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Empirical Methods for Detecting Bid-rigging Cartels

Thèse de Doctorat

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Résumé

Organisée en cinq chapitres, cette thèse de doctorat vise à étudier et à développer des méthodes empiriques pour détecter les cartels de soumission. Elle en propose également une étude économique et étudie leur fonctionnement notamment en se fondant sur l'expérience professionnelle de l'auteur. Chaque chapitre correspond à un article indépendant s'intégrant de façon cohérente à la thèse de doctorat.

Le chapitre 1 détaille le cas du Tessin. Toutes les entreprises actives dans le domaine de la construction des routes ont formé un cartel au Tessin de janvier 1999 à avril 2005. Le cartel du Tessin utilisait un mécanisme d'attribution des contrats entre ses participants fondé sur les coûts de chaque entreprise. Ce type de mécanisme a permis au cartel du Tessin d'extraire la rente cartellaire maximale et démontre le degré élevé d'organisation du cartel. Durant la période du cartel, les entreprises ont truqué l'ensemble des contrats. Grâce à la qualité des données encore inexploitées du cas du Tessin et parce que nous pouvons discriminer de manière parfaite entre les périodes du cartel et post-cartel, le cas du Tessin est idéal pour évaluer la performance des méthodes de détection de cartels de soumission.

Le chapitre 2 applique les tests économétriques proposés dans le papier fondateur de *Bajari and Ye* (2003) au cas du Tessin et montre que les tests économétriques produisent beaucoup trop de faux négatifs pour détecter le cartel du Tessin. En d'autres termes, les tests économétriques de *Bajari and Ye* (2003) auraient dû détecter l'ensemble du cartel de soumission mais n'ont détecté qu'une part mineure des entreprises impliquées. Ces résultats questionnent la pertinence de la méthode de détection proposée par *Bajari and Ye* (2003) et suggèrent de se tourner vers une autre méthode de détection.

Le chapitre 3 répond à ce besoin et présente une nouvelle méthode de détection construite à partir de simples indicateurs statistiques appelés *screens*. Ce chapitre montre que ces simples indicateurs statistiques saisissent avec succès la déformation de la distribution des offres due au cartel de soumission du Tessin.

Le chapitre 4 montre comment les indicateurs statistiques simples présentés dans le chapitre

3 peuvent être améliorés et combinés pour développer une méthode de détection capable de tenir compte du problème de collusion partielle, c'est à dire quand les entreprises truquent seulement une partie des contrats et non l'ensemble des contrats comme dans le cas de Tessin. Le chapitre 4 démontre également que les indicateurs statistiques simples remplissent les conditions nécessaires pour établir un soupçon initial suffisant permettant l'ouverture d'une enquête.

Finalement, le chapitre 5 montre en utilisant des techniques issues du machine learning que le taux de prédiction de la méthode présenté dans le chapitre 3 est très élevé. Le chapitre 5 utilise des données provenant de quatre cartels de soumission différents (parmi lesquels le cas du Tessin) et démontre par conséquent que la méthode de détection fondée sur des indicateurs simples est d'application générale et non spécifique à un cas.

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Introduction

The fight against bid rigging has become one of the priorities of competition authorities around the world, since bid-rigging cases represent a significant share of cartel enforcement in many countries (see *OECD*, 2016, page 6). A large share of economic activities is realized through auction mechanisms, especially in the public sector, and may be affected by bid rigging. OECD estimate that the elimination of bid rigging could help reduce procurement prices by 20% or more.[1] Since public procurement represents approximately 13% of gross domestic product in OECD Members and 29% of general government expenditure, the potential damage of bid rigging can be enormous (see *OECD*, 2016, page 6). If it is difficult to report a precise estimation on both the scale of bid-rigging activities and the increase in prices caused by bid rigging, it is, however, certain that bid rigging directly harms taxpayers. In a context of tight budgetary restrictions since the financial crisis, it has never been so important for the public sector to ensure competition in public procurement (see *OECD*, 2016, page 6).

In Switzerland, bid rigging is also a pervasive issue. The Swiss Competition Commission (hereafter: COMCO) rendered seven decisions against bid-rigging cartels in 2017 and a major decision in 2018.[2] In that last decision, COMCO specified that cartel participants rigged more than 400 contracts in the construction sector, for a value exceeding 100 million CHF. In her communication, COMCO stated that bid-rigging cartels cause prices to increase, maintain inefficient market structure, lower the quality of products and services, and reduce incentives to innovate.[3] During the last decade, COMCO regularly rendered decision against bid-rigging cartels.[4] If bid rigging mainly concerns the construction sector, it also affects other sectors like tunnel cleaning or the installation of electric

[1]See OECD internet page: http://www.oecd.org/competition/cartels/fightingbidrigginginpublicprocurement.htm.

[2]See the following internet pages: https://www.weko.admin.ch/weko/fr/home/actualites/communiques-de-presse/nsb-news.msg-id-69339.html, https://www.weko.admin.ch/weko/fr/home/actualites/communiques-de-presse/nsb-news.msg-id-70566.html and https://www.newsd.admin.ch/newsd/message/attachments/52182.pdf.

[3]See section II at the following internet webpage: https://www.newsd.admin.ch/newsd/message/attachments/52182.pdf.

[4]See *Strassenbeläge Tessin* (LPC 2008/1, pp. 85-112), *Elektroinstallationsbetriebe Bern* (LPC 2009/2, pp. 196-222), *Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Aargau* (LPC 2012/2, pp. 270-425), *Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich* (LPC 2013/4, pp. 524-652), *Tunnelreinigung* (LPC 2015/2, pp. 421-60), *Hoch- und Tiefbauleistungen Münstertal* (LPC 2017/3, pp. 193-245) and *Bauleistungen See-Gaster* (available at the following internet webpage: https://www.weko.admin.ch/weko/fr/home/actualites/dernieres-decisions.html).

systems[5].

In order to launch an investigation, COMCO must have a sufficient suspicion, which must be coherent and objective, but should not constitute a proof on itself.[6] The sufficient suspicion must credibly substantiate the existence of a potential bid-rigging cartel. In other words, it must raise substantial doubt regarding the presence of bid rigging.

COMCO mainly relies on whistle-blowers, customer complaints and leniency programs to open an investigation. Specific to bid-rigging cases, whistle-blowers or complaints from procurement bodies play a major role in the opening of investigations. If leniency programs are certainly an effective tool providing crucial information to prosecute bid-rigging cartels, many are however applied after the opening of an investigation.

In order to mitigate its dependency on those sources of information, COMCO decided to initiate a pilot project in order to construct a method for detecting bid-rigging cartels. The detection method should be simple to apply with a modest data requirement, easy to understand in court and reliable for providing a sufficient suspicion.

The author of this PhD thesis crucially contributed to the pilot project of COMCO by developing a detection method based on simple screens. By applying it, he successfully detected one bid-rigging cartel.[7] Based on the results produced by the detection method, COMCO opened an investigation in 2013, and sanctioned the involved firms in 2016 as court of first instance.[8]

The success of COMCO raised a special international interest. Several competition agencies contacted COMCO for the detection method developed and applied by COMCO.[9] COMCO also held several presentations on the detection method based on simple screens. The last presentation was in January 2018 in Paris for a workshop organized by the OECD on cartel detection.[10]

The regular implementation of a detection method based on simple screens certainly has a strong potential deterrent effect. The probability to be detected destabilizes bid-rigging cartels and makes them harder and less profitable to organize. Moreover, if trying to "beat" the detection method is still possible, it will increase the coordination costs among cartel participants. Moreover, once the competition agency knows how firms coordinate their bids to beat the screens, it can still quickly adapt and refine the implemented detection method. Finally, regular screening activities could also

[5] See *Elektroinstallationsbetriebe Bern* (LPC 2009/2, pp. 196-222) and *Tunnelreinigung* (LPC 2015/2, pp. 421-60).

[6] A majority of the jurisdictions in EU also uses a similar concept for the "initial suspicion", called "reasonable grounds", "reasonable suspicion" or "founded suspicion". See the investigative powers report available at the following internet page: http://ec.europa.eu/competition/ecn/documents.html.

[7] See Chapter 4 of this PhD thesis.

[8] see the following internet page: https://www.weko.admin.ch/weko/fr/home/actualites/communiques-de-presse/nsb-news.msg-id-64011.html.

[9] Among others: Brazil, Canada, Germany, Japan, Netherlands, Norway, Portugal, Sweden.

[10] See the following OECD's internet page : http://www.oecd.org/competition/workshop-on-cartel-screening-in-the-digital-era.htm.

foster application to leniency programs.

Organized in five chapters, this PhD thesis investigates empirical methods for detecting bid-rigging cartels. It also proposes an economic analysis of bid rigging and a study of the functioning of bid-rigging cartels based on the experience of the author. Each chapter of the thesis corresponds to an independent paper. However, all the papers are integrated in a coherent way. Chapter 1 presents the Ticino bid-rigging cartel in details. Since we can perfectly discriminate between the cartel and post-cartel periods, the Ticino case is ideal to evaluate the performance of any detection method. Moreover, the data of the Ticino case were previously unexploited. Chapter 2 applies the econometric tests proposed in the seminal paper of *Bajari and Ye* (2003) to the Ticino case and shows that they produce too many false negative results for detecting bid rigging cartels. Such results question the relevance of the econometric tests proposed by *Bajari and Ye* (2003) and suggest the necessity of another detection method. Chapter 3 answers that need and presents a new detection method based on simple screens. These simple statistical screens can successfully capture the impact of bid rigging in the distribution of bids in the cartel period of the Ticino case. Chapter 4 shows how the simple screens used in chapter 3 can be refined and combined to develop a detection method able to consider the problem of partial collusion, i.e., when firms collude not on all contracts as for the Ticino case, but on selected contracts. Chapter 4 also proves that the simple screens can fulfil the requirement of sufficient suspicion in order to open an investigation. Chapter 5 goes a step further and shows that the prediction rate of a detection method based on simple screens is high by using machine learning techniques. In chapter 5 we use data of four different bid-rigging cases (among them the Ticino case), and therefore we are able to show that the simple screens are widely useful and not case-specific.

In the following, we propose a non-technical summary of each chapter.

Chapter 1 describes the Ticino bid-rigging cartel and analyzes its internal functioning. The infringements of the Ticino cartel were serious and severe: all firms for road construction and related engineer services in Ticino participated in the cartel and rigged all contracts from 1999 to April 2005, when the cartel was discovered. Unlike most bid-rigging cases, the unlawful agreement of the Ticino cartel was a written document, called the convention, stipulating the rules of the cartel. Therefore, the organization of the cartel was meticulous and accurate. Firms organized weekly meetings and they discussed all public contracts and all private contracts above 20’000 CHF. The contract allocation process consisted of two steps. First, the cartel designated the firm that should win the contract according to criteria listed in the convention. Second, they discussed and fixed the price of the winning bids and of the cover bids.

The Ticino cartel functioned without monetary transfers and was therefore a weak cartel to use the words of *McAfee and McMillan* (1992). A weak cartel cannot achieve the first-best collusive gain unless it operates with a Ranking Mechanism, as suggested by *Pesendorfer* (2000). The Ranking Mechanism purposes to determine the lowest-cost bidder to win the contract in order to achieve the first-best collusive gain. Chapter 1 shows that the cartel convention plays the role of such a Ranking Mechanism. The convention clearly defined the criteria to allocate contracts among cartel participants: the distance to contract location, the capacity of firms engaged in current contracts and the specialisation of each firm. In other words, the criteria enumerated in the convention are the most used cost variables in the literature (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b), and the convention sought to allocate the contract to the lowest-cost bidder in a tender.

Estimating logit models, we find that the variables of costs explain the probability to submit the lowest bid in a tender. Though "weak", the Ticino cartel successfully applied its convention and achieved the first-best collusive gain by using a bid rotation scheme based on costs. To verify the robustness of the results, we estimate the logit models including not only cost variables but also contract allocation variables, which a cartel might use in a simple bid rotation scheme, as suggested by *Ishii* (2009). We show that the contract allocation variables do not explain the probability to submit the lowest bid in a tender, and we can therefore exclude a simple bid rotation mechanism. We also verify the robustness of the results by splitting the sample in two subsamples. The first subsample solely contains the individual bids whereas the second regroups all bids submitted in consortium. We find the same results for both subsamples. To sum up, the cost variables explain the probability to submit the lowest bid in a tender in all estimations.

Chapter 1 also examines the connection between the distribution of the bids and the distribution of the costs by estimating ordered logit models. We assume that higher bids, respectively higher ranks, imply higher costs, and we find that the distribution of the ranks for the bids in a tender matches the distribution of the costs. Therefore, the Ticino cartel did not only select the lowest-cost bidder to win the contract, but also attributed the cover bids submitted by the other cartel participants, which were depending on their costs: A firm with higher costs submitted a higher bid than a firm with lower costs, who submitted a lower bid. Again, we check the robustness of the results by adding variables for contract allocation following *Ishii* (2009). The results show that the cost variables explain the distribution of the bids and that the contract allocation variables refute a logic of a simple bid rotation mechanism. We conclude that the cartel carefully manipulated the winning bids as well as the cover bids in each tender by implementing its convention.

To sum up, chapter 1 shows that the lowest-cost bidder submitted the lowest bid in a tender allowing the cartel to achieve the first-best collusive gain. Moreover, the manipulation of the cover bids was based on the costs of each firms.

Chapter 2 applies the econometric tests proposed by *Bajari and Ye* (2003) to the Ticino cartel. First, the conditional independence test checks if bids are independent between firms, conditional on some observable covariates. Chapter 2 shows that 89% of the pairs of firms do not fail the conditional independence test at 5% risk level in the cartel period. The tests for the conditional independence therefore produce too many false negative results since almost 9 pairs of firms out of 10 are not classified as bid-rigging cartels, although we implement the tests in the cartel period. Since the Ticino cartel was a complete cartel, we should have found in the cartel period rejections for all pairs of firms. In other words, bids are independent in the cartel period since the residues are for the most pairs of firms uncorrelated conditional on covariates, highlighting rather a competitive behavior of the firms.

Second, the test for the exchangeability of the bids examines if firms react in the same way considering their own costs. To put it differently, if we permute the costs of firm i with the costs of firm j, then firm i should submit the same bids as firm j. In our case, we find again too many false negative results for the cartel period, since 68% of the pairs pass the test at 5% risk level. Therefore, almost 7 pairs of firms out of 10 react in the same way, when their own costs are exchanged, fitting the competitive hypothesis of the test for the exchangeability of bids in the cartel period.

Finally, we check the robustness of these false negative results by implementing both tests on two subsamples. Cover bids are fake by definition and therefore less connected to the cost variables. Therefore, we expect to find more rejection if we implement the tests only on cover bids. Since the model used by *Bajari and Ye* (2003) assumes that bids are independent conditional on some covariates, bids in a tender are not conditional on being winning bids or cover bids. We can therefore exclude a possible sample selection bias for implementing the tests in two different sub-samples.

For the first subsample, we exclude all pairwise observations with a winning bid and we perform again both econometric tests on the pairwise observations solely formed with cover bids. We find again that both tests produce too many false negative results. For the second subsample, we consider all pairwise observations excluded in the first subsample so that the addition of the two subsample gives the whole sample. In that second subsample, we have for each pairwise observation one winning bid and one cover bid. We implement only the conditional independence test, as we have less observations for the second subsample. In contrast to all previous tests, we find a higher number of rejection, since 69% of the pairs fail the test. This result may suggest that the test of the conditional independence is appropriate to detect bilateral agreements, i. e., incomplete cartels.

To conclude, chapter 2 illustrates the limits of the econometric tests proposed by *Bajari and Ye* (2003). They produce too many negative results. Therefore, there is a necessity for a better performing method that is able to detect the Ticino cartel. We develop such a method t in the following chapter.

Chapter 3 presents an inductive method based on simple screens for detecting bid-rigging cartels. If the statistics used in the calculation are simple, their application is original. COMCO currently uses the method based on simple screens presented in chapter 3 and 4. Moreover, the method has also been discussed at the OECD and several competition agencies has shown their interest in applying it.

Cartel participants raise their bids to increase their profit. However, they cannot exaggerate them without raising flagrant suspicions, since the procurement body may approximate the costs for a contract and therefore the underlying distribution of the bids. Such bid coordination reduces the support of the distribution of the bids affecting its variance. We find that the coefficient of variation and the kurtosis statistic both reflect well the reduction of the distribution of the bids in the cartel period.

Moreover, the difference between the first and the second lowest bids is important in procurement when the price is essential but is not the only criterion in awarding contracts. Cartel participants maintain a specific difference between the first and the second lowest bid to ensure that the contract is awarded to the firm designated by the cartel. In addition, the differences between losing bids become smaller, as firms do not want to appear too expensive. With such a cover-bidding mechanism, the coordination of bids produces an asymmetry in the discrete distribution of the bids. We find that the difference in percent between the first and the second lowest bids, the skewness statistic, and the relative distance capture well such asymmetry in the distribution of the bids.

By applying the screens to the Ticino cartel, four periods emerged from our data: the pre-cartel period (1995 to 1997), the year 1998, the cartel period (1999 to March 2005) and the post-cartel period (April 2005 to 2006). By regressing dummy variables for each period on each screen, we find that the effect of each periods is significant and conditional on control variables. Moreover, we estimate the effect of bid rigging on each screen so that we can derive thresholds or benchmarks for future cases. As the Ticino cartel is one of the severest cartels known in Switzerland, we suggest considering the estimated effects of bid rigging as conservative thresholds or benchmarks.

Finally, repeated bid coordination may produce a specific bidding pattern because of cover bids and the possible rotational element due to contract allocation within the cartel. The bid rotation screen proposed by *Imhof et al.* (2017) can detect such a specific colluding pattern. Unlike the previous screens, this screen does not characterize the discrete distribution of the bids in a tender but focuses on the interaction of one firm with another or the interaction of one firm within a group of firms. We find in the Ticino case that repeated coordination of bids within the cartel participants strongly affects the distribution of the bids. Furthermore, the depicted interactions between firms suggest that the bid-rigging cartel operates in a rotation pattern through contract allocation. When contrasted with the cartel period, our results clearly indicate a radical change for the post-cartel period, and the behavior of firms fits the hypothesis of competition predicted by the screen.

To sum up, simple screens capture well the impact of bid rigging in the distribution of the bids for the Ticino case. In the following chapters, we show that this result applies to other bid-rigging cases.

Chapter 4 resumes the major results of the pilot project in which the author plays a crucial role in developing the detection method currently used by COMCO.[11] Based on the joint paper "Screening for bid rigging: does it work?" with Yavuz Karagök and Samuel Rutz, the author implements two simple screens, namely the coefficient of variation and the relative distance, presented in chapter 3. The dataset contains 282 contracts tendered by the canton of St. Gall in the construction sector. For all these contracts, no information about potential bid-rigging cartels was available.

We find that neither screen produces unambiguous evidence as to whether bid rigging is likely to exist in the whole sample. A possible reason for this result is that the statistical methods suggested in the literature are not particularly well suited to detect partial collusion, i.e., collusion that does not involve all firms and/or all contracts in a dataset. Therefore, we design an approach that allows testing for partial collusion. In general, our approach amounts to a collection of mutually reinforcing tests to identify potential collusion between subsets of firms. In particular, we show how benchmarks derived from past investigations and the combination of (uncorrelated) screens may be used to identify subsets of conspicuous contracts and firms. To substantiate and validate suspicions of collusive behavior, we further discuss a collection of mutually reinforcing tests providing conclusions as to whether a bid-rigging cartel is likely to exist.

With the help of these tests, chapter 4 shows how it is possible to isolate a group of "suspicious" firms in our sample that exhibit the characteristics of a local bid-rigging cartel, operating with cover bids and a more or less pronounced bid rotation scheme. Based on these results, COMCO opened an investigation in 2013.[12] The resulting house searches produced proof of collusion and led to a

[11]Chapter 4 is based on the paper "Screening for bid rigging: does it work?" in collaboration with Yavuz Karagök and Samuel Rutz, forthcoming in the Journal of Competition Law and Economics.

[12]See press release on 4 October 2016 on COMCO's website: https://www.weko.admin.ch/weko/de/home/aktuell/medieninformatione news.msg-id-64011.html. COMCO's decision is, however, currently pending before the appeals court.

conviction and sanctioning of the involved firms in 2016.

To sum up, chapter 4 shows that a detection method based on simple screens can produce reliable results to build a sufficient suspicion for the opening of an investigation. Moreover, simple screens can deal with more complex issue as partial collusion.

Chapter 5 combines machine learning techniques with the simple screens presented in chapter 3 for predicting bid rigging.[13] In the working paper "Machine learning with screens for detecting bid-rigging cartels" with Martin Huber, we use an original dataset of 483 tenders, representative for the construction sector in Switzerland. The data cover four different bid-rigging cartels and we can therefore construct a binary collusion indicator that serves as dependent variable for the collusive and competitive (post-collusion) tenders. More concisely, chapter 5 investigates the performance of the simple screens as predictors using two techniques of machine learning, namely the lasso and the ensemble method.

The results of chapter 5 suggest that the combination of machine learning and screening is a powerful tool for detecting bid rigging. Lasso logit regression correctly predicts out of sample 82% of all tenders. This result indicates that 4 out of 5 tenders are correctly classified. It contrasts with the results of the tests for the conditional independence proposed by *Bajari and Ye* (2003), which correctly classify solely 1 pair of firms out of 10 as bid-rigging cartels. However, the rate differs across cartel and non-cartel cases. While lasso correctly classifies 91% of the collusive tenders (i.e. 9% are false negatives), it correctly classifies 69% of the competitive tenders (31% false positives classified as collusive in the absence of bid rigging). Thus, false positives rates are more than three times higher than false negatives. To reduce the share of false positives (which generally comes with an increase of false negatives), we consider tightening the classification rule, by only classifying a bid as collusive if the predicted collusion probability is larger than or equal to 0.7 (rather than 0.5). In this case, lasso correctly classifies 77% of collusive tenders (23% false negatives) and 85% of competitive tenders (15% false positives). By gauging the choice of the probability threshold, a competition agency may find an optimal tradeoff between false positives and false negatives. The ensemble method also confirms the high prediction rates produced by the lasso.

As lasso is a variable selection method for picking important predictors, it allows determining the most powerful screens. In chapter 5, we find that two screens play a major role for detecting bid-rigging cartels, namely the ratio of the price difference between the second and (winning) first lowest bids to the average price difference among all losing bids and the coefficient of variation of bids in a tender.

[13] Chapter 5 is based on the working paper "Machine learning with screens for detecting bid-rigging cartels" in collaboration with Martin Huber, currently under revision at the International Journal of Industrial Organization.

Chapter 1

The Intern Functioning of the Ticino Bid-Rigging Cartel

1.1 Introduction

From January 1999 to April 2005, all firms in the road construction sector participated to a bid-rigging cartel and rigged all contracts in Canton Ticino. Prices increased by 25-30% and the cartel rent approximatively amounted to 135-162 million Swiss Francs. Such damages are enormous and directly harming taxpayers. Since the investigation on the Ticino cartel has provided numerous documents, we purpose to study the intern functioning of the Ticino cartel and to confront the results with theoretical findings regarding bid rigging and auction.

Unlike most bid-rigging cases, the unlawful agreements concluded by all firms in Ticino was a written document, called the convention, stipulating all rules for the cartel to function. The infringement was serious and severe. For more than six years, all firms in Ticino in the road construction sector participated to the cartel and rigged all contracts. Moreover, the organization of the cartel was meticulous and accurate. Firms organized weekly meetings and they discussed all public contracts and all private contracts above 20'000 CHF. The contract allocation process consisted in two steps. First, the cartel designated the firms who should win the contracts according to criteria listed in the convention. Second, they discussed and fixed together the price of the designated winner by the cartel as the cover bids.

To achieve the first-best collusive gain, a bid-rigging cartel must meet three conditions in procurement (see *Hendricks et al.*, 2015). First, the cartel participants must reveal their true costs. Second, the cartel participant with the lowest costs must win the contract. Third, the bid from the cartel must not be above the reserve price. In our case, the condition of the reserve price is irrelevant since the canton Ticino did not set any reserve prices in the procurement procedure. However, even if there was no reserve price announced by the procurement agency, we assume the existence of an implicit reserve price since firms could not exaggerate the submitted bids, otherwise exaggerated bids would alarm the procurement agency for potential bid-rigging conspiracies. In brief, the firm with the lowest costs from the cartel should win the contract at the highest possible price in order to maximize the cartel payoff that is the difference between the bids and the costs.

A bid-rigging cartel functioning with monetary transfer achieves the first-best collusive gain, as shown by the seminal paper of *McAfee and McMillan* (1992). Such cartels are called ”strong” cartels contrasting with ”weak” cartels, which do not function with monetary transfer. According to *McAfee and McMillan* (1992), a ”weak” cartel can only submit the same bid and let the procurement agency chooses one winner. Such randomized contract allocation is not efficient and does not meet the conditions to achieve the first-best collusive gain.

McAfee and McMillan (1992) establish their results in a one-shot auction game, where monetary transfer is the unique enforcement device for the cartel stability. However, in repeated auctions punishment from deviation is possible and can therefore sustain the existence of a bid-rigging cartel even if the cartel is ”weak”. Some papers show that ”weak” bid-rigging cartels in repeated auctions without communication can be sustained, yet they cannot achieve the first-best collusive gain (see *Skrzypacz and Hopenhayn*, 2004; *Athey et al.*, 2004). When communication between firms in repeated auctions is possible, the cartel can achieve the first-best collusive gain, if firms are sufficient patient (see *Athey and Bagwell*, 2001) or if there is a centre, who constrain the firms to reveal their true valuations (see *Aoyagi*, 2007). In addition, *Pesendorfer* (2000) shows that a ”weak” bid-rigging cartel can be equally efficient as a ”strong” cartel if it meets two conditions: First, it must have many contracts to allocate between its members. Second, a Ranking Mechanism should organize the ”weak” bid-rigging cartel and select the firm with the lowest costs in procurement to win the contract. The first condition implies a context of repeated auction, whereas the second condition indicates that firms truly reveal their valuation.

To sum up, ”weak” bid-rigging cartels function with contract allocation and the cartel participants use in some way their future market share as a sort of indirect monetary transfer committing them to an intertemporal agreement. In the paper, we differentiate between two types of contract allocation: a simple bid rotation scheme (see *McAfee and McMillan*, 1992; *Skrzypacz and Hopenhayn*, 2004; *Athey et al.*, 2004) contrasting with a bid rotation scheme based on costs (see *Athey and Bagwell*, 2001; *Pesendorfer*, 2000; *Aoyagi*, 2007). Solely a bid rotation scheme based on costs allows the cartel to achieve the first-best collusive gain by designating the lowest-cost bidder to win the contract at the highest possible price.

The Ticino cartel functioned without monetary transfers and was therefore a ”weak” cartel. In the paper, we examine whether the Ticino cartel used a simple bid rotation scheme or one based on the costs. The question is not trivial because no study of cartels has shown so far that a ”weak” bid-rigging cartel can achieve the first-best collusive gain. *Asker* (2010) studies the intern organization of a bid-rigging cartel in a context of English auction (oral ascendant auction), but the cartel functioned with monetary transfers and was therefore a ”strong” cartel. *Ishii* (2009) investigates a bid-rigging cartel for road construction and maintenance in Osaka. The cartel operated without monetary transfer and was ”weak” as the Ticino cartel. However, *Ishii* (2009) finds that the number of days without winning a contract explains the best the probability to win a contract. The ”weak” cartel investigated by *Ishii* (2009) did not use a bid rotation scheme based on the costs of each bidder, but a simple bid rotation mechanism and could therefore not achieve the first-best collusive gain.

The criteria listed in the convention of the Ticino cartel operates like a Ranking Mechanism, as suggested by *Pesendorfer* (2000). It defined clearly the criteria to allocate contracts among cartel participants: the distance to contract location, the capacity of firms engaged in current contracts and the specialisation of each firm. In other words, the criteria enumerated in the convention are the most used cost variables in the literature (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b), and the convention sought to allocate the contract to the lowest-cost bidder in a tender. The paper investigates if the Ticino cartel successfully applied its convention, respectively if the cartel designated the lowest-cost firm to win the contract. If we proof that the Ticino cartel applied the convention and its criteria to allocate contract based on the costs, then we show that the Ticino cartel used a bid rotation mechanism based on costs and could extract the maximum payoff though the absence of direct monetary transfer.

Estimating logit models, we find that the variables of costs explain the probability to submit the lowest bid in a tender. To verify the robustness of this result, we estimate the logit models including not only the cost variables but also variables for contract allocation, which a cartel might use in a simple bid rotation scheme. *Ishii* (2009) proposed different sorts of variables for contract allocation used in a simple bid rotation scheme. We show that these variables of contract allocation cannot explain the probability to submit the lowest bid in a tender. Therefore, we can exclude a simple bid rotation mechanism. We also verify the robustness of the results by splitting the sample in two subsamples. The first subsample solely contains the individual bids whereas the second regroups all bids submitted in consortium. We find the same results for both subsamples. To sum up, the cost variables explain the probability to submit the lowest bid in a tender in all estimations. This result ultimately validates that the Ticino cartel, though "weak", used a bid rotation scheme based on costs and achieved the first-best collusive gain.

In a second step, we examine the connection between the distribution of the bids and the distribution of the costs by estimating ordered logit models. We assume that higher bids, respectively higher ranks, imply higher costs, and we find that the distribution of the ranks for the bids in a tender matches the distribution of the costs. Therefore, the Ticino cartel did not only select the lowest-cost bidder to win the contract, but also attributed the cover bids a rank according to their costs: A firm with higher costs should submit a higher bid than a firm with lower costs, who should submit a lower bid. Again, we check also the robustness of the results by adding the variables of contract allocation following *Ishii* (2009). The results show that the cost variables explain the distribution of the ranks and that the variables of contract allocation refute a logic of a simple bid rotation mechanism. We conclude that the cartel carefully manipulated the winning bids as well as the cover bids in each tender by implementing its convention.

The next section discusses the Ticino case. In section 1.3, we present the data and the empirical strategy. Section 1.4 presents the results from the logit and the ordered logit model. Section 1.5 draw some policy recommendations and concludes the paper.

1.2 The Ticino Case

All firms, active in the road construction sector, participated to a bid-rigging cartel in the Canton of Ticino, and they rigged all contracts from January 1999 to April 2005 without exception.[1] The firms concluded a written agreement called the convention to arrange the organisation of the bid-rigging cartel. By the means of weekly meetings, they discussed all public contracts and all private contracts above 20'000 CHF in details, and they allocated contracts among the cartel participants according to the criteria listed in the convention. Once, decided on the allocation of the contracts, cartel participants fixed together the prices of the winning bids and of the cover bids.

In the following, we present first the market structure, then the procurement process and data. Second, we discuss in details the allocation mechanism of contracts and the related criteria listed in the convention. Finally, we examine the price increase and the damages caused by the bid-rigging cartel.

[1] One firm did not participate to the bid-rigging cartel. However, since we do not have any bid from firm 13 in our data, we assume that it was not active in the road construction sector.

1.2.1 Market structure

COMCO defined two relevant markets in its investigation: a market for road construction and pavement in Canton Ticino and an upstream market for asphalt pavement materials. In total, 17 firms were active in the road construction sector and two firms (firm 1 and 2) were mixing plants, solely active on the upstream market for asphalt pavement materials. Asphalt pavement materials are a crucial input for covering and pavement works. It has to be heated at a mixing plant in order to be mixed and quickly transported to the contract location to cover the road before getting cold. Market specialists say that the duration of heated asphalt once mixed is comprised between one hour and one hour and half; firms can therefore operate in a radius of 50-80 km from the production mixing plant. Because of its importance for the road construction sector and the necessity to transport it heated, asphalt pavement materials typically are a strategic input.

Regarding the firms active in the road construction sector, four firms vertically integrated the production process by owning an asphalt mixing plant (see firms 3, 4, 5 and 6 in table 1.5). However, because the infrastructure for an asphalt mixing plant is important and expensive, firms joined their effort in vertical integration by commonly owning an asphalt production plant. In our case, 10 firms owned the biggest asphalt production plant (firm 1), which had a market share of 50%-60% in the market for asphalt pavement materials in Ticino. Firm 3 and firm 15 commonly owned the second biggest asphalt plant (firm 2), which had a capacity of 10%-20% in the market for asphalt pavement materials in Ticino. Furthermore, firm 1 was also shareholder in firm 2. To sum up, 12 road construction firms jointly owned two asphalt production plants with a market share of 60%-80% in the market for asphalt pavement materials in Ticino.

The convention forbade access to asphalt materials or other strategic inputs to third firms not involved in the convention.[2] The clause thus foreclosed the road construction market and put serious entry barriers for new competitors. Any new entrant should have built its own mixing plant, which was a prohibitive investment. Any cartel participant deviating from the convention could be also harshly punished. Such disciplinary effects from the mixing plants were real and enormous on the cartel participants. Defecting to the cartel, respectively not taking part to the convention could have resulted in exclusion from the road construction market.

If such entry barriers undoubtedly favour the stability of the bid-rigging cartel, other factors also facilitate the existence of the Ticino cartel. OECD lists the factors or the structural screens identified by the literature making markets more prone to collusion (see *OECD*, 2014). The table 1.1 reviews some market characteristics for the Ticino case, with (+) if it indicates that the factor facilitates

[2] See the convention of the Ticino bid-rigging cartel in decision *Strassenbeläge Tessin* (LPC 2008/1, pp. 78-80), Art. 6 Obligation.

collusion and with (-) if it indicates that the factor enhances competition in our case.

Market concentration favour collusion. However, we observe no market concentration in our case, but rather a high number of competitors, as we report 17 regular firms, active in the road construction sector. Such high number of competitors should render collusion less likely (see *Bain*, 1956; *Tirole*, 1988). Likewise, table 1.3 shows that the sum of contracts won varies from 8 to 26 million CHF. Thus, firms had asymmetric production capacities, which should destabilize collusion, since firms with large capacities have an incentive to undercut rivals with low capacities for retaliation (see *Brock and Scheinkman*, 1985; *Compte et al.*, 2003).

However, many characteristics favour collusion in the Ticino case. First, the same firms frequently participated to procurement because of the numerous contracts tendered per year (see table 1.3). Moreover, the tender process was also transparent because the records of the submitted bids were publicly available, at least for the firms. The literature has shown that high degree of interaction between competitors and market transparency enhance the likelihood of collusion (see *Stigler*, 1964; *Green and Porter*, 1984; *Bernheim and Whinston*, 1990; *Hendricks et al.*, 2015).

Other characteristics related to the supply side increase the likelihood of collusion. Poor Innovation if none characterized the road construction market in Ticino and favours collusion. Future potential innovations on a market reduce the profitability of collusion as the degree of retaliation (see *Ivaldi et al.*, 2003). Firms in road construction segment in Ticino exhibited similar cost structure, which facilitate collusion, since dissimilar costs make more difficult to reach an agreement (see *Rey*, 2006). Moreover, the upstream market for asphalt materials clearly showed cross-shareholding relationship. Such structural links between competitors help sustaining collusion (see *Gilo et al.*, 2006).

The public sector mainly organized procurement for road construction contracts, and the demand of the public sector for road construction in Ticino was rather inelastic. Moreover, demand fluctuation was low and predictable. Such possibility to identify future contracts to be procured by the state secures and stabilizes agreements among cartel participants. To sum up, the environment of the Ticino cartel was certainly a fertile ground for collusion.

1.2.2 The procurement process and data

Procurement agencies in Ticino announced a fixed date to submit bids for a specific contract, and provided all the relevant bidding documents for the tender process. Firms, interested by the contract, calculated and submitted their bids before the date fixed by the procurement agencies. After that date, the call for bids was closed and procurement agencies proceeded to the opening of the

Table 1.1: Structural screens

Market Characteristics	Collusive Assessment for the Ticino Case
Market Concentration	(-)
Entry barriers	(+)
High frequency of interaction between competitors	(+)
Market Transparency	(+)
Mature Industry with little innovation	(+)
Similar cost Structure	(+)
Asymmetric Distribution of Production Capacities	(-)
Homogenous Product	(+)
Cross-shareholdings	(+)
Demand Fluctuation	(+)
Elasticity of the Demand	(+)

submitted bids and recorded them on a bid summary. More precisely, they established an official record of the bid opening. On the bid summaries, we find information pertaining to the bids, the identity of the bidders, the location and the type of the contract.

Once the record of the bids over, procurement agencies began to examine the bids in details. Different criteria played a role in deciding which bid to select, as the references of the firms, the organization and the timing of the work, social and environmental aspects of the bids. However, the price was generally the essential criterion, and even if procurement agencies did not neglect to examine other criteria, they awarded contracts to the lowest bid in a tender. During the cartel period, all winning bids were the lowest bids in tender. To sum up, the procurement process in Ticino followed the mechanism of a first-price sealed-bid auction.

The database contains 334 tenders from 1995 to April 2006 (see table 1.2). However, we have the bid summaries only for 238 contracts, that is information on 1381 submitted bids about the identity of bidders, the price of each bids and the location of the contracts. Less information is available for 96 tenders for the years 1995 to 1998.

Table 1.2: General descriptive statistics

Number of tenders	334
Number of submitted bids	2179
Number of tenders with details	238
Number of submitted bids	1381
Number of submitted bids from individual firm	1100
Number of bids from consortia	281
Number of winning bids from individual firms	148
Number of winning bids from consortiums	90

Table 1.3 recapitulates the amounts of contracts in CHF tendered per year. For the years 1995,

1996 and 2006, the data does not contain all contracts publicly tendered. However, we have the most of the contracts for the years 1997 to 2005 and we observe variation in the sum of contracts publicly tendered per year, especially between the years 1997 to 2001. There is a maximal difference of 23 million CHF between the years 1998 and 1999 representing 45% of the maximal amount tendered per year. In fact, the canton of Ticino tendered every two years six major contracts for road maintenance, explaining the variation of the sum of contracts per year in our data. Each contract amounts to 2-4 million CHF. Those major contracts for road maintenance were predictable for the Ticino cartel, since the Canton tendered them each two years.

Table 1.3: Number and value of annual tenders in Ticino (CHF)

Year	Contracts	Amount
1995	7	16’365’378.95
1996	18	15’881’311.40
1997	50	42’929’902.85
1998	36	28’802’066.70
1999	28	51’896’534.75
2000	27	31’479’500.25
2001	24	46’762’575.10
2002	30	38’713’586.60
2003	21	38’985’740.80
2004	45	35’282’493.70
2005	35	20’926’231.70
2006	14	19’079’459.70
Total	334	387’104’782.50

Table 1.5 reports descriptive statistics for the 17 firms of the Ticino cartel. Table 1.4 describes the number of bids per tenders for the period from 1995 to 2006. We must also keep in mind that the number of bids does not necessarily match the number of bidders because the possibility of building consortia.[3] In general, firms submitted bids in consortium for important contracts.

Table 1.4: The distribution of the bids

Number of bids	2	3	4	5	6	7	8	9	10	11	12	13	Total
Number of tenders	13	31	53	37	42	44	33	30	18	15	10	8	334

[3]A consortium is a joint bidding or a business combination: two bidders or more submit jointly a bid and execute the contract together if they win.

Table 1.5: Descriptive statistics for each firm

Firm	Bids	Win. Bids	Success Rate	Work.	Vertical Integr. Firm	Sh. of Firm1	Sh. of Firm2	Sum of Contracts won	Percent of "Market Share"
3	58	18	0.31	16	1	0	0	17'761'137.20	6%
4	143	17	0.12	45	1	0	1	17'742'856.60	6%
5	44	24	0.55	33	1	0	0	17'693'104.70	6%
6	77	11	0.14	39	1	0	0	13'498'334.30	4%
7	18	7	0.39	8	0	0	0	8'268'229.46	3%
8	125	24	0.19	35	0	1	0	21'954'142.30	7%
9	206	31	0.15	61	0	1	0	20'668'237.20	7%
10	132	21	0.16	70	0	1	0	19'278'461.80	6%
11	86	21	0.24	42	0	1	0	20'421'747.70	7%
12	58	34	0.59	41	0	1	0	26'726'938.70	9%
14	114	22	0.19	27	0	1	0	20'937'920.70	7%
15	192	34	0.18	48	0	1	0	25'287'349.40	8%
16	50	15	0.30	30	0	0	1	13'770'686.40	4%
17	143	20	0.14	36	0	1	0	16'384'310.40	5%
18	81	19	0.23	31	0	1	0	17'917'527.00	6%
19	143	21	0.15	60	0	1	0	19'971'476.70	6%
20	65	13	0.20	35	0	0	0	14'458'499.40	5%

1.2.3 The contract allocation mechanism

The cartel convention was a written document of three pages with 13 articles.[4] It instituted weekly mandatory meeting, at which all firms in the road construction sector participated. The convention sanctioned absence to these meetings without valid and legitimate reasons, and punished absent cartel participants by possible loss of future contracts. In practice, it is unknown if such punishment took effectively place. In each meeting, cartel participants had to announce every new construction contract from public procurement authorities as every other private construction contract above 20'000 CHF.[5] During the meetings, cartel participants discussed the contracts to allocate and the bids to submit.[6] In the following, we discuss the criteria stated in article 7 to allocate contracts among the cartel participants.

According to article 7 of the convention, the cartel should observe the following criteria to allocate contracts:

* **(a)** Amount of contracts won

* **(b)** Localisation of contract

[4]See the convention of the Ticino bid-rigging cartel in decision *Strassenbeläge Tessin* (LPC 2008/1, pp. 78-80).
[5]See convention of the Ticino bid-rigging cartel, art. 5 Scope.
[6]See convention of the Ticino bid-rigging cartel, art. 7 allocation of contracts and art.4 organization.

* **(c)** Specialisation of each firm

* **(d)** Private bidding

* **(e)** Collegial discussion

The first criterion was the most important criterion in contract allocation. The firm with the less amount of work should be then selected to be the designated winner. This criterion matches the firm capacity used in econometric analysis of the bidding function (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b). It seems economically logic to allocate a contract to the cartel participant the less engaged in current contracts, respectively, with the most available "free" capacity to be used.

The second criterion was the localisation, and should play an essential role for contract below 500'000 CHF, i.e. for low value contracts. The firm the least distant from the localisation of the contract should be according to the convention selected to be the designated winner. The second criterion follows again an economical logic and matches the variable distance, which is also used in econometric analysis of the bidding function alongside with the variable capacity.

Specialisation of each firm was the third criterion to allocate contracts. It seems also to be economical logic to give the priority for specific contracts to firms, which are known to be specialized in such works.

The fourth criterion privileged participants first invited by private actors to estimate a quotation. Estimating a quotation causes costs, which are not recoverable if another firm wins the contract. To avoid such sunk costs, the convention stipulated that the firm, who first announced a private contract, had the priority on the contract. It also had another purpose: To have priority on the private contract, the firm had to announce it during the convention meetings. Therefore, it fostered the announcement of private contracts, which are more difficult to observe than public contracts. Since it was important for the convention to appropriately calculate the amount of works for each firm in order to allocate future contracts, cartel participant had to announce private contracts, and if they did not announce them at the convention, the convention could not have properly calculated the amount of works of each firm. Therefore, ensuring the award of private contracts fosters cartel participants to announce it at the meetings.

Finally, the fifth and last criterion to allocate contracts was based on collegial discussion. This criterion is not based on an economical rationale like the previous criteria, but rather on a simple bid rotation scheme, i.e. on a reciprocal agreement of gentleman. The final decision of contract allocation was adopted by a majority. In case of divergence, firms vote in secret, except firms involved in litigation.

The Ticino bid rigging cartel never used monetary transfers, and is therefore a ”weak” cartel (see *McAfee and McMillan*, 1992, for the definition of a weak and a strong cartel). Following *Pesendorfer* (2000), two elements allow a ”weak” cartel to achieve the first-best collusive gain like a strong cartel. First, there must be many contracts to allocate every year within the cartel participants. As we can see from table 1.3, the high number of contracts meets the first condition, with a total of 175 public contracts for the years 1999 to 2004. We also point out that the first condition is a necessity for each ”weak” bid-rigging cartel, functioning with a simple bid rotation scheme or one based on costs.

Second, a Ranking Mechanism, as described by *Pesendorfer* (2000), constraints the cartel participants to report truly their costs, and it should select the lowest-cost bidder among all cartel participants for each contract. It seems that the convention have played the role of the Ranking Mechanism as described by *Pesendorfer* (2000). First, the main criteria of the convention to allocate contracts among cartel participants are based on economic variables as the capacity of firm and the distance to contract location. Second, the convention calculated each week the amount of work for every firm. To some extent, the convention itself estimated the costs for each cartel participants for the contracts and allocate the contracts to the lowest-cost bidder. Such systematic control from the convention avoids adverse selection problem and untrue reports of costs. The convention worked also like the centre suggested by *Aoyagi* (2007). Determining the lowest-cost bidder to win the contract ultimately allows the cartel to achieve the first-best collusive gain. The section 1.4 investigates if the cartel effectively applied the criteria of its convention.

1.2.4 Price increase and damages

After allocating contracts between cartel members, firms discussed prices. For public contracts, firms had to submit a bid in public tenders and therefore to calculate their bids before the meetings.[7] The convention compelled them to submit a bid, respectively a cover bid. Therefore, the coordination of the bids to submit was crucial for the functioning of the cartel.

COMCO did not investigate how the cartel determined the bid of the designated winner. However, it is likely that they should have used a rule or any other mechanism to determine the winning bid. In fact, without such a rule, discussions about price could linger too much. One rule could be the following: the designated winner revealed his bid and, if the price was not exaggerated, the cartel validated the bid. Another rule could be that cartel participants revealed their bids for a specific contract and calculated the arithmetic mean of the bids; the bid of the designated winner could

[7] See convention of the Ticino bid-rigging cartel, art. 6 Scope.

match the mean of the bids calculated for a specific contract.

If other cartel participants had calculated a cheaper bid than the bid fixed in the discussion, they inflated their bids by some factor to ensure that the designated winner actually win the contract. The convention required that the submitted cover bids should be calculated and justifiable for each position on the bidding documentation provided by procurement agencies.[8] Moreover, the cover bids should have been high enough relative to the winning bid so that they would not be considered by procurement agencies ensuring the rewarding of the contract to the designated winner.

The cartel could "freely" determine the winning bid because of the absence of competitive pressure. COMCO decided that the Ticino cartel succeeded to suppress all effective competition on the market for road construction in Ticino. First, participants did not deviate from the convention, mainly because of the threat to be excluded from the supply of asphalt materials and other strategic inputs. Potential exclusion from the road construction market was a sufficient harsh punishment to prevent firms from deviating the cartel. Second, COMCO defined the canton Ticino as the geographic market for road construction, because of the border in the South with Italy and because of the mountains in the North with the rest of Switzerland. To a certain extent, the market was geographically well delimited by natural and political borders so that little competition from outside could have challenged the cartel. And even if new entrants from outside could try to enter the market, they should have had access to mixing plants, which was solely granted to the cartel participants taking part to the convention. To sum up, there was no competitive pressure, which could in any way constraint the cartel in fixing the winning bids.

Absence of competitive constraints for the Ticino cartel resulted in a dramatic price increase. COMCO investigated the evolution of the price indices for road construction in Switzerland, and found that the Ticino price index for road construction was significantly higher, especially since the year 2002. In fact, price indices for the rest of Switzerland decreased in 2002, whereas the price index for Ticino continued rising, as depicted on figure 1.1.[9]

However, the evolution of price indices gives little information about the damages due to the Ticino cartel. To approximate the damages, we rely on the difference between the engineer estimates and the sudden fall in prices at the end of the cartel. COMCO showed in her decision that the means of the bids in a tender were more than 30% cheaper than the engineer estimates in the post-cartel period, whereas the means of the bids were just above the engineer estimates for the cartel period and until the end of the cartel in April 2005.[10]

[8] See convention of the Ticino bid-rigging cartel, art. 4 Organization.

[9] Source: Swiss Federal Statistical Office.

[10] See decision Strassenbeläge Tessin, LPC 2008-1, p. 103.

