Regulatory Capture in Public Procurement: Evidence from Revolving Door Bureaucrats in Japan*

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Abstract We study how hiring public officials affects firms' ability to win government contracts. By linking personnel transitions of public officials and government construction projects awarded to firms in Japan, we find evidence consistent with the exchange of post-public employment for an increase in government contract awards. Our results suggest that quid-pro-quo arrangements are not made as simple bilateral agreements between an individual public official and a firm, but rather maintained through repeated interactions between generations of public officials and firms, consistent with the model of Salant (1995).

Key words: regulatory capture, revolving door, public procurement

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1 Introduction

There is wide-spread agreement among practitioners and academics that government capture harms the public interest. While blatant acts of corruption seem relatively infrequent, at least in developed countries, government officials can be influenced in many subtle ways that are legal, but are just as harmful as outright corruption.

In this paper, we study the effect of hiring public officials on the awards of government contracts. While the practice of hiring public officials is common in many countries, it is considered to be a serious potential risk for conflict of interest and government capture (see, e.g., OECD (2009) and Carpenter (2013)). Using data from Japan, our paper provides empirical evidence consistent with this concern. By linking personnel transitions of public officials to private contractors and government construction projects awarded to firms that hire them, we find evidence consistent with the exchange of post-public employment for government contracts.

The data we use for our analysis come from two sources. The data on public construction projects are obtained from the Ministry of Land Infrastructure and Transportation (MLIT), the largest single procurement buyer in Japan. The MLIT procures most of its projects through auctions. We obtain data on all bids, identity of bidders, and project characteristics for fiscal years 2001 through 2004. The second dataset is the personnel records of public officials published by the National Personnel Authority (NPA). The Authority maintains a list of public officials who are hired by firms that have close ties with the ministry for whom the officials previously worked. We have information on the identity of the public officials and the identity of the employers that hire them, as well as the positions held by the public officials during the five years prior to leaving for the private sector. We merge the procurement data with the personnel data on the name of construction firms.

Our identification and estimation exploit variation in the timing of hires of public officials and the differential change in the bidding pattern of firms. The baseline specification includes firm fixed effects and year-month fixed effects. We find that firms experience an increase of about 7.9%–11.7% (0.9–1.3 percentage points) in the probability of winning a contract after hiring a public

official, but no change in the winning bid. Although the fact that firms can gain an advantage in winning contracts that are tendered through competitive bidding may seem puzzling, public officials in the ministry have discretion over various dimensions of the contract and the bidding process, as we discuss below. The officials also have access to confidential information, such as the identities of the bidders, that they may disclose to some bidders but not others.

While our baseline results are consistent with the quid-pro-quo view of the revolving door, alternative explanations are possible. One explanation is that public officials accumulate valuable skills while in government (often called regulatory schooling), making firms that hire them more efficient. The efficiency gain can lead to a correlation between hiring of a public official and an increase in the winning probability even in the absence of capture. We test this hypothesis by examining the differential effect of hiring government engineers vis-a-vis non-engineers. The non-engineers are public officials who held administrative positions in departments such as benefits and human resources. We find that the estimated effect on the winning probability is not smaller for hires of non-engineers. Relative to engineers, officials in administrative positions seem to have limited capacity to lower the marginal cost of construction. Hence, the regulatory-schooling hypothesis is unlikely to explain the increase in the winning probability condidtional on participation.

Another explanation is that hiring of public officials is correlated with strong growth prospects. For example, if the firm anticipates rapid growth for exogenous reasons, it may hire many workers, some of whom turn out to be retiring officials. Alternatively, public officials may work for firms with good future prospects. In both examples, there would be a correlation between hiring a public official and awards of government contracts. To explore these explanations, we examine more closely the timing around which the probability of winning increases. We find that the increase is concentrated around the year in which the firm hires a public official. In particular, we do not find an upward trend in the years before the firm makes a hire. If our baseline results are driven by expectations of future growth, we should expect a smoother, more gradual upward trend around the hiring of the public officials. The fact that the effect seems to be concentrated around the hiring date suggests that this explanation is unlikely to account for our results.

Our estimates are also informative of how quid-pro-quo arrangements are sustained. In particular, our findings suggest that the quid-pro-quo arrangements seem to be maintained through repeated interactions between a firm and an overlapping generation of public officials, consistent with the model of Salant (1995). The multilateral nature of the quid-pro-quo arrangement suggested by our results implies the presence of organizational involvement.

Our paper is motivated by the ongoing policy discussion in many countries over how best to regulate post-public employment. One of the key themes in the policy debate is how to strike the right balance between possible efficiency gains from the free flow of skilled workers and the potential risk of government capture. While these two considerations are opposite in terms of desirability, empirically distinguishing between them is often quite difficult because they both increase firm value. Given the detailed data on the career of public officials and frequent bidding data on procurement projects, our setting allows us to plausibly differentiate between the two. This is an aspect of the paper that is different from some of the previous work that uses stock price movements around the announcement of hires of government personnel (see, e.g., Luechinger and Moser (2014)). By providing empirical evidence of government capture, our paper can contribute to a more informed policy debate.

More broadly, our paper is also related to the ongoing policy discussion regarding how much discretion should be granted to public officials. The traditional view emphasizes prespecified rules and procedures out of concerns for corruption and government capture. However, the view that seems to have become more dominant recently emphasizes discretion out of concerns for rigidity and inefficiency (see, e.g., Kelman (1990)). The evidence we provide in this paper suggests that efficiency gains from wider discretion should be carefully weighed against the potential costs of government capture.

2 Literature

This paper is most directly related to the set of empirical papers that study the revolving door of public officials, in particular, corporate appointments of former public officials.[1] Cohen (1986) provides one of the first quantitative analyses on the topic. He studies the relationship between voting patterns of FCC commissioners and their post-commission employment in industry using cross-sectional variation. A recent paper by Luechinger and Moser (2014) analyzes stock market returns around announcements of corporate appointments of public officials from the U.S. Department of Defense (as well as political appointments from the private sector). The authors find positive abnormal returns around the announcements. Lucca et al. (2014) examine worker flows between the banking sector and various regulatory bodies, such as the Fed and the FDIC. deHaan et al. (2015) study job transitions of SEC trial lawyers to private law firms and how they are related to the SEC's enforcement outcomes. Tabakovicy and Wollmann (2017) study the behavior of the U.S. Patent and Trademark Office examiners who later work for patent law firms. Much of the evidence is consistent with the regulatory-schooling view of the revolving door, which claims that workers obtain skills while in government and later transition out to firms that have high demand for these skills.[2] On the other hand, empirical evidence for quid-pro-quo is more limited.[3]

This paper also contributes to the literature on the value of a firm's political connectedness. Studies such as Fisman (2001), Fisman et al. (2012), Fisman and Parsley (2009), Goldman et al. (2009), and Acemoglu et al. (2016) identify the value of a firm's political connectedness by utilizing the variation of a firm's stock prices around political events. Fisman (2001), for example, uses news about President Suharto's health and examines how they differentially affected the stock market returns of Indonesian firms that were closely related to Suharto and those that were not. Goldman et al. (2013) examine the political affiliation of corporate boards of directors and how

[1] A number of papers study appointments of corporate executives to public service. An early empirical work by Gormley (1979) looks at the voting behavior of commissioners of the FCC who were formerly from the industry.

[2] See Che (1995) for a theoretical analysis of the revolving door that incorporates both the regulatory-schooling aspect and the capture aspect of the revolving door. See also Dal Bó (2006) for a review of the literature.

[3] Tabakovicy and Wollmann (2017) find that the patent office examiners grant more patents to firms located in the same city as their eventual employers. There is also evidence of quid-pro-quo arrangements associated with job transitions that occur within the private sector. See, e.g., Cornaggia et al. (2016), who look at credit-rating analysts.

it affected the allocation of U.S. government procurement contracts after the Republican Party gained control of both the House and the Senate in the 1994 election. Blanes i Vidal et al. (2012) and Bertrand et al. (2014) study the value of political connections among lobbyists. Khwaja and Mian (2005) study the effect of political connectedness on corporate lending.

