Learning by Doing in Public Construction Contracts

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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.

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1. 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 firms learn by doing how to perform better public contracts, becoming 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 proposals it wins out of the proposals that it bids for in the future. 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 1 presents the data, Section 2 the empirical strategy, Section 3 the results and Section 4 performs robustness checks.

1.1 Data

Our 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. The Table 1.1 shows descriptive statistics for the sample employed.

Table 1.1: 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	38200	1	2.53e+08	2.4e+09	2.47e+11	0.6
Difference between 1st bid and 2nd (%)	38200	0.705	0.0911	0.16	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	0.214	0.299	1	0
Offers won by Firm	15500	1	2.46	6.09	146	0

1.2 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 the coefficients.

Our two main OLS specification are presented in equations 1.1 and 1.2. Here, S_{it2} is the share of contracts won in period 2 of slice t, EXP^k_{it1} and $EXP^k_{it1} > 0$ 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.

$$S_{it2} = \alpha + \beta_k(EXP^k_{it1} > 0) + T_t + \varepsilon_{it} \tag{1.1}$$

$$S_{it2} = \alpha + \gamma_k EXP^k_{it1} + T_t + \varepsilon_{it} \tag{1.2}$$

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.

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.

Now we describe our treatment variables. We employ as treatment variables i) an indicator of past experience $EXP_{it1}^k > 0$ and ii) total experience $EXP_{it1}^k > 0$. Moreover, we consider two ways of *computing* the total experience EXP_{it1}^k for a firm i, which we index by k, $k \in \{1, 2\}$. The first alternative computes experience as total amount of contracts won in a fixed period of length σ, comprising the period $[t - \sigma, t]$ before the outcomes period $[t, t + \tau]$. As our baseline, we set $\sigma =$ two years. We call this computation strategy rolling experience.

The second alternative computes experience cumulatively by summing contracts developed up until time t and dividing this number by the number of years since the firm's first win. 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 experience.

For each firm/slice we link experience computed with method one or two (period 1 of the slice) to the outcomes in the next period (period 2 of the slice). We end up with a dataset (for each k) where each observation is a firm-slice pair, 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. $EXP_{it}^k, EXP_{it}^k > 0,\ k = 1, 2$).

Finally, we obtain additional slices by creating experience-outcomes pairs at several t's in time, spaced by a year each. Since our dataset contains 10 years, we end up with five period 1/period 2 pairs (i.e. slices) employing rolling experience and six

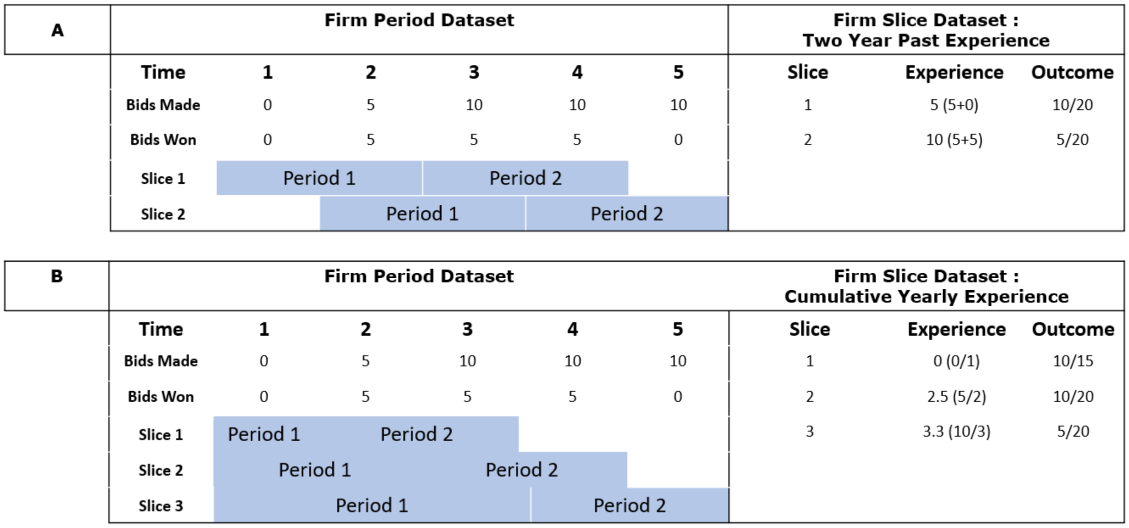

Figure 1-1: Example computation of slice-firm dataset, employing two-year fixed periods of past experience (A), and cumulative yearly experience (B).
Note:

pairs employing annualized experience.

The diagram in Figure 1-1 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 no contracts had experience as an awarding criteria.

After the transformation steps, we obtain ten slice-firm datasets for each measure of experience. Tables 1.2 and 1.3 show the amount of observations in each slice by the type of experience measure employed. Recall that every observation is a firm-level aggregate of past experience and summary of future outcomes and has the form of the rightmost table in Figure 1-1.

Table 1.2: 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 1.3: 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

1.2.1 Endogeneity and Identification

We discuss two problems in the causal interpretation of equations 1.1 and 1.2: endogeneity and heterogenous effects. We then present the empirical approach to identify consistently a feature of the distribution of treatment effects, the Local Average Treatment Effect.