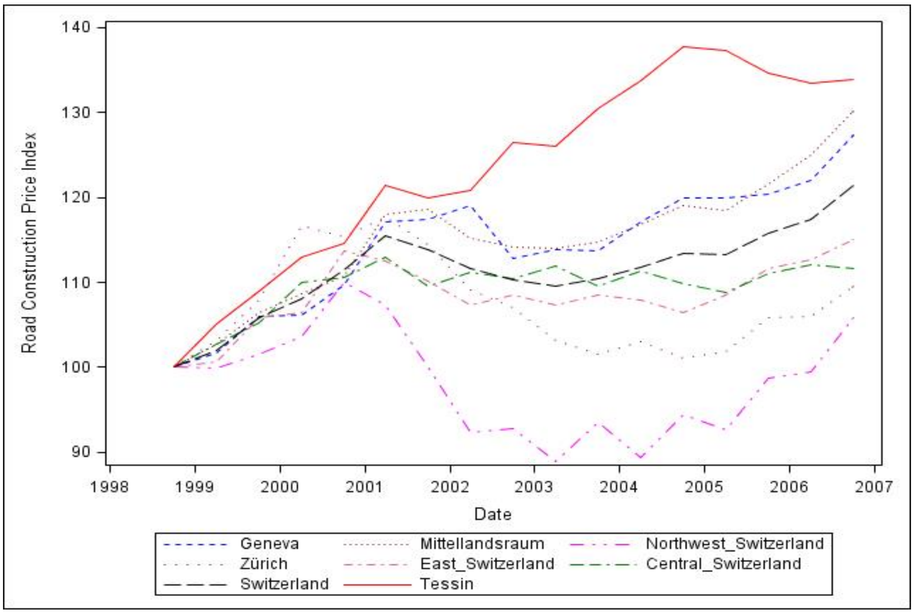


Figure 1.1: The price index of road construction in Switzerland

The notable difference in prices at the end of the cartel indicates that engineers were unaware of the high price paid for road construction contract. They estimated too high prices based on the price evolution, which was completely manipulated by the Ticino cartel. In other words, engineers progressively endogenized the higher cartel price, as proposed by *Harrington and Chen* (2006). Thus, that observation is an indication to use with caution engineer estimates to normalize the bids for constructing the dependent variable, as used by *Bajari and Ye* (2003).

If we consider that the price increase resulting from the Ticino cartel amounted to roughly 30%, then we can approximate the damages due to bid rigging. For the period from year 1999 to year 2003, 278 million CHF were discussed in the convention, which covered 62% of the overall market for road construction. Therefore, the whole market for road construction amounted for this period to 449 million CHF. We reasonably assume that price increases affected the whole market and not just the 278 million discussed in the convention. It would have been very suspect to notice high price differences between the bids for the contracts announced to the convention and the bids submitted for other contracts. Therefore, the damage due to bid rigging amounted to 135 million CHF considering the whole market. The damage was certainly higher since the calculation is based on the available turnover of the cartel participants from year 1999 to year 2003. The turnover from year 2004 to April 2005 is missing. However, using a simple rule of three, if the damages amounted to 135 million CHF for 5 years, it should have amounted to 162 million CHF for 6 years. In summary, the damages of the Ticino cartel were enormous, especially for a local bid-rigging cartel.

1.3 Data and Estimation Strategies

1.3.1 The dependent variables and the hypotheses to test

In the empirical estimation, we investigate first if the lowest-cost bidder submits the lowest bid in a tender and wins the contract. To that purpose, we construct a binary dependent variable, called *lowestbid*, which takes the value 1 if the firm submits the lowest bid in a tender and wins the contract; otherwise, it takes the value 0. We use a logit model and we check whether the cost variables explain the *lowestbid* better than the variables of contract allocation. If the lowest-cost bidder submits the lowest bid in tender, then the cost variables should be significant. In this case, the Ticino cartel achieved the first-best collusive gain although it functioned without monetary transfer. However, if the cost variables are insignificant or if the contract allocation variable based on a simple bid rotation mechanism are significant, then the cartel did not properly apply its convention. In such case, the cartel did not achieve the first-best collusive gain because the designated winner from the cartel did not matched the lowest-cost bidder.

The convention indicates that the cartel participants should discuss the cover bids and not just the winning bids. In other words, cover bids should not be fake, but justifiable according to the costs of each firm. Therefore, we analyse the cover bids and check if and how they are correlated to the cost variables. If firms discussed seriously the cover bids, then bidders with higher costs should submit higher cover bids. In order to examine this hypothesis, we construct the variable *bidrank*, which reflects the rank of each firm in tender t according to their submitted bids. The ranks are from 1 (the lowest bid) to 8 (the highest bid). Rank superior to 8 are uncommon and takes the value of 8. We verify first if bids with higher ranks have higher costs. Then, we check if contract allocation variables based on a simple bid rotation mechanism explain better the distribution of the ranks in a tender than the cost variables. Note, that at the difference of *Porter and Zona* (1993) and *Ishii* (2009), we use an ordered logit model and not a conditional multinomial model, mainly because the order of the bids matters in our analysis.

1.3.2 The cost variables

The distance

A larger distance between firms and contract location increases the costs, other things being equal, and a rational firm should submit a higher bid in a distant location and a lower bid in a closer location. Therefore, the *distance* should negatively affect the probability of submitting the lowest bid in the logit model for all firm i. On the contrary, it should affect positively the distribution of the ranks in the ordered logit model, since the convention assumes that firms with higher costs submit higher bids.

We use the number of kilometres calculated from Google Map between the contract location and the firm place, which is the location indicated on the official record of the bid summary. Then, we add 1 for all observations in kilometre to exclude 0 in order to take the logarithmic of the kilometres.[11] The variable *distance* is $LDIST_{it} = ln(km_{it} + 1)$ for all firm i and for all contract t.

For contracts with two different locations, we calculate the distance in kilometres for both locations and compute a simple mean for the value of the *distance* per firm i. Furthermore, some contracts are provided every two years and concern principally maintenance works in a particular region of Canton Ticino. We took then the three most important locations situated in the designated region considering the importance of the road network, and we compute again a simple mean for the *distance*.[12]

The capacity of firms

The assumption for the *capacity* is the same for the *distance*: A firm with a full own capacity engaged in current contracts should submit a higher bid (if it submits one at all) compared to a firm with little capacity engaged in current contracts. A firm with few contracts won should therefore submit aggressive low bids to win contracts in order to fill its order backlog. As for the *distance*, the *capacity* should affect negatively the probability of submitting the lowest bid in a tender for all firm i. On the contrary, it should affect positively the distribution of the ranks in the ordered logit model, since the convention assumes that firms with higher costs submit higher bids.

Capacity of firms is usually measured in the literature as the volume of contracts won from the beginning of the year until contract t divided by the volume of contracts won during the entire year (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b). Because some major contracts were tendered every two years and because the strong annual fluctuating income of each firm, we calculate the capacity in this paper taking account of the dynamic evolution in the volume of contract won for each firm. First, we sum the contracts won for each firm i and for each contract t. Then, the value of the contracts won is discounted for all firms i by 10% for each month except from 15th July to 15th August and from 15th December to 15th January. These periods are known as vacation period for the construction industry and the activity is paused or significantly reduced. The depreciation rate of 10% represents

[11] Kilometres equal zero if the contract location is in the same place than the firm location.

[12] We asked native people from Ticino to determine the three most important locations for the road network in a particular region.

the amount of work completed by each firm in each month. Once we have calculated the sum of the discounted value of all contracts won for all firms i and at all tender t, we divide the *capacity* by the firm average turnover per year as stated in the COMCO decision.[13]. This last step normalizes the variable *capacity*, which varies from 0 to value over 1, because the firm turnover strongly fluctuates over the years.

Since we do not know the identity of the bidders before January 1999, we assume that all firms start in January 1999 with a capacity equal to the half of its average turnover per year. Thus, the first observation for the *capacity* for all firms i takes the value of 0.5. We denote it CAP_{it} for all firms i at the time of contract t. Note also that we exclude the year 2004 from our sample because we do not have information pertaining to the date for 20% of the contracts tendered and we could therefore not have constructed properly the *capacity*. Our sample includes all tenders from 1999 to 2003.

We also consider the number of workers per firm as explanatory variable. The number of worker remains constant for all firms during the cartel period.

1.3.3 Contract allocation mechanism

If cost variables do not explain the lowest bid in a tender, then the cartel should have found another mechanism to allocate contracts among cartel participants. To sustain an allocation of contracts during many years, the mechanism should have been fair and accepted by all cartel participants involved. *Ishii* (2009) proposes three possible mechanisms, which we present in the following.

First, the number of days without winning a tender is certainly a good candidate as variable for a simple bid rotation mechanism, mainly because it is the simplest way to allocate contracts among cartel participants on a fair basis. If firms involved in a cartel may be willing to be patient and to wait for their turn to take, they surely expect to be rewarded with future contracts. If a firm waits too long to be granted with a contract, it can change its behaviour and leaves the cartel. Therefore, the cartel must assure that all cartel participants win contracts one after another on a fair basis.

In a simple bid rotation mechanism, the cartel picks the firm with the highest number of days without winning a tender to be the winner of the contract. We create a variable for the number of days without winning (*lastwin*) and we assume that it has a positive impact on the probability that the firm submits the lowest bid. However, if we find no impact or a negative impact for the variable *lastwin*, then the Ticino cartel did not consider the number of days without winning as a possible variable for contract allocation. In the ordered logit models, we however expect that *lastwin* negatively affects the distribution of the ranks. If a firm recently won a contract, it should be more

[13] See, Strassenbeläge Tessin (LPC 2008/1, p. 99).

likely that the firm submit a bid on higher ranks than a firm not having won a contract for a while, which should bid on lower ranks.

If the number of days without winning does not explain the contract allocation mechanism, then it is possible that the cartel uses a more sophisticated rule to allocate contracts among cartel participants. The cartel might calculate the turnover won by the participants (and not the number of tenders) to allocate fairly contracts based on the revenue of its members. In other words, the cartel tries to equalize the revenue of all its members.

Concretely, we build a variable for the revenue equalization (*equalization*) and we sum from the beginning of the cartel all the contracts won by each firm. If the cartel has equalized the revenue of its member, then the firms with the lowest revenue won in the cartel period should be the winner. Therefore, we assume that *equalization* should have a negative impact on the probability of winning, respectively on the probability of submitting the lowest bid. However, if we find a positive impact, then it would mean that bigger firms win more contracts. Such result would at least exclude a contract allocation based on a simple bid rotation mechanism. In an ordered logit model, we expect that cartel participants with higher revenue won submitted higher bids than cartel participants with lower revenue won. *Equalization* should therefore have a positive impact on the distribution of the ranks. If it positively affects the distribution of ranks, it would mean that bigger firms submit rather lower bids than smaller firms, which would reflect the greater capacity of bigger firms.

The last possible mechanism to allocate contracts within the cartel consists to score points between cartel participants. The firm with the greatest amount of points is then designated to be the winner of the contract. Even if the mechanism to score points between firms can be complex, we assume in the following an easy mechanism to construct the variable *score*. We associate points to measure the contribution of each firm with the number of cover bids. For each tender t, we sum the number of cover bids submitted by each firm i from the beginning of the cartel period.

This assumption does not seem to be unrealistic, because the cartel should reward participation by allocating more contracts to firms, which often cover other cartel participants. Therefore, we expect that *score* has a positive impact on the probability to be the designated winner by the cartel, respectively on the probability to submit the lowest bid. If we find a negative impact, it would mean that the cartel does not reward participation in form of submitting cover bids. In the ordered logit model, we however expect that *score* has a negative impact, i.e., cartel participants with few cover bids are placed on higher ranks (because they have less chance to be granted with a contract) and that cartel participants having submitted many cover bids should be placed on lower ranks (because they have more chance to be granted with a contract).

1.3.4 Dummies for firms

We use different dummies to discriminate the type of firms. First, we include a dummy for building a consortium. *Consortium* takes the value of 1 for each firm if the firm jointly bids with another firm. We also construct a dummy for vertical integration (*vertint*) taking the value of 1 if the firm owns a mixing plant for asphalt and other materials. The dummy *shareh1* takes the value of 1 if the firm is shareholder in firm 1, and *shareh2* takes the value of 1 if the firm is shareholder in firm 2. We also add dummies for all firms to capture the specialisation of each firm or specific firm advantages.

1.3.5 Contract specific variables and dummies

We use different dummies to discriminate the type of contracts. First, we discriminate between contract location with the dummy *sottoceneri*, taking the value of 1 if the contract location is in the region called Sottoceneri, which is in the South part of the canton Ticino. If *sottoceneri* takes the value 0, the contract location is in the North part of Canton Ticino, called Sopraceneri, which is a more mountainous region. We also add dummies for the types of works. If the contract is related to a cantonal road, the dummy *cantonal* takes the value of 1; for highways or national roads, the dummy *national* takes the value of 1; for maintenance contract on two years, the dummy *maintenance* takes the value of 1.

1.3.6 Consortia

Firms in consortium have the same value for both endogenous variables (*lowestbid* and *bidrank*), but different individual values for the explanatory variables. To verify the robustness of the results for the whole sample, we estimate all models for the subsample solely containing bids from consortium. We also estimate all models for the subsample of individual bids in order to compare the results obtained from both subsamples. Note that the addition of both subsamples gives the whole sample.

1.3.7 Descriptive statistics

Table 1.6 recapitulates all the variables used in the estimation and table 1.7 presents the descriptive statistics.

Table 1.6: Description of the variables

Variables	Description
LOWESTBID	The variable LOWESTBID takes the value 1 if the bid submitted in tender t is the lowest.
BIDRANK	The variable BIDRANK gives the rank of the submitted bids in tender 1 from 1 (the best bid) to 8 (the highest bid). If there is more than 8 bids in a tender, every bid superior to 8 takes also the value of 8.
CAP	Capacity of firm i in tender t.
Distance	Distance of firm i to contract location.
WORKER	The number of workers for firm i. The value does not change for whole cartel period.
LASTWIN	The variable LASTWIN gives the number of days without winning for firm i in tender t.
Equalization	The variable REVSUM gives the turnover already won for firm i in tender t.
SCORE	The variable SCORE gives the number of cover bids submitted by firm i in tender t.
CANTONAL	The dummy variable CANTONAL takes the value 1 if contract is cantonal road.
NATIONAL	The dummy variable NATIONAL takes the value 1 if the contract is a national road or a highway.
MAINTENANCE	The dummy variable MAINTENANCE takes the value 1 if the contract is a maintenance contract tendered every two years.
SHAREH1	The dummy variable SHAREH1 takes the value 1 if the firm is shareholder in firm 1.
SHAREH2	The dummy variable SHAREH2 takes the value 1 if the firm is shareholder in firm 2.
VERTICAL	The dummy variable VERTICAL takes the value 1 if the firm has its own mixing plant.
SOTTOCENERI	The dummy variable SOTTOC takes the value 1 if the contract localisation is in Sottoceneri, 0 if it is in Sopraceneri.
CONSORTIUM	The dummy variable CONSORTIUM takes the value 1 if the firm submits a bid in a consortium with another firm.
DUMMIES FOR FIRMS	Dummies for firms to capture individual specific characteristics of each firm.
NBRBIDS	The variable nbrbids gives the number of submitted bids in tender t.

Table 1.7: Descriptive statistics

Variables	Mean	Median	Std	Min	Max	N
Cap	0.56	0.54	0.26	0	1.45	1030
Distance	2.92	3.18	1.06	0	4.60	1030
Worker	44.20	42	13.84	8	70	1030
Equalization	6.06	5.84	4.07	0	16.97	1030
Score	31.70	28	23.59	0	102	1030
Lastwin	130.96	98	123.67	0	760	1030

Note: “Std”, “Min”, “Max”, and “N” denote the standard deviation, the minimal value, the maximal value and number of observations, respectively.

1.4 Empirical Analysis

1.4.1 Analysis of the lowest bid

We analyze the lowest bids submitted by the cartel participants in different logit models by progressively including the variables presented in the section 1.3. We perform the estimations for the whole sample and for two subsamples. The first subsample solely aggregates the individual bids. The second subsample regroups the bids submitted in consortium. Since firms generally formed consortia for important contract, the subsample of individual bids mainly contains contracts of lower value. The estimation in subsample allow us to test whether the results obtained for the whole sample are robust and coherent.

First, we find that *capacity* and *distance* both negatively affect the probability to submit the lowest bid in a tender. In other words, the results in tables 1.8, 1.9, 1.10 and 1.11 indicate that the lowest bid matches the lowest-cost bidder in a tender. Moreover, if the *distance* is always significant at 1% in all models, the *capacity* is less significant for the sample of the individual bids (see table 1.10). It is however significant for all estimations for the subsample of the bids in consortium, except for estimation (1) (see table 1.11). This result supports a complete implementation of the convention, because the distance should be the first criterion to allocate contracts below 500’000 CHF. Since the subsample of individual bids aggregates contracts of lower value than the contracts for the subsamble of the bids in consortium, this result indicates that the cartel privileged the *distance* for contracts of lower value, as stated in the convention.

Concerning the variables of contract allocation, we find that *equalization* and *score* are significant at 1% for all estimated models. However, both variables do not indicate a logic of a simple bid rotation mechanism. *Equalization* positively affects the probability to submit the lowest bid. Therefore, bigger firms win more often compared to smaller firms, which is economically coherent and refutes a logic based on a simple bid rotation mechanism. The coefficient of *score* is negative and indicates rather that firms win more often if they submit less cover bids. It negates the possibility for the cartel to allocate contracts based on the participation in form of cover bids. Finally, *lastwin* is in most cases insignificant refuting a possible take turn logic among cartel participants.

Firms specific dummies are insignificant except the dummy for consortium. It reflects the facts that the portion of winning bids stemming from bid in consortium is higher that the portion of individual winning bids (see table 1.2). In addition, fixed effects for firms are solely significant for model (1) in table 1.9. It confirms that no specific firm characteristics played a major role in explaining the probability of submitting the lowest bid.

To sum up, the results from the logit models strongly suggest that first the Ticino cartel succeeded in designating the lowest-cost bidder to win the contract. Second, the results refute a logic based on a simple bid rotation mechanism, that is based on the revenue won (*equalization*), or on the participation in the cartel (*score*) or on the number of days without winning (*lastwin*).

Table 1.8: Logit estimation

	(1)	(2)	(3)	(4)	(5)
Intercept	0.84**	-0.46	-0.618	0.83	0.05
	(0.360)	(0.439)	(0.543)	(0.632)	(0.724)
Cap	-0.73**	-1.54***	-1.27***	-1.45***	-1.51***
	(0.305)	(0.384)	(0.403)	(0.409)	(0.424)
Distance	-0.33***	-0.279***	-0.47***	-0.42***	-0.49***
	(0.071)	(0.077)	(0.086)	(0.094)	(0.098)
Worker	-0.02***	0.01	0.001	0.007	0.002
	(0.006)	-0.007	(0.008)	(0.008)	(0.008)
Equalization		0.31***	0.27***	0.28***	0.28***
		(0.039)	(0.041)	(0.041)	(0.042)
Score		-0.057***	-0.047***	-0.05***	-0.05***
		(0.007)	(0.008)	(0.008)	(0.008)
Lastwin		0.0002	0.0001	-0.0001	0.0003
		(0.0007)	(0.0008)	(0.0008)	(0.001)
Consortium			1.45***		1.49***
			(0.193)		(0.282)
Vertint			0.10		0.03
			(0.364)		(0.368)
Shareh2			0.32		0.38
			(0.369)		(0.373)
Shareh1			0.23		0.15
			(0.388)		(0.392)
Nbrbids				-0.17***	-0.07
				(0.045)	(0.049)
Maintenance				0.35	-0.66*
				(0.345)	(0.401)
Cantonal				0.65***	0.15
				(0.249)	(0.278)
National				0.75**	-0.04
				(0.307)	(0.359)
Sottoceneri				-0.16	-0.09
				(0.207)	(0.216)
N	1030	1030	1030	1030	1030
R^2	0.04	0.12	0.18	0.16	0.19

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. “N” and “R^2” denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.9: Logit estimation with fixed effects

	(1)	(2)	(3)	(4)
Intercept	-0.06	0.99*	0.35	1.56*
	(0.430)	(0.545)	(0.563)	(0.808)
Cap	-1.53***	-2.01***	-1.52***	-2.03***
	(0.403)	(0.532)	(0.533)	(0.574)
Distance	-0.28***	-0.27***	-0.45***	-0.525***
	(0.076)	(0.079)	(0.088)	(0.102)
Equalization		0.36***	0.32***	0.33***
		(0.057)	(0.058)	(0.060)
Score		-0.07***	-0.06***	-0.06***
		(0.012)	(0.012)	(0.012)
Lastwin		0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)
Consortium			1.39***	1.52***
			(0.196)	(0.287)
Nbrbids				-0.07
				(0.050)
Maintenance				-0.86**
				(0.413)
Cantonal				0.13
				(0.284)
National				0.01
				(0.366)
Sottoceneri				-0.39
				(0.252)
Fixed effects	Yes***	Yes	Yes	Yes
N	1030	1030	1030	1030
R^2	0.09	0.14	0.19	0.16

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.10: Logit estimation for the individual bids

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	0.35	-1.05	-1.22*	-0.59	-2.26**	-1.11	0.52
	(0.581)	(0.668)	(0.710)	(0.933)	(0.984)	(1.221)	(1.285)
Cap	-0.32	-0.53	-1.42**	-1.09	-1.41**	-1.45**	-1.09
	(0.482)	(0.657)	(0.613)	(0.924)	(0.630)	(0.648)	(0.945)
Distance	-0.51***	-0.42***	-0.44***	-0.40***	-0.42***	-0.41***	-0.42***
	(0.101)	(0.108)	(0.109)	(0.114)	(0.112)	(0.128)	(0.136)
Worker	-0.02**		0.011713		0.01	0.01	
	(0.010)		(0.013)		(0.013)	(0.014)	
Equalization			0.363***	0.41***	0.38***	0.357***	0.39***
			(0.065)	(0.101)	(0.069)	(0.072)	(0.102)
Score			-0.06***	-0.07***	-0.06***	-0.06***	-0.07***
			(0.012)	(0.020)	(0.013)	(0.013)	(0.020)
Lastwin			0.001	0.003*	0.001	0.001	0.003
			(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Vertint					0.80	0.76	
					(0.687)	(0.700)	
Shareh2					0.80	0.68	
					(0.570)	(0.581)	
Shareh1					1.22	1.15	
					(0.777)	(0.79)	
Nbrbids						-0.12	-0.12
						(0.071)	(0.075)
Cantonal						0.08	0.12
						(0.313)	(0.325)
National						0.15	0.33
						(0.533)	(0.559)
Sottoceneri						-0.01	-0.18
						(0.343)	(0.407)
Fixed Effects	No	Yes***	No	Yes	No	No	Yes
N	607	607	607	607	607	607	607
R^2	0.06	0.14	0.15	0.19	0.16	0.16	0.20

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.11: Logit estimation for the bids in consortia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	2.45***	1.22*	1.59**	2.32***	1.76**	1.60*	2.87***
	(0.577)	(0.669)	(0.676)	(0.806)	(0.765)	(0.936)	(1.081)
Cap	-0.60	-1.32**	-1.28**	-1.96***	-1.27**	-1.66***	-2.72***
	(0.430)	(0.536)	(0.541)	(0.687)	(0.545)	(0.590)	(0.757)
Distance	-0.58***	-0.50***	-0.53***	-0.52***	-0.53***	-0.56***	-0.64***
	(0.143)	(0.149)	(0.149)	(0.153)	(0.150)	(0.180)	(0.186)
Worker	-0.024***		-0.004		-0.002	-0.0002	
	(0.008)		(0.009)		(0.010)	(0.010)	
Equalization			0.21***	0.27***	0.22***	0.23***	0.29***
			(0.050)	(0.072)	(0.050)	(0.052)	(0.076)
Score			-0.04***	-0.05***	-0.04***	-0.04***	-0.05***
			(0.010)	(0.015)	(0.010)	(0.010)	(0.015)
Lastwin			-0.0004	-0.001	-0.0005	-0.0002	-0.001
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Vertint					-0.24	-0.21	
					(0.485)	(0.492)	
Shareh2					0.06	0.14	
					(0.516)	(0.526)	
Shareh1					-0.27	-0.30	
					(0.497)	(0.507)	
Nbrbids						0.01	0.01
						(0.069)	(0.071)
Cantonal						0.64*	0.79**
						(0.381)	(0.396)
National						0.46	0.69**
						(0.323)	(0.342)
Sottoceneri						-0.14	-0.51
						(0.300)	(0.355)
Fixed Effects	No	Yes**	No	Yes	No	No	Yes
R^2	0.07	0.09	0.11	0.12	0.11	0.10	0.14
N	423	423	423	423	423	423	423

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

1.4.2 Analysis of the ranks of the bids

As for the logit estimation, we estimate the ordered logit models in the whole sample and in two subsamples (see tables 1.12, 1.13, 1.14 and 1.15). We find that *capacity* and *distance* are significant and positively affect the ranks of the bids. It means that firms with higher capacity engaged in current contracts and with higher distance to contract location submit also higher bids. The result is economically coherent in the sense that we expect firms with higher costs to submit higher bids and therefore to be on higher ranks.

If the *capacity* is less significant compared to the *distance* for the subsample of individual bids, the phenomenon is less salient than for the analysis of the lowest bid. Nonetheless, it still suggests that *distance* was the most important criterion to discuss the price and the ranks of the submitted bids for the contracts of lower value.

The variables for contract allocation again refute a logic based on a simple bid rotation mechanism. *Equalization* and *score* are significant in all model estimated whereas *lastwin* is generally insignificant, as for the analysis of the lowest bids. *Equalization* negatively affects the ranks of the bids. This suggests that firms with less revenue won submit bids on higher ranks and that bigger firms submit bids on lower ranks. *Score* positively affects the ranks of the bids. It implies that firms regularly bidding submit bids on higher ranks. The opposite implies that firms less frequently bidding submit bids on lower ranks. This result excludes a possible bid rotation mechanism based on the contribution of each cartel participant.

Contrasting with the analysis of the lowest bids, we find that specific dummies and fixed effects for firms are significant in explaining the distribution of the ranks. The dummy consortium is significant and negatively affects the ranks of the bids. It logically indicates that the building of consortium reduces the number of bids and the number of ranks. Therefore, bids in consortium are situated on lower ranks than individual bids.

The dummy for vertical integration is significant and negatively affects the rank of the bids for the whole sample (see table 1.12) and the subsample of individual bids (see table 1.14). Firms vertically integrated submit bids on lower ranks than other firms. This result could somehow reflect the bargaining power of vertical integrated firms. As exposed in section 1.2, asphalt and construction materials are strategic inputs and owning a mixing plant could be a valuable advantage compared to other cartel participants.

Fixed effects for firms are generally significant in the ordered logit estimations. This indicates that the specialization of each firm influences the distribution of the ranks.

To sum up, bids submitted by cartel participants are not just fake, but they also imitate the competitive process by matching higher cost bidders with higher bids (reflected in the distribution of the bids). It shows the ability of the cartel not only to agree on the winning bids, but also to discuss the cover bids with accuracy, as stated in the convention.

Table 1.12: Ordered logit estimation

	(1)	(2)	(3)	(4)	(5)
Intercept	-0.14	0.88***	1.24***	-2.09***	-1.43***
	(0.263)	(0.308)	(0.372)	(0.446)	(0.503)
Cap	0.66***	1.36***	1.12***	1.12***	1.24***
	(0.211)	(0.271)	(0.278)	(0.286)	(0.288)
Distance	0.22***	0.20***	0.41***	0.39***	0.41***
	(0.053)	(0.054)	(0.057)	(0.065)	(0.065)
Worker	0.01***	-0.01**	0.00	-0.001	-0.002
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Equalization		-0.27***	-0.22***	-0.21***	-0.20***
		(0.029)	(0.03)	(0.030)	(0.031)
Score		0.04***	0.03***	0.036***	0.030***
		(0.005)	(0.005)	(0.005)	(0.005)
Lastwin		-0.0001	0.0005	0.0006	0.0009
		(0.0005)	(0.0006)	(0.0006)	(0.0006)
Consortium			-1.92***		-1.03***
			(0.132)		(0.180)
Vertint			-0.63**		-0.61**
			(0.252)		(0.259)
Shareh2			-0.01		0.19
			(0.246)		(0.250)
Shareh1			0.01		0.10
			(0.251)		(0.257)
Nbrbids				0.37***	0.31***
				(0.030)	(0.033)
Maintenance				-0.22	0.38
				(0.227)	(0.253)
Cantonal				-0.31**	-0.05
				(0.148)	(0.155)
National				-0.47**	-0.07
				(0.192)	(0.207)
Sottoceneri				0.21	0.140
				(0.137)	(0.137)
N	1030	1030	1030	1030	1030
R^2	0.01	0.03	0.09	0.1	0.12

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.13: Ordered logit estimation with fixed effects

	(1)	(2)	(3)	(4)
Intercept	0.29	-0.36	0.61	-2.34***
	(0.291)	(0.379)	(0.388)	(0.544)
Cap	1.23***	1.77***	1.40***	1.60***
	(0.247)	(0,330)	(0.336)	(0.351)
Distance	0.18***	0.19***	0.41***	0.44***
	(0.054)	(0.055)	(0.058)	(0.067)
Equalization		-0.28***	-0.22***	-0.20***
		(0.041)	(0.042)	(0.042)
Score		0.05***	0.03***	0.03***
		(0.008)	(0.008)	(0.008)
Lastwin		-0.001	0.000	0.000
		(0.001)	(0.001)	(0.001)
Consortium			-1.94***	-1.11***
			(0.135)	(0.182)
Nbrbids				0.31***
				(0.033)
Maintenance				0.54**
				(0.258)
Cantonal				-0.07
				(0.154)
National				-0.10
				(0.208)
Sottoceneri				0.27*
				(0.147)
Fixed effects	Yes***	Yes***	Yes***	Yes***
N	1030	1030	1030	1030
R^2	0.03	0.05	0.10	0.13

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.14: Ordered logit estimation for the individual bids

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	0.78**	1.04***	1.47***	0.99**	1.99***	-1.08*	-2.32***
	(0.353)	(0.374)	(0.408)	(0.498)	(0.477)	(0.637)	(0.695)
Cap	0.02	0.43	0.77**	0.99**	0.91**	0.99***	1.28***
	(0.278)	(0.326)	(0.357)	(0.444)	(0.359)	(0.364)	(0.456)
Distance	0.33***	0.32***	0.34***	0.35***	0.338***	0.32***	0.36***
	(0.063)	(0.065)	(0.064)	(0.066)	(0.065)	(0.073)	(0.075)
Worker	0.01		-0.005		-0.008	-0.007	
	(0.010)		(0.007)		(0.007)	(0.007)	
Equalization			-0.23***	-0.19***	-0.24***	-0.18***	-0.16***
			(0.040)	(0.058)	(0.042)	(0.043)	(0.058)
Score			0.03***	0.02*	0.03***	0.03***	0.03**
			(0.007)	(0.011)	(0.007)	(0.007)	(0.011)
Lastwin			-0.0002	-0.0004	0.0004	0.001*	0.001
			(0.0008)	(0.001)	(0.0008)	(0.0008)	(0.001)
Vertint					-1.08***	-1.10***	
					(0.317)	(0.330)	
Shareh2					0.09	0.43	
					(0.302)	(0.310)	
Shareh1					-0.24	-0.10	
					(0.304)	(0.317)	
Nbrbids						0.32***	0.33***
						(0.039)	(0.041)
Cantonal						0.06	0.04
						(0.160)	(0.161)
National						-0.11	-0.23
						(0.256)	(0.262)
Sottoceneri						0.15	0.25
						(0.176)	(0.187)
Fixed Effects	No	Yes***	No	Yes***	No	No	Yes***
N	607	607	607	607	607	607	607
R^2	0.01	0.04	0.03	0.05	0.04	0.07	0.09

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

Table 1.15: Ordered logit estimation for the bids in consortia

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-2.78***	-1.58***	-2.13***	-2.33***	-2.58***	-3.75***	-4.13***
	(0.483)	(0.541)	(0.560)	(0.660)	(0.663)	(0.847)	(0.926)
Cap	0.81**	1.43***	1.60***	2.38***	1.65***	1.78***	2.71***
	(0.344)	(0.410)	(0.448)	(0.540)	(0.453)	(0.496)	(0.602)
Distance	0.61***	0.57***	0.60***	0.59***	0.60***	0.69***	0.75***
	(0.120)	(0.124)	(0.123)	(0.125)	(0.123)	(0.162)	(0.165)
Worker	0.03***		0.01*		0.01	0.01	
	(0.006)		(0.007)		(0.008)	(0.008)	
Equalization			-0.22***	-0.26***	-0.22***	-0.22***	-0.25***
			(0.044)	(0.062)	(0.044)	(0.046)	(0.065)
Score			0.03***	0.04***	0.03***	0.03***	0.04***
			(0.008)	(0.012)	(0.008)	(0.009)	(0.013)
Lastwin			0.001	0.002*	0.001	0.001	0.002
			(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Vertint					0.36	0.31	
					(0.438)	(0.451)	
Shareh2					0.18	0.17	
					(0.448)	(0.460)	
Shareh1					0.64	0.61	
					(0.450)	(0.472)	
Nbrbids						0.26***	0.26***
						(0.066)	(0.066)
Cantonal						-0.51*	-0.71**
						(0.306)	(0.317)
National						-0.35	-0.52*
						(0.256)	(0.266)
Sottoceneri						0.16	0.41
						(0.251)	(0.273)
Fixed Effects	No	Yes***	No	Yes**	No	No	Yes***
N	423	423	423	423	423	423	423
R^2	0.04	0.06	0.07	0.08	0.07	0.08	0.09

Note: ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "N" and "R^2" denote the number of observations and the pseudo R squared. See table 1.6 for the description of the variables.

1.5 Conclusion

To sum up the results, we find first in the logit models that the variables of costs explain the probability of submitting the lowest bid in a tender. Moreover, the variables of contract allocation refute a logic of simple bid rotation mechanism. The results are also robust when we split the sample in two subsamples, namely in a sample of the individual bids and in a sample of the bids submitted in consortium. We conclude therefore that the Ticino cartel, though ”weak”, functioned a bid rotation scheme based on costs and achieved the first-best collusive gain.

Second we find that the distribution of the ranks of the bids in a tender matches the distribution of the costs. Again, the variables of contract allocation negate a simple bid rotation scheme. The results are also robust in subsamples. Therefore, we conclude that the Ticino cartel did not only select the lowest-cost bidder to win the contract, but also attributed the cover bids a rank according to their costs. The cartel carefully manipulated the winning bids as well as the cover bids in each tender by implementing its convention. Such careful manipulation could explain why the detection method proposed by *Bajari and Ye* (2003) only partially detects the Ticino cartel (see *Imhof*, 2017a). However, even if the Ticino cartel carefully manipulated the bids, the detection method based on simple screens proposed by *Imhof* (2017b) clearly shows the impact of bid rigging on the discrete distribution of the bids.

Chapter 2

Econometric Tests to Detect Bid-rigging Cartels: Do they Work?

2.1 Introduction

Bid-rigging cartels are a pervasive and persistent problem, as attested by the numerous cases prosecuted by competition agencies around the world.[1] Switzerland is not an exception.[2] At the international level, the Organization for Economic Cooperation and Development (OECD) has addressed such issues several times.[3] Fighting against these harmful and inefficient practices should be one of the top priorities for competition agencies. Although leniency programs are surely an important tool to enforce laws for competition agencies, they may also have limits. Competition agencies should therefore be more reactive, if not proactive, in deterring and destabilizing bid-rigging cartels.[4] However, competition agencies need an appropriate detection method in order to be proactive. Does such a method exist for detecting bid-rigging cartels, and, if so, is it an appropriate tool for competition agencies?

The seminal paper of *Bajari and Ye* (2003) proposes two econometric tests for detecting bid-

[1]See, OECD, Ex officio Cartel Investigations and the Use of Screens to Detect Cartels (2013), (http://www.oecd.org/daf/competition/exofficio-cartel-investigation-2013.pdf), and the Report on implementing the OECD Recommendation (2016) (http://www.oecd.org/daf/competition/Fighting-bid-rigging-in-public-procurement-report-2016.pdf).

[2]See *Strassenbeläge Tessin* (LPC 2008/1, pp. 85-112), *Elektroinstallationsbetriebe Bern* (LPC 2009/2, pp. 196-222), *Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Aargau* (LPC 2012/2, pp. 270-425), *Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich* (LPC 2013/4, pp. 524-652), *Tunnelreinigung* (LPC 2015/2, pp. 421-60), *Hoch- und Tiefbauleistungen Münstertal* (LPC 2017/3, pp. 193-245) and *Bauleistungen See-Gaster* (available at the following internet webpage: https://www.weko.admin.ch/weko/fr/home/actualites/dernieres-decisions.html). Furthermore, the Swiss Competition Commission (COMCO) is still investigating three cases at the beginning of 2018.

[3]See, OECD, the Guidelines for Fighting Bid-rigging in Public Procurement (2009), the Recommendation on Fighting Bid-rigging in Public Procurement (2012), and the Report on implementing the OECD Recommendation (2016). Documents are available at the OECD homepage (http://www.oecd.org/daf/competition/fightingbidrigginginpublicprocurement.htm).

[4]See, OECD, Ex officio Cartel Investigations and the Use of Screens to Detect Cartels (2013).

rigging cartels. Based on a first sealed-bid asymmetric procurement model, they formalized a method to detect and screen bid-rigging cartels in an *ex ante* analysis.[5] Different papers have tried to replicate the econometric tests proposed by *Bajari and Ye* (2003). Since they were not able to discriminate clearly between bid rigging and competition, none of them could evaluate exactly how well the econometric tests proposed by *Bajari and Ye* (2003) performed. In this paper, we address this lack of empirical validation by replicating the detection method proposed by *Bajari and Ye* (2003) in the Ticino case. [6] Since we can perfectly discriminate between bid rigging and competition in the Ticino case, we are able to evaluate if the econometric tests proposed by *Bajari and Ye* (2003) are appropriate tools to use in an *ex ante* screening analysis by competition agencies.

In estimating the bidding function following *Bajari and Ye* (2003), we find that the estimated coefficients are consistent in the cartel period with a competitive behavior of firms. Then, we apply the two econometric tests proposed by *Bajari and Ye* (2003) on a pairwise base: Each test considers only two firms, hereafter called a pair of firms or simply a pair.

The conditional independence test checks if bids are independent within firms conditional on some observable covariates. The null hypothesis of competition stipulates that the residues of firm i, drawn from the estimation of the bidding function, are uncorrelated with the residues of firm j. The alternative hypothesis states that residuals are correlated among firms, whose bids are therefore not independent, indicating intended bid rigging. In our case, we find that 89% of the pairs of firms do not fail the conditional independence test at a 5% risk level in the cartel period. The conditional independence test produces too many false negative results, because we should find only rejections for all pairs of firms in the cartel period. In other words, bids are independent in the cartel period since the residues are uncorrelated for most of the pairs of firms, highlighting a competitive behavior of the firms.

The test for the exchangeability of the bids examines if firms react in the same way considering their own costs. To put it differently, if we permute the costs of firm i with the costs of firm j, then firm i should submit the same bids as firm j. Formally, the null hypothesis of competition specifies that the estimated coefficients of firm i do not differ from those of firm j. If the estimated coefficients are not identical across firms, this may be indicative of bid rigging. In our case, we find too many false negative results for the cartel period, since 68% of the pairs pass the test at a 5% risk level. Therefore, many pairs of firms reacted in the same way, when their own costs are exchanged. In summary, firms again behave competitively in the cartel period.

[5] An *ex ante* analysis is an analysis carried out before the opening of an investigation. (See *Imhof et al.*, 2017, for an *ex ante* analysis.)

[6] See *Strassenbeläge Tessin* (LPC 2008/1, pp. 85-112).

Finally, we check the robustness of these false negative results by implementing both tests on two different subsamples. Since cover bids are fake by definition, they should be less informative than winning bids. Putting it differently, the cover bids might be less related to costs than the winning bids. Therefore, we expect to find more rejection if we implement the tests only on cover bids. Since the model of *Bajari and Ye* (2003) assumes that bids are independently conditional on some covariates, bids in a tender are not conditional on being winning bids or cover bids. We can therefore exclude a possible sample selection bias by implementing the tests in two different subsamples.

For the first subsample, we consider only the pairwise observation when firm i and firm j do not win the contract and when they both submit cover bids in the same tender. The first subsample contains solely covers bids, excluding all winning bids.[7] We call it the *indirect cover bids* sample, because neither firm i nor firm j wins the contract, but they both submit cover bids in favor of a third firm g. For the *indirect cover bids* sample, we do not observe better results for the conditional independence test or for the exchangeability test: both tests produce a high number of false negative results once again. Such results also confirm the absence of a sample selection bias, since if bids would have been conditional on being winning bids or cover bids, we would certainly have found more rejection in the conditional independence test. In estimating the bidding function, we find that cover bids are related to cost variables. Cover bids are, therefore, not just fake but also contain some informative value.

For the second subsample, we consider solely the pairwise observation, where firm i wins the contract and firm j submits a cover bid, or the inverse. We denote it as the *direct cover bids* sample, because firm i wins the contract, whereas firm j submits a cover bid that is an intentionally higher bid than that of firm i. The second subsample contains all pairwise observations excluded in the first subsample so that the addition of the two subsamples gives the whole sample. We implement only the conditional independence test, as we have fewer observations for that subsample. In contrast to all previous tests, we find a higher number of rejections, since 69% of the pairs fail the test. This result supports the existence of the Ticino cartel and suggests that the conditional independence test is an appropriate tool to detect bilateral coordination of bids.

Discussing the results obtained, we conclude that the failure of one test should be sufficient to classify a pair of firms as candidates for further investigation. Moreover, the tests suggest that competition agencies should initiate a deeper investigation if a third of the pairs, or more, fail one of the two tests proposed by *Bajari and Ye* (2003). Then, we compare the method of *Bajari and Ye* (2003) with another method, based on simple statistical screens to detect bid-rigging cartels, proposed by

[7] Note that all winning bids are the lowest bids in a tender for the cartel period.

Imhof (2017b). We show that simple statistical screens are less data intensive and produce better results for the Ticino case. We clearly observe the impact of bid rigging on the distribution of the bids, and we find significantly fewer false negative results in the cartel period. We also present the strengths of both methods and how we could combine them to use the strengths of each test.

This paper is related to the papers of *Porter and Zona* (1993, 1999) and *Pesendorfer* (2000). They estimate the bidding function based on costs to illustrate the effects of bid rigging. Using information drawn from previous cases, these authors demonstrated first that colluding bidders do not fit basic economical logic: their bids are not related to their own costs. Second, estimates for colluding bidders differ significantly from estimates for competitive bidders. However, it is not possible to apply the method developed by these authors in an *ex ante* analysis; they aim to prove the anticompetitive effects of bid rigging, and for that purpose, they need prior information on collusion. In contrast to these authors, we do not intend to demonstrate the *ex post* effects of a bid-rigging cartel but rather to investigate how well the method proposed by *Bajari and Ye* (2003) would detect the Ticino cartel in an *ex ante* analysis.

Three papers apply the two econometric tests established by *Bajari and Ye* (2003). First, *Jakobsson* (2007) applied to a Swedish database only the conditional independence test using the Spearman rank correlation as a nonparametric method. She finds that approximately 50% of the pairs failed the test. Since she does not have any prior information about bid rigging, she cannot correctly assess the false positive or false negative results produced by the tests. Second, *Chotibhongs and Arditi* (2012a,b) implement both tests and find evidence of collusion in a group of 6 firms. Even if three of these six firms were involved in bid-rigging cases or bidding fraud, the authors cannot make any conclusions regarding false positive or false negative results. Third, *Aryal and Gabrielli* (2013) assume that costs under competition must first-order stochastically dominate costs under collusion, because collusion increases firm mark-up. Therefore, costs under competition must be higher than costs under collusion. Then, considering four potential colluding bidders identified with the tests proposed by *Bajari and Ye* (2003), they test the first order stochastic dominance with the costs recovered under a competitive model against the costs estimated with a collusive model; they find no evidence for collusion. The estimation of the bidding function is also related to the estimation of structural models for competition and collusion (see *Baldwin et al.*, 1997; *Banerji and Meenakshi*, 2004).

However, all of these papers contrast strongly with very few papers using simple screens to detect bid-rigging cartels (see *Feinstein et al.*, 1985; *Imhof et al.*, 2017; *Imhof*, 2017b).

Section 2.2 introduces the asymmetric first-price procurement model. Section 2.3 presents the data used, and section 2.4 discusses the estimation for the bidding function. Section 2.5 implements the tests for the conditional independence and for the exchangeability of the bids. Section 2.6 proposes a robustness analysis by implementing the econometric tests in two different subsamples. Section 2.7 discusses, in detail, the results of the papers for making policy recommendations. Section 2.8 concludes the paper.

2.2 The Model

In this section, we introduce the asymmetrical procurement auction model drawn from *Bajari and Ye* (2003). Using the properties derived from the equilibrium established by the literature on asymmetrical auctions, they demonstrate that a set of specific conditions has to hold true for a competitive model. Based on these conditions, they formulate tests for the conditional independence and for the exchangeability of the bids. Recapitulating *Bajari and Ye* (2003)'s model allows us to more precisely describe the conditions underlying the two tests, which are applied in the next sections.

Bajari and Ye (2003) consider a procurement auction model with N risk-neutral firms competing for a contract to build a single indivisible public work contract. The firms have independent cost estimates; firm i knows its own cost estimate (c_i) but not its competitors' (c_{-i}). Cost estimates c_i are drawn from a cumulative distribution function $F_i(c_i)$ with the associated probability density function $f_i(c_i)$. Both $F_i(c_i)$ and $f_i(c_i)$ are common knowledge among all competitive firms participating in the auction.

They further assume, first, that for all i, the distribution of costs $F_i(c_i)$ has the support $[\underline{c}, \bar{c}]$ and that the associated probability density function $f_i(c_i)$ is continuously differentiable. Second, for all i, $f_i(c_i)$ is positive on its support $[\underline{c}, \bar{c}]$.

The strategy function of firm i is a function of $B_i(c_i)$, which is assumed to be strictly increasing and differentiable on the support of c_i for all i. We also suppose that its inverse bid function $\phi_i(b_i)$ is strictly increasing and differentiable on the support of the bids. Then, given the costs, cost function, bids and strategic bidding function, *Bajari and Ye* (2003) expressed the expected profit function for firm i as the probability of winning given the strategic bidding functions of competitor j, times a certain mark-up, captured by the difference between the bid and the costs. The expected profit function $\pi_i(.)$ for firm i can be written as

$$\pi_i(b_i, c_i; B_{-i}) = (b_i - c_i)\Psi_i(b_i), \tag{2.1}$$

where

$$\Psi_i(b_i) = \prod_{j \neq i}[1 - F_j(\phi_i(b_i))] \tag{2.2}$$

is the probability of firm i to win the contract, i.e., to submit the lowest bid considering competitors' costs and competitors' inverse bid functions, which can be expressed by $Pr(c_j > \phi_j(b_i))$.

The equilibrium in pure strategies is a Bayes-Nash equilibrium, where the strategic function $B_i(c_i)$ maximizes the profit function b_i for all i and c_i in its support. The first-order condition is given by the following equation:

$$\frac{\partial}{\partial b_i}\pi_i(b_i, c_i; B_{-i}) = (b_i - c_i)\Psi_i^{'}(b_i) + \Psi_i(b_i) = 0, \tag{2.3}$$

where $\Psi_i(b_i)$ is given by 2.2.

By rearranging the first-order condition, they formulated the following differential equation for all i:

$$c_i = b_i - \frac{1}{\sum_{j \neq i} \frac{f_j(\phi_j(b_i))\phi_j^{'}(b_i)}{1 - F_j(\phi_j(b_i))}}. \tag{2.4}$$

Lebrun (1996) and *Maskin and Riley* (2000b) demonstrate that there exists an equilibrium in pure strategies, and the equilibrium bid function is strictly monotone and differentiable. The uniqueness of this equilibrium has been shown in the literature (see *Maskin and Riley*, 2000a,b; *Lebrun*, 1996, 2002).

After identifying the model, *Bajari and Ye* (2003) derive five conditions to be satisfied in equilibrium by the distribution of bids $G_i(b; z)$, where z is a set of observable covariates. Three conditions are classic and imply that, first, the support of each distribution of $G_i(b; z)$ is identical for all i. Second, the equilibrium bid function must be strictly monotone. Third, boundary conditions for the bid function have to be held in equilibrium.

Because of the model specifications, they impose two additional conditions. From these two additional conditions, *Bajari and Ye* (2003) derive the two econometric tests to diagnose collusion. First, firm i's bid and firm j's bid are independently distributed, conditional on a set of covariates z observable to all firms. Conditional independence can be expressed with the following equation:

$$G(b_1, \ldots, b_N; z) = \prod_{i=1}^{N} G_i(b_i; z). \tag{2.5}$$

Testing this equation directly with limited data is not a simple empirical implementation. However, we estimate the distribution of the bids in the next sections, by regressing a set of covariates z in the bids. Then, we test if the residuals are correlated across firms. If residuals are uncorrelated, then the distribution of the bids are independent, conditional on the observable covariates used.