3 Public Procurement and Revolving Door in Japan

3.1 Tendering of Construction Projects by the MLIT

Public procurement of construction projects constitutes a significant portion of the overall economy in Japan. During our sample period, the national and local governments of Japan spent a combined total of 25 trillion yen per fiscal year, or about 5% of GDP, on construction projects, on average. Among various government agencies involved in public procurement, the MLIT is by far the largest single procurement buyer. In FY 2001, the first year of our sample period, the MLIT spent around 2 trillion yen or approximately 20 billion dollars on construction.

The MLIT tenders most of the construction projects through auctions.The format of almost all auctions is first-price sealed-bid.[4] Each bidder submits one sealed bid, and the project is awarded to the lowest bidder at a price equal to the bid subject to the secret reserve price.[5] Bidders do not know the identities of the other participating bidders.[6]

Participation in the MLIT auctions is not fully open. Contractors are grouped into different tiers according to firm size every two years, and participation in a given auction is restricted to the set of firms on a particular tier. In addition to segmenting the market by tiers, for auctions with a reserve price of less than 200 million yen, the MLIT does not make a public announcement of the tender, thereby limiting participants to only those who are solicited.

[4]The MLIT procured some projects through a scoring auction. In a scoring auction, the project is allocated to the bidder with the highest score, that is, the highest ratio of quality to price. The bidder's quality is based on the bidder's proposal such as time to completion, impact on the environment, and various characteristics of the bidder including prior experience with similar projects.

[5]If no bid is below the secret reserve price, a second round of bidding occurs. See Kawai and Nakabayashi (2014) for details.

[6]Electronic bidding was phased in starting October of 2001 and completed by the beginning April of 2003.

3.2 MLIT Officials and the Revolving Door

The MLIT has about 45,000 full-time employees.[7] Similar to other ministries in the Japanese government, employment and promotions in the MLIT are based on the merit system. Almost all full-time employees are recruited right after finishing high school or college, and promotions occur from within. The public officials in the MLIT can be classified into very distinct groups based on the way in which they are recruited. For our purposes, the important distinction is between the engineers and the non-engineers.[8] The MLIT hires engineers and non-engineers as separate tracks, and the career paths of the officials are largely defined by the tracks. In fact, almost all positions within the ministry are reserved for either of the tracks.

While public officials enjoy job security through their early to mid career, some officials start getting counseled out beginning in their late 40s.[9] It is common for the ministry to secure employment for those who are counseled out. Typically, the secretary of the ministry acts as the liaison between public officials and those seeking to hire them.

3.3 Potential Channels of Capture

In general, the scope for government capture is closely related to the amount of discretion that public officials have.[10] When officials have more discretionary powers, influencing the actions of public officials in their favor becomes more worthwhile for firms. While the first-price sealed-bid auction format used to allocate projects in our setting leaves little room for discretion once the bids are submitted, government officials can yield influence over the allocation in various ways.

One possibility is to affect the specifications and designs of projects. Different contractors prefer different specifications and designs, depending on the technology that they possess. While contract officers must follow internal guidelines, officers may still have room to exercise discretion

[7] Reference Material 6, Annual Report of the National Personnel Agency (2005).

[8] Another important distinction is between the elite officials who are recruited through the Level 1 Examination and the non-elites. Because the elite officials rarely start working in the private sector immediately after resigning from the ministry, most of the officials in our data set are non-elites.

[9] See Inatsugu (2011) for a brief summary of the personnel management practices in Japanese Ministries.

[10] See, e.g., OECD (2017) for details.

in adjusting the specifications and designs of projects. In a particularly egregious incident, the officials in the Ministry of Defense were convicted of manipulating the specification and design of the next-generation UH-X multipurpose helicopter to favor Kawasaki Heavy Industries over Fuji Heavy Industries.[11]

Another possibility for public officials to affect contract allocation is to change the size of procurement projects by splitting large projects into smaller ones. Recall from section 3.1 that participation constraints in the MLIT auctions are determined by the reserve price. For example, in most regions, the contractors for general construction work are partitioned into four groups, tier A through tier D. In 2004, tier-A firms were eligible to participate only in auctions with a reserve price above 690 million yen. Similarly, participation for tier-B firms was limited to auctions with a reserve price between 300 million and 690 million yen; tier-C firms were limited to auctions with a reserve price between 60 million and 300 million yen; and tier-D firms were limited to auctions with a reserve price less than 60 million yen. Moreover, for projects with a reserve price of less than 200 million yen, the MLIT does not make a public announcement of the tender.[12] Instead, the MLIT invites a subset of firms in the corresponding tier with slightly different requirements depending on whether or not the reserve price is above 100 million yen.[13]

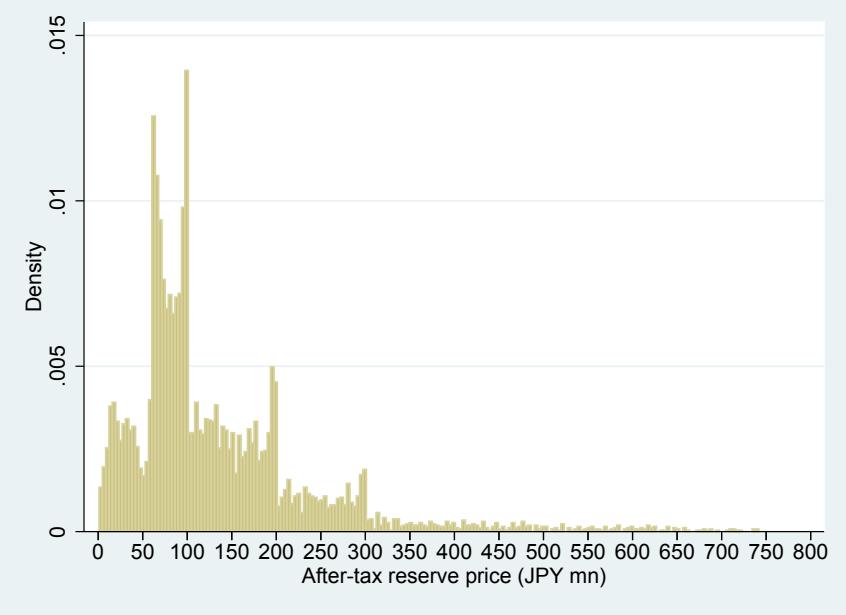
By adjusting the size of the procurement project, public officials can target a specific firm to be included in and excluded from the auction. The histogram of the reserve price in Figure 1 is consistent with the possibility that public officials exercise discretion in setting the reserve price. The figure plots the reserve price of general construction work in FY 2004 in our sample. The histogram shows clear bunching around the thresholds that determine the set of firms that are eligible to participate. It is quite conceivable that public officials exercise discretion over project size for the benefit of firms that employ former MLIT officials. In fact, news reports have alleged

[11] See a report issued by the Self Defense Ministry summarizing the investigation regarding the selection of firms in the development of UH-X multipurpose helicopter (July 31, 2013).

[12] For projects with a reserve price above 200 million yen, a public announcement of the tender is made, and all interested contractors on the corresponding tier can apply to be a participant.

[13] For projects between 100 million and 200 million yen, the invited firms that wish to participate submit documents to the MLIT, including a summary of similar projects completed in the recent past and a brief construction plan. Based on the application documents, the MLIT chooses the set of participants. For projects with a reserve price of less than 100 million yen, all of the invited firms can participate in the auctions.

Figure 1: Distribution of Project Size for General Construction Work in Fiscal Year 2004



Notes: We exclude projects procured in Hokkaido, because Hokkaido imposed different participation restrictions.

that invitation to participate in floodgate auctions was restricted to firms that had employed former MLIT officials.[14]

Another possible channel of government capture is leakage of confidential information to specific firms, such as the identity of the participating bidders in upcoming auctions and plans regarding future procurement projects. For example, in a criminal case involving the Hokkaido Regional Development Bureau of the MLIT, the officials were charged with, among other things, leaking confidential information such as the identity of the participating bidders to former colleagues who began to work for contractors after leaving public service.[15]

Lastly, the MLIT officials can suggest desirable winners of contracts to potential bidders. For example, in the collusion case of floodgate manufacturers in 2007, it was found that the MLIT officials were found actively involved in allocating contracts. In particular, the officials suggested to the collusion leaders that the contract allocation should be matched to the number of ex-MLIT

[14] See Mainichi Shimbun (2007a,b).

