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. In order to do so, we slice the data in specific points in time and examine how past experience for a firm is related to future outcomes.

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 research 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 distinguishing 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 MM 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.

We have contracts in our sample which are awarded based on experience. Contracts awarded with experience as an awarding factor are relevant from the point of past experience, but would bias up our estimates if considered in the computation of outrcomes. Table shows how many contracts have experience in their awarding criteria, how many have price and some descriptive statistics of the weight of each one in the sample.

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 different levels of previous experience in the market. We first present the baseline OLS specification and then discuss endogeneity issues that arise.

Our two main OLS specification are as follows. 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.

$$S_{it} = \alpha + \beta EXP^k_{it-1} + T_t + \varepsilon_{it} \tag{1.1}$$

Our outcome variable S_{it} is the share of contracts won out of the total amount of contracts bid for, in a specific timeframe. 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 parameter which controls the duration of the periods in which we compute both outcomes. In our initial specification, we consider each τ to be equal to two years. Employing a proportion of contracts won instead of total contracts has two advantages. First, we implicitly control by the size of the firm. Second, we capture the learning effects which manifest by being able to bid for contracts where less experienced firms do not submit proposals.

We make an important filtering step before computing outcomes, as we only consider contracts for which previous experience is not among the awarding criteria of the contract. This is because including 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 experience computation, which we now describe.

We consider two ways of computing the previous experience EXP^k_{it} for a firm which we index by k, $k \in \{1, 2\}$. The first alternative computes experience as the total amount of contracts won in a fixed period before the outcomes' period t. The second alternative computes experience as contracts per year developed up until the period of outcomes t.

The first alternative is implemented as follows. We fix a specific start date and end date to define a first period (Period 1), which is used to compute the total contracts won for each firm. Then, for each firm we link this experience to the outcomes in a subsequent period of equal length (Period 2). We end up with a dataset where each observation is a firm, the dependent variable is a measure of he firm's outcomes in Period 2 (i.e. S_{it}), and the independent variable is a measure of the (past) experience of the firm in Period 1 (i.e. EXP^1_{it}). 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 ("slices").

The length in years of period 1 and period 2 is an arbitrary parameter in this strategy. 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, we relax this assumption in the robustness checks and experiment with a wider array of periods' lengths.

For the second alternative to measure experience, EXP^2_{ij}, 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 outcomes' period, as in the previous method, we consider all the previous years when we 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 for each firm.

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 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. If this was not the case, the set of contracts from which the

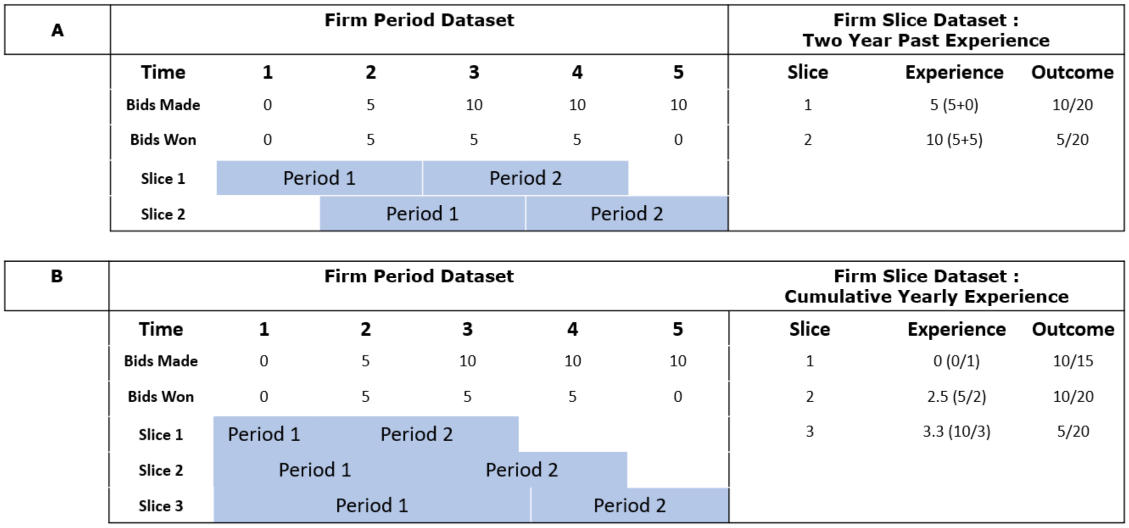

Figure 1-1: Example computation of slice-firm dataset, employing two-year fixed periods of past experience (A), and cumulative yearly experience (B).
Note:

outcome variable would be computed in each step would be smaller or equal, since we would only consider contracts without experience as an awarding criteria when computing outcomes.

