Abstract

Using 43,000 public construction contracts in Chile procured employing open calls for proposals, I study the effect of firm experience on the likelihood of winning a contract in the future. To address endogeneity of experience (better firms tend to win more contracts in the past and in the future), I instrument firm experience with the number of past contracts won in closely contested auctions, where close auctions are defined as either i) having close monetary bids and price as an important awarding factor ii) involving closely ranked firms (via a modified ELO algorithm) . The IV estimates indicate that firm experience increases the proportion of contracts won by seven percentage points (roughly a third of the winning rate of firms with no experience). I investigate possible mechanisms that could explain this increase in market success by improvements along i) cost measures and ii) quality variables. I find that experienced firms submit bids which are three percentage points lower than firms with no experience, which is correlated with an increase in winning probability. Additionally, experienced firms increase in ten percentage points the approval rate of their proposals in the first stage of the awarding process. I discuss the magnitude of the findings and possible implications for public auction design.

1. Introduction

Public purchases constitute a sizable proportion of the government budget. Taxpayers expect public purchases to be transparent, efficient in cost and effective in the production public goods. The existence of competitive markets for each of the types of products purchased by the government is seen as a necessary condition for efficient procurement. Usually, competitiveness is accepted to be negatively affected by the existence of artificial entry barriers, like regulation or collusion. However, a more complicated case arises if participants in the market can gain competitive advantages through experience ("learning by doing"). In this case, the human and organizational capital acquired by performing works can improve a firm's competitivity and overall short term social welfare, but at the same time it can curb future competition in the market by reducing entry or making it difficult for new entrants to succeed.

This thesis investigates whether past experience causally improves future outcomes for contractors in the market for public construction contracts. We consider as outcomes of interest the share of contracts won by each firm, out of total contracts bid for, in subsequent time periods. The treatment variables considered is experience, measured as past wins in the market. We consider several ways of both computing experience (i.e. rolling, cumulative) and also several functional forms (binary indicator and total).

The empirical design consists on producing several "slices" in time, each composed by a period in which we compute experience and a subsequent period where we compute the outcomes, for each firm. We employ these slices to perform regressions between different measures of experience as the treatment variable and winning shares of firms as the outcome variable. Our 11-year data allows us to produce analysis at several points in time, which helps to prevent confounding noise from temporal market trends.

We employ a dataset of more than 43,000 contracts of public construction projects in Chile, totaling approximately 150,000 individual firm bids across 11 years, to study the treatment effect of experience on future bidding outcomes. The sample contains all geographic regions and a collection of more than 900 individual buyers (government units) and 15,000 individual sellers (firms). For most of the government units included in the sample, the data is comprehensive in its coverage of auctions held for projects of the construction. The wide scope of the data is achieved because of key laws passed in the last 15 years in Chile aimed at increasing transparency and efficiency, which have created information reporting requirements for government units regarding public purchases.

The OLS results of regressions on outcomes on experience show that the existence of positive experience is associated with an increase of between 6.1 and 7.4 percentage points in mean future winning shares, which equals around 20% of the dependent variable's standard deviation and almost a third of its mean. Every extra contract won in the past period is associated with between 1.0 and 2.7 extra percentage points in winning shares. All the key estimates are significant at $p < 0.01$ and with low standard errors. We find however high heterogeneity in outcomes and low R^2 in our regressions.

The research objective is to identify the treatment effect of experience on the outcomes of firms in the market of public construction projects, but because experience is likely to be endogenous with unobserved cost factors, specific to each firm, the OLS estimates are not likely to be consistent. We employ external variation on experience to produce consistent estimates of the treatment effect. Our identification strategy employs closely won contracts as the source of random variation in experience levels, arguing that they cannot be attributed to unobserved cost advantages. We define "close wins" by two alternative strategies. The first one labels a win as close if price was more than half of the awarding criteria and winning bids were close to other competitors' bids. The second alternative labels a win as close if all firms participating in the auction had a similar rank, which we compute for every firm at every point in time via a multiplayer ELO algorithm. We argue that the empirical strategy for the binary experience treatment identifies the Local Average Treatment Effect for the compliers.

The resulting IV estimates are close but higher than OLS counterparts: between 6.3 and 8.4 percentage points for an indicator of positive experience as treatment and between .7 and 3.2 percentage points for total experience. We perform robustness analysis on several of the parameters employed either to construct our analysis sample or in the identification strategy, especially the ones related to the definition of a close win, like thresholds of closeness between bids and for the allowed bandwidth for firm's ranks. The results show robustness to most of the parameters employed, although we lose power to obtain significant estimates at very high thresholds for the instruments, especially for the price IV strategy.

Next, we present and investigate two hypothesis regarding the underlying mechanisms that could explain the improved outcomes for firms that acquire experience: improvements in cost measures and quality improvements in proposals. We test the first hypothesis by analyzing the evolution of firm bids' amounts among firms with different levels of experience. We find evidence that confirms that more experienced firms submit lower bids: the treatment effect of positive experience on bids is to reduce standardized bid amounts (i.e. the quotient of monetary bids on government estimates of the cost of the project) by around four percentage points. The effect is relevant considering that the average difference between lowest and second lowest bid is around nine percentage points.

Regarding the second hypothesis, we test it by analyzing the rate of acceptance of firms' proposals in the first stage of the awarding process, which controls that the proposals fulfill a set of basic non-economic, mostly formal criteria. Employing similar identification techniques as before, we find that the treatment effect of strictly positive experience is to increase in around ten percentage points the future mean acceptance rates, which is around a third of the standard deviation of the outcome variable in the analysis sample.

The Discussion reviews the magnitude of the estimates found, analyzes the strengths and limitations of the findings and examine the econometric interpretation of the estimates. We also discuss the heterogeneity of outcomes and possible effects in the competitiveness in the market.

We chose to examine specifically the construction sector because of several reasons. First, construction projects are more differentiated in comparison to other types of goods procured by the government, which makes them more complex and expectedly more difficult for newcomers. Second, several types of the projects procured by the government in this sector are not developed in the private sector, such as roads and parks. Finally, given the magnitude of the spending required to perform construction projects, they are usually one of the main focus in the study of public efficiency. Moreover, in the aftermath of the pandemic produced by COVID-19, one of the trends observed across countries has been to propose increases in the budget for these types of projects.

The structure is as follows. Chapter two presents the relevant literature. Chapter three describes the institutional context of public purchases, especially for construction projects. Chapter four details the source and characteristics of the data. Chapter five contains our main analysis of the effect of experience on outcomes. Chapter six studies the possible operational ways in which experience can increase the advantages of a firm in the market. Chapter seven presents a discussion of the results obtained and chapter eight concludes.

2. Experience and Outcomes

This chapter addresses the main research question of whether public experience improves future prospects for firms in the market of public construction projects. The rationale behind the hypothesis is that through experience firms become more efficient and delivering better products; and get familiarized with the bidding process and the bureaucracy of the public sector.

The empirical strategy proceeds by slicing the data in specific points in time and examining how past experience for a firm is related to the proportion of future contracts that it wins out of the future contracts that it bids for. The focus is on the existence of a discontinuity in the outcomes of firms with strictly positive experience and the outcomes of firms with no experience.

Section 2.1 presents the empirical strategy, Section 2.2.1 presents the data and the construction of the regression sample, Section 2.3 shows the main results and finally Section 2.4 the robustness checks.

2.1 Empirical Strategy

Our empirical strategy consists in a Regression Discontinuity design in which we compare the bidding outcomes for firms with different levels of previous experience in the market. This section presents the main OLS specifications and the variables of the regression. The next section deals with the causal interpretation of results.

Our two main OLS specification are presented in equations 2.1 and 2.2. Here, S_{it2} is the share of contracts won in period 2 of slice t, EXP_{it1}^{k} and $EXP_{it1}^{k} > 0$ are the experience treatment variables, and T_t are period fixed effects. We employ indexes 1 and 2 to make explicit that each time slice t involves two periods: period 1 of experience computation and period 2 of outcome computation. Also, the slice is indexed by time t which is the date in between the two periods. Period fixed effects are added for each period of outcomes to control for changes in the market environment throughout the sample.

$$S_{it2} = \alpha + \beta_k (EXP^k_{it1} > 0) + T_t + \varepsilon_{it} \tag{2.1}$$

$$S_{it2} = \alpha + \gamma_k EXP^k_{it1} + T_t + \varepsilon_{it} \tag{2.2}$$

The outcome variable S_{it2} is the share of contracts won out of total contracts bid for, in the second period of a given slice t. That is, for slice t, the outcome variable for firm i is $\frac{W_{it}}{B_{it}}$ where B_{it} are the bids submitted by firm i on the period $[t, t+\tau]$, W_{it} are the contracts won in period $[t, t+\tau]$ and τ is a parameter that controls the length of the periods where we compute the outcomes. In our initial specification, we consider each $\tau =$ two years.

The treatment variables are either i) an indicator of past experience $EXP^k_{it1} > 0$ and ii) total experience EXP^k_{it1}. Moreover, we consider two ways of *computing* the total experience EXP^k_{it1} for a firm i, which we index by k, $k \in \{1, 2\}$. The first measure of experience computes experience acquired in a fixed period immediately before t (*rolling experience*). The second measure of experience computes experience by adding up all the contracts won for a firm before t and dividing by the number of years in which it was accrued (*annualized cumulative experience*). Details are given in the data section.

2.1.1 Endogeneity and Identification

A structural interpretation of equations 2.1 and 2.2 must recognize the presence of endogeneity and heterogenous effects, which prevents from considering the OLS estimates as causal treatment effects directly. In this section, we present the problem of endogeneity, propose and IV strategy to address it, and then discuss the causal interpretations of the estimates obtained with it.

First we discuss endogeneity. Unobserved cost variables, specific to each firm, are omitted in the OLS regressions above and expectedly endogenous. If there are highly efficient firms who are able to bid more aggressively or submit better proposals, they should win more projects, and in turn accumulate more experience over time. We thus expect our estimates $\hat{\beta}$ and $\hat{\gamma}$ in 2.1 and 2.2 to be biased upwards due to correlation (expectedly positive) between omitted cost variables or efficency variables (which we call W_i) and the amount of past experience. That is, $E(\varepsilon \cdot EXP) \neq 0$ in the structural interpretation of Equations 2.1.