[15] See page 5 of Report by the Committee on Prevention of Bid Rigging in Hokkaido Regional Development Bureau (April 28, 2009).

officials hired by each bidder.[16] Similarly, in the case involving the MLIT officials in Hokkaido, the officials suggested to the potential bidders the names of the firms that would be desirable winners of contracts.

4 Data

We use two datasets in our analysis: data on construction projects procured by the MLIT though standard first-price sealed-bid auctions and personnel data that track transitions from the MLIT to private businesses.[17] The bidding data span fiscal years 2001 through 2004 (April 2001 through March 2005) and cover most of the construction works auctioned by the MLIT during the period.[18] The data include information on bids, identity of the bidders, secret reserve prices, auction dates, and project characteristics.

We obtained the personnel data of public officials from the NPA for fiscal years 2001 through 2004. The Authority publishes information on government officials who are hired within two years of resignation by firms that have close ties to the ministry for which the official worked. The data contain information on the names of the government officials, the positions that the officials held in the last five years prior to leaving, the name of the hiring firm, and the date on which the official started employment with the firm. One limitation of the NPA dataset is that it does not record public officials who are initially hired by non-profit organizations and only later hired by firms that have close ties to the ministry. Because most high-ranked officials work in non-profit organizations before moving to for-profit firms, our sample consists mostly of mid-ranked officials.[19]

In order to use within-firm variation to estimate the effect of hiring public officials, we restrict our baseline sample to firms that hire at least one public official during our sample period.

[16] Mainichi Shimbun (January 12, April 16, 2007).

[17] We do not use unit-price auctions, scoring auctions, and sole-source contracts, which account for 1.2%, 0.2%, and 0.8% of the projects tendered by the MLIT, respectively.

[18] We do not use data from fiscal years 2005 and later, because the MLIT introduced substantial changes in the auction format starting in mid-2005. In particular, most auctions became scoring auctions by the end of year 2006, and participation restrictions were significantly relaxed during the same time.

[19] Our sample does not include many officials hired through the Type I national civil service examination. However, they account for a small fraction of the MLIT employees.

Table 1: Summary Statistics of Auctions

Reserve price	# of bidders	Winning bid	Winning bid/ Reserve price	Observations
129.6	10.12	124.0	0.957	33,259
(194.7)	(2.127)	(184.8)	(0.0543)	

Notes: Standard deviations are in parentheses. Reserve price and Winning bid are in JPY million.

Table 2: Summary Statistics of Firms

# of auctions participated	# of auctions won	Revenue	# of officials employed	Observations
250.6	27.31	3,435.0	1.161	242
(413.6)	(64.48)	(5,886.8)	(0.440)	

Notes: Standard deviations are in parentheses. Revenue is in JPY million.

Moreover, we drop firms that never participate in auctions before hiring a public official, or never participate in auctions after hiring a public official. Dropping these firms reduces measurement error caused by mergers and changes in firms' names.[20] We also drop four firms that are acquired or file for bankruptcy.[21] This leaves us with a sample of 242 firms and 33,259 auctions.[22]

Table 1 reports the summary statistics of the auctions in our sample. On average, the reserve price is 129.6 million yen, and the number of bidders is 10. The winning bid is around 124 million yen, which is equivalent to around 96% of the reserve price. Table 2 reports the summary statistics of the firms in our sample. The contractors in our sample, on average, participate in 251 auctions, win 27 auctions worth a total of around 3.4 billion yen, and hire 1.2 public officials during our sample period. The value of contracts awarded to the firms in our sample is close to 200 billion yen per fiscal year, accounting for about 0.5% of the national government tax revenue.

The contractors in our sample employ 281 officials in total during the sample period. The average age of public officials in our sample is 56.5 at the time of their resignation, with a standard

[20]We identify a firm by its name and region. Hence, if the name of a firm changes, we treat the same firm as different firms. By restricting the sample to firms that bid at least once before and after hiring a public official, we reduce the risk that a firm changes its name during the sample period.

[21]The Online Appendix contains estimation results that include these four firms for robustness.

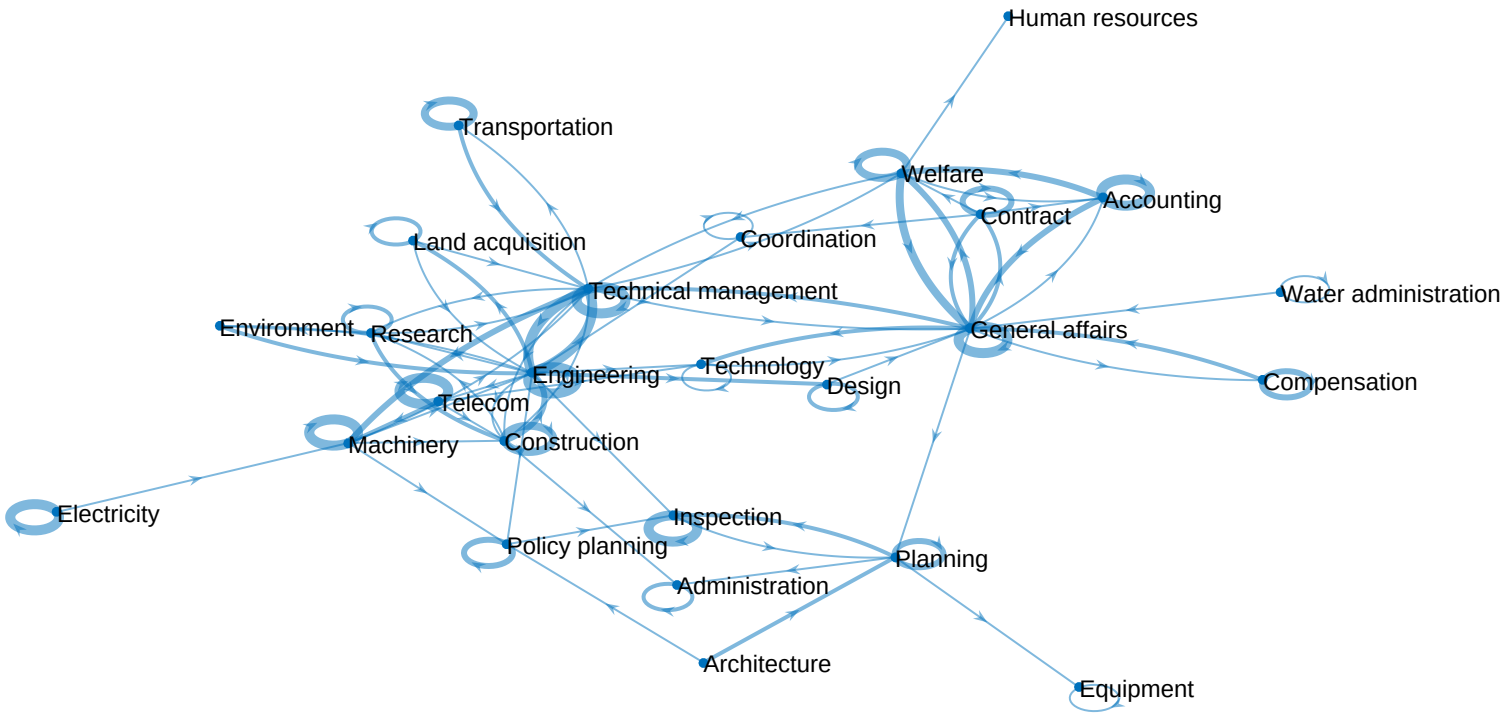
[22]During fiscal years 2001 through 2004, there are a total of 63,597 first-price sealed-bid auctions and 27,216 firms that participate in them.

deviation of 2.7.[23] Figure 2 shows the (intra-ministry) transitions in the positions of public officials in the last five years before their resignation. The nodes in the figure correspond to the top 30 divisions that occur most frequently in our sample.[24] The directed arcs between nodes represent transitions from division to division, and their thickness is proportional to the frequency of transitions. The figure shows that two clusters exist: one centered around Engineering, Technical Management, and Telecom divisions and the other centered around General Affairs and Accounting divisions. The figure reflects the fact that public officials in our sample are divided into engineers and non-engineers, in which the former work at technology-oriented positions and vice versa.