Finally, we add period fixed effects for each period of outcomes being considered to control for changes in the market environment throughout the sample.

After the transformation steps, we obtain five slice-firm datasets for the first measure of experience and six slices for the second measure of experience. The following table shows 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 have the form of the righmost table in Figure 1-1.

1.2.1 Endogeneity and Identification

Causal interpretation of the regression in Equation 1.1 is problematic since unobserved cost variables, specific to each firm, are omitted in the regression (since they are unobservable) and also endogenous. If there are highly efficient firms which due to this advantages are able to bid more aggressively or submit better proposals, they should win more projects, and in turn accumulate more experience. We expect our estimate $\hat{\beta}$ (coefficient on experience) in 1.1 to be biased upwards due to correlation (expectedly positive) between omitted cost variables and the amount of past experience.

To identify the causal effect of experience on outcomes, one alternative is to employ external variation to instrument the experience of a firm in an Instrumental Variables (IV) approach. We discuss what would be the optimal way of producing external variation, and, since the data does not allow us to employ this strategy, we propose two second-best alternatives.

Ideally, one could use close wins as the source of exogenous variation in past experience. The argument is that close wins should be less or not at all attributed to unobserved cost factors, or other efficiency advantages, but instead attributable to random differences, for example, the conservativeness of each firms' engineers' estimates. 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 next (or several) 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.

In this approach, our first stage takes the form of Equation 1.2 . Here EXP_{it} is the total set of contracts won in slice $t-1$ for firm i, while $EXPCLOSE_{it}$ is the subset of the contracts won which was won closely as per the definition above, and ν_{it} is an error term uncorrelated with $EXPCLOSE_{it}$.

$$EXP_{it} = EXPCLOSE_{it} + T_t + \nu_{it} \tag{1.2}$$

The second stage is shown in Equation 1.3.

$$EXP_{it} = EXPCLOSE_{it} + T_t + \varepsilon_{it} \tag{1.3}$$

Clearly, both measures of experience (EXP and $EXPCLOSE$) 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.

Unfortunately, the previous strategy is unfeasible a with the data we have avalaible. 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

1.2.2 Close wins by close bids

The first method follows the same theoretical setting as before, but changes how close wins are singled out. Close wins are now operationally identified as the wins meeting copulatively three conditions: i) the price weight in the awarding decision criteria is 70% or higher ii) the contract was awarded to the lowest bid and iii) the difference between the lowest bid and the second lowest bid is less than .05%. We expect that this way of identifying close wins does 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 8% of winning bids being classified as a close one. In the robustness checks, we also consider a different values for these parameters, 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 defined as above 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 there are no underlying project characteristics that could affect competition for these contracts.

1.2.3 Close wins as close rank

There are two main objections to the previous identification strategy:

- The price is not the only awarding factor. Thus, it is possible that even in

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

a contract where price is a major component, the cost advantages of a firm manifest in terms of other awarding factor, like quality. That is, the firm offers similarly priced goods but at a much superior quality.

- The closeness between the first and the second bid does not take into account the full pool of participants.

The current alternative for close wins employs a ranking mechanism to label close wins when 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 is attributable to close factors.

Obviously, the main issue is how to construct a good ranking measure. We proceed by modeling each auction as a multi-player game (in the non-economic sense of the term) event 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 in the initial version). Then, it is awarded 24 points for winning againsta a similar oponnent and substracted 8 by losing. The implementation of the algorithm recommends that points awarded and substracted sum to zero, so we fix awarded points and choose substracted points such that on average (given the number of players in an auction) this condition holds. Against non-similar opponents, the algorithm makes a correction based on the ranking of the players and the outcome of the game. The details of the algorithm are given in the Appendix.

Having assigned to each firm a ranking for each point in time(i.e for each auction), we label a win as a "close win" to the one where the highest rank among the bidders for the auction was not more than 3% higher than the lowest rank among them. This yields around of close wins in the final sample of analysis classified as a close win. 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.

Just as before, in Table 1.3 we present summary statistics for close wins identified via close wins.