The second condition added by *Bajari and Ye* (2003) postulates that the distribution of bids is exchangeable in equilibrium. For any π and any index i, the following equality holds true:

$$G_i(b; z_1, z_2, \ldots, z_N) = G_{\pi(i)}(b; z_{\pi(1)}, z_{\pi(2)}, \ldots, z_{\pi(N)}). \tag{2.6}$$

This equation implies that if we permute the costs of firm i by π, then the bids of firm i should also be permuted by π. Concretely, we test whether the estimated coefficients of firm i from the estimated regression differ from those of firm j.

These five conditions allow *Bajari and Ye* (2003) to formalize the two following theorems. First, if the distribution of bids $G_i(b; z)$ for all i is generated from a Bayes-Nash equilibrium, then the set of five conditions identified must hold. Second, if the distribution of bids $G_i(b; z)$ satisfies the five conditions, then it is possible to construct the distribution of costs $F_i(b \mid z_i)$ that uniquely rationalizes the observed bids $G_i(b; z)$ in equilibrium. Therefore, the following equation estimates the cost for firm i, given the observable covariates z in equilibrium.

$$c_i = b_i - \frac{1}{\sum_{j \neq i} \frac{g_i(b;z)}{1 - G_i(b;z)}}. \tag{2.7}$$

Equilibrium assumes competition, and we can recover the costs of all firms i with the equation 2.7, if all firms i do not collude. If firms collude, we cannot recover the costs with equation 2.7. However, it is still possible to reformulate the equation 2.7 when collusion occurs. For example, the equation 2.8 proposes to rationalize the lowest bid submitted from the cartel, denoted c, and the bids of noncartel firms, denoted i. C depicts the cartel subset.

$$c_c = b_c - \frac{1}{\sum_{j \neq i, j \notin C} \frac{g_i(b;z)}{1 - G_i(b;z)}}. \tag{2.8}$$

If we consider a closer equation 2.8, we need noncartel firms i to rationalize the costs for the lowest bid submitted from the cartel. Without noncartel firms i, the denominator of the equation 2.8 would be unfixed, and it would be impossible to recover the costs of the lowest bid submitted from the cartel. With the Ticino case, we face such a problem. Because all firms participated in the cartel, we do not have noncartel firms i, and it is therefore impossible to recover the costs with the equation 2.8. However, our approach is different since we do not intend to recover the costs of each firm with the equation 2.8. Since we directly observe the costs, we use them as covariates z in the regressions to perform the econometric tests. Also note that in our case, the cost variables explain the cover bids (see section 2.6). Therefore, we conclude that cover bids still have some informative value in our case.

2.3 Data

The bids

In the econometric estimations, we consider the natural logarithm of all submitted bids $LNBID_{it}$ for all firm i and for all contracts t as dependent variables. It is worth noting that, in contrast to the literature (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Aryal and Gabrielli*, 2013), we do not normalize the bids through the engineer estimates of each contract. First, we possess only a few engineer estimates. Second, engineers in Ticino gradually endogenize the increase in price caused by the cartel, exactly as predicted by *Harrington and Chen* (2006). Indeed, after the breakdown of the cartel, prices fell 30% under those engineer estimates. Therefore, we would not recommend using them in our case. Since we do not standardize the submitted bids, we estimate the regression in the next section with robust variance clustered by firms to account for heterogeneity.

The distance

A more distant contract location increases the costs for a rational firm because of the time lost in transportation for both staff and construction equipment. With other factors being equal, a rational firm would submit a higher bid for a more distant location and a lower bid for a closer location. Therefore, the *distance* positively affects the bids of all firm i. Usually, the literature confirms this positive relationship.

For the construction of the variable *distance*, we refer to section 1.3. Also note that the construction of the variable *distance* is based solely on information pertaining to the bid summaries, which raises some issues. First, some contracts have two different locations on the bid summaries. We therefore calculate the distance for both locations and compute a simple mean for the *distance* of each firm i. Furthermore, the canton of Ticino tendered maintenance contracts for a particular region in Ticino in each of the two years. We consider then, the three most important locations situated in the designated region, considering the importance of the road network, and we computed a simple mean for the *distance* of each firm once again.[8]

Finally, we assume that the addresses of the firms on the bid summaries matched the location of the operation center of each firm. If we cannot verify the information without having access to more detailed information than the bid summaries, both locations usually correspond. Moreover, even if the distance between the operation center of each firm and the contract location is relevant, the distance from the mixing plant to the contract location matters as well. However, we have no

[8] We asked native people from Ticino to determine the three most important locations for the road network in a particular region.

information from the bid summaries about the location where firms buy their materials.

To summarize, the construction of the variable *distance* based on the bid summaries raises some issues. Since the cost variables are only proxies for the real costs of each firm, it should be possible to construct the variable *distance* based solely on the bid summaries. However, if one considers that the bid summaries are not enough to construct the variable *distance* and that additional information is required, then one should question the use of the econometric tests proposed by *Bajari and Ye* (2003) in an ex ante analysis. In fact, if we need specific information on a firm that was not gathered in the bid summaries to construct the variable *distance*, it should be very difficult to apply the tests ex ante in secrecy without drawing the cartel's attention.

The capacity of firms

The assumption for *capacity* is the same as that for *distance*: A firm with a full capacity engaged in current contracts should submit a higher bid (if it submits one at all) than a firm with a greater free capacity. A firm with few contracts should submit aggressive bids to win contracts in order to fill its order backlog. Therefore, the *capacity* that is engaged in current contracts positively affects the bids of all firms i.

For the construction of the variable *capacity*, we again refer to section 1.3. It also raises some issues. Since we construct the variable *capacity* solely based on the bid summaries, we observe only a portion of the market and not the overall market. In fact, the volume of contracts discussed in the convention constitutes approximately 60% to 80% of the market volume, as stated in the COMCO decision. Note that all publicly tendered contracts are included in the convention. Therefore, if we miss a certain portion of the overall market, the construction of the variable *capacity* constitutes a good approximation of the real capacity engaged in current contracts of each firm. If one should need a better measure of the variable *capacity*, one should ask firms for the list of all contracts won each year. Such request, however, seems unrealistic in an *ex ante* analysis run in secrecy. If the bid summaries are insufficient to construct the variable *capacity*, the econometric tests proposed by *Bajari and Ye* (2003) for detecting bid-rigging cartels in an *ex ante* analysis are useless. However, the variable *capacity* is only a proxy for the real costs of each firm and, as presented in the next section, it provides coherent results to capture the effect of the capacity engaged in current contracts of each firm.

Strategic interaction variables

We create two variables to take into account the strategic interaction between firms. The first variable is the minimal distance among rivals, described as $LMDIST_{it}$ for each firm i for contract t. We expect that the minimal distance among rivals positively affects the bid of firm i. The intuition is the following: if firm i knows that all its rivals are very distant from the contract location, it might raise its bid because it assumes that the competition has softened. The second variable is the minimal capacity engaged in current contracts among rivals, described as $MCAP_{it}$ for each firm i for contract t. We expect that the minimal capacity among rivals positively affects the bid of firm i. The intuition is the same as for the minimal distance among rivals. If firm i knows that all potential competing firms have their full capacity engaged in different contracts, it might raise its bid assuming that competition will be less fierce.

Consortium

A consortium is a business combination of multiple firms (in general, 2 or 3 firms). Usually, firms organize a consortium for a specific contract, yet for the Ticino case, we repeatedly find the same consortia formed with the same firms. Therefore, we identify those regular consortia and give them an identification number from 21 to 26. Also note that we give the value of 0 for irregular and occasional consortia.

To determine the cost variables for each consortium, we consider the minimum value of the firms in the consortium for each variable. To deal with consortia, the convention stipulates two mechanisms. One of them takes into consideration the minimum value of each firm in the consortium in order to allocate contracts between cartel participants. Moreover, it is economically logical to consider the minimum value for the cost of each firm forming the consortium, since the purpose of a consortium itself consists of circumventing capacity restrictions, distant location or any other disadvantages. Table 2.1 provides basic descriptive statistics for the cartel and post-cartel period.

Table 2.1: Summary of descriptive statistics

Descriptive statistics for the cartel period

Variable	Mean	Std	Min	Max	N
LNBID	13.57	1.12	9.99	15.65	778
LDIST	2.83	1.12	0	4.60	778
CAP	0.54	0.27	0	1.45	778
MLDIST	1.95	1.09	0	4.47	778
MCAP	0.25	0.20	0	1.11	778

Descriptive statistics for the post-cartel period

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
LNBID	13.12	0.88	11.03	15.04	226
LDIST	2.93	1.12	0	4.58	226
CAP	0.42	0.21	0	1.07	226
MLDIST	2.14	1.21	0	4.42	226
MCAP	0.18	0.13	0	0.68	226

Note: "Std", "Min", "Max", and "N" denote the standard deviation, the minimal value, the maximal value and the number of observations, respectively. "LNBID", "LDIST", "CAP", "MLDIST" and "MCAP" denote the natural logarithm of the bids, the logarithm of the distance, the capacity engaged in current contracts, the minimal distance among rivals, the minimal capacity engaged in current contracts among rivals, respectively.

2.4 Estimating the bidding function

As explained in section 2, it is convenient to use a regression analysis in order to test the conditional independence and the exchangeability of the bids. The related papers in the literature generally estimate a panel model to analyze the structural relationship between the observable covariates z and the bids (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Chotibhongs and Arditi*, 2012a,b; *Aryal and Gabrielli*, 2013). We therefore formulate the following panel model:

$$y_{it} = x_{it}^{'}\beta + f_{it}^{'}\alpha + \varepsilon_{it}. \tag{2.9}$$

In equation 2.9 , x_{it} includes K estimators, and $f_{it}^{'}\alpha$ captures the individual fixed effects, including a constant term, where i is the subscript for firms and t is the subscript for contracts. y_{it} is the logarithm of the bids, submitted by firm i for contract t.

In our case, the exogenous variables x_{it} are the cost variables presented in the previous section, namely, the distance ($LDIST_{it}$), capacity (CAP_{it}), and strategic interaction variables ($LMDIST_{it}$ and $MCAP_{it}$). If $LDIST_{it}$ and CAP_{it} vary for all firms i and for all contracts t, then each $LMDIST_{it}$ and $MCAP_{it}$ solely comprise two values per contract t. For example, the variable $LMDIST_{it}$ of the least distant bidder for contract t takes the value of the second least distant bidder for contract t. For all other bidders (excluding the least distant bidder) for contract t, $LMDIST_{it}$ takes the value of the least distant bidder. Also note that the cost variables are solely proxies for the real costs of each firm. Following the bidding function proposed by *Bajari and Ye* (2003), we add dummies for contracts (α_t) and for firms (γ_i) to capture fixed effects.

Based on these cost variables and fixed effects, we estimate the following panel equation, with robust variance clustered by firms:

$$LNBID_{it} = \beta_0 + \gamma_i + \alpha_t + \beta_1 LDIST_{i,t} + \beta_2 CAP_{i,t} + \beta_3 LMDIST_{i,t} + \beta_4 MCAP_{i,t} + \epsilon_{it}. \tag{2.10}$$

Empirical results

Table 2.2 reports the estimation of equation 2.10 for the cartel period and the post-cartel period. For the cartel period, we use 130 dummies for contracts (α_t) and 23 dummies for firms (γ_i) in the estimation. We have a total of 778 observations and 158 regressors.

The estimated coefficients for the *distance* ($LDIST_{it}$), the *capacity* (CAP_{it}), and the *minimal capacity* ($MCAP_{it}$) used among rivals are positive and significant. The results are coherent with the expected behavior of firms in competition. If, all things being equal, $LDIST_{it}$ of firm i increases by 1%, it raises firm i's bid by 0.79%. The same goes for the firm capacity: if, all things being equal, firm i's CAP_{it} engaged in previous contracts increases, then firm i will submit a higher bid.

Turning to the strategic interaction variables, we find that the minimal distance among rivals is nonsignificant. However, the minimal used capacity among rivals ($MCAP_{it}$) is significant and has even a stronger positive effect on firm i's bid than its own used capacity. Intuitively, if firm i knows that other firms already have a high capacity engaged in other contracts, it bids higher, assuming that competition is soft since the other firms have too much capacity used to compete aggressively. The fixed effects for contracts (α_t) and for firms (γ_i) are also significant. Moreover, we notice that the adjusted R^2 is very high. Such a high adjusted R^2 is not uncommon for panel models, and it also suggests that the bidding function as expressed by equation 2.10 explains almost all of the variation observed in the bids. However, if we retrieve the fixed effects for contracts (α_t), the $Adj.R^2$ decreases to 0.3534, indicating that the fixed effects mainly contribute to explain the variation in the bids. To summarize, the results in the cartel period depict that firms adopted a behavior that was fit for a competitive situation.

For the post-cartel period, we use a total of 63 regressors, including 39 dummies for contracts (α_t) and 19 dummies for firms (γ_i). With 224 observations, we have 3.5 times fewer observations than in the cartel period. All variables are insignificant in the estimation of the equation 2.10, which can be at least partially explained by the high values for the standard deviation, 10 times higher than in the cartel period. The small sample size and the transition from a well-organized cartel to a sudden competitive situation may explain such inaccuracy in the estimation. Thus, even if we replicate the econometric tests on the post-cartel sample for comparison purpose, we must interpret them with caution. Finally, the adjusted R^2 is again very high, although all variables are insignificant. The adjusted R^2 reduces to 0.2922 when excluding the fixed effects, indicating again that the high number of dummies compared to the sample size mostly explains the variation in the bids.

Table 2.2: Estimation of the bidding function

Variable	Cartel Period	Post-Cartel Period
C	13.525***	12.8866***
	(0.0159)	(0.1325)
LDIST	0.0079***	0.0258
	(0.0025)	(0.0212)
CAP	0.019***	0.0844
	(0.0062)	(0.0635)
LMDIST	-0.0012	0.028
	(0.0041)	(0.0322)
MCAP	0.0429**	0.2709
	(0.0208)	(0.2416)
Dummies for contracts	130	39
Dummies for firms	23	19
N	778	224
R^2	0.9995	0.9852

Note: "C", "LNBID", "LDIST", "CAP", "MLDIST", "MCAP", "N" and "R^2" denote the intercept, the natural logarithm of the bids, the logarithm of the distance, the capacity engaged in current contracts, the minimal distance among rivals, the minimal capacity engaged in current contracts among rivals, the number of observations and the adjusted R squared, respectively. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

2.5 Testing for Collusion

In this section, we implement the econometric tests for the conditional independence and exchangeability of the bids. To implement the tests, we estimate the same equation as equation 2.10 but we allow coefficients to vary for all firms i. This is necessary if we want to implement the tests of the exchangeability of the bids. All tests presented hereafter are based on the following panel equation:

$$LBID_{it} = \beta_0 + \gamma_i + \alpha_t + \beta_{1,i} LDIST_{i,t} + \beta_{2,i} CAP_{i,t} + \beta_{3,i} LMDIST_{i,t} + \beta_{4,i} MCAP_{i,t} + \epsilon_{it}. \tag{2.11}$$

Also note that in the rest of the section, we present results not only for a standard risk level of 5% but also for a risk level of 10%. As explained in the introduction, we find too many false negative results. In other words, the tests do not reject the null hypothesis of competition, although they should reject it because of the bid-rigging cartel. A possible way to address the problem of false negative results consists of raising the risk level to 10%. Because raising the risk level from 5% to 10% would increase the false positive results, we do not recommend doing it in other cases, especially in an *ex ante* screening analysis. However, our case is special since we are trying to investigate why the tests do not reject the null hypothesis of competition in the cartel period. Therefore, presenting the results for a risk level of 10% allows us to discuss the sensitivity of the tests. If many pairs fail the tests at 10% but not at 5%, such results could indicate an issue in the power of the tests.

2.5.1 The conditional independence test

After estimating the equation 2.11, we test if the residuals of firms i and j are correlated. If the residuals are uncorrelated between firms i and j, their bids are independent conditional on the observed cost variables and the fixed effects included in the regression. However, if we find that residuals are correlated, bids are not independent conditional on the covariates used in the regression. In such a case, we reject the null hypothesis of competition, which is the Nash equilibrium of the asymmetric procurement model. Formally, the null hypothesis is the following:

$$H_0 : \rho_{ij} = 0, \tag{2.12}$$

where ρ_{ij} is the Pearson correlation coefficient. We perform the test only if there are at least five observations for a pair of firms, in other words, if both firms simultaneously bid on at least five contracts. If r is the coefficient of correlation calculated from the data and n the number of observations for each pair (where $n \geq 5$), we apply the following Fisher Z transformation:

$$Z = \frac{1}{2} ln \frac{1+r}{1-r}, \tag{2.13}$$

which is approximatively normal with

$$\mu_Z = \frac{1}{2} ln \frac{1+\rho}{1-\rho} \quad and \quad \sigma_Z = \frac{1}{\sqrt{n-3}} \tag{2.14}$$

If we normalize Z in order to obtain the standard normal distribution, we have

$$z = (Z - \mu_Z)\sqrt{n-3}, \tag{2.15}$$

where $\mu_Z = 0$ under the null hypothesis, i.e., $\rho = 0$. The test statistic is then $Z\sqrt{n-3}$.

We perform the conditional independence test on 133 pairs in the cartel period, and we reject the null hypothesis at a 5% risk level for 15 pairs and at a 10% risk level for 24 pairs (see table 6.1 in the appendix for chapter 2). The failure proportion is 11% and 18%, respectively. Such a result is surprising because it suggests that false negative results are still substantial, even if we consider a risk level of 10% for the tests.

For the post-cartel period, we apply the conditional independence test on 47 pairs and we find that 7 pairs fail the test at a 5% risk level, and 24 pairs fail at a 10% risk level (see table 6.2 in the appendix for chapter 2). The failure proportion is 15% and 21%, respectively. The result is confusing because we find slightly more rejection for the post-cartel period than for the cartel period. We would have expected the contrary. Because we perform the tests in the post-cartel period, the failure proportion is to be interpreted as false positive results. However, the small sample size for the post-cartel period and the possible inaccurate estimation of the standard deviation both indicate to be cautious when interpreting the results. Nonetheless, if the econometric tests produce too many false negative results for the cartel period, we cannot exclude that they also produce some false positive results for the post-cartel period.

2.5.2 Test for the exchangeability

The test for the exchangeability of the bids examines if the coefficients estimated in equation 2.11 are identical between firms. If they are identical, firms react in the same way based on their own costs. In other words, if we permute the costs of firm i with the costs of firm j, then firm i should submit the same bids as firm j. The null hypothesis of competition specifies that the estimated coefficients of firm i do not differ from those of firm j. Formally, the null hypothesis for the exchangeability of the bids is the following:

$$H_0 = \beta_{ki} = \beta_{kj} \quad \forall i, j, i \neq j, \quad and \quad \forall k = 1, \cdots, 4. \tag{2.16}$$

The test is implemented with the following F-statistic

$$F = \frac{(SSR_C - SSR_U)/J}{SSR_U/(N-k)}, \tag{2.17}$$

which has an F-distribution with parameters (J, N-k) under the null hypothesis, where J is the number of constraints, N the sample size and k the number of regressors.

Again, we implement the test solely on pairs of firms, which simultaneously bid for at least five contracts, for which we have at least five observations. Table 6.5 in the appendix for chapter 2 shows the results for 133 pairs. We find that 42 pairs fail the test at a 5% risk level and 58 fail at a 10% risk level. The failure proportion is 32% and 44%, respectively. Failures are more important for pairs with fewer observations. The pairs failing the test at 5% have, on average, 14 pairwise observations, whereas the pairs passing the test have, on average, 19 pairwise observations.

We apply the test for the exchangeability of bids in the post-cartel period to 47 pairs of firms (see table 6.6 in the appendix for chapter 2). We find that 4 pairs fail the test at a 5% risk level and 8 at a 10% risk level. The failure proportion is 9% and 17%, respectively. Therefore, the failure percentage decreases from the cartel to the post-cartel period as expected.

To summarize, the tests for the exchangeability of bids perform better, because they produce fewer false negative results than the conditional independence tests. However, false negative results remain important, because more than half of the pairs pass the test, although they should fail. Furthermore, the decreasing number of failures of the tests of the exchangeability of bids in the post-cartel period indicates coherent results contrasting with the conditional independence test. However, we cannot exclude that the test of the exchangeability of bids does not produce false positive results, as does the conditional independence test.

Considering the simultaneous application of both tests at a 5% risk level, we find that only 5 pairs fail both tests in the cartel period and 53 pairs fail solely one of them. The failure proportion is 4% and 40%, respectively. At a 10% risk level, we find that solely 9 pairs fail both tests and 73 pairs fail only one of them. The failure proportion is 7% and 55%, respectively. This result suggests that the failure of one test should be sufficient to raise concerns about the existence of intended bid rigging. In that case, the econometric tests proposed by *Bajari and Ye* (2003) correctly classify four pairs of ten as bid-rigging cartels, whereas six pairs of ten in the Ticino cartel escape the tests. Such a failure proportion is low, yet the failure proportion increases from 40% to 55% when considering a 10% risk level. This indicates that the number of failures changes significantly with the risk level, which could indicate possible issues in the power of the tests.

For the post-cartel period, we find that no pair simultaneously fails both tests at a 5% or 10% risk level. However, we find that 11 pairs fail at least one test at 5% and 18 at 10%. The failure proportion is 23% and 38%, respectively. Therefore, one pair of five are classified as potential bid-rigging cartels but are competing, when considering a 5% risk level. Again, the results for the post-cartel period have to be interpreted with caution. Nonetheless, we cannot exclude that the econometric tests proposed by *Bajari and Ye* (2003) also produce many false positive results.

Table 2.3: Summary of the econometric tests

Summary of the tests on 133 pairs for the cartel period

Test	*Risk Level*	Failure	% of Failure
Conditional Independence	$\alpha = 0.05$	15	11 %
	$\alpha = 0.1$	24	18%
Exchangeability	$\alpha = 0.05$	42	32%
	$\alpha = 0.1$	58	44%
Fail one test	$\alpha = 0.05$	53	40%
	$\alpha = 0.1$	73	55%
Fail both tests	$\alpha = 0.05$	5	4%
	$\alpha = 0.1$	9	7%

Summary of the tests on 47 pairs for the post-cartel period

Test	*Risk Level*	Failure	% of Failure
Conditional Independence	$\alpha = 0.05$	7	15%
	$\alpha = 0.1$	24	21%
Exchangeability	$\alpha = 0.05$	4	9%
	$\alpha = 0.1$	8	17%
Fail one test	$\alpha = 0.05$	11	23%
	$\alpha = 0.1$	18	38%
Fail both tests	$\alpha = 0.05$	0	0%
	$\alpha = 0.1$	0	0%

2.6 Robustness Analysis

In the previous section, we test for bid rigging, and we find a high number of false negative results. The results show that firms behave in a competitive way, although we implement the tests in the cartel period. Thus, we examine in this section, if these results are robust for two different samples, which allows us to analyze the cover bids more closely. Since cover bids are fake by definition, they might be less related to the cost variables than the winning bids. If this assumption is true, we should find more rejection for these two samples, including fewer false negative results. Since the model used by *Bajari and Ye* (2003) assumes that bids are independent conditional on some covariates, bids in a tender are not conditional on being winning bids or cover bids. We can therefore exclude a possible sample selection bias for implementing the tests in two different samples.

For the first sample, we consider only the pairwise observation, when firm i and firm j both submit cover bids. We call this first subsample the *indirect cover bids* sample because neither firm i nor firm j wins the contract, but they both submit a cover bid in favor of a third cartel participant. The sample for the indirect cover bids contains solely cover bids and excludes all winning bids, which are the lowest bids in the cartel period.

For the second sample, we select solely the pairwise observation, where firm i wins the contract and firm j submits a cover bid. We call this sample the *direct cover bids* sample because firm i wins the contract, whereas firm j submits a cover bid directly in favor of firm i. Also note that the second sample contains all pairwise observations excluded in the first subsample so that the addition of the two samples shows the whole sample.

In the following, we implement the conditional independence test and the test for exchangeability of bids on the indirect cover bids sample. Then, we apply solely the conditional independence test on the direct cover bids sample since we have fewer observations for that sample.

2.6.1 Testing collusion for the indirect cover bids sample

The conditional independence test

Since cover bids should be fake by definition, they should not be independent conditional on covariates but rather dependent on each firm submitting a higher bid in order to cover the designated bid by the cartel to win the contract. Since the higher cover bids may not match the costs of each firm, the residuals drawn from the regression should be correlated across firms. It is therefore interesting to apply the conditional independence tests solely on the indirect cover bids sample, for which we expect more failure.

To implement the test, we use the same residuals of equation 2.11 from section 4, but we suppress the residuals of the winning bids for each contract. We calculate the Pearson correlation coefficient and use, as in section 4, the Fisher transformation. Table 6.3 in the appendix for chapter 2 recapitulates the results. The tests reject the null hypothesis for 14 pairs at a 5% risk level and for 25 pairs at a 10% risk level; the failure proportion is 15% and 26%, respectively. Therefore, the proportion of failures for the conditional independence tests does not vary with that sample. For the whole sample, we find, again, too many false negative results.

The test for the exchangeability of the bids

As for the conditional independence tests, we suppress all winning bids in the cartel period, and we estimate equation 2.11 once again. We apply the test only if we have at least five pairwise observations for each pair of firms. The motivation to reduce the sample solely based on the indirect cover bids is different from the reason mentioned for the conditional independence test. Looking at figure 2.1 drawn from *Imhof et al.* (2017), we observe an important gap between the winning bids and the cover bids. In fact, the average gap is approximately 5%. This contrasts with the gaps of the cover bids, which are significantly smaller. Such pattern is observable for the majority of the contracts in the cartel period (see *Imhof*, 2017b). Therefore, if cover bids are very close one with another and if costs differ for each firm, then the estimated coefficients of the equation 2.11 could also differ among firms. In other words, we expect a greater number of failures.

Bids (in ascending order, CHF)		Difference X_n-X_{n+1}
X_1	520'816	
		Δ_1= 36'800
X_2	557'616	
		Δ_2= 4'648
X_3	562'264	
		Δ_3= 3'561
X_4	565'825	
		Δ_4= 6'141
X_5	571'966	
		Δ_5= 4'151
X_6	576'117	

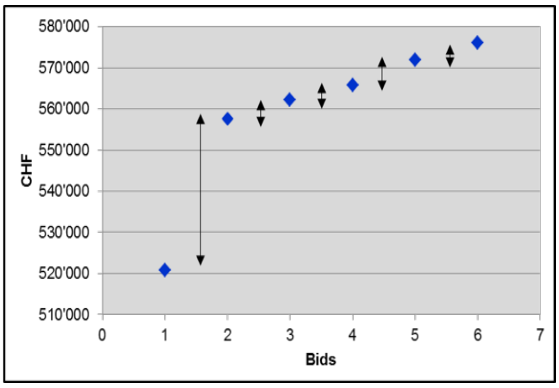


Figure 2.1: Typical cover bidding mechanism in Ticino

Table 2.4 presents the results for the estimation of the equation 2.11 for the indirect covers bids sample. We note that all variables are positive and significant. The distance has virtually the same effect on the bids, as shown in the whole cartel period sample: if, all things being equal, the distance of firm *i* increases by 1%, it raises firm *i*'s bid by 0.75%. The own used capacity and the minimal used capacity among rivals have a weaker effect on the bids compared to the whole cartel period sample.

They are significant only at a 10% risk level. Interestingly, we observe that the minimal distance among rivals is positive and significant at a 10% risk level, whereas it is insignificant for the whole sample. The results of the regression may be surprising, since we would have expected cover bids to be less connected with the cost variables. However, it seems that cover bids follow an economic rationale in the cartel period.

Table 2.4: OLS estimation for the bidding function of the cover bids

Variable	Cover Bids
C (constant)	13.4877***
	(0.0129)
LDIST	0.0075***
	(0.002)
CAP	0.0092*
	(0.0047)
LMDIST	0.0066*
	(0.0034)
MCAP	0.0337*
	(0.0181)
Dummies for contracts	130
Dummies for firms	22
N	645
R^2	0.9998

Note: "C", "LNBID", "LDIST", "CAP", "MLDIST", "MCAP", "N" and "R^2" denote the intercept, the natural logarithm of the bids, the logarithm of the distance, the capacity engaged in current contracts, the minimal distance among rivals, the minimal capacity engaged in current contracts among rivals, the number of observations and the adjusted R squared, respectively. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 6.7 in the appendix for chapter 2 presents the results of the tests for 96 pairs. We find that 19 pairs fail at a 5% risk level and 28 fail at a 10% risk level. The failure proportion is 20% and 29%, respectively. Then, the portion of pairs failing the test of exchangeability for the indirect cover bids sample decreases by 12% and 15%, respectively.

We would have expected to find more failures for this sample. We explain this result by two causes that are mutually nonexclusive. First, the costs of the cover bids do not differ as much as we could have expected. However, if they differ, they enter in a symmetric way in the bidding function. Second, firms met together each week, and they extensively discussed the bids for public contracts, as stated in the cartel convention. Regular discussions could explain why costs, if they differ, enter in a symmetric way in the bidding function. In any case, the tests confirm, again, the high number of false negative results in the cartel period, as observed for the whole sample.

2.6.2 Testing collusion for the direct cover bids sample

By applying the conditional independence test, we again calculate the Pearson correlation coefficient and use the Fisher transformation as we did in section 2.4. Table 6.4 in the appendix for chapter 2 presents the results. We consider, again, all pairs with at least five observations, and we test 35 pairs for the direct cover bids sample. As expected, 24 pairs reject the null hypothesis at a 5% risk level, and 28 reject the null hypothesis at a 10% risk level. The proportion of failing pairs is 69% and 80%, respectively. Therefore, we find fewer false negative results for the direct cover bids sample. Such results are consistent with the Ticino cartel.

Figure 2.2 depicting the pairwise residuals of firms 9 and 15 and for the whole sample may intuitively explain why we find more rejection for the direct cover bids sample and not for the whole sample. In figure 2.2, we differentiate the type of cover bids between indirect and direct cover bids, represented by circles and crosses, respectively. In the previous section, we found that the pair (9,15) had 62 simultaneous bids with an insignificant correlation of -0.0595, indicating that the pair does not fail the conditional independence test. Considering only the indirect cover bids (circles on the figure), we find 50 simultaneous (indirect) cover bids with a significant positive correlation of 0.3498. However, if we restrict the sample solely to the direct cover bids (crosses on the figure), we observe 12 simultaneous bids and a significant negative correlation of -0.8866.

In fact, the positive correlation from the indirect cover bid sample cancels the negative correlation from the direct cover bid sample. Therefore, we find that the correlation for the whole sample is insignificant. Such a phenomenon is common for many pairs, which successfully pass the conditional independence test for the whole sample but fail the direct cover bids sample. The result also indicates that the conditional independence test is better designated for detecting bilateral agreement and not a complete bid-rigging cartel, as in the Ticino case.

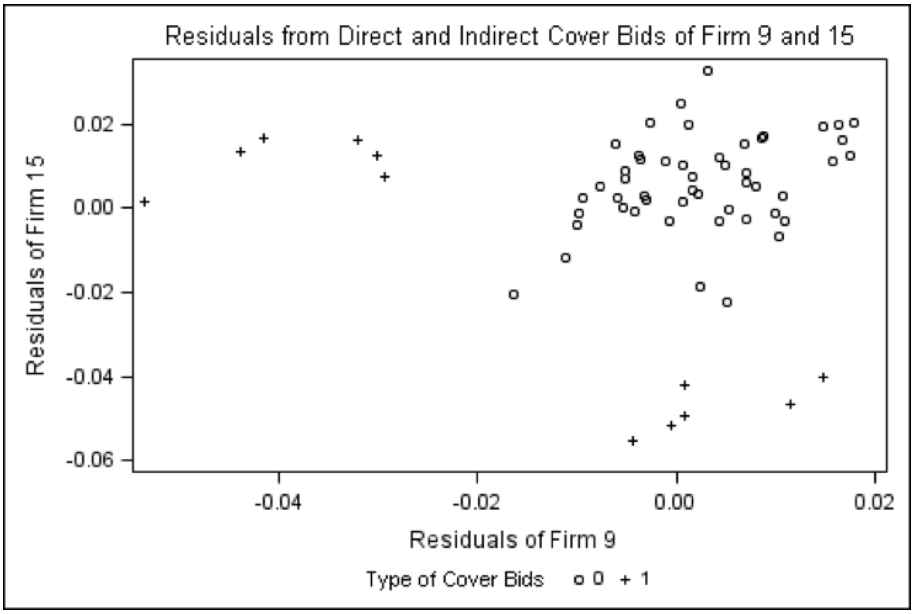


Figure 2.2: Pairwise residuals of firm 9 and 15

2.7 Policy Implication for Competition Agencies

In this section, we first discuss the high number of false negative results. Second, we address the question of classifying a pair of firms as a potential cartel. Third, we compare the econometric tests proposed by *Bajari and Ye* (2003) with the simple screens developed by *Imhof* (2017b) and *Imhof et al.* (2017) for detecting bid-rigging cartels. From that comparison, we deduce some recommendations for competition agencies.

The results of the previous sections show that the econometric tests of *Bajari and Ye* (2003) produce too many false negative results for the Ticino case. In statistics, a false negative result is a type II error: the test should reject the null hypothesis but it does not reject it, whereas the null hypothesis is definitively false. For both econometric tests, the null hypothesis is competition, and we do not reject it for a large percentage of pairs for both tests, although we implement the econometric tests in the cartel period. Therefore, considering the severity of the Ticino cartel, how can we explain the high number of false negative results observed?

Incorrect data or misconstructed variables should contribute to reject the null hypothesis of competition. It would be very unlikely to have incorrect data or misconstructed variables fitting to the hypotheses of Nash equilibrium in a first-price sealed-bid asymmetric procurement model. Moreover, the estimation of the bidding function suggests that firms behave following an economic rational. It would, again, be very unlikely to find such a result if data are too imprecise or if the variables are misconstructed. Therefore, it seems realistic to exclude a data-driven explanation for the many false negative results produced by the econometric tests. In any case, if the bid summaries are in-

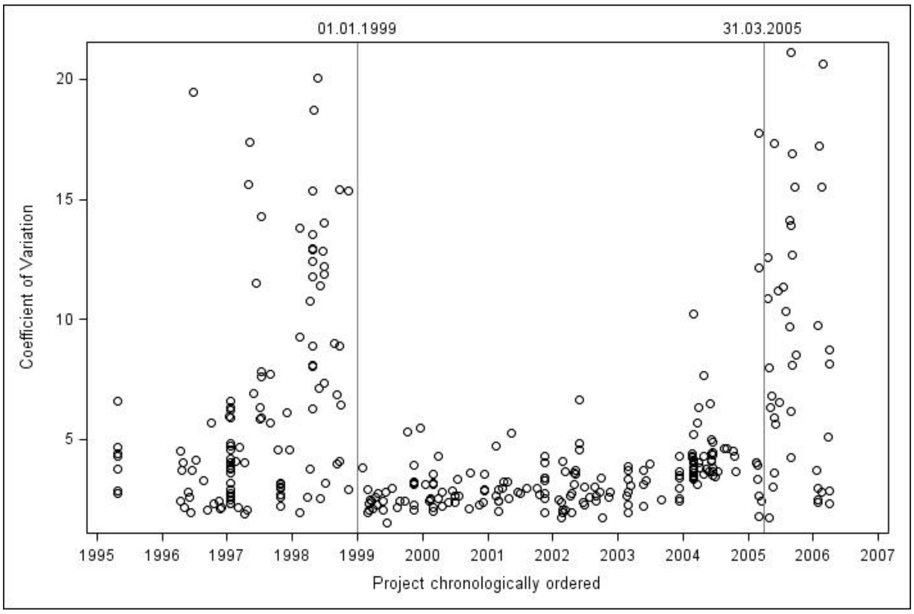


Figure 2.3: The evolution of the coefficient of variation

sufficient to construct the variables for estimating the bidding function, then the data requirement would be too high to implement *ex ante* the econometric tests proposed by *Bajari and Ye* (2003). One can also attempt to question the bidding function. However, the literature has well established the empirical identification for the bidding function used in this paper (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Chotibhongs and Arditi*, 2012a,b; *Aryal and Gabrielli*, 2013).

If firms pass the two econometric tests, although they collude on all contracts, it might imply that they manage to pass through the tests proposed by *Bajari and Ye* (2003). Under some circumstances, *Bajari and Ye* (2003) admits the possibility that the competitive model might also encompass collusion. Indeed, if cartel members scale their bids with a common factor, the assumptions underlying the competitive model proposed by *Bajari and Ye* (2003) remains nonviolated: the cartel can pass through the tests.

However, we can exclude, in our case, the bid-scaling phenomenon. *Imhof* (2017b) applies simple statistical screens for detecting the Ticino cartel and finds that the screens effectively capture the impact of the bid rigging. Figure 2.3 drawn from *Imhof* (2017b) depicts the evolution of the coefficient of variation, where the two vertical lines delimit the cartel period. The difference between the cartel and the post-cartel period is eye-catching: the coefficient of variation is significantly lower in the cartel period, indicating intended bid rigging, as predicted by the variance screen (see *Imhof et al.*, 2017; *Imhof*, 2017b).

Simple screens function only if cartel members do not scale their bids. If they scale their bids, it is impossible to detect collusion with simple screens, as shown by *Imhof* (2017b). In summary, we can exclude the bid-scaling phenomenon for the Ticino case. Therefore, it seems realistic to consider that a bid-rigging cartel can manage to pass through the econometric tests proposed by *Bajari and Ye* (2003) without scaling their bids. The result is somehow pessimistic towards the econometric tests proposed by *Bajari and Ye* (2003) because, if one bid-rigging cartel of the worst kind may pass through the tests, how many others could there be?

The following arguments can moderate such a pessimistic conclusion. First, we find comparatively better results for the conditional independence tests applied to the direct cover bids sample: 69% of the pairs fail the test. This may be a clue that the conditional independence test performs better to detect bilateral agreements between firms than complete bid-rigging cartels, as in the Ticino case. It is also worth noticing that the test for exchangeability performs better in the cartel period than the conditional independence test. In any case, our results from the Ticino case strongly suggest that failing one econometric test should be sufficient to classify a pair of firms as a potential candidate for further investigation. However, the requirement of failing of both tests seem to be an inappropriate measure to classify a pair as a potential cartel: only 6% of the pairs fail both tests, meaning that 94% of the pairs would not have been classified as being part of a potential bid-rigging cartel. However, if we consider the failure of one test as being sufficient to flag a pair of firms as a potential bid-rigging cartel, then 40% of the pairs would be flagged due to failing one test in the cartel period. It therefore seems more appropriate to apply both econometric tests in future cases and to consider the failure of one test as sufficient to classify a pair as a potential bid-rigging cartel. Moreover, finding more than a third of pairs failing one test should be enough to alarm the competition agencies to undertake a deeper investigation.

Third, the internal organization of the bid-rigging cartel can explain why the cartel manages to pass through the tests. Indeed, cartel members meet every week, and they discuss extensively the bids to submit for each contract, considering criteria such as the capacity of each firm, the distance to the contract location and the specialization of each firm. The criteria used in the convention to allocate contracts among cartel participants also match the cost variables mostly used in the empirical estimation of the bidding function. Therefore, the bids, and especially the cover bids, are certainly connected to the cost variables in the cartel period. In some ways, that allows the bid-rigging cartel to successfully pass through the econometric tests proposed by Bajari.

However, if bids are cleverly fake, why can an alternative method, such as the screening method proposed by *Imhof et al.* (2017), perform better to detect the Ticino case than the econometric tests of *Bajari and Ye* (2003)? For comparison purposes, we apply the *ex ante* screening method based on simple screens proposed by *Imhof et al.* (2017) in the Ticino case. The authors combine two simple screens, the coefficient of variation and the relative distance, to classify contracts as conspicuous or nonconspicuous. We find that the screening method classifies 82% of the contracts in the cartel period as conspicuous. Therefore, the comparison is striking with the econometric tests of *Bajari and Ye* (2003): Simple screens produce significantly fewer negative results in the cartel period, even if the cartel members extensively discuss the cover bids to submit.

Finally, if one competition agency wishes to enforce the law and destabilize bid-rigging cartels, which detection method should it choose in the first place? We believe that both methods may be complementary and not substitutable. The method based on simple screens provides quick information on potential serious bid-rigging issues. It works well to detect bid-rigging cartels, and it seems to produce significantly fewer false negative results for Ticino's case, compared to the econometric tests of *Bajari and Ye* (2003). Moreover, its low data requirement is particularly advantageous to screen a high number of contracts, since it uses solely information about bids and not about costs. Bids are easier to observe than costs, especially if we run the method *ex ante* in secrecy in order to not alarm potential bid-rigging cartels.

The econometric tests proposed by *Bajari and Ye* (2003) focus on the bilateral interaction of each pair of firms. The conditional independence tests perform better in the sample of the direct cover bids, suggesting that the tests detect bilateral agreements. However, such bilateral agreements among firms are not as damaging as a complete cartel. Nevertheless, bilateral agreements affect the outcome of tenders and are, therefore, harmful. However, if we suspect that collusion takes the form of bilateral agreements rather than a complete bid-rigging cartel, as in Ticino's case, we recommend implementing the econometric tests proposed by *Bajari and Ye* (2003).

A problem now arises from the fact that bilateral colluding firms may not collude on all contracts: they may partially collude on selected contracts. It may be a serious problem, especially considering the high number of possible false negative results produced by the econometric tests of *Bajari and Ye* (2003). In such cases, the screening method proposed by *Imhof et al.* (2017) can remedy the problem of partial collusion for selected contracts. Indeed, the authors propose to construct a filter based on different screens to focus solely on conspicuous contracts. If there is some reason to suspect that firms do not collude on all contracts, then the screening method proposed by *Imhof et al.* (2017) may help to screen the contracts that are most likely to be rigged. After the first selection, the investigator can apply the econometric tests of *Bajari and Ye* (2003) to confirm the suspicion of intended bid-rigging activities. Even if not necessary, finding that both methods produce evidence of potential bid rigging would also reinforce the initial suspicion to trigger an investigation ex officio.

2.8 Conclusion

Our paper contributes to the literature regarding detecting bid-rigging cartels in multiple ways. We show that the econometric tests proposed by *Bajari and Ye* (2003) produce a high number of false negative results for the Ticino cartel. Considering the severity of the bid-rigging cartel, we would have expected to detect it with the econometric tests proposed by *Bajari and Ye* (2003). Moreover, bid-scaling phenomenon cannot explain such a result. Therefore, the Ticino cartel finds a way to pass through the tests and to challenge the competitive model used by *Bajari and Ye* (2003).

We also apply both tests on the first subsample composed by indirect cover bids, and we again find too many negative results. However, the conditional independence test produces better results consistent with the existence of the Ticino cartel for the second subsample formed with the direct cover bids. The result suggests that the conditional independence test detects bilateral agreements rather than complete cartels.

We also conclude that the high number of negative results suggests that the failure of one test should be sufficient to classify a pair of firms as a potential bid-rigging cartel. Moreover, if more than a third of the pairs fail at least one test, it should be sufficient to alarm competition agencies to carry out a deeper investigation.

Finally, we compare the econometric tests of *Bajari and Ye* (2003) with the results of the screening method developed by *Imhof et al.* (2017). We find that the screening method performs better to detect the Ticino cartel. We also believe that both methods may be complementary in certain cases.

To summarize, bid-rigging cartels are a serious problem, and competition agencies should be more proactive in order to deter and destabilize them. Detection methods are not the unique instrument to achieve such a goal but are rather one instrument among a set of multiple possible actions. However, in order to use a detection method as a valid instrument, competition agencies must be able to gauge its performance, strengths and limitations. Our paper contributes to address the viability of econometric tests for detecting bid-rigging cartels.

Chapter 3

Simple Statistical Screens to Detect Bid-rigging Cartels

3.1 Introduction

Bid rigging remains a pervasive problem and may concern a major share of economic activities realized through auctions. Generally, price-fixing and bid-rigging cartels inflate prices up to 10-20% (see *OECD*, 2002). *Connor and Lande* (2005) even finds that the median price increases due to collusion are approximatively 25%. In the case studied in this paper, prices fell by 25-30% after the collapse of the bid-rigging cartel. Considering that public procurement accounts for approximately 15% of the gross domestic product (GDP) in OECD countries, the potential damage of bid rigging may be enormous, causing a vast waste of public money from governments. Therefore, the fight against bid rigging is a priority for competition agencies. However, they generally depend on whistle-blowers or leniency applications to launch an investigation (see *OECD*, 2014; *Froeb et al.*, 2014). To enhance the fight against bid-rigging cartels, competition agencies need to develop a proactive approach to prosecuting bid-rigging cartels. They need a simple detection method for broad screening, and the detection method used must produce reliable results in order to trigger an investigation. Does such an instrument capable of detecting bid-rigging cartels even exist?

In this paper, we construct a detection method that fits the needs of competition agencies. The method is simple to replicate, fast to implement, easy for courts to understand, and produces reliable results. Following *Imhof et al.* (2017), we develop theoretical and practical arguments for the implementation of simple statistical screens.

For a bid-rigging cartel, the exchange of information is a prerequisite for coordinating bids in procurement. The coordination of bids is crucial if the cartel wants to control the submitted bids from its members. A cartel that cannot control the bids of its members would have little chance of being successful. We assume that bid coordination through the exchange of information reduces the support for the distribution of the bids. Since bids from the cartel are closer because of the exchange of information, lower variance and greater convergence of the bids occur. Moreover, the difference between the first and the second lowest bids is important in procurement when the price is essential but is not the only criterion in awarding contracts. Therefore, cartel participants maintain a specific difference between the first and the second lowest bids to ensure that the contract is awarded to the firm designated by the cartel. In addition, the differences between losing bids become smaller, as firms do not want to appear too expensive. With such a cover-bidding mechanism, the coordination of bids produces asymmetry in the discrete distribution of the bids.

We use the Ticino bid-rigging cartel to investigate how well screens capture the impact of bid rigging on the discrete distribution of the bids. The Ticino cartel was a well-organized and market-embracing cartel, also called complete cartel. All firms in Ticino participated in the cartel, and all tenders were rigged from January 1999 to March 2005, causing prices to increase by 25% - 30%. Since we have data for the Ticino case, both in the collusive and competitive periods, we can study the performance of simple screens for detecting bid rigging.

We implement the coefficient of variation and the unbiased kurtosis statistic as variance screens to the Ticino case. We find that bids are closer during the cartel period. The coefficient of variation is significantly lower, and the kurtosis statistic is higher. We conclude that exchange of information and bid coordination reduce the distribution of bids. As cover-bidding screens, we implement the difference in percent between the first and the second best bids, the skewness statistic and the relative distance (see also *Imhof et al.*, 2017, for the relative distance). We find that the distribution of the bids becomes more asymmetric, i.e., more negatively skewed in the cartel period. The difference in percent between the first and the second best bids is significantly higher, the skewness statistic is lower, and the relative distance is higher. When distance between the first and second lowest bids matters, bid rigging transforms the distribution of the bids in a more negatively skewed distribution.

From the application of the screens, four periods emerged from our data: the pre-cartel period (from 1995 to 1997), the year 1998, the cartel period (from January 1999 to April 2005) and the post-cartel period (from April 2005 to the end of 2006). The results indicate that the pre-cartel period is more similar to the cartel period than to the post-cartel period or the year 1998 because incomplete cartels characterized the pre-cartel period. This means that firms or a subset of firms rigged a subset of contracts, which partially affected the value of the screens in the direction of those in the cartel period. Contrasting with the cartel and pre-cartel periods, competition among firms characterizes the post-cartel period and year 1998, which is very similar to the post-cartel period. Both periods indicate that firms effectively compete for contracts. By regressing dummy variables for each period on each screen, we find that the effect of each periods is significant, conditional on control variables. Moreover, we estimate the effect of bid rigging on each screen so that we can derive thresholds or benchmarks for future cases. As the Ticino cartel is one of the severest cartels known in Switzerland, we suggest considering the estimated effects of bid rigging as conservative thresholds or benchmarks.

Finally, repeated bid coordination may produce a specific bidding pattern because of cover bids and the possible rotational element due to contract allocation within the cartel. The bid rotation screen proposed by *Imhof et al.* (2017) can detect such a specific colluding pattern. Unlike the previous screens, this screen does not characterize the discrete distribution of the bids in a tender but focuses on the interaction of one firm with another or the interaction of one firm within a group of firms. We find in the Ticino case that repeated coordination of bids within the cartel participants strongly affects the distribution of the bids. Furthermore, the depicted interactions between firms suggest that the bid-rigging cartel operates in a rotation pattern through contract allocation. When contrasted with the cartel period, our results clearly indicate a radical change for the post-cartel period, and the behavior of firms fits the hypothesis of competition predicted by the screen. Therefore, the results for the Ticino case support the use of the bid rotation screen as proposed by *Imhof et al.* (2017).