[23] We observe their ages at the time of their resignation for 275 out of 281 officials.

[24] We do not graph the divisions that have no link to the other top 30 divisions (Disaster prevention, Road, Development, and Maintenance).

Figure 2: Transitions in the Positions of Public Officials



Notes: Nodes correspond to the top 30 divisions in which public officials in our sample worked during the five years before their resignation. Directed arcs between nodes represent transitions from division to division, and their thickness is proportional to the frequency of transitions. We do not graph the divisions that have no link to the other top 30 divisions (Disaster prevention, Road, Development, and Maintenance).

5 Empirical Results

5.1 Main Results

We begin by estimating the effect of hiring a public official on the probability of winning an auction. In particular, we consider a two-way fixed-effects linear probability model:

$$Winner_{ij} = \beta B_{ij} + g(Num.bid_j) + d_t + f_{ir} + \epsilon_{ij}, \tag{1}$$

where $Winner_{ij}$ is a dummy variable that is equal to 1 if firm i is the winner of auction j. In the first regression, the main variable of interest is B_{ij}, which is the cumulative number of public officials hired by participating firm i by the date of auction j. The coefficient β captures the change in the winning probability after hiring a public official. $g(Num.bid_j)$ is a non-parametric function of the number of bidders in auction j, d_t is a year-month fixed effect, and f_{ir} is a firm-region fixed effect.[25][26]

In order to track how the winning probability changes over time, we also consider the following model:

$$Winner_{ij} = \sum_{K \in \mathcal{K}} \beta^K B_{ij}^K + g(Num.bid_j) + d_t + f_{ir} + \epsilon_{ij}, \tag{2}$$

where $\mathcal{K} = \{(-\infty, -2), (-2, -1), (-1, 0), (0, 1), (1, 2)\}$. $\mathcal{K}$ is a set that indexes the time period in which firm i hires a public official relative to auction j.

The main variables of interest are B_{ij}^K ($K \in \mathcal{K}$). $B_{ij}^{(0,1)}$ denotes the number of public officials hired by participating firm i within one year after the date of auction j. $B_{ij}^{(1,2)}$ denotes the number of public officials hired after one year but before two years from the date of auction j. $B_{ij}^{(-1,0)}$, $B_{ij}^{(-2,-1)}$, and $B_{ij}^{(-\infty,-2)}$ are similarly defined. For example, $B_{ij}^{(-\infty,-2)}$ denotes the number of public officials hired more than two years before the date of auction j.[27] β^K is a coefficient associated

[25] A firm can have a strong presence in one region but not in others. We include a firm-region fixed effect to account for this heterogeneity.

[26] We include separate dummies for the number of bidders when that is less than 15. $g(Num.bid_j)$ takes the same value if the number of bidders is at least 15.

[27] Consider firm i that hires a public official on July 1, 2003, a second one on April 1, 2004, and a third one on April

with B_{ij}^K.

Note that there is a deterministic relationship between the regressand $Winner_{ij}$ and $Winner_{i'j}$ in models (1) and (2); that is, $Winner_{ij} = 1$ implies $Winner_{i'j} = 0$ for any $i' \neq i$. Thus, we pick at most one firm for each auction for estimation when multiple firms in our sample participate in the same auction. For robustness, we consider three different procedures for choosing at most one firm from each auction as follows.

In the benchmark procedure, we choose the firm whose number of participations in auctions is smallest during our sample period (Procedure I). This method improves the efficiency of the firm fixed-effect model we use, because it reduces the risk of dropping a group due to an insufficient number of observations. We report the estimation results based on Procedure I in the first and fourth columns of Table 3. In our second procedure, we randomly pick one firm (Procedure II). The second and fifth columns of the table show the coefficient and standard error averaged over 100 regression results. In our third procedure, we simply drop any auction in which more than two firms from our sample participate (Procedure III). The third and sixth columns of the table correspond to the estimation results based on Procedure III. In each procedure, we cluster the errors at the firm-region level.

The first three columns of Table 3 report the estimation results of equation (1). We find that our estimate of β ranges between 0.86 percentage points and 1.28 percentage points. Given that the winning probability is about 10.9% on average, the estimated increase amounts to around 7.9%–11.7% of this average. Our estimates are statistically significant at the 5% level, regardless of the procedure we use.

While a positive estimate of β in equation (1) is consistent with the presence of quid-pro-quo arrangements between contractors and public officials, it is also consistent with alternative hypotheses. In particular, hiring of public officials may be correlated with factors that are unobserved to the researcher that put the firm on a strong growth trajectory. This can be the case if the firm anticipates rapid growth for exogenous reasons and hires many workers, some of whom turn out to

1, 2006. For auction j dated on August 1, 2003, $(B_{ij}^{(-\infty,-2)}, B_{ij}^{(-2,-1)}, B_{ij}^{(-1,0)}, B_{ij}^{(0,1)}, B_{ij}^{(1,2)}) = (0, 0, 1, 1, 0)$.

be retiring officials. Alternatively, public officials may work for firms with good future prospects, which would also generate a similar correlation. In order to explore these alternative explanations, we now examine how the winning probability evolves over time.

The last three columns of the table show the estimation results of equation (2). The general pattern is that the estimates of $\beta^{(1,2)}$ and $\beta^{(0,1)}$ are negative or relatively small in magnitude, while the estimates of $\beta^{(-1,0)}$, $\beta^{(-2,-1)}$, and $\beta^{(-\infty,-2)}$ are positive. Focusing on column (2)-(I), for example, we find that the estimates of $\beta^{(1,2)}$ and $\beta^{(0,1)}$ are -0.387 and -0.466, respectively. On the other hand, we find that the estimates of $\beta^{(-1,0)}$, $\beta^{(-2,-1)}$, and $\beta^{(-\infty,-2)}$ are 0.672, 0.394, and 0.833, respectively. The fact that $\beta^{(1,2)}$, and $\beta^{(0,1)}$ are negative or close to zero implies that there is no increase in the winning probability before the hiring takes place. The fact that $\beta^{(-1,0)}$, $\beta^{(-2,-1)}$ and $\beta^{(-\infty,-2)}$ are positive implies that the winning probability increases immediately after hiring a public official and remains high. The coefficients reported in columns (2)-(II) and (2)-(III) show the same general pattern. In particular, we note that the estimates of $\beta^{(-1,0)}$ and $\beta^{(-\infty,-2)}$ reported in column (2)-(III) are positive and statistically significant at the 5% and 10% level, respectively.

In order to visualize the relative magnitude of the coefficients, Figure 3 plots the estimates of $\{\beta^K\}$ and their confidence intervals. Panels (2)-I, (2)-II, and (2)-III of the figure correspond to the fourth, fifth, and sixth columns in Table 3, respectively. The plots of the coefficients show a notable increase in the winning probability between one year before and one year after hiring a public official.

The estimation results of equation (2) shed light on whether or not the change in the winning probability is attributed to factors that are unobserved to the researcher that put the firm on a strong growth trajectory. If the change in the probability is due to firm growth, we would expect a smooth, gradual upward trend in the winning probability. Because our results show a concentrated increase in the winning probability around the time of hiring a public official, the change in the winning probability is unlikely to be associated with firm growth.