Table 1.3: Comparison between close and non-close wins

Variable	Mean (Not close win)	Mean (Close win)	Sd (Not close w
Bid (all)	1.82e+10	7.66e+08	5.95e+12
Winning Bid	2.65e+08	2.62e+08	2.31e+09
Difference between 1st bid and 2nd (%)	0.063	0.0789	0.0818
Number of Bidders per Contract	3.28	2.99	2.37
Year	2016	2015	2.96

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 is not clearly defined.

1.3 Main Results

First we explore graphically the relationship between our first measure of experience and outcomes. Figure 1-2 , Panel A, shows the relationship between past experience and outcomes (i.e. winning share). While we can see an increase in the average share of contracts won with more past experience, there is considerable heteregeneity, as it can be seen in the wide error bars (which show interquartile range). Panel B contains only two types of firms: firms that either bid but not won contracts in period $(t-1)$ or firms which won one or more close contracts in period t, and thus is akin to results of a reduced form regression.

Table 1.4 shows the results for OLS and IV regressions for our first experience

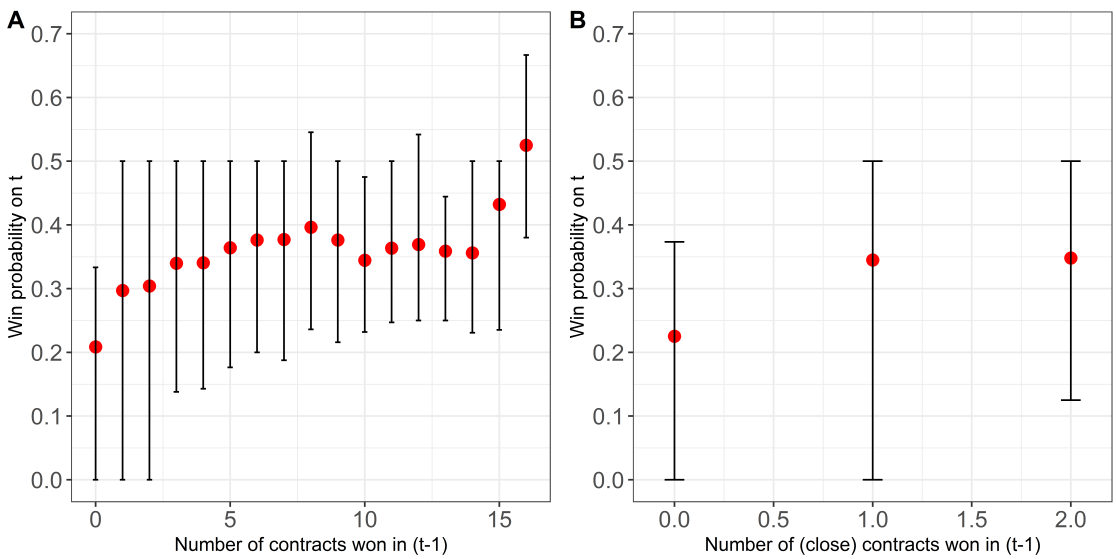

Figure 1-2: Relationship between contracts won on $t-1$ and mean winning probability across contractors in t.

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. Since the data contains ten years, the observations correspond to outcomes computed for eight outcome periods. Only x for which the number of observations is greater than ten are shown. Error bars correspond to the interquartile range. Panel A: all sample observations are considered. Panel B: shows results for a subsample where the only contractors considered are those who either i) won closely one or more close contracts on period 1 or ii) did bid but not win a contract in period 1 measure (i.e. rolling two year periods) while Table 1.5 shows the results for our second measure of experience (i.e. 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 any level experience against not having experience (binary) is around 0.10, for both ways of computing experience (and). 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 experience measures 1 and 2 respectively). All the estimates for the experience-related coefficients are significant at $p = 0.01$ 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 experience generally stay within ± 0.03 of their OLS counterparts. There is however an increase in the binary measure of experience coefficient, for both measures of experience, since for the first measure of experience **this rises from and for the second measure this rises from** . This is different than we expected, as our initial concern was that omitted cost variables would bias estimates upwards.