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 estimate $\hat{\beta}, \hat{\gamma}$ in 1.1 and 1.2 to be biased upwards due to correlation (expectedly positive) between omitted cost variables and the amount of past experience.

To estimate consistently the treatment effect of experience on outcomes, we employ external variation 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 (experience). If we are able to find wins where the success of a firm is less or not at all attributed 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 1.3. 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 and 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 1.4.

$$EXP_{it2} > 0 = \delta EXPCLOSE_{it} > 0 + T_t + \nu_{it} \tag{1.3}$$

$$S_{it2} = \beta EXP_{it2} > 0 + T_t + \varepsilon_{it} \tag{1.4}$$

Both measures of experience (EXP and $EXPCLOSE$) should be correlated since every extra unit of experience increases the probability of having at least one close win, fulfilling this way the rank condition. Moreover, close wins should not be correlated with cost measures, as they are attributed to random factors, such as risk-aversion differences between firms, random approximation differences between engineering teams in each firm, etc. and thus ensuring a valid instrument as well.

Even tough our estimate $\hat{\beta}$ is consistent, we do not expect to identify the a single Treatment Effect because treatment effects should be heterogenous:

- 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 for our binary treatment, i.e. $EXP > 0.$, i.e. the average treatment effect for the firms that are affected by the experience treatment if and only if they win a contract by chance (i.e. "compliers"). 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 identification for the instrument of close wins, the problem remains of how to successfully find close wins and label them as such, which is the purpose of the next sections. 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. Both are discussed and analyzed in the next sections.

1.2.2 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 proposals from firms are scored in several criteria, weighted, and finally summed to produce the total score for that firm. Unfortunately, the previous strategy is unfeasible with the data we have available. 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

In this method, close wins are operationally identified as the wins where i) the winning bid was not more than .05% below the second lowest win, if he had the lowest bid, ii) the winning bid was not more than 0.05% below the lowest bid, if he did not submit the lowest bid and iii) the weight of the price item in the awarding decision is more than 50%.copulatively two conditions: i) the price weight in the awarding decision criteria is 50% or higher and ii) the difference between the lowest bid and the second lowest bid is less than .05%. This way of identifying close wins should indeed capture a subset of the random wins, namely, random wins in projects where price is the major awarding criteria.

This definition of close wins leads to approximately 2% of winning bids being classified as a close one. In Table 1.4 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.

Table 1.4: 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 is verified via a regression of experience on close experience. The F-Statistic of this regression is 118.2 for the indicator treatment and 1,500 for the continuous measure.

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 subtract 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 in the initial version). 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 5,800 closely won contracts (11% of the contracts in the analysis sample) which corresponds to 17,000 observations (11% of the observations in the analysis sample). In Table 1.5 we present summary statistics for close wins identified via rank.

Table 1.5: Comparison between close and non-close wins

Variable	Mean (Not close win)	Mean (Close win)	Sd (Not close win)	Sd (Close win)
Bid (all)	1.53e+10	5.74e+11	5.44e+12	7.57e+13
Winning Bid	2.51e+08	2.57e+08	2.21e+09	3.22e+09
Difference between 1st bid and 2nd (%)	0.0913	0.101	0.164	0.157
Number of Bidders per Contract	3.11	3.01	3.25	1.39
Year	2016	2014	3.15	3.18

In the analysis, we drop the first year 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 1-2. 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.

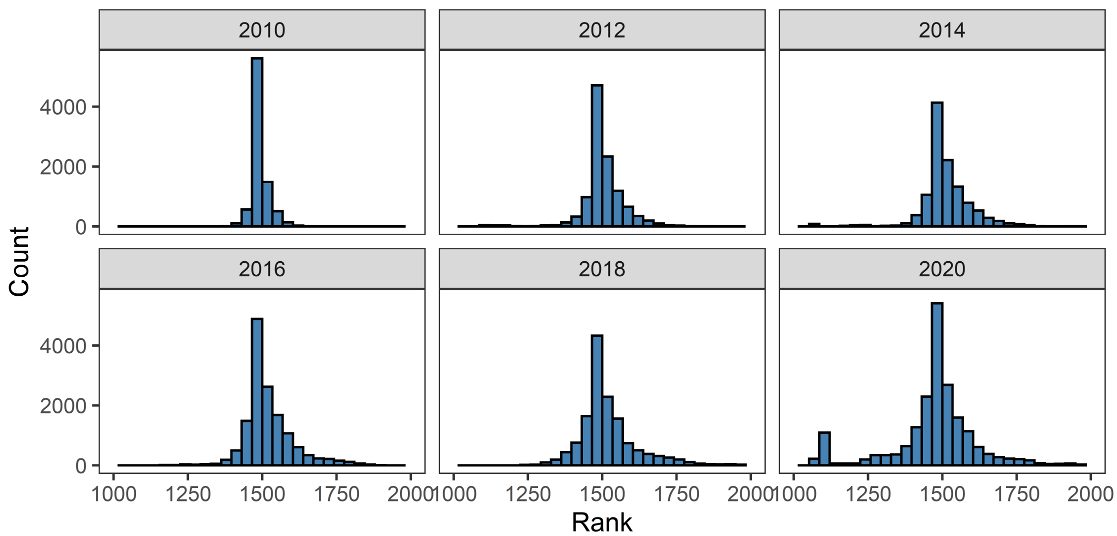

Figure 1-2: Evolution of ranks by selected years

1.3 Main Results

First we explore graphically the relationship between experience and outcomes. Figure 1-3 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 1.6 shows the results for OLS and IV regressions for our first experience measure (i.e. rolling two year periods) while Table 1.7 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, almost 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 (0.2), the effect of having experience is equivalent to an increase of almost 30% of the winning share of a firm (i.e. around 7 percentage points out of 21 percentage points). This points towards significant importance of previous experience in future outcomes.