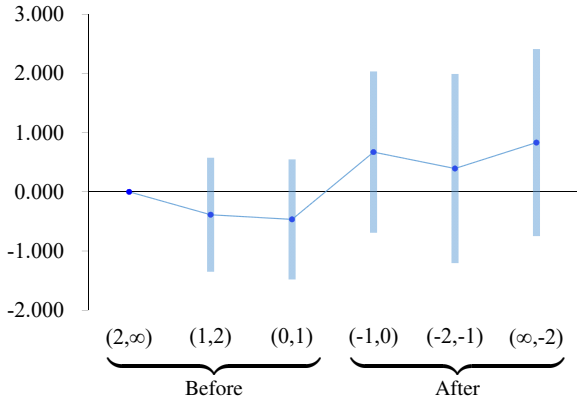
We next study whether the increase in the winning probability is driven simply by more aggressive bidding by firms that hire public officials. In particular, we estimate the effect of hiring

Table 3: The Effect of Hiring a Public Official on Winning Probability

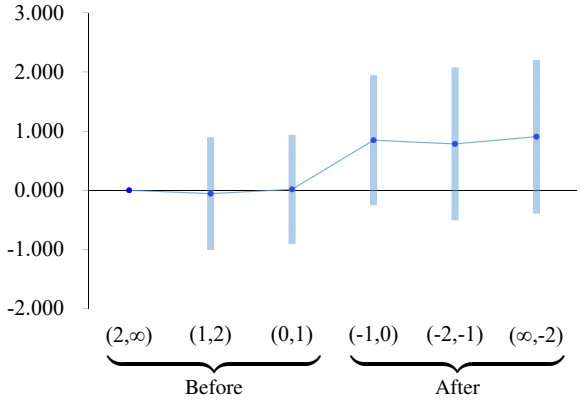
	(1)			(2)		
	(I)	(II)	(III)	(I)	(II)	(III)
β	1.055**	0.864**	1.280**			
	(0.454)	(0.410)	(0.642)			
$\beta^{(1,2)}$				-0.387	-0.0569	0.265
				(0.492)	(0.487)	(0.779)
$\beta^{(0,1)}$				-0.466	0.0151	0.687
				(0.518)	(0.470)	(0.681)
$\beta^{(-1,0)}$				0.672	0.847	1.839**
				(0.696)	(0.560)	(0.901)
$\beta^{(-2,-1)}$				0.394	0.784	1.484
				(0.816)	(0.659)	(1.035)
$\beta^{(-\infty,-2)}$				0.833	0.907	1.649*
				(0.807)	(0.664)	(0.971)
Number of bidders	X	X	X	X	X	X
Month-year fixed effect	X	X	X	X	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	33,259	33,259	16,753	33,259	33,259	16,753
Within R^2	0.014	-	0.020	0.014	-	0.020

Notes: Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004. When there are multiple firms in the same auction that hire public officials between fiscal years 2001 and 2004, we either include only one firm or drop the auction entirely. In the first and fourth columns, we keep the firm whose number of participations in auctions is smallest during our sample period. In the second and fifth columns, we randomly keep one firm and run the regression. After repeating this process 100 times, we report the average coefficient and standard error as well as statistical significance according to the t-value based on them. In the third and sixth columns, we exclude all auctions in which multiple firms hire public officials between fiscal years 2001 and 2004.

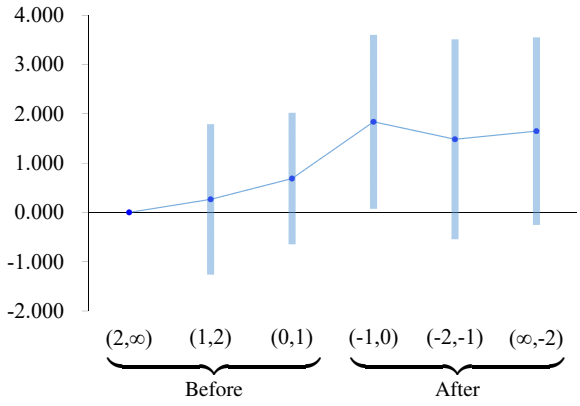
Figure 3: The Effect of Hiring a Public Official on Winning Probability



(2)-I



(2)-II



(2)-III

Notes: The estimates of $\{\beta^K\}$ and their 95% confidence intervals (in bars) are presented. Panel (2)-I, Panel (2)-II, and Panel (2)-III of the figure correspond to the fourth, fifth, and sixth columns in Table 3, respectively.

a public official on the winning bid by using the following two-way fixed-effects linear regression models:

$$Win.bid_{ij} = \beta B_{ij} + g(Num.bid_j) + \gamma reserve_j + d_t + f_{ir} + \epsilon_{ij}, \quad (3)$$

$$Win.bid_{ij} = \sum_{K \in \mathcal{K}} \beta^K B_{ij}^K + g(Num.bid_j) + \gamma reserve_j + d_t + f_{ir} + \epsilon_{ij}. \quad (4)$$

$Win.bid_{ij}$ is the winning bid of firm i in auction j. We consider two ways of defining $Win.bid_{ij}$, one by using the raw bid and the other by using the normalized bid, defined as dividing the raw bid by the reserve price. Using the normalized winning bid allows us to control for the heterogeneity in the size of projects.[28] The main variables of interest in equations (3) and (4) are B_{ij} and B_{ij}^K as we defined earlier. We include year-month fixed effects, d_t, and firm-region fixed effects, f_{ir}, in both equations. The variable $reserve_j$ is the reserve price of auction j, which is included as a regressor only when we use the raw bid for the regressand.

The first two columns of Table 4 present the estimation results of regression (3). Column (3)-(a) corresponds to the raw bid and column (3)-(b) corresponds to the normalized winning bid. We find that the relationship between hiring a public official and the winning bid is small and statistically insignificant. For example, our estimate of β in column (3)-(a) is about -430 (thousand yen), which is about 4,000 USD, or 0.3% of the average reserve price. Our estimate of β in column (4)-(b) is only -0.06 percentage points. Hence, we do not find evidence of more aggressive bidding after the hiring date.

We next present our estimate of the effect of hiring a public official on firm revenue. In particular, we estimate the following models:

$$Revenue_{irt} = \beta \overline{B}_{it} + d_{rt} + f_{ir} + \epsilon_{irt}, \quad (5)$$

$$Revenue_{irt} = \sum_{K \in \mathcal{K}} \beta^K \overline{B}_{it}^K + d_{rt} + f_{ir} + \epsilon_{irt}, \quad (6)$$

[28] Substantial heterogeneity exists in the size of projects on which a given firm bids. The ratio between the maximum and the minimum project size on which a given firm bids is 79 at the median.

where $Revenue_{irt}$ is the logged annual award amount of firm i in region r for fiscal year t. In equation (5), the main variable is $\overline{B}_{it}$, which is the fiscal-year equivalent of B_{ij} in equations (1) and (3). Specifically, $\overline{B}_{it}$ is the number of public officials hired up to fiscal year $t-1$ plus the number of public officials hired in fiscal year t, prorated by the fraction of the year during which each public official is in employment.[29] Similarly, $\{\overline{B}_{it}^{K}\}$ in equation (6) are the fiscal-year equivalent of $\{B_{ij}^{K}\}$ in equations (2) and (4). For example, $\overline{B}_{it}^{(0,1)}$ is the number of public officials hired by firm i within one year of fiscal year t, so that $\overline{B}_{it}^{(0,1)}$ is simply the difference between $\overline{B}_{it+1}$ and $\overline{B}_{it}$.[30] We include year-region fixed effects, d_{rt}, and firm-region fixed effects, f_{ir}, in both equations[31].

The fifth and sixth columns of Table 4 report our estimates of equations (5) and (6). Our point estimate of β, reported in the third column, is 0.0443. This result suggests that a firm experiences a revenue increase of 4.4% after hiring a public official, although the estimated effect is not statistically significant. The sixth column reports our estimates of $\{\beta^{K}\}$. Our point estimate of $\beta^{(-1,0)}$ (0.0387) is greater than the estimate of $\beta^{(0,1)}$ (-0.0127), which implies that firm revenue increases (by about 5.1 percentage points) in the year after the firm hires a public official, relative to the year before. However, we also find that the estimate of $\beta^{(-\infty,-2)}$ is -0.103, suggesting that the firm's revenue two years later is less than that in the fiscal years before hiring a public official.

