction j in event-time e centered on its auction; Quint_j is a vector of indicators for treatment intensity quintile; b_{je} equals 1 for auction winners in the post-auction period, 0 otherwise; and κ_t, α_e, and λ_j are calendar-time, event-time, and firm-by-auction fixed effects, respectively. The estimation sample is described in Section 3. Treatment intensity is measured as the total auction winnings in event-time 0 divided by sales in event-time −1. The coefficient θ is allowed to differ in the procurement contract period and post-contract period, and we only report the estimates of θ from the post-contract period. Standard errors are clustered at the firm level. The figure includes 95% (outer notches) and 90% (inner notches) confidence intervals around the coefficient estimates.

for winners and runners-up over event-time, where event-time is defined relative to the end of the contract duration (e.g., "2yr after" means 2 years after contract completion).[14] In panels C and D of Figure 2, we plot event-time-specific differences between winners and runners-up with corresponding 90% and 95% confidence intervals. We find large effects of auction wins on firm size even several years after the end of the contract period. Finally, in Figure 3, we allow the effect of auction wins to vary with treatment intensity, defined by the contract value divided by the firm's preauction sales. For small demand shocks, we find negligible long-run effects. For larger demand shocks, we find statistically and economically significant long-run effects.

Are long-run effects of auction wins unique to startups? In Figure 4, we estimate the long-run effects of auction wins using a sample that includes both startups and mature firms. We find that the long-run effect of auction wins decreases sharply with firm age, being economically and statistically insignificant for firms aged above 10. The results in Figure 4 could follow from startups being smaller than mature firms, rather than from being younger. In Internet Appendix Section C, we use a matching procedure to identify mature firms with comparable levels of revenue, employment, and assets as startups,

[14] Because all our firm outcomes are measured at a yearly frequency, we define the contract- and post-contract periods according to the calendar year. For example, if a firm wins a 12-month contract in July 2010, we define 2010 as the contract period year and 2011 as the first post-contract period year, even though the contract period lasts until July 2011. Expanding the definition of the contract duration with one extra calendar year does not change the main results. In Figure 2, we show that the effects of auction wins persist even 5 years after the contract period.

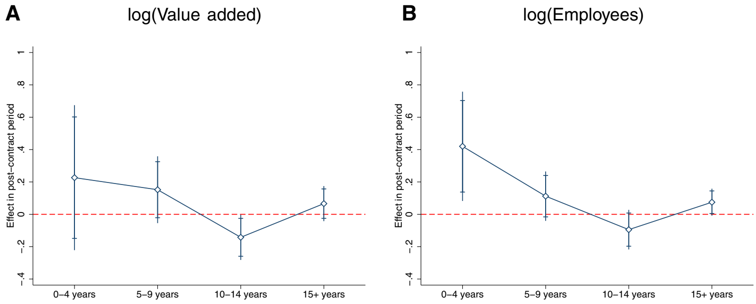

Figure 4
Effect on value added and employment by age bin
The figure reports the firm-age-specific post-contract treatment effect of procurement auction wins on log(Value added) and log(Employees). The treatment effects are estimated separately for firms aged 0–4, 5–9, 10–14, and 15+ years in the focal auction year. Specifically, we estimate the following regression specification within each age bin: $y_{je}=\theta b_{je}+\kappa_t+\alpha_e+\lambda_j+\varepsilon_{je}$, where y_{je} is the outcome for firm-by-auction unit j in event-time e centered on its auction; b_{je} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_j are calendar-time, event-time, and firm-by-auction fixed effects, respectively. The coefficient θ is allowed to differ in the procurement contract and post-contract periods, and we only report the estimates of θ from the post-contract period. The estimation sample comprises both startup and nonstartup auction winners and runner-ups. Standard errors are clustered at the firm level, and the figure includes 95% (outer notches) and 90% (inner notches) confidence intervals around the coefficient estimates.

and estimate the effects of wins for these "pseudo-startups". We find no long-run effect of auction wins for the pseudo-startups.

About 70% of the firms in the estimation sample used in Table 2 are in the construction industry. To assess external validity, in Internet Appendix Section D, we weigh the observations in the main regression to match the industry composition of the Norwegian startup population, using a similar approach as Akerman, Gaarder, and Mogstad (2015) (in these regressions, the construction industry has weight around 20%). The resultant estimates, reported in Table IA.4 of Internet Appendix Section D, are very similar to those reported in Table 2, though the standard errors are naturally larger. These weighted regression results are important because they suggest that our results are not an artifact of the presence of many construction firms in our sample.[15]

5. Identification Concerns

Section 4 shows that startups that win a procurement auction are more than 20% larger in terms of sales and employment than startups that place bids but

[15] As another way to assess external validity, we can compare our estimates to those found in the existing macroeconomics literature. Moreira (2016) uses longitudinal data for all firms in the U.S. between 1976 and 2011 to study the impact of firms' initial economic conditions (stage in the business cycle) on later firm size, as measured by employment. Moreira (2016) finds that a 1% deviation of GDP from its trend is associated with a 1% increase in the average size of the firms born in the same year. A similar elasticity of early-life demand to later-life size can be obtained in our setting by taking the ratio of the post-contract employment effect to the contract period effect on sales (the initial demand shock). Our causal estimates suggest an elasticity of 0.88, which is broadly similar to the estimate in Moreira (2016).