To estimate consistently the parameters of interest, we employ variation in experience uncorrelated with firm's internal characteristics to instrument the experience of a firm in an Instrumental Variables (IV) approach. We propose to employ close wins as an instrument for total wins. If we are able to find wins where the success of a firm is less or not at all attributable to unobserved cost factors, or other efficiency advantages, but instead attributable to random differences (e.g. the conservativeness of each firms' engineers' estimates), we can estimate consistently the coefficient of interest by instrumenting total wins with close wins.

In this approach, our first stage takes the form of Equation 2.3. Here $EXP > 0^k_{it1}$ is an indicator for contracts won in period 1 of slice t for firm i, while $EXPCLOSE > 0_{it1}$ is an indicator for a close win in the same period, and ν_{it} is an error term uncorrelated with $EXPCLOSE_{it}$. The second stage is shown in Equation 2.4.

$$EXP_{it2} > 0 = \delta EXPCLOSE_{it} > 0 + T_t + \nu_{it} \tag{2.3}$$

$$S_{it2} = \tilde{\beta} EXP_{it2} > 0 + T_t + \varepsilon_{it} \tag{2.4}$$

The consistency of the estimates obtained require as the assumptions of i) validity and ii) rank conditions. The validity condition is satisfied if $E((EXPCLOSE_i > 0) \cdot W_i)$=0, i.e. close wins are uncorrelated with the omitted advantage variables in the OLS regression. As we argued, a close win, if correctly identified, can be attributed to noise, rather than fundamental differences between firms. For example, risk-aversion differences between firms, random approximation differences between engineering teams. The Data section examines how close wins are identified and labeled as such in practice with the data.

The rank condition, on the other side, requires correlation between experience and close experience. Indeed, by construction, we expect this condition to be fulfilled. The data section show results that prove that this condition is fulfilled.

If conditions i) and ii) are fulfilled the estimates $\hat{\hat{\beta}}$ and $\hat{\hat{\gamma}}$ are expected to be consistent. However, estimates should not to be interpreted as a single treatment effect. The treatment effect should be heterogeneous, mainly because:

- Experience itself is heterogenous given the complexity, length and size of a project, so it is expected that treatment effects are also heterogenous.
- Firm's absorptive capacity and learning ability depends on internal skill, financial strength and other organizational variables.
- More experienced firms should see diminishing returns to experience.

Following the discussion of (Angrist and Imbens, 1995) as presented in (Hansen, 2009), we argue that the estimation strategy identifies the Local Average Treatment Effect (LATE) for our treatment. This is a weighted average treatment effect for the firms who only get the extra experience due to a close wins (i.e. "compliers"). For example, a firm in this sample could be a firm not efficient enough to win a contract through its cost advantages alone.

This interpretation, additionally to rank and validity, also requires a monotonicity condition, that here is equivalent to having no firms negatively impacted in their experience by experiencing a close win. This condition is satisfied in our setting, since a close win belongs by construction to the set of all wins.

Having discussed the theoretical rationale and consequences of employing close wins as instruments, the problem remains of how to successfully find close wins and label them as such, which is analyzed in subsection 2.2.2. Two alternatives are proposed: first, find contracts with very close wins where price was heavily weighted, and second, develop a ranking measure of firms to find "balanced" auctions.

2.2 Data

2.2.1 Construction of the regression dataset

Our starting dataset consists in a set of bids submitted by firms in auctions developed by the government in Chile between 2010 and 2020 for construction projects. The source and main characteristics of the dataset employed in the investigation were detailed in the previous chapters and the Table 2.1 repeats some relevant descriptive statistics. The main purpose of this section is to describe the process employed to create the dataset employed in the estimation of equations 2.1 and 2.2.

Table 2.1: Sample Descriptive Statistics

name	N	Complete Cases	mean	std	max	min
Bid (all)	153000	1	7.92e+10	2.61e+13	1e+16	0
Winning Bid	38500	1	2.52e+08	2.39e+09	2.47e+11	0.6
Difference between 1st bid and 2nd (%)	38500	0.707	0.0933	0.162	1	0
Number of Bidders per Contract	49400	1	3.1	3.09	466	1
Year	49400	1	2016	3.19	2021	2010
Offers made by Firm	15500	1	9.83	27.9	1980	1
Win prob. by Firm	15500	1	0.216	0.3	1	0
Offers won by Firm	15500	1	2.48	6.13	146	0

The creation of the analysis sample requires defining distinct periods of time to compute experience and outcomes. We proceed by fixing several points in time t's in the sample, spaced by one year each, starting one year after the earliest date in the dataset. For each t, we employ a period immediately before it to compute experience (Period 1) and a period immediately after to compute outcomes (Period 2). We call a pair Period 1 - Period 2 a *slice* and we index it by t. For every firm in a slice, we link experience computed with method one or two in the first period (period 1) to the outcomes in the next period (period 2), to form an individual observation.

The period of outcome computation is always two years. On the other side, the period of experience computation depends on the experience measure employed. Recall that we compute experience as either rolling experience or annualized cumulative experience. Rolling experience computes experience as the total amount of contracts won in a fixed period of length σ previous to t, comprising the period $[t-\sigma, t]$ before the outcomes period $[t, t+\tau]$. As our baseline, we set $\sigma =$ two years.

The second alternative (annualized cumulative experience) computes experience cumulatively by summing contracts developed from the start of our bid dataset (i.e the year 2010) up until time t and dividing this number by the number of years since the firm's first win. That is, the experience computation period is $[2010, t]$ for every t. Instead of restricting our measure of past experience to two years before the outcomes' period, as in the previous method, we consider all the previous years when counting contracts won. We call this computation strategy annualized cumulative experience.

We end up with two datasets (one for each k) where each observation is the linked experience-outcomes pair for a firm i at a given t, the dependent variable is a measure of the firm's outcomes in Period 2 (i.e. S_{it2}), and the independent variable is a measure of the (past) experience of the firm in Period 1 (i.e. $EXP^k_{it},\ EXP^k_{it} > 0,\ k = 1, 2$).

We make an important filtering step before computing outcomes, as we only consider contracts for which previous experience is not among the awarding criteria to choose the winner. This is because including contracts for which experience is among the awarding criteria would i) render (expectedly) trivially positive and significant results and ii) confound the true effect of learning by doing among contracts which do not include experience as awarding criteria. Note that this filtering step is only carried out for outcomes' computation and not for experience computation.

The diagram in Figure 2-1 shows a toy example of how we transform the data from per-firm/period to a per firm/slice dataset. The original firm-period level dataset has, for every period, the contracts bid for and contracts won. The second dataset aggregates these results by slice. Note that this diagram assumed that no contracts had experience as part of awarding criteria.

After the transformation steps, we obtain ten slice-firm datasets for each measure of experience. Tables 2.2 and 2.3 show the dates of period 1 and period 2 for each slice, their lengths in years, and the number of observations in each slice. Recall that every observation has the pair firm-slice as a unique identifier and contains as key variable the past experience and a summary of future outcomes, having the form of the rightmost table in Figure 2-1.

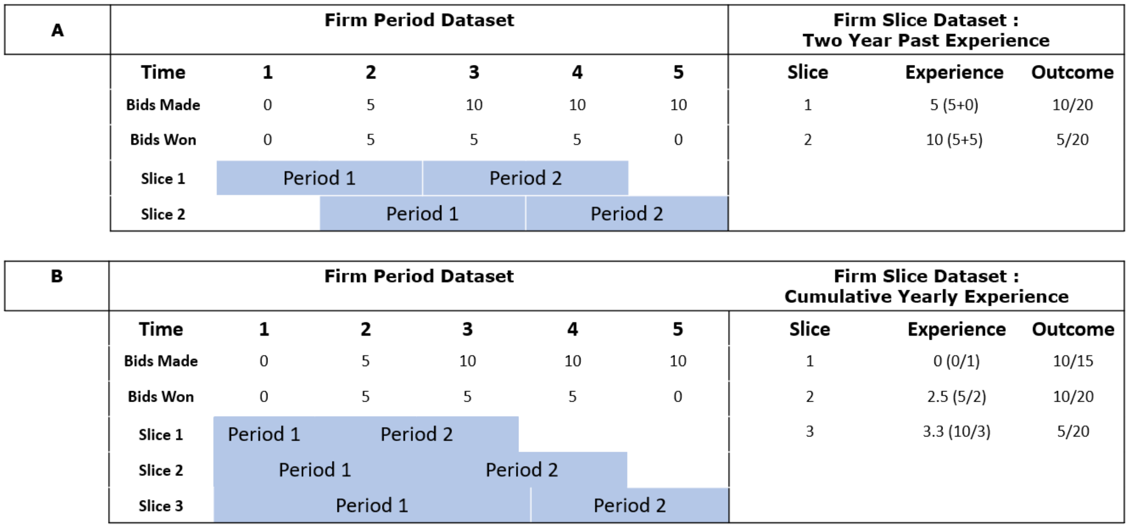


Figure 2-1: Example computation of slice-firm dataset, employing two-year fixed periods of past experience (A), and cumulative yearly experience (B).