Overall, the coefficients in equations (5) and (6) are not precisely estimated. The reason for the large standard errors and the estimate of $\beta^{(-\infty,-2)}$ may be due to issues with aggregating firm revenue at the yearly level. If a firm merges or exits part way through the fiscal year, this appears as a large drop in the revenue of the pre-merger firm. As we show in the Online Appendix, when we estimate regression (6) on a restricted sample of firms whose year-to-year revenues are relatively stable, the estimate of $\beta^{(-\infty,-2)}$ becomes positive.

[29] Consider again the example of footnote 27. $\overline{B}_{i2003}$ is equal to $275/366$. The numerator corresponds to the number of days in which the official worked for the firm in 2003 i.e., from July1, 2003 to the end of the fiscal year (March 31, 2004). The denominator is the number of days in FY 2003.

[30] In the case of footnote 27, $\overline{B}_{i2003}^{(-\infty,-2)}$ is 0, $\overline{B}_{i2003}^{(-2,-1)}$ is 0, $\overline{B}_{i2003}^{(-1,0)}$ is $\frac{275}{366}$, $\overline{B}_{i2003}^{(0,1)}$ is $1+\frac{91}{365}$, and $\overline{B}_{i2003}^{(1,2)}$ is 0.

[31] We control for region-specific time trends in both equations, because we observe differential time trends in the total number of auctions across regions.

Table 4: The Effect of Hiring a Public Official on Winning Bids and Firm Revenue

	(3)		(4)		(5)	(6)
	(a)	(b)	(a)	(b)		
β	-431.6	-0.0602			0.0443	
	(428.6)	(0.166)			(0.0477)	
$\beta^{(1,2)}$			631.3	0.0748		0.0304
			(530.9)	(0.196)		(0.0508)
$\beta^{(0,1)}$			610.2	0.108		-0.0127
			(546.1)	(0.179)		(0.0505)
$\beta^{(-1,0)}$			88.13	0.0409		0.0387
			(600.9)	(0.225)		(0.0594)
$\beta^{(-2,-1)}$			-135.9	-0.0541		0.000962
			(721.0)	(0.263)		(0.0748)
$\beta^{(-\infty,-2)}$			-266.5	0.0526		-0.103
			(670.1)	(0.241)		(0.0685)
Reserve price	X	-	X	-	-	-
Number of bidders	X	X	X	X	-	-
Time fixed effect	X	X	X	X	-	-
Region-time fixed effect	-	-	-	-	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	6,610	6,610	6,610	6,610	2,003	2,003
Within R^2	0.996	0.029	0.996	0.029	0.057	0.062

Notes: The dependent variable in the first and third columns is the winning bid. The dependent variable in the second and fourth columns is the winning bid as a percentage of the reserve price. The dependent variable in the last two columns is the logged annual revenue per region-firm. Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004.

5.2 Quid-pro-quo or Regulatory Schooling?

In the previous subsection, we document evidence suggesting that firms that hire public officials increase the winning probability. In general, firms benefit from hiring public officials through quid-pro-quo or through efficiency gains attained by the human capital of public officials (regulatory schooling). Although these two channels both increase firm value, they have opposite implications for policy and welfare. To differentiate between the two channels, we focus on the distinction between non-engineers and engineers as defined in section 4. Given our definition of non-engineers and engineers, non-engineers seem less likely to lower the marginal cost of construction. Hence, under the regulatory-schooling hypothesis, the effect of hiring engineers on the winning probability is likely to be driven by hiring of engineers.

We separately estimate the effect of hiring non-engineers and engineers on the winning probability as follows:

$$Winner_{ij} = \beta B_{ij} + \gamma Z_{ij} + g(Num.bid_j) + d_t + f_{ir} + \epsilon_{ij}, \quad (7)$$

$$Winner_{ij} = \sum_{k \in \mathcal{K}} \beta^K B_{ij}^K + \sum_{k \in \mathcal{K}} \gamma^K Z_{ij}^K + g(Num.bid_j) + d_t + f_{ir} + \epsilon_{ij}, \quad (8)$$

where B_{ij} and Z_{ij} and are the cumulative number of all public officials (both engineering and non-engineering) and non-engineering officials hired by participating firm i by the date of auction j, respectively. B_{ij}^K and Z_{ij}^K are the number of all public officials and non-engineering officials hired during interval K relative to the date of auction j, respectively. B_{ij} and B_{ij}^K are the same variables that we defined in equations (3) and (4). β and β^K capture the effect of hiring an engineering official. γ and γ^K capture the differential effect of hiring a non-engineer official relative to an engineer. As we explained in our discussion of equations (1) and (2), we consider three different ways of choosing at most one firm from each auction for estimation.

The first three columns of Table 5 report the estimation results of equation (7). We find that our estimates of β range from 0.88 percentage points to 1.15 percentage points, which are comparable to the corresponding estimates of β in equation (1). Our estimates of γ are positive or close to zero,

ranging from -0.0942 to 0.595. These results suggest that the effect of hiring a non-engineering public official on the winning probability is unlikely to be smaller than that of hiring an engineering public official.

The next three columns report the estimation results of equation (8). The first five rows correspond to the estimates of $\{\beta^K\}$. The point estimates of the first two coefficients corresponding to the pre-period are negative or relatively small in magnitude, whereas the estimates of the next three coefficients corresponding to the post-period are positive. These estimates are similar to the corresponding estimates of $\{\beta^K\}$ in equation (2). The next five rows report the estimates of $\{\gamma^K\}$, which capture the differential effect of hiring a non-engineer at period K. We find that the estimates of $\gamma^{(1,2)}$ are negative, whereas the estimates of $\gamma^{(0,1)}$ and $\gamma^{(-1,0)}$ can be negative or positive, depending on the procedure we use. We also find that the estimates of $\gamma^{(-2,-1)}$ and $\gamma^{(-\infty,-2)}$ tend to be positive. Overall, we find no positive differential effect of hiring a non-engineer during the pre-period and the first year of the post-period. From the second year of the post-period, there seems to be a positive differential effect of hiring a non-engineer. These results suggest that hiring non-engineering public officials is as effective as hiring engineers.

5.3 Manipulation of Project Size as a Channel of Quid-pro-quo

We now explore whether government officials use discretion in setting the reserve price to influence the allocation of public projects. As we dicussed in Section 3.2, the reserve price determines the set of eligible bidders. Hence, making small adjustments to the project size around the threshold can serve as potential channel through which government officials can affect allocation of contracts.

In order to explore this possibility, we first identify the thresholds that are relevant for influencing project allocation. The histogram of project size exhibits bunching at 100 million yen, 200 million yen, and at each of the (project category-specific) thresholds that determine the tier of bidders who are eligible to bid. However, not all of these values may be relevant for bidder participation. In particular, 100 million yen does not correspond to any important threshold that

determine how the government chooses the set of bidders.[32]

We identify the relevant thresholds by looking for a sharp change in the characteristics of the participating bidders around each of the thresholds. Specifically, we compare the average one-year backlog of the bidders who participate in auctions that are right above and right below a given threshold as follows:

$$\log(1 + Backlog_{ij}) = \eta 1_{\{reserve_j < T\}} + \gamma \log(reserve_j) + d_t + \varepsilon_{ij}.$$

The left-hand side of the regression is the log backlog of firm i in auction j. The first regressor is a dummy for an auction whose reserve price is below threshold T, the second regressor is the reserve price of an auction, and d_t is a year dummy. The coefficient on the first regressor, η, captures the effect of the threshold on the firm's backlog net of a linear trend, which is captured by $\gamma \log(reserve_j)$. We estimate this regression on the sample of auctions whose reserve falls within either $\pm$10 million yen or $\pm$20 million yen of T.

Table 6 presents the results of the regression. The top block presents our estimates for the case when we set T equal to 100 million yen. The left column corresponds to the results when we estimate the regression using the sample of auctions with a reserve price between 90 million and 110 million yen. The right column corresponds to the results when the sample consists of auctions with a reserve price between 80 and 120 million yen. In this block, our estimate of η is not statistically distinguishable from zero, which suggests that the characteristic of the bidders do not change around this threshold. This finding is consistent with the fact that 100 million yen does not coincide with thresholds that determine the tier of bidders who are eligible to bid or with other obvious thresholds that determine the rules of the auction. The results seem to indicate that this threshold does not play an important role in restricting bidder participation.