All simple screens used in this paper are based on simple assumptions. For the cartel period, they show the impact of intended bid rigging on the distribution of the bids, as theoretical and empirical arguments predict it. We illustrate how simple screens function to detect bid-rigging cartels in a large dataset by using only information on bids. Moreover, since their implementation is uncomplicated, we show that simple screens are an appropriate instrument for competition agencies.

The next section reviews the literature on screening methods. Section 3.3 presents the theory on simple screens. Section 3.4 implements the screens on the Ticino case. Section 3.5 illustrates the application of the bid rotation screen. Section 3.6 discusses policy recommendations for competition agencies. Section 3.7 concludes the paper.

3.2 Literature on Screening Methods

The literature divides screening methods in two types: structural and behavioral methods (see *Harrington*, 2006; *OECD*, 2014; *Froeb et al.*, 2014). Structural methods list the factors that influence the likelihood of collusion. There are three categories of factors, as follows: structural factors, such as the number of competitors, market transparency or entry barriers; supply-side factors, such as homogeneous products, similar costs between competitors or poor innovation in the market; and demand-side factors, such as the demand fluctuations, strong buying power, demand elasticity or growing demand.

Unlike structural screens, behavioral methods aim to detect cartel by analyzing the behavior of firms in markets. Generally, behavioral screens use prices to study the behavior of firms, but other variables, such as quantities, market shares or firm investments can serve to study whether or not firms behave in a competitive way. However, many behavioral screens focus on the pricing strategy of firms, which is the simplest variable to analyze how firms behave. We mainly divide behavioral screens in two categories, i.e., complex and simple methods. For example, the econometrics of structural auction models or the estimation of ARCH and GARCH models for price series are typical complex methods. Regarding simple methods, *Harrington* (2006) proposes a list of simple screens for price and quantity, such as strategic variables. Generally, higher prices and lower variance are the simple screens that are most often used in the literature (see *Harrington*, 2006; *Jimenez and Perdiguero*, 2012; *OECD*, 2014).

One can apply both complex and simple screens for detecting price-fixing and bid-rigging conspiracies. However, many papers implement econometric methods to detect bid-rigging cartels (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Chotibhongs and Arditi*, 2012a,b; *Imhof*, 2017b), which is in contrast with the unique paper of *Feinstein et al.* (1985) that applies simple screens to detect bid-rigging cartels. The present paper and *Imhof et al.* (2017) fill this gap by building a detection method based on simple screens to uncover bid-rigging cartels.

In the following literature review on screening methods, we also differentiate between *ex ante* and *ex post* analyses. An *ex ante* analysis means that we screen markets without information about collusion or bid rigging. *Imhof et al.* (2017) is one example of such *ex ante* analysis. In contrast, an *ex post* analysis refers to a situation where information about collusion or bid rigging is available, and the researcher can discriminate between competition and collusion. The distinction between competition and collusion is necessary to evaluate the performance of any screen or any detection method. This paper is one example of such an *ex post* analysis.

3.2.1 Behavioral screens

Price-fixing cartels

Some papers show *ex post*, i.e., after the detection of the cartel, the impact of collusion using the variance screen. *Abrantes-Metz et al.* (2006) analyze the movements of prices for the sale of frozen seafood to the Defense Personnel and Support Center (DPSC) in Philadelphia. After the breakdown of the cartel, they observe that the simple mean of prices decreases by 16%, whereas the standard deviation increases by over 200%. *Esposito and Ferrero* (2006) analyze the Italian gasoline and baby food markets using the simple mean and the standard deviation for prices. Again, they find that prices are higher and variance lower, when firms collude. More complex methods, such as econometric analysis of price series, are also implemented for price-fixing cartels. *Bolotova et al.* (2008) demonstrate the impact of the lysine cartel and the citric acid cartel by analyzing the price evolution with an ARCH and GARCH model.

Very few papers try to identify *ex ante* possible price-fixing cartels where no prior information about collusion is available. *Abrantes-Metz et al.* (2012) show possible evidence of Libor manipulation using different indicators. One of them is the coefficient of variation calculated with the daily quote of banks. The authors conclude that a sudden increase in the variance may be indicative of an anomalous outcome for the period prior that sudden increases. *Jimenez and Perdiguero* (2012) propose a thorough review of empirical papers using the variance screen. With the coefficient of variation and the average price for each gas station, they analyze the retail gasoline market in the Canary Islands. Because they have no information on collusion, they rely on two benchmarks, i.e., a monopoly firm located on two islands and an independent firm acting more aggressively on the gasoline market. First, they show the negative impact of independent gas stations on prices and on a rigid pricing structure. They find that prices are lower and the variance is higher in the presence of an independent gas station. Second, firms in an oligopoly situation behave very closely to the monopoly situation, which indicates potential competitive issues. Their empirical analysis contributes to illustrating the relationship between price rigidity and market structure, i.e., we should consider it when applying the variance screen.

Bid-rigging cartels

The detection of bid-rigging cartels generally relies on structural econometrics of auction models. Some papers illustrate the impact of bid rigging *ex post*, as follows: *Porter and Zona* (1993) analyze the rank of the bids with a multinomial logit model. They show that the ranks of cover bids are not related to the control variables, such as the distance or the capacity of a firm. However, they find the opposite results for the non-cartel firms, whose bids are, in contrast. related to the control variables. In addition, *Porter and Zona* (1999) analyze the milk school market and they find that collusive bidders bid lower in more distant contract locations than in nearer contract locations. They argue that this result does not fit a competitive bidding behavior. *Pesendorfer* (2000) also uses the data from the school milk market and emphasizes the difference between a strong and a weak cartel.[1] He demonstrates that weak cartels can achieve the first-best collusive gain if there are many contracts to allocate among the cartel participants. Using the statistics order property, he shows that non-cartel bids stochastically dominate cartel bids. Then, he estimates the bidding function, and he validates the theoretical prediction that the residuals of non-cartel bidders stochastically dominate the residuals of the cartel members.

Bajari and Ye (2003) formalize a method to detect bid-rigging cartels *ex ante* with no prior information, using auction theory developments, especially in a first-price sealed-bid auction with asymmetric bidders (see *Lebrun*, 1996, 2002; *Maskin and Riley*, 2000a,b). They propose two econometric tests, namely, the conditional independence test and the test for the exchangeability of bids. The test for the conditional independence of the bids checks if the residuals between firms are correlated. A contemporaneous correlation between firms could indicate collusive issues since the competitive model predicts that bids should be independent conditional on a set of covariates. The second test postulates that if the covariates are permuted among firms, then their bids should also be permuted, that is, the bids are exchangeable conditional on the set of covariates. In other words, the control variables used in the regression as covariates should enter into the bidding function symmetrically for each bidder. They apply the two tests and find that three firms may be colluding in two potential bid-rigging cartels.

Jakobsson (2007) applies the conditional independence test on a Swedish database using the spearman rank correlation test and finds significant correlation for 50% of the pairs of firms. *Chotibhongs and Arditi* (2012a,b) implement the two econometric tests, and show evidence of collusion for a group of six firms. Three of these six firms were involved in bid-rigging cases or bid frauds. Related to the econometric estimation of the bidding functions, *Ishii* (2009) uses a conditional logit model to explain the intern functioning of a bid-rigging cartel in Osaka. He validates that the cartel operates in a simple bid rotation scheme, which allocates contracts among cartel participants based on a simple rule, as follows: the number of days of no winning determines the cartel participant to whom the cartel allocates the contract.

Some papers use structural estimation of the auction model to analyze bid rigging. *Baldwin et al.* (1997) construct a competitive model and a collusive structural model and apply them to oral-timber auction data. They find that the collusive model outperforms the competitive model. *Banerji and Meenakshi* (2004) also find that a collusive model explains the data better than a competitive model when applied to oral ascending auctions for rice. *Aryal and Gabrielli* (2013) combine both

[1] A strong cartel operates with side-payment whereas a weak cartel functions without side-payment (see *McAfee and McMillan*, 1992).

econometric and structural estimations of auction models to detect bid-rigging cartels in an *ex ante* analysis. They suggest that cost under collusion must stochastically dominate cost under competition but find no conclusive results.

As we can see, the detection methods for bid-rigging cartels extensively use econometric or structural estimations. However, if we look for simple methods to detect bid-rigging cartels, we find very few papers. *Feinstein et al.* (1985) develop a model of collusive behavior in a multi-period auction market where purchasers are asymmetrically misinformed by bidders. They test their model on the highway construction cartels of North Carolina and find that the coefficient of variation is lower when bidders collude, and colluding bidders submit higher bids. They also find that collusion is characterized by frequent and repeated interactions of the same group of bidders. Finally, this paper and *Imhof et al.* (2017) are in the specific segment of the screening literature for detecting bid-rigging cartels with simple screens.

Structural screens

Many theoretical papers discuss structural screens, which identify market characteristics favoring collusion. Generally, researchers use a Cournot or a Bertrand model in a context of a supergame or repeated interactions to study tacit collusion (see *Shapiro*, 1989, for a theory of oligopolies). Factors that favor tacit collusion may also facilitate explicit collusion. However, any screen developed in a context of tacit collusion generally produces too many false positive results, for example, if only a few firms are active in an industry with high entry barriers, it does not mean that they are necessary colluding. On the other hand, structural screens produce very few false negative results, i.e., if a high number of firms are active in an industry with no entry barriers, the likelihood of collusion is very low in such a case. Structural screens may therefore help to exclude industries for deeper investigations and to suggest industries as suitable candidates for applying behavioral screens. Both types of screens are complementary, so competition agencies should closely investigate industries flagged by both structural and behavioral screens. Furthermore, structural screens can test for the consistency of the results obtained with the behavioral screens.

OECD (2014) divides the factors that more likely render collusion into the three following groups: structural, supply-related and demand-related factors. Among all factors, concentration in a peculiar industry increases the likelihood of collusion (see *Tirole*, 1988; *Bain*, 1956). Empirical studies also confirm this theoretical prediction. *Fraas and Greer* (1977) analyze more than 600 cases and prove that few firms participate in a majority of the cartels examined. Related to concentration, entry barriers, high degree of interaction among firms and market transparency enhance the likelihood of a collusive outcome (see *Stigler*, 1964; *Green and Porter*, 1984; *Snyder*, 1996). *Stigler* (1964) show that transparency allows for immediate retaliation in the case of deviation and therefore favors a tacit cooperative outcome.

Concerning supply-related factors, production capacities have an ambiguous effect. Using a Bertrand supergame with an exogenous capacity constraint, *Brock and Scheinkman* (1985) show that the minimum discount factor supporting tacit collusion depends non-monotonically on firm capacity. When the total capacity of all firms is slightly below the monopoly outcome, the severity of the punishment exceeds the deviation gains from collusion, so firms continue to collude. In contrast, when the total capacity of all firms increases, the harshness of punishment diminishes, and collusive equilibria are sustainable until the gains from one period deviation outweighs the punishment effect. *Compte et al.* (2003) also use a Bertrand supergame with exogenous capacity and find that asymmetry in capacity may have a pro-competitive effect even if the market is concentrated. Compared to a situation where firms are symmetric, large firms have an incentive to cheat because short-term gains are superior considering the limited capacity of small firms to retaliate. *Benoit and Krishna* (1987) and *Davidson and Deneckere* (1990) endogenize the firm capacity in a model with a capacity choice game followed by a price supergame. Both papers find that collusion implies capacities in excess to punish deviation from a collusive outcome. Furthermore, *Davidson and Deneckere* (1990) show that any increase in the collusive price is paired with a higher level of capacity.

A multimarket contact may support collusion because punishment for deviation from a collusive equilibrium affects all the different markets. *Bernheim and Whinston* (1990) show that multimarket contacts enhance the probability of collusion if markets and firms are asymmetric. In case of symmetry, multimarket contacts do not, however, influence incentives for colluding. *Gilo et al.* (2006) demonstrate that cross-shareholding in competitive firms may favor the emergence of a cooperative outcome. Antitrust practitioners also consider product homogeneity as a characteristic that supports collusion. However, the theoretical results remain ambiguous (see *Ross*, 1992). Conversely, *Hay and Kelley* (1974) analyze previous antitrust cases and show that products are relatively homogeneous in most cases of collusive agreements. In addition, collusion is more likely in mature industries with poor innovation rates.

Concerning the demand side, growing demand or stable demand might favor collusion and price wars appear to be more frequent in a period of recession. *Green and Porter* (1984) suggested that lower demand triggers price wars unless those observed sharp price drops are a self-enforcement policy used by the cartel. In contrast, *Rotemberg and Saloner* (1986) show that collusion is more profitable when demand is low because punishment is tougher than when demand is high. Empirically, *Suslow* (1991) addresses that question and finds that recession and economic depression increase the probability of collapse for a cartel.

The power of a buyer may also hinder collusion: *Snyder* (1996) demonstrates that a buyer can reduce the likelihood of collusion if he groups his purchases in large and less frequent orders. *Pesendorfer* (2000) also concluded that large contracts are better than small and medium sized contracts, because it impedes firms to reach sustainable agreements without side-payment through contract allocation to each cartel participant.

Grout and Sonderegger (2005) empirically investigate the relevance of structural screens. They use disaggregated data for industries classified by three digits, and they estimate logit and ordered logit models to investigate the relationship between uncovered cartels as the endogenous variable and structural variables favoring collusion as the exogenous variables. They find that growing demand positively affects the likelihood of collusion. In contrast, a demand fluctuation has a negative impact on collusion.

3.3 Detection Strategy with Simple Screens

3.3.1 Variance screen

Many of the empirical and theoretical papers discussed in section 3.2 indicate that price rigidity may underline competitive issues. The variance screen is an appropriate tool to capture such price rigidity by using simple statistics as the standard deviation or the coefficient of variation. In a context of bid rigging, the use of the coefficient of variation is advantageous because it is scale invariant: we can implement it to compare and to characterize tenders of different values. *Feinstein et al.* (1985) and *Imhof et al.* (2017) find that lower values for the coefficient of variation indicate bid-rigging conspiracies.

The coefficient of variation CV_t is calculated for each tender t as the standard deviation σ_t divided by the arithmetic mean μ_t:

$$CV_t = \frac{\sigma_t}{\mu_t} \tag{3.1}$$

The mean μ in a tender t necessarily increases because cartel participants submit higher bids to raise their profit. Therefore, the evolution of σ determines the effect on the coefficient of variation. We assume that σ decreases because the bid coordination and exchange of information reduce the support of the distribution of the bids in the case of bid rigging.

Assumption 1: *In case of bid rigging, the variance of the bids decreases in a tender.*

Let the distribution of the bids $G(b)$ and its probability density function $g(b)$ continuously differentiable in b with the following support: $[\underline{b},\ \bar{b}]$. Let us further assume that the procurement authority has information on the $G(b)$, i.e., it cannot directly depict $G(b)$ but can approximate it with its support.

If firms collude, they must exchange information to coordinate their bids. For example, a basic exchange of information could specify that firms should bid over a certain value of a. This may occur in a brief meeting or by call, phone message, fax or emails with a simple message such as "bid over a." In the case of bid rigging, a is necessarily greater than $\underline{b}$, since the cartel participants primarily want to raise their profit. Moreover, cartel participants are also aware that the procurement authority has information about $G(b)$ because it hires engineers and regularly organizes tenders. Firms cannot choose fancy values for a, which should therefore be less than $\bar{b}$. In summary, we reasonably assume that $\underline{b} < a < \bar{b}$ and that a truncates the distribution of the bids $G(b)$. We denote the truncated distribution of the bids $\tilde{G}(b)$ with its probability function $\tilde{g}(b)$ and its support $[a, \bar{b}]$ where $a > \underline{b}$. The reduction of the support due to the truncation point a automatically decreases the standard deviation σ for $\tilde{G}(b)$. Therefore, we postulate in proposition 1 that the coefficient of variation for $\tilde{G}(b)$ is lower than the coefficient of variation for $G(b)$

Proposition 1: *Let $G(b)$ be the normal cumulative distribution of the bids with the distribution support $[\underline{b},\ \bar{b}]$ and $\tilde{G}(b)$ are the normal truncated cumulative distribution of the bids with the distribution support $[a,\ \bar{b}]$ where $a > \underline{b}$. The coefficient of variation of $\tilde{G}(b)$ is lower than the coefficient of variation of $G(b)$.*

Proof. See appendix.

If proposition 1 is true, the following equation holds:

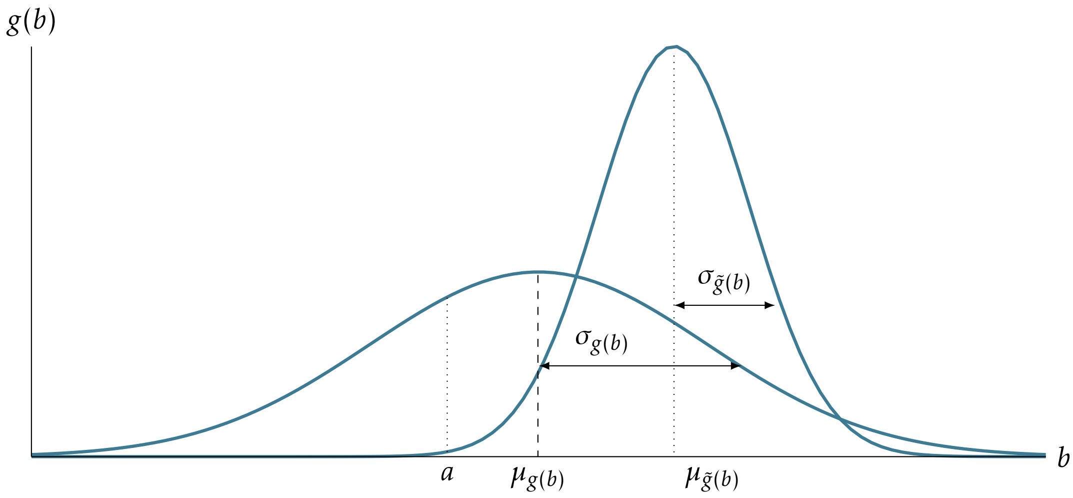
$$CV_{G(b)} = \frac{\sigma_{G(b)}}{\mu_{G(b)}} > \frac{\sigma_{\tilde{G}(b)}}{\mu_{\tilde{G}(b)}} = CV_{\tilde{G}(b)} \tag{3.2}$$

In summary, bid-rigging cartel participants need to coordinate to increase their bids and raise their profit. Bid coordination implies explicit exchange of information on prices and truncates the distribution of the bids, since cartel participants cannot exaggerate their bids. Bid coordination therefore reduces both the support for the distribution of the bids and the coefficient of variation.

The truncation of the distribution of the bids is also followed by a density reallocation of the bids because cartel participants do not automatically renounce submitting a bid, even if their bids would have been smaller than a in a competitive situation.[2] If the cartel participants with bids smaller than a submit a bid higher than the value a, then it reshapes the distribution of the bids as depicted in the graphic 3.1. The truncated distribution of the bids $\tilde{G}(b)$ becomes sharper than $G(b)$ since bids

[2]In the Ticino case, the convention requires firms to submit a bid in public tender (see section 1.2).

Figure 3.1: The untruncated and truncated distributions



converge. Note that the result from equation 3.2 still holds, because the support for the distribution of the bids remains reduced. We check the convergence of the bids with the following unbiased kurtosis statistic[3] for each tender t:

$$Kurt(b_t) = \frac{n(n+1)}{(n-1)(n-2)(n-3)} \sum_{i=1}^{n} (\frac{b_{it} - \mu_t}{\sigma_t})^4 - \frac{3(n-1)^3}{(n-2)(n-3)} \tag{3.3}$$

If the exchange of information transforms the distribution of the bids in a sharper distribution, we expect higher values for the kurtosis statistics, which shows the convergence of the bids during the cartel period.

Assumption 2: *In case of bid rigging, bids converge, and the distribution of the bids becomes sharper.*

We have implicitly assumed that cartel participants do not cleverly scale their bids following *Bajari and Ye* (2003). Bid scaling would not reduce the support of the distribution of the bids, and it would produce the effect of a geometrical translation, preserving more or less the properties of the function $G(b)$. Hence, if firms cleverly scale their bids with a common factor according to their true costs, it would be impossible to detect bid rigging with the variance screen, as stated in proposition 2.

Proposition 2: *If all firms collude in a specific tender t and scale their bids b_i with a common factor a, the coefficient of variation remains unchanged.*

The trivial proof of proposition 2 is included in the Appendix. In addition, proposition 2 holds not only for the coefficient of variation but also for all simple screens presented in the paper. However, if bid scaling is theoretically possible, it is limited in practice because procurement authorities

[3]The unbiased skewness statistic is calculated for each tender with a number of bids superior to 3.

have some knowledge of $G(b)$ and its support. Bids above the estimated $\bar{b}$ from the procurement authorities would raise concern about bid rigging or bid frauds. If cartel participants want to simultaneously raise a and not exceed $\bar{b}$ to increase their rents and not exaggerate prices, bid scaling is limited and bid coordination still reduces the support for the distribution of the bids; therefore, the variance decreases, and bids converge.

3.3.2 Cover-bidding screen

Difference in the first and second lowest bids in a tender

A bid-rigging cartel, who controls the bids of the cartel participants, artificially manipulates the difference between the bids in a tender, especially between the first and the second lowest bids. This is crucial because a bid-rigging cartel without monetary transfer can solely function if contract allocation within the cartel participants is successful (see *Pesendorfer*, 2000). To ensure that the designated winner from the cartel actually wins the tender, the cartel maintains a certain distance between the first and the second lowest bids. In other words, cartel participants, who submit artificially higher bids, cover the designated winner from the cartel.

The distance to place between the first and the second lowest bids depends on the context. The data used in the paper are characterized by a first-price sealed-bid auction, where the price is an essential criterion, but not the only criterion in the awarding procedure of contracts. Procurement authorities regard other criteria, such as work timing, organization, references, quality and environmental aspects, in the awarding process. Bid coordination must therefore consider the non-price competition fixing the distance between the first and the second lowest bids to warrant that the designated firm from the cartel wins the contract. COMCO observation confirms the prediction. Witnesses in bid-rigging cases have reported that firms from bid-rigging cartels regularly put a cover distance of 3-5% between the first and the second lowest bids submitted from the cartel. [4]

Assumption 3: *In the case of bid rigging, the difference in the first and the second lowest bids increases.*

To capture such a cover-bidding mechanism, we calculate the percentage difference between the first and the second lowest bids and we check if it increases in the cartel period. Any important and non-temporary evolution could indicate bid-rigging issues.

[4] For example, see Strassenbeläge Tessin (LPC 2008-1, pp. 85-112, in particular recital 60) or Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich (LPC 2013-4, pp. 524-652, in particular p. 561, recital 182 and p. 573, recital 309 and 314).

Differences in the losing bids

Several practical reasons explain why the differences in the losing bids decrease. First, firms do not want to appear too expensive, since a firm submitting bids too high may give a negative signal to procurement authorities. The potential reputation costs associated with bids that are too high encourage firms to submit similar cover bids, which therefore reduces the differences in the cover bids. Second, cover bids are close to replicate a fictive competition process, i.e., (losing) firms hardly compete for the contract and a firm (the designated firm by the cartel) submits only a slight better bid. Third, calculating bids is time-consuming and firms submitting cover bids thus have no interest in investing time to calculate an accurate bid, and focus on the bid of the designated winner by the cartel for calculating the cover bids. Since all firms who submit cover bids behave in the same way, i.e., focusing on the bid of the designated winner by the cartel, then the cover bids may be close to each other. All these practical reasons suggest that the differences in the losing bids decrease in the case of bid rigging.

Assumption 4: *In the case of bid rigging, differences in the losing bids decrease.*

Smaller differences in the cover bids influence the distribution of the bids and transform it in a (more) negatively skewed distribution, as depicted in figure 3.2. We directly calculate the skewness for the discrete distribution of the bids for each tender t to check assumption 4 with the following unbiased skewness statistic[5]:

$$Skew(b_t) = \frac{n}{(n-1)(n-2)} \sum_{i=1}^{n} \left(\frac{b_{it} - \mu_t}{\sigma_t}\right)^3 \tag{3.4}$$

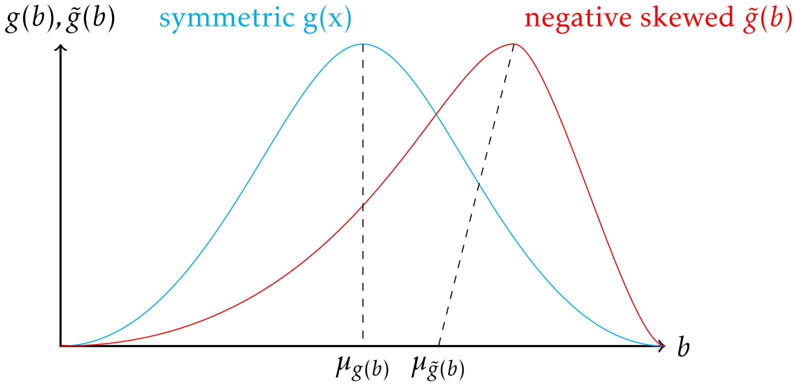
Note that assumption 3 also reinforces the skewness in the distribution of the bids. If the differences in the cover bids are small, and if the differences in the first and the second lowest bids are important, then the skewness will be more striking. Therefore, we expect to find a more negatively skewed distribution of the bids in the cartel period.

If we combine assumptions 3 and 4, we can precisely build a screen to check for tenders, where the difference between the first and second lowest bids is important and the difference between the cover bids is small. *Imhof et al.* (2017) propose using the relative distance to capture such a cover-bidding mechanism. The relative distance divides the difference in the first and second lowest bids $\Delta_{1t} = b_{2t} - b_{1t}$ by the standard deviation of the losing bids $\sigma_{t,losingbids}$.

$$RD_t = \frac{\Delta_{1t}}{\sigma_{t,losingbids}} \tag{3.5}$$

[5]The unbiased skewness statistic is calculated for each tender with a number of bidders equal to or superior to 2.

Figure 3.2: The normal and skewed distributions



Formula 3.5 normalizes the difference in the first and second lowest bids by the standard deviation of the losing bids. If the RD is superior to 1, it indicates that the difference in the first and second lowest bids exceeds the differences in the losing bids. In this case, the distribution of the bids is negatively skewed. If the ratio of the relative distance is equal to 1, there is no significant differences in the distribution of the bids. However, if the RD is less than 1, it indicates that the difference in the first and second lowest bids is small, and the second lowest bid could be a credible alternative for the procurement authorities. For the cartel period, we expect to find values for the RD above 1 and values under 1 for the post-cartel period.

3.4 Empirical results for the screens

3.4.1 Variance screen

The graphic 3.3 depicts the evolution for the coefficient of variation and each point on the graphic represents the value of the coefficient of variation for a specific tender. The two vertical lines on graphic 3.3 delimit the cartel period between 1999 and April 2005, and the coefficient of variation is clearly lower in the cartel period than in the years 2005, 2006 and 1998. Exactly at the beginning of the cartel in January 1999, the coefficient of variation fell, and it abruptly increased at the end of the cartel in March 2005. The match between the cartel period and the abrupt evolution of the coefficient of variation is perfect and bid rigging negatively affects the coefficient of variation. The median and the mean of the coefficient of variation in the cartel period are 3.1 and 3.4, respectively (see table 5.2). For the post-cartel period[6], the median and the mean of the coefficient of variation increase to 8.1 and 8.9, respectively. The Mann-Whitney test and the Kolmogorov-Smirnov test both reject the null hypothesis of no difference in the coefficient of variation between the cartel period and the post-cartel period (see table 3.1).

[6]The post-cartel period starts in April 2005.

Higher values for the coefficient of variation in 1998, which precedes the cartel period, also confirm the allegations of the defendants, according to whom the firms would have entered into a price war during the mid-nineties. Neither the Mann-Whitney test nor the Kolmogorov-Smirnov test reject the null hypothesis of no difference in the coefficient of variation between the post-cartel period and the year 1998. Thus, the values of the coefficients of variation in the year 1998 are quite similar to those in the post cartel period (see table 5.2). If high values for the coefficient of variation indicate competition, it means that competition should have characterized both periods.

The Mann-Whitney test and the Kolmogorov-Smirnov test both reject the null hypothesis of no difference in the coefficient of variation between the post-cartel period and the pre-cartel period (all years before 1998) and between the cartel period and the pre-cartel period. The coefficient of variation in the pre-cartel period is lower than in the post-cartel period but is higher than in the cartel period, although the coefficient of variation in the pre-cartel period is closer to the cartel period than to the post-cartel period, as illustrated by the median and the mean in the pre-cartel period, as shown in table 5.2. Therefore, the bidding behavior of the firms in the pre-cartel period is in the middle of the post-cartel period and the cartel period. It is then likely that incomplete cartels have characterized the pre-cartel period, in which firms, or a subset of firms, solely colluded for a subset of contracts. In summary, four periods emerge from the analysis of graphic 3.3 as follows: the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the year 1998 and the pre-cartel period (from 1995 to 1997).

Table 3.1: Statistical tests for the coefficient of variation

cvbid	z-statistic	p-value MW	KSa	p-value KS
cartel vs post-cartel	6.43	$<.0001$	3.78	$<.0001$
cartel vs year98	7.10	$<.0001$	4.06	$<.0001$
cartel vs pre-cartel	3.38	0.0007	2.08	0.0032
year98 vs post-cartel	0.85	0.3965	0.75	0.6277
pre-cartel vs post-cartel	4.26	$<.0001$	2.47	$<.0001$
pre-cartel vs year98	5.12	$<.0001$	2.98	$<.0001$

Note: ”cvbid”, ”z-statistic”, ”p-value MW” denote the coefficient of variation, the z-statistic of the Mann-Whitney test and the p-value of the Mann-Whitney test, respectively. ”KSa” and ”p-value KS” denote the asymptotic Kolmogorov-Smirnov statistic and the p-value of the Kolmogorov-Smirnov test, respectively. ”cartel”, ”post-cartel”, ”pre-cartel” and ”year98” denote the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, respectively.

The graphic 3.4 shows the evolution of the kurtosis statistic. The values are higher in the cartel period compared to those in the post-cartel period, as attested in table 5.2. Bid rigging therefore affects the distribution of the bids, which becomes more concentrated in the cartel period, showing the convergence of bids. In other words, the distribution of the bids is more leptokurtic in the cartel

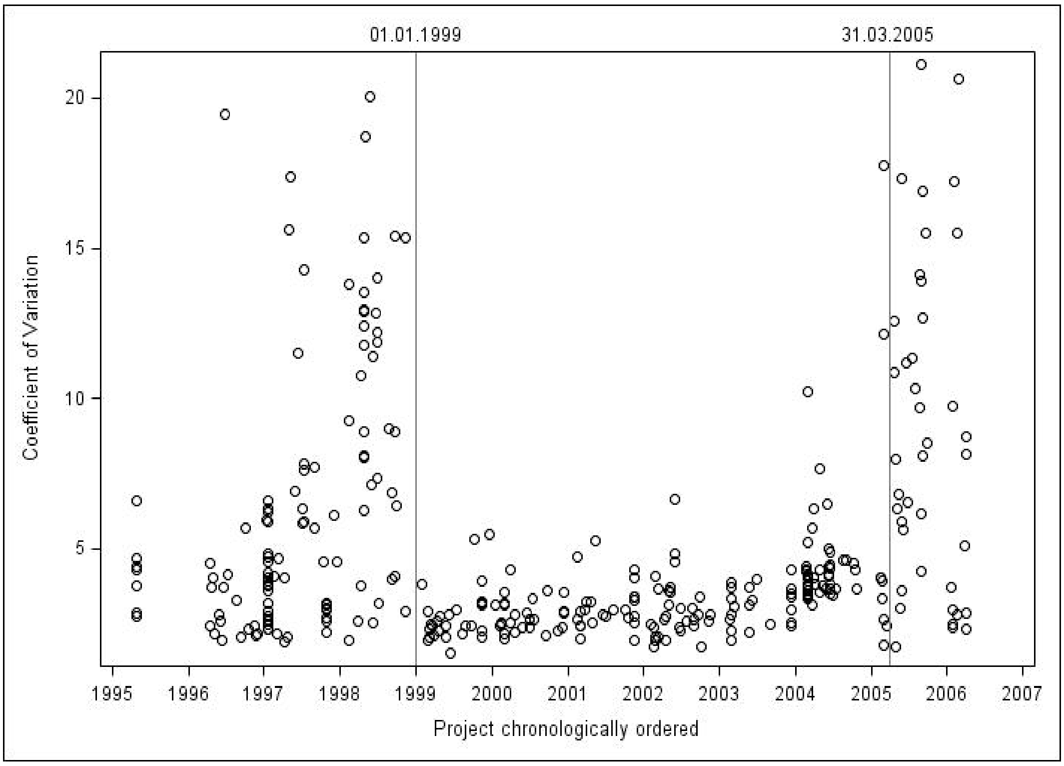


Figure 3.3: The evolution of the coefficient of variation

period, contrasting with a mesokurtic or even platykurtic distribution of the bids in the post-cartel period. Table 5.2 indicates negative values or values near zero for the median and the mean of the kurtosis statistic.

The Mann-Whitney and the Kolmogorov-Smirnov tests confirm that the cartel period significantly differs from the post-cartel period and the year 1998 (see table 3.2). However, the results are unclear for the pre-cartel period. The Mann-Whitney test indicates no difference between the cartel and the pre-cartel periods; however, the Kolmogorov-Smirnov test rejects the hypothesis of no difference between both periods. This result indicates that the difference between both periods are to be found in the spread of both distributions, rather than in the central tendency. Again, the post-cartel does not significantly differ from 1998. Both periods, however, significantly differ from the pre-cartel period. The kurtosis statistic in the pre-cartel period indicates results between those of the cartel and post-cartel periods in table 5.2; however, the non-rejection of the Mann-Whitney test indicates that the pre-cartel period is closer to the cartel period than the post-cartel period or the year 1998. It again supports the evidence of incomplete cartels for the pre-cartel period.

3.4.2 Cover-bidding Screen

Graphic 3.5 depicts the evolution for the percentage difference in the first and second lowest bids. During the cartel period, many observations exhibit an approximate percentage difference of 5% and

Table 3.2: Statistical tests for the kurtosis statistic

kurto	z-statistic	p-value MW	KSa	p-value KS
cartel vs post-cartel	-6.41	< .0001	3.23	< .0001
cartel vs year98	-4.92	< .0001	2.62	< .0001
cartel vs pre-cartel	-1.20	0.2314	1.37	0.0475
year98 vs post-cartel	-0.53	0.5977	1.05	0.2178
pre-cartel vs post-cartel	-4.59	< .0001	2.27	< .0001
pre-cartel vs year98	-3.60	0.0003	1.79	0.0034

Note: "kurto", "z-statistic", "p-value MW" denote the kurtosis statistic, the z-statistic of the Mann-Whitney test and the p-value of the Mann-Whitney test, respectively. "KSa" and "p-value KS" denote the asymptotic Kolmogorov-Smirnov statistic and the p-value of the Kolmogorov-Smirnov test, respectively. "cartel", "post-cartel", "pre-cartel" and "year98" denote the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, respectively.

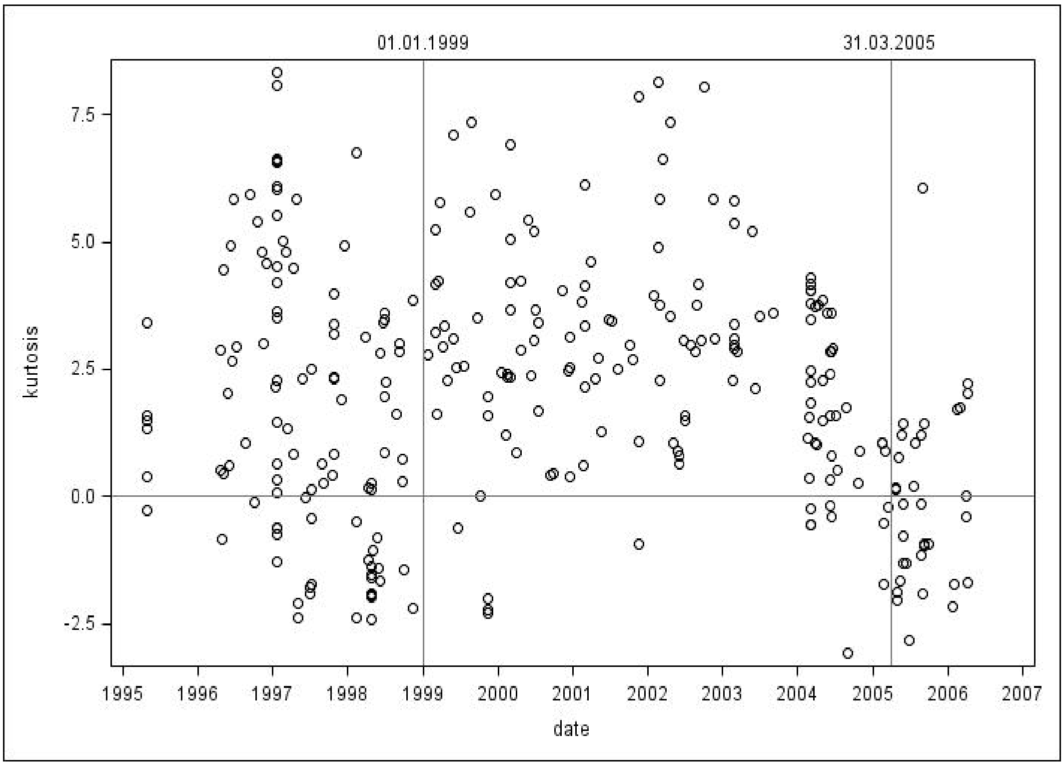


Figure 3.4: The evolution of the kurtosis statistic

very few observations are below 2.5%. Contrasting with the cartel period, the percentage difference substantially decreases in the post-cartel period. Nevertheless, we still find five observations in the post-cartel period above the level of 5%. We explain this high percentage difference by the large cut in prices after the collapse of the cartel, and not by the existence of bid-rigging conspiracies. [7] Similar to the post-cartel period, we again find many observations under 2.5% in 1998, i.e., before the cartel onset.

Moreover, the statistical tests shown in table 3.3 indicate that the cartel period significantly differs from the year 1998 and the post-cartel period. However, we find no rejection of the hypothesis of no difference between the pre-cartel period and all other period. Moreover, the year 1998 and the post-cartel period do not significantly differ. In summary, the statistical tests clearly reject the null hypothesis of no difference solely when comparing competitive and collusive periods. Nevertheless, the results confirm that the bid-rigging cartel artificially manipulates the difference in the first and the second lowest bids.

Table 3.3: Statistical tests for the percentage difference in the first and second lowest bids

diffperc	z-statistic	p-value MW	KSa	p-value KS
cartel vs post-cartel	-2.78	0.0054	1.78	0.0036
cartel vs year98	-2.58	0.0100	1.81	0.0028
cartel vs pre-cartel	-0.94	0.3486	1.36	0.0501
year98 vs post-cartel	-0.09	0.9295	0.41	0.9959
pre-cartel vs post-cartel	-1.38	0.1667	1.28	0.0767
pre-cartel vs year98	-1.27	0.2043	1.27	0.0789

Note: ”diffperc”, ”z-statistic”, ”p-value MW” denote the percentage difference in the first and second lowest bids, the z-statistic of the Mann-Whitney test and the p-value of the Mann-Whitney test, respectively. ”KSa” and ”p-value KS” denote the asymptotic Kolmogorov-Smirnov statistic and the p-value of the Kolmogorov-Smirnov test, respectively. ”cartel”, ”post-cartel”, ”pre-cartel” and ”year98” denote the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, respectively.

Graphic 3.6 illustrates the evolution of skewness calculated with an unbiased estimator for each tender t. The skewness statistic analyzes the difference in all the bids in a tender, and not just the difference in the first and second lowest bids, as the percentage difference presented above. In the cartel period, the skewness statistic is negative and the distribution of bids is skewed to the left side. However, we find for the post-cartel period a more centered or even positively skewed distribution of bids with near zero or positive values.

In addition, the statistical tests shown in table 3.4 indicate that the distribution of the skewness statistic significantly differs between the cartel period and year 1998 or the post-cartel period. The pre-cartel period also significantly differs from 1998 and the post-cartel period. However, the statis-

[7] In section 1.2, we show that since April 2005, prices fell approximatively 25-30% lower than prices in the cartel period.

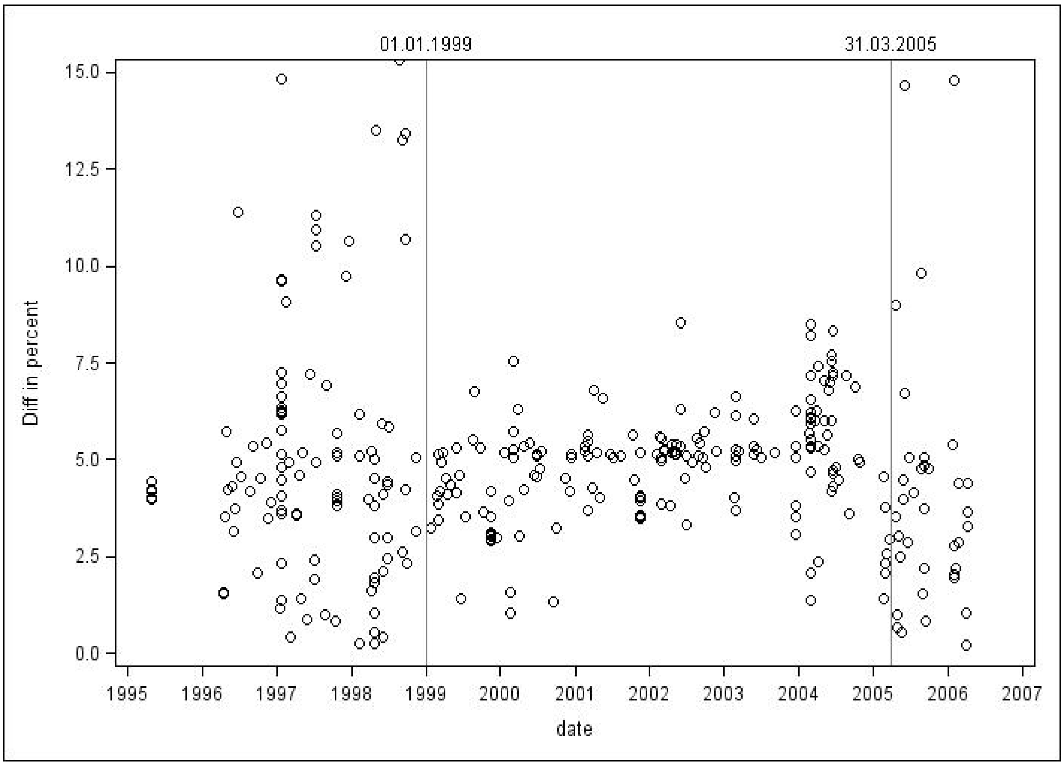


Figure 3.5: The evolution of the difference between the first and second lowest bid

tical tests do not indicate differences between the cartel and the pre-cartel periods. The results again support the evidence of incomplete cartels in the pre-cartel period. Unlike the other screens, we find a significant difference between the year 1998 and the post-cartel period.

Table 3.4: Statistical tests for the skewness statistic

skew	z-statistic	p-value MW	KSa	p-value KS
cartel vs post-cartel	6.64	< .0001	3.14	< .0001
cartel vs year98	3.60	0.0003	1.83	0.0024
cartel vs pre-cartel	-0.64	0.5209	0.84	0.4888
year98 vs post-cartel	-2.69	0.0072	1.63	0.0096
pre-cartel vs post-cartel	5.90	< .0001	3.02	< .0001
pre-cartel vs year98	3.40	0.0007	1.86	0.0019

Note: ”skew”, ”z-statistic”, ”p-value MW” denote the skewness statistic, the z-statistic of the Mann-Whitney test and the p-value of the Mann-Whitney test, respectively. ”KSa” and ”p-value KS” denote the asymptotic Kolmogorov-Smirnov statistic and the p-value of the Kolmogorov-Smirnov test, respectively. ”cartel”, ”post-cartel”, ”pre-cartel” and ”year98” denote the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, respectively.

Graphic 3.7 depicts the evolution of the relative distance whereby the horizontal line of 1 indicates that the difference between the first and second best bids equals the standard deviation of the losing bids. We consider all tenders above the threshold of 1 as potentially suspicious, and we find that bid rigging strongly affects the relative distance, which increases in the cartel period. Table 5.2

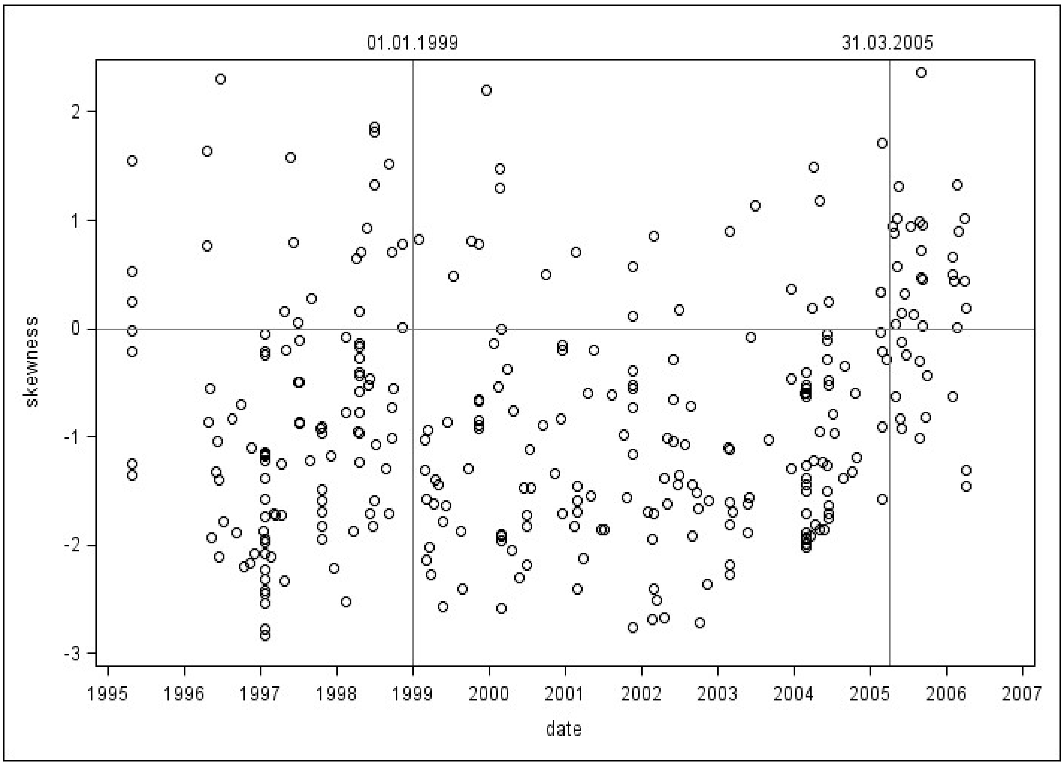


Figure 3.6: The evolution of the skewness statistic

shows that the median and the mean of the relative distance in the cartel period are 3.08 and 4.15, respectively, contrasting with the median and the mean of the post-cartel period, which are 0.62 and 0.84, respectively.

The Mann-Whitney and the Kolmogorov-Smirnov tests both reject the null hypothesis of no difference between the cartel period against the post-cartel period (see table 3.5). We find again that the year 1998 and the post-cartel period do not significantly differ. However, the cartel period and the pre-cartel period significantly differ with all the other periods.

In summary, the results of the screen analysis identifies four periods, as follows: the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the year 1998 and the pre-cartel period (from 1995 to 1997). All screens produce evidence of bid rigging and successfully capture the effects of the manipulation of the bids in the cartel period. The year 1998 and the post-cartel period exhibit the same pattern, except for the skewness statistic, which indicates that firms behave quite similarly between both periods characterized by competition. Finally, the pre-cartel period is generally comprised between the cartel period and the post-cartel period or the year 1998, although the results of the statistical analysis indicate that the pre-cartel period is nearer to the cartel period than to the post-cartel period and year 1998, supporting the evidence of incomplete cartels for the pre-cartel period.