Table 2.2: Analysis dataset characteristics for experience computed in rolling periods of two years

Slice	Period 1 dates	Period 2 dates	Observations	Length Period 1	Length Period 2	Contracts in Period 1	Contracts in Period 2
1	2010-01-04/2012-01-04	2012-01-04/2014-01-04	2485	2	2	6056	2994
2	2011-01-04/2013-01-04	2013-01-04/2015-01-04	2391	2	2	8360	2465
3	2012-01-04/2014-01-04	2014-01-04/2016-01-04	2515	2	2	8470	2771
4	2013-01-04/2015-01-04	2015-01-04/2017-01-04	2682	2	2	7870	2993
5	2014-01-04/2016-01-04	2016-01-04/2018-01-04	2585	2	2	9425	2588
6	2015-01-04/2017-01-04	2017-01-04/2019-01-04	2300	2	2	9978	2061
7	2016-01-04/2018-01-04	2018-01-04/2020-01-04	2183	2	2	9007	1806
8	2017-01-04/2019-01-04	2019-01-04/2021-01-04	2230	2	2	8637	1900
9	2018-01-04/2020-01-04	2020-01-04/2022-01-04	1577	2	2	9212	1198

Table 2.3: Analysis dataset characteristics for experience computed as cumulative annualized

Slice	Period 1 dates	Period 2 dates	Observations	Length Period 1	Length Period 2	Contracts in Period 1	Contracts in Period 2
0	2010-01-04/2011-01-04	2011-01-04/2013-01-04	2334	1	2	2393	2892
1	2010-01-04/2012-01-04	2012-01-04/2014-01-04	2485	2	2	6056	2994
2	2010-01-04/2013-01-04	2013-01-04/2015-01-04	2391	3	2	10753	2465
3	2010-01-04/2014-01-04	2014-01-04/2016-01-04	2515	4	2	14526	2771
4	2010-01-04/2015-01-04	2015-01-04/2017-01-04	2682	5	2	18623	2993
5	2010-01-04/2016-01-04	2016-01-04/2018-01-04	2585	6	2	23951	2588
6	2010-01-04/2017-01-04	2017-01-04/2019-01-04	2300	7	2	28601	2061
7	2010-01-04/2018-01-04	2018-01-04/2020-01-04	2183	8	2	32958	1806
8	2010-01-04/2019-01-04	2019-01-04/2021-01-04	2230	9	2	37238	1900
9	2010-01-04/2020-01-04	2020-01-04/2022-01-04	1577	10	2	42170	1198

The next section describes how to identify and label close wins, which is a key part of the identification strategy.

2.2.2 Definition of a close win

We discuss what would be the optimal way of finding close wins, and, since the data does not allow us to employ this strategy, we propose two second-best alternatives. The optimal way to identify close wins would be to single out auctions for which the winning firm had a final weighted score which was marginally superior to the ones of its competitors. Recall that, for each contract, the firm's proposals are scored in several criteria. The scores are then weighted and added up to produce the total score for that firm. Unfortunately, the optimal strategy described above is unfeasible with the data we have available, since our data only allows us to see the criteria employed in each contract and the weight of each factor, but not the individual scores for each firm. We attempt two alternative methods detailed in the subsections below.

Close wins by price

This method of identifying close wins relies on the fact that price is a major awarding criteria, as was shown in the Data section. Wherever price is a major awarding criteria, cost advantages play a part in the outcome of the bidding procedure as more efficient contractors can submit lower bids than its competitors. The strategy is the to single out auctions where price is a major factor and bids where close. This way, cost advantages can plausibly be disregarded as the cause behind a win. This strategy should indeed capture a subset of the random wins, namely, random wins in projects where price is the major awarding criteria.

Close wins are operationally identified as the wins where i) the winning bid was not more than .05% below the second lowest win, if the winner had the lowest bid, ii) the winning bid was not more than 0.05% below the lowest bid, if the winner did not submit the lowest bid and iii) the weight of the price item in the awarding decision was more than 50%.

This definition of close wins leads to approximately 2% of winning bids being classified as a close one. In Table 2.4 we examine whether close wins defined as above are different from the population in several types of metrics. We can see that in most aspects these bids have less dispersion in variables such as participants and less size. These might because of fat tails in the distributions of sizes and participants. However, the size and number of participants are both relatively close.

Table 2.4: Comparison between close and non-close wins, by price

Variable	Mean (Not close win)	Mean (Close win)	Sd (Not close win)	Sd (Close win)
Bid (all)	8.06e+10	2.14e+08	2.63e+13	7.45e+08
Winning Bid	2.53e+08	1.87e+08	2.41e+09	7.21e+08
Difference between 1st bid and 2nd (%)	0.0957	0.00216	0.164	0.00155
Number of Bidders per Contract	3.08	3.96	3.1	2.36
Year	2016	2015	3.19	3.08

The rank condition, required for consistent estimates, is verified in the first stage regression of normal wins (either as a binary indicator or continuous) on and indicator of close wins. For the rolling experience computation, the F-Statistic of this regression is 118.2 for the indicator treatment and 285 for the continuous measure. The coefficients of close experience are positive and significant at $p < 0.01$. The details are included in Appendix **??**.

Close wins by rank

The second strategy to identify close wins does not rely in prices or any other aspect of the bid itself. Instead, we label a winning bid as a close win if all the firms involved in the auction were close in ranking. The argument here is that, given a well constructed ranking, winning a contract against closely placed opponents should be attributable to random factors.

Obviously, the main issue is how to construct a good ranking measure. We proceed by modeling each auction as a multi-player game event (in the non-economic sense of the term) in which firms gain points by winning the project and lose points by not winning it. We award and substract points based on a modified Elo algorithm suited for multi-player games.

Each firm has its ranking initialized at a pre-specified level (1,500 for the main analysis). Then, it is awarded 25 points for winning against a similar opponent and subtracted 8 by losing. The implementation of the algorithm recommends that points awarded and subtracted sum to zero, so we fix awarded points and choose subtracted points so that on average (given the number of players in an auction) this condition holds. Against non-similar opponents, the algorithm makes a correction on points awarded and subtracted based on the ranking of the players and the outcome of the game.

Proceeding from the oldest to the most recent auction, we update the initial rankings for each firm and obtain for each firm its ranking at any point in time. Next, we label a win as a "close win" when the highest rank among the bidders for the auction was not more than 3% higher than the lowest rank among the same set of bidders. This yields around 6,900 closely won contracts (13.7% of the wins in the analysis sample) which corresponds to 20,924 observations (14% of the observations in the analysis sample). In Table 2.5 we present summary statistics for close wins identified via rank.

Table 2.5: Comparison between close and non-close wins

Variable	Mean (Not close win)	Mean (Close win)	Sd (Not close win)	Sd (Close win)
Bid (all)	9.17e+10	4.61e+08	2.81e+13	1.08e+10
Winning Bid	2.52e+08	2.5e+08	2.22e+09	3.03e+09
Difference between 1st bid and 2nd (%)	0.0914	0.0987	0.164	0.157
Number of Bidders per Contract	3.11	3.03	3.29	1.42
Year	2016	2014	3.14	3.19

In the analysis, we drop the first two years of data to allow for a period of rank adjustment. This is necessary since the algorithm does not work well when the average rank in the population is not clearly defined. The way ranks evolve as time progresses can be seen in Figure 2-2. Note that ranks appear highly concentrated at the end of the first year of data, while they are much more dispersed at the end. In the robustness checks we analyze both i) different values for the won/lost points after an auction and ii) the threshold in ranking for a close win.

More details for this strategy, regarding the theory, algorithm and results, can be found on Appendix **??**.

The rank condition, required for consistent estimates, is again verified via a regression of experience on close experience, now defined via rank. For the rolling experience case, the F-Statistic of this regression is over 2,000 for the indicator treatment and over 1,000 for the continuous treatment. Also, we find positive and significant coeffi-

Figure 2-2: Evolution of ranks by selected years

cients of close experience on experience. The details are included in Appendix **??**.

2.3 Main Results

First we explore graphically the relationship between experience and outcomes. Figure 2-3 shows the relationship between rolling (top row) and annualized (bottom row) measures of experience and outcomes. Each column represents a different subsample and dependent variable. The first column (panels A and D) selects all firms and displays past experience in the x-axis. The second column (panels B and E) contains only firms with equal experience and close experience (including zero). The x-axis displays the close wins. The third column (panels C and F) is analogous to column two but employs the definition of a close win as close win by firm rank.

We observe that average winning shares increase with more experience. The effect appears to be close to linear, although for experiences higher than ten contracts performed (rolling) or five contracts performed (annualized) we have wide error bars or no observations available. In the case of our "reduced form" graphs, we observe that almost always the close wins seem to improve average winning shares, although we observe wide error bars in the second column, caused by the low amount of observations that fulfill the conditions imposed.

Next we show the results from our regression analysis. Table 2.6 shows the results for OLS and IV regressions for our first experience measure (i.e. rolling two year periods) while Table 2.7 shows the results for our second measure of experience (i.e. annualized experience). The first three panels in each table employ as treatment the binary indicator of experience, whereas the last three panels employ total experience.

The OLS estimate of the effect of having experience on winning proportion is 0.07 for rolling experience and 0.06 for annualized experience. IV estimates of the coefficient are very close to OLS counterparts or even higher, for the case of annualized experience. The specification with linear returns on experience shows that experience renders a 0.01 and 0.03 increase in winning share per extra contract developed (for rolling and annualized experience respectively). IV estimates of linear effect of experience are again close to OLS counterparts. Finally, all the estimates for the experience treatments are significant at $p < 0.01$ with robust standard errors.

A concerning result is the low R^2 of the regressions, which shows that although the effect of experience on the mean outcome is significant, there is much variability among firms' outcomes which is not explained by the increase in experience.

Given the average winning shares (approximately 0.23), depending on the sample), the effect of having experience is equivalent to an increase of around 30% of the winning share of a firm (i.e. around 7 percentage points out of 23 percentage points). This points towards significant importance of previous experience in future outcomes.