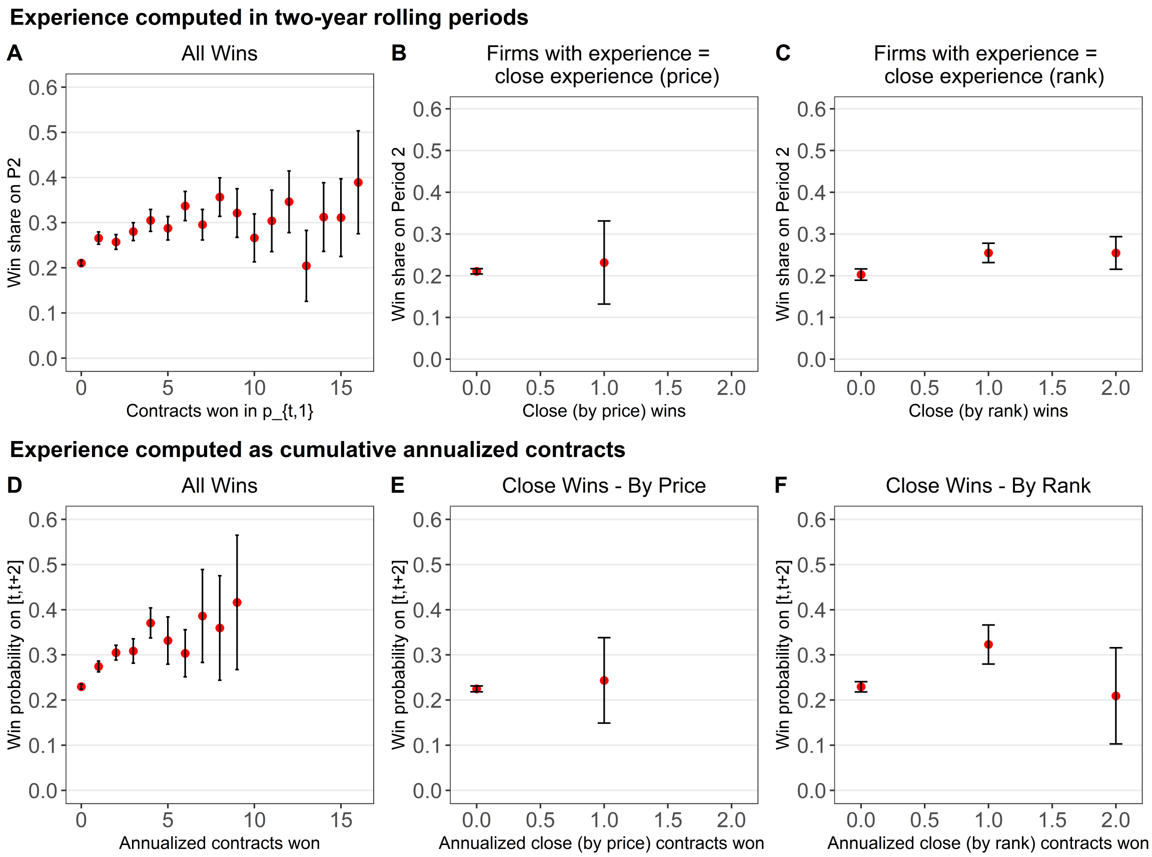

Figure 1-3: Relationship between contracts won on $t-1$ and mean winning probability across contractors in t.

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 second row employs cumulative annualized experience.

Table 1.6: Regression for OLS and IV specifications with Experience computed in rolling 2-year periods

	Dependent variable:					
	Share of Contracts won in t					
	OLS	*instrumental variable*		*OLS*	*instrumental variable*	
	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***	0.080***			
	(0.005)	(0.007)	(0.007)			
Experience in (t-1) (Linear)				0.010***	0.006**	0.011***
				(0.001)	(0.002)	(0.001)
Constant	0.258***	0.262***	0.237***	0.273***	0.278***	0.253***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(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	0.013	0.013
Residual Std. Error	0.344 (df = 20938)	0.344 (df = 20938)	0.339 (df = 16064)	0.345 (df = 20938)	0.345 (df = 20938)	0.339 (df = 16064)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 1.7: Regression for OLS and IV specifications with Experience computed as annualized cumulative experience

	Dependent variable:					
	Share of Contracts won in t					
	OLS	*instrumental variable*		*OLS*	*instrumental variable*	
	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***	0.080***			
	(0.005)	(0.014)	(0.014)			
Experience in (t-1) (Linear)				0.027***	0.021***	0.021***
				(0.002)	(0.006)	(0.005)
Constant	0.282***	0.278***	0.254***	0.284***	0.288***	0.277***
	(0.007)	(0.012)	(0.013)	(0.007)	(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	0.013	0.016	0.016	0.016
Residual Std. Error	0.346 (df = 21695)	0.347 (df = 21695)	0.334 (df = 12317)	0.347 (df = 21695)	0.347 (df = 21695)	0.333 (df = 12317)
Note:						*p<0.1; **p<0.05; ***p<0.01

1.3.1 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 1-4 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.