Table 3.5: Statistical tests for the relative distance

rd	z-statistic	p-value MW	KSa	p-value KS
cartel vs post-cartel	-7.85	< .0001	4.07	< .0001
cartel vs year98	-6.83	< .0001	3.73	< .0001
cartel vs pre-cartel	-3.13	0.0017	1.91	0.0013
year98 vs post-cartel	-0.51	0.6075	0.72	0.6729
pre-cartel vs post-cartel	-4.7	< .0001	2.62	< .0001
pre-cartel vs year98	-4.38	< .0001	2.24	< .0001

Note: ”rd”, ”z-statistic”, ”p-value MW” denote the relative distance, the z-statistic of the Mann-Whitney test and the p-value of the Mann-Whitney test, respectively. ”KSa” and ”p-value KS” denote the asymptotic Kolmogorov-Smirnov statistic and the p-value of the Kolmogorov-Smirnov test, respectively. ”cartel”, ”post-cartel”, ”pre-cartel” and ”year98” denote the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, respectively.

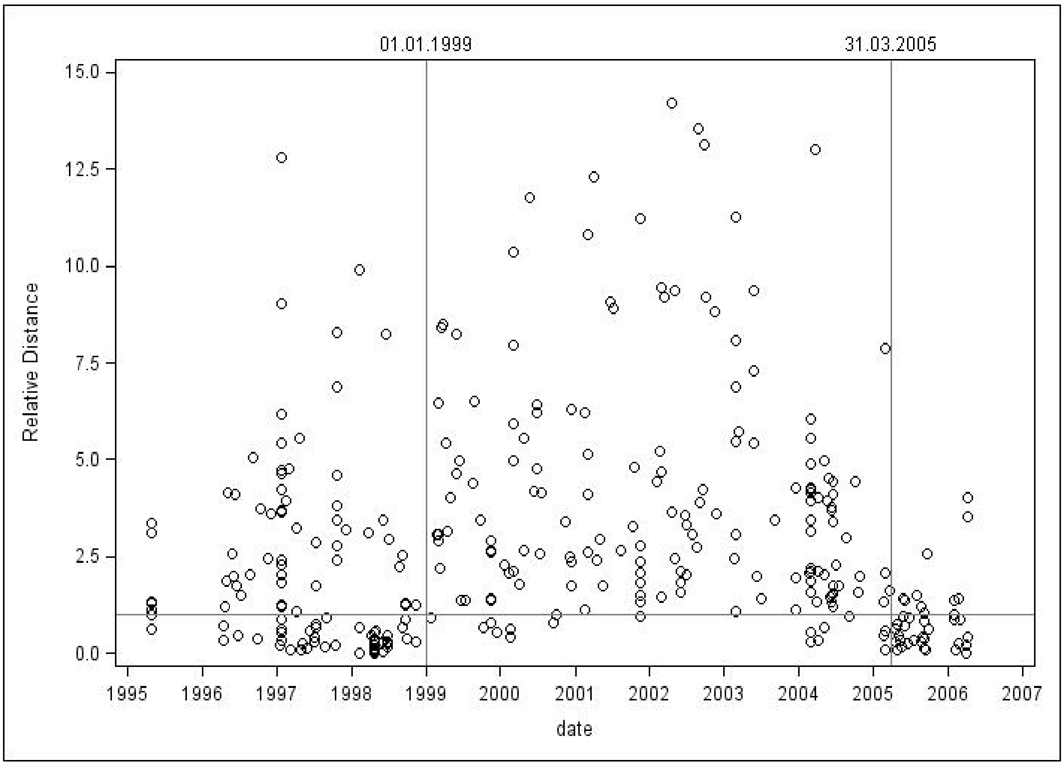


Figure 3.7: The evolution of the relative distance

Table 3.6: Descriptive statistics for the screens

Screens	Period	Mean	Median	Std	N
Coefficient of variation	Pre-cartel period	4.76	3.95	3.39	75
	Year 1998	9.63	9.15	4.76	36
	Cartel period	3.43	3.13	1.68	183
	Post-cartel period	8.92	8.10	5.40	40
Relative distance	Pre-cartel period	3.49	2.05	4.81	75
	Year 1998	1.25	0.46	2.14	36
	Cartel period	4.15	3.08	3.59	172
	Post-cartel period	0.84	0.62	0.89	38
Kurtosis statistic	Pre-cartel period	2.38	2.27	2.62	73
	Year 1998	0.40	0.15	2.35	35
	Cartel period	2.71	2.84	2.12	149
	Post-cartel period	-0.08	-0.16	1.78	33
Skewness statistic	Pre-cartel period	-1.10	-1.25	1.10	75
	Year 1998	-0.37	-0.49	1.08	36
	Cartel period	-1.06	-1.29	0.98	172
	Post-cartel period	0.24	0.37	0.82	38
The percentage difference	Pre-cartel period	5.10	4.47	2.87	75
	Year 1998	5.31	4.04	5.47	36
	Cartel period	5.08	5.11	2.26	183
	Post-cartel period	5.64	3.86	5.78	40

Note: "Std" and "N" denote the standard deviation and the number of observations, respectively.

3.4.3 Regression analysis

In the following, we estimate an OLS model to capture the effect of each period on each screen conditional on the structural screens. We assume that the periods, which indicate collusion or competition, directly and causally affect the distribution of the bids and therefore the value of the screens. We use the number of bids in a tender and the value of the contract as structural screens. We reasonably assume that the number of bids in a tender positively affects the competition. The more firms that submit bids in a tender, the fiercer is the competition. The same yields for a contract with higher value. Firms compete fiercer for contracts with higher value because they earn a higher income. Since the structural screens positively affect the competition, both should positively affect the coefficient of variation as the skewness statistic and negatively affect the percentage difference, the kurtosis statistic and the relative distance.

We estimate the following equation with White robust standard deviation for each screen.

$$screen_{it} = \beta_1 nbrbids_t + \beta_2 minbids_t + \beta_3 pre-cartel + \beta_4 year98 + \beta_5 cartel + \beta_6 postcartel + \epsilon_t \quad (3.6)$$

Where "nbrbids" and "minbids" denote the number of bids in a tender t and the value of a contract t. "precartel", "year98", "cartel" and "postcartel" denote the dummy variables for the pre-cartel period, the year 1998, the cartel period and the post-cartel period, respectively. Note that i is the subscript for each screen and t is the subscript for contracts.

For the coefficient of variation and the relative distance, we find that all estimated coefficients of each period are significant in table 3.7. For the kurtosis statistic and the skewness statistic, the estimated coefficients of 1998 are insignificant. All coefficients are insignificant for the percentage difference in the first and second lowest bids. Moreover, the estimated coefficients for the number of bids in a tender indicate opposite results from the hypothesis of competition for the coefficient of variation, the kurtosis statistic and the skewness statistic. Solely for the relative distance, we find that the number of bids in a tender negatively affects the value of the relative distance in the direction of a more competitive behavior when the number of bids in a tender rises. However, that estimated coefficient is only significant at 10%. Moreover, the value of the contract is significant solely for the coefficient of variation and indicates that the coefficient of variation decreases when the value of the contract increases. The result, therefore, indicates that competition softens when the value of the contract tendered rises.

As shown in the implementation of the simple screens, the statistical tests for all screens, except for the skewness statistic, indicate that 1998 does not significantly differ from the post-cartel period. We test this prediction again for all screens in the OLS estimation. We find again in table 3.8 that the estimated coefficients of 1998 do not differ from those of the post-cartel period for all screens, except for the skewness statistic. In summary, the tests again confirm the similarity between 1998 and the post-cartel period.

In a last step, we want to capture only the impact of collusion on the screens. Therefore, we exclude from the sample all observations from the pre-cartel period and we only use observations from the year 1998, the cartel period and the post-cartel period. We estimate the following OLS models with White robust standard deviation for each screen.

$$screen_{it} = \beta_0 + \beta_1 nbrbids_t + \beta_2 minbids_t + \beta_3 cartel + \epsilon_t \tag{3.7}$$

Where "nbrbids" and "minbids" denote the number of bids in a tender t and the value of a contract t, receptively. "cartel" denote the dummy variable for t the cartel period. Note that i is the subscript for each screen and t is the subscript for contracts.

Because we purpose estimating the impact of bid rigging on the screens, we add only a dummy variable for the cartel period. The constant term captures the effect for both the year 1998 and the post-cartel period. The results in table 3.9 indicate conservative thresholds and benchmarks for

Table 3.7: Estimation of OLS models for each screen

Endog. Var.	cv	rd	kurto	skew	diffperc
Nbrbids	-0.13*	-0.17*	0.14**	-0.06**	0.34
	(0.078)	(0.094)	(0.064)	(0.028)	(0.553)
Minbids	-0.71***	-0.18	-0.07	0.02	0.51
	(0.166)	(0.221)	(0.166)	(0.061)	(1.122)
Pre-cartel	6.49***	4.96***	1.33**	-0.70**	2.02
	(0.857)	(1.178)	(0.653)	(0.288)	(5.309)
Year98	11.34***	2.83***	-0.79	0.08	2.05
	(1.108)	(1.002)	(0.782)	(0.343)	(5.643)
Cartel	5.18***	5.42***	1.84***	-0.75***	1.61
	(0.684)	(0.853)	(0.508)	(0.218)	(5.507)
Post-cartel	10.33***	2.06***	-1.00*	0.56**	3.17
	(1.013)	(0.732)	(0.570)	(0.240)	(4.406)
N	334	321	290	321	334
R^2	0.77	0.49	0.51	0.48	0.21

Note: "Endog. Var.", "cv", "rd", "kurto", "skew" and "diffperc" denote the endogenous variable in the OLS model, the coefficient of variation, the kurtosis statistic, the skewness statitistic and the percentage difference in the first and second lowest bids, respectively. "nbrbids", "minbids", "cartel", "post-cartel", "pre-cartel", "year98", "N" and "R^2" denote the number of bids in a tender, the value of a contract, the cartel period (from January 1999 to April 2005), the post-cartel period (from April 2005 to the end of 2006), the pre-cartel period (from 1995 to 1997) and the year 1998, the number of observations and the adjusted R squared, respectively.

Table 3.8: Statistical tests for the post-cartel period against year 1998

	cv	rd	kurto	skew	diffperc
Chi^2	0.78	3.04	0.15	3.92	0.38
P-value	0.38	0.08	0.70	0.048	0.54

Note: "cv", "rd", "kurto", "skew" and "diffperc" denote t the coefficient of variation, the kurtosis statistic, the skewness statitistic and the percentage difference in the first and second lowest bids, respectively. "Chi^2" and "P-value" denote the Chi squared statistic and the p-value associated to the calculated Chi squared statistic.

Table 3.9: Estimation of OLS models for the bid-rigging effect of each screen

Endog. Var.	cv	rd	kurto	skew	diffperc
Intercept	10.49	1.92**	-0.94	0.26	2.01
	(1.021)	(0.839)	(0.663)	(0.274)	(5.643)
Nbrbids	-0.09	-0.11	0.14*	-0.04	0.41
	(0.092)	(0.093)	(0.072)	(0.030)	(0.640)
Minbids	-0.71***	-0.06	0.02	-0.01	0.66
	(0.175)	(0.231)	(0.179)	(0.063)	(1.206)
Cartel	-5.57***	3.00***	2.71***	-1.05***	-1.02
	(0.594)	(0.337)	(0.327)	(0.145)	(0.945)
N	259	246	217	246	334
R^2	0.45	0.17	0.25	0.18	0.21

Note: "Endog. Var.", "cv", "rd", "kurto", "skew" and "diffperc" denote the endogenous variable in the OLS model, the coefficient of variation, the kurtosis statistic, the skewness statititic and the percentage difference in the first and second lowest bids, respectively. "nbrbids", "minbids", "cartel", "N" and "R^2" denote the number of bids in a tender, the value of a contract, the cartel period (from January 1999 to April 2005), the number of observations and the adjusted R squared, respectively.

potential bid-rigging conspiracies. For instance, if the coefficient of variation decreases by approximately 5% in a market, it could potentially indicate a bid-rigging conspiracy. In any case, such a result should suggest that deeper investigations are needed. Likewise, if the comparison of similar markets shows such differences, as observed for the cartel period, one should investigate the possibility of bid-rigging conspiracies deeper.

A similar interpretation can be made for the other screens, except for the percentage difference in the first and second lowest bids. Since the Ticino cartel is certainly one of the severest uncovered bid-rigging cartels known in Switzerland, we suggest that the estimated coefficients for the cartel period should be considered conservative thresholds and benchmarks. Finding smaller differences or a weaker evolution in the screens would of course not indicate the absence of potential bid-rigging issues (see *Imhof et al.*, 2017, for detecting incomplete bid-rigging cartels, i.e., when firms do not rig all contracts or/and when only a subset of firms participate in the bid-rigging cartel).

Finally, most structural screens in the estimated equations in table 3.9 are insignificant. Only the number of bids in a tender is significant for the kurtosis statistic, but it positively affects the kurtosis statistic excluding a competitive effect due to more bids in a tender. Finally, the value of a contract is significant only for the coefficient of variation and excludes fiercer competition for a contract with a higher value.

3.5 Bid Rotation Screen

Following *Imhof et al.* (2017), we use the bid rotation screen to analyze relationships among firms. As the cover-bidding screens, it relies again on the same assumptions (see assumptions 3 and 4). However, unlike the previous screens presented in section 3, which characterize the discrete distribution of the bids for a peculiar tender, the bid rotation screen characterizes the interrelationship between firms in all the tenders in which they participate.

If we consider a tender as a game, the bid rotation screen analyses the emergence of equilibria in repeated games. The turn taking literature has shown how repetition affects the adoption of any equilibrium, and how history-dependent strategies play a crucial role in the emergence of a cooperative equilibrium (*Mailath and Samuelson*, 2006). Teaching history-dependent strategies is also a component of the successful implementation of turn taking (*Cason et al.*, 2013). Thus, the emergence of equilibria requires the repetition of similar strategies, and repeated strategies leave distinct signals in the bidding behavior of firms. We suggest that the bid rotation screen is adequate to detect the strategies of the cartel participants to manipulate repeatedly the bids. In the following, we describe how we normalize the bids to compare tenders of different values and we present the hypotheses of competition and those of a bid-rigging cartel operating with contract allocation.

To analyze the interaction among bidders in different tenders, we normalize the bids with the following min-max formula:

$$\hat{b}_{it} = \frac{b_{it} - b_{min,t}}{b_{max,t} - b_{min,t}} \in [0,1] \tag{3.8}$$

The min-max transformation assigns to all normalized bids $\hat{b}_{it}$ a value between 0 and 1, where the lowest bid in a tender takes the value of 0 and the highest bid takes the value of 1. Unlike the variance or the cover bidding screens, the min-max formula does not focus on the variance of the bids, but on the intern distribution of the bids per tender. By using the formula 3.8, we compute the Cartesian coordinates comprised in the space [0, 1] x [0, 1] for each pair of firms participating at the same tender.

In a competitive situation, the normalized bids are, over time, distributed in all the regions of the space [0, 1] x [0, 1], as illustrated in figure 3.8. Competing firms calculate their bids based on their costs independently from the costs and bids of other firms. Since the specialization, the remaining free capacity of each firm or the location of each firm vary according to each contract, and the costs necessarily differ among firms. Differences in cost imply that some firms have a cost advantage for a subset of contracts and bid more aggressively than firms with higher costs. The bids of cost advantaged firms are located in the bottom left quadrant or near the axes in figure 3.8, whereas bids of firms with higher costs should be located in the remaining space of figure 3.8, especially in the top right quadrant.[8]

When bid manipulation occurs repeatedly, the normalized bids are no longer distributed in all the regions of the space [0, 1] x [0, 1]. For the cover-bidding screens, the difference in the first and second lowest bids as well as the differences in the losing bids are crucial for bid coordination to ensure that the designated winner actually wins the contract. Since contract allocation to the cartel participants means that each firm wins a contract one after another, it produces a rotation pattern. To capture such a bid rotation pattern, we show the difference between two types of coordinates in figure 3.8. The first type of coordinate includes the bids from a firm i, who wins the contract, and the bid from another firm j, who submits a cover bid in favor of i. We find such types of coordinates on the abscissa in the bottom right quadrant or on the ordinate in the top left quadrant, as depicted by the gray shadow in figure 3.8.

The second type of coordinates are those when both firm i and firm j deliberately submit higher bids to conceal another firm g winning the contract. This type of coordinate lies in the top right quadrant, as indicated by the red space of figure 3.8. In this red space, both bids are sufficiently high to cover a third cartel participant. Note that if firm i and j agree to cover the designated winner g from the cartel, it is only because the reciprocal is true, that is, firm g agrees to cover firm i and j in the future or has already done so in the recent past. Such reciprocity is necessary for the stability of a bid-rigging cartel to function without monetary transfer and such repeated cover bidding produces a rotational pattern. To ensure that there is effectively a rotational pattern in the cover-bidding mechanism, we expect to find the same pattern for all cartel participants with both types of coordinates. In summary, the red and the gray space of figure 3.8 depict the non-competitive space because bids remain too high to be considered as potential alternatives for the procurement authority. Finding an anomalous high number of coordinates in these two regions may therefore be indicative of cover bidding with rotation among the cartel participants.

3.5.1 Empirical implementation

Figure 3.9 illustrates the bid rotation screen in the cartel period. For each graphic in figure 3.9, the abscissa depicts a single firm, whereas the ordinate shows the rest of the cartel participants. We can therefore analyze the interaction of one firm within the cartel. The results are unambiguous. Many of the normalized bids plot in the non-competitive spaces, as defined in figure 3.8, contrasting with

[8]The figure is modified from *Imhof et al.* (2017).

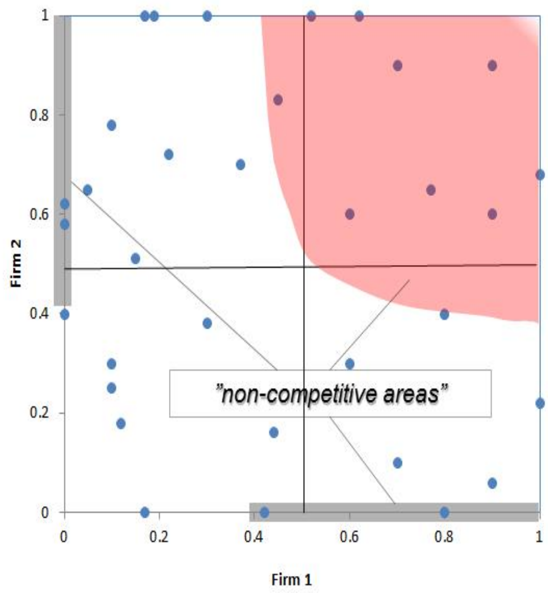


Figure 3.8: Illustration of the cover Bidding screen

the very few normalized bids (if none) in the left bottom quadrant or near both axes. Furthermore, we find the same pattern for all cartel participants, indicating that the cartel functions with contract allocation.

The dataset, which includes a large number of tenders for the cartel period of 5 years, ensures the robustness of the results. The symmetry observed in the graphics is noteworthy, since such a rotation pattern, typical of a bid-rigging cartel functioning with contract allocation, excludes that the cost advantage of one firm over the other bidders could explain the differences in the first and the second lowest bids. Putting it differently, if cost advantages explain the observed bidding pattern in the cartel period, it would imply that all winners systematically have a significant and a similar cost advantage in each contract over the rest of the firms and that the similar cost advantage would vary among all firms and contracts.

Such a random phenomenon cannot be excluded, but it is less likely, especially if we consider the large dataset used and the period of five years. However, it seems more probable that a cover-bidding mechanism explains such a systematical pattern, especially considering that we find the same pattern for all graphics in figure 3.9. In other words, this finding would have raised serious doubts in an *ex ante* analysis about the existence of a bid-rigging cartel operating with contract allocation, certainly sufficient to justify a deeper investigation.

In summary, all graphics in figure 3.9 fit the cover-bidding pattern for a bid-rigging cartel operating through contract allocation. The repeated cover-bidding pattern observed for all the cartel participants illustrates the rotation in the designated winner. Cartel participants submit higher bids to cover the designated winner and were reciprocally covered when designated by the cartel to win the contract.

The results for the post-cartel period singularly contrast with those of the cartel period. For all graphics in figure 3.10, we find the opposite phenomenon. The normalized bids are distributed in all the space [0, 1] x [0, 1], including the bottom left quadrant and the space near the axes. This observed bidding pattern fits a competitive situation, i.e., costs vary among firms and they submit both low and high bids, which is reflected by the distribution of the normalized bids in all the space of the graphics in figure 3.10. In conclusion, we validate the use of the bid rotation screen proposed by *Imhof et al.* (2017).

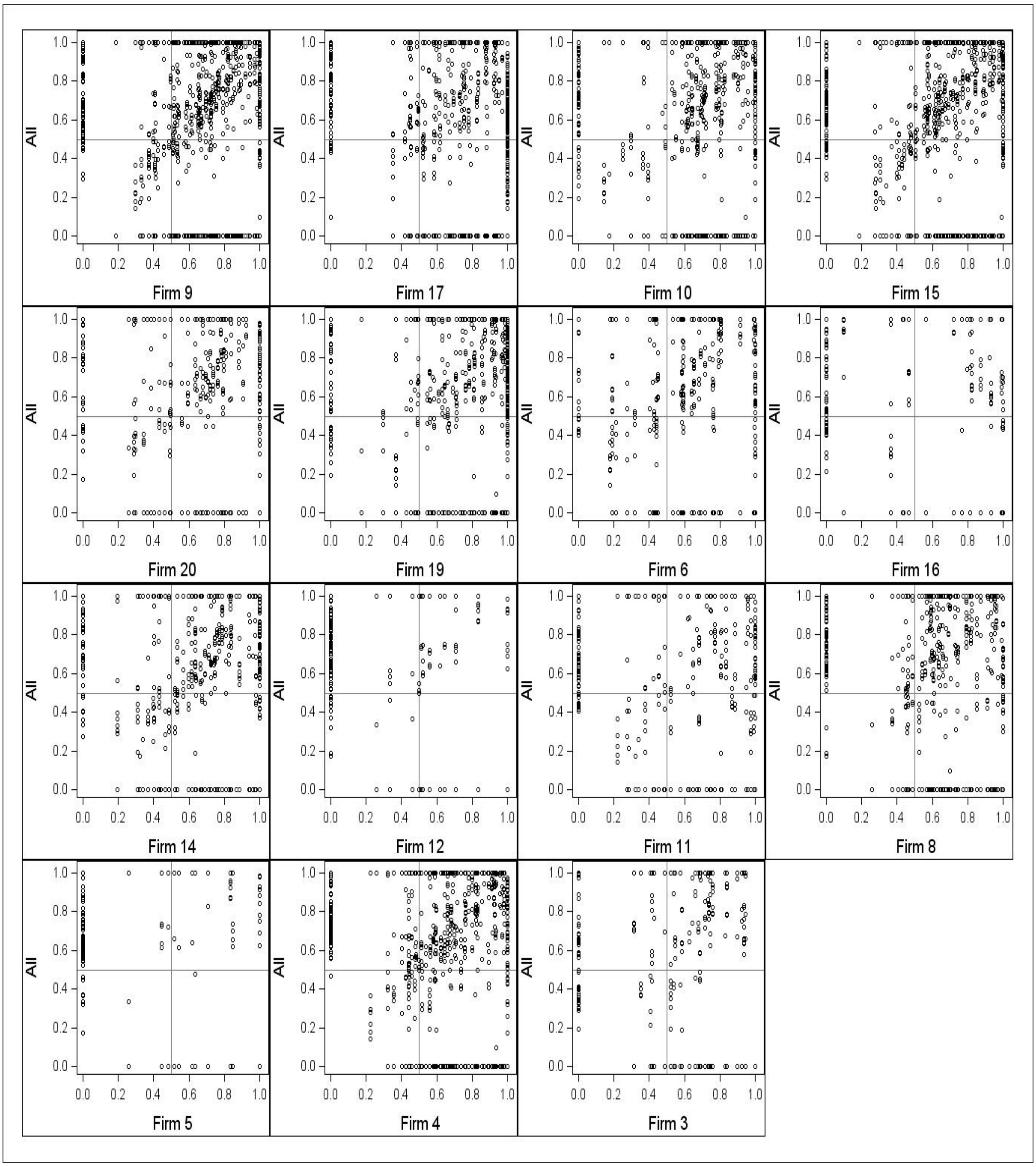


Figure 3.9: The bid rotation screen in the cartel period

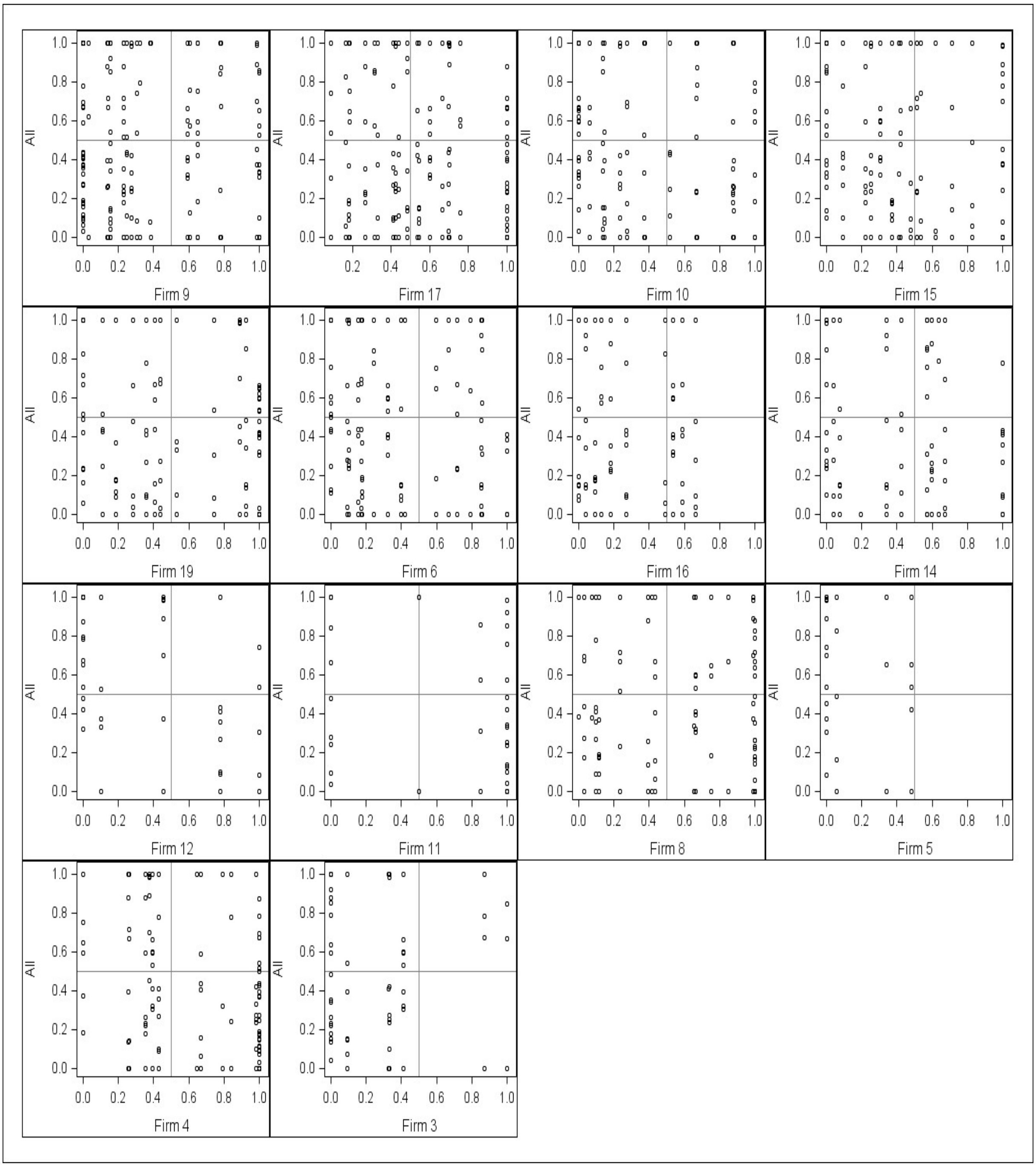


Figure 3.10: The bid rotation screen in the post-cartel period

3.6 Discussion

Any detection method should be simple. Non-economist, especially agency lawyers or judges in court must assess the results produced by the detection method to decide whether to open an investigation or how to decide in a certain case. If they do not understand the detection method applied, they would certainly not approve opening an investigation, nor would they sign warrants to search firms for evidence. In addition, the detection method should be as little time consuming as possible. Competition agencies have limited resources; therefore, they cannot spend considerable resources to screen markets, since they need resources to prosecute in parallel a multitude of different cases and fulfil a variety of tasks along with the investigations. If the detection method is complex and consumes many resources, competition agencies would be reluctant to implement it. To save resources effectively, the detection method must be suitable to screen large datasets. Only a detection method screening large datasets minimizes the resources invested, and it is therefore appropriate for competition agencies. Finally, the detection method should run in secrecy. This implies again that the data requirements for the detection method should be uncomplicated, and it must rely essentially on publicly available data. In summary, any detection method must be simple to understand for agency lawyers and judges and should minimize the resources invested and must allow large dataset to be screened in secrecy.

The detection method presented in this paper fulfils the requirement of simplicity. Because we model how bid rigging affects the distribution of the bids, we only use information about the observed bids, which are publicly available. Its collection, therefore, does not raise the cartel member's attention. As the data requirements are uncomplicated, we can apply simple screens even in a context where little information is available, and it is useful for researchers or practitioners facing data restriction problems. In addition, the implementation of simple screens does not require special know-how, and competition agencies, procurement bodies as large customers, can also screen markets. Finally, the simple screens are flexible and may be adaptable to other cases or industries, extended or refined depending on available information, as illustrated in *Imhof et al.* (2017).

Simple screens fulfil the requirement of simplicity, but are they reliable? To answer this question, we must first highlight one point. The detection method does not intend to prove a case by itself but aims at providing enough proof or in terms of the Swiss Cartel Act "sufficient suspicion" that allows an agency to open an investigation.[9] The definition of "sufficient suspicion" that allows an agency to

[9]Note that simple screens or other economic methods can also serve to prosecute cartels. However, their purpose is not to prove the existence of cartels in itself, but they can show the effect of bid rigging and support the gathered evidence. (For the use of simple statistical screens to support evidence, see Strassenbeläge Tessin (LPC 2008/1, pp. 85-112, in particular pp. 102-103 and recitals 139-141); see also Bauleistung See-Gaster, decision of the 8th July 2016 (pp. 244 ff., in particular recitals 838-841), available at the following web page: https://www.weko.admin.ch/weko/de/home/aktuell/letzte-

open an investigation depends on the legal framework of each jurisdiction. Generally, a "sufficient suspicion" must be coherent and objective. It must credibly substantiate the existence of a potential bid-rigging cartel, which also means that it must raise substantial doubt regarding the presence of bid rigging.

For the Ticino case presented in the paper, the results are clear: simple screens reveal striking irregularities. If we would have obtained the same *ex ante* results as in the ex post analysis of the Ticino case, the likelihood of triggering an investigation ex officio would have been high. Nonetheless, future cases might not be as obvious as the Ticino case, and this observation raises another question, i.e., which degree of irregularity should we demonstrate to open an investigation? There is, of course, no clear threshold, and the answer depends on human judgement. However, the following arguments may help to assess the results obtained from simple screens.

Any non-temporary and significant evolution for one screen shows a potential problem. In an *ex-ante* analysis, we recommend looking for structural changes. If no structural screens can sufficiently explain the non-temporary and significant evolution observed for one screen, then the market may be worthy of deeper investigation. Drawn from the Ticino case, we suggest conservative benchmarks for the screens. Such conservative benchmarks can indicate how important the non-temporary and significant evolution should be to flag a market. However, their values are indicative and are not binding. Competition agencies could also rely on previous information from their own closed cases to approximate problematic thresholds and benchmarks for suspect evolutions in the screens. In summary, if we consider all things being equal, that is, with no significant change in the structural screens, the stronger the evolution, the more suspect the market is. Moreover, the size of the sample determines the robustness of the results obtained from the screens. This last argument is also true for any other detection method.

For the Ticino case, we observe a clear non-temporary and significant evolution in the simple screens. However, we do not expect future cases to be as obvious as the Ticino case. Notably, the bid-rigging cartel in Ticino was an unusual case by the scale of its bid-rigging activity, i.e., all firms participated to the bid-rigging cartel, and they rigged all contracts for a period of 5 years. Such a case is certainly uncommon and may not be representative of all bid-rigging cartels. However, if the Ticino case is not representative of the scale of the bid-rigging activity, it is representative of how firms rigged contracts, and how they manipulate bids in a context of a first-price sealed-bid auction without monetary transfer. *Imhof et al.* (2017) apply *ex ante* the coefficient of variation, the relative distance and the bid rotation screen to another Swiss canton and find a bid-rigging cartel. Therefore,

entscheide.html).

the results, obtained with simple screens for the Ticino cartel, are not case-dependent, i.e., the scale of its bid-rigging activity does not explain the performance of simple screens. We believe that the assumptions made for the variance screens, the cover-bidding screens and the bid rotation screen are valid in general, especially in a context of a first-price sealed-bid auction where the price is not the unique criterion but is an essential one.

When we do not observe a non-temporary and significant evolution, can simple screens still be useful? For example, consider that we observe solely the cartel period and the values indicating bid-rigging activity. In such cases, we need to compare the results with similar markets. To control if the markets are similar, we must again rely on structural screens to assess the validity of the comparison. Indeed, if we compare markets that are dissimilar, the results obtained with simple screens may be dubious. However, if markets are similar and if there are significant differences between markets, unexplained by the structural factors, the market flagged by simple screens may be worthy of deeper investigation. Partial collusion, also called an incomplete cartel, may explain why the screens do not detect bid-rigging activities. *Imhof et al.* (2017) show that firms selectively collude on specific contracts. Because of the flexibility of simple screens, they combine both the coefficient of variation and the relative distance to focus the analysis on a subset of potential collusive firms and rigged contracts.

If the results obtained from simple screens are unclear, we also recommend contacting procurement agencies and confronting them with the irregularities shown by the simple screens. Procurement agencies are market-specialists, and they can confirm the suspicion of the simple screens or refute it by providing an objective explanation. Procurement agencies can as provide other evidence unrelated to simple screens. The OECD provides a checklist to detect irregularities in the tender process.[10] For example, if the suspected firms have made the same mistake in the calculation for a contract flagged by simple screens, this additional element gives, along with the results produced by the simple screens, rise to serious suspicion of the existence of a potential bid-rigging cartel. Therefore, procurement agencies can confirm the suspicion indicated by simple screens and crucially contribute to assessing whether the suspicion is sufficient for the agency to open an investigation.

The possible large implementation of simple screens certainly has a strong potential deterrent effect and destabilizes bid-rigging cartels. However, some bid-rigging cartels will adapt their behavior and they will try ”to beat” the screens once they know how competition agencies implement simple screens. If this is true, it will cause them additional coordination costs. Additional coordination to beat the screens may also increase the possibility of finding hard evidence. Moreover, once the

[10]See https://www.oecd.org/competition/cartels/42851044.pdf.

competition agency knows how firms coordinate their bids to beat the screens, it can still quickly adapt and refine the implemented detection method. *Imhof et al.* (2017) illustrates how flexible and adaptable simple screens are by constructing self-reinforcing tests to detect partial collusion.

3.7 Conclusion

The paper contributes to the literature on bid-rigging detection in several ways. We show that simple screens successfully capture the coordination of bids and the effect of bid rigging on the distribution of the bids. The use of simple screens relies on few general assumptions, allowing for a broad application. First, we show that bid rigging reduces the support of the bids involving a lower variance of the bids, as illustrated by the coefficient of variation. Because the support of the bids is reduced and because firms do not necessarily refuse to submit bids in public tenders, bids converge, as is shown by the higher values for the kurtosis statistic.

Second, the difference between the first and the second lowest bid is important to make sure that the procurement authority awards the contract to the designated cartel member in a context where a monetary transfer is absent and where price is not the unique criterion to award the contract. This cover-bidding mechanism implies a more negatively skewed distribution of the bids, which is captured by the skewness statistic and the relative distance.

Finally, we have shown that repeated bid coordination leads to a specific behavioral pattern due to the existence of cover bids and the bid rotation pattern due to contract allocation within the cartel. In addition, we observed a radical change after the cartel collapse, i.e., the interaction within firms fit the hypothesis of competition predicted by the screen. Therefore, we have validated the use of the bid rotation screen proposed by *Imhof et al.* (2017).

The simple screens used in this paper are uncomplicated and reliable. They are appropriate for competition agencies to screen a large amount of data in secrecy. Because the simple screens solely use the bids, which are publicly available, competition agencies can collect quickly data and apply the simple screens in secrecy. Moreover, it is also possible to refine or develop the simple screens depending on the context and based on the knowledge of competition agencies about how firms collude. Therefore, simple screens are an ideal tool for competition agencies to screen markets.

Chapter 4

Screening for Bid Rigging: Does it Work?[1]

4.1 Introduction

Bid rigging involves groups of firms conspiring to raise prices or lower the quality of goods or services offered in public tenders. Although illegal, this anti-competitive practice costs governments and taxpayers vast sums of money each year.[2] It is therefore not surprising that the fight against bid-rigging is currently a top priority in many countries and also a much-debated issue internationally.[3] In Switzerland, it was acknowledged a few years ago that the fight against bid-rigging in the procurement sector should be a priority, not least because the Swiss Competition Commission (COMCO) uncovered several bid-rigging cartels in the recent past.[4]

To detect bid-rigging (and other competition law infringements) national competition authorities rely heavily on leniency programs (see *OECD*, 2014). Switzerland is no exception: whistle-blowers or leniency applicants are the common denominator of recently prosecuted cases, and they contributed significantly to the uncovering of bid-rigging cartels. To mitigate the dependency on these external sources and actively reinforce the fight against bid-rigging, COMCO decided to initiate a long-term project in 2008. One of the goals of this project was to develop a statistical screening tool with the

[1]Chapter 4 is based on the paper "Screening for bid rigging: does it work?" in collaboration with Yavuz Karagök and Samuel Rutz, forthcoming in the Journal of Competition Law and Economics.

[2]On average, procurement amounts to 29% of the government expenditure in OECD countries and to 13% of GDP. For Switzerland, procurement accounts for 23% of the government expenditure and represents 8% of the Swiss GDP (see *OECD*, 2014).

[3]In 2009, the OECD adopted the Guidelines for Fighting Bid-rigging in Public Procurement. These guidelines were followed by the adoption of a Recommendation on Fighting Bid-rigging in Public Procurement in 2012, which calls for governments to assess their public procurement laws and practices at all levels of government in order to promote more effective procurement and reduce the risk of bid-rigging in public tenders. The two documents mentioned and many other documents related to bid-rigging are available at the OECD homepage (http://www.oecd.org/daf/competition/fightingbidrigginginpublicprocurement.htm).

[4]See Strassenbeläge Tessin (LPC 2008/1, pp. 85-112), Elektroinstallationsbetriebe Bern (LPC 2009/2, pp. 196-222), Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Aargau (LPC 2012/2, pp. 270-425), Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich (LPC 2013/4, pp. 524-652) and Tunnelreinigung (LPC 2015/2, pp. 193-245). Furthermore, COMCO regularly institutes proceedings concerning bid-rigging cases (in the beginning of 2017 several proceedings were still being investigated by COMCO).

following properties:

1. *Modest data requirements*: Screening exercises will often have to rely on limited available public data, e.g. data collected by a procurement agency or a statistical office. Gathering detailed information from private firms will hardly ever be an option since this would immediately raise the suspicion of potential cartel members and lead to the destruction of any proof of collusion.

2. *Simplicity*: For a competition authority to conduct screening exercises on a regular basis, the applied method should be as simple as possible. In other words, it is rather unlikely that methods based on complex, econometric models are suited for broad-based screening activities. Such models are often data-intensive and time-consuming to implement.

3. *Flexibility*: Of course it cannot be expected, that there is a ”one-size-fits-all” approach to detect bid-rigging cartels. The screening method should therefore be simple to adapt to different situations, e.g. to specific circumstances of the screened industry or to the available data.

4. *Reliable results*: In general, a screening method will not produce hard evidence for the existence of a cartel. This – explicitly – is not the goal of an ex ante screening exercise. In principle, screening methods can only help to identify possible deviations from competitive procurement processes. In this sense, one should not expect a screening method to produce proof of collusion but evidence sufficiently reliable to convince a competition authority to open an investigation.

We choose the following procedure to build a detection method meeting these four requirements: Starting from the existing screening literature, we apply two screens – also called markers – to a procurement dataset in which no prior information about (potential) collusion was available. Both of these screens assume that collusive behavior, e.g., in the form of explicit coordination or an exchange of information, modifies the distribution of the bids. Neither screens, however, produces unambiguous evidence as to whether collusion is likely to exist or not in our sample. A possible reason for this result is that the statistical methods suggested in the literature are not particularly well suited to detect partial collusion, i.e., collusion that does not involve all firms and/or all contracts in a dataset. Therefore, we design an approach that allows testing for partial collusion. In general, our approach amounts to a collection of mutually reinforcing tests to identify potential collusion between subsets of firms. With the help of these tests, it is possible to isolate a group of “suspicious” firms in our sample that exhibit the characteristics of a local bid-rigging cartel, operating with cover bids and a – more or less pronounced – bid rotation scheme. Based on these results, COMCO opened in 2013, an investigation at the end of which eight firms were sanctioned for bid-rigging.[5]

In this article, we present our detection method in detail. It is organized as follows: Section 4.2

[5]See press release on 4 October 2016 on COMCO’s website: https://www.weko.admin.ch/weko/de/home/aktuell/medieninformatione news.msg-id-64011.html. COMCO’s decision is, however, currently pending before the appeals court.

presents the literature on screening methods. Section 4.3 then explains the setup of our dataset and provides some descriptive statistics. In section 4.4 we apply two simple screens to our dataset. Given the ambiguous results in section 4.4, section 4.5 combines these two screens and shows how this may help to detect partial collusion. Furthermore, several tests serving to reinforce suspicions of partial collusion are discussed in section 4.5. Another test, the bid rotation test, is then discussed separately in section 4.6. Section 4.7 concludes the paper.

4.2 Screening Methods

There is a growing literature on cartel detection which can roughly be divided into two strands: Some literature discusses structural methods for the empirical identification of markets prone to collusion. Such structural methods try to analyze the market structure in different industries, aimed at the identification of factors that are known to enhance respectively sustain collusion.[6] In general, this approach uses relatively aggregated data on the industry level, and can therefore only indicate whether collusion is more or less likely to occur in certain industries. In contrast, the so-called behavioral methods analyze the concrete behavior of firms in specific markets. To this purpose a multitude of more or less complex statistical tests may be employed.

Harrington (2006) summarizes the literature on behavioral methods and discusses a number of statistical markers that may help to distinguish competitive from collusive behavior. Some of these markers rely on theoretical considerations from the literature on collusion, while others are based on empirical observations from uncovered cartels (see *Harrington*, 2007; *OECD*, 2014; *Froeb et al.*, 2014). In general, price- and quantity-related markers may be distinguished. Conceptually, in the case of tenders, the price-related markers use the information contained in the structure of the winning and losing bids to identify suspect bidding behavior. In contrast, the quantity-related markers attempt to identify collusive behavior from developments in the market shares that are prima vista not compatible with competitive markets.

The most comprehensively tested price-related marker is the so-called variance screen: Several empirical papers provide evidence for the fact that in the case of collusion prices are often less responsive to effective costs than in a competitive environment, i.e., price variability is lower in a collusive environment. For price-fixing conspiracies, *Esposito and Ferrero* (2006) show, e.g., that the use of the variance screen would have been successful in detecting two cartels – one in the fuel market and another one in the market for baby food products sold in pharmacies – investigated by the Italian Competition Authority (AGCM). *Bolotova et al.* (2008) provide mixed evidence for the

[6] See *Grout and Sonderegger* (2005) for an empirical discussion on the structural approach.

lysine and the citric acid cartels: In the lysine cartel, the standard deviation of bids was indeed significantly lower during the cartel period. However, these results could not be confirmed for the citric acid cartel. *Abrantes-Metz et al.* (2012) use – *inter alia* – a variance screen to show that daily bank quotes for the Dollar Libor behaved abnormally compared to other short-term borrowing rates. *Jimenez and Perdiguero* (2012) provide another application of the variance screen: They use the screen to examine price variability in the fuel market in the Spanish Canary Islands. Although they do not find (clear) evidence for collusion, they confirm that lower competition in markets tends to lower price variability and to increase the level of prices.

The variance screen was also applied to bid-rigging conspiracies. In the context of the highway construction cartels in North Carolina, *Feinstein et al.* (1985) report, for example, that the coefficient of variation is significantly lower in periods where bidders collude. They also find that collusion is characterized by frequent and repeated interaction of the same group of bidders. *Abrantes-Metz et al.* (2006) examine a US bid-rigging cartel for frozen fish. They show that prices for frozen perch fell – on average – by 16% after the collapse of the cartel and the standard deviation of bids increased by more than 250%. More recently, *Imhof* (2017b) illustrates for the Ticino bid-rigging cartel that simple statistical screens (among them the variance screen) do well to capture

The variance screen has also been applied by competition authorities for both price-fixing and bid-rigging conspiracies. *Ragazzo* (2012), for example, describes a method developed by the Brazilian Competition Policy System (BCPS) to screen regional gasoline markets for price-fixing conspiracy. However, the Mexican competition agency used price screens to identify bid-rigging for different types of drugs: *Mena-Labarthe* (2012) as well as *Estrada and Vasquez* (2013) report the typical pattern of low price variance during collusive periods and a significant increase of price variance after the cartel collapsed.

So far, economic theory has not provided a wholly convincing explanation for the link between collusion and price variability. There are two theoretical contributions in the literature attempting to explain why price variability may be lower in a collusive environment. *Athey et al.* (2004) consider an infinitely repeated Bertrand game in which each firm's cost is private information and varies over time. In each period messages concerning the firm's costs are exchanged and then prices are chosen. The basic problem colluding firms face is to induce truthful revelation of costs. Assuming an inelastic demand, *Athey et al.* (2004) show that – if firms are sufficiently patient – optimal collusion will be characterized by price rigidity.[7] *Harrington and Chen* (2006) choose a different approach: They start

[7] The results reported by *Athey et al.* (2004) also apply for bid-rigging conspiracies. In particular, they generalize the weak-cartel model proposed by *McAfee and McMillan* (1992) and show that their finding of pricing rigidity provides additional theoretical support for identical bidding, i.e., for lower price variance when bid rigging is present.

out from the idea that cartels try to avoid detection by buyers, who become suspicious whenever they perceive anomalous changes in the history of prices. Assuming that a cartel is aware of how its price choice affects the beliefs of buyers, *Harrington and Chen* (2006) show that prices are less responsive to cost shocks than in a non-collusive environment, i.e., there is a certain degree of price rigidity.

While price- and quantity-related markers, such as the discussed variance screen, are relatively simple to apply and may be implemented with a limited amount of information, there is also some literature addressing more complex, econometric detection methods for bid-rigging cartels. However, such methods often require firm-specific data, e.g., cost estimates for concrete contracts, information about cost structure and capacity utilization of respective firms, or the distance between the location of a firm and the project site. Additionally, these methods usually require the modeling of a (competitive) auction process serving as a counterfactual model for a situation without collusion. The contributions by *Porter and Zona* (1993), *Porter and Zona* (1999) *Pesendorfer* (2000), *Bajari and Ye* (2003) or *Ishii* (2009) can be cited as examples of such detection methods. Typically, these authors use data from bid-rigging cartels uncovered earlier and condemned by a competition authority. They then model counterfactuals fitting the specific circumstances of the examined cartels. Such methods may be very useful for a competition authority in order to show the anti-competitive effects of a specific bid-rigging cartel within a particular investigation. Furthermore, one may learn important lessons concerning the behavior of collusive firms, yet it is questionable whether complex, econometric methods are indeed suited for a wider, preventive screening activity. The sparsely documented attempts to use such methods for ex ante screening are – so far – not very encouraging(see *Aryal and Gabrielli*, 2013).

4.3 Sample Construction and Descriptive Statistics

The starting point for the construction of our sample was the annual submission statistics of a Swiss canton[8], listing all awarded contracts, grouped by the categories services, deliveries and construction. These statistics contain the name of the winner of each tender, details on the price granted, and a very short description of the contract. There is, however, no information on the losing bids. It was decided to focus on the category construction for two reasons: First, in this sector several bid-rigging cartels have been uncovered and investigated by COMCO in the recent past. Thus, it seems to be a sector that is prone to collusion. Second, due to the relatively high number of annual contracts in this sector, the setup of a meaningful sample seemed realistic. All contracts not relating to standard construction work were eliminated from the sample. These were, e.g., contracts for road surveillance

[8]With respect to population size and surface area, this canton can be characterized as an average Swiss canton.

equipment or protection equipment against rock fall. Furthermore, contracts for tunnel construction were eliminated since such contracts are only executed by a handful of specialized firms. After this process of elimination, roughly 400 contracts connected to road construction remained.