Table 3
Contract delays and cost overruns

	Mean	p(5)	p(10)	p(25)	p(50)	p(75)	p(90)	p(95)	*N*
A. Full sample									
Delay (months)	1.52	−1.00	0.00	0.00	0.00	2.00	6.00	8.00	1,749
Cost overrun (%)	15.58	−18.89	−10.85	−0.59	10.27	26.47	50.45	72.57	1,749
B. Matched sample									
Delay (months)	1.76	0.00	0.00	0.00	1.00	3.00	5.00	6.00	45
Cost overrun (%)	12.90	−19.96	−8.86	−2.65	7.21	18.79	46.28	58.99	45

The table reports summary statistics on contract delays and cost overruns relative to the winning auction bid. Panel A summarizes the full sample of 1,749 contracts. Panel B summarizes a subsample of 45 contracts won by startups. Internet Appendix Section E provides more details on the data and sample construction.

come second, even several years after the contract work ends. In this section, we investigate whether these long-term effects can be interpreted as causal. The two key identification concerns are contract work delays and unobserved differences between auction winners and runners-up not captured by the firm and time fixed effects of Equation (1), the main regression model.

5.1 Contract work delays and follow-on contracts

The empirical analysis has aimed to identify the long-term effects of auction wins on startup size, namely, the effects that persist after the contract work has ended. To this end, in Section 4, we separately estimate the effects of the auction win during and after the contract period, using data on contract duration collected from protocols written before the contract work starts. It is possible that the long-term effects in Section 4 result from contract work delays.

To assess delays relative to the estimated contract duration, we collected post-contract completion data for 1,749 Public Roads contracts over the period of 2003–2017.[16] For each contract, we observe the final payment (including any additional payments due to unforeseen events) and the contract completion date. Panel A of Table 3 summarizes contract delays in months relative to the estimated duration (at contract signing). Both the mean and median delays are small: 1.52 and 0 months, respectively. Even in the right tail, the delays are small: the 95th percentile delay is 8 months. Panel B considers the subset of the 1,749 contracts that are won by startups (i.e., the main estimation sample). The distribution of contract delays in panel B is very similar to the full sample reported in panel A. Overall, the data suggest that contract delays are too small to materially affect the long-run effects of auction wins, which are shown in Figure 2 to persist for several years after the auction.

While large contract delays are rare, cost overruns occur frequently. In panel A of Table 3, we find that the final payment exceeds the initial auction bid for about 75% of the contracts. The mean difference between the firm's auction bid and the final payment is 15.58%, with a median of 10.27%. Importantly, as the

16 The data were obtained from the Infrastructure Department of Public Roads. Internet Appendix Section E provides more details on the data.

contract work rarely is delayed (see panel A of Table 3), these extra payments are captured by the contract period estimates of auction win on firm sales, not by the post-contract period estimates, which are our main focus.

While it seems unlikely that contract delays can explain the main results, one might still be concerned about maintenance or follow-on work after the initial contract spilling into the long-run estimates. For example, a firm that wins a contract to construct a road might, even years after the road is built, be tasked with maintenance or ancillary construction, such as erecting fences around the same road. To address this concern, we emphasize two aspects of the Norwegian Procurement regulation (reviewed in Internet Appendix Section A). First, all relevant aspects of the contract work should be described in the initial contract. If the work is expected to include long-term maintenance, this should be reflected in the initial contract and the estimated contract duration. Second, all procurement should be subject to announcement and competition. This means that if follow-on work is required after the initial contract work is completed, a new auction must be held. In practice, the procurer has ways to circumvent a full procurement process and engage in a "direct purchase" if, for example, there is only one qualified supplier, which could be the case for follow-on work. It should be noted that our data contain information on *all* Public Roads procurements, including direct purchases, and that less than 0.3% of Public Roads procurement value comes from direct purchases. Finally, as competitive auctions could be de facto follow-on contracts, in Internet Appendix Section F, we combine accounting data and procurement data and find that Public Roads contracts account for about 10% of overall revenue for winners in the years after contract completion. However, auction losers also obtain about 10% of their future revenue from Public Roads contracts. So, in percentage terms, winners do not become more reliant on Public Roads than losers. In other words, follow-on contracts through competitive auctions do not seem to play an important role in explaining the long-term effects of auction wins.

5.2 Placebo tests and timing-of-event analysis

The main regression specification in Equation (1) includes firm fixed effects, which control for time-invariant observed and unobserved quality differences between winner and runner-up startups. However, time-varying quality differences that are correlated with bidding strategies remain a concern. If firms with better future prospects systematically place lower bids, comparing winners to runners-up with similar but not identical bids could produce biased difference-in-differences estimates. Here we explore whether bid size differences between winners and runners-up could reflect unobserved differences in future prospects.

First, as a placebo exercise, we compare the characteristics of runners-up and *third-placed* startups in periods before and after the focal auction. If lower bids are correlated with better future prospects, we would expect runners-up and third-placed startups to have different outcomes in the post-auction period.

Conversely, if differences in bids are not correlated with future prospects, there would be no or small such differences. To analyze this question, we keep the runners-up from the estimation sample used in Table 2 and add new observations for third-placed startups (this adds 155 startup-in-auction units). Panel B of Figure 2 shows that runners-up and third-placed startups are similar in both levels and trends of their preauction characteristics. In Internet Appendix Section G, we estimate Equation (1) using runner-up startups as the treated group and third-placed startups as the control. We do not find economically or statistically significant differences between the treated and control group after the auction.