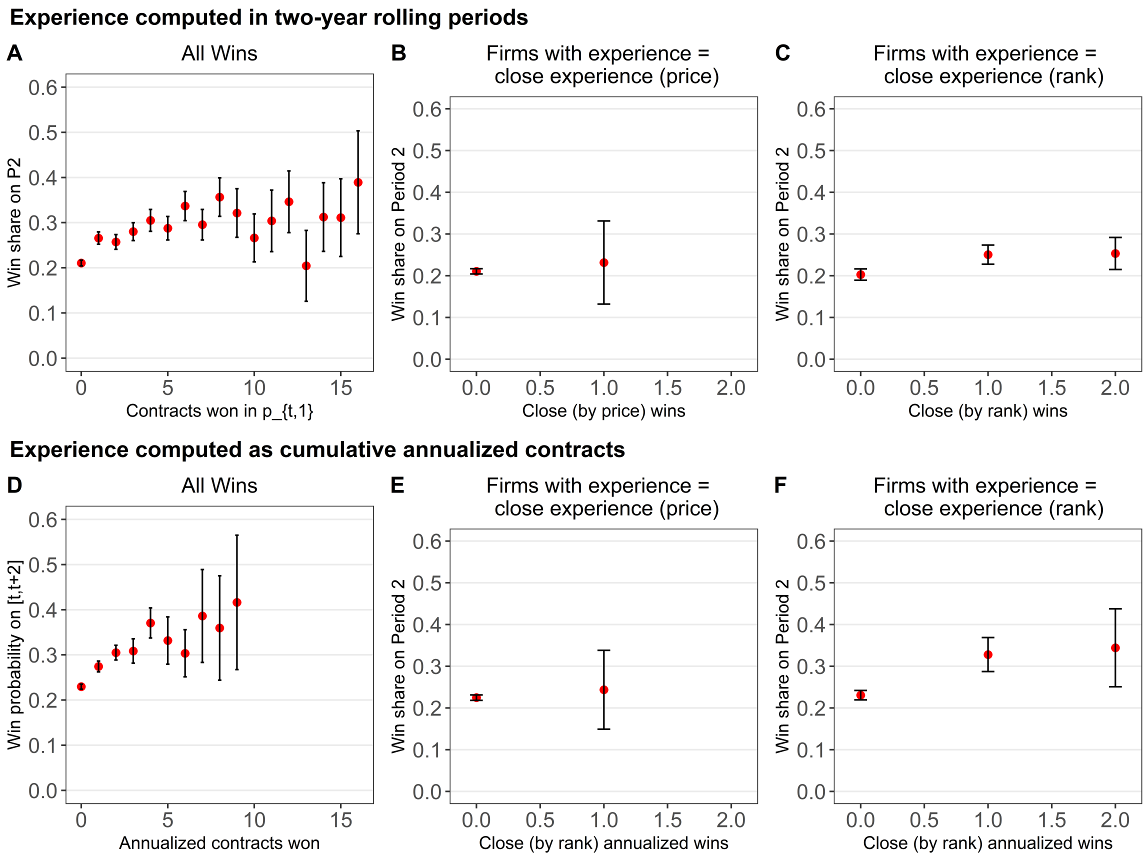


Figure 2-3: Relationship between contracts won on $t-1$ and mean winning probability across contractors in t.

Note: The plots show the mean across firms of the number of contracts won out of the number of contracts bid for in period t (in the y-axis), against experience accrued in period $(t-1)$ in the x-axis. t and $t-1$ correspond to two periods of two years each for the top row, for the bottom row t is also a period of two years, but $t-1$ are all years in the interval $[2010, t]$. Error bars correspond to means plus/minus two standard errors. First column: all sample observations are considered. Second column: only contractors with experience = close experience. Third column: analogous to second column employing the rank definition of close win. The first row definition of experience is rolling experience while the second row employs cumulative annualized experience.

Table 2.6: Regression for OLS and IV specifications with Experience computed in rolling 2-year periods

	Dependent variable:					
	Share of Contracts won in t					
	OLS	IV (Price)	IV (Rank)	OLS	IV (Price)	IV (Rank)
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.074***	0.063***	0.082***			
	(0.005)	(0.019)	(0.007)			
Experience in (t-1) (Linear)				0.010***	0.007***	0.017***
				(0.001)	(0.002)	(0.001)
Constant	0.258***	0.262***	0.237***	0.273***	0.277***	0.245***
	(0.007)	(0.010)	(0.008)	(0.007)	(0.008)	(0.007)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,948	20,948	16,072	20,948	20,948	16,072
R^2	0.018	0.017	0.017	0.015	0.014	0.010

Note: *p<0.1; **p<0.05; ***p<0.01

The regression analyzes the relationship between past experience (dependent var.) and future winning shares in the market (indep. var.). The winning share is contracts bid / contracts won in the next two years. Experience is contracts won in the past two years. The instrument for panels 2,3,5 and 6 is a binary indicator for a closely won contract among the past contracts won. Robust (HC1) error are on parethesis.

Table 2.7: Regression for OLS and IV specifications with Experience computed as annualized cumulative experience

	Dependent variable:					
	Share of Contracts won in t					
	OLS	IV (Price)	IV (Rank)	OLS	IV (Price)	IV (Rank)
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.061***	0.079***	0.084***			
	(0.005)	(0.016)	(0.013)			
Experience in (t-1) (Linear)				0.027***	0.024***	0.032***
				(0.002)	(0.005)	(0.005)
Constant	0.282***	0.278***	0.251***	0.284***	0.286***	0.264***
	(0.008)	(0.009)	(0.013)	(0.008)	(0.008)	(0.012)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,705	21,705	12,327	21,705	21,705	12,327
R^2	0.016	0.016	0.012	0.016	0.016	0.015

Note: *p<0.1; **p<0.05; ***p<0.01

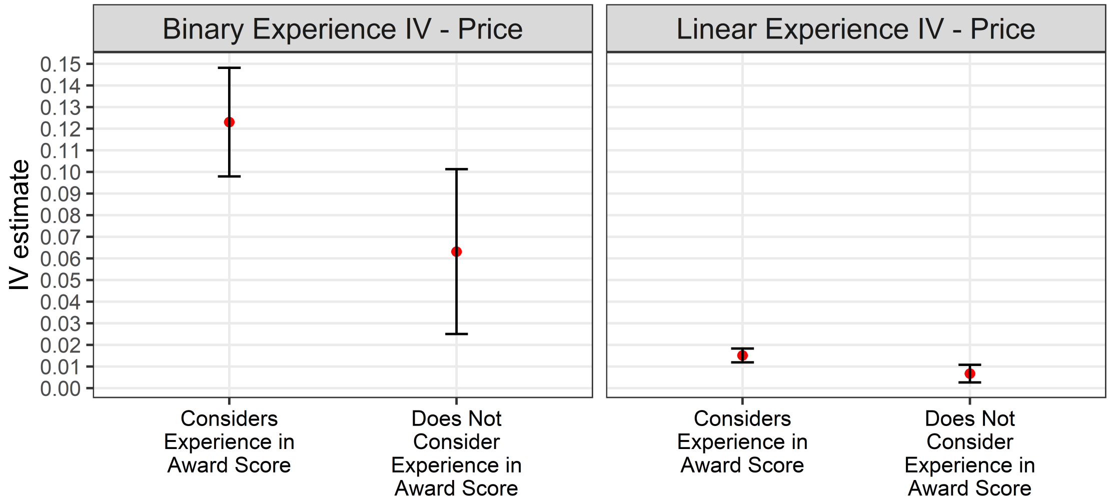
The regression analyzes the relationship between past experience (dependent var.) and future winning shares in the market (indep. var.). The winning share is contracts bid / contracts won in the next two years. Experience is contracts won since the beggining of the sample, divided by number of years since the first win. The instrument for panels 2,3,5 and 6 is a binary indicator for a closely won contract among the past contracts won. Robust (HC1) error are on parethesis.

2.3.1 Comparing with contracts that do include experience in awarding score

We compare the main results obtained in the previous section with the results obtained by considering for outcome computation only contracts which *do* require experience in the awarding criteria. This helps to put the results in context and also serves as a validation check of the empirical strategy. We expect to find greater estimates for the effect of experience on outcomes among contracts which explicitly reward experience.

Figure 2-4 shows the estimate from the IV specifications, both with linear and binary functional forms of experience, by the type of contract considered to compute outcomes (we only employed rolling experience). It can be seen that the effect of experience on outcomes is about twice as big in contracts which do consider experience as a factor in the awarding criteria with respect to those who do not.

Figure 2-4: Comparison between estimates obtained in contracts with and without experience in the awarding criteria employed by the government



Note: The plot shows a comparison between the estimate of a regression of winning shares on various forms of experience (instrumented with close wins >0), considering on the a-xis two types of analysis samples. The first considers only contracts that did not include experience in the awarding criteria (left of each panel). The second considers only contracts without experience in the awarding criteria (right of each panel). Close wins are defined by price (see text). Error bars are confidence intervals at 95% with robust standard errors.

2.4 Robustness checks

Several of the parameters in the empirical strategy of the previous section admit more than one reasonable choice. This section considers alternatives for them. Robustness checks are studied for the following parameters:

1. Periods of outcome computation.

2. Definition of a close win (by price).

3. Definition of a close win (by rank).

2.4.1 Periods of outcomes

In the main analysis, we computed outcomes across a period of two years for each of our slices. This choice is sensibilized by computing outcomes in one and three year periods as well. While varying the length of the period where outcomes are computed, the procedures to compute experience are kept the same as before.

A shorter timeframe would be a better parameter choice if: firms bid frequently, so their true outcomes manifest quickly; learning is itself instantaneous, so past experience immediately influences outcomes; or the learning effect is short lived, which would make much more important for the outcomes the recent history. Conversely, a longer time frame is better in the case of infrequent bidding, slow learning, and long lasting knowledge.

For construction projects, it is expected that the better parameter would be more close to a longer timeframe than to a shorter one. Construction projects, especially complex ones, can be less frequently auctioned than in simpler, undifferentiated products. More importantly, since construction projects take longer to perform than regular purchases, it is reasonable to expect a longer learning process.

Table 2.8 shows estimated experience coefficients where outcomes were computed in periods of 1, 2 (the original specification) and 3 years. The rows correspond to OLS, IV (by price) and IV (by rank) specifications. Notably, i) all results are significant with $p < 0.01$ and ii) estimates are close to each other across different values of the parameter. Standard errors decrease with the number of years considered because of the increase in sample size. In almost every case, estimates remain within one and a half a standard error of the original estimates, and in all cases they remain within two standard errors.

2.4.2 Definition of a close win - Price IVs

In the main section, close wins by price were defined as those in which the winning contractor submitted a bid that i) was not more than .05% below the second lowest win, if he had the lowest bid, ii) was not more than 0.05% below the lowest bid, if he

Table 2.8: Robustness analysis for the coefficient on Experience (Rolling) by length of outcome computation period

Experience Computation	Specification	1 year outcomes	2 year outcomes (Main)	3 year outcomes
Indicator	IV-Price	0.098 (0.028) ***	0.063 (0.019) ***	0.07 (0.017) ***
Indicator	IV-Ranks	0.075 (0.014) ***	0.082 (0.01) ***	0.081 (0.009) ***
Indicator	OLS	0.076 (0.006) ***	0.074 (0.005) ***	0.07 (0.004) ***
Linear	IV-Price	0.009 (0.003) ***	0.007 (0.002) ***	0.008 (0.002) ***
Linear	IV-Ranks	0.014 (0.003) ***	0.017 (0.002) ***	0.017 (0.002) ***
Linear	OLS	0.009 (0.001) ***	0.01 (0.001) ***	0.012 (0.001) ***

Note: ***p<0.01,**p<0.05

did not submit the lowest bid and iii) the weight of the price item in the awarding decision is more than 50%. In this section the main estimates are sensibilized to different values of the threshold parameter and the weight parameter.