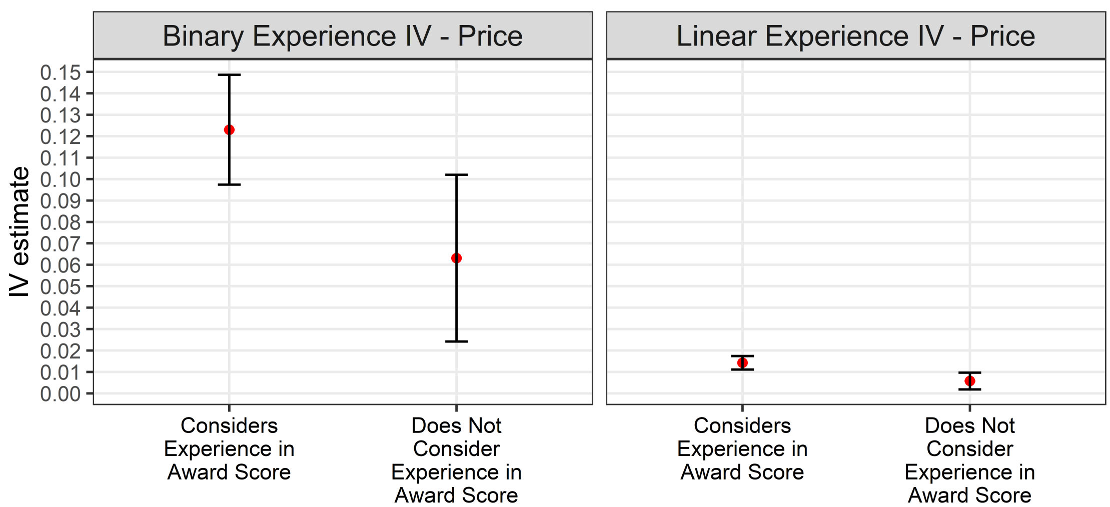

Figure 1-4: Comparison between estimates obtained in contracts with and without experience in the awarding criteria employed by the government

1.4 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).

1.4.1 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 1.8 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 a standard error of the original estimates, and in all cases they remain within two standard errors.

Table 1.8: 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	0.095 (0.028) ***	0.061 (0.019) ***	0.067 (0.017) ***
Indicator	IV-Ranks	0.067 (0.014) ***	0.077 (0.011) ***	0.078 (0.009) ***
Indicator	OLS	0.075 (0.006) ***	0.073 (0.005) ***	0.069 (0.004) ***
Linear	IV-Price	0.007 (0.002) ***	0.006 (0.002) ***	0.007 (0.002) ***
Linear	IV-Ranks	0.01 (0.002) ***	0.013 (0.002) ***	0.014 (0.002) ***
Linear	OLS	0.009 (0.001) ***	0.01 (0.001) ***	0.012 (0.001) ***

1.4.2 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 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 1-5 displays the coefficient of interest and 95% confidence as we vary the threshold for a close win. For thresholds below .25%, we obtain much wider standard errors. The reduction in sample size for the instrument is significant below .5%, 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.