Second, we adopt a "timing-of-event" specification that uses future startup winners as the control group for current startup winners (see Internet Appendix Section H.1 for details). As both treated and control firms are, or eventually become, winners, the two groups may be better matched on their unobserved quality. Moreover, this analysis circumvents bid differences between winners and runners-up because runners-up are not included in the analysis. The timing-of-event coefficients in Internet Appendix Section H.1 show post-contract period effects of 31% and 30% for employment and value added, respectively, both effects highly statistically significant. By comparison, the corresponding effects from the main difference-in-differences model in Table 2 are 22% and 19%, respectively. One reason the timing-of-event coefficients are larger than the main specification coefficients (although not outside their confidence interval) could be that startups that win earlier have faster growth than startups that win later. To analyze this possibility, in Internet Appendix Table IA.7, we analyze the trends of treated and control groups in the 2 years before the procurement auction. We find no evidence of differential trends between the two groups, supporting the validity of the timing-of-event specification. Internet Appendix Figure IA.9 presents graphical evidence to the same effect.

Finally, in Internet Appendix Section H.3, we adopt a regression discontinuity (RD) design based on bid-level data where identification comes from comparing winner and loser startups with near-identical bids. Specifically, at the cutoff point where the RD effect is measured, the win margin is smaller than 0.1%. The results obtained from the RD design are very similar to the main results in Table 2, which suggests that the main results are not likely to be driven by auctions with larger win margins. The RD estimates are robust to including auction fixed effects and to a wide range of functional form specifications on the running variable (i.e., the win margin). The results are also robust to estimating the RD effect using local linear regressions with optimal bandwidths based on the procedure in Calonico, Cattaneo, and Titiunik (2014).

Overall, the analyses in this subsection suggest that unobserved quality differences between winners and runners-up are not likely to be driving our main results.

5.3 Within-auction comparison

The main regression specification in Equation (1) compares startups that win an auction to startups that come second, but these startups do not necessarily compete in the same auction. There may be unobserved firm heterogeneity at the auction level not captured by the firm and time fixed effects in Equation (1). For example, a firm that wins a technology-intensive tunneling contract and a firm that loses a pavement construction contract could differ in their future prospects for reasons unrelated to their auction win and loss.

To address this concern, in Internet Appendix Section H.2 we estimate several difference-in-differences models that compare winners and losers from the same auction. The estimation sample includes both startup and mature bidders from about 3,700 unique auctions in total. By comparing startup winners to startup losers from the same auction, in column 3 of Table IA.9 of the Internet Appendix, we find long-term effects of auction wins that are numerically almost identical to the main startup estimates in Table 2. Specifically, for log value added and log employment, the within-auction estimates in Table IA.9 are 21% and 19%, while the corresponding estimates in Table 2 are 19% and 22%. Comparing mature winners to mature losers from the same auction, Table IA.9 of the Internet Appendix shows no long-term effect of auction wins for mature firms. Overall, the results in Internet Appendix Section H.2 support the main results of the paper.

6. Startup Dynamics

Sections 4 and 5 show that startups that win a procurement auction have significantly higher employment and sales than runners-up, even several years after the contract work is completed. In this section, we describe the financing, personnel, and product market choices that support the winning startups' growth, that is, *how* the startups expand. We use the same difference-in-differences model as in Section 4 and compare changes in outcomes for startups that win a procurement auction to changes in outcomes for startups that come second. In Section 7, we relate these and additional findings to theory and discuss likely mechanisms underlying the main results, that is, *why* the startups expand.

6.1 Financing and restaffing

Financing. We first address how startups finance the expansions documented in Section 4. During the contract period, the procurement demand shock generates increases in both debt and paid-in capital (11% and 18%, respectively), as shown in columns 1 and 2 of panel A, Table 4. The increase in paid-in capital is statistically significant. In the post-contract period, the effect on debt is 8% and the effect on paid-in capital is 28%. Again the latter effect is statistically significant. The leverage ratio, as shown in column 3, is significantly lower in the post-contract period than in the preauction period.

These results suggest that debt is initially an important source of funding, while equity financing becomes more important in the longer run. This squares well with the empirical findings of Robb and Robinson (2014) and Cole (2013).

Managerial quality and pay. Do auction winners replace their manager, and if so, is this manager of higher quality? As a proxy for managerial quality, we follow Klein et al. (2018) and use managerial wage in the year *before* the focal auction. The advantage of this measure is that it is likely exogenous to the auction outcome. A firm's managerial quality will increase (decrease) if the current manager is replaced with one that had higher (lower) wages in the year before the auction, and it will remain unchanged if the manager is not replaced. By estimating Equation (1) using only firms that replace their managers, we find an economically significant long-run increase in managerial quality (13%) for auction winners relative to runners-up. However, as fairly few firms replace their managers, the standard errors are large and the effect is not statistically significant.[17] Using the full sample of firms, including those that do not replace their managers, the increase in average managerial quality is economically significant and borderline statistically significant both in the contract period (5% increase, p-value = .09) and in the post-contract period (6% increase, p-value = .11). Managers of winning firms have about 8% higher remuneration in the contract period, and this effect increases to about 14% in the post-contract period. The latter effect is statistically significant at the 5% level, as shown in column 6 of Table 4. In unreported regressions, we find that the post-contract increase in managerial pay holds for both firms that replace their manager and those that do not.

Worker quality and pay. We also explore whether startups increase the quality and pay of their workers during expansion. Analogous to the managerial quality analysis, we define worker quality as workers' wages in the year before the focal auction, and analyze whether auction winners increase the quality of their workforce over time relative to runners-up. Somewhat surprisingly, in column 7 of Table 4, we do not find any change in the average workforce quality. We estimate Equation (1) using the log of average worker wage (excluding the manager) as the outcome. In column 8 of Table 4, we find no economically or statistically significant effect of auction wins on worker wages. Thus, winning startups appear to hire higher-quality managers, but not higher-quality workers.