We first sensibilize the threshold for bid differences for the linear estimate of experience in the rolling experience measure. The plot in Figure 2-5 displays the coefficient of interest and 95% confidence as we vary the threshold for a close win. For thresholds below .025%, we obtain much wider standard errors. The reduction in sample size for the instrument is significant below .05%, since this percentage is already at around the 15th percentile of bid differences in the sample. However, we keep significant outcomes at p=0.05 for all values analyzed.

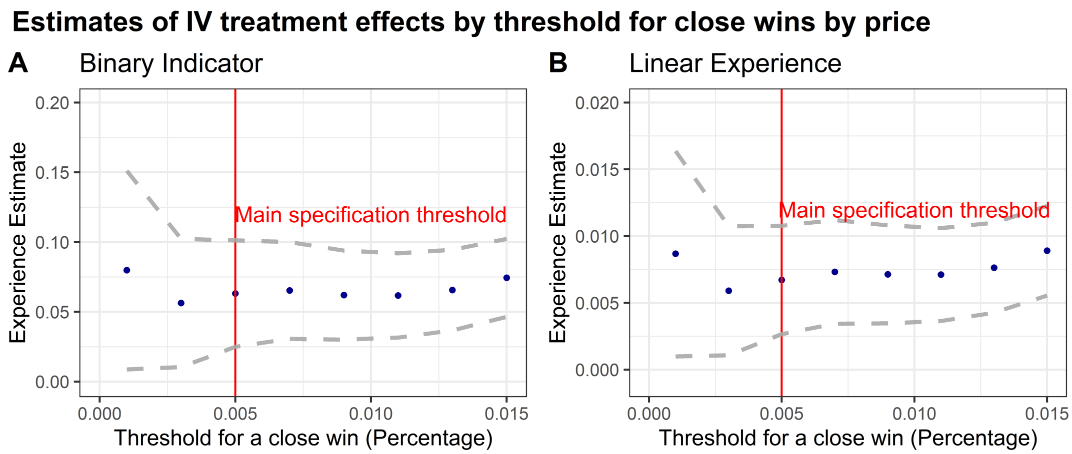


Figure 2-5: Robustness analysis for threshold of close wins

Note: The plot shows the coefficient on experience as in the specification of Panels 4 (left) and 5 (right) of table 2.6, that is, the dependent variable is the share of contracts won in period t and the independent variable is an indicator of experience or linear experience. Experience is instrumented with close wins > 0 in period $(t-1)$. The x-axis shows how the coefficient varies with the threshold for the maximum percentage difference between the winner's bid and the closest bid of interest.

Next we examine the parameter for the weight of the price component in the total score. We replicate our main IV-price results but consider weights of 60%, 70%, and 80% as the minimum weights of the price component in the factors considered to evaluate proposals. Table 2.9 shows the results. At 60%, most results remain significant, but beyond 70% almost all results are not. Since 60% is the 80th percentile of the score weight across contracts, we have again a sample size problem for the instrument when there are higher requirements for the threshold of the price weight.

Table 2.9: Robustness analysis for the price weight parameter in the IV Regression by price

Experience Computation	Functional Form	50	60	70	80
Annualized	Binary Indicator	0.079 (0.016) ***	0.079 (0.019) ***	0.059 (0.023) ***	0.051 (0.031)
Annualized	Linear	0.024 (0.005) ***	0.024 (0.006) ***	0.017 (0.007) ***	0.015 (0.009)
Rolling	Binary Indicator	0.063 (0.019) ***	0.059 (0.024) **	0.028 (0.028)	0.045 (0.04)
Rolling	Linear	0.007 (0.002) ***	0.006 (0.003) **	0.003 (0.003)	0.004 (0.004)

Note: ***p<0.01,**p<0.05. Numeric columns show the weight required for the price parameter to be considered a close win.

2.4.3 Definition of a close win - Rank IVs

The IV-Rank estimates are sensibilized by choosing alternative thresholds for the maximum difference between the highest and lowest bidder's rank (bandwidth) and different values for the points awarded for a win. Recall that an auction is labeled as close in the main specification if the difference in rank between the highest and lowest ranked in the auction is less than 3%. In the main specifications, 25 points are awarded for a win and eight are subtracted for a loss.

We analyze bandwidths of 1%, 2%, 3% and 4%. Regarding points for a win, we analyze as alternatives 10, 15, 25, 35 and 50 points. Again, to preserve stability, points subtracted for a loss are approximately a third of the points awarded for a win. Since average bidders are close to three, we divide awarded points by three to obtain subtracted points

Given the amount of possible parameter combinations, results are shown in graphic form in Figure 2-6 and they only consider the first type of experience computation (rolling). Results show that IV estimates are robust to all the alternatives considered. Considering a lower thresholds for the difference in ranks does increase the standard errors. However, estimates do not vary much, staying close to .075 for a binary indicator of experience as treatment and to .012 for the total experience treatment.

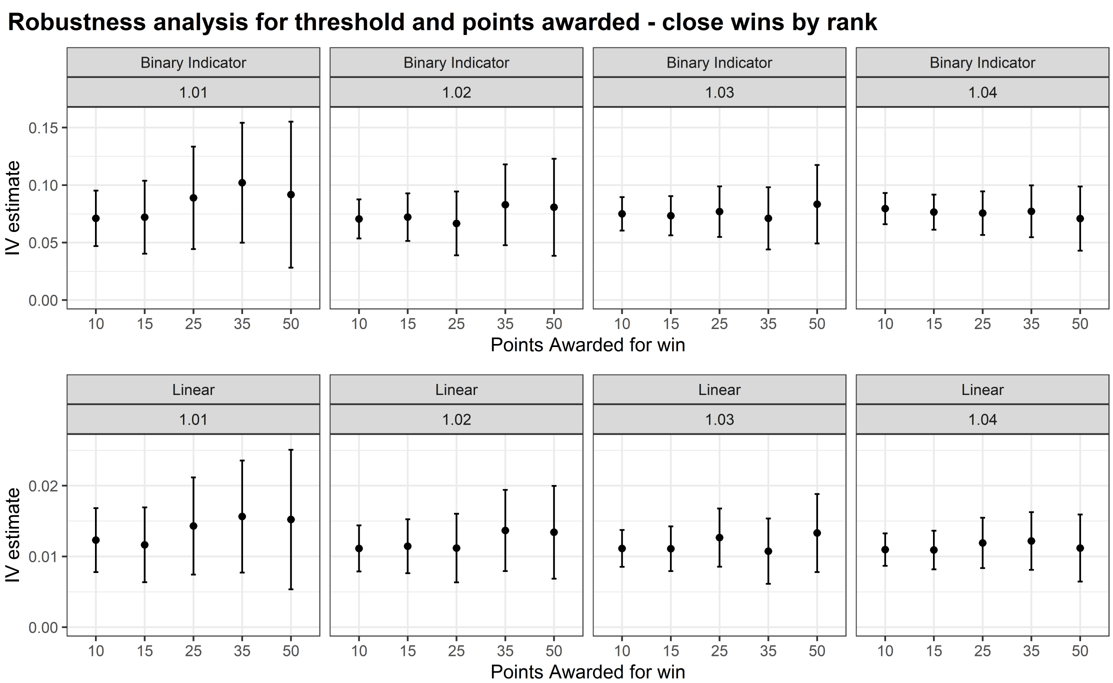


Figure 2-6: Robustness analysis for parameters in the IV-Rank strategy

Note: The plot shows the robustness analysis for the parameters of the rank instrument strategy. The first parameter sensibilized is the bandwidth allowed between the highest ranked and lowest ranked participants in the auction (secondary top box). The second parameter (in the x-axis) are the amount of points awarded for a win. The primary top box shows the type of treatment (i.e. binary or continuous).

3. Operational Mechanisms of Experience Improvement

Having established positive and significant treatment effects of experience on outcomes in the market for public construction projects, we seek to investigate how does experience operate in practice to produce improved outcomes in the treated firms. The objective is to provide evidence of some of the changes that might have taken place within firms and helped them achieving a higher rate of success.

We start presenting the following working hypothesis regarding the benefits of experience among firms. Each details one way in which a firm might have experienced improvements. The chapter objective is to test these hypothesis as well as possible with the data available.

First we present our hypothesis:

1. H1: experience produces improvements in cost measures in the firm, keeping constant the type of project. This improvement in cost operates either via economies of scale, since after winning the project the firm is bigger than before; or via adjustments in the production function itself, for example, by changing the relative inputs employed to produce a unit of the product.

2. H2: experience allows the firm to produce at higher quality than before, constant the cost of the works. This improvement operates because the firm, having performed certain tasks once, is able to better predict potential problems, and adapt accordingly. For our purposes, we hypothesize that the technical quality of the firm's *proposal* improves, and we assume that this is in direct correlation with executed quality.

Section 3.1 investigates the first hypothesis while Section 3.2 investigates the second. In each section the data is characterized and the empirical strategy discussed, before showing the results. Most of these elements are very similar to their previous chapter counterparts so the expositions are brief.

3.1 Bids and experience

This section investigates whether experience causes improvements in cost levels for treated firms. We approach this hypothesis by examining how do firm's bids evolve after the firm has been treated, i.e. after it has acquired experience. We assume that bid amounts are a non-decreasing function of bids' costs, which seems a plausible assumption.

The relationship between bids and several firms characteristics has been investigated several times in the construction and economics literature, which is discussed in the Literature Review. Previous studies have generally found aggressiveness in new entrants, but also reduced bids from incumbents. The identification strategy employed is, to our knowledge, novel.

3.1.1 Data

Our main dataset is the same as in the previous chapter, i.e. a set of bids submitted by firms in auctions for public construction projects. However, instead of aggregating firm's experience and outcomes in time slices, our observations are the bids themselves, so the original unit of observation (i.e. the bid) is kept. We still employ aggregation to compute previous experience at each point in time for every firm. Similarly as before, contracts where experience is employed in the awarding factors of the contract are filtered out from the analysis sample, but not from experience computation).

