Chapter 1

Experience and Outcomes

The purpose of this chapter is to investigate how past experience affect current outcomes in the market for public construction projects. Section 1 outlines the empirical strategy, .

1.1 Data

Recall that our dataset consist in a set of bids submitted by firms in first-price, sealed bid 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 section. Now we detail the specifics of the subset employed for the current sresearch question.

We further filter the dataset in the following way. we only consider contracts with and estimated price above 20.000.000 CLP to exclude extremely simple contracts, and proposals below 10.000.000 CLP as well. We also excluded contracts without an official estimate. We exclude non-single-item proposals. Finally, we exclude contracts with several proposals from a given contractor as we have no clear way of distinguisinh which was the last submitted one.

As a result of the previous filtering steps we end with around 43,000 construction contracts, of the original sample of about 74,000 contracts. We excluded around 5% of the original sample which had no official estimate(which are excluded), and around

2% which are not single-item proposals. By far the most important filtering step is excluding contracts with estimated values of less than 20.000.000 CLP, which excludes around 41% of the original dataset (around 30,000 contracts). Finally, around a 1,200 contracts had multiple proposals from the same contractor. Note that some of the previous conditions overlapped among them.

The table shows descriptive statistics for the final sample employed.

Table 1.1: Descriptive Statistics

name	N	mean	std	max	min
Bid (all)	119000	1.73e+10	5.8e+12	2e+15	1e+07
Winning Bid	32200	2.27e+08	2.54e+09	2.47e+11	1e+07
Difference between 1st bid and 2nd (%)	32200	0.0638	0.0859	0.984	0
Number of Bidders per Contract	32200	3.18	2.25	23	1
Year	32200	2020	2.89	2020	2010
Offers made by Firm	13800	8.64	18.5	846	1
Win prob. by Firm	13800	0.213	0.294	1	0
Offers won by Firm	13800	2.33	5.66	111	0

Note: The table shows sample summary statistics for the public construction dataset after filtering has been applied (see text). The difference between 1st(winning) and 2nd (runner-up) bid is only avalaible in approx. 70% od the contracts, with two or more bidders.

1.2 Empirical Strategy

Our empirical strategy consists in a Regression Discontinuity design in which we compare the bidding outcomes of firms with varying degrees of previous experience in the market. Our main interest is the difference between the firms with some and the firms with none experience, but we consider also increasing measures of experience.

Our main outcome variable is the share of contracts won out of the total amount of contracts bid for, in a specific period of time. That is, if we consider period t, then 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 reasonable parameter which controls the duration of the periods i which we compute both experience and outcomes. In our initial specification, we consider each tau to be equal to two years, and each t is the first day of the year in our dataset. Employing a proportion of contracts won instead of total contracts has two advantages. First, we implicitly control by size. Second, we can capture directly the impact of firms which bid in contracts with no competitors.

Regarding the measurement of experience for a given firm and period, we consider two main options. The first one is to consider experience as the total amount of contracts won in a fixed period before the period of outcomes being considered. The second alternative we consider is a rolling average of yearly contracts developed up until that same period of outcomes. The robustness checks consider also other measures of experience.

The first option is implemented as follows. We create a dataset where observations are period-firm pairs and variables are measures of past experience and current outcomes in the following way. We fix a specific start date and an end date to define a first period (Period 1), which is used to compute the experiences of each firm. Then, for each firm we link this experience to the outcomes in a subsequent period of equal length (Period 2). This way, we construct a dataset where each observation is a firm, the dependent variable is a measure the firm's outcome in Period 2, and the independent variables is a measure of the (past) experience of the firm in Period 1. We repeat this process, considering as Period 1 successive two-year periods in our dataset with one year of overlap between them. Since our dataset contains 10 years, we end up with four two-year pairs (we do not have outcomes for the last two years in our data).

A key parameter in this strategy is the length in years of period 1 and period 2. They are arbitrary and could be differ from each other. As our baseline, we employ two-years periods for the following reasons. First, we do not expect that an active firm will spend more than one year without bidding. Our full dataset shows that for every firm on the data who bid having previous experience, a 50% has developed a contract within the last 2 years. Second, we do not want to employ too long periods as that would confound the effect of experience for early-period entrants. However, periods of one or three years could be reasonable as well, so we relax this assumption in the robustness checks and experiment with a wider array of periods' lengths.

For the second alternative to measure experience we construct an annualized measure of experience in the following way. Our success periods are constructed in the same way as before. However, instead of restricting our measure of past experience to two years before the beginning of the period, we consider all the previous periods to count contracts won. In order to obtain comparable estimates across successive years, for each period we divide the total contracts developed by the firm up until that moment by the number of years where we are considering experience. This way, we obtain an "annualized" measure of experience.

Our two main specification are of the following form, where S_{it} is the share of contracts won in the period of interest, EXP_{it} is the measure of experience of firm i in period $t-1$ or up until t(depending on the specification), and T_t are period fixed effect.

$$S_{it} = \alpha + \beta EXP_{it-1} + T_t$$

In some specifications we include firm fixed effects based on size. It is possible that smaller firms face higher competition due to less-complex contracts, and so their baseline level of success in the market will be lower. Additionally, we add period fixed effects for each period of outcomes being considered to control for changes in the market environment throughout the sample.

1.2.1 Endogeneity and Identification

Causal interpretation of the regression above is problematic since unobserved cost variables are endogenous. It would be expected that highly efficient firms are able to bid more aggressively, win more projects, and in turn accrue more experience in the market of public construction projects. In the base case, we expect our estimates of the effect of experience on outcomes to be biased upwards due to unobserved cost variables which should have positive correlation with experience.

In order to identify the causal experience of experience on outcomes, we employ external variation in the experience of a firm. We employ as the main source of identification the exogenous variation produced by close wins, which should be less or not at all attributed to unobserved cost factors. We arbitrarily define a win as a close win if the percentage difference between the winner and the runner-up is less than 0.5%. This leads to approximately 8% of winning bids being classified as a close one. In the robustness checks, we also consider a different approach to close wins, where we consider close wins where three or more competitors are all within a 1% difference in their bids.

In the next table we examine whether close wins are different from the population in several types of metrics. We can see that in most aspects these bids are not exceedingly different from the rest of the sample, so we expect that the only difference between these close wins and regular ones is the difference in bids and there are no underlying project characteristics that could explain them.

Table 1.2: Comparison of key statistics between close wins(<0.05% difference between 1st and runner-up) and regular wins

Variable	Mean (Not close win)	Mean (Close win)	Sd (Not close win)	Sd (Close win)
Bid	6.3e+08	3.32e+10	1.06e+10	8.11e+12
Bid_Winning	3.18e+08	2.37e+08	3.56e+09	2.62e+09
Difference between 1st bid and 2nd (%)	0.14	0.0186	0.115	0.0147
Number of Bidders	3.86	4.08	2.12	2.23
Year	2020	2020	2.92	2.89
Offers made by Firm	4.4	6.17	7.34	11.2
Win prob. by Firm	0.191	0.171	0.3	0.274
Offers won by Firm	0.972	1.37	2.1	3.23

Our designs employs close wins in past periods to instrument total wins (experience). Clearly, both measures are correlated since every extra unit of experience increases the probability of having at least one close win. 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 we should also have a valid instrument.

1.3 Main Results

Table 1.3 shows the results for specifications with the reduced form regression and IV estimates for our first Experience computation measure (i.e. rolling two year periods) while Table 1.4 shows the results for our way of computing experience (i.e. total annualized experience).

In both tables, OLS results are in the first to third panels. The first panel shows that the OLS coefficient on the effect of having experience against not having experience (binary) is around 0.10, for both ways of computing experience. Our specification with linear returns on experience shows that experience renders a 0.02 and 0.05 increase in winning share per extra contract developed (for computations 1 and 2 respectively) . All the estimates for the experience-related coefficients are significant at 0.01 with robust standard errors.

The IV results (fourth to sixth panels in each table) show that the linear and quadratic estimates of the coefficients on experince generally stay within $\pm$ 0.03 of their OLS counterparts. There is however an increase in the binary measure of experience coefficient, for both computations of experience.

Table 1.3: Regression results for OLS and IV specifications where experience is computed in 2-year rolling periods

	Dependent variable:					
	Share of Contracts won in t					
		OLS			*instrumental variable*	
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.119***			0.178***		
	(0.004)			(0.010)		
Experience in (t-1) (Linear)		0.022***	0.038***		0.022***	0.039***
		(0.002)	(0.001)		(0.001)	(0.003)
(Experience in (t-1)) (Squared)			−0.001***			−0.001***
			(0.0001)			(0.0003)
Constant	0.260***	0.276***	0.270***	0.245***	0.276***	0.269***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,959	37,959	37,959	37,959	37,959	37,959
R^2	0.035	0.028	0.034	0.028	0.028	0.034
Residual Std. Error	0.316 (df = 37950)	0.317 (df = 37950)	0.316 (df = 37949)	0.317 (df = 37950)	0.317 (df = 37950)	0.316 (df = 37949)
Note:						*p<0.1; **p<0.05; ***p<0.01

The next table shows the results with a different measure of experience.

There does not seems to be conclusive evidence regarding different results when

Table 1.4: Regression results for OLS and IV specifications where experience is computed as past annualized contracts won.

	Dependent variable:					
	Share of Contracts won in t					
	OLS				*instrumental variable*	
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.109*** (0.004)			0.168*** (0.010)		
Experience in (t-1) (Linear)		0.059*** (0.016)	0.107*** (0.004)		0.057*** (0.004)	0.108*** (0.012)
(Experience in (t-1)) (Squared)			−0.013*** (0.001)			−0.013*** (0.003)
Constant	0.288*** (0.005)	0.289*** (0.006)	0.285*** (0.005)	0.277*** (0.006)	0.290*** (0.005)	0.285*** (0.005)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	36,844	36,844	36,844	36,844	36,844	36,844
R^2	0.032	0.027	0.031	0.025	0.027	0.031
Residual Std. Error	0.321 (df = 36835)	0.322 (df = 36835)	0.321 (df = 36834)	0.322 (df = 36835)	0.322 (df = 36835)	0.321 (df = 36834)
Note:						*p<0.1; **p<0.05; ***p<0.01

employing quadratic rather than linear functional forms. For example, Figure 1-1 shows the mean confidence intervals, employing as period fixed effects the last period in the sample. It can be seen that the fitted total predicted value does not seen to vary greatly from the linear to the quadratic specification.

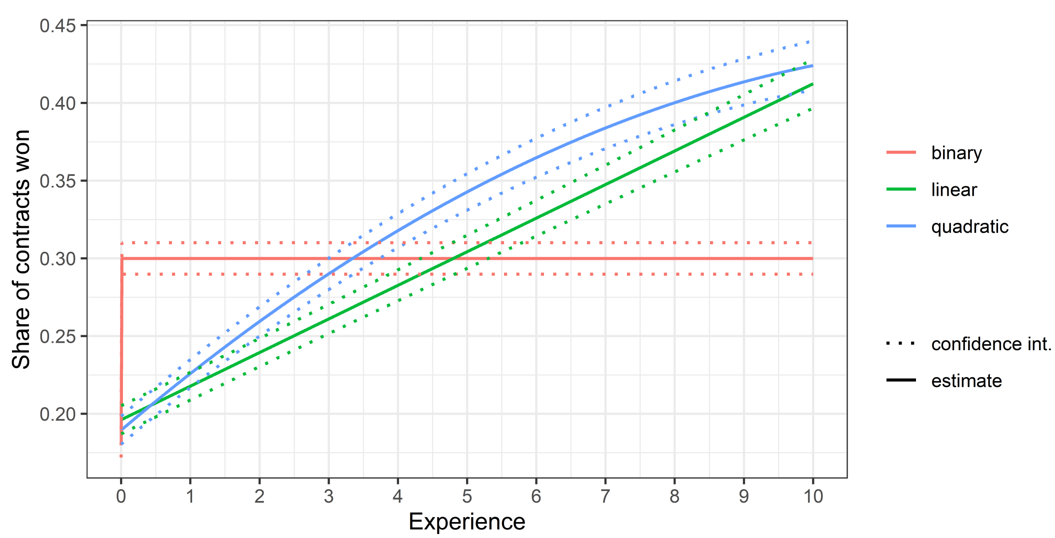

Figure 1-1: Predicted values for the mean of the outcome variable (share of contracts won), by total experience accrued in the previous period. We employ fixed effects as in the last period of the dataset.

1.4 Experience and Type of Project

1.5 Experience and Firm Size

An important variable in the investigation of the effect of experience should be firm size. First, it is possible that there are different levels of cost efficiency between small and big firms. As arguably bigger firms should have more experience on average, this could skew our estimates. A second concern is that we might expect experience to matter more for smaller firms, if there is a decreasing or "maximum" level effect of experience on future outcomes.

If our identification strategy is correct, firm size should not be a problem for the reliability of the estimates obtained in the previous section, while the effect of experience by size is still unclear. In this section we attempt to develop specific estimates of the effect of experience for different levels of firm size. Developing intra-category estimates serves as both as an identification strategy and as robustness check of our previous findings.

We follow the following approach. First we select a subsample from our original dataset which we can classify acoording to annual sales. We obtain intra-category estimates of the effect of experience and interpret them. Finally, we discuss the results and some of the empirical challenges of controlling for size.

In order to study and control for firm size we employ a publicily avalaible classification of firms according to their annual sales, maintained by the chilenan Tax Bureau Office (*Servicio de Impuestos Internos*). Firms are categorized in 13 categories. Category number one corresponds to 'tax data not enough to classify', but from catoegory two upo to thirteen, each category is defined by an increasing level of minimum yearly sales. However, this data is avalaible only for non-individual firms. After merging with our initial sample,we are left with around 30% of our original firm sample. Table **??** shows how many firms do we have in our sample for each category, average annual sales for these firms, and statutory annual sales thresholds for each category. Note that we have much more firms at intermediate categories than extreme

Table 1.5: Sample Firm descriptive statistics with statutory sales thresholds per category

Category	Number of Firms	Sample Average Annual Sales (CLP UF)	Statutory Sales Minimum (CLP UF)	Statutotry Sales Maximum (CLP UF)
1	98	402	NA	NA
2	107	453	0	200
3	139	4317	200	600
4	496	661	600	2400
5	541	856	2400	5000
6	636	1598	5000	10000
7	852	2163	10000	25000
8	519	4567	25000	50000
9	301	9941	50000	100000
10	197	12449	100000	200000
11	153	22209	200000	600000
12	36	57219	600000	1000000
13	72	70506	1000000	NA

ones.

We estimate the effect of linear experience with our first measure for each category of firms by OLS and IV. Specifications are the same as equation with period fixed effects. The results are presented graphically in 1-2 (the coefficient for category two is omitted because it was much bigger than the rest and distorted the visualiztion). Full results are avalaible at the Appendix.

We only obtain significant effects at intermediate sales categories' levels. However, everytime a coefficient is significant it is also positive. The results are not supreising given i) the reduced sample we are employing ii) the expected reduced importance of experience for very big firms.

Controlling for firm size is challenging mainly because of statistical reasons. First, firm size distribution in the sample is not uniform as there are less very small and very big firms. Second, the within-size distribution of experience within extreme categories has very few observations with more than five contracts of experience. Third, this sample is already smaller due to filtering single-person companies. Both factors make it hard to obtain per-category estimates with enough statistical power experience.