6.2 Product market choices

The accounting data described in Section 2.1 do not contain information on customer relationships. However, the Public Roads auctions data detail bidding firms' relationship with Public Roads after the focal auction through their activity in consecutive procurement auctions. This enables us to follow how startups' product market choices evolve. We are interested in whether startup

[17] In total, 37 startups replace their managers after the focal auction. Of these, 26 are winning firms, while only 11 are runners-up, suggesting that winning startups replace their managers at a higher rate than runners-up.

Table 4
Other outcomes

A: Capital structure and employee quality

	log(Debt)	log(Paid-in cap.)	Lev.	CEO quality$^{\text{Switchers}}$	CEO quality$^{\text{All}}$	log(CEO wage)	Worker quality	log(Avg. worker wage)
θ (Contract)	0.11	0.18**	−0.06**	0.10	0.05*	0.08	−0.02	−0.02
	(1.37)	(2.24)	(−2.07)	(1.00)	(1.74)	(1.00)	(−1.01)	(−0.95)
θ (Post-contract)	0.08	0.28**	−0.08**	0.13	0.06	0.14**	−0.02	−0.01
	(0.87)	(2.55)	(−2.38)	(0.90)	(1.58)	(2.22)	(−0.89)	(−0.27)
Adj R^2	.78	.82	.47	.72	.89	.59	.76	.78
N	2,942	2,891	2,944	351	1,604	1,633	2,263	2,270

B: Later auction participation and winning

	Participation						Winning	
	Competes	Large	Larger	High TFP	New product	Quality	New product	Quality
θ (Contract)	0.05*	0.10***	0.01	0.09**	0.11***	0.00	0.05***	−0.00
	(1.76)	(2.76)	(0.66)	(2.53)	(2.89)	(0.02)	(3.11)	(−0.06)
θ (Post-contract)	0.13***	0.07***	0.12***	0.12***	0.10***	0.01	0.05**	0.01
	(3.74)	(3.02)	(4.21)	(4.10)	(3.11)	(0.59)	(2.56)	(0.66)
Adj R^2	.53	.32	.28	.35	.30	.22	.19	.14
N	3,761	3,761	3,761	3,606	3,761	3,761	3,761	3,761

The table reports estimates of θ from the following regression: $y_{je}=\theta b_{je}+\kappa_t+\alpha_e+\lambda_j+\varepsilon_{je}$, where y_{je} is the outcome for firm-by-auction unit j in event-time e centered on its auction; b_{je} equals one for auction winners in the post-auction period, zero otherwise; and κ_t, α_e, and λ_j are calendar-time, event-time, and firm-by-auction fixed effects, respectively. The estimation sample is defined in Section 3. The coefficient θ is allowed to differ in the contract and post-contract periods. In panel A, CEO and worker quality are measured as the CEO's or worker's wage in the year before the focal auction, as explained in Section 6.1. In columns 4–6, we include in the estimation sample only the firms that have a CEO both before and after the focal auction. In column 4, we further restrict attention to firms that change CEOs in the post-auction period. In panel B, the outcomes are indicators for whether a firm competes in a price-only auction; competes in a large (above median value) auction; competes in an auction that is larger than the focal auction; competes in auctions in which the competing bidders are above-median TFP; competes in auctions for new products; competes in auctions in which price is not the only winning criterion; wins in auctions for new products; or wins in auctions in which price is not the only winning criterion. Standard errors are clustered at the firm level. t-statistics in parentheses. *$p < .1$; **$p < .05$; ***$p < .01$.

expansions are associated with vertical movement into higher ends of the market, with horizontal movements into new markets, or both.

In panel B of Table 4, we explore whether auction wins affect whether firms participate in subsequent Public Roads auctions, and which types of auctions they participate in. Column 1 shows that auction winners are 13 percentage points more likely to participate in another auction in the future compared to runners-up. Regarding auction types, columns 2 and 3 show that winners are more likely to participate in large (above-median value) auctions and are more likely to participate in auctions that are larger than the focal auction. Moreover, in column 4, we find that winners are more likely to compete in auctions where the competing bidders have above-median total factor productivity, which we interpret as winners moving vertically into more challenging markets.[18] Column 5 shows that winners are more likely to enter auctions for new products (relative to the focal auction product). They also win more auctions for new products, as column 7 shows. Columns 6 and 8, respectively, show that winners neither participate nor win more often in auctions where product quality is a winning criterion.

7. Discussion

This section provides an economic interpretation of the main findings in Section 4 and Section 6. After reviewing the main findings in Section 7.1, in Section 7.2 we discuss several possible mechanisms in light of the theoretical literature, and present some additional empirical results. In Section 7.3, we collect and analyze case evidence from newspaper articles.

7.1 Review of results

This paper investigates the long-term effects of temporary demand shocks for startups by comparing startups that win a procurement auction with startups that place bids but come second. Our empirical findings suggest that temporary demand shocks have several effects on long-run outcomes. Winning startups are more than 20% larger than runners-up in terms of sales and employment, and are more profitable, even several years after the contract period, despite being very similar ex ante. The results are not only statistically significant but also large: about 50% of the contract value is transmitted into a long-run effect on startup sales.

7.2 Mechanisms

In a simple neoclassical model with a constant environment, no financial constraints, and no learning, a firm faces the same profit-maximization problem

[18] We measure total factor productivity (TFP) at the startup-year level using accounting data on total revenues, capital costs, and labor costs, using the same factor weights and Cobb-Douglas production function as in Bloom et al. (2013). We caution that firm-level TFP is notoriously difficult to estimate when, as in our case, the data only contain firm-level aggregates of revenues, capital, and labor (see, e.g., Syverson 2011).

in each period, and a temporary demand shock creates only a temporary increase in firm size. Several theoretical models that deviate from this setting suggest possible mechanisms. We review these theories in turn and relate them to the evidence. We begin by considering our preferred interpretation — that the results are driven by learning-by-doing effects from the contract work — and then discuss several alternative, not mutually exclusive, mechanisms. Internet Appendix Section I formalizes the theories in a simple unified framework.