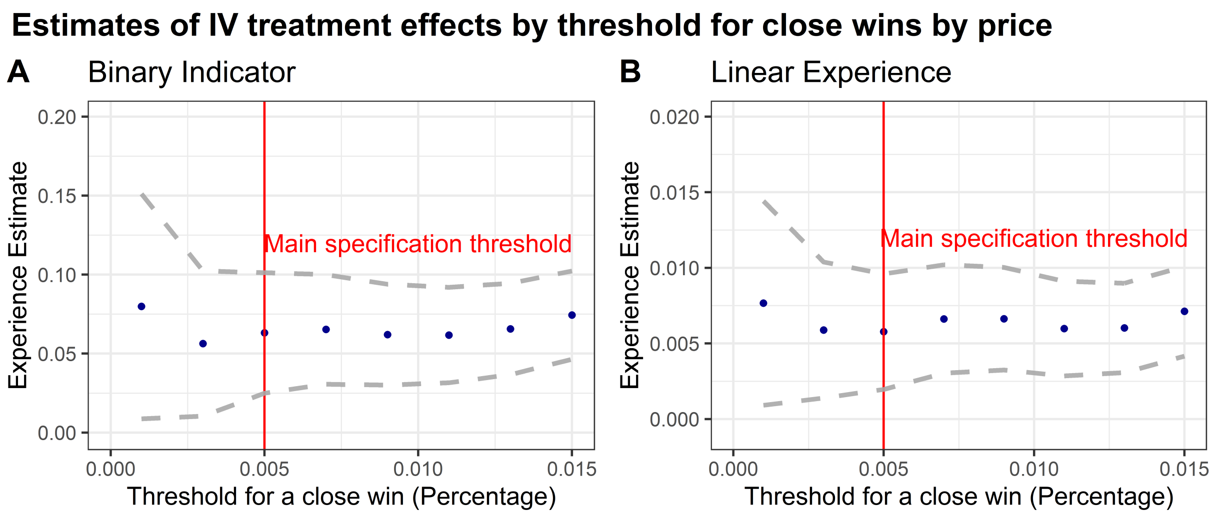

Figure 1-5: Robustness analysis for threshold of close wins

Note: The plot shows the coefficient on experience as in the specification of Panels 4 (left) and 5 (right) of table 1.6, 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 in period $(t-1)$. The x-axis shows how the coefficient varies with the threshold for what is considered a close win.

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 1.9 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 1.9: 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	0.021 (0.006) ***	0.017 (0.007) **	0.011 (0.008)	0.011 (0.013)
Rolling	Binary Indicator	0.063 (0.019) ***	0.059 (0.024) **	0.028 (0.028)	0.045 (0.04)
Rolling	Linear	0.006 (0.002) ***	0.006 (0.003) **	0.002 (0.003)	0.004 (0.004)

1.4.3 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 1-6 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.

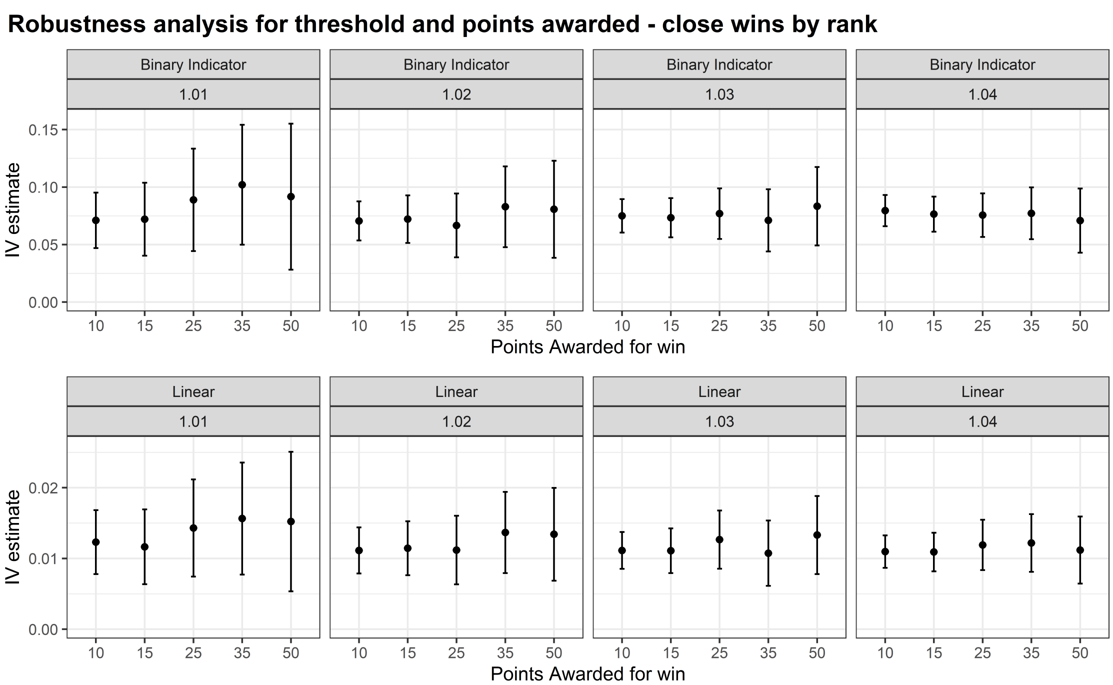

Figure 1-6: Robustness analysis for parameters in the IV-Rank strategy

sss

2. 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. Our 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 in the market.

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 that led to increased success in the market. 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 2.1 investigates the first hypothesis while Section 2.2 investigates the second. In each section we characterize the data and the empirical strategy before showing the results. Most of these elements are very similar to their previous chapter counterparts so we keep the exposition brief.

2.1 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.

2.1.1 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 we keep the original unit of observation (i.e. the bid) for our outcomes. We still employ aggregation to compute previous experience at each point in time for every firm. As before, we filter those contracts where experience is employed in the awarding factors of the contract (but we do not filter for experience computation).

Furthermore, we filter the first year in the data for our 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 data are employed to compute experience, as in the previous section.