Information concerning the losing bids was gathered from the official records of the tender opening, which contain the name of the bidders and their final bids. For 282 of the 400 contracts in the road construction sector, the procurement body was able to provide the official records of the tender opening. They cover the time period from 2004 to 2010. Table 4.1 summarizes some key data of the contracts in our sample. All in all, 138 firms submitted roughly 1'500 bids for the 282 contracts. Consortiums submitted 228 bids and won the contract in 78 cases.[9] Consequently, 204 contracts were won by an individual firm.[10] Overall, the total value of the 282 contracts – measured by the sum of all winning bids – amounts to roughly CHF 216 million.

Table 4.1: Overview of the sample (2004-2010)

Number of tenders	282
Number of bids submitted	1'491
Number of firms involved	138
Number of bids from consortiums	228
Number of winning bids from individual firms	204
Number of winning bids from consortiums	78
Total value of all 282 projects (in CHF million)	216

Furthermore, Table 4.2 shows the distribution of the contracts over the time period considered and the corresponding annual total value of the contracts. The annual number and the total value of the conducted contracts are quite evenly distributed over the years.[11] Note, however, that the year 2005 is an exception since a particularly large contract of CHF 25 million was tendered. The value of the majority of the contracts in the sample is between CHF 100'000 and CHF 600'000. The median value of the contracts amounts to roughly CHF 400'000 and the average contract value is approximately CHF 770'000. The considerable difference between the average and the median indicates a skewed distribution. This asymmetry is due to a few very large contracts in the sample. The average number of bids per tender amounts to 7 while the median (6) is only marginally lower.

The tenders differ furthermore with regard to the tender procedure (invitation vs. open procedure). In an invitation procedure, the procurement agency invites firms directly to submit a bid, i.e., there is no public tender and the number of submitters is limited. In general, public procurement

[9] A consortium is a business combination in which two or more firms submit a common bid for a specific contract.

[10] This does not exclude the possibility that in some of these cases other firms were involved as sub-contractors.

[11] Although the sample includes only approx. 60% of the initially identified 400 contracts, these 282 contracts reflect – on a value basis – roughly 95% of all the contracts in the road construction sector. Thus, the sample does not include all contracts but prima vista all the important ones.

agencies are legally obliged to solicit at least three bids. The invitation procedure may be used for contracts with a value of up to CHF 500'000. For contracts with a value of more than CHF 500'000, public procurement agencies in Switzerland must institute an open procedure, in which, all interested firms – without any constraints – may submit a bid. Thus, the contract is publicly tendered.

Table 4.2: Number and value of annual tenders (CHF)

Year	Number of Submissions	Total value (CHF million)
2004	35	26
2005	40	55
2006	44	23
2007	37	30
2008	40	22
2009	46	28
2010	40	32

Our sample contains 135 contracts that were tendered publicly (open procedure) and 147 contracts that were tendered by an invitation procedure. The average and the median of the contract values largely coincide (approx. CHF 250'000) in the invitation procedures. In contrast, there is a notable difference between the average (approx. CHF 1.3 million) and the median (approx. CHF 816'000) for the open procedures which can be explained by the existence of a certain number of very large contracts in the sample. The value of the 135 contracts tendered publicly amounts to roughly CHF 185 million, i.e., roughly 85% of the total value of all contracts in the sample. There is also a significant difference between the invitation and the open procedures with respect to the number of submitted bids: While procurement bodies usually invite 4 or 5 firms to submit a bid in an invitation procedure, more than 20 firms bid for certain large contracts in the open procedure.

4.4 Two Simple Statistical Markers

Our dataset is not well-suited to test for all the statistical markers suggested in the literature. The quantity-related markers[12] are in particular not likely to produce meaningful results for two reasons: First, the contracts in our sample most likely represent only a part of the firms' construction activities, i.e., the firms in our sample may also be active in sectors other than road construction (e.g., construction of buildings). Furthermore, the sample is restricted to road construction contracts, tendered by the procurement body, and does not account for tenders by local procurement bodies or private stakeholders. Consequently, there is no reliable information concerning firm-specific market

[12] *Harrington* (2006) suggests three quantity-related markers: (1) highly stable market shares over time, (2) subsets of firms for which each firm's share of total supply is highly stable over time and (3) firms' market shares negatively correlating with each other in time.

shares in our sample. Second, annual demand for road construction (i.e. the number and the size of the tendered contracts) may fluctuate. This notion is supported by strongly fluctuating market shares (measured by the annual total value of realized contracts) of the firms in our sample. It is thus rather unlikely that an agreement on market shares can be realized in the short term, yet, a focus on long-term market shares largely eliminates the intertemporal structure in the data imposed by possible collusion.

Therefore, we decided primarily to focus on price-related markers. Again, due to different reasons, it was not possible to test for all the price-related markers suggested in the literature. For example, to test whether there is a high degree of uniformity across firms in dimensions such as prices for ancillary services, one needs information not available in the records of the tender opening. Given the information available in our dataset, it seemed most promising to focus on markers analyzing the structure among firms' bids. In what follows, we apply two such markers to our dataset.

Variance screen

As discussed in section 4.2, the variance screen is the most comprehensively tested statistical marker to detect collusion. Therefore, it seems natural to start the analysis with this particular marker. In the context of bid-rigging, the coefficient of variation is normally used to implement the variance screen since the measure is scale-invariant and thus allows for the comparison of bidding behavior for contracts with significantly differing values (See, *Feinstein et al.*, 1985; *Abrantes-Metz et al.*, 2012; *Jimenez and Perdiguero*, 2012; *Ragazzo*, 2012; *Imhof*, 2017b, e.g.,). In general, the coefficient of variation CV_t is defined as the standard deviation σ_t divided by the arithmetic mean μ_t of all bids submitted for contract t:

$$CV_t = \frac{\sigma_t}{\mu_t} \tag{4.1}$$

The empirical literature assumes that low values of the coefficient of variation indicate price rigidity, i.e., suspicious bidding behavior. More precisely, significant non-temporary decreases in the coefficient of variation are taken an indication of periods of collusion, and *vice versa*.

Figure 4.1 shows the coefficient of variation of the bids submitted in both types of procedures for the – chronologically organized – 282 contracts. As can be observed, there is no peculiar evolution of the coefficient of variation over time, i.e., there are prima vista no time periods, where the coefficient of variation systematically differs from other time periods. There is, however, a notable difference between invitation and open procedures: On average, the coefficient of variation of the open procedures amounts to 0.081 while the corresponding value for the invitation procedures amounts to

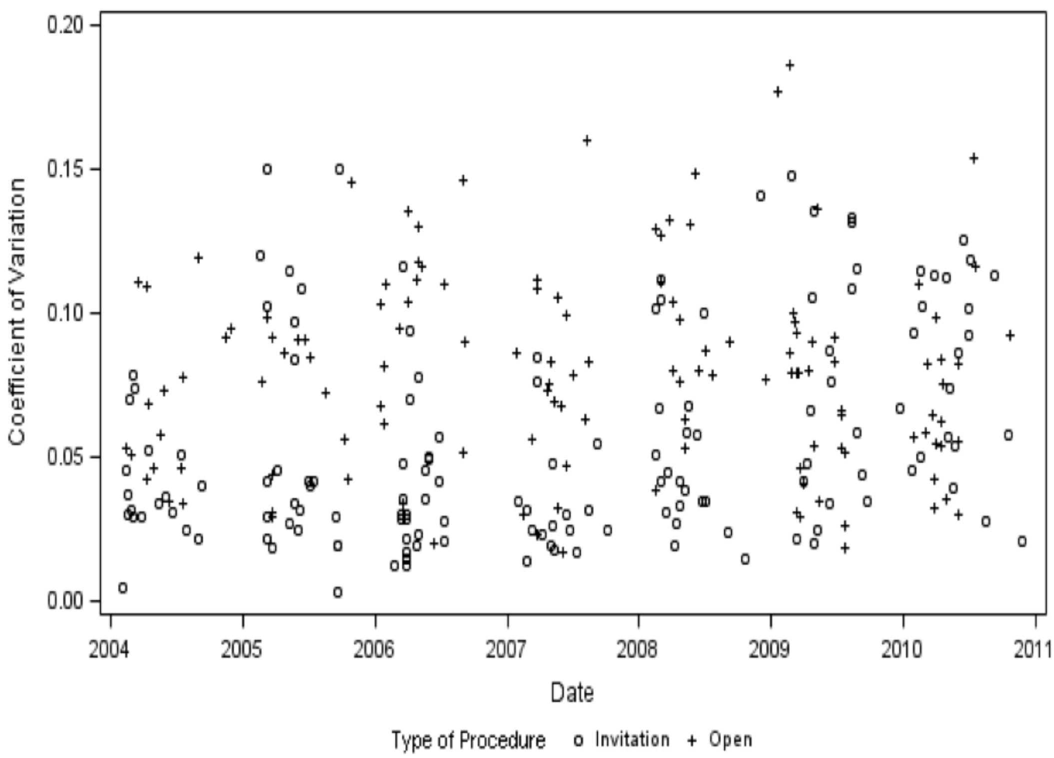


Figure 4.1: Variance screen

0.058. Statistical tests confirm that the difference between the two types of tender procedures is significant.[13] This finding may be interpreted as a (weak) indication that invitation procedures are more prone to bid-rigging than open procedures.

Cover-bidding screen

In the past few years, COMCO has uncovered several bid-rigging cartels in Switzerland.[14] In many of these bid-rigging cases, it was striking that the difference between the losing bids was systematically smaller than the difference between the winning bid and the second-best bid. Figure 4.2 illustrates this finding.[15]

Intuitively, such bidding behavior may be explained by the presence of cover bidding: Bidders not intending to win a contract offer distinctly higher prices than the agreed winner. This practice ensures that the designated winner gets the contract and that the winning bid appears to be competitive. There are three reasons why such bidding behavior is realistic in practice: First, in many

[13]Note that the coefficient of variation in our sample is not distributed normally: The Kolmogorov-Smirnov test for normality rejects the null hypothesis at the 5% significance level. Other normality diagnostic tests –Shapiro-Wilk, Cramer-Von Mises and Anderson-Darling – also reject the null hypothesis (at the 1% significance level). The difference between the two types of tender procedures is confirmed by the Mann-Whitney test, which rejects the null hypothesis of no difference between the two distributions with a z-statistic of 5.58 (p-value: < 0.0001). In addition, the Kolmogorov-Smirnov test rejects the null-hypothesis of no difference between the distribution of the coefficient of variation for both procedures with an asymptotic Kolmogorov-Smirnov statistic of 3.27 (p-value: < 0.0001).

[14]See footnote 4.1.

[15]The example in figure 4.2 is taken from the case Strassenbeläge Tessin (LPC 2008/1, pp. 85-112).

Bids (in ascending order, CHF)		Difference X_n-X_{n+1}
X_1	520'816	
		Δ_1= 36'800
X_2	557'616	
		Δ_2= 4'648
X_3	562'264	
		Δ_3= 3'561
X_4	565'825	
		Δ_4= 6'141
X_5	571'966	
		Δ_5= 4'151
X_6	576'117	

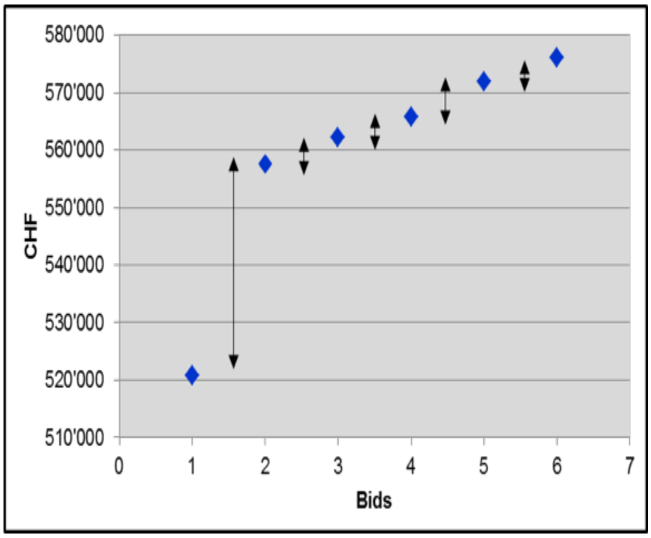


Figure 4.2: Typical bidding behavior in rigged tenders

contracts the price is not the only criterion procurement authorities take into consideration. Other criteria, such as the offered technical solution, quality or environmental aspects, may be taken into account when deciding on the winner of a contract. Such non-monetary criteria may influence the award of a contract and undermine intended bid-rigging, particularly when bids are close to each other. Second, witnesses in bid-rigging cases have reported that members of bid-rigging cartels usually make sure that the designated winning bid is 3-5% lower than the second-best bid.[16] Third, losing bids may be close to each other because no bidder wants to risk being perceived as overly expensive in the eyes of the procurement agency.

Based on the described bidding behavior, it is possible to construct an alternative price-related marker by considering the difference between losing bids and the difference between the two best bids for a specific contract.[17] To test whether cover bidding might be present, we calculate the ratio between the difference in the two lowest bids $\Delta_{1t} = b_{2t} - b_{1t}$ and the standard deviation of the losing bids $\sigma_{t,losingbids}$. This yields the following formula for the measure of relative distance RD_t:

$$RD_t = \frac{\Delta_{1t}}{\sigma_{t,losingbids}} \tag{4.2}$$

Note that the standard deviation should be calculated for only the losing bids since the difference between the two best bids is anomalously high when collusion is present.[18] Without this correction, the standard deviation would be distorted upward.[19] The relative distance measure has to be in-

[16] See e.g. *Strassenbeläge Tessin* (LPC 2008/1, pp. 85-112, in particular recital 60) or *Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich* (LPC 2013/4, pp. 524-652, in particular p. 561, recital 182 and p. 573, recital 309 and 314).

[17] See also *Imhof* (2017b) for the implementation of the relative distance on the Ticino bid-rigging cartel.

[18] Note also that we can calculate the RD only for contracts with three bids or more.

[19] It is of course possible to define the measure for relative distance differently. For instance, one may calculate the difference between the two best bids and divide it by the mean of the differences between losing bids instead of the standard

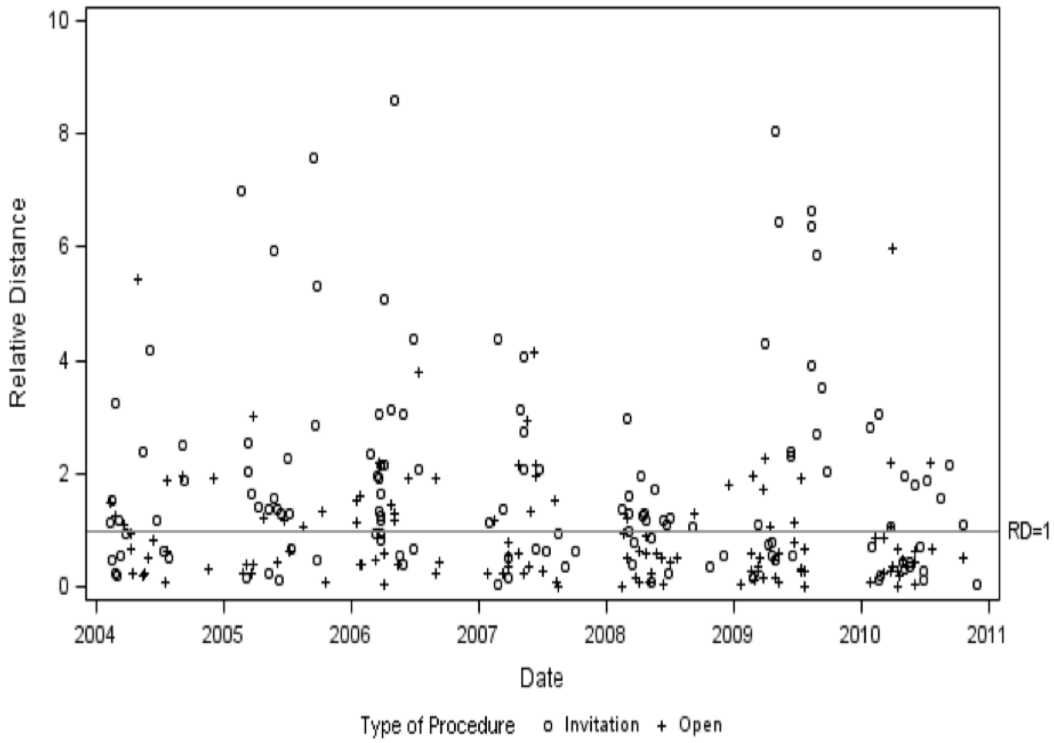


Figure 4.3: Cover-bidding screen

terpreted as follows: An RD of approximately 1 indicates that there is no difference in the bidding behavior of the winner and the rest of the bidders (see the reference line in Figure 4.3), i.e. there is no suspicious bidding behavior. An RD (much) larger than 1 indicates, however, that cover bidding may have taken place. Figure 4.3 depicts the relative distance for all contracts in chronological order and by procedures.

From Figure 4.3, conclusions similar to the case of the variance screen can be drawn. On the one hand, there are no peculiar developments of the RD observable over time, i.e., time periods where the RD systematically differs from other time periods cannot be identified. On the other hand, with an average of 1.92, the cover bidding test again suggests that collusion is more likely to be present in invitation procedures. In contrast, the average of the relative distance measure for open procedures amounts to only 1.2.[20]

deviation. We also performed the cover bidding test in this alternative way: the results remain, however, qualitatively the same.

[20]As in the case of the coefficient of variation, the Kolmogorov-Smirnov test (at the 5% significance level) – as well as the Shapiro-Wilk, Cramer-Von Mises and Anderson-Darling tests (at the 1% significance level) – rejects the normality hypothesis. The Mann-Whitney test confirms that there is a significant difference between the two types of tender procedures (z-statistic: 5.58; p-value: < 0.0001), a result also corroborated by the Kolmogorov-Smirnov test (asymptotic Kolmogorov-Smirnov statistic: 3.27; p-value: < 0.0001).

4.5 Screening for Partial Collusion

Our analysis so far indicates that firms in our sample do not seem to be involved in a systematic market-embracing collusive scheme. Furthermore, the two applied markers suggest that collusion – if present at all – is more likely to occur in invitation procedures. Both of these results are not surprising. COMCO's investigations concerning bid-rigging have revealed that cartels in construction markets often are partial, i.e., they only involve a subset of colluding firms and/or collusion is targeted at specific contracts. Thus, excluding the presence of bid rigging from the results derived above would be premature. In the remainder of this section we will show how partial collusion may be detected.

4.5.1 Multistep procedure to detect partial collusion

A crucial prerequisite to detect partial collusion with a statistical marker is a sufficient degree of regular interaction between stable groups or sub-groups of firms. Irregular and selective bid-rigging agreements between firms loosely connected (e.g. for special types of projects) are, however, extremely hard – if not impossible – to identify with a screen. Our approach amounts to a collection of mutually reinforcing tests, which allow conclusions as to whether collusion is likely to exist between subgroups of firms. All of the suggested tests may be extended, refined and adapted to the specific features of other cases in which bid-rigging is suspected.

Our procedure consists of four steps. In the first step, we isolate contracts and firms exhibiting a specific (suspicious) bidding pattern from our dataset. To this purpose, we combine the variance and the cover bidding test, and screen for contracts that simultaneously exhibit a low coefficient of variation and a high relative distance measure. As a benchmark, we use CV- and RD-values from two uncovered road construction bid-rigging cartels in Switzerland. The reason for combining the two screens is simply that we want to produce a conservative sample of suspicious contracts and firms. Given that results pointing to the existence of bid rigging may in practice trigger the opening of an antitrust investigation – most likely accompanied by drastic investigative measures such as house searches, it seems to be a reasonable strategy to minimize the probability of type I errors right from the start.[21]

Since a certain degree of repeated interaction is a basic ingredient of most bid-rigging cartels (see *Feinstein et al.*, 1985, e.g.), we analyze in a second step whether there are groups of firms regularly submitting bids for the same conspicuous contracts. There is no obvious "automatic" process

[21] Statistically, a *type I error* occurs when the null hypothesis is incorrectly rejected. In our case, a *type I error* would imply a contract is wrongly labelled as collusive. By combining the screens, we attempt to reduce the risk of erroneously flagging a contract as collusive since two different criteria must be satisfied simultaneously.

that could be used to identify possible groups of colluding firms. Statistical methods potentially suited for such a purpose, e.g., cluster analysis, are explorative processes. In other words, there is no given algorithm that could be applied to our sample – rather, the goal is to find an appropriate algorithm. Based on a simple iterative process, we identify conspicuous groups of firms and analyze the interaction between firms within such groups.

In a third step, we analyze geographical bidding behavior. More precisely, we want to know whether the identified conspicuous groups of firms are active in the entire territory of the canton or whether possible collusion is restricted to certain regions. Delineating the area where a potential bid-rigging cartel is active then allows for analyzing local competitive forces, i.e., how many firms regularly submit bids in a certain region, whether these are mainly "suspect" firms or whether there are other firms active in this region, etc. Overall, such an analysis provides important conclusions as to whether (suspected) collusion is likely to be stable. Furthermore, an affirmative result reinforces and substantiates the group formation process.

In the absence of side payments, bid-rigging agreements usually involve a rotation element to sustain collusion (see *Pesendorfer*, 2000, e.g.). In other words, a rational firm will only renounce submitting a truly competitive bid for a contract if other cartel members reward it for this behavior in the future. Typically, the reward for such cover bidding or bid suppression is the assignment of future contracts. In a fourth step – presented separately in section 4.6 – we develop a graphical method designed to visualize bid rotation within a group of firms.

4.5.2 Empirical implementation of the multistep procedure

Identification of conspicuous contracts and firms

In the first step of the multistep procedure, we want to isolate conspicuous contracts and firms from our sample by simultaneously applying the variance and the cover bidding screen. Two issues need to be discussed in this context: First, although the variance and the cover bidding screen capture conceptually different aspects of the price setting behavior of colluding firms, it cannot be excluded that the results of the two tests correlate in practice. In this case, combining the two screens would be of limited value. The correlation between the CV and the RD in our dataset amounts to -0.15 (p-value: 0.0811) for open procedures and -0.16 (p-value: 0.0623) for invitation procedures.[22] In other words, for both types of procedures, there is no significant correlation between the two markers.

[22] We use the Spearman correlation test because the CV and the RD are not normally distributed (see section 4.4). In section 4.4, we highlighted a significant difference between the two types of tender procedures: the coefficient of variation is lower and the relative distance measure larger for the invitation procedure as compared to the open procedure. Given these differences, it seems appropriate to apply the correlation test separately to each procedure type.

The second issue to discuss concerns the applied benchmark scenarios: To separate conspicuous from inconspicuous tenders, a threshold for the CV and the RD has to be defined. In the case of the RD this is relatively straightforward: A RD larger than 1 points to a conspicuous contract (see section 4.2). However, the determination of a reasonable benchmark for the CV is less obvious – there is no theoretical argument for a specific level of the CV separating conspicuous from inconspicuous contracts, yet, practical experience with bid-rigging cartels in the road construction sector may be a viable way to determine a threshold for the CV. Calculations made by COMCO have, for example, revealed that in the case of the road construction cartel in the canton of Ticino, the CV amounted to 0.03 on average during the cartel phase. Additionally, there were almost no rigged tenders with CV values higher than 0.05. After the breakdown of the cartel, the CV – on average – increased to 0.098.[23] Given that this cartel was very well organized (the members of the cartel, e.g., held weekly cartel meetings. For details, see section 1.2) and basically involved all firms located in the canton of Ticino, a CV value of 0.03 may be interpreted as a conservative benchmark for rigged contracts. In contrast, the road construction cartel in the canton of Aargau may serve as an example of a much more loosely organized cartel.[24] The cartel was characterized by partial collusion between 17 construction firms and collusion was not targeted at all road construction contracts in the canton. The average CV for the roughly 100 rigged contracts that were investigated by COMCO amounted to 0.06. Thus, for an initial screen, one may arrive at the hypothesis that tenders with a CV above 0.06 and a RD below 1 are inconspicuous, and *vice versa*.

Applying this initial screen to our dataset (scenario 1 in Table 4.3) results in the identification of 80 conspicuous contracts, i.e., in this scenario, bid-rigging cannot be excluded for more than 25% of all contracts in our sample. Given our results in section 4.4, it is also not surprising to find that the majority of these contracts (approx. 80%) were tendered by invitation procedure. Still, a non-negligible fraction of the contracts identified in scenario 1 is tendered by open procedure. Scenario 1 is a relatively rigorous screen. We therefore tested two more conservative scenarios (scenarios 2 and 3 in Table 4.3). Even in the most conservative scenario ($CV \leq 0.03$ and $RD > 1.30$), we identify 38 contracts deemed conspicuous.

Having isolated different sets of conspicuous contracts, we proceed by identifying all firms bidding for the corresponding contracts. More precisely, we identify the firms that have submitted a bid for at least 10% of all conspicuous contracts for each scenario in Table 4.3 (e.g., for scenario 1, we consider only firms that submitted a bid for at least eight conspicuous contracts). The purpose of this threshold is to eliminate ”fringe bidders”, i.e., firms that do not regularly submit bids for

[23] See Strassenbeläge Tessin (LPC 2008/1, pp. 85-112), especially p. 103.

[24] See Wettbewerbsabreden im Strassen- und Tiefbau im Kanton Zürich (LPC 2013/4, pp. 524-652).

conspicuous contracts. Such firms are unlikely to be part of a stable collusive scheme.

Table 4.3: Identification of conspicuous contracts – 3 scenarios

Scenario	CV	RD	Contracts	Share in Total Sample	Invitation Proc.	Open Proc.
1	≤ 0.06	> 1.00	80	28.4%	63	17
2	≤ 0.05	> 1.15	65	23.1%	53	12
3	≤ 0.03	> 1.30	38	13.5%	30	8

Interestingly, the list of firms turns out to be independent of the chosen thresholds: in all three scenarios the same 17 firms submitted a bid for at least 10% of the conspicuous contracts. The only difference between the scenarios is the ranking of the firms as pertaining to the absolute number of bids submitted for conspicuous contracts. Thus, the observed suspect bidding behavior can be exclusively attributed to 17 firms. Accounting for the fact that overall 138 firms have at least once submitted a bid in our sample, this result suggests that, if bid rigging occurred in our sample, these 17 firms were most likely involved.[25]

Validation of the results

However, as noted above, roughly 80% of the identified conspicuous contracts in each of the three scenarios were tendered by invitation procedure, which – by definition – limits bidder participation. Given our finding in section 4.4 that the CV (RD) is significantly lower (higher) for the invitation procedure, this result is not surprising. It raises the question whether the limited number of bidders or any other specific characteristics of the invitation procedure affects the results reported in table 4.3. To validate our results, we first consider the correlation between the number of bids and our two markers. In a second step, we examine whether varying characteristics of the tender procedures influence the identification process of conspicuous contracts.

For the entire sample, the correlation between the CV and the number of bids amounts to 0.27 (p-value: < 0.0001), while the corresponding value for the RD is -0.28 (p-value: < 0.0001).[26] Consequently, there is a weak but significant correlation between the number of bids and our two markers. However, it is interesting to note that the observed correlation vanishes when excluding the 80 conspicuous contracts from the entire sample. For the reduced sample, the correlation between the CV and the number of bids amounts to 0.02 (p-value: 0.7884), while the corresponding value for the RD is -0.04 (p-value: 0.61). This suggests that the observed correlation in the entire sample is due

[25] Of course, this does not permit the reverse conclusion that all other firms in our sample were not involved in collusion. One can only draw the conclusion that these firms do not exhibit a bidding behavior that the applied screen identifies as conspicuous.

[26] We use the Spearman correlation test because the CV and the RD are not normally distributed (see section 4.4).

to the subset of conspicuous contracts. In other words, there is no general correlation between the number of bidders and our two markers: the observed correlation is a specific feature of the subset of conspicuous contracts.

We next examine whether – in addition to the number of bidders – there are other systematic differences between the tender procedures that may influence our results. Put differently, we want to exclude the possibility that the CV (RD) is generally lower (higher) for the invitation procedure, i.e., for reasons not connected to collusive behavior of the involved firms. For that purpose, we again use the reduced sample, and we test whether there is a significant difference between invitation procedures and open procedures for our two screens. For the CV, the results are unambiguous: We find no significant difference for invitation and open procedures.[27] However, the results for the RD are mixed. While the Kolmogorov-Smirnov test indicates that there is no significant difference, the Mann-Whitney test suggests the contrary.[28] To resolve this contradiction, we resort to an analysis of the concrete differences in the mean and the median of the RD for the distinct types of contract. Table 4.4 reports the mean and the median of the RD for the conspicuous contracts and the contracts in the reduced sample. The latter values are furthermore reported separately for the invitation and open procedure.

Table 4.4: Comparative values of the RD

	Mean of the RD	Median of the RD
Conspicuous contracts	5,22	2,06
Reduced sample: all contracts	1,22	0,56
Reduced sample: contracts tendered by invitation proc.	1,59	0,67
Reduced sample: contracts tendered by open proc.	0,99	0,5

The difference between the mean of the conspicuous contracts and the contracts in the reduced sample amounts to 4, while the corresponding value for the difference between contracts tendered by invitation and open procedures in the reduced sample is 6.67 times smaller (0.6). This suggests that the procedure type explains a maximum of 15% of the difference between conspicuous contracts and the contracts in the reduced sample. Considering the values for the median leads to similar results: The difference between the mean of the conspicuous contracts and the contracts in the reduced sample amounts to 1.5, while the corresponding value for the difference between contracts tendered by

[27] The Mann-Whitney test does not reject the null hypothesis of no difference between the invitation and the open procedure for the inconspicuous sample with a z-statistic of −0.89 (p-value: 0.3751). In addition, the Kolmogorov-Smirnov test does not reject the null-hypothesis of no difference between the invitation and the open procedure for the inconspicuous sample with an asymptotic Kolmogorov-Smirnov statistic of 1.20 (p-value: 0.1112).

[28] The Kolmogorov-Smirnov test does not reject the null hypothesis of no difference between the invitation and the open procedure for the inconspicuous sample with an asymptotic Kolmogorov-Smirnov statistic of 1.18 (p-value: 0.1222). However, the Mann-Whitney test rejects the null hypothesis of no difference with a z-statistic of 2.36 (p-value: 0.0194).

invitation and open procedures in the reduced sample is 8.8 times smaller (0.17). Thus, the analysis of the medians of the different samples also suggests that the procedure type explains only a minor part (11%) of the difference between conspicuous and inconspicuous contracts.

To sum up, although the Mann-Whitney test seems to suggest that the procedure type influences the RD, our analysis of the concrete differences in the mean and the median of the RD for the distinct types of contract shows that this influence is weak. In any case, the above presented results are not called into question: If bid-rigging occurred in our sample, it is most likely that the 17 identified firms were involved and the results of the first step remain valid.

Analysis of firm interaction

To analyze the interaction between the 17 suspect firms, we start with a simple matrix quantifying how many times a firm had participated in a conspicuous tender at the same time as another firm. To arrive at the most comprehensive result possible, we decided to continue the analysis with the 80 conspicuous contracts identified in scenario 1. Our results show that some firms often and regularly submitted bids for the same conspicuous contracts while others either never interacted with other suspect firms or only on a very limited basis. Since it is natural to assume that a bid-rigging cartel involves a certain degree of (regular) interaction between firms this finding is indicative of the non-existence of a collusive agreement among all 17 firms. Based on this argument, the matrix was reduced to sub-matrices of firms that interacted (more or less) regularly with each other. By iterating the process, two potentially interesting groups of firms were condensed. For illustrative purposes we will only focus on one of these groups in what follows.[29]

As can be observed from table 4.5, firms 2, 4, 5 and 6 seem to interact often and regularly. Consider firm 2, for example: Overall, firm 2 submitted 17 bids for conspicuous projects. For 16 of these projects (94%), firm 4 also submitted a bid. Furthermore, for 9 (53%) and 15 (88%) of these 17 projects, firms 5 and 6 likewise submitted a bid, respectively. A similar pattern can be found when analyzing the bidding behavior of firms 4, 5 and 6. Additionally, all of these firms submitted a comparable number of bids for conspicuous projects. Thus, the high degree and symmetry of interaction between these firms may serve as an indication for a group of colluding firms.

Consider next that firm 3 is a much larger construction company than the other four firms (2, 4, 5 and 6). This is reflected in the fact that this firm submitted altogether 45 bids for conspicuous contracts. In addition to this fact, the bidding behavior of firm 3 is more or less comparable to the

[29] All analyses discussed in the following were also conducted for the group of firms identified in the second sub-matrix. Overall, results for this second group of firms are somewhat less indicative. In particular, the degree of interaction between these firms is lower and bidding behavior for conspicuous contracts is less symmetric. In other words, in case there is collusion between the firms in this second group it is not as pronounced as the suspected collusion in the first group.

Table 4.5: Interaction between firms in conspicuous contracts

Firm	1	2	3	4	5	6
1	15	2	8	5	1	4
2	–	17	14	16	9	15
3	–	–	45	18	11	17
4	–	–	–	23	12	19
5	–	–	–	–	14	12
6	–	–	–	–	–	20

other four firms, which suggests that firm 3 may also be a member of the identified group of possibly colluding firms. Finally, the somewhat special case of firm 1 needs to be discussed. Firm 1 is a relatively large construction company, too, which, – following a merger – exited the market in 2006. This explains the lower interaction between firm 1 and the rest of the firms in table 4.5. Still, until 2006, firm 1 seems to have interacted regularly with the other firms. Therefore, it is not unlikely that firm 1 had also been a member of a collusive group of firms until 2006.

Geographical analysis

The analysis of the bidding interaction conducted above results in the identification of a group of six firms that were possibly involved in a collusive scheme. By means of the official records of the tender opening, it is further possible to allocate each contract to a specific region. Table 4.6 shows the number of submitted bids for conspicuous contracts for the six suspect firms, sorted by the eight regions. The numbers in the brackets refer to the number of contracts actually won by the respective firm.

As can easily be observed from table 4.6, it is in particular regions A and E where the suspect firms are jointly active. These are in fact neighboring regions. Participation in conspicuous contracts in region E is, however, substantially lower than in region A, and – with the exception of firm 3 – no firm ever won a conspicuous contract in this region. In fact, firms 2, 4, 5 and 6 won conspicuous contracts only in region A, which suggests that the analysis should focus on this region. Overall, 21 conspicuous contracts are identified in region A whereby firms 2, 4, 5 and 6 won 19 of these tenders, either alone or as members of a consortium.[30] Only two conspicuous contracts were not won by a member of the suspect group of firms. It is further interesting to note that (with the exception of firm 1 which exited the market in 2006) all firms submitted bids for at least 13 conspicuous contracts and won between three and five contracts.

[30]Note that the numbers in the brackets for the contracts won in region A sum up to 22 and not to 19. This is due to the fact that a consortium of two firms won a contract in three cases.

As mentioned above, firm 3 is much larger than the other firms, which is also confirmed by its wider geographic activity. Although firm 3 submitted the highest number of bids for conspicuous contracts in region A, it cannot be excluded that this firm is involved in other (regional) collusive schemes, e.g., in regions B and G. Keeping in mind that firm 1 is also a large construction company and exited the market in 2006, the same can be said for this firm: in absolute numbers, firm 1 submitted the majority of its bids for conspicuous contracts in region A. However, it also won conspicuous contracts in regions C and H and could therefore have been involved in bid-rigging activities in these regions.

Table 4.6: Regional bidding for conspicuous contracts

Firm	Region: A	B	C	D	E	F	G	H
1	5 (1)	1 (0)	2 (1)	1 (0)	–	–	4 (0)	3 (2)
2	13 (3)	–	–	–	4 (0)	–	–	–
3	17 (3)	8 (3)	4 (0)	–	6 (2)	2 (0)	10 (4)	3 (1)
4	18 (5)	–	–	–	5 (0)	–	–	–
5	13 (5)	1 (0)	–	–	2 (0)	–	–	–
6	16 (5)	–	–	–	4 (0)	–	–	–

To sum up, the geographical analysis largely validates the results of the group formation process and raises suspicions concerning a local bid-rigging cartel operating in region A.[31] In this context, it is further interesting to note that the potential for competition from non-members of the identified suspicious group of firms in region A is limited: According to the 2008 firm census of the Swiss Federal Statistical Office, there are only 6 construction firms located in region A that identified road construction as their principal business activity. Moreover, region A is to a certain extent isolated by a range of hills from other regions of the canton under consideration. This implies a certain distance protection due to transportation costs, which play an important role in the construction sector. In addition, region A borders several other cantons, and such political frontiers may limit market access for potential competitors. Thus, the geographical characteristics of region A certainly may create an environment where collusion could potentially be sustained and stabilized.

Still, one has to account for the possibility that the observed bidding pattern may not be attributable to collusion but to specific characteristics of the tendered construction contracts in region A. Given the information in our dataset, we can control for two important factors: the number of bids and the size of the contracts. To test whether the number of bidders and the size of the contracts differ significantly between region A and the other regions, we again use a Mann-Whitney

[31] Our observations are also supported by a Chi test: There is a statisticallly significant relationship between regions and conspicuous contracts in our sample.

and a Kolmogorov-Smirnov test. For both factors, we do not find significant differences between region A and other regions.[32] Hence, it can be excluded that these two factors explain the identified conspicuous bidding pattern in region A.

4.6 Screening for Bid Rotation

In a final step, we focus on the practice of bid rotation in order to further substantiate the group formation process and to produce a better understanding of the organization and operation of a possible bid-rigging cartel.

4.6.1 Connection between bid rotation and cover bids

The practice of bid rotation typically involves submitting cover bids for contracts. Bid rotation is likely to produce a distinct bidding pattern: whenever the designated winner submits a "low" bid, all other firms will submit a deliberately "high" bid. To test whether the members of the potential bid-rigging cartel systematically behave in a way consistent with bid rotation, we start by normalizing bids. This is necessary since the value of the contracts in our sample varies considerably, i.e., it is not possible to directly compare individual bids from different contracts. A well-known standard transformation to normalize bids in a contract t is the following:

$$\hat{b}_{it} = \frac{b_{it} - b_{min,t}}{b_{max,t} - b_{min,t}} \in [0, 1] \tag{4.3}$$

where b_{it} denotes the bid of firm i and $b_{min,t}$ ($b_{max,t}$) the lowest (highest) bid in tender t. This transformation assigns a value between 0 and 1 to each bid in our sample and therefore allows for a comparison of different-valued bids. Note that value 0 is always assigned to the lowest bid, while the highest bid gets assigned value 1.

With the help of these normalized bids, it is now possible to analyze the bidding behavior of the suspect firms pairwise. The basic idea of this analysis is illustrated in figure 4.4: For all conspicuous contracts in which two suspect firms simultaneously submitted a bid, the corresponding normalized values are shown in the x/y-space. A point on the ordinate or the abscissa implies that one of the two firms actually won the contract, i.e., submitted the lowest bid in a distinctive contract.[33] For all other points, none of the two firms considered in the diagram were assigned the contract.

[32]The Mann-Whitney test does not reject the null hypothesis of no difference for the number of bidders per tender between the region A and the other regions, with a z-statistic of 0.45 (p-value: < 0.65). In addition, the Kolmogorov-Smirnov test does not reject the null hypothesis of no difference with an asymptotic Kolmogorov-Smirnov statistic of 0.99 (p-value: < 0.33). We find the same qualitative results for the size of contracts with a z-statistic of 0.99 (p-value: < 0.33)

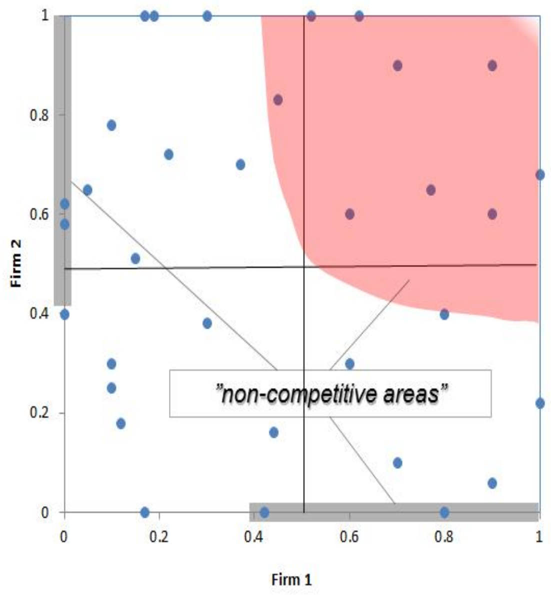


Figure 4.4: Illustration of competitive vs. non-competitive bids

In a competitive environment – i.e., when firms calculate bids independently – one would expect the combinations of bids to be distributed (more or less) randomly in the x/y-space.[34] The most competitive combinations of bids are to be found in the bottom left quadrant close to the origin. Furthermore, combinations of bids where only one firm bids aggressively are to be found close to the ordinate or close to the abscissa. In general, when firms bid independently (i.e., in a situation without collusion), one would expect to find a certain mass of points in the bottom left, the top left and the bottom right quadrant of figure 4.4.

In contrast, if bids are systematically calculated to ensure that a designated firm wins a tender – i.e., cover bidding is present – one would expect to find the following graphical pattern: First, there should be a tendency to find cover bids submitted for the other considered firm either on the ordinate in the top left quadrant or on the abscissa in the bottom right quadrant. Second, if the considered firms jointly and repeatedly cover other cartel firms, this will lead to a certain mass of points in the top right quadrant. The shaded areas of figure 4.4 show where cover bids are likely to be found.[35] For the Ticino bid-rigging cartel, *Imhof* (2017b) shows that cover bidding indeed produced the described bidding pattern.

and an asymptotic Kolmogorov-Smirnov statistic of 1.09 (p-value: < 0.18).

[33]All of the 21 conspicuous contracts analyzed in section 5 and 6 were awarded to the lowest bidder.

[34]This intuition is confirmed by an analysis of the bidding behavior of non-suspicious firms and contracts in our sample: bids are not accumulated in particular regions of the x/y-space, i.e., they are – more or less – evenly distributed all over the x/y-space.

[35]Note that it is not possible to precisely determine the boundaries of the areas where cover bids are likely to be found. The boundaries of the shaded areas in figure 4.4 should be regarded as indicative.

4.6.2 Empirical implementation

Figure 4.5 shows the pairwise bidding behavior of the six suspect firms for the conspicuous contracts in region A.[36] Each contract is assigned a number indicating the firm that actually won the respective contract. Note that there is one contract not won by the group of the six suspect firms. This contract is marked with a zero. Furthermore, when a consortium wins the tender, the number of both firms is indicated. As can easily be observed, the individual diagrams do not point in the direction of much competitive interaction between the suspect firms. Rather, the depicted bidding behavior seems compatible with cover bidding: there are hardly any points in the area where competitive bids would be expected. The bottom left quadrant is in all cases empty or near empty. Furthermore, there are no losing bids notably lower than 0.4, which suggests that there are substantial price differences between the winning and the losing bids in all respective contracts.

This first result does not come as a complete surprise since all considered contracts showed a certain conspicuousness as pertaining to the cover bidding test, i.e., these contracts are inter alia characterized by the fact that the difference between losing bids is systematically smaller than between the winning and the second-best bid. The diagrams, however, contain much more information. In particular, they visualize the connection between cover bids and bid rotation: From figure 4.5, we observe that all suspect firms submit bids for conspicuous contracts with pronounced regularity.[37] Each of the suspect firms has, on average and simultaneously with another suspect firm, submitted bids for roughly 10 conspicuous contracts. An additional analysis shows that suspect firms exclusively submitted bids for 14 contracts, and that 91% of all submitted bids came from the suspect group of firms. These results and figures point in the direction of a high degree of entanglement between the suspect firms.

There is another interesting observation derived from figure 4.5. Considering the winning bids on the ordinates and abscissas, we observe a certain symmetry: The number of winning and (possible) cover bids between the individual firms is largely equal.[38] This may be taken as an indication of the fact that "scores" between the firms exist and get settled. In summary, the identified group-internal bidding behavior may well be compatible with a bid-rigging cartel operating with cover bids and a – more or less pronounced – rotation scheme.

[36]We renounce showing three graphs in Figure 4.5 since they are characterized by very few interactions between the two bidders and are therefore not illustrative. All three suppressed graphs involve firm 1 which exited the market in 2006.

[37]It should not be assumed that all suspect firms submit a bid for every rigged tender. Factors, such as the specialization of firms, distance to the construction site, and capacity utilization, etc., decide which firms of a cartel will submit a bid for a distinct contract. Furthermore, the possibility of bid suppression has to be kept in mind.

[38]Since the distinct contracts vary with respect to contract values, there is no reason to believe that the number of winning and (possible) cover bids between two firms must necessarily be equal. A cover bid for a large contract may, e.g., be worth two cover bids for smaller contracts.

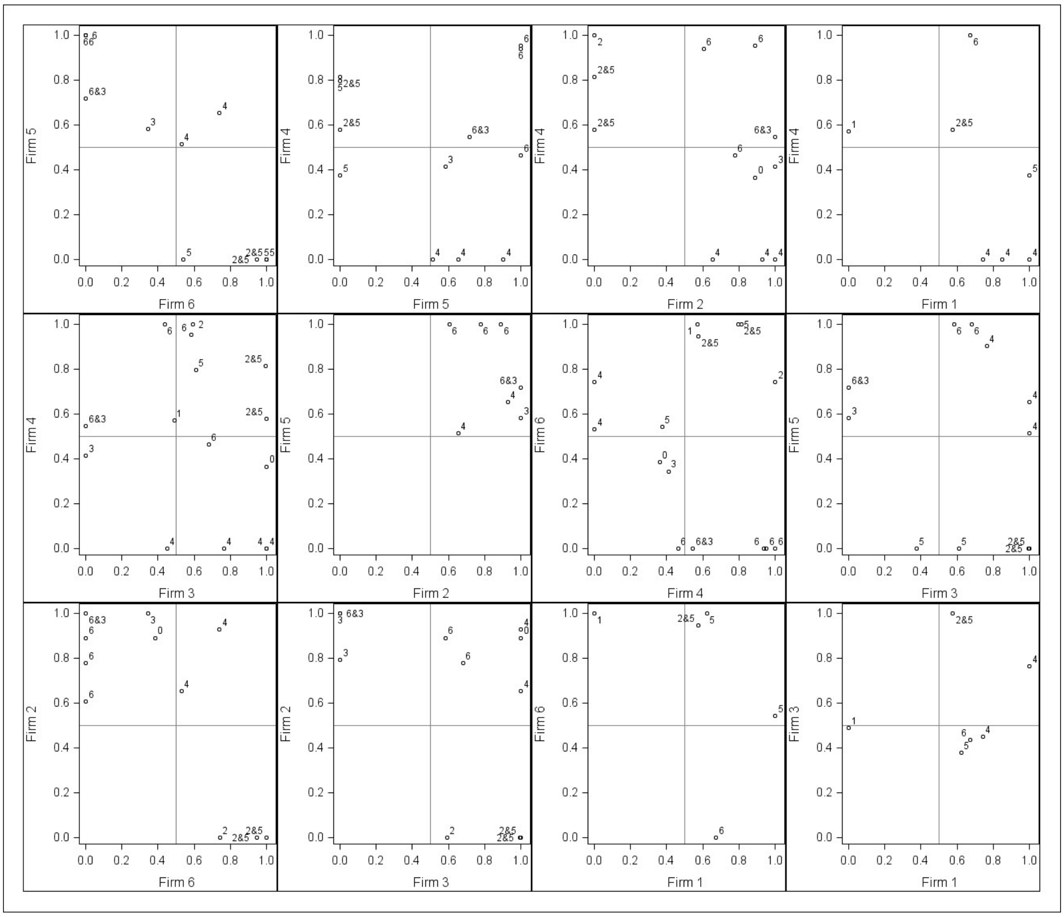


Figure 4.5: Pairwise bidding behavior for suspect firms in region A

4.7 Conclusion

A successful fight against bid-rigging today still largely depends on whistle-blowers or leniency applicants. Screening tools may therefore constitute important instruments to mitigate the dependency on these external sources and actively reinforce the fight against bid-rigging. In addition to the benefit of identifying concrete bid-rigging cartels, the successful implementation of cartel detection instruments is furthermore likely to have a strong deterrence effect. In this paper, we presented – based on simple collusive markers – a detection method characterized by the following four properties: Its data requirement is relatively modest, it is simple and flexible to apply, and it has produced sufficient evidence to open an antitrust investigation. In our view, all of these properties are crucial for preventive screening activities of a competition or procurement authority.