Learning-by-doing. Arrow (1962) argues that knowledge underlies the production function of a firm, and that knowledge increases with experience. The learning-by-doing hypothesis involves a positive feedback loop: experience — which, in our context is gained by completing a Public Roads contract — enhances the firm's capabilities, increases investments, and leads to size differences between winning and losing startups that can persist even in the long run. In Section 6.2, we found that winning startups tend to enter subsequent auctions with larger competitors, move into new product categories, and hire managers of higher quality. These findings are consistent with learning-by-doing from the contract work.

The learning-by-doing hypothesis formulated by Arrow posits that learning-by-doing has decreasing returns; the more accumulated experience, the less learning at the margin.[19] We therefore expect learning-by-doing effects from contract work to be stronger for younger firms. Consistent with this, Figure 4 shows that the treatment effects decrease with firm age.

Learning-by-doing effects being stronger for startups implies that startups should bid more aggressively for procurement contracts than mature firms because the benefits of a contract win spills into the future. To assess this hypothesis, we utilize the bid-level data described in Section 2.2. Unlike in the main analysis, which focuses on auctions won by startups, we use all the price-only auctions in the data (Internet Appendix Section J summarizes this sample). We use each auction bid as the unit of analysis and estimate the following cross-sectional regression:

$$Y_{ik} = \alpha_k + \beta \text{Startup}_{ik} + \epsilon_{ik}, \tag{2}$$

where Y_{ik} is the natural logarithm of the bid size of bidder i in auction k, and Startup_{ik} is an indicator for whether i is a startup as opposed to a mature firm. We include auction-level fixed effects, α_k, to control for differences between startup and mature firms that are fixed at the auction level, including the size of the contract, the type of work, the number of bidders, and the contract duration.[20] Hence, β measures differences in Y_{ik} between startup and mature bidders net

[19] See, for example, Benkard (2000) for evidence that learning-by-doing effects are stronger when firms are young. Thompson (2012) reviews the literature on learning-by-doing.

[20] An alternative approach to using auction-level fixed effects would be to include a rich set of observable auction characteristics as control variables in Equation (2). In Internet Appendix Section J we find that, conditional on a wide range of control variables, startups on average place bids that are about 1.8% lower than mature firms.

Table 5
Comparing startup and mature firms' auction bids

	log(Bid)	Winner	log(Bid)
Startup	−0.014**	0.025*	
	(−2.399)	(1.651)	
Startup & Age < 5			−0.017*
			(−1.904)
Startup & Age ≥ 5			−0.012
			(−1.586)
Adj R^2	.99	−.26	.99
N	12,729	12,729	12,729

The first two columns report estimates of β from Equation (2) using log bid size and an auction win indicator as the outcomes. The data are at the bid level, and there are 12,729 bids spread out across 3,677 Public Roads price-only auctions (Table IA.14 in the Internet Appendix provides summary statistics). The third column reports estimates from a modified version of Equation (2) in which Startup_i has been replaced with separate indicators for startups aged below and above 5 years. Standard errors are clustered at the firm level. t-statistics in parentheses. t-statistics in parentheses. $*p < .1$; $**p < .05$; $***p < .01$.

of the average bid size in i's own auction. If $\beta < 0$, then startups on average place more aggressive bids than mature firms, holding the auction fixed.

In Table 5, we find that startups, on average, place bids that are 1.4% lower than those of mature firms, statistically significant at the 5% level. That startups tend to place lower bids than mature firms, holding the auction fixed, implies higher win rates for startups than for mature firms. Indeed, as shown in Table 5, startups have a 2.4 percentage points higher win rate than mature firms, statistically significant at the 10% level. Internet Appendix Section J shows that these results are robust to excluding outlier auctions with very large contract values. Arrow's idea of decreasing returns to learning implies that younger startups should bid the most aggressively. In Table 5, we reestimate Equation (2) but allow startups aged 0-4 years and 5 years or above to have different β coefficients. For the youngest group, the estimated β is 1.7%, and for the oldest group it is 1.2%, suggesting that the youngest startups bid the most aggressively.[21]

To sum up, several pieces of evidence indicate that startups enhance their capabilities through learning following an auction win. Winning startups enter subsequent auctions with larger competitors, hire managers of higher quality, and move into new product categories. The long-run effects of auction wins are considerably stronger for startups than for mature firms, consistent with decreasing returns to learning. Finally, startups seem to be aware of the future benefits of auction wins and bid more aggressively than mature firms.

Demand frictions. Startups are initially a lesser-known entity and may reduce demand frictions through the development of a reputation (e.g., Foster, Haltiwanger, and Syverson 2016). Much like learning-by-doing, such

21 We also analyzed how auction wins affect TFP, as measured following Bloom et al. (2013) using accounting data on total revenues, capital costs, and labor costs. The results (unreported) are ambiguous; the overall TFP effects are zero, but the effects are increasing in treatment intensity. That is, we find a slightly negative effect for small wins and a slightly positive effect for large wins. These ambiguous results may be because measuring TFP is especially difficult for startups (see, e.g., Foster, Haltiwanger, and Syverson 2008).

reputation effects involve a positive feedback loop: an increase in sales (in our context, from winning an auction) reduces demand frictions and leads to increased future demand, which could sustain long-run differences between auction winners and runners-up.