Furthermore, the first year in the data is excluded from the regression sample, since all firms have zero experience at this point and keeping it would introduce noise in the estimates due to spurious treatments set to zero. All the available years in the

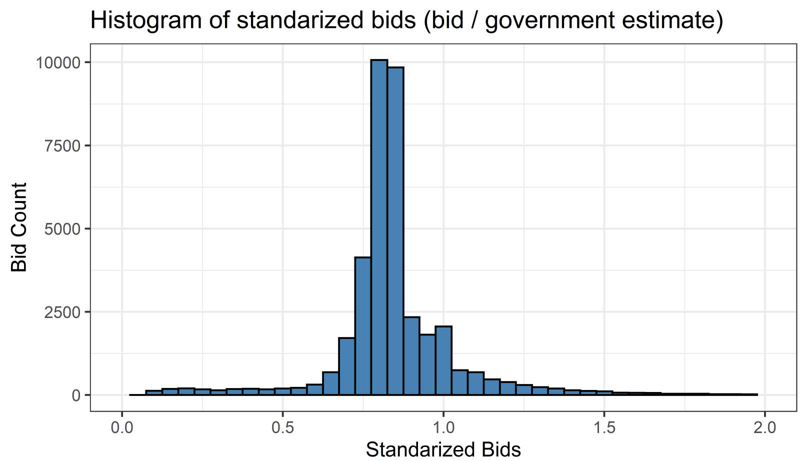


Figure 3-1: Histogram of standarized bids

data are employed to compute experience, as in the previous section.

The data includes two key variables for this section: bid amounts and a government estimate of how much the project "should" cost, called the official estimate. The estimate is prepared by the government unit in charge of the auction and usually disclosed after the auction has taken place. It is of interest for the government to produce a reasonable estimate, since if the winning bid is below a certain fraction of the official estimate, the government unit must undergo additional administrative steps to justify the awarding decision.

We produce comparable bid amounts across different contracts by dividing each bid by the corresponding government estimate, obtaining a new variable which we call standardized bid. This procedure helps to prevents some heteroskedastic effects, and also reflects that most effects in our regression are expected to act "per-dollar" unit of a contract (Bajari, Houghton, and Tadelis, 2014). We filter from the dataset standardized bids less than 0.1 and over 5.0, since they could correspond to outlier cases and not to a regular auctioning procedure or project, or could by a symptom of a very bad initial estimate from the government. This last step eliminates around 1,000 contracts. Figure 3-1 shows a histogram of standardized bid amounts (we restrict the visualization range for convenience).

Table 3.1 shows descriptive statistics of the observations employed in the analysis sample for this section. Note that there are modifications with respect to Table 3.1,

Table 3.1: Sample descriptive statistics for bid analysis

name	N	mean	std	max	min
Bid (all)	38700	7.52e+08	6.74e+09	2.54e+11	2500000
Winning Bid	10100	4.13e+08	4.44e+09	2.47e+11	4940000
Difference between 1st bid and 2nd (%)	10100	0.0735	0.0956	0.912	0
Number of Bidders per Contract	12500	3.2	2.42	33	1
Year	12500	2015	2.85	2021	2011
Offers made by Firm	7430	5.21	9.89	265	1
Win prob. by Firm	7430	0.232	0.325	1	0
Offers won by Firm	7430	1.36	3.12	64	0

given by the extra filtering steps employed for this analysis. .

3.1.2 Empirical Strategy

Our empirical strategy relies on a regression of the form:

$$BID_{ijt} = \alpha + \beta EXP > 0_{ijt} + X_j + FIRST_{ijt} + \varepsilon_{ijt} \tag{3.1}$$

$$BID_{ijt} = \alpha + \beta EXP_{ijt} + X_j + FIRST_{ijt} + \varepsilon_{ijt} \tag{3.2}$$

Here, the outcome variable BID_{ijt} is the standardized bid submitted by firm i at time t to contract j. Our treatment variable is experience, either in binary form $EXP > 0$ or continuous form EXP. We compute experience by summing all contracts won up to t. Each bid in our main dataset (after the filtering steps detailed above) is an observation in the regression. We add controls X_j corresponding to the region and year of the contract. Finally, we add an indicator variable $FIRST_{ij}$ which is 1 if firm i is on its first year in the market when bidding for contract j, because from the theorical analysis and empirical literature we expect a positive effect due to "aggressiveness" of first entrants.

Similarly as before, we expect to have unobserved cost variables, specific to each firm, which might bias estimates upwards due to positive correlation with experience. We repeat the same strategy as before to produce consistent estimates, using closely won bids to produce random variation in total experience. The setting is an IV regression where we instrument EXP_{it} with $EXPCLOSE_{it} > 0$, the presence of close wins by a firm up to time t. Wins are labeled as close wins if they fulfill the conditions established in the previous chapter. For brevity, we only employ rank instruments in this section.

The first stage (regression of wins on an indicator of close wins by rank) shows an F-statistic of 816 for the binary indicator and 370 for linear experience, which brings evidence to strong instruments.

Finally, to interpret our indicator estimate as the LATE, we again require a monotonicity condition, which is satisfied by construction.

3.1.3 Results

We show graphical results in Figure 3-2. Panel A shows standardized bids against experience, employing all bids and firms in the sample. It can be seen that the average bid for firms without experience (0.89) is higher than the average of firms with any amount of positive experience. Panel B shows only firms close win (by rank) equal to wins. Firms with one close wins (and no regular wins) submit bids that are slightly lower that those firms without experience, although firms with two close wins have bids around three percentage points lower.

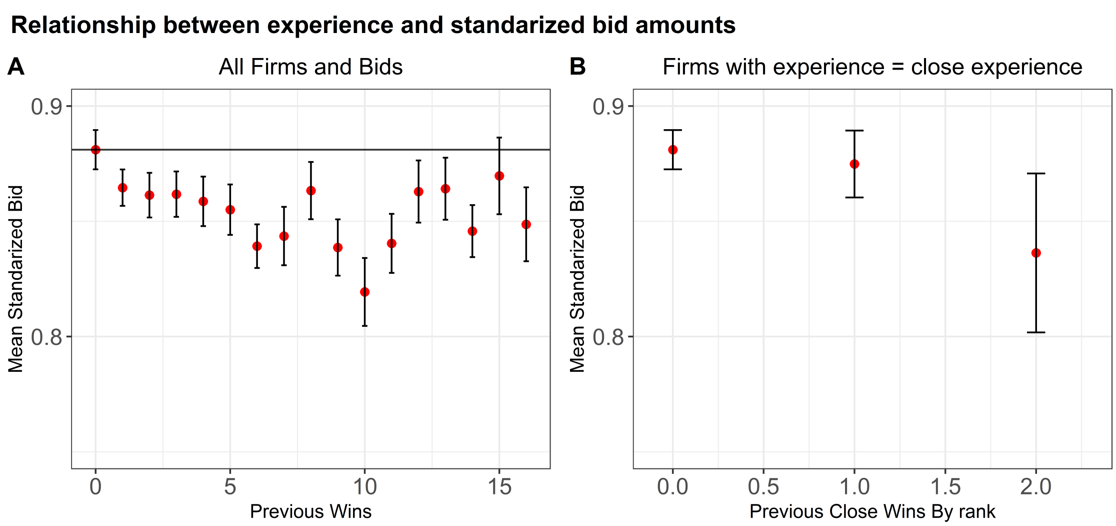


Figure 3-2: Relationship between experience and standardized bid amounts

Note: The plots show the relationship between experience and standarized bids (monetary bid amount/government's estimate of the cost). The x-axis show experience, either as total previous wins (left) or close wins(right). Range of the close wins in the right panel is cut at two.

We perform four regressions between experience and standardized bids. The first two are the OLS and IV results employing binary experience as treatment; while the third and fourth are the OLS and IV regressions employing total experience as treatment. Table 3.2 presents our main results. The OLS estimates of the effect of having experience on bid amounts is around -0.040 for OLS estimates and -0.038 for IV estimates. Although this is only around 15% of the standard deviation of the standardized bid, given that the average difference between the lowest and second lowest bid is around eight percentage points, the effect is relevant to auction outcomes.

The continuous OLS and IV estimates are small but significant. Notably, the IV estimate is twice as big as the OLS one at 0.001. We advance a possible explanation of this result based on our empirical strategy. This is further analyzed in the Discussion section.

Table 3.2: Regression of bid amounts to experience

	Dependent variable:			
	Standarized Bid			
	OLS	*instrumental variable*	*OLS*	*instrumental variable*
	(1)	(2)	(3)	(4)
Experience in (t-1) (Binary)	-0.040^{***}	-0.038^{***}		
	(0.005)	(0.010)		
Experience in (t-1) (Linear)			-0.0005^{***}	-0.001^{***}
			(0.0001)	(0.0002)
IndFirstYear	-0.019^{***}	-0.018^{***}	-0.009^{***}	-0.013^{***}
	(0.003)	(0.005)	(0.003)	(0.004)
Constant	0.858^{***}	0.856^{***}	0.825^{***}	0.828^{***}
	(0.011)	(0.014)	(0.009)	(0.010)
Fixed effects By Period and Region	Yes	Yes	Yes	Yes
Observations	38,714	38,714	38,714	38,714
R^2	0.025	0.025	0.023	0.023

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

The regression analyzes the relationship between past experience (dependent var.) and standarized bid amount (indep. var.). The standarized bid amount is the monetary bid amount divided by the government estimate of the cost of the project. We include an indicator IndFirstYear for firms that are bidding on their first year in the market. Robust standard errors are on parenthesis.

Our main hypothesis of interest, which was that experience produces cost advantages among treated firms, seems to be substantiated by the results. Although we cannot speak with certainty about the levels of the effect, we can conclude that experience does allow firms to submit lower bids as a source of competitive advantage. Results show treatment effects implying bids at least two percentage points higher on average for firms without experience compared with firms with strictly positive experience.

3.2 Quality and Experience

In order to test hypothesis number two, in this section we study if experience treatments causes firms to submit higher quality proposals. We proceed by analyzing whether experienced firms have higher proposal acceptance rates in the first stage of the awarding process, in which government units in charge of the auction discard proposals that do not fulfill basic formal requirements and/or technical specifications.