Our approach to detecting bid rigging contributes to the screening literature in several ways. First, we present a new statistical marker to detect cover bidding. Second, we call attention to the possibility of partial collusion, which implies that the classical markers discussed in the literature will fail, and propose a way to address this problem. In particular, we show how benchmarks derived from past investigations and the combination of (uncorrelated) screens may be used to identify subsets of conspicuous contracts and firms. To substantiate and validate suspicions of collusive behaviour, we further discuss a collection of mutually reinforcing tests providing conclusions as to whether a bid-rigging cartel is likely to exist. Of course, it has to be emphasized once more that our method is primarily a decision aid for competition authorities whether or not to open an investigation in concrete cases. In the words of *Harrington* (2006): *"At best, collusive markers can serve to screen industries to determine whether they are worthy of more intense investigations"*.

Applying our method to a road construction procurement dataset in which no prior information about collusion was available, we succeeded in isolating a group of suspicious firms exhibiting the characteristics of a local bid-rigging cartel operating with cover bids and a – more or less pronounced – bid rotation scheme. Based on these results COMCO opened an investigation in 2013. The conducted house searches produced proof of collusion and led to a conviction and sanctioning of the involved firms in 2016 by COMCO in court as the first instance. In particular, all of the six firms identified by our screening method were condemned by COMCO for participating in a bid-rigging cartel. In the course of COMCO's investigation, the proceedings were extended to two additional firms. One of these two firms actually never submitted a bid for a contract tendered by the considered Swiss Canton. It was therefore not possible to identify this firm as a cartel member with our data. The other firm submitted less than 8 bids in conspicuous tenders and was therefore eliminated from the sample of conspicuous firms by our method. To sum up, our method did not produce any false positives, i.e., no firm was wrongly identified as a member of the cartel, and all identified firms were convicted for bid rigging. Moreover, the problem of false negatives, i.e., the fact that our method did not identify two members of the bid-rigging cartel, seems to not be pervasive – it could easily be remedied during the investigation of COMCO.

Although our method delivers coherent results applied to uncovered bid-rigging cases in Switzerland, it remains to a certain degree, as all other methods discussed in the literature, case specific and data driven. Depending on the specific features of the industry in which bid rigging is suspected, some of the suggested tests may be inapplicable. Others may have to be extended, refined and adapted. Given that collusion may take a multitude of forms in the real world and data availability may differ from case to case, the flexibility of our toolbox-approach seems to be more of an advantage than a disadvantage.

Chapter 5

Combining Screening Methods and Machine Learning[1]

5.1 Introduction

In competition policy, screens are specific indices derived from the bidding distribution in tenders for distinguishing between competition and collusion as well as for flagging markets and firms likely characterized by collusion. They are thus of interest for competition agencies in order to detect cartels and to enforce competition laws. In the light of the vast variety of screens proposed in the literature (see *Harrington*, 2006; *Jimenez and Perdiguero*, 2012; *OECD*, 2014; *Froeb et al.*, 2014), the question arises which detection method some competition agency choose in practice. However, very few papers, if any, systematically investigate the performance of the screens based on statistical methods.

In this paper, we combine machine learning techniques with several screening methods for predicting collusion. We evaluate the out of sample prediction accuracy in a data set of 483 tenders that are representative for the construction sector in Switzerland. The data cover 4 different bid-rigging cartels and comprise both collusive and competitive (post-collusion) tenders, based on which we define a binary collusion indicator that serves as dependent variable. More concisely, we consider the screens proposed by *Imhof* (2017b) for detecting bid-rigging cartels and investigate the performance of machine learning techniques when using these screens as predictors. Firstly, we investigate lasso logit regression, (see *Tibshirani*, 1996), to predict collusion as a function of the screens as well as their interactions and higher order terms. Secondly, we apply an ensemble method that consists of a weighted average of predictions based on bagged regression trees (see *Breiman*, 1996), random forests

[1] Chapter 5 is based on the working paper "Machine learning with screens for detecting bid-rigging cartels" in collaboration with Martin Huber, currently under revision at the International Journal of Industrial Organization.

(see *Ho*, 1995; *Breiman*, 2001), and neural networks (see *McCulloch and Pitts*, 1943; *Ripley*, 1996).

We use cross validation to determine the optimal penalization in lasso regression as well as the optimal weighting in the ensemble method. We randomly split the data into training and test samples and perform cross-validation and estimation of model parameters in the training data, while out of sample performance is assessed in the test data. We repeat these steps 100 times to estimate the average mean squared errors and classification errors. The latter is defined by the mismatch of actual collusion and predicted collusion, which is 1 if the algorithm predicts the collusion probability to be 0.5 or higher and 0 otherwise.

In our analysis, we distinguish between false positive and false negative prediction errors. A false positive implies that the machine learning algorithm flags a tender as collusive even though no collusion occurs. From the perspective of a competition agency, this might appear to be the worst kind of prediction error, as it could induce an unjustified investigation. In contrast, a false negative implies that the method does not flag a tender as collusive, although collusion occurs. This is undesirable, too, because any method that produces too many false negatives appears not worth being implemented due to a lack of statistical power in detecting collusion. A method that is attractive for competition agencies therefore needs to have an acceptable overall out of sample performance that satisfactorily trades off false positive and false negative error rates.

Our results suggest that the combination of machine learning and screening is a powerful tool to detect bid-rigging. Lasso logit regression correctly predicts out of sample 82% of all tenders. However, the rate differs across cartel and non-cartel cases. While lasso correctly classifies 91% of the collusive tenders (i.e. 9% are false negatives), it correctly classifies 69% of the competitive tenders (31% false positives classified as collusive in the absence of bid-rigging). Thus, false positives rates are more than three times higher than false negatives. To reduce the share of false positives (which generally comes with an increase of false negatives), we consider tightening the classification rule, by only classifying a bid as collusive if the predicted collusion probability is larger than or equal to 0.7 (rather than 0.5). In this case, lasso correctly classifies 77% of collusive tenders (23% false negatives) and 85% of competitive tenders (15% false positives). By gauging the choice of the probability threshold, a competition agency may find an optimal tradeoff between false positives and false negatives.

As lasso is a variable selection method (based on constraining the sum of the absolute values of the estimated slope coefficients) for picking important predictors, it allows determining the most powerful screens. We find that two screens play a major role for detecting bid-rigging cartels, namely the ratio of the price difference between the second and (winning) first lowest bids to the average price difference among all losing bids and the coefficient of variation of bids in a tender. By far less important predictors are the number and skewness of bids.

Concerning the ensemble method, we note that it very slightly dominates lasso in terms of overall performance with 83% of classifications being correct when the probability threshold is 0.5. While the correct prediction rate of the ensemble method is slightly below that of lasso for collusive tenders (88%), it is higher for competitive tenders (76%). When setting the probability threshold to 0.7, the ensemble method does slightly better than lasso both among correctly classified collusive tenders (80%) and competitive tenders (86%), but the performance of either method appears satisfactory.

As policy recommendation, we propose a two-step procedure to detect bid-rigging cartels. The first step relies on our combination of machine learning and screening. Competition agencies may calculate the screens for each tender from the distribution of submitted bids, an information typically available in procurement processes. They may then apply the predictive model based on screening suggested in our paper to predict collusive and competitive tenders. Concerning classification into collusive and competitive tenders, it seems advisable in our data to use a tighter decision rule, by raising the probability threshold from 0.5 to 0.7. This importantly reduces the risk of false positives, at the cost of somewhat increasing the rate of false negatives. The second step consists of scrutinizing tenders flagged as collusive by machine learning. Following *Imhof et al.* (2017), competition agencies should investigate if specific groups of firms or regions can be linked to the suspicious tenders. In particular, agencies can apply the cover-bidding screen, see *Imhof et al.* (2017), which investigates the interaction among suspected firms, to check whether their group-specific interactions match a bid-rigging behavior.

Our paper is related to a small literature on implementing screens to detect bid-rigging cartels (see *Feinstein et al.*, 1985; *Imhof et al.*, 2017; *Imhof*, 2017b). This literature differs from the majority of studies on detecting bid-rigging cartels that use econometric tests typically not only relying on bidding information, but also on proxies for the costs of the firms (see *Porter and Zona*, 1993, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b; *Imhof*, 2017a). However, such cost information is not easily available before the opening of an investigation and the data collection process in order to implement such tests (see *Bajari and Ye*, 2003) is rather complex when compared to our method based on machine learning and screening. Finally, our paper is also related to studies on screens in markets not characterized by an auction process (see *Abrantes-Metz et al.*, 2006; *Esposito and Ferrero*, 2006; *Hueschelrath and Veith*, 2011; *Jimenez and Perdiguero*, 2012; *Abrantes-Metz et al.*, 2012).

The remainder of the paper is organized as follows. Section 2 reviews our data, which includes four bid-rigging cartels in the Swiss construction sector. Section 3 discusses the screens used as predictors for collusion. Section 4 presents the machine learning techniques along with the empirical results. Section 5 discusses several policy recommendations of our method. Section 6 concludes.

5.2 Bid-Rigging Cartels and Data

In this section, we discuss our data which contain information about four different bid-rigging cartels in Switzerland. The first cartel, denoted as cartel A, was formed in the canton of Ticino (see *Imhof*, 2017b), the second one, denoted as cartel B, in the canton of St. Gallen (see *Imhof et al.*, 2017). The Swiss Competition Commission (hereafter: COMCO) rendered a decision for bid-rigging cartel B but four firms appealed against the decision.[2] The third and fourth cartels are denoted by C and D and their data had not been considered prior to the present paper. For confidentiality reasons, we do not report more detailed information on cartels C and D. All data on the four cases come from official records on the bidding processes at the cantonal level.

The four bid-rigging cartels concerned road construction and maintenance as well as any related further engineering services. More special engineering services as bridge or tunnel construction are, however, not included in the data. Even though the four cartels were formed in different cantons of Switzerland, the structure of the construction sector, in which the cartels were active, is quite comparable. Therefore, the contracts included in the data are representative for the whole of Switzerland.

For each of the cartels in our data, we observe the cartel period as well as a competitive post-cartel period. Table 5.1 reports the number of tenders by cartel and period. Firms rigged all tenders in the cartel period and all firms submitting bids in the cartel period participated in the bid-rigging cartel. Therefore, when we subsequently refer to tenders in the cartel period, it is implied that the bid-rigging cartels are complete in the sense that all firms participating in the tender process were colluding. Furthermore, the firms were successful in the sense that they adhered to their agreements. The opposite holds for all tenders in the post-cartel periods of our data, in which firms fully competed to win contracts. Thus, we have an uncontaminated sample in the sense of having either periods of perfect collusion or perfect competition for evaluating the performance of sample screens.

Collusive agreements were comparable across the four bid-rigging cartels and can be described as a two-steps procedure. The first step consists of determining the designated winner of the tender by the cartel. Various factors play a role for how contracts are distributed among firms in a cartel, namely the distance between firms and the contract location, capacity constraints, and specialization in terms of competencies. Contract allocation has to be beneficial to all in the sense that all firms

[2] See https://www.weko.admin.ch/weko/fr/home/actualites/communiques-de-presse/nsb-news.msg-id-64011.html.

Table 5.1: Number of collusive and competitive tenders

Cartel Period	Perc.	Post-cartel	Period	Perc.	Total
Cartel A	148	82%	33	18%	181
Cartel B	19	50%	19	50%	38
Cartel C	94	53%	85	47%	179
Cartel D	39	46%	46	54%	85
Total	300	62%	183	38%	483

should win contracts, otherwise certain firms would not have incentives to participate in bid-rigging cartels. The second step consists of determining the price of the designated winner by the cartel. This is crucial because the cover bids should be higher than the bid of the designated winner to ensure contract allocation as intended by the cartel. In other words, all firms know the price at which the designated winner of the cartel submits the bid.

In all four cartels, the procurement procedure was based on a first-price sealed bid auction. The procurement agency announced a deadline for submitting bids for a particular contract and provided all relevant documents for the tender process. Interested firms calculated and submitted their bids prior to the deadline. After the deadline passed, the call for bids was closed and the procurement agency opened the submitted bids to establish a bid summary, i.e. an official record of the bid opening which indicates the bids, the identities of the bidders, and the location and type of the contract.

In the paper, we use solely information on bids coming from the official records of the bid opening to calculate the screens for each tender. Since access to the bid summaries is either publicly granted or easily established through procurement agencies, screening can be organized in a rather discrete manner. That is, competition agencies can conduct the screening process without attracting the attention of the bid-rigging cartel, which is crucial for any detection method.

Essentially two types of procedures are used by procurement agencies at the Switzerland: the open procedure and the procedure by invitation. In an open procedure, all firms that meet the conditions provided in the tender documentation may submit a bid. It is legally stated that open procedures should be used for contracts above 500'000 CHF. In contrast, in the procedure by invitation, the procurement agency determines potential bidders by inviting a subset of firms (at least 3 firms, but generally more). Contracts above 500'000 CHF cannot be tendered based on invitation. Thus, competition can vary depending on the type of procedure and one would suspect it to be fiercer in the open procedure than by invitation. To take account of such differences in the pressure to compete, we include the number of bidders and the value of the contract as potential predictors in the empirical analysis.

Prices indicated in the bids play a major role for allocating the contracts, although procurement agencies in Switzerland take also further criteria into consideration. This includes the organization of work, the quality of the solution offered by the firm, the references of the firm, and environmental as well as social aspects. Even if such additional criteria become more important as the complexity of the contracts increases, the price remains the most decisive feature in the procurement process.

5.3 Screens

A screen is a statistical tool to verify whether collusion likely exists in a particular market and its purpose is to flag unlawful behavior through economic and statistical analysis. Using a broader definition, screens comprise all methods designed to detect markets, industries, or firms for further investigation associated with an increased likelihood of collusion (see *OECD*, 2014). The literature typically distinguishes between behavioral and structural screens (see *Harrington*, 2006; *OECD*, 2014). Behavioral screens aim to detect abnormal behavior of firms whereas structural screens investigate the characteristics of entire markets that may favor collusion, in order to indicate if an industry is likely prone to collusion.

Behavioral screens are divided into complex and simple methods. Complex methods generally use econometric tools or structural estimation of auction models to detect suspicious outcomes (see *Porter and Zona*, 1993; *Baldwin et al.*, 1997; *Porter and Zona*, 1999; *Pesendorfer*, 2000; *Bajari and Ye*, 2003; *Banerji and Meenakshi*, 2004; *Jakobsson*, 2007; *Aryal and Gabrielli*, 2013; *Chotibhongs and Arditi*, 2012a,b; *Imhof*, 2017a). Simple screens analyze strategic variables as prices and market shares to determine whether firms depart from competitive behavior. While there are many applications of simple screens to various regular markets, applications to bid-rigging cases are rather rare (see *Feinstein et al.*, 1985; *Imhof et al.*, 2017; *Imhof*, 2017b, for exceptions).

In this paper, we propose the application of simple screens combined with machine learning to detect bid-rigging cartels. Following *Imhof* (2017b), we consider several statistical screens constructed from the distribution of bids in each tender to distinguish between competition and collusion. Because each screen captures a different aspect of the distribution of bids, the combined use of different screens potentially allows accounting for different types of bid manipulation.

In the following, we in more detail present the screens used in the empirical analysis. First, we discuss two kinds of behavioral screens: variance screens and the cover-bidding screens. We consider the coefficient of variation and the kurtosis as variance screens, and the percentage difference between the first and second lowest bids, the skewness, and two measures of the difference of the second and first lowest bids relative to the differences among all losing bids as cover bidding screens.

For each behavioral screen, we describe by means of the Ticino case (cartel A) how collusion affects the distribution of bids and the screens. Second, we consider two structural screens: The number of bidders per tender and the size of the contract. We discuss their possible influence on the pressure to compete, which determines the likelihood of collusion.

5.3.1 Variance screens

Coefficient of variation

Cartel members must exchange information in order to coordinate bids and assure that the designated winner by the cartel actually acquires the contract. That exchange of information on bids likely affects the support of the distribution of bids. First, cartel members do not make too low bids, since their aim is to raise the bid of the designated winner in order to extract a positive cartel rent. Second, cartel members cannot submit too high bids either, because procurement agencies might have a certain prior about a realistic distribution of bids. Therefore, cartel members are constrained to submit higher bids than under competition that are, however, still below a certain threshold. This reduces the support of the distribution of bids and thus, in general the variance.

To capture the effect of the support reduction in the distribution of bids, we consider the coefficient of variation represented by the following formula:

$$CV_t = \frac{s_t}{\mu_t}, \tag{5.1}$$

where s_t and μ_t are the standard deviation and mean of the bids, respectively, in some tender t. μ_t is higher in cartels than in competitive markets, since cartel members submit higher bids. Furthermore, s_t decreases because the support is reduced. Since μ_t increases and s_t decreases, CV_t necessarily decreases when bid rigging occurs. The subsequent graph illustrates this for the Ticino case, (see *Imhof*, 2017b). The vertical lines delimit the cartel period going from January 1999 to April 2005.

It can be seen that the coefficient of variation is significantly lower in the cartel period compared to the post-cartel period or the year 1998. During the cartel period, the coefficient of variation exhibits a mean and a median amounting to 3.43 and 3.13, respectively. In contrast, in the post-cartel period the mean and median are 8.92 and 8.10, respectively, which is approximatively corresponds the values for the cartel period (see *Imhof*, 2017b). Therefore, the coefficient of variation is expectedly lower when our dependent variable *cartel period* takes the value 1. In the empirical analysis further below, we investigate if the lasso coefficient on the predictor *coefficient of variation* is negative and how it impacts the likelihood of collusion.

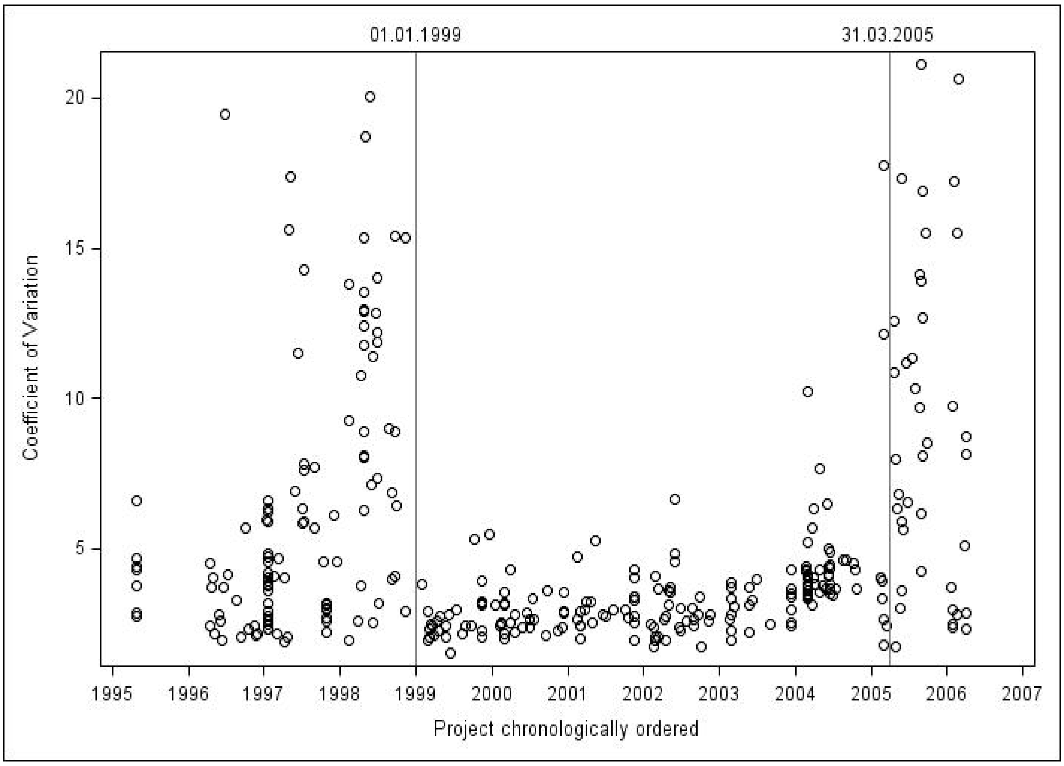


Figure 5.1: The evolution of the coefficient of variation

Observation 1: *the coefficient of variation decreases in the case of bid rigging.*

Kurtosis statistic

We suspect that bids converge when bid rigging occurs. Bidders exchange their bids, in particular that of the designated winner of the cartel. All other firms submit phony bids that use the bid of the designated winner as focal point. Because other cartel members submit phony bids slightly higher than that of the designated winner, the distribution of the bids is more compressed than in competitive markets, i.e. there is a tendency of convergence. Similar to support reduction, convergence tends to decrease the variance. Yet the two phenomena are conceptually not exactly the same: Convergence of bids implies that the mean absolute difference between bids is reduced, possibly beyond the reduction of the support of the distribution of bids. To analyze the convergence effect of bid rigging on the distribution of bids, we consider the following kurtosis statistic:

$$Kurt(b_t) = \frac{n(n+1)}{(n-1)(n-2)(n-3)} \sum_{i=1}^{n} (\frac{b_{it} - \mu_t}{s_t})^4 - \frac{3(n-1)^3}{(n-2)(n-3)}, \tag{5.2}$$

where n denotes the number of bids in some tender t, b_{it} the i^{th} bid, s_t the standard deviation of bids, and μ_t the mean of bids in tender t.[3] In the Ticino case, we observe that the kurtosis is positive and significantly higher in the cartel period than in the post-cartel period. During the cartel period, bids show a tendency of convergence with the mean and median kurtosis amounting to 2.71 and 2.84, respectively. In contrast, in the post-cartel period the mean and median are -0.08 and -0.16, respectively, which approximatively corresponds to the values of a normal distribution, (see *Imhof*, 2017b). Therefore, the kurtosis is expectedly positive and higher when the dependent variable *cartel period* takes the value 1.

Observation 2: *the kurtosis statistic is positive and increases in the case of bid rigging.*

5.3.2 Cover-bidding screens

Percentage difference

We suspect that the difference between the first and second lowest bids matters to ensure that the designated winner by the cartel indeed wins the contract. If the second lowest bid is too close to the lowest one, the procurement agency might place the contract with the second lowest bid when criteria other than the price appear more favorable (in a way that they compensate for the price difference). As mentioned before, such criteria comprise the technical solution offered, the quality, the references of the firms, and environmental or social aspects. While for standard construction works it is the prices that are most decisive, references and quality of the technical solution may matter for more specialized works. Since our data mainly consist of standard construction works and related engineering services, price is the major criterion for awarding the contract. But even then, firms might want to maintain a certain minimum difference between the first and the second lowest bids to guarantee the outcome desired by the cartel. This is decisive for the stability of the bid-rigging cartel.

To analyze the difference between the second and first lowest bids in a tender, we calculate the percentage difference using the following formula:

$$Diff.Perc._t = \frac{b_{2t} - b_{1t}}{b_{1t}}, \tag{5.3}$$

where b_{1t} is the lowest bid and b_{2t} the second lowest bid in tender t.

In the Ticino case, the percentage difference lies between 2.5% and 5% during the cartel period, while it decreases in the post-cartel period and is below 2.5% for half of the observations, (see *Imhof*,

[3] As the kurtosis can only be calculated for tenders with more than 3 bids, our data contain only tenders with 4 or more bids.

2017b). In other cartels, it has been observed that the second lowest bid is generally between 3% and 5% higher than the lowest one when collusion occurs. Therefore, we expect the percentage difference between the second and first best bids to increase when the dependent variable *cartel period* takes the value 1.

Observation 3: *The percentage difference between the two lowest bids in a tender increases in the case of bid rigging.*

Skewness statistic

We suspect the distribution of bids to become (more) asymmetric in the case of bid-rigging compared to competition, due to a greater difference between the first and the second lowest bids, see the discussion of the previous section. We analyze this using the skewness statistic:

$$Skew(b_t) = \frac{n}{(n-1)(n-2)} \sum_{i=1}^{n} (\frac{b_{it} - \mu_t}{s_t})^3, \tag{5.4}$$

where n denotes the number of the bids in some tender t, b_{it} the i^{th} bid, s_t the standard deviation of the bids, and μ_t the mean of the bids in tender t.[4]

In the Ticino case, the skewness statistic is negative in the cartel period with a mean of -1.06 and a median of -1.29 such that the distribution of bids is left-skewed. In contrast, the distribution is almost symmetric in the post-cartel period with the mean and median skewness amounting to 0.24 and 0.37, respectively, (see *Imhof*, 2017b). Therefore, when the dependent variable *cartel period* takes the value 1, the skewness is expectedly negative, indicating an asymmetric distribution of bids.

Observation 4: *The skewness decreases in the case of bid rigging and is negative.*

Relative difference

Concerning our relative difference measure, we combine the arguments of the percentage difference and skewness. We assume simultaneously that the difference between the second and first lowest bids increases and the difference among losing bids decreases. To analyze this cover bidding behavior, we construct a relative difference ratio following *Imhof et al.* (2017) by dividing the difference between the second and first lowest bids $\Delta_{1t} = b_{2t} - b_{1t}$ by the standard deviation of all losing bids.

$$RD_t = \frac{\Delta_{1t}}{s_{t,losingbids}}, \tag{5.5}$$

[4]The skewness can only be computed for tenders with more than 2 bids. Our data contains only tenders with 4 or more bids.

where b_{1t} denotes the lowest bid, b_{2t} the second lowest bid, and $s_{t,losingbids}$ the standard deviation calculated among the losing bids in some tender t. A relative difference higher than 1 indicates that the difference between the second and first lowest bids is greater than one standard deviation among losing bids. This points to a cover-bidding mechanism.

For the Ticino case, we observe that the relative difference is higher than 1 in the cartel period with a mean of 4.15 and a median of 3.08, such that the differences between the second and first lowest bids are generally clearly larger than the respective standard deviations among losing bids. As shown in graph 5.2 below, very few values are below 1 in the cartel period, while in the post-cartel period, the mean and the median of the relative differences only amount to 0.84 and 0.62, respectively, (see *Imhof*, 2017b). Therefore, when the dependent variable *cartel period* takes the value 1, the relative distance is expectedly larger (than 1), pointing to an asymmetric distribution of bids.

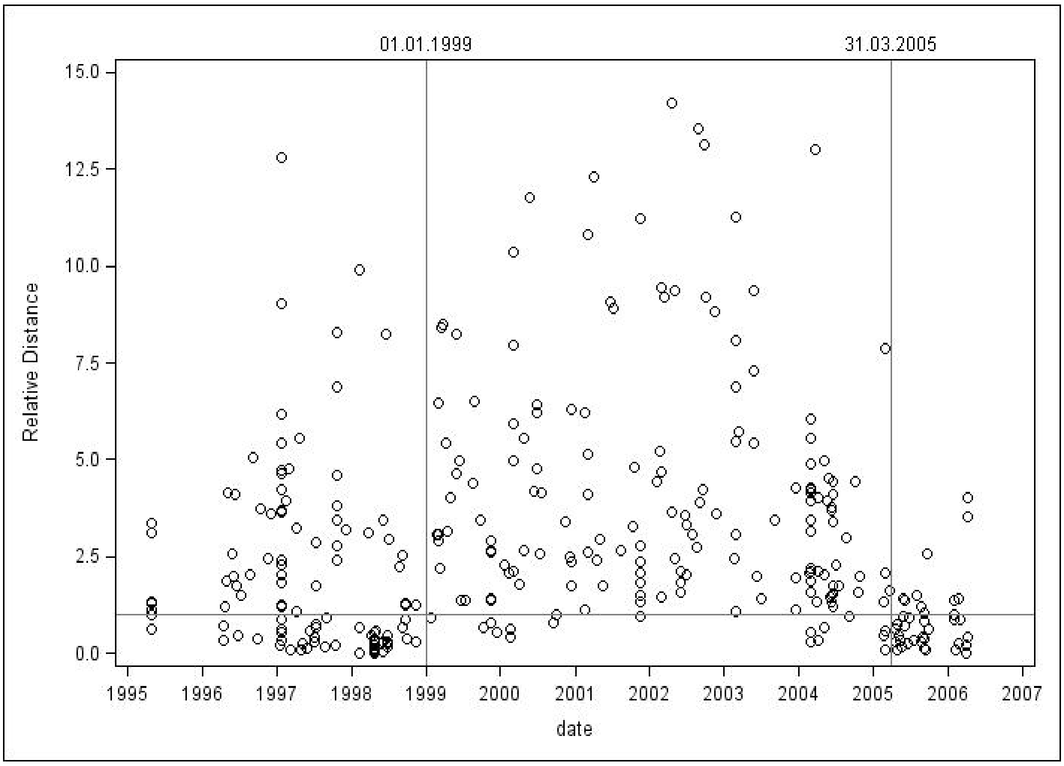


Figure 5.2: The evolution of the relative distance

Observation 5: *The relative difference increases in the case of bid rigging and is higher than 1.*

Alternative relative difference

We also calculate the relative difference in an alternative way, based on the same expectations as for the previously considered relative difference: The difference between the second and first lowest bids presumably increases, while the difference among losing bids generally decreases. However, we now use the mean difference between losing bids (rather than the standard deviation) as denominator in our alternative ratio for the relative difference:

$$ALTRD_t = \frac{b_{2t} - b_{1t}}{\frac{(\sum_{i=2,j=i+1}^{n-1} b_{it} - b_{jt})}{n-2}}, \tag{5.6}$$

where n is the number of bids, b_{1t} is the lowest bid, b_{2t} the second lowest bid, and the term in bracket is the sum of differences between losing bids in some tender t. A value larger than 1 indicates that the difference between the second and first lowest bids is larger than the average difference between losing bids.

Observation 6: *The alternative relative difference increases in the case of bid rigging and is greater than 1.*

5.3.3 Structural screens

Concerning structural screens, we consider two variables: the number of bidders per tender and the value of the contract. Both variables may influence the intensity of competition in the tender process.

Number of bidders

The number of bidders supposedly affects the intensity of competition. The larger the number of bidders is, the fiercer competition is generally expected to be. The pressure to compete may also have an impact on the magnitudes of behavioral screens or interact with them. It thus appears important to control for the number of bidders in our empirical analysis.

Contract value

The value of the contract presumably affects competition in a positive way. As the value of the contract increases, so does the revenue of the firm winning the contract. Therefore, we suspect firms to compete more fiercely for a contract worth, say, 2 million CHF than for 0.2 million CHF. If the value of the contract affects intensity of competition, it may interact with behavioral screens, making it a potentially important control variable in the empirical analysis.

5.3.4 Descriptive statistics

Table 5.2 provides descriptive statistics for all screens, separately for collusive and competitive periods. We see that the screens generally differ in terms of means and standard deviations across both groups of periods. According to the Mann-Whitney test, these differences are statistically significant at the 1% level for all but the percentage difference screen. Furthermore, the Kolmogorov-Smirnov test for equality in distributions is significant at 1% for all screens. This points to the potential usefulness of the screens for predicting collusion, which is empirically assessed in the next section.

Table 5.2: Descriptive statistics

Screens	Mean	Median	Std	Min	Max	Obs
			Cartel periods			
Number of bidders	6.75	6.00	2.34	4	13	300
Relative Distance	2.69	1.58	2.95	0.11	23.02	300
Alternative Relative Distance	2.23	1.98	1.31	0.19	6.95	300
Coefficient of Variation	3.53	2.99	1.96	0.69	13.74	300
Kurtosis	1.48	1.16	2.23	-4.03	8.14	300
Skewness	-0.60	-0.61	1.04	-2.76	2.2	300
Perc. Difference	4.03	4.02	2.62	0.43	24.93	300
Contract Value	730’603.68	382’491.26	866’905.88	23’159.81	4’967’503.78	300
			Competitive periods			
Number of bidders	6.16	6.00	1.98	4	13	183
Relative Distance	0.83	0.51	0.96	0.01	5.85	183
Alternative Relative Distance	1	0.83	0.73	0.01	3.67	183
Coefficient of Variation	7.94	7.23	3.94	1.49	22.59	183
Kurtosis	0.22	0.07	1.81	-5.40	6.06	183
Skewness	0.31	0.34	0.86	-1.83	2.36	183
Perc. Difference	4.90	3.30	5.18	0.03	38.93	183
Contract Value	690’019.83	458’394.79	714’775.21	43’973.44	5’180’863.95	183

Note: “Std”, “Min”, “Max”, and “Obs” denote the standard deviation, minimum, maximum, and number of observations, respectively.

5.4 Empirical Analysis using Machine Learning

We apply machine learning methods to train and test models for predicting bid-rigging cartels based on the screens presented in Section 3. Specifically, we consider two approaches: Lasso regression (see *Tibshirani*, 1996) for logit models and a so called ensemble method that consists of a weighted average of several algorithms, in our case bagged regression trees (see *Breiman*, 1996), random forests (see *Ho*, 1995; *Breiman*, 2001), and neural networks (see *McCulloch and Pitts*, 1943; *Ripley*, 1996).

5.4.1 Lasso regression

We subsequently discuss prediction based on lasso logit regression as well as the evaluation of its out of sample performance. First, we randomly split the data into two subsamples. The so-called training sample contains 75% of the total of observations and is to be used for estimating the model parameters. The so-called test sample consists of 25% of the observations and is to be used for out of sample prediction and performance evaluation. After splitting, the presence of a cartel is estimated in the training sample as a function of a range of predictors, namely the original screens as well as their squares and interaction terms to allow for a flexible functional relation.

Lasso estimation corresponds to a penalized logit regression, where the penalty term restricts the sum of absolute coefficients on the regressors. Depending on the value of the penalty term, the estimator shrinks the coefficients of less predictive variables towards or even exactly to zero and therefore allows selecting the most relevant predictors among a possibly large set of candidate variables. The estimation of the lasso logit coefficients is based on the following optimization problem:

$$\max_{\beta_0,\beta}\left\{\sum_{i=1}^{n}\left[y_i\left(\beta_0+\sum_{j=1}^{p}\beta_j x_{ij}\right)-log\left(1+e^{\beta_0+\sum_{j=1}^{p}\beta_j x_{ij}}\right)\right]-\lambda\sum_{j=1}^{p}|\beta_j|\right\}. \tag{5.7}$$

β_0, β denote the intercept and slope coefficients on the predictors, respectively, x is the vector of predictors, i indexes an observation in our data set (with n being the number of observations), j indexes a predictor (with p being the number of predictors), and λ is a penalty term larger than zero.

We use the lasso logit procedure in the "hdm" package for the statistical software "R" by *Spindler et al.* (2016) and select the penalty term λ such that it minimizes the mean squared error, which we estimate by 15-fold cross-validation. This is performed by randomly splitting the training sample into 15 subsamples, also called folds. 14 folds are used to estimate the lasso coefficients under different candidate values for the penalty term. 1 fold is used as so-called validation data set for predicting cartels based on the different sets of coefficients related to the various penalties and for computing the mean squared error (MSE). The latter corresponds to the average of squared differences between the prediction and the actual presence of a cartel in the validation data. The role of folds is then swapped in the sense that each of them is used once as validation data set and 14 times for coefficient estimation, yielding 15 MSEs per penalty term. The optimal penalty term is chosen as the value that minimizes the average over the 15 respective MSE estimates.

In a next step, we run lasso logit regression in the (entire) training sample based on the (sample-size adjusted) optimal penalty term to estimate the coefficients. Finally, we use these coefficients to predict the collusion probability in the test sample. To assess the performance of out of sample prediction, we consider two measures: first, the MSE of the predicted collusion probabilities in the test sample and second, the share of correct classifications. To compute the latter measure, we create a variable which takes the value one for predicted collusion probabilities greater than or equal to 0.5 and zero otherwise, and compare it to the actual incidence of collusion in the test sample. We repeat random sample splitting into 75% training and 25% test data and all subsequent steps previously mentioned 100 times and take averages of our performance measures over the 100 repetitions.

5.4.2 Ensemble method

Prediction and performance evaluation for the ensemble method has in principle the same structure as for the lasso approach. The difference is that rather than lasso logit regression, any estimation step now consists of a weighted average of bagged classification trees, random forests, and neural networks. The first two algorithms depend on tree methods, i.e. recursively splitting the data into subsamples in a way that minimizes the sum of squared differences of actual incidences of collusion from the collusion probabilities within the subsamples. Both methods estimate the trees in a large number of samples repeatedly drawn from the original data and obtain predictions of collusion by averaging over the tree (or splitting) structure across samples. However, one difference is that bagging considers all explanatory variables as candidates for further data splitting at each step, while random forests only use a random subset of the total of variables to prevent correlation of trees across samples. Finally, neural networks aim at fitting a system of functions that flexibly and accurately models the influence of the explanatory variables on collusion. We note that for these three machine learning algorithms, no higher order or interaction terms are included in addition to the original screens, as they are (in contrast to lasso logit) inherently nonparametric.

Cross-validation in the training sample determines the optimal weight each of the three machine learning algorithm obtains in the ensemble method, just analogously to the determination of the optimal penalty term in lasso regression. To this end we apply the "SuperLearner" package for "R" by *van der Laan et al.* (2008) with default values for bagged regression tree, random forest, and neural network algorithms in the "ipredbagg", "cforest", and "nnet" packages, respectively. The optimal combination of algorithms is then used to predict the collusion probabilities in the test sample and to compute the performance measures.

5.4.3 Empirical results

Table 5.3 reports the out of sample performance of the lasso and ensemble methods in the total of the test data, as well as separately for periods with and without collusion. Both algorithms perform similarly well in terms of the MSE and the share of correctly classified cartels in out of sample data containing both cartel and post-cartel periods. The ensemble method slightly dominates with an MSE of 0.12 and a correct classification rate of 83%, compared to 0.13 and 82% for lasso. When considering cartel periods only, both methods have a very similar MSE of roughly 0.08, but the lasso performs slightly better in terms of the correct classification (of collusion) with a rate of 91%, compared to 88% for the ensemble method. The latter, however, works better in non-cartel periods: the MSE and the correct classification rate (of no collusion) amount to 0.18 and 76%, respectively, while the lasso attains 0.20 and 69%. It is worth noting that both methods perform considerably better in cartel rather than competitive periods.

Table 5.3: Performance of the lasso and ensemble methods

	MSE	MSE.cart	MSE.comp	corr	corr.cart	corr.comp
lasso	0.127	0.084	0.196	0.824	0.907	0.690
ensemble	0.120	0.082	0.181	0.834	0.881	0.760

Note: “MSE”, “MSE.cart”, “MSE.comp” denote the mean squared errors in the total sample, in cartel periods, and in periods with competition, respectively. “corr”, “corr.cart”, and “corr.comp” denote rates of correct classification in the total sample, in cartel periods, and in periods with competition, respectively.

From a policy perspective, incorrectly classifying cases of non-collusion as collusion (false positives) and thus, unnecessarily filing an investigation, might be relatively more sensitive than incorrectly classifying cases of collusion as non-collusion (false negatives) and thus not detecting a subset of bid-rigging cartels. A way to reduce the incidence of incorrect classifications of actual non-collusion is raising the probability threshold for classifying a prediction as collusion from 0.5 to some higher value between 0.5 and 1. This, however, generally comes at the cost of reducing the likelihood of detecting actual cartels and increases therefore the false negative results. Competition agencies therefore need to appropriately trade off the likelihood of false positive and false negative results to derive an optimal rule concerning the probability threshold.

To see the tradeoffs in classification accuracy, Figure 5.3 reports the correct classification rates of either method in the total test data as well as separately for periods with and without collusion across different probability thresholds. As expected, the correct classification rate in competitive periods increases in the probability threshold for the decision rule. The improvement is steeper for the lasso estimator, which starts out at an inferior level for a threshold of 0.5, but slightly outperforms the ensemble method after 0.8. In contrast, the correct classification rate in cartel periods deteriorates much faster in the threshold when using the lasso estimator rather than the ensemble method. For a threshold of 0.9, the rate is at 38% for the lasso (i.e. substantially worse than flipping a coin), while it is still at 59% for the ensemble method. For this reason, the overall correct classification rate in the total of the test data decreases slower for the ensemble estimator than for the lasso estimator. For a probability threshold of 0.9, the share of correct predictions amounts to 72% for the former, but just 60% for the latter method.

From a policy perspective, a probability threshold value of 0.7 for the decision rule appears to be

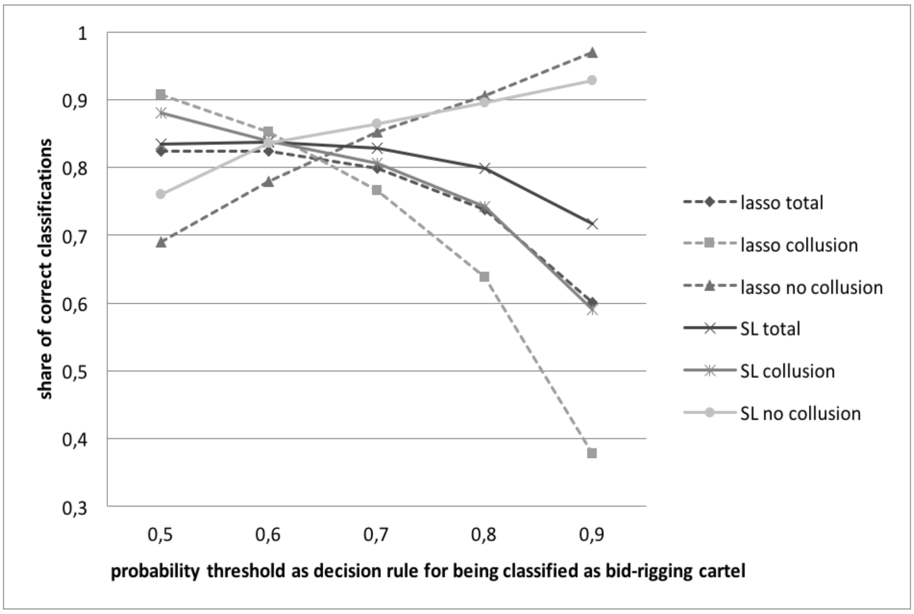


Figure 5.3: False positive and false negative results by tightening the decision rule

a pertinent choice. Lasso correctly classifies 77% of the collusive tenders and 85% of the competitive tenders, whereas the ensemble method correctly classifies correctly 80% of the collusive tenders and 86% of the competitive tenders. Even if the ensemble method does slightly better than lasso, their performances are quite similar. When further tightening the decision rule by raising the probability threshold to 0.8, both methods correctly classify roughly nine out of ten competitive tenders, while false negative results increase: the ensemble algorithm still detects 3 out of 4 bid-rigging cartels when collusion is present and therefore outperforms the lasso estimator with identifying roughly 2 out 3 bid-rigging cartels. The advantage of combining screening methods and machine learning consists in quantifying the trade-off of false positives and false negatives such that competition agencies are capable to determine the decision rule that best suits their needs.

To judge the relative importance of predictors for determining collusion, Table 5.4 reports the average absolute values of lasso coefficients across regressions in the 100 trainings samples (obtained by repeatedly splitting the total sample into training and test samples) that are larger than or equal to 0.03. It needs to be pointed out that in general, the estimates do not allow inferring on causal associations, as lasso may importantly shrink the coefficient of a relevant predictor if it is highly correlated with another relevant predictor.[5] Nevertheless, Table 5.4 allows spotting the most prominently selected predictors among the total of regressors provided in the lasso regressions. We observe that

[5]We also note that in our framework, causality goes from the dependent variable to the predictors rather than the other way round as it would be the case in contexts of causal inference. In causal terms, it is the incidence of collusion which as explanatory variable affects the distribution of bids and thus the screens, which can be regarded as outcome variables. Our prediction problem therefore consists of analysing a reverse causality: By investigating the screens, one infers on the existence of their cause, namely collusion.

the alternative relative difference (ALTRD) and the coefficient of variation (CVBID) have by far the highest predictive power.

Table 5.4: Average absolute values of important lasso coefficients

ALTRD	0.699
CVBID	0.394
NBRBID	0.039
SKEW	0.035
RD	0.030

Note: “ALTRD”, “CVBID”, “SKEW”, and “RD” denote the the alternative relative difference, the coefficient of variation, the number of bids, the skewness, and the relative difference, respectively.

Table 5.5 reports the coefficients of standard logit regressions using various sets of screens according to their importance in lasso regression as indicated in Table 5.4. For the statistically significant variables, we confirm the expectations concerning the behavior of the screens inferred from the Ticino case. The coefficient of variation (CVBID) is negatively related with the probability of collusion. That is, bid rigging decreases the coefficient of variation, even conditional on the other predictors used in the four different specifications. In contrast, the alternative relative difference (ALTRD) has a (conditionally) positive association. This implies that the distribution of the bids converges and that the differences between the second and first lowest bids are significantly higher than the differences between the losing bids. The same pattern can be observed for the relative difference (RD), for which the association is, however, statistically insignificant. The coefficients of the number of bidders (NBRBIDS) are positive and therefore go against the expectation that a higher number of bidders should increase competition, but are never statistically significant at the 5% level. Skewness (SKEW) does not show any statistically significant association either, at least conditional on the other, more predictive screens.

Table 5.6 reports the “marginal” effects (albeit not to be interpreted causally in our predictive framework) for the average observation in the sample (i.e. at the sample means of the predictors) and various sets of screens. In accordance with the estimates in Table 5.5, the coefficient of variation and the alternative relative distance, which are significant in all models, have in absolute terms by far the largest marginal effects for explaining the probability of collusion. Augmenting the coefficient of variation by one unit decreases the probability of collusion by roughly 10 percentage points, while a one unit increase in alternative distance makes the probability increase by 16 to 20 percentage points, depending on the model. The marginal effects of the other predictor a much closer to zero and not statistically significant at the 5% level.

Table 5.5: Logit coefficients for selected screens

	(1)	(2)	(3)	(4)
Constant	0.82	1.04**	1.02**	1.51***
	(0.56)	(0.51)	(0.5)	(0.4)
CVBID	-0.48***	-0.49***	-0.49***	-0.47***
	(0.06)	(0.06)	(0.06)	(0.06)
ALTRD	0.73**	0.89***	0.92***	0.95***
	(0.29)	(0.23)	(0.17)	(0.16)
NBRBIDS	0.13*	0.10	0.09	
	(0.07)	(0.06)	(0.06)	
SKEW	0.04	-0.05		
	(0.23)	(0.21)		
RD	0.17			
	(0.19)			
Obs	483	483	483	483
R^2	0.41	0.41	0.41	0.41

Note: "ALTRD", "CVBID", "SKEW", and "RD" denote the the alternative relative difference, the coefficient of variation, the number of bids, the skewness, and the relative difference, respectively. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively. "Obs" and "R^2" denote the number of observations and the pseudo R squared.

Table 5.6: Marginal effects for selected screens

	(1)	(2)	(3)	(4)
CVBID	-0.10***	-0.11***	-0.11***	-0.10***
	(0.02)	(0.02)	(0.02)	(0.01)
ALTRD	0.16**	0.20***	0.20***	0.20***
	(0.06)	(0.05)	(0.03)	(0.03)
NBRBIDS	0.03*	0.02	0.02	
	(0.02)	(0.01)	(0.01)	
SKEW	0.01	-0.01		
	(0.05)	(0.05)		
RD	0.04			
	(0.04)			

Note: "ALTRD", "CVBID", "SKEW", and "RD" denote the the alternative relative difference, the coefficient of variation, the number of bids, the skewness, and the relative difference, respectively. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

5.5 Policy Implications

The results of the previous section demonstrate the usefulness of simple screens combined with machine learning, amounting to an out of sample rate of correct classifications of 82% and 83% for the considered lasso and ensemble approaches, respectively. They confirm the observations and assumptions drawn from the Ticino case for all the screens used in the regressions. Furthermore, we discussed that the machine learning approach allows trading off the likelihood of false positive and false negative predictions by changing the probability threshold in a way that is considered optimal by a competition agency. This appears to be an important innovation in the literature on detecting collusion, as to the best of our knowledge no other study has directly assessed the performance of their method with respect to false positive and false negative results. We subsequently discuss some further implications of our method for policy makers, namely its advantages in terms of data requirements and use, its apparent generalizability to different empirical contexts of collusion, and its integration in a process of ex-ante detection of collusion.

5.5.1 Data requirements and data use

The method proposed in this paper has several advantages relative to other studies in the field. First, data requirements are comparably low. Implementing the machine learning approach and calculating the screens is straightforward and solely relies on information coming from official records of the bid opening and the bids submitted. Not even the identification of bidders is essential, which allows collecting information and implementing the method discretionarily, without attracting the attention of a cartel. This appears crucial, because if some cartel gets aware of the process, it might destroy evidence such that opening an investigation would be unsuccessful. In contrast, other detection methods as the econometric tests proposed by *Bajari and Ye* (2003) require data on the cost variables, which are difficult to obtain without having access to firm level data. This may compromise the secrecy in which competition agencies should implement any method aimed at detecting collusion.