The finding that startups bid more aggressively than do mature firms is consistent not only with learning-by-doing but also with reputation effects. Other evidence suggest that reputation effects are not of first-order importance. Recall that Public Roads has two main auction formats: auctions where price is the only winning criteria, and auctions where perceived product quality is one of the winning criteria. If auction wins reduce demand frictions vis-à-vis Public Roads, we would expect winners to increase their participation in future quality-criteria auctions, in which Public Roads may use their discretion to favor suppliers they already know, and to increase their win rate.[22] As shown in panel B of Table 4, however, winning firms are no more likely to participate in quality-criteria auctions than are runners-up. Table 4 shows that startups also do not win such quality auctions more often. With the important caveat that the data allow us to evaluate demand frictions only vis-à-vis Public Roads, there appears to be little direct evidence for the demand frictions hypothesis.

Sunk costs. If investments in physical capital are irreversible, a temporary demand shock could justify investments that make a firm more productive and larger in the longer run (e.g., Sutton 1991; Dixit 1992; Das, Roberts, and Tybout 2007). For example, if investing in a long-lived machine that persistently reduces marginal cost can be justified by the contract work, and that machine has a low resale value, for example, because of costs associated with dismantling, shipping, and installing it elsewhere, procurement auction winners may be larger than runners-up in the longer run even if they were identical before the auction.

To assess the sunk cost hypothesis, we explore the timing of startups' increase in long-term tangible assets. Figure IA.5 of the Internet Appendix shows that the effect of auction wins on long-term tangible assets is statistically and economically significant only 3 years after the contract period ends. This suggests that sunk investments in tangible assets are not driving the long-term effects, as the investments occur later than, for example, the increases in sales or employment. It might seem odd that sales and employment increase during the contract period, while tangible assets do not. Table 2 shows that auction winners increase their use of intermediate inputs (an earnings statement item that in Norway covers raw materials and subcontracting costs) during the contract period. The asymmetry in response for intermediate inputs and investments in tangible assets — the former responding early and the latter late — evidenced

[22] Of course, if a startup performed poorly in its first Public Roads job, Public Roads might be less inclined to accept that firm's bid in a future auction where product quality is a winning criterion. We do not have data on whether Public Roads is satisfied with suppliers' performance, but note that publicized court cases with startups are rare (see Internet Appendix Section A.1). Also, as shown in Table 3, substantial time and cost overruns of contract work are rare.

in Table 2 suggests that startups initially expand by increasing purchases and make investments only after the contract work is done to sustain their new size.[23]

We find that winners increase their tangible assets only after the contract work ends, which suggests that investments are a consequence, rather than a cause, of growth. Still, we acknowledge that the apparent lack of investment during the contract period could be due to measurement problems. For example, investments creating sunk cost effects need not be in tangible capital, but could be in intangibles, such as market knowledge or worker human capital — investments that are notoriously difficult to capture with accounting data. We do not have access to data on training costs, but note from column 8 of Table 4's panel A that average worker wage does not increase in the longer run, which suggests that worker training, if it occurs, does not appear to increase general human capital. In the end, the sunk cost mechanism is a priori plausible, but our admittedly limited empirical tests do not suggest that it has first-order importance.

Financial frictions. In the Evans and Jovanovic (1989) and Fan, Kuhn, and Lafontaine (2017) models, startups are financially constrained and operate at a suboptimal scale. Winning a procurement contract can provide a startup with cash flow that allows it to finance an expansion that brings the startup closer to the optimal scale. Consequently, relaxed financial constraints could drive the long-run effects of temporary demand shocks observed in Section 4.[24]

Two pieces of evidence suggest that a contract win reducing financial constraints does not play a major role in our setting. First, we analyze procurement in the construction industry, where investments are highly tangible and funding is likely to be relatively easily available. Indeed, more than 80% of the startups in our sample either raised internal capital or secured bank credit the year before the focal auction (see Table IA.16). Second, in Internet Appendix Section K, we analyze whether treatment effects vary with financial constraints, as measured in the year before the auction. We use three simple measures of financial constraints (e.g., Almeida, Campello, and Weisbach 2004; Denis and Sibilkov 2010): the cash-to-assets ratio, dividend payments, and firm size. The results, discussed in greater detail in Internet Appendix Section K, show no systematic relationship between the long-term effects of auction wins and measures of financial constraints. Still, it is difficult to firmly conclude that financial constraints play no role. Our measures of financial constraints may not necessarily capture actual financial constraints very well (see Farre-Mensa and Ljungqvist 2015 for a critique of some common measures of financial

[23] In Figure IA.6 of the Internet Appendix, we compare the long-term effects of auction wins on sales for startups and mature firms at comparable levels of treatment intensity (contract size relative to sales). We find no effects for mature firms even at levels of treatment intensity that yield substantial effects for startups. Under the investment hypothesis, one would arguably expect long-run effects of auction wins also for mature firms.

[24] Available evidence suggests that size differences based on differences in financial constraints can be long-lasting (e.g., Cabral and Mata 2003).

constraints). Also, some of our evidence point in the direction of financial constraints, at least in the short run. For example, we find that winning startups do not increase their levels of tangible assets during the contract period; instead, they increase their use of intermediate inputs (i.e., purchases from suppliers) to ramp up production.