Recall that, for each auction, firm proposals are analyzed in two steps. The first step examines mostly if the proposals fulfill formal requirements. Formal requirements include the inclusion of required legal documents, submitting each of the technical documents asked for in the bidding documents, etc[1]. In essence, the first stage verifies that all proposals can be evaluated in equal terms and that the minimum legal requirements are fulfilled. Clearly, whether a proposal was accepted is a measure of its quality, albeit an imperfect one. Although it leaves out a significant part of the variation that would be expected in proposal's qualities, it is nonetheless an interesting measure of quality because formal acceptance is a necessary condition to win a contract.

Quality is explicitly evaluated in many contracts by including an item in the awarding criteria labeled as "technical specifications" or just "quality of the proposal". Employing string pattern matching, we estimate that around 30% of contracts include some measure of technical evaluation in the awarding criteria. Ideally, we would test the hypothesis that experience improves the quality of a firm's proposals by employing the score that each firm obtained in the technical or quality item of the evaluation criteria of the project. However, since our data has not this item available by firm, we must employ this alternative strategy.

Our research design, detailed below, tests whether experienced firms have a higher formal acceptance rate than unexperienced firms at the first stage of the awarding

[1] In some units/contracts, the first step can be a time-consuming and important part of the process. For example, in contracts from the Ministry of Public Works, the first step is called "Technical Evaluation" and examines thoroughly the bids to analyze if the technical standard of the proposal is adequate.

Figure 3-3: Histograms of proposal acceptance rate by firms in the dataset

process.

3.2.1 Data

We employ our bid dataset similarly as in the previous chapter. We create time slices exactly as detailed in Section 2.1 so we do not repeat the explanation of the full process. Each observation consists in the outcomes of a firm in period 2 of slice t and experience acquired during period 1 of the same slice t. Due to possible self-selection effects for firms with experience, contracts which include experience in the awarding factor are again we still filtered out for outcome computation.

To compute outcomes an indicator variable $INDACC_{ijt}$ is employed, which is 1 if the proposal submitted by firm i at time t for contract j is accepted or not. The aggregated outcome is the mean of this indicator variable across the proposals submitted during the outcome period.

We show a histogram of the acceptance rates in Figure 3-3. We can already see that the fraction of firms getting all proposals rejected decreases if we consider firms with more than one proposal, which could be caused by the effect of learning about the formal revision stage after the first few bidding processes.

3.2.2 Empirical Strategy

We test whether experience leads to a higher rate of formal proposal acceptance employing the following regression:

$$ACCRATE_{it2} = \alpha + \beta EXP_{it1} + T_t + \varepsilon_{it} \quad (3.3)$$

Here, $ACCRATE_{it2}$ is the share of proposals accepted out of proposals submitted in period 2 of slice t, EXP_{it1} is the measure of experience employed for firm i in slice t (gained in period 1), and T_t are period fixed effects. We employ indexes 1 and 2 to make explicit that each slice has two periods: one of experience computation and one of outcome computation, and every slice is indexed by t, which is date in between the two periods.

To be more explicit, let C_{itk} be the set of contracts where firm i submitted a proposal at period k of slice t. Then, the outcome variable $ACCRATE_{it2}$ can also be written as:

$$ACCRATE_{it2} = \frac{\sum_{j \in C_{it2}} INDACC_{ijt}}{|C_{it2}|}$$

We again expect unobserved cost advantages that are endogenous to experience, so we repeat our instrumentation of experience with the existence of strictly positive close wins the same as the previous chapter and section. Since we apply the same sample procedure as in the previous chapter, the same discussion and results regarding validity and rank apply.

We perform six regressions between proposal acceptance rates and experience. The first three are the OLS and IV results employing our binary treatment; and the third to sixth employ a continuous experience treatment. We employed our first alternative (rolling experience) to compute experience, i.e. we employ two year periods to compute experience and subsequent two year periods to compute outcomes. The first stage shows an F-statistic of 118 and 285, the same as in the main section for thistype of experience (as expected).

3.2.3 Results

Figure 3-4 displays graphic results. Panel A displays a clear discontinuity between the mean of the acceptance indicator variable for proposals sent by firms without experience and firms with any amount of positive experience. The mean acceptance rate for firms with no experience is .68, whereas it is equal or above .79 for proposals belonging to firms with positive experience.

To be more stringent with the sample, panel B displays the same analysis but here we leave out all firms except those which have only one previous proposal (won or lost), so they are new entrants to the market which may have won or lost their first contract (we analyze their next submitted proposal). Notably, mean acceptance rates increase from .74 ($N = 4,490$) for firms which lost their first auction to .87 ($N = 1,024$) for firms which won their first auction.

Furthermore, we find that, for observations in the fifth quintile of acceptance rate, 40% of them correspond to firms with strictly positive experience. On the other side, only 26% of the observations in the first quintile of acceptance come from firms with no experience (at the point of observation, since a firm can be in both quintiles at different points in time).

Panels C and D show the mean acceptance rate against close experience as per the instrument level. We consider only firms having equal experience to close experience. In Panel C, the instrument is close experience by price and in D the instrument is close experience by rank. In both panels, we see an increase in the mean acceptance rate, although the sample is so reduced in panel C that we obtain very big standard errors.

Our regression results are shown in Table 3.3. The first three panels show the results for binary experience as treatment and the last three the treatment is total experience. We find positive and significant treatment effects of experience on outcomes: having positive experience results in almost 11 percentage points higher mean acceptance rates in future proposals (next two years). This means that having experience increases acceptance rates in around a third of a standard deviation of the outcome variable (.33). The IV results are close to OLS estimates (between one and two percentage points higher), but the price instrument specification has high standard errors.

Regarding the treatment effect per unit of experience, we find that each new contract performed increases mean acceptance rates by around 1.2 percentage points.

Again, the IV results are higher than the OLS results for the two alternative instruments.

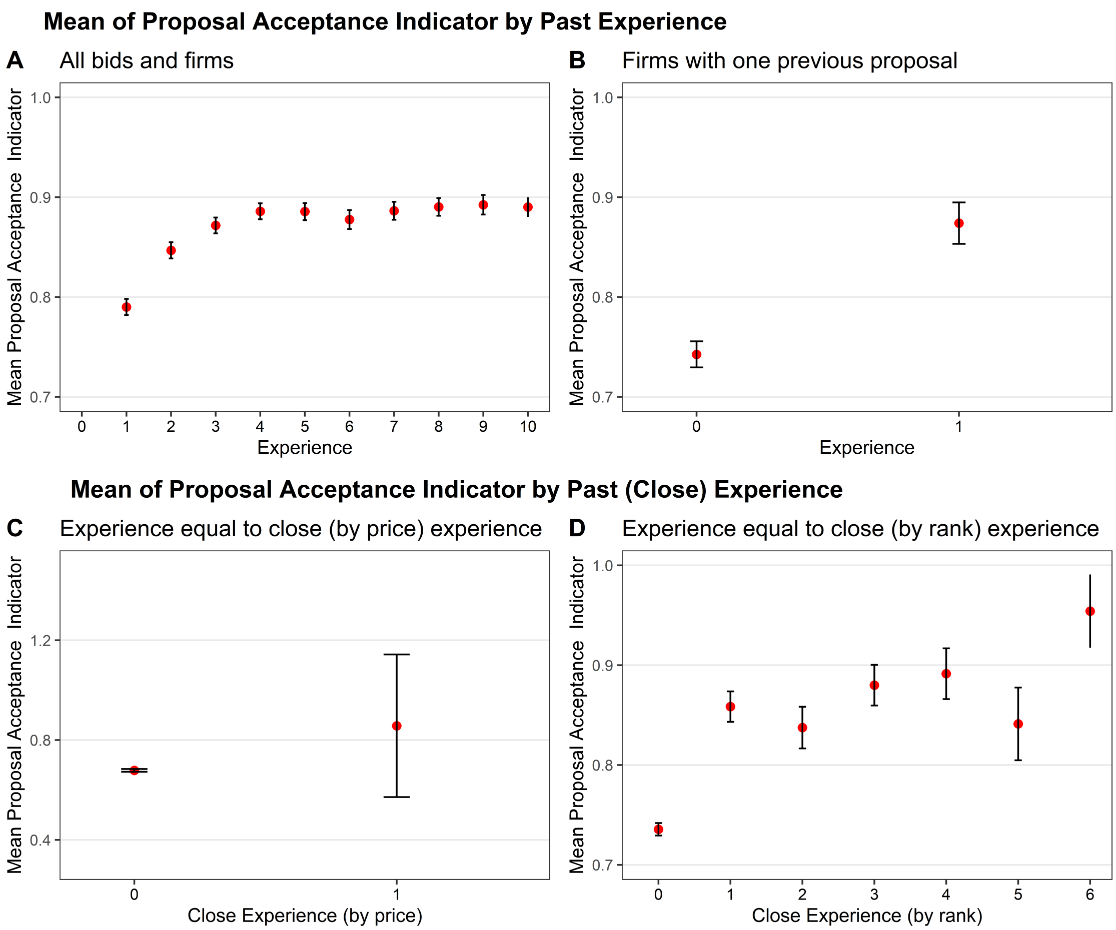


Figure 3-4: Acceptance rate for proposals sent by firms to auctions for public construction projects.

Table 3.3: Regression of proposal acceptance on experience

	Dependent variable:					
	Proposal Acceptance Rate					
	OLS	IV (by price)	IV (by rank)	OLS	IV (by price)	IV (by rank)
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) > 0 (Binary)	0.105***	0.127***	0.111***			
	(0.004)	(0.014)	(0.006)			
Experience in (t-1) (Continuous)				0.012***	0.014***	0.023***
				(0.001)	(0.002)	(0.001)
Constant	0.787***	0.779***	0.787***	0.811***	0.809***	0.799***
	(0.007)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
Fixed effects By Period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,948	20,948	13,559	20,948	20,948	13,559
R^2	0.025	0.024	0.024	0.013	0.013	−0.001

Note: *p<0.1; **p<0.05; ***p<0.01

Results of a Regression of proposal acceptance on experience. The independent variable is contracts won in the previous two years(rolling experience). The dependent variable is the rate of proposal acceptance over the next two years, i.e. (proposals accepted by the government unit in charge of the auction) / (proposals sent by the firm) . The proposals are accepted or rejected at the first stage of the awarding decision, where formal and/or technical requirements are checked.