The middle block reports the estimation results when we set the threshold T to 200 million yen. We find that the coeficient on η is positive and statistically significant. This implies that the

[32]Recall from Section 3.3 that the main difference between auctions that are smaller than 100 million and those that are bigger seems to be the procedural simplicity for the buyer.

backlog of firms that participate in auctions just below the threshold of 200 million is actually larger than that of the firms that participate in auctions just above it.

Recall that 200 million yen is the threshold that determines whether the government makes a public announcement of a tender. All interested bidders who qualify can participate in an auction whose reserve price is above this threshold. Below the threshold of 200 million, however, there is no announcement of the tender, and bidder participation is by invitation only. The fact that our estimate of η is different from zero confirms that 200 million yen plays an important role in restricting bidder participation. Moreover, the fact that our estimate is positive suggests that the goverrnment is inviting firms with higher backlog more frequently to invitation-only auctions. This is consistent with the possibility that certain firms are treated preferentially.

The bottom block of the table corresponds to the regression results when we set T to be the values that determine the tier of the eligible bidders. In particular, for each region, project, fiscal year triplet, we identify the thresholds used to determine the set of eligible bidders. We then take auctions whose reserve price falls within either ± 10 million yen or ± 20 million yen of the threshold. The backlog of the bidders participating in these auctions constitute the estimating sample for the top block. The left column corresponds to the results when we take a band of 10 million yen and the right column corresponds to that of a band of 20 million yen. Our estimate of η is negative and significant, which suggests that the threshold is relevant for determining the set of eligible bidders.

We now compare the effect of hiring a public official when the project size lies around the thresholds. In particular, we consider estimating the following regression.

$$Winner_{ij} = \beta B_{ij} + \beta_T (B_{ij} \times 1_{\{T-\Delta \leq reserve_j < T\}}) + g(Num.bid_j) + d_t + f_{ir} + \epsilon_{ij}, \qquad (9)$$

This regresion is the same as (1), but with the addition of an extra term, $\beta_T (B_{ij} \times 1_{\{T-\Delta \leq reserve_j < T\}})$. This term captures the differential effect of hiring a public official when the reserve price is between $T - \Delta$ and T. A coefficient of β_T that is positive suggests that employment

of governement officials increases the probability of winning the auction when the reserve price is just below T.[33] The results in Table 6 suggests that the threshold of 100 million yen may serve as a good placebo.

Table 7 presents the results. The top block of the table corresponds to the case in which T is set to 100 million yen. The first three columns correspond to Δ equals 10 million yen and the last three correspond to Δ equals 20 million yen. There are three columns each that correspond to the way the estimating sample is constructed as we explained for Table 7. The table shows that β_T is negative, and the net effect ($\beta + \beta_T$) is also close to zero. These results seem to imply that the effect of hiring a public official is not stronger when the project is just below 100 million yen. In the bottom block of the table, we present results for the case when T is set to 200 million yen and to each of the participation thresholds. In this block, we estimate positve coefficients for β_T, suggesting that there is a stronger relationship between hiring a government official and winning the auction when the auction is close to these thresholds. Overall, the results of the table are consistent with the hypothesis that there is a differential effect on the winning probability around thresholds that coincide with participation restrictions.

5.4 Discussion

Our findings in the previous sections provide a plausible explanation of the nature of the quid-pro-quo agreement between the MLIT and the contractors. In principle, hiring a public official can affect firm performance before or after the hiring date. If the effect is observed before the hiring date, it is likely to reflect quid-pro-quo, in particular, one in which the official is favoring the eventual employer while he is still working in the government. Note that this particular form of quid-pro-quo arrangement is bilateral; that is, an individual official and a firm exchange special favor for employment without necessarily involving other officials.

If the effect is observed after the hiring date, it is likely that the firm is favored by the gov-

[33] We focus on auctions with a reserve price just below the threshold because bunching occurs to the left of the threshold except at 60 million yen for civil engineering projects.

ernment in return for the employment of the public official unless the firm is benefiting from the expertise of the public official (regulatory schooling). Note that this case is a quid-pro-quo arrangement between the government and the firm, but it is not a simple bilateral agreement between the individual official and the firm. In this case, the public official employed by the firm is not the firm's direct benefactor. Rather, the current officials are likely to be favoring the firm in return for the employment of their retiring colleagues, perhaps with the expectation that by doing so they will gain employment in the future. The multilateral nature of the quid-pro-quo arrangement sustained by generations of officials is similar to the model of Salant (1995). In his model, a quid-pro-quo arrangement is an equilibrium phenomenon that involves an overlapping generation of officials: the firm hires officials with the expectation that it will be treated favorably in the future, and the current officials favor the firm with the expectation that they will be hired by the firm in the future.

Indeed, a criminal case involving the MLIT officials in Hokkaido illustrates this point. In this case, three MLIT officials along with 10 public officials were convicted of obstruction of auctions. The three convicted officials were found to have suggested to the potential bidders the names of the firms that would be desirable winners of contracts. The suggested winners reflected the number of public officials that firms had hired. According to the MLIT's report issued in the aftermath of the case, the officials did so in order to secure employment for retiring colleagues, with the expectation that future generation of MLIT officials would do likewise.[34] Overall, the case of Hokkaido is consistent with the view that the quid-pro-quo arrangement suggested by our estimates is an equilibrium phenomenon involving an overlapping generation of officials.

An important question regarding government capture is whether it is an incidence caused by a few malfeasant individuals or a more serious problem with an organizational involvement. In the case of the MLIT, the multilateral nature of the quid-pro-quo arrangement suggested by our results is more consistent with the latter.

[34]See page 47 of Report by the Committee on Prevention of Bid Rigging in Hokkaido Regional Development Bureau (April 28, 2009)

Table 5: The Effect of Hiring a Public Official on Winning Probability

	(7)			(8)		
	(I)	(II)	(III)	(I)	(II)	(III)
β	1.019**	0.884**	1.149*			
	(0.451)	(0.429)	(0.654)			
$\beta^{(1,2)}$				-0.0899	0.330	0.512
				(0.614)	(0.642)	(0.956)
$\beta^{(0,1)}$				-0.425	0.169	0.639
				(0.620)	(0.598)	(0.817)
$\beta^{(-1,0)}$				0.766	1.136*	1.868*
				(0.737)	(0.653)	(0.973)
$\beta^{(-2,-1)}$				0.347	0.925	1.134
				(0.839)	(0.732)	(1.082)
$\beta^{(-\infty,-2)}$				0.797	0.964	1.391
				(0.783)	(0.706)	(0.967)
γ	0.213	-0.0942	0.595			
	(0.772)	(0.769)	(1.011)			
$\gamma^{(1,2)}$				-1.074	-1.219	-0.310
				(1.208)	(1.212)	(1.944)
$\gamma^{(0,1)}$				0.175	-0.138	0.893
				(1.258)	(1.272)	(1.993)
$\gamma^{(-1,0)}$				-0.346	-0.980	0.0687
				(1.412)	(1.297)	(1.838)
$\gamma^{(-2,-1)}$				0.761	0.0494	3.104
				(1.566)	(1.441)	(2.172)
$\gamma^{(-\infty,-2)}$				0.0367	0.358	1.308
				(1.503)	(1.512)	(2.213)
Number of bidders	X	X	X	X	X	X
Month-year fixed effect	X	X	X	X	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	33,259	33,259	16,753	33,259	33,259	16,753
Within R^2	0.014	-	0.020	0.014	-	0.020

Notes: Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004. When there are multiple firms in the same auction that hire public officials between fiscal years 2001 and 2004, we either include only one firm or drop the auction entirely. In the first and fourth columns, we keep the firm whose number of participations in auctions is smallest during our sample period. In the second and fifth columns, we randomly keep one firm and run the regression. After repeating this process 100 times, we report the average coefficient and standard error as well as statistical significance according to the t-value based on them. In the third and sixth columns, we exclude all auctions in which multiple firms hire public officials between fiscal years 2001 and 2004.