In sum, our results indicate that learning-by-doing is a plausible mechanism explaining why auction wins have long-term effects for startups. We find less evidence that reduced demand frictions, relaxed financial constraints, or sunk costs play a large role, but we acknowledge that the empirical analysis is imperfect in several ways. First, while accounting data capture investments in physical equipment reasonably well, to a lesser extent do they capture investments in intangible assets, such as human capital, a possible source of sunk cost effects. Second, although we find little evidence of winning startups enhancing their reputation vis-a-vis Public Roads, winning a Public Roads contract could be utilized in startups' marketing to obtain private sector contracts. We also acknowledge that the potential mechanisms are not mutually exclusive and could interact in many ways. For example, learning-by-doing could induce startups to make investments that later become partially sunk (one such example, the firm Paneda, is discussed in Section 7.3), thus reinforcing the long-term effects.[25]

Another possible mechanism for the observed path dependence in startup outcomes is that managers gain self-confidence following a quasi-random win, attributing the win to their own skill rather than to luck. Anagol, Balasubramaniam, and Ramadorai (2021) pursue this idea in the context of individual investors that participate in initial public offering (IPO) lotteries in India, finding that exogenous return shocks associated with the IPO stock strongly affect treated investors' trading volume in non-IPO stocks. If managers attribute auction wins to skill rather than luck, we would expect winning startups to invest more, as their perceived marginal productivity is higher, with subsequent increases in gross revenues. At the same time, we would expect marginal profits to be negative, as the managers' beliefs are excessive. However, in Table 2, column 4, we find a positive effect on profits for winning firms, which suggests that winning firms do not over-invest. Hence a positive shock to managerial self-confidence does not appear to be a main driver of the results.[26]

[25] In the Jovanovic (1982) model, firms start up with heterogeneous productivity but are initially unaware of these differences in productivity. Gaining experience (in our context, from contract work), startups learn about their own productivity, which leads those that receive positive news about own productivity to expand and those that receive negative news about own productivity to exit at a higher rate (Pakes and Ericson 1998). In panel A of Table 2, however, we find that auction winners do not have higher exit rates than runners-up, neither during nor after the contract period. Thus, we do not find much support for this mechanism.

[26] It is also conceivable that higher self-confidence following an auction win leads the manager to dare take on riskier projects. Comparing the standard deviations of log value added and log employment for startup winners and runners-up before and after procurement auctions, we find no effects of auction wins on the volatility of startup outcomes. This suggests that winning managers do not subsequently take on riskier projects.

7.3 Case evidence

To help inform mechanisms further, we collected newspaper articles that describe startups' experiences with winning a Public Roads procurement auction. The results are broadly consistent with the findings from the register data analysis.

To assemble the newspaper case database, we used Atekst, an online news platform with exhaustive coverage of articles from local and national media outlets. We searched for articles covering a Public Roads contract win and kept articles describing wins by startups. This left us with 72 articles.[27] After excluding 36 articles that contain mere announcements of a contract win and two articles that describe legal disputes, we are left with 34 articles covering 27 startups. This gives a "finding rate" at the firm level of about 13%, as shown in panel A of Figure 5. As expected, the finding rate is higher for larger contracts. This lack of representativeness can be viewed as an advantage, as the effects uncovered in the register data analysis are disproportionately driven by the winners of the larger contracts (see Section 4). To avoid double-counting startups that are mentioned in multiple newspaper articles, for each of the 27 startups, we only keep the article that was published first.[28]

The majority of the sampled articles (more than 90%, as shown in panel B of Figure 5) include an interview with the startup's manager or employees. Most of the newspaper articles (more than 80%, as shown in panel C of Figure 5) are written just after the Public Roads auction and focus on short-term operational challenges with the forthcoming contract work, such as staffing or investment needs. This limits the amount of information the articles contain about the longer-run evolution of the firm.

With these caveats in mind, we divided the articles into five categories corresponding to the five potential mechanisms discussed in Section 7.2.[29] Very few cases had content that overlapped these five categories. Panel D of Figure 5 presents the results. Of the 27 newspaper cases, 12 (44%) mention *investments* in new equipment and 7 cases (26%) mention better utilization of existing equipment, so that a broad investment category picks up 19 (70%) of the cases. Outside of individual cases, some reviewed below, it is difficult to judge which fraction of these investments will later become "sunk" and thus are more likely to have long-run effects as an independent mechanism. The amount invested is also unknown. 5 (19%) cases highlight the *learning* experience expected by the startup, and 2 (7%) cases describe how winning a contract involves expansion

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[27] As in the main analysis, we define startups as firms that are 10 years or younger in the auction year, were not a subsidiary at birth, and had less than US$2 million in total assets in the first 2 years of operation. In the newspaper analysis, this gave a slightly larger sample of startups than in the main analysis, since we also kept startups that won auctions after 2015 (when our main sample ends).

[28] Analogously, the estimation sample in Section 4 only includes startups' first auction wins. In the rare scenario where the same startup is mentioned in multiple articles within the same year, we choose a random article.

[29] For robustness, we used a simple text recognition algorithm on the sampled articles and obtained broadly similar results.