4. Discussion

4.1 Experience and Outcomes

Winning rates of firms with experience were successfully showed to be superior to the winning rates of firms with no experience. We found an increase of between seven and nine percentage points in winning rates for the treatment with any experience, and between .05 and 2 percentage points for every extra unit of experience. The magnitude of these effects seems to be relevant for the overall outcomes of a firm, since the mean rate of success when bidding is around 22%. That means experience can render almost a 30% improvement in future outcomes, measured as contracts won out of contracts bid for.

Our instrumental variables approaches to obtain consistent estimates were very different between them but rendered similar results. The first relied on close wins identified by close competition on price, while the second relied in finding contests between "similar firms", via a ranking algorithm. The advantage of the price strategy is that it is more interpretable, however, the conditions imposed were so stringent that the resulting "complier" sample was very small. The rank strategy is less interpretable, but theoretically it should control for any unobservable factor that influences firm's outcomes, not just cost advantages. The major weakness of the rank strategy is the necessity of an adjustment period for newcomers, so ranks for first entrants (which are the most important ones) are less precise than those of firms which have been longer in the market. Additionally, having an additional set of parameter (points awarded for win and lose) can make the strategy less robust.

Interestingly, the IV strategies rendered almost always higher estimates than the

OLS, when the original hypothesis was that an upwards bias would be found. Two points can be mentioned to explain this. First, the experience measure (contracts won, in any of its forms) is a noisy measure of experience, since actual learning or improvements depends highly on the size of the contract, type of project, etc. Then, there is an attenuation effect in the OLS estimates.

The second possibility is that there is a selection effect which takes out firms from the market when they are unable to gain experience. In principle, the effect of experience on entry and exit is uncertain. If the environment is too hostile, for example, and firms encounter a high level of bureaucracy in their contracts, experience might induce exit. However, if firms perceive returns to experience, we should see increased exit among non-experienced firms. In the latter case, the treatment effect of experience underestimates the true returns to experience, since firms in the market survive precisely because of the it. The OLS estimates underestimate the true effect of experience because we do not observe outcomes for firms that were unable to gain experience and had to abandon the market following defeats in the auctions. We briefly show in plot how exits disaggregated in terms of % of firms that exit with and without experience per year.

A limitation of the analysis for the binary treatment is that it was only able to identify the Local Average Treatment Effect, which in the current context is interpreted as the treatment effect for those firms that can only acquire experience through a close win. Given our restrictive instruments' definition, this feature of the distribution of the causal effects is only applicable to a small part of our observations (between 2% and 15%, depending on the instrument). However, this is arguably the most important subsample, because in it there are firms that would achieve significant improvements after acquiring experience. Also, this discussion could show more evidence as to why we obtain higher IV than OLS estimates. Given the choice of the instrument, a firm that would only win in a close win should not have an absolute advantages in the market already, so it has more room to "grow".

The comparison of estimates for the treatment effect of experience between contracts that explicitly rewarded experience and those that did not (the main results) is relevant because it shows that the implicit effect of experience on outcomes is almost 60% as the explicit effect. The explicit estimate of the treatment effect of any experience was around twelve percentage points, while for contracts that did not require it was seven percentage points. Given this, policymakers might prefer to only employ experience as a prequalification method, since it seems to largely keep operating in the case of no explicit reward for experience.

We found low R^2 in our regressions which shows that there is considerable heterogeneity in the outcomes. This can be attributed to the fact that we employed minimal types of controls in the regressions and wide array of types, locations, buyers and sellers. An alternative strategy would have been to i) add more controls or ii) consider a more restrictive market. Option i) was not employed because the sample is unbalanced in many ways and also because we do not have detailed contract description variables that could have been employed as controls. Option ii) could be used to obtain a more precise estimate in a clearly defined subsample, like contracts that need prequalification in the Ministry of Housing or Public Works. However, for these two government units the information was either incomplete or there was not a clear way to distinguish more "restricted" contracts beyond size.

4.2 Operational Mechanisms of Experience

The mechanisms section's objective was to test hypothesis about the improvements caused by experience in treated firms. Two possibilities were examined: improvements in cost measures, measured by the level of standardized bids submitted; and quality levels, measured by the rate of acceptance of offers in a stage of the procurement process that verifies fulfillment of formal and/or technical requirements in proposals.

Firstly, the hypothesis that experience causes reduction in cost measures was tested. It was found that bids of firms with more than zero experience were between three and four percentage points lower than those that did not have any. The average difference between lowest and second lowest firms is around seven percentage points, so the impact can be significant if there is a binary reward to the lowest bid submitted.

In this investigation, unlike most of our tests, we found linear experience to not have a significant coefficient. This might because this analysis employed total experience, with no adaptations such as annualizing or considering shorter periods. Given that at the last observations we have firms with very high measures of experiences (>100 contracts) it is expected that due to diminishing returns a linear return on experience is not the best choice.

Is an improvement of three percentage points truly useful to win more contracts? The results on lower bid amounts were significant, but it could be argued that the wide amount of factors employed to award projects render the effect negligible. However, a quick regression of the winning outcome (0-1) of the auction (for each firm that submitted a proposal) on standardized bids, with the usual fixed effects (see Appendix for details) shows that for every ten less percentage point on bid amounts, winning probability increase by around 2 percentage points. Thus, there at least correlation between lowered bid submitted and winning probability.

The result that first entrants bid more aggressively than firms with more than one year in the market was in line with previous literature results. Notably, the net effect of experience and first entry shows that an experienced firm still submits lower bids on average than first entrants.

The second hypothesis examined was that experience improves the quality of the proposals that a firm submits for auctions. The acceptance rate of proposals in the formal check stage of the procurement process was employed as a quality measure. We found that firms with strictly positive experience have acceptance rates that are around ten percentage points higher than firms with no experience. This effect is relevant considering that the average rate of acceptance is around 80%, so the effect of experience drives acceptance rates close to 90%.

It could be argued that the effect observed corresponds only to an adaptation experienced naturally after participating in the first "trial" auctions and that it only comes from bidding instead of experience. However, the analogous treatment effect of *bidding experience* on outcomes is less than the effect of experience (details on the Appendix). While there seems to be a component of the effect related to "knowing the market", the effect of experience goes above and beyond this.

A remark should be made regarding the assumption that improved acceptance rates are related to improved quality. The improved quality identified in the result should be interpreted narrowly here as a better consideration of formal requirements in the proposal. A reasonable assumption is that all quality aspects of a bid are correlated and then that this relates to overall improvements in quality measures for the firm.

Overall, we mostly discussed costs measures, bids and quality as evolving due to within-firm changes. In this context, increased winning rates and improved acceptance rates are "positive". However, a part of these outcomes could be related to rent-seeking and capture of the market, by knowing "tricks" that inexperienced firms do not, or even corruption. The existence of legal rules and the employment of a digital platforms constructed to prevent communication or knowledge of bidders before the awarding decision should diminish the opportunity for these types of situations. Still, we cannot completely rule them out.

Another regrettable omission of the data is the lack of comprehensive data for the Ministry of Public Projects. While on absolute numbers the contracts of this government unit are less to the ones of municipalities, for example, because of their complexity and size they are expected to have high returns to experience and of more interest. This organism started publishing their data comprehensively only since 2017, so before that year the data is incomplete.

4.3 Implications for the market

The magnitude of the effects found for experience could work as an entry barrier for new entrants to public construction projects. However, the econometrical interpretation of our treatment effects allows only us to say that firms that get in the market because of "random" wins improve in their outcomes. In that sense, the results points towards experience as an entry barrier for firms without strong comparative advantages in the market *ex ante*.

The effect on the competitiveness on the market (only considering the treatment found) is then to limit the rise of "bad" firms which would become "good" with some experience. While this would be probably an undesirable feature in a private market, it could be argued that public markets should be focused on procuring goods as efficiently as possible. Then, depending on the tolerance to the distortion of preventing some firms to develop in the market, a policymaker might not be troubled by the results observed.

A possible effect suggested by the results is double-counting when considering experience in the award criteria. As it was seen, almost 60% of the contracts include experience in the awarding criteria. However, it was also seen that more experience contractors already display qualities that make them more likely to win projects, like lower cost measures and better proposals. Then, from a competitive perspective, it could be better to rise technical or economical requirements to award the project but diminish the experience requirement (which as it was discussed is also a noisy measure of skill). If experience is truly a desirable property, we expect its effects to manifest in other aspects of the proposal that will make experienced candidates more likely to be awarded the project anyways.

5. Conclusion

The paper's objective was to understand the treatment effect of experience on outcomes in the market of public construction projects. The investigation analyzed around 150,000 bids from 43,000 calls for proposals to compare the rate at which firms with and without experience win contracts in the future. It also analyzed possible mechanisms that would explain improved outcomes for firms with experience: a diminution of firm's cost measures, as measured by standardized bids submitted; and improved proposal quality, as measured by the acceptance rate of firm's proposals in a stage of the procurement process where formal requirements are processed.

The results pointed towards significantly improved outcomes in the future for firms with previous experience. Experienced firms win more contracts, bid more aggressively, and submit better quality proposals (as measured by their acceptance rate). The identification strategy, although limited in scope, renders significant and mostly precise estimates of the relevant parameters, for a subset of the firms that acquire experience only in close contests.

This investigation is relevant to the literature in bidding, public purchases and industrial organization because of its wide scope and empirical findings. The data employed spans a whole country, most of the public purchases developed in the construction sector and more than ten years of data. Regarding the empirical contributions, this investigation adds a dynamic component to the static investigation of auction competition. Also, it treats experience as an endogenous variable, developing a causal analysis of the influence of experience on market outcomes, while the existing literature usually develops OLS regressions. The size of the sample allows to identify with precision a feature of the causal effects distribution, namely, the Local Average

Treatment Effect. The same strategy allowed to gain insight into actual operational differences that give firms advantages, bringing together both economics and engineering analysis.

Given the sizable impacts identified for firms that acquire experience through close contracts, the results are relevant for policymakers aiming to improve the competitiveness of public markets and those looking to improve the design of public auctions.

Finally, the work could be complemented in the future by a general model of bidding in the public sector. The heterogeneity of outcomes that we found shows that this effort would require to improve in the characterization of both units and contracts to yield detailed results at the firm level. On a separate field, the variables constructed would be also useful to construct a machine learning approach to detect suspicious awarding decisions to improve overall procurement transparency and efficiency.

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