Table 6: The Change in Firm Characteristic around the Thresholds

	$[T-10$ mn yen$, T+10$ mn yen$]$	$[T-20$ mn yen$, T+20$ mn yen$]$
$T \in \{100$ mn yen$\}$		
η	0.0615	0.149
	(0.171)	(0.123)
γ	0.828	2.277***
	(1.402)	(0.504)
Year fixed effect	X	X
Observations	36,074	61,564
R^2	0.004	0.004
$T \in \{200$ mn yen$\}$		
η	1.217***	1.549***
	(0.242)	(0.183)
γ	-3.014	12.43***
	(4.151)	(1.593)
Year fixed effect	X	X
Observations	12,261	20,681
R^2	0.009	0.006
$T \in \{$thresholds for participation$\}$		
η	-4.678***	-4.364***
	(0.103)	(0.0752)
γ	4.383***	4.093***
	(0.0803)	(0.0582)
Year fixed effect	X	X
Observations	32,528	57,455
R^2	0.130	0.143

Notes: Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In the first column, we consider auctions whose reserve price ranges from T - 10 million yen to T + 10 million yen. In the second column, we consider auctions whose reserve price ranges from T - 20 million yen to T + 20 million yen. In the top block, we use 100 million yen as the threshold. In the middle block, we use 200 million yen as the threshold. In the bottom block, we focus on the thresholds that determine the set of firms that are eligible to participate. In order to appropriately measure a 1-year backlog, we exclude auctions in fiscal year 2001.

Table 7: The Effect of Hiring a Public Official on Winning Probability

	$\Delta = 10$ mn yen			$\Delta = 20$ mn yen		
	(I)	(II)	(III)	(I)	(II)	(III)
$T \in \{100$ mn yen$\}$						
β	1.170**	0.894**	1.367**	1.186***	0.885**	1.378**
	(0.456)	(0.409)	(0.644)	(0.457)	(0.411)	(0.648)
β_T	-1.530***	-0.432	-1.069**	-1.194***	-0.195	-0.838*
	(0.411)	(0.485)	(0.541)	(0.372)	(0.420)	(0.434)
Number of bidders	X	X	X	X	X	X
Month-year fixed effect	X	X	X	X	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	33,259	33,259	16,753	33,259	33,259	16,753
Within R^2	0.014	-	0.020	0.014	-	0.020
$T \in \{$thresholds for participation, 200 mn yen$\}$						
β	1.008**	0.828**	1.172*	1.018**	0.833**	1.153*
	(0.448)	(0.407)	(0.634)	(0.453)	(0.411)	(0.633)
β_T	0.653	0.494	1.388*	0.352	0.280	1.209*
	(0.580)	(0.542)	(0.708)	(0.519)	(0.437)	(0.619)
Number of bidders	X	X	X	X	X	X
Month-year fixed effect	X	X	X	X	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	33,259	33,259	16,753	33,259	33,259	16,753
Within R^2	0.014	-	0.020	0.014	-	0.020

Notes: Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In the first three columns, we consider auctions whose reserve price ranges from T - 10 million yen to T million yen as auctions that are right below thresholds. In the last three columns, we consider auctions whose reserve price ranges from T - 20 million yen to T million yen as auctions that are right below thresholds. In the top block, we use 100 million yen as the threshold. In the bottom block, we focus on the thresholds that determine the set of firms that are eligible to participate as well as 200 million yen. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004. When there are multiple firms in the same auction that hire public officials between fiscal years 2001 and 2004, we either include only one firm or drop the auction entirely. In the first and fourth columns, we keep the firm whose number of participations in auctions is smallest during our sample period. In the second and fifth columns, we randomly keep one firm and run the regression. After repeating this process 100 times, we report the average coefficient and standard error as well as statistical significance according to the t-value based on them. In the third and sixth columns, we exclude all auctions in which multiple firms hire public officials between fiscal years 2001 and 2004.

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6 Online Appendix

In Table 8, we present estimation results of regressions (5) and (6) for the subset of firms that have relatively small year-to-year fluctuation in revenue. In particular, we select the set of firms whose revenue in the worst fiscal year is greater than 50% of the revenue in the best fiscal year. Although we cannot track mergers or exits of firms in our sample, conditioning on this subset of the firms may alleviate problems with aggregating revenue at the fiscal year.

The first column of Table 8 reports our estimates of regression (5). We find that the estimate of β is 0.0224 , implying that firm revenue is about 2% higher after the firm hires an official. The second column reports our estimates of regression (6). We estimate $\beta^{(0,1)}$ to be -0.0246 and $\beta^{(-1,0)}$ to be 0.0000136, suggesting that there is an increase in firm revenue one year after the firm hires an official relative to the year before. Our estimate of $\beta^{(1,2)}$ and $\beta^{(2,\infty)}$ are 0.0359 and 0.0195, suggesting that revenue remains relatively high. Unlike in column (6) of Table 4, our estimate of

Table 8: The Effect of Hiring a Public Official on Firm Revenue

	(5)	(6)
β	0.0224	
	(0.0278)	
$\beta^{(1,2)}$		0.00524
		(0.0344)
$\beta^{(0,1)}$		-0.0246
		(0.0348)
$\beta^{(-1,0)}$		0.0000136
		(0.0340)
$\beta^{(-2,-1)}$		0.0359
		(0.0412)
$\beta^{(-\infty,-2)}$		0.0195
		(0.0492)
Region-time fixed effect	X	X
Region-firm fixed effect	X	X
Observations	324	324
Within R^2	0.187	0.193

Notes: The dependent variable is the logged annual revenue per region-firm. Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004. We selected the set of firms whose revenue in the worst fiscal year is greater than 50% of the revenue in the best fiscal year.

$\beta^{(2,\infty)}$ is positive, suggesting that issues related to aggregation explain our large negative estimate of $\beta^{(2,\infty)}$ in column (6).

For reference, in Table 9, we present the estimation result of equations (1) and (2) for the set of firms that include the four ceasing entities excluded in the benchmark analysis. Overall, the estimation result of Table 9 is similar to that of the corresponding table in the benchmark analysis.

Table 9: The Effect of Hiring a Public Official on Winning Probability

	(1)			(2)		
	(I)	(II)	(III)	(I)	(II)	(III)
β	0.979**	0.837**	1.247*			
	(0.474)	(0.419)	(0.649)			
$\beta^{(1,2)}$				-0.430	-0.102	0.122
				(0.520)	(0.498)	(0.837)
$\beta^{(0,1)}$				-0.285	-0.0241	0.664
				(0.543)	(0.486)	(0.718)
$\beta^{(-1,0)}$				0.732	0.803	1.776*
				(0.749)	(0.594)	(0.946)
$\beta^{(-2,-1)}$				0.362	0.643	1.443
				(0.888)	(0.700)	(1.106)
$\beta^{(-\infty,-2)}$				0.895	0.890	1.727*
				(0.864)	(0.705)	(1.030)
Number of bidders	X	X	X	X	X	X
Month-year fixed effect	X	X	X	X	X	X
Region-firm fixed effect	X	X	X	X	X	X
Observations	33,632	33,632	16,317	33,632	33,632	16,317
Within R^2	0.015	-	0.021	0.015	-	0.021

Notes: Standard errors are in parentheses and are clustered by region-firms. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In all columns, the sample excludes firms that did not hire any public official between fiscal years 2001 and 2004. When there are multiple firms in the same auction that hire public officials between fiscal years 2001 and 2004, we either include only one firm or drop the auction entirely. In the first and fourth columns, we keep the firm whose number of participations in auctions is smallest during our sample period. In the second and fifth columns, we randomly keep one firm and run the regression. After repeating this process 100 times, we report the average coefficient and standard error as well as statistical significance according to the t-value based on them. In the third and sixth columns, we exclude all auctions in which multiple firms hire public officials between fiscal years 2001 and 2004.