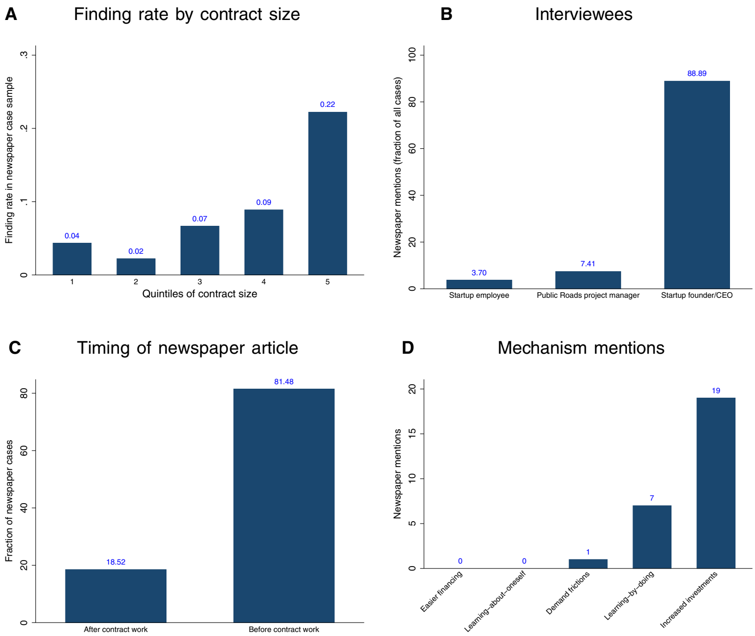

Figure 5
Summary statistics: Newspaper cases
The figure presents summary statistics from 27 newspaper articles describing startups' experiences with winning Public Roads procurement auctions. In panel A, we plot the "finding rate" (the count of firms in the newspaper sample divided by the count of startup winners in the estimation sample) by quintiles of Public Roads contract size. Contract size is measured as the contract value divided by firm sales in the year before the auction. Panel B describes the interviewees in the 27 newspaper articles. The bars in panel B sum to 100% because all of the sampled newspaper articles include an interview. Panel C describes whether the newspaper articles were written before or after the Public Roads contract work had started. Panel D counts the number of newspaper articles that mention each of the five main theoretical mechanisms (investment, learning-by-doing, demand frictions, learning about oneself, and relaxed financial constraints) discussed in Section 7.2.

into product areas that are new to the startup. Thus, with a broad definition of learning, 7 (26%) cases are included. Regarding *demand frictions*, only one (4%) case mentions that the contract experience can be used as a reference to attract new customers later on. None of the newspaper articles mentions that the contract will ease *financial constraints* (some cases describe the need to raise new capital to finance the expansion, consistent with the findings of the register data analysis).[30]

To summarize, 26% of the sampled newspaper articles mention learning effects of winning a Public Roads auction; 4% mention demand friction effects; and none mentions relaxed financial constraints effects from winning an auction. Thus, consistent with the discussion in Section 7.2, the newspaper case evidence points to learning-by-doing as a key driver of long-run effects

30 None of the newspaper articles mentions firms learning about their own productivity, as in Jovanovic (1982).

in the register data. 44% of the newspaper articles mention new investments following an auction win, but it is difficult to say which of these are likely to involve sunk costs.

Case summaries. On *learning-by-doing*, Holdahl Maskin og Transport won a major contract on road maintenance in 2015. In an interview, CEO Levi Holdahl explains that the contract work entailed learning-by-doing effects on their planning and risk analysis abilities, facilitating future work. A startup that experienced both investment effects and learning-by-doing effects from winning a Public Roads contract, is the construction company Frosta Entreprenør. Founded in 2010, Frosta Entreprenør won a Public Roads contract in 2013 to move a stretch of road away from agricultural land. One of the firm's truck operators explains in an interview that the firm acquired a specialized Hitachi 210 truck for the job. A key innovation of the Hitachi 210 truck is a sophisticated and very precise GPS system that warns the truck operator of any deviations exceeding 1 centimeter from the Public Roads blueprint. In the interview, the truck operator explains that the Public Roads job will allow him to acquaint himself with the new Hitachi truck, which in turn will increase productivity in future jobs.

Paneda provides an interesting example of how winning a Public Roads contract can enhance a firm's knowledge and capabilities. Paneda specializes in DAB transmission in road tunnels — of which there are exceptionally many in Norway — and won its first Public Roads contract in 2013. Paneda has since developed a number of complementary products to the DAB transmitter. Similarly, Betongpartner AS, originally a specialist in the construction of ferry docks, describes in a newspaper article that in tandem with winning a Public Roads contract, the firm is undertaking major investments to transition into tunnel work, which requires related but distinct technical expertise.

On *reduced demand frictions*, Orbiton AS specializes in photo and video monitoring of bridges using drone technology. Two years after its inception, in 2015, Orbiton landed a major Public Roads contract involving the monitoring of more than 300 bridges nationally. The CEO of Orbiton, Thomas Moss, states in an interview that Orbiton will use the Public Roads contract as a reference in their marketing toward other potential customers.

One example of a startup *investing* after winning a Public Roads contract is Norsk Bergsikring, which specializes in securing roads from stone and mud avalanches by building protective fences. In a 2016 interview, Roy Sævik, the CEO of Norsk Bergsikring, describes how winning a Public Roads contract enabled the firm to invest in a highly specialized Menzi Muck climbing machine — of which, according to Sævik, "there are only a handful in Norway" — that can work in especially steep and challenging terrain. Another newspaper article concerns Dokka Entreprenør, which obtains 85% of its revenue from Public Roads contracts. In this article, Henry Ringvold, the CEO and majority stakeholder of the firm, states that as a direct consequence of a Public Roads contract, Dokka Entreprenør has invested NOK 14 million ($\approx$ US$2 million) in new machinery, such as excavators. This expanded the firm's inventory to a total of 50 machines. These two cases illustrate heterogeneity in the degree of "sunkness": excavators are standard equipment for road builders and should be a liquid asset, while a specialized climbing machine would likely be more difficult to resell.

8. Conclusion

We have assessed the effects of temporary demand shocks on long-term startup outcomes by comparing startups that win a procurement auction with runners-up. The main empirical finding is that these temporary demand shocks have large long-run effects on firm size; startups that win a procurement auction are more than 20% larger in terms of sales and employment than startups that narrowly lose an auction, even several years after the end of the contract work. The empirical analysis suggests that the effects of learning-by-doing from contract work is a plausible mechanism, but it does not rule out the possibility that other mechanisms could play a role, possibly by interacting with learning-by-doing effects.

Our work primarily informs the large academic literature that explores factors that may explain why some startups perform better than others. We highlight that an overlooked factor, temporary demand shocks, can play an important role in shaping long-term outcomes.

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