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

Table 1.4: Regression for OLS and IV specifications

	Dependent variable:					
	Share of Contracts won in t					
	OLS			*instrumental variable*		
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.072^{***}			0.123^{***}		
	(0.005)			(0.015)		
Experience in (t-1) (Linear)		0.011^{***}	0.017^{***}		0.012^{***}	0.020^{***}
		(0.003)	(0.001)		(0.001)	(0.004)
(Experience in (t-1)) (Squared)			-0.0003^{***}			-0.001^{**}
			(0.0001)			(0.0003)
Constant	0.266^{***}	0.279^{***}	0.275^{***}	0.250^{***}	0.278^{***}	0.273^{***}
	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,037	20,037	20,037	20,037	20,037	20,037
R^2	0.018	0.016	0.018	0.012	0.016	0.017
Residual Std. Error	0.346 (df = 20027)	0.346 (df = 20027)	0.346 (df = 20026)	0.347 (df = 20027)	0.346 (df = 20027)	0.346 (df = 20026)
Note:						*p<0.1; **p<0.05; ***p<0.01

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

Table 1.5: Regression for OLS and IV specifications

	Dependent variable:					
	Share of Contracts won in t					
	OLS				*instrumental variable*	
	(1)	(2)	(3)	(4)	(5)	(6)
Experience in (t-1) (Binary)	0.109***			0.172***		
	(0.003)			(0.010)		
Experience in (t-1) (Linear)		0.058***	0.107***		0.057***	0.107***
		(0.013)	(0.004)		(0.003)	(0.011)
(Experience in (t-1)) (Squared)			−0.013***			−0.013***
			(0.001)			(0.003)
Constant	0.282***	0.284***	0.279***	0.271***	0.284***	0.279***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Fixed effects By period	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42,517	42,517	42,517	42,517	42,517	42,517
R^2	0.033	0.028	0.033	0.025	0.028	0.033
Residual Std. Error	0.317 (df = 42507)	0.318 (df = 42507)	0.317 (df = 42506)	0.318 (df = 42507)	0.318 (df = 42507)	0.317 (df = 42506)
Note:						*p<0.1; **p<0.05; ***p<0.01

employing quadratic rather than linear functional forms for experience. For example, Figure 1-3 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.

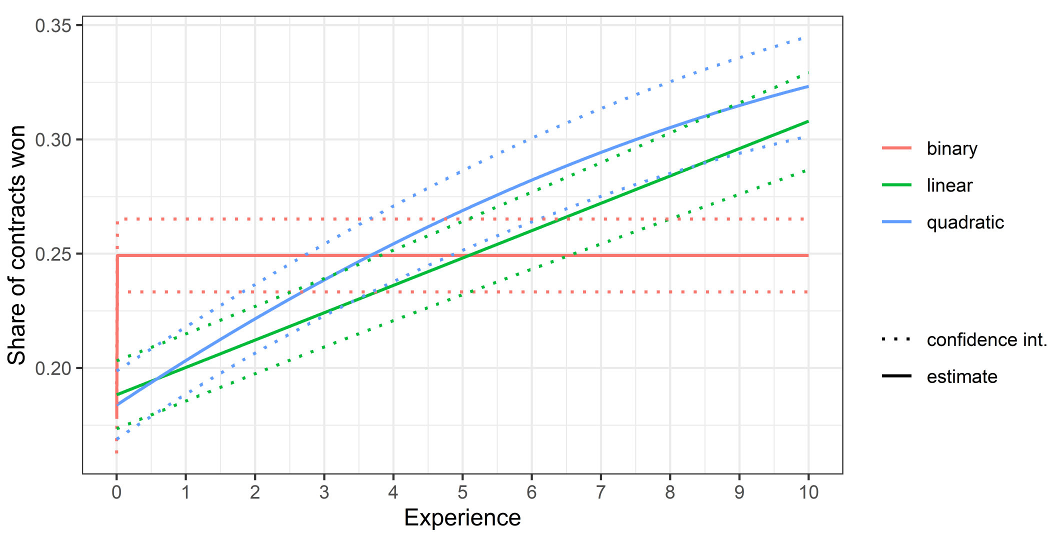

Figure 1-3: Predicted values for the mean of the outcome variable (share of contracts won), by total experience accrued in the previous period. We employ fixed effects as in the last period of the dataset.

1.4 Experience and Type of Project

Given our previous results a natural concern is if whether all projects exhibits the same returns to experience or if experience is more important in certain types of works. In this section we replicate the previous analysis by disaggregating by type of project. In order to do this, we classify certain projects according to their description, then run similar regressions as in the last section, and present the results.

First we describe briefly how we construct categories for the prrojects and which ones are avalaible for the analysis. Our original dataset includes a name variable which describes the type of project with some extent. We extracted this name variable and looked for i) common single words (unigrams) and ii) common pairs of words (bigrams). We select the most common unigrams and bigrams and map similar words and bigrams to project categories. The full categorization mapping can be found in the Appendix.