We note that if the official records of the bid openings are numerous and representative for a large share of the market, it may be possible to compute cost variables for an econometric estimation of the bidding function even without directly accessing firm level data, (see *Imhof*, 2017a). Even in this case, the construction of the cost variables might be complex and time consuming (relative to the simple screens) and therefore potentially wasteful for competition agencies, which should ideally concentrate their resources on the prosecution of cartels. The resource argument is particularly relevant in the light of the high number of false negatives produced by the method of *Bajari and Ye*

(2003) when applied to the Ticino case, see *Imhof* (2017a), implying that such cost variable-based approaches might have low power.

Finally, the combination of screening and machine learning allows assessing and gauging the accuracy of classifying collusion out of sample, which is relevant for data yet to be analysed. It therefore tells us something about how well past data can be used to predict collusion in future data. To the best of our knowledge, none of the other detecting methods has so far properly assessed out of sample performance based on distinguishing between training and test data. Furthermore, by applying cross-validation to tune the algorithms, we aim at defining the best predictive model for the screens at hand, while other approaches neglect this optimization step w.r.t. model selection. Wrapping up, our paper appears to be the only one in the literature on detecting collusion that acknowledges the merits of machine learning for optimizing the predictive performance of estimators and for appropriately assessing their out of sample behavior.

5.5.2 Generalization of results

An important question for the attractiveness of our method is whether it yields decent results also in other contexts than that investigated in this paper. We expect our approach to perform well even in other industries or countries whenever the procurement procedure is similar to that considered in this paper, i.e. corresponding to a first-sealed bid auction. Furthermore, the transferability of our approach is facilitated by the use of several, distinct screens that are sensitive to different features of the distribution of bids and thus potentially cover different bid-rigging mechanisms.

Because the detection method based on simple screens is inductive, it, however, needs to be verified in the empirical context at hand. As competition agencies typically have access to data from former cases, they could easily apply the suggested screens to check their appropriateness even in different industries or countries. Moreover, if the data base is large enough, an agency might directly estimate its own predictive model based on screening and machine learning to identify suspicious tenders. This is fundamentally different to establishing rigorous models for testing collusion in a deductive approach, see *Bajari and Ye* (2003). While deduction allows for systematic generalization if the model is correctly specified, there is the threat that a specific empirical context does not match the model parametrization and hypotheses it is based upon. At least for the Ticino case, *Imhof* (2017a) has shown that simple screens outperform the econometric tests proposed *Bajari and Ye* (2003). Combining screening with machine learning makes this flexible, data-driven approach attractive as a generally applicable tool for detecting collusion.

5.5.3 Ex-ante detection of collusion

The trained predictive models obtained by machine learning can be applied to newly collected data in order to screen tenders for bid-rigging in an ex ante procedure. Following *Imhof et al.* (2017), we outline possible steps of such a procedure.

Initially, a competition agency has to collect data from official records of the bid opening and compute the screens for each tender as outlined in Section 3. Applying the trained models, e.g. the lasso coefficients from the training data, to these screens allows computing the predicted collusion probabilities in the newly collected data set. Next, the competition agency needs to select a probability threshold to flag tenders as suspicious or competitive. Based on our results, using a threshold of 0.7 appears sensible, but this could be reconsidered in other empirical contexts. We stress that the determination of suspicious tenders of such an approach is not (exclusively) based on human judgement, but data-driven and its accuracy generally improves with the amount of observations used to train the models.

Once suspicious tenders have been identified, there appear two possible options concerning next steps. The first consists of immediately opening an investigation, the second one of substantiating the initial suspicion. The decision to launch an investigation should be driven by the predicted results based on machine learning. If the detection method classifies a large share of tenders in a specific period as collusive, competition agencies might want to initiate a deeper investigation immediately. For instance, a share of more than 50% may appear sufficient to inquire the opening of an investigation. In contrast, shares between 20% and 50% might seem too small to launch a (potentially costly) investigation. In this case, the market might be analyzed further to substantiate the initial suspicion.

Several approaches may be used to substantiate the initial suspicion for bid-rigging. First, the firms participating in the suspicious tenders could be more closely examined to identify a specific group logic. In order to have a well-functioning bid-rigging cartel, firms must cooperate regularly over a certain period. Regular interactions between firms might make it possible to find a particular group logic in suspicious tenders. Second, geographical analysis may help identifying bid-rigging cartels situated in particular regions. One therefore needs to determine where the suspicious tenders are localized. If they are all clustered in the same area, this might point to a local bid-rigging cartel. (Note also that if one identifies a local bid-rigging cartel, one should generally also identify a colluding group of firms based in the region.) Third, *Imhof et al.* (2017) assumes that bid-rigging cartels produce a rotational pattern, which one can detect by a collusive interaction test. If one is able to determine a specific group of firms regularly participating in suspicious tenders (e.g. in some region) and find that contract placement in the potential bid-rigging cartel operates in a rotational scheme, this provides further indices for substantiating the initial suspicion.

The steps proposed by *Imhof et al.* (2017) are not exhaustive and competition agencies might want to perform further tests and checks (e.g. according to recommendations of the OECD) to substantiate their initial suspicion. At the end of the process, the competition agencies should ideally be capable to credibly demonstrate that the suspicious bids are not coincidental, but follow an identifiable logic of collusion.

5.6 Conclusion

In this paper, we combined two machine learning algorithms, namely lasso regression and an ensemble method (including bagging, random forests, and neural networks), with screens for predicting collusion in tender procedures within the Swiss construction sector that are based on patterns in the distribution of bids. We assessed the out of sample performance of our approach by splitting the data into training samples for model parameter estimation and test samples for model evaluation. More than 80% of the total of bidding processes were correctly classified by both lasso regression and the ensemble methods as collusive or non-collusive. As the correct classification rate, however, differs across truly non-collusive and collusive processes, we also investigated tradeoffs in reducing false positive vs. false negative predictions. That is, rather than classifying a tender process as collusive whenever the collusion probability predicted by machine learning is greater than or equal to 0.5, one could use a higher probability threshold for classification, e.g. 0.7. This reduces incorrect predictions among truly non-collusive processes, i.e. false positives, at the cost of somewhat increasing errors among truly collusive processes, i.e. false negatives.

We demonstrated that setting the probability threshold to 0.7 in our data entailed acceptably low shares of both false positives and false negatives. Lasso regression correctly classified 77% of collusive tenders and 85% of competitive tenders, whereas the ensemble method correctly classified 80% of collusive tenders and 86% of competitive tenders. Finally, we discussed several policy implications for competition agencies aiming at detecting bid-rigging cartels, namely advantages of our method in terms of data requirements/use, its generalizability to different empirical contexts of collusion, and its integration in a process of ex-ante detection of collusion.

Conclusion

The PhD thesis has shown that a detection method based on simple screens can uncover bid-rigging cartels. Simple screens perform well to capture the impact of bid rigging in the distribution of the bids, as shown in chapter 3. They perform far better than the econometric tests proposed by *Bajari and Ye* (2003) investigated in chapter 2. The poor performance of the econometric tests can be related to a certain extent to the specificity of the Ticino cartel. Indeed, chapter 1 has shown that the Ticino cartel allocated contracts using a bid rotation scheme based on costs. However, if the econometric tests of Bajari cannot detect the bid-rigging cartels, simple screens can easily do it. In any case, the results of chapter 2 suggest considering the econometric tests proposed by Bajari with caution.

One-step further, chapter 4 shows that a detection method based on simple screens can provide a sufficient suspicion for the opening of an investigation. In addition, simple screens can also tackle the problem of partial collusion, i. e., when firms collude on a subset of contracts. Finally, chapter 5 shows that the detection method based on simple screens exhibits a high prediction rate by using data of 4 different bid-rigging cartels. Therefore, we conclude that the performance of the simple screens is not case-specific and we generalize their application for the construction sector in Switzerland.

The method based on simple screens fits the need of competition agencies. It is simple to apply and uses publicly available data. Therefore, competition agencies can run the tests in secrecy and broadly without drawing the cartel attention. In addition, the simple screens are flexible and may be adaptable to other cases or industries. The PhD thesis does not only present the application of one detection method but also explains how other competition agencies or researchers can construct a detection method based on simple screens. Such detection method is mainly inductive. In other words, if the direct replication of the detection method proposed in this PhD thesis can be questionable, the construction of the method can be replicated in other countries or for other industries. For instance, if competition agencies have previous benchmarks for bid-rigging cases, they can rely on them to construct an adjusted detection method based on simple screens fitting the specificity of their cases.

However, the author considers that the detection method proposed in this PhD thesis can be directly applied in similar context with minor adjustments. Two prerequisites ensure the similarity of the context. First, the bid-rigging cartel should function through contract allocation, without monetary transfer. Second, the procurement procedure should be a first-price sealed-bid auction, where the price is not the unique criterion, but an essential one. In such cases, the adjustment of the detection method is minimal, if necessary at all.

The author purposes to carry on researching this field. It is now crucial to access data from different industries and/or different countries in order to test and improve the detection method based on simple screens. The simple screens presented in the chapter 3 can also be extended and refined. One goal should be to search for a "superscreen" capable to improve the prediction rate of simple screens indicated in the chapter 5. The author believes also that it is important to study different auction mechanisms to see how the screens react. Finally, the assumptions proposed in chapter 3 should constitute the basis for building a model, which can describe the optimal functioning of bid rigging in a first-price sealed-bid auction, where the price is not the unique criterion, but an essential one. At the end of this PhD thesis, many enticing ideas emerge and the author intends to pursue them.

Chapter 6

Appendix

6.1 Appendix for chapter 2

6.1.1 Test of the conditional independence

Table 6.1: Test of the conditional independence for the cartel period

Pair of firms	*N*	*Pearson coeff.*	*Z − statistic*
(6, 18)	19	−0.667	−3.2214
(3, 24)	5	−0.9718	−3.0039
(23, 24)	25	−0.5595	−2.9645
(12, 17)	9	−0.8198	−2.8318
(8, 17)	31	−0.4709	−2.705
(9, 12)	13	0.6652	2.5363
(23, 25)	13	−0.6459	−2.4294
(24, 26)	7	−0.8221	−2.3265
(14, 15)	34	−0.3894	−2.2891
(6, 20)	14	−0.5769	−2.1818
(4, 24)	21	−0.4655	−2.1394
(21, 22)	9	−0.7013	−2.1308
(9, 25)	5	−0.8968	−2.0584
(9, 17)	49	−0.2917	−2.0379
(6, 14)	24	−0.4041	−1.9635
(16, 18)	5	−0.8606	−1.8323
(4, 23)	17	−0.4527	−1.8262
(11, 24)	5	0.8564	1.8097
(11, 20)	9	−0.6246	−1.7944
(11, 15)	18	−0.427	−1.7671
(4, 17)	45	−0.2657	−1.7639
(17, 18)	17	−0.4365	−1.7509

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$Pair\ of\ firms$	N	$Pearson\ coeff.$	$Z-statistic$
(14,19)	19	−0.4017	−1.7027
(4,19)	31	0.3076	1.6823
(3,11)	10	−0.5456	−1.6196
(22,23)	16	−0.4178	−1.6044
(11,14)	17	−0.4031	−1.5988
(4,9)	58	−0.2113	−1.591
(5,12)	8	−0.608	−1.5781
(11,18)	15	−0.4134	−1.5233
(4,14)	36	−0.2571	−1.5108
(14,18)	25	0.3078	1.4922
(4,15)	51	−0.212	−1.4913
(6,19)	19	−0.3532	−1.4765
(8,18)	10	−0.4987	−1.4489
(3,17)	9	−0.5189	−1.408
(9,10)	33	−0.2451	−1.3704
(4,10)	31	−0.2499	−1.3511
(8,15)	30	−0.2537	−1.3475
(9,19)	34	−0.2269	−1.2857
(10,16)	7	0.566	1.2832
(3,9)	12	0.4033	1.2828
(16,17)	8	−0.5064	−1.2474
(4,18)	24	0.2541	1.1904
(15,24)	9	−0.437	−1.1476
(3,20)	6	0.5748	1.1339
(4,22)	7	0.5002	1.0992
(22,24)	13	−0.3289	−1.0801
(8,10)	16	−0.286	−1.0607
(21,24)	9	−0.4076	−1.06
(15,16)	9	−0.3961	−1.0264
(4,25)	8	−0.4196	−1.0001
(6,9)	29	−0.1926	−0.9946
(17,21)	5	0.6028	0.9866
(11,17)	14	0.2854	0.9737
(10,15)	32	−0.1774	−0.9653
(15,19)	31	−0.1803	−0.9646
(3,18)	14	−0.2781	−0.9474
(3,10)	6	−0.4898	−0.928
(5,19)	5	−0.5661	−0.9076
(6,17)	24	0.1945	0.9029

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$Pair\,of\,firms$	N	$Pearson\,coeff.$	$Z-statistic$
(14, 20)	15	0.2466	0.8723
(17, 20)	29	−0.1671	−0.8602
(6, 11)	15	0.2416	0.8538
(5, 20)	6	−0.4531	−0.8464
(4, 11)	21	−0.1878	−0.8061
(10, 17)	24	−0.171	−0.7914
(3, 15)	12	0.2575	0.7903
(6, 15)	26	−0.161	−0.7791
(17, 19)	26	−0.1583	−0.7658
(21, 23)	11	−0.2601	−0.7529
(15, 21)	5	−0.4868	−0.7522
(20, 24)	6	−0.4058	−0.7457
(4, 20)	31	−0.1378	−0.734
(18, 24)	9	0.2848	0.7173
(10, 20)	18	−0.1718	−0.672
(8, 11)	10	0.2485	0.6715
(18, 20)	12	0.2053	0.6248
(9, 14)	38	0.102	0.6058
(9, 24)	10	0.2204	0.5928
(12, 19)	5	−0.3875	−0.5783
(17, 24)	10	−0.2132	−0.5727
(8, 24)	5	−0.3642	−0.5398
(16, 20)	6	0.301	0.5379
(18, 23)	5	−0.3626	−0.5372
(3, 4)	13	0.1655	0.5283
(14, 17)	25	−0.1121	−0.5282
(6, 8)	15	0.1512	0.5279
(8, 14)	19	−0.1291	−0.5193
(9, 11)	19	−0.1256	−0.5049
(16, 19)	5	−0.3342	−0.4915
(6, 24)	6	0.2743	0.4875
(19, 20)	18	−0.1243	−0.4838
(15, 17)	45	−0.0729	−0.4735
(12, 20)	5	−0.3221	−0.4723
(9, 15)	62	−0.0595	−0.4576
(4, 21)	9	−0.1772	−0.4387
(19, 23)	6	0.2354	0.4155
(8, 19)	18	−0.1049	−0.4076
(14, 16)	5	0.273	0.3961

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$Pair\, of\, firms$	N	$Pearson\, coeff.$	$Z-statistic$
$(9,16)$	10	0.1486	0.396
$(12,15)$	10	0.143	0.381
$(15,20)$	27	0.0753	0.3697
$(6,16)$	8	0.1638	0.3696
$(9,18)$	22	−0.0837	−0.3656
$(4,8)$	32	−0.0669	−0.3606
$(8,16)$	7	−0.1765	−0.3568
$(15,18)$	21	0.0817	0.3473
$(10,18)$	11	−0.1157	−0.3288
$(3,14)$	14	0.095	0.3159
$(24,25)$	5	−0.2158	−0.31
$(6,23)$	6	0.1747	0.3058
$(10,11)$	9	−0.1195	−0.2941
$(14,24)$	8	−0.1289	−0.2898
$(10,19)$	23	−0.0631	−0.2827
$(5,17)$	6	−0.1594	−0.2785
$(23,26)$	9	−0.1057	−0.2599
$(6,10)$	15	0.0735	0.2549
$(10,14)$	16	0.069	0.2493
$(22,25)$	5	−0.1697	−0.2423
$(8,9)$	33	−0.0441	−0.2417
$(4,16)$	10	−0.0881	−0.2336
$(17,23)$	6	0.1321	0.2302
$(5,9)$	8	−0.0929	−0.2083
$(3,8)$	7	−0.0865	−0.1734
$(18,19)$	9	0.0695	0.1706
$(9,21)$	7	−0.0692	−0.1387
$(3,6)$	7	−0.0616	−0.1233
$(8,20)$	20	0.0176	0.0725
$(4,6)$	34	−0.0095	−0.0528
$(9,20)$	30	0.007	0.0365
$(5,15)$	6	0.0137	0.0238
$(11,19)$	8	−0.01	−0.0223

Note: ”Pair of firms”, ”N”, ”Pearson coeff.” and ”Z-statistic” denote the firms in the pair tested, the number of pairwise observations for the pair and the calculated Z-statistic as presented in section 2.5.1, respectively.

Table 6.2: Test of the conditional independence for the post-cartel period

$Pair\,of\,firms$	N	$Pearson\,coeff.$	$Z-statistic$
(4,9)	13	−0.7623	−3.1676
(6,8)	6	0.926	2.8227
(15,19)	12	−0.673	−2.4487
(9,15)	18	−0.5396	−2.3378
(4,8)	9	−0.7177	−2.2117
(8,9)	11	0.6227	2.0632
(4,6)	9	−0.6683	−1.9784
(4,16)	5	−0.8685	−1.8768
(9,18)	5	0.8682	1.8746
(8,19)	8	−0.666	−1.7968
(14,19)	5	0.8029	1.5653
(10,17)	10	−0.5271	−1.5508
(10,15)	6	−0.6876	−1.4609
(6,15)	7	−0.6204	−1.4514
(6,14)	7	0.5959	1.3735
(6,10)	9	−0.4926	−1.3215
(6,19)	8	0.5055	1.2449
(16,19)	6	−0.6038	−1.2108
(4,15)	10	0.4023	1.1282
(9,17)	18	−0.2665	−1.0577
(14,16)	6	0.499	0.949
(16,17)	8	−0.3692	−0.8665
(4,18)	5	−0.5446	−0.8635
(10,14)	5	−0.5317	−0.8379
(6,16)	6	0.4229	0.7816
(9,14)	9	−0.2988	−0.755
(8,16)	5	0.472	0.725
(4,14)	6	−0.3882	−0.7096
(14,15)	5	−0.4514	−0.6879
(4,19)	8	−0.298	−0.6873
(8,10)	7	−0.2996	−0.6181
(8,17)	11	−0.2106	−0.6048
(14,17)	9	−0.2293	−0.5719
(9,16)	6	−0.3066	−0.5486
(4,24)	5	−0.3674	−0.545
(4,17)	13	−0.1288	−0.4098
(15,17)	15	−0.1169	−0.4069
(15,16)	5	0.2616	0.3787
(17,18)	5	−0.2315	−0.3334

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$Pair\,of\,firms$	N	$Pearson\,coeff.$	$Z-statistic$
$(6,9)$	12	-0.1081	-0.3257
$(10,19)$	8	-0.1388	-0.3123
$(9,10)$	12	-0.0576	-0.1731
$(6,17)$	11	0.0559	0.1584
$(9,19)$	12	0.0527	0.1582
$(17,19)$	13	0.0442	0.1398
$(4,10)$	8	0.0264	0.059
$(8,15)$	9	-0.0221	-0.0541

Note: ”Pair of firms”, ”N”, ”Pearson coeff.” and ”Z-statistic” denote the firms in the pair tested, the number of pairwise observations for the pair and the calculated Z-statistic as presented in section 2.5.1, respectively.

Table 6.3: Test of the conditional independence only for the cover bids

$Pair\,of\,firms$	N	$Pearson\,coeff.$	$Z-statistic$
(6, 14)	20	−0.7212	−3.7526
(6, 18)	12	−0.7717	−3.0736
(4, 22)	5	0.9659	2.8673
(4, 23)	8	−0.8519	−2.824
(4, 17)	37	−0.437	−2.7318
(11, 20)	7	−0.8682	−2.6516
(9, 15)	50	0.3498	2.5038
(10, 17)	17	−0.5791	−2.4737
(14, 19)	17	−0.541	−2.2656
(9, 12)	5	0.9175	2.2246
(6, 20)	14	−0.5769	−2.1818
(23, 26)	5	−0.9024	−2.0997
(22, 23)	7	−0.7717	−2.049
(11, 18)	12	−0.5861	−2.015
(14, 18)	18	0.4578	1.9151
(11, 14)	15	−0.4947	−1.8785
(4, 19)	24	0.382	1.8438
(9, 14)	35	0.3124	1.8283
(15, 18)	14	0.4966	1.8067
(11, 15)	14	−0.4943	−1.7968
(9, 19)	30	−0.3318	−1.7917
(16, 17)	5	−0.8457	−1.7546
(17, 19)	24	−0.3639	−1.7477
(8, 10)	12	−0.5127	−1.6991
(3, 17)	5	−0.8246	−1.6561
(4, 14)	31	−0.2983	−1.6279
(6, 11)	12	0.4811	1.5731
(4, 16)	7	−0.6508	−1.5534
(17, 20)	26	−0.2959	−1.4628
(8, 15)	23	−0.312	−1.4433
(17, 18)	14	−0.4093	−1.4418
(11, 17)	13	0.408	1.3698
(8, 9)	28	−0.2557	−1.3076
(6, 9)	24	−0.276	−1.2986
(10, 16)	7	0.566	1.2832
(15, 20)	24	0.269	1.2639
(4, 9)	50	−0.1683	−1.1648
(8, 17)	23	−0.247	−1.1279
(8, 18)	7	−0.5039	−1.109

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$Pair\ of\ firms$	N	$Pearson\ coeff.$	$Z-statistic$
$(9,11)$	16	-0.2944	-1.094
$(18,19)$	6	0.5587	1.0927
$(10,15)$	25	0.217	1.034
$(4,24)$	17	-0.2599	-0.9952
$(6,8)$	12	-0.3148	-0.9775
$(3,11)$	6	-0.51	-0.9747
$(10,18)$	6	0.4878	0.9234
$(4,10)$	23	0.2033	0.9221
$(4,8)$	23	-0.2029	-0.92
$(22,24)$	10	-0.334	-0.9189
$(9,18)$	16	0.248	0.9133
$(14,20)$	15	0.2466	0.8723
$(23,25)$	5	-0.5408	-0.856
$(4,6)$	28	0.1644	0.8295
$(23,24)$	9	-0.3143	-0.7968
$(6,15)$	24	-0.1581	-0.7306
$(8,11)$	9	0.2866	0.7221
$(10,14)$	13	0.2142	0.6879
$(6,10)$	14	0.2015	0.6776
$(6,19)$	17	0.174	0.6576
$(9,16)$	8	-0.2847	-0.6546
$(10,20)$	15	0.1831	0.6414
$(3,14)$	10	-0.2366	-0.6382
$(14,17)$	24	-0.1383	-0.638
$(4,20)$	27	0.1271	0.6262
$(15,16)$	7	-0.3014	-0.622
$(15,17)$	38	-0.1016	-0.6032
$(4,18)$	19	0.1445	0.582
$(18,20)$	10	0.2132	0.5728
$(3,4)$	9	0.2282	0.5689
$(16,20)$	6	0.301	0.5379
$(6,24)$	5	0.3451	0.509
$(3,18)$	8	-0.2146	-0.4873
$(19,20)$	18	-0.1243	-0.4838
$(6,17)$	23	0.0961	0.4311
$(3,6)$	6	0.2314	0.4081
$(9,20)$	27	0.0789	0.3872
$(11,19)$	6	-0.218	-0.3838
$(10,19)$	21	-0.0852	-0.3623

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$Pair\,of\,firms$	N	$Pearson\,coeff.$	$Z-statistic$
$(17,24)$	5	0.229	0.3296
$(8,20)$	17	−0.0656	−0.246
$(19,23)$	5	0.1716	0.2451
$(6,16)$	6	0.1391	0.2424
$(9,17)$	41	−0.0393	−0.2421
$(14,24)$	5	0.166	0.2369
$(3,15)$	8	0.0983	0.2205
$(8,14)$	16	−0.0568	−0.2049
$(9,10)$	25	−0.0356	−0.167
$(15,19)$	27	−0.0311	−0.1522
$(8,19)$	15	0.0418	0.145
$(3,9)$	7	0.0658	0.1317
$(14,15)$	29	−0.0106	−0.054
$(18,24)$	7	−0.0239	−0.0478
$(4,15)$	41	0.0039	0.0237
$(10,11)$	6	0.005	0.0087
$(4,11)$	16	−0.0006	−0.0022

Note: ”Pair of firms”, ”N”, ”Pearson coeff.” and ”Z-statistic” denote the firms in the pair tested, the number of pairwise observations for the pair and the calculated Z-statistic as presented in section 2.5.1, respectively.

Table 6.4: Test of the conditional independence only for the direct cover bids

$Pair\,of\,firms$	N	$Direct\,cover\,bids$	$Pearson\,coeff.$	$Z-statistic$
(9,15)	62	12	−0.8866	−4.2174
(4,9)	58	8	−0.9434	−3.9536
(9,17)	49	8	−0.9385	−3.8579
(4,10)	31	8	−0.9317	−3.7368
(4,15)	51	10	−0.8783	−3.6201
(10,17)	24	7	−0.9403	−3.4813
(10,18)	11	5	−0.9834	−3.3822
(6,18)	19	7	−0.9321	−3.3483
(10,15)	32	7	−0.9294	−3.3079
(8,15)	30	7	−0.9243	−3.2355
(23,25)	13	8	−0.877	−3.0469
(23,24)	25	16	−0.6826	−3.0069
(9,10)	33	8	−0.8728	−3.0069
(8,17)	31	8	−0.8668	−2.9518
(4,23)	17	9	−0.8342	−2.9438
(6,9)	29	5	−0.9672	−2.8949
(14,15)	34	5	−0.9602	−2.7556
(4,6)	34	6	−0.9181	−2.7311
(9,18)	22	6	−0.9141	−2.688
(4,11)	21	5	−0.9554	−2.6733
(4,8)	32	9	−0.7831	−2.5801
(4,17)	45	8	−0.8148	−2.5517
(4,14)	36	5	−0.9424	−2.4877
(15,18)	21	7	−0.8098	−2.2529
(15,17)	45	7	−0.7483	−1.9382
(3,9)	12	5	0.8571	1.8135
(17,24)	10	5	−0.8332	−1.695
(22,23)	16	9	−0.5916	−1.666
(9,12)	13	8	0.5261	1.3075
(8,9)	33	5	−0.6101	−1.0028
(4,18)	24	5	−0.5846	−0.9467
(21,23)	11	8	0.2557	0.5847
(3,18)	14	6	−0.3209	−0.5762
(14,18)	25	7	0.2372	0.4836
(12,15)	10	6	0.1019	0.1771

Note: ”Pair of firms”, ”N”, ”Direct cover bids”, ”Pearson coeff.” and ”Z-statistic” denote the firms in the pair tested, the number of pairwise observations for the pair in the whole sample, the number of pairwise observation for the test (direct cover bids) and the calculated Z-statistic as presented in section 2.5.1, respectively.

6.1.2 Test of the exchangeability of the bids

Table 6.5: Test of the exchangeability of the bids for the cartel period

$Pair\, of\, firms$	N	$F-statistics$	$Df.$	$P-value$
(9,12)	13	9.0016	(4,534)	0.0000
(5,19)	5	6.927	(4,534)	0.0000
(5,9)	8	6.852	(4,534)	0.0000
(12,19)	5	6.2252	(4,534)	0.0001
(12,15)	10	5.7752	(4,534)	0.0002
(3,9)	12	4.9744	(4,534)	0.0006
(3,10)	6	4.6618	(4,534)	0.001
(5,15)	6	4.6344	(4,534)	0.0011
(3,4)	13	4.4522	(4,534)	0.0015
(5,20)	6	4.4016	(4,534)	0.0017
(4,8)	32	4.045	(4,534)	0.0031
(12,17)	9	3.7659	(4,534)	0.005
(8,9)	33	3.7452	(4,534)	0.0051
(8,17)	31	3.5372	(4,534)	0.0074
(3,15)	12	3.4913	(4,534)	0.0079
(9,18)	22	3.4605	(4,534)	0.0084
(12,20)	5	3.3704	(4,534)	0.0097
(8,24)	5	3.3377	(4,534)	0.0103
(8,15)	30	3.2351	(4,534)	0.0122
(10,11)	9	3.2208	(4,534)	0.0125
(4,11)	21	3.1885	(4,534)	0.0133
(9,11)	19	3.1263	(4,534)	0.0147
(3,8)	7	3.1031	(4,534)	0.0153
(4,18)	24	3.062	(4,534)	0.0164
(3,17)	9	2.9938	(4,534)	0.0184
(23,25)	13	2.9051	(4,534)	0.0213
(10,17)	24	2.8342	(4,534)	0.024
(10,18)	11	2.8341	(4,534)	0.024
(4,23)	17	2.8207	(4,534)	0.0246
(23,26)	9	2.8158	(4,534)	0.0248
(3,20)	6	2.7972	(4,534)	0.0255
(8,20)	20	2.7305	(4,534)	0.0285
(18,19)	9	2.712	(4,534)	0.0294
(9,21)	7	2.7031	(4,534)	0.0298
(8,18)	10	2.6991	(4,534)	0.03
(9,24)	10	2.5771	(4,534)	0.0367
(19,23)	6	2.5283	(4,534)	0.0398

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$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
$(5,17)$	6	2.4986	$(4,534)$	0.0418
$(17,23)$	6	2.4587	$(4,534)$	0.0446
$(6,23)$	6	2.4348	$(4,534)$	0.0464
$(9,17)$	49	2.4317	$(4,534)$	0.0466
$(14,18)$	25	2.397	$(4,534)$	0.04933
$(4,17)$	45	2.3517	$(4,534)$	0.0531
$(8,10)$	16	2.3232	$(4,534)$	0.0556
$(6,24)$	6	2.2839	$(4,534)$	0.0593
$(24,26)$	7	2.2576	$(4,534)$	0.0618
$(18,20)$	12	2.2523	$(4,534)$	0.0624
$(6,8)$	15	2.2275	$(4,534)$	0.0649
$(11,19)$	8	2.1581	$(4,534)$	0.0725
$(17,19)$	26	2.1227	$(4,534)$	0.0767
$(15,21)$	5	2.119	$(4,534)$	0.0772
$(11,15)$	18	2.0871	$(4,534)$	0.0812
$(24,25)$	5	2.082	$(4,534)$	0.0819
$(20,24)$	6	2.0247	$(4,534)$	0.0897
$(4,19)$	31	2.0179	$(4,534)$	0.0906
$(8,11)$	10	2.0086	$(4,534)$	0.092
$(4,21)$	9	2.0037	$(4,534)$	0.0927
$(17,24)$	10	1.9348	$(4,534)$	0.1033
$(15,18)$	21	1.9338	$(4,534)$	0.1035
$(6,18)$	19	1.9319	$(4,534)$	0.1038
$(4,22)$	7	1.8959	$(4,534)$	0.1098
$(11,24)$	5	1.8057	$(4,534)$	0.1263
$(17,21)$	5	1.7372	$(4,534)$	0.1404
$(3,6)$	7	1.7036	$(4,534)$	0.1478
$(9,14)$	38	1.6967	$(4,534)$	0.1493
$(17,18)$	17	1.6456	$(4,534)$	0.1614
$(22,23)$	16	1.6339	$(4,534)$	0.1643
$(3,14)$	14	1.6058	$(4,534)$	0.1714
$(14,24)$	8	1.5417	$(4,534)$	0.1887
$(15,17)$	45	1.5251	$(4,534)$	0.1935
$(9,25)$	5	1.5135	$(4,534)$	0.1968
$(21,22)$	9	1.5106	$(4,534)$	0.1977
$(8,14)$	19	1.5085	$(4,534)$	0.1983
$(9,19)$	34	1.492	$(4,534)$	0.2032
$(15,24)$	9	1.4854	$(4,534)$	0.2052
$(4,6)$	34	1.4818	$(4,534)$	0.2063

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$Pair of firms$	N	$F-statistics$	$Df.$	$P-value$
(4,25)	8	1.4536	(4,534)	0.2151
(8,19)	18	1.4308	(4,534)	0.22243
(4,14)	36	1.4074	(4,534)	0.2302
(8,16)	7	1.4047	(4,534)	0.2311
(11,20)	9	1.3945	(4,534)	0.2346
(3,11)	10	1.3899	(4,534)	0.23612
(10,14)	16	1.375	(4,534)	0.2413
(22,25)	5	1.365	(4,534)	0.2449
(11,17)	14	1.3607	(4,534)	0.2464
(11,14)	17	1.3123	(4,534)	0.2642
(4,24)	21	1.3105	(4,534)	0.2649
(16,18)	5	1.2626	(4,534)	0.2836
(4,9)	58	1.2286	(4,534)	0.2976
(14,17)	25	1.2174	(4,534)	0.3023
(5,12)	8	1.1416	(4,534)	0.336
(10,19)	23	1.1215	(4,534)	0.3455
(3,18)	14	1.1148	(4,534)	0.3487
(6,17)	24	1.0585	(4,534)	0.3764
(3,24)	5	1.0113	(4,534)	0.401
(6,10)	15	0.9976	(4,534)	0.4083
(9,15)	62	0.9811	(4,534)	0.4173
(18,24)	9	0.9753	(4,534)	0.4205
(22,24)	13	0.9731	(4,534)	0.4217
(18,23)	5	0.9612	(4,534)	0.4284
(6,9)	29	0.9598	(4,534)	0.4291
(19,20)	18	0.8989	(4,534)	0.4643
(14,15)	34	0.881	(4,534)	0.475
(17,20)	29	0.8448	(4,534)	0.4972
(21,23)	11	0.7895	(4,534)	0.5323
(15,19)	31	0.7231	(4,534)	0.5764
(10,15)	32	0.7183	(4,534)	0.5797
(4,15)	51	0.7002	(4,534)	0.5921
(4,20)	31	0.6913	(4,534)	0.5982
(9,10)	33	0.6701	(4,534)	0.6129
(16,17)	8	0.6629	(4,534)	0.618
(9,16)	10	0.6416	(4,534)	0.633
(14,19)	19	0.6354	(4,534)	0.6375
(4,16)	10	0.6337	(4,534)	0.6387
(6,19)	19	0.6212	(4,534)	0.6476

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$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
$(6,15)$	26	0.5975	$(4,534)$	0.6646
$(6,11)$	15	0.5814	$(4,534)$	0.6762
$(10,20)$	18	0.5728	$(4,534)$	0.6825
$(23,24)$	25	0.5511	$(4,534)$	0.6983
$(14,20)$	15	0.5138	$(4,534)$	0.7257
$(15,20)$	27	0.4836	$(4,534)$	0.7478
$(4,10)$	31	0.4759	$(4,534)$	0.7535
$(21,24)$	9	0.4731	$(4,534)$	0.7555
$(11,18)$	15	0.463	$(4,534)$	0.7629
$(16,20)$	6	0.4619	$(4,534)$	0.7637
$(6,20)$	14	0.4364	$(4,534)$	0.7824
$(14,16)$	5	0.4261	$(4,534)$	0.7899
$(16,19)$	5	0.4194	$(4,534)$	0.7947
$(10,16)$	7	0.3396	$(4,534)$	0.8513
$(6,14)$	24	0.3017	$(4,534)$	0.8769
$(6,16)$	8	0.2129	$(4,534)$	0.9313
$(15,16)$	9	0.1817	$(4,534)$	0.9479
$(9,20)$	30	0.0617	$(4,534)$	0.993

Note: ”Pair of firms”, ”N”, ”F-statistic”, ”Df.” and ”P-value” denote the firms in the pair tested, the number of pairwise observations for the pair, the calculated F-statistic as presented in section 2.5.2, the degree of freedom of the test and the associated P-value, respectively.

Table 6.6: Test of the exchangeability of the bids for the post-cartel period

$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
(8,16)	5	3.1582	(4,91)	0.0177
(10,19)	8	2.7524	(4,91)	0.0327
(16,19)	6	2.5916	(4,91)	0.0417
(9,14)	9	2.5847	(4,91)	0.0422
(10,14)	5	2.3966	(4,91)	0.056
(14,17)	9	2.1108	(4,91)	0.0858
(14,19)	5	2.1022	(4,91)	0.0869
(17,19)	13	2.0748	(4,91)	0.0905
(6,16)	6	1.9251	(4,91)	0.113
(6,9)	12	1.777	(4,91)	0.1403
(6,10)	9	1.7419	(4,91)	0.1476
(6,14)	7	1.7076	(4,91)	0.1551
(14,15)	5	1.4598	(4,91)	0.221
(6,19)	8	1.4532	(4,91)	0.223
(9,16)	6	1.4232	(4,91)	0.2326
(8,15)	9	1.4165	(4,91)	0.2348
(4,14)	6	1.3734	(4,91)	0.2494
(6,8)	6	1.3486	(4,91)	0.2581
(15,19)	12	1.331	(4,91)	0.2645
(9,10)	12	1.167	(4,91)	0.3307
(8,9)	11	1.1366	(4,91)	0.3444
(8,17)	11	1.1338	(4,91)	0.3456
(8,10)	7	1.0795	(4,91)	0.3714
(16,17)	8	1.0779	(4,91)	0.3722
(14,16)	6	1.0256	(4,91)	0.3985
(6,17)	11	1.0076	(4,91)	0.4078
(10,15)	6	0.9327	(4,91)	0.4487
(4,18)	5	0.9244	(4,91)	0.4534
(4,16)	5	0.9222	(4,91)	0.4547
(9,19)	12	0.8061	(4,91)	0.5245
(17,18)	5	0.8025	(4,91)	0.5267
(4,19)	8	0.7878	(4,91)	0.5361
(9,15)	18	0.7516	(4,91)	0.5595
(9,18)	5	0.72568	(4,91)	0.5767
(6,15)	7	0.6854	(4,91)	0.6039
(4,6)	9	0.6748	(4,91)	0.6112
(8,19)	8	0.5898	(4,91)	0.6709
(4,24)	5	0.5735	(4,91)	0.6826
(15,16)	5	0.4881	(4,91)	0.7444

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$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
$(10,17)$	10	0.4051	$(4,91)$	0.8045
$(15,17)$	15	0.3887	$(4,91)$	0.8162
$(4,10)$	8	0.3569	$(4,91)$	0.8386
$(9,17)$	18	0.2711	$(4,91)$	0.8959
$(4,15)$	10	0.2581	$(4,91)$	0.904
$(4,17)$	13	0.2563	$(4,91)$	0.9052
$(4,9)$	13	0.2223	$(4,91)$	0.9254
$(4,8)$	9	0.193	$(4,91)$	0.9415

Note: ”Pair of firms”, ”N”, ”F-statistic”, ”Df.” and ”P-value” denote the firms in the pair tested, the number of pairwise observations for the pair, the calculated F-statistic as presented in section 2.5.2, the degree of freedom of the test and the associated P-value, respectively.

Table 6.7: Test of the exchangeability of the bids for the cover bids

$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
(22,23)	7	8.0317	(4,406)	0.0000
(4,23)	8	5.1874	(4,406)	0.0004
(23,24)	9	3.9651	(4,406)	0.0036
(8,15)	23	3.9448	(4,406)	0.0037
(4,22)	5	3.8624	(4,406)	0.0043
(6,9)	24	3.5694	(4,406)	0.0071
(8,17)	23	3.5612	(4,406)	0.0072
(6,15)	24	3.4618	(4,406)	0.0085
(4,6)	28	3.3789	(4,406)	0.098
(8,9)	28	3.3546	(4,406)	0.0102
(6,17)	23	3.3136	(4,406)	0.0109
(6,10)	14	3.2137	(4,406)	0.0129
(6,24)	5	3.192	(4,406)	0.0134
(4,8)	23	3.1462	(4,406)	0.0145
(23,25)	5	3.0913	(4,406)	0.0159
(8,14)	16	3.0425	(4,406)	0.0172
(6,8)	12	2.8831	(4,406)	0.0224
(6,19)	17	2.6106	(4,406)	0.0351
(4,24)	17	2.4795	(4,406)	0.0435
(23,26)	5	2.3597	(4,406)	0.0528
(9,24)	8	2.3432	(4,406)	0.0543
(8,10)	12	2.1611	(4,406)	0.0727
(15,19)	27	2.1508	(4,406)	0.0739
(10,11)	6	2.1066	(4,406)	0.0792
(19,23)	5	2.0967	(4,406)	0.0805
(3,6)	6	2.0394	(4,406)	0.0881
(9,11)	16	1.9982	(4,406)	0.094
(17,19)	24	1.9726	(4,406)	0.0979
(17,24)	5	1.9536	(4,406)	0.1008
(10,17)	17	1.9371	(4,406)	0.1034
(9,19)	30	1.9085	(4,406)	0.1082
(4,11)	16	1.9012	(4,406)	0.1094
(9,17)	41	1.8978	(4,406)	0.11
(4,19)	24	1.8688	(4,406)	0.115
(3,17)	5	1.813	(4,406)	0.1254
(11,15)	14	1.7788	(4,406)	0.1322
(8,20)	17	1.7431	(4,406)	0.1396
(10,19)	21	1.6728	(4,406)	0.1554
(11,17)	13	1.665	(4,406)	0.1573

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$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
(10, 14)	13	1.5945	(4, 406)	0.1749
(6, 18)	12	1.5817	(4, 406)	0.1783
(3, 11)	6	1.5572	(4, 406)	0.1849
(6, 14)	20	1.4708	(4, 406)	0.2102
(22, 24)	10	1.4663	(4, 406)	0.2116
(4, 17)	37	1.3989	(4, 406)	0.2335
(6, 11)	12	1.2937	(4, 406)	0.2718
(8, 19)	15	1.2209	(4, 406)	0.3013
(15, 17)	38	1.2067	(4, 406)	0.3074
(9, 14)	35	1.1768	(4, 406)	0.3204
(17, 20)	26	1.1504	(4, 406)	0.3324
(11, 19)	6	1.1079	(4, 406)	0.3523
(14, 19)	17	1.0885	(4, 406)	0.3617
(11, 14)	15	1.085	(4, 406)	0.3635
(14, 24)	5	1.0368	(4, 406)	0.3879
(3, 4)	9	1.0099	(4, 406)	0.402
(14, 17)	24	0.9961	(4, 406)	0.4094
(14, 20)	15	0.9925	(4, 406)	0.4114
(3, 14)	10	0.9771	(4, 406)	0.4198
(9, 15)	50	0.9647	(4, 406)	0.4267
(3, 15)	8	0.9431	(4, 406)	0.4389
(10, 15)	25	0.9156	(4, 406)	0.4547
(3, 18)	8	0.9111	(4, 406)	0.4573
(4, 14)	31	0.9025	(4, 406)	0.4624
(19, 20)	18	0.8946	(4, 406)	0.4671
(10, 18)	6	0.8654	(4, 406)	0.4847
(3, 9)	7	0.8555	(4, 406)	0.4908
(6, 20)	14	0.8272	(4, 406)	0.5084
(11, 20)	7	0.7853	(4, 406)	0.5352
(16, 17)	5	0.7776	(4, 406)	0.5402
(9, 12)	5	0.7451	(4, 406)	0.5617
(8, 11)	9	0.7371	(4, 406)	0.5671
(8, 18)	7	0.6726	(4, 406)	0.6113
(15, 20)	24	0.6085	(4, 406)	0.6567
(4, 18)	19	0.5857	(4, 406)	0.6732
(9, 18)	16	0.5842	(4, 406)	0.6743
(6, 16)	6	0.5608	(4, 406)	0.6913
(14, 15)	29	0.5279	(4, 406)	0.7153
(18, 20)	10	0.5222	(4, 406)	0.7194

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$Pair\,of\,firms$	N	$F-statistics$	$Df.$	$P-value$
(10,16)	7	0.5192	(4,406)	0.7217
(18,19)	6	0.483	(4,406)	0.7483
(9,20)	27	0.4697	(4,406)	0.758
(11,18)	12	0.4538	(4,406)	0.7696
(10,20)	15	0.4306	(4,406)	0.7865
(18,24)	7	0.4223	(4,406)	0.7926
(17,18)	14	0.4214	(4,406)	0.7932
(4,20)	27	0.392	(4,406)	0.8144
(15,18)	14	0.376	(4,406)	0.8258
(9,10)	25	0.3681	(4,406)	0.8313
(4,10)	23	0.3285	(4,406)	0.8588
(4,16)	7	0.2957	(4,406)	0.8807
(9,16)	8	0.2726	(4,406)	0.8956
(14,18)	18	0.2513	(4,406)	0.9088
(15,16)	7	0.2087	(4,406)	0.9335
(16,20)	6	0.2026	(4,406)	0.9369
(4,15)	41	0.1809	(4,406)	0.9483
(4,9)	50	0.084	(4,406)	0.9873

Note: ”Pair of firms”, ”N”, ”F-statistic”, ”Df.” and ”P-value” denote the firms in the pair tested, the number of pairwise observations for the pair, the calculated F-statistic as presented in section 2.5.2, the degree of freedom of the test and the associated P-value, respectively.

6.2 Appendix for Chapter 3

Proposition 1 *Let be $G(b)$ the cumulative distribution of the bids with the largest distribution support $[\underline{b},\ \bar{b}]$ and $\tilde{G}(b)$ the cumulative distribution of the bids with the smallest distribution support $[a,\ \bar{b}]$ where $a > \underline{b}$. The coefficient of variation of $\tilde{G}(b)$ is lower than the coefficient of variation of $G(b)$.*

Proof of proposition 1 Let be the normal cumulative distribution of the bids $G(b)$ with mean μ and variance σ. Let be a truncation point a. Then, the bids b have a left truncated normal distribution and the probability density function is

$$\frac{1}{\sqrt{2\pi}\sigma}e^{-(b-\mu)^2/2\sigma^2}\left[\frac{1}{\sqrt{2\pi}\sigma}\int_a^{+\infty}e^{-(b-\mu)^2/2\sigma^2}db\right]^{-1} \tag{6.1}$$

$$= \sigma^{-1}\phi\left(\frac{b-\mu}{\sigma}\right)\left[1-\Phi\left(\frac{a-\mu}{\sigma}\right)\right]^{-1}, \quad a \leq b < +\infty, \tag{6.2}$$

where $a \in [\underline{b},\ \bar{b}]$ (see *Johnson et al.*, 1994, chapter 10.1, page 156 ff.). From *Johnson et al.* (1994), the expected value of the truncated distribution of the bids is:

$$E[B|b>a] = \mu + \sigma\frac{\phi\left(\frac{a-\mu}{\sigma}\right)}{1-\Phi\left(\frac{a-\mu}{\sigma}\right)}. \tag{6.3}$$

We have $\sigma > 0$, because the variance is always positive; $1 > \phi\left(\frac{a-\mu}{\sigma}\right) > 0$ first because a density cannot be negative and second because a is comprised in the support of the distribution of the bids, it cannot be equal to zero; $1-\Phi\left(\frac{a-\mu}{\sigma}\right) > 0$ because a is comprised in the support of the distribution of the bids it cannot be equal to zero, it cannot be either equal 1 or 0.

Thus, the following strict inequality is necessarily true:

$$E[B|b>a] > E[B]. \tag{6.4}$$

From *Johnson et al.* (1994), the variance of the truncated distribution of the bids is given by:

$$Var[B|b>a] = \sigma^2\left[1+\frac{((a-\mu/\sigma)\phi(a-\mu/\sigma))}{[1-\Phi(a-\mu/\sigma)]}-\left(\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}\right)^2\right] \tag{6.5}$$

$$= \sigma^2-\sigma^2\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}\left[\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}-(a-\mu/\sigma)\right] \tag{6.6}$$

$$= \sigma^2\left[1-\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}\left[\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}-(a-\mu/\sigma)\right]\right]. \tag{6.7}$$

An important result is that (see *Green*, 2003, chapter 22):

$$0 < \frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}\left[\frac{\phi(a-\mu/\sigma)}{[1-\Phi(a-\mu/\sigma)]}-(a-\mu/\sigma)\right] < 1.$$

Thus, the following strict inequality is necessarily true:

$$Var[B|b>a] < Var[B]. \tag{6.8}$$

If equation 6.4 and 6.8 are true, then the following strict inequality holds:

$$CV_{G(b)} = \frac{\sqrt{Var[B]}}{E[B]} > \frac{\sqrt{Var[B|b>a]}}{E[B|b>a]} = CV_{\tilde{G}(b)}. \qquad \blacksquare \tag{6.9}$$

Proposition 2. *If all firms participating to a specific tender t scale their bids b_i with a common factor a, the coefficient of variation remains unchanged*

Proof of proposition 2. The following formula gives the simple mean and the standard deviation of the discrete distribution of the bids for a particular tender t where n bids are submitted:

$$\mu_t = \frac{\sum_{i=1}^{n} b_{it}}{n}; \qquad \sigma_t = \sqrt{\frac{1}{n}\sum_{i=1}^{n}(b_{it}-\mu_t)^2} \tag{6.10}$$

We scale now every bid b_{it} with some proportional factor a. Thus