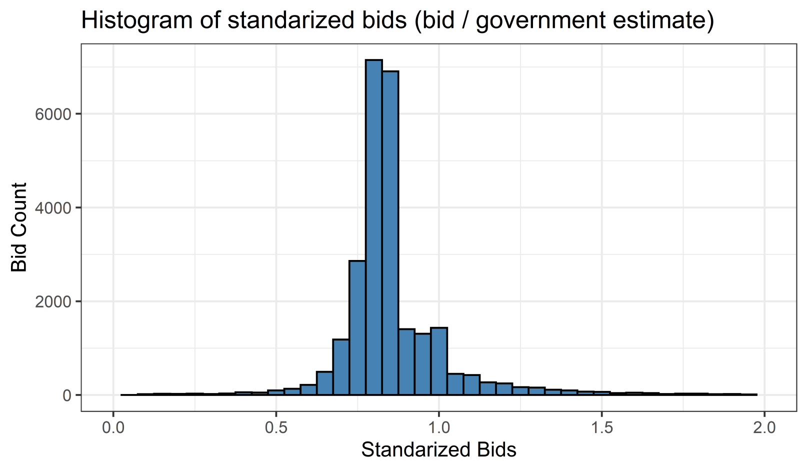

Figure 2-1: Histogram of standarized bids

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 2-1 shows a histogram of standardized bid amounts (we restrict the visualization range for convenience).

Table 2.1 shows descriptive statistics of the observations employed in the analysis sample for this section. Note that there are modifications with respect to Table 2.1, given by the extra filtering steps employed for this analysis. .

Table 2.1: 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

2.1.2 Empirical Strategy

Our empirical strategy relies on a regression of the form:

$$BID_{ijt} = \alpha + \beta EXP > 0_{ijt} + X_j + FIRST_{ijt} + \varepsilon_{ijt} \tag{2.1}$$

$$BID_{ijt} = \alpha + \beta EXP_{ijt} + X_j + FIRST_{ijt} + \varepsilon_{ijt} \tag{2.2}$$

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 linear 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 $EXPCLOSE_{it}$, the number 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.

Our consistency strategy relies in validity and relevance assumptions. The first one requires uncorrelatedness between close wins and cost measures. The second requires that our instrument does produce variation in the independent variable. We test this assumption by developing a regression of bids won on bids closely won (by price). The regression on wins on close wins by rank shows instead an F-statistic of 814 for the binary indicator and 631 for linear experience. Finally, to interpret our indicator estimate as the LATE, we again require a monotonicity condition, which is satisfied by construction.

2.1.3 Results

We show graphical results in Figure 2-2. 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 with either one close win (by rank) or zero wins. Notably, firms with one close win (and no regular wins) submit bids that are on average almost 9 percentage points lower that those firms without experience. This equals around 40% of the standard deviation of standardized bids (0.23).

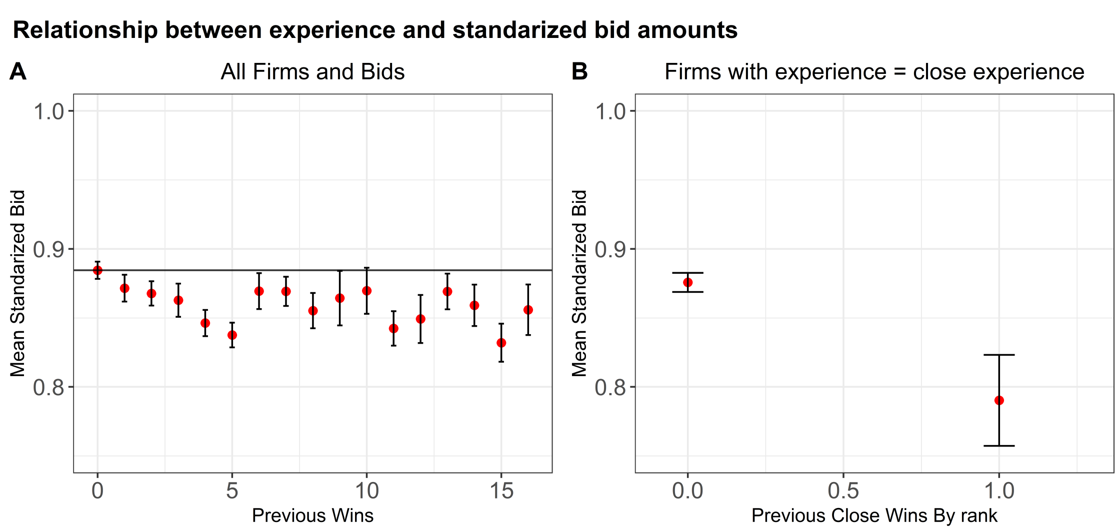

Figure 2-2: Relationship between experience and standardized bid amounts

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 2.2 presents our main results. The OLS estimates of the effect of having experience on bid amounts is around -0.03 for OLS estimates and -0.024 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 linear OLS estimate is very small and the IV result is not significant. For this specifications, we obtain higher standard errors that prevents us from obtaining a precise estimates of the level of the treatment effects. We advance a possible explanation of this result based on our empirical strategy. Since now we examined experience fully cumulatively, after 10 years we might have extremely highly experienced firms which means higher variance in the independent variable, while the links between i) experience and bids and ii) close and regular wins decrease in strength. Among highly experienced firms, it is probable that the effect of experience is not relevant anymore and close wins do not have as a close relation with outcomes.

Notwithstanding higher standard errors, 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.

2.2 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

Table 2.2: 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^{***}	-0.024^{**}		
	(0.004)	(0.010)		
Experience in (t-1) (Linear)			-0.0005^{***}	-0.00004
			(0.0001)	(0.0001)
IndFirstYear	-0.019^{***}	-0.012^{***}	-0.009^{***}	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.858^{***}	0.842^{***}	0.825^{***}	0.820^{***}
	(0.011)	(0.010)	(0.011)	(0.011)
Fixed effects By Period and Region	Yes	Yes	Yes	Yes
Observations	38,714	38,714	38,714	38,714
R^2	0.025	0.024	0.023	0.022
Residual Std. Error (df = 38686)	0.229	0.229	0.229	0.230
Note:			*p<0.1; **p<0.05; ***p<0.01	

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. 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 project.

Note that 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, it is estimated 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 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 process.

2.2.1 Data

We employ our bid dataset similarly as in the previous chapter. We create time slices exactly as detailed in Section 1.2 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, we still filter out

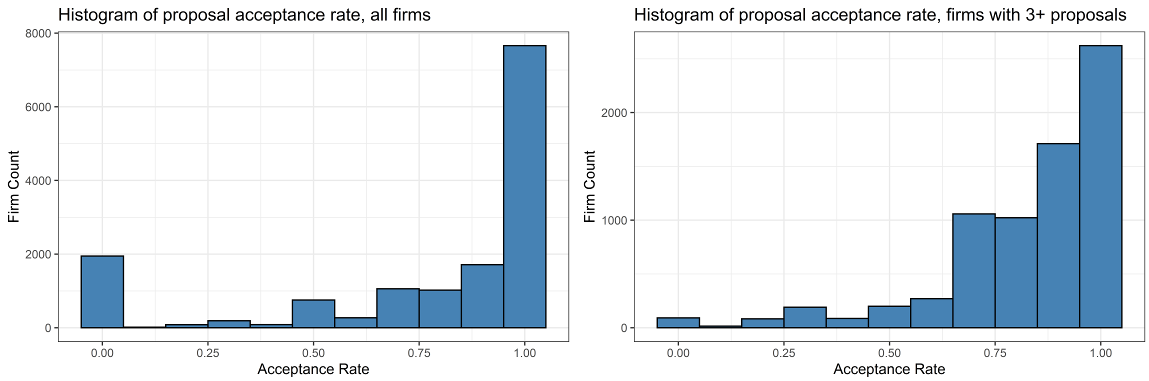

Figure 2-3: Histograms of proposal acceptance rate by firms in the dataset

contracts which include experience in the awarding factor for outcome computation. We again filter the first year of the data in our analysis sample to prevent confounding effects.

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 2-3. 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.

2.2.2 Empirical Strategy

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

$$ACCRATE_{it} = \alpha + \beta EXP_{it-1} + T_t + \varepsilon_{it} \tag{2.3}$$

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:

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

Additionally to unobserved cost advantages that could be endogenous to experience, we expect different levels of baseline levels of proposal-making abilities among firms, so we repeat our instrumentation of experience with 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 about validity and rank applies.

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 linear experience treatment. We employed our first alternative to compute experience, i.e. we employ two year periods to compute experience and subsequent two year periods to compute outcomes.

2.2.3 Results

Figure 2-4 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 .73, whereas it is equal or above .80 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 .75 ($N = 4,374$) for firms which lost their first auction to .87

($N = 990$) for firms which won their first auction.

Furthermore, we find that, for observations in the first quintile of acceptance rate, 40% of them correspond to firms with strictly positive experience. On the other side, only 20% 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 2.3. 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 10 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 (.32). The IV results are close to OLS estimates, however, the standard errors are higher.

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 almost the same as the OLS results for the two alternative instruments.

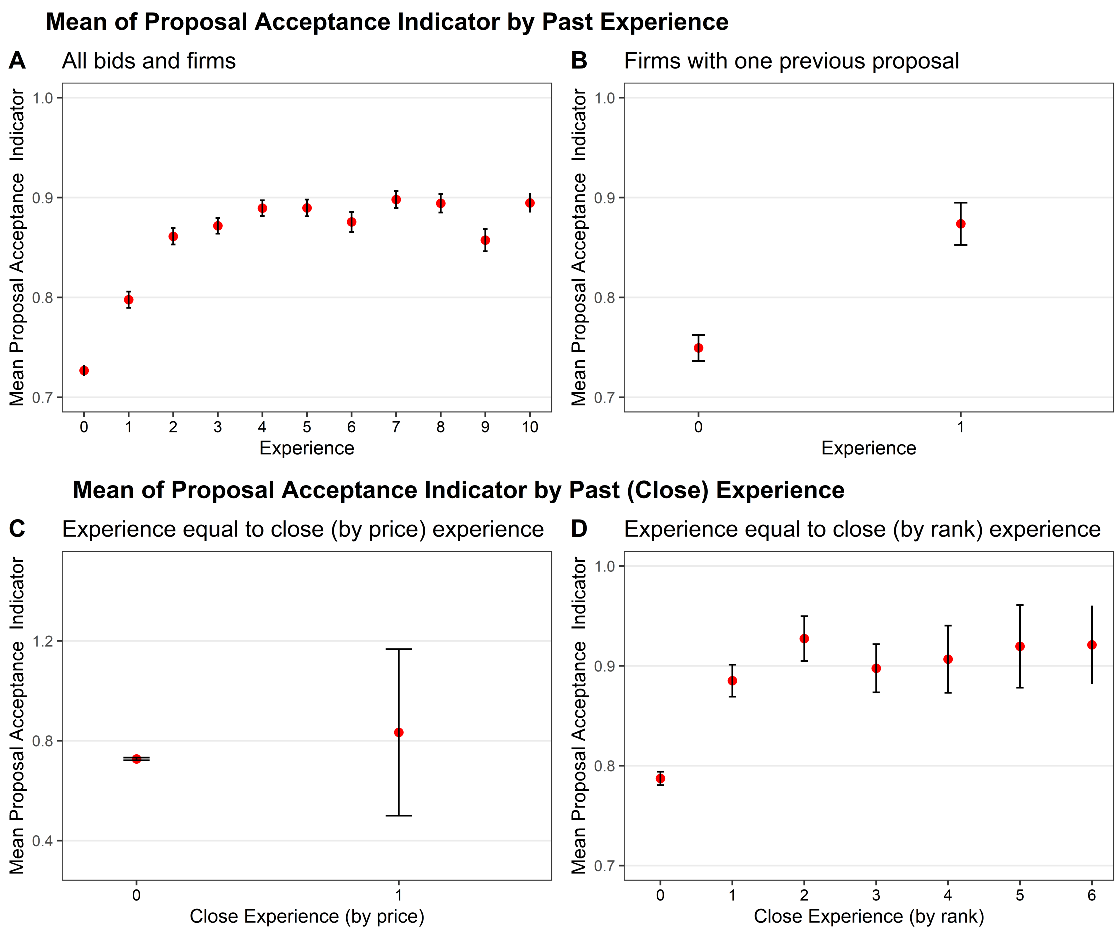

Figure 2-4: Acceptance rate for proposals sent by firms to auctions for public construction project.

Table 2.3: Regression of proposal acceptance on experience

	Dependent variable:					
	Proposal Acceptance Rate					
	OLS	*instrumental variable*		*OLS*	*instrumental variable*	
	OLS	IV (by price)	IV (by rank)	OLS	IV (by price)	IV (by rank)
	(1)	(2)	(3)	(4)	(5)	(6)
winspre >0	0.094***	0.110***	0.099***			
	(0.005)	(0.006)	(0.007)			
winspre				0.012***	0.012***	0.015***
				(0.001)	(0.002)	(0.001)
Constant	0.805***	0.800***	0.803***	0.824***	0.823***	0.821***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
Fixed effects By Period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,266	20,266	13,130	20,266	20,266	13,130
R^2	0.020	0.020	0.022	0.011	0.011	0.011
Residual Std. Error	0.320 (df = 20256)	0.320 (df = 20256)	0.318 (df = 13123)	0.321 (df = 20256)	0.321 (df = 20256)	0.320 (df = 13123)
Note:						*p<0.1; **p<0.05; ***p<0.01

3. Appendix

Table 3.1: Regressions for rank condition verification

	Dependent variable:			
	Rolling Experience > 0		Rolling Experience	
	(1)	(2)	(3)	(4)
Close Experience > 0 (Price)	0.619***			
	(0.004)			
Close Experience (Price)			5.187***	
			(0.231)	
Close Experience > 0 (Rank)		0.823***		
		(0.004)		
Close Experience (Rank)				1.840***
				(0.028)
Constant	0.345***	0.149***	0.986***	0.279***
	(0.009)	(0.006)	(0.045)	(0.035)
Fixed effects By period	Yes	Yes	Yes	Yes
Observations	20,948	16,072	20,948	16,072
R^2	0.048	0.560	0.117	0.429
Residual Std. Error	0.477 (df = 20938)	0.325 (df = 16064)	2.836 (df = 20938)	2.365 (df = 16064)
F Statistic	118.209*** (df = 9; 20938)	2,919.408*** (df = 7; 16064)	307.129*** (df = 9; 20938)	1,721.691*** (df = 7; 16064)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 3.2: Regressions for rank condition verification

	Dependent variable:			
	Annualize Experience > 0		Annualized Experience	
	(1)	(2)	(3)	(4)
Close Experience > 0 (Price)	0.578^{***}			
	(0.004)			
Close Experience > 0 (Rank)		0.445^{***}		
		(0.006)		
Close Experience (Price)			5.022^{***}	
			(0.255)	
Close Experience (Rank)				2.153^{***}
				(0.057)
Constant	0.264^{***}	0.431^{***}	0.457^{***}	0.241^{***}
	(0.009)	(0.013)	(0.024)	(0.044)
Fixed effects By period	Yes	Yes	Yes	Yes
Observations	21,705	12,327	21,705	12,327
R^2	0.085	0.282	0.120	0.244
Residual Std. Error	0.474 (df = 21695)	0.364 (df = 12317)	1.007 (df = 21695)	1.115 (df = 12317)
F Statistic	223.339^{***} (df = 9; 21695)	537.503^{***} (df = 9; 12317)	327.225^{***} (df = 9; 21695)	441.923^{***} (df = 9; 12317)
Note:				*p<0.1; **p<0.05; ***p<0.01