We end up with contract classified under categories. Importantly, if a contract includes unigrams or bigrams in its name belonging to more than one category, it is included in the analysis of both categories. The number of contracts, average amount, average number of bidders for each category is shown in Table 1.8. We can see that the biggest categories of projects are school-related, vecinal works, parks and pavements (including sidewalks). The Appendix contains more details on the types of projects included in each category.

Next we run similar regressions as in the previous section for each project type, considering as our dataset only the contracts in that project category . We employ the same specification of with a linear functional form on experience and period fixed effects, and our first measure of experience. The results are presente in Figure 1-4he Appendix includes more detailed tables with full regression results for each sproject type. A few results stand out. First, we get the biggest coefficients on experience on graveyard projects, footbridges and housing. The first two should be almost exclusively procured by government units. Housing was also one of our hypothsized types of projects which should have high coefficients. At the bottom of

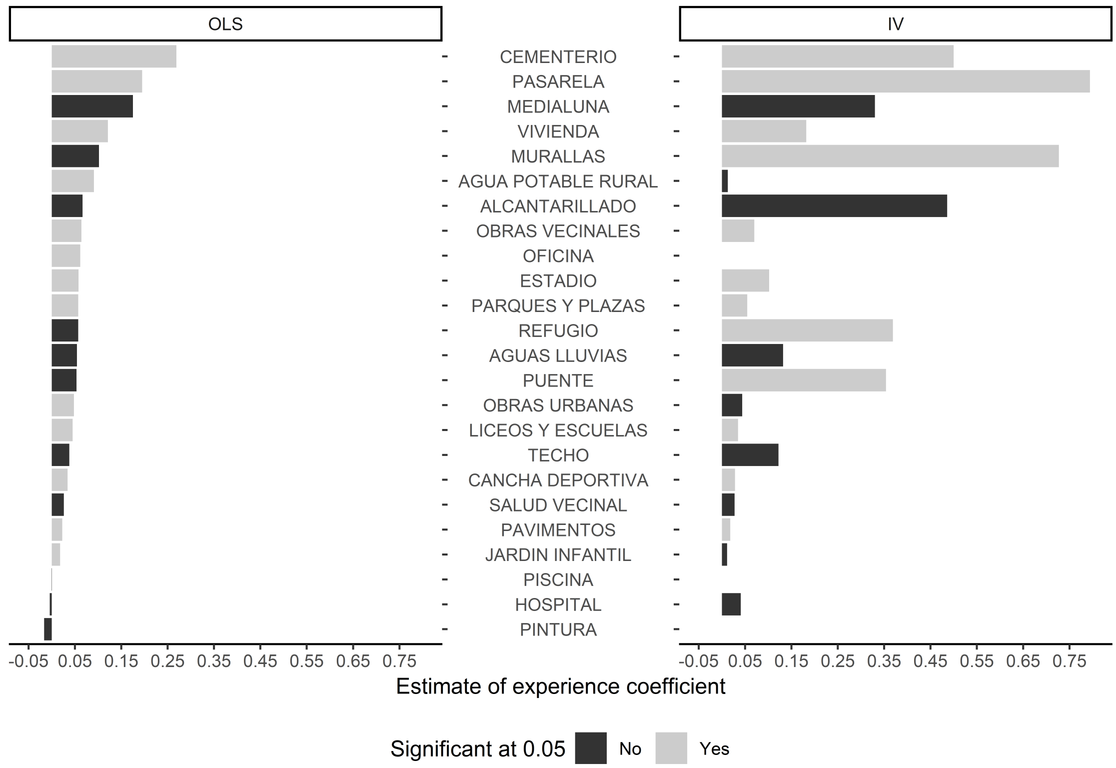

Figure 1-4: Experience coefficient by type of project.

the distribution, interestingly, we find daycares, sports courts, hospitals, and schools. The results could be explained because these projects are mostly composed of normal construction works which also have private close substitutes.

The results on hospitals should be surprising, as they are usually very big projects with a lot of specific knwoledge required.

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.

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 categories two up to thirteen, each category is defined by an increasing level of minimum yearly sales.

This data is avalaible only for firms not being fully assimilable to final taxpayers. After merging our 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 ones. In our estimation we group firms from contiguous groups together to increase power.

We estimate the effect of linear experience with our first measure for each group of categories of firms by OLS and IV. Specifications consider our first measure of experience with a binary presence of experience and with period fixed effects. The results are presented graphically in **??** (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.

Table 1.6: Sample Firm descriptive statistics with statutory sales thresholds per category

Category	Sample 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

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.