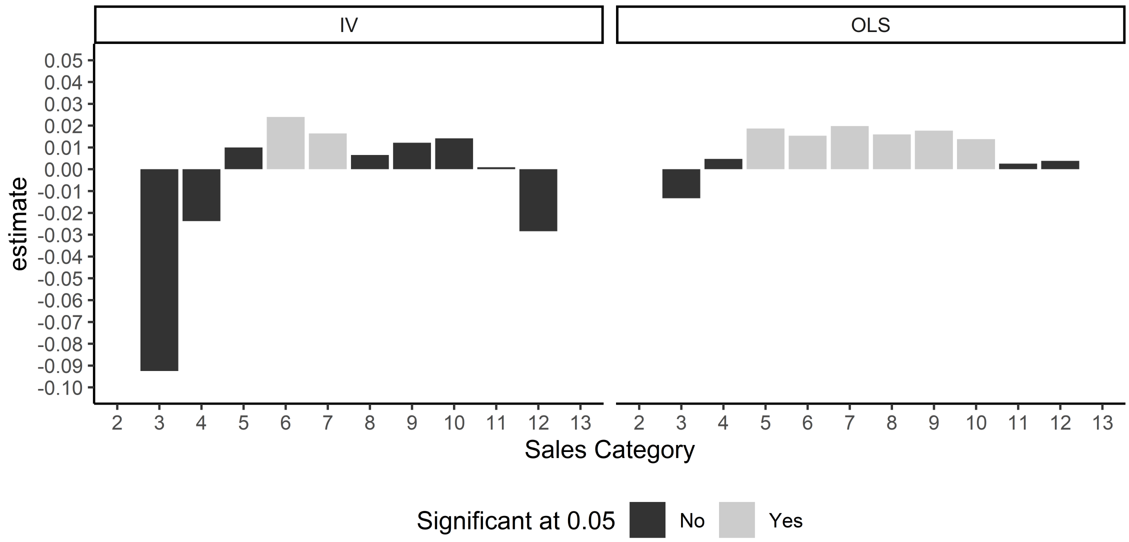

Figure 1-2: Experience coefficient by tax sales category

1.6 Robustness checks

Several of our choices in the previous section admit several arbitrary choices. In this section we consider several extensions in parameters which could influence the results obtained before. We consider robustness checks in the following areas:

1.6.1 Periods of outcomes

In the previous section, we measured outcomes occuring in two year periods. We now consider outcomes occuring in one and three year periods as well. Note that in this part we only vary the length of the period where outcomes are computed and we maintain the procedure to compute experience as before. Table shows outcomes computed for periods of 1, 2(the original specifications) and 3 years. The first three columns employ the experience measured in the two-period previous to the outcome period while the 3-6 compute experience as annualized cumulative experience as discussed in the previous section.

Table 1.6: Regression for OLS and IV specifications

	Contracts Won/Contracts Bid in Outcome Period					
	Outcome period of length (years):					
	1	2 (Original)	3	1	2 (Original)	3
Experience	0.022***	0.020***	0.023***			
	(0.001)	(0.001)	(0.001)			
Annualized Cumulative Experience				0.060***	0.058***	0.061***
				(0.002)	(0.002)	(0.002)
Constant	0.270***	0.309***	0.256***	0.281***	0.257***	0.260***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.007)	(0.004)
Observations	38,739	29,415	43,453	37,623	28,234	42,358
R^2	0.028	0.031	0.025	0.026	0.028	0.023
Residual Std. Error	0.316 (df = 38730)	0.338 (df = 29405)	0.305 (df = 43445)	0.320 (df = 37614)	0.342 (df = 28224)	0.309 (df = 42350)
Note:						*p<0.1; **p<0.05; ***p<0.01

1.6.2 Periods of experience computation

For the first measure of experience, we consider computing experience over 1-year periods. The original specification considered computing experience in two-year rolling periods.

In practice, considering longer periods to compute outcomes decreases the variance of th

1.6.3 Definition of a close win

In the previous section, we considered close wins as wins where the winning contractor submitted a bid that was not more than 0.05% below the runner up. Now, we sensibilize our main coefficient to different values of this parameter.

The plot in 1-3 displays the coefficient of interest in the IV specification as we vary the threshold for a close win. The specifications consider linear effect of experience and fixed effects by period. It can be seen that results are robust to a range of the threshold for considering a win as a close wins. Note that the results remain significant across the different values of the parameters, even when employ our lower bound for the threshold(0.01%) where we have less close wins. As expected, the standard error increases towards this bound while but decreases towards less stringent definitions of close wins (because of the increase in power in the instrument). Finally, note across that all confidence intervals at 95% remain within 0.0180 and 0.0275.

Figure 1-3: Robustness analysis for threshold of close wins