Table 1.7: Interaction between firm size and experience

	Dependent variable:			
	Share of Contracts won in t			
	OLS		*instrumental variable*	
	(1)	(2)	(3)	(4)
Experience (binary)	0.067*** (0.014)		0.097*** (0.028)	
Experience (linear)		0.007*** (0.002)		0.008*** (0.002)
Group [1,2)	0.038*** (0.013)	0.032*** (0.009)		
Group [2,4)	−0.025 (0.022)	−0.041** (0.019)		
Group [6,7)	−0.016 (0.017)	−0.030* (0.018)	−0.045** (0.023)	−0.075*** (0.018)
Group [7,9)			−0.060** (0.024)	−0.043*** (0.012)
Group [9,14)	0.053*** (0.012)	0.051*** (0.010)	0.014 (0.018)	0.013 (0.010)
Group 00	0.044*** (0.013)	0.032*** (0.011)	0.027 (0.019)	−0.007 (0.012)
Experience(Binary)Group [1,2)	0.041*** (0.015)	0.044*** (0.013)	0.022 (0.024)	0.007 (0.014)
Experience(Binary)Group [2,4)	−0.006 (0.018)			
Experience(Binary)Group [6,7)	−0.058 (0.111)			
Experience(Binary)Group [7,9)	−0.027 (0.052)		0.057 (0.143)	
Experience(Binary)Group [9,14)			0.049 (0.045)	
Experience(Binary)Group 00	0.020 (0.016)		0.040 (0.035)	
ExperienceGroup [1,2)	−0.045** (0.021)		−0.085* (0.051)	
ExperienceGroup [2,4)	−0.008 (0.022)		−0.040 (0.049)	
ExperienceGroup [6,7)		0.002 (0.002)		
ExperienceGroup [7,9)		−0.010 (0.053)		
ExperienceGroup [9,14)		−0.003 (0.081)		0.059 (0.060)
ExperienceGroup 00				0.003 (0.003)
winspre:groupGroup 00		0.007* (0.004)		0.008** (0.004)
winspre:groupGroup [4,6)		−0.004 (0.013)		0.002 (0.013)
winspre:groupGroup [6,7)		−0.003 (0.005)		−0.003 (0.007)
Constant	0.226*** (0.012)	0.242*** (0.010)	0.248*** (0.016)	0.278*** (0.010)
Fixed effects By period	Yes	Yes	Yes	Yes
Observations	19,128	19,128	18,795	18,795
R^2	0.022	0.020	0.016	0.018
Residual Std. Error	0.343 (df = 19107)	0.343 (df = 19107)	0.344 (df = 18776)	0.344 (df = 18776)
Note:				*p<0.1; **p<0.05; ***p<0.01

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 1.8: 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 1-5 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.

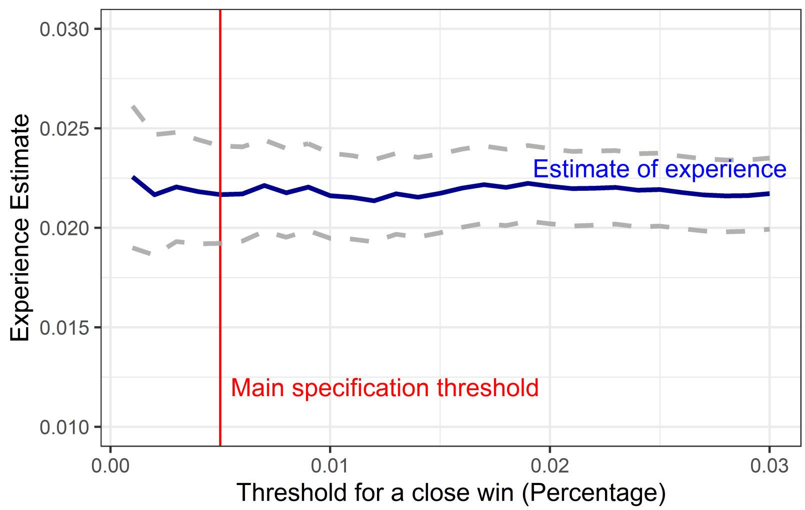

Figure 1-5: Robustness analysis for threshold of close wins

Note: The plot shows the coefficient on experience as in the specification of Panel (5) of table 1.4, that is, the dependent variable is the share of contracts won in period t and the dependent variable is linear experience, i.e. number of contracts won in period $(t-1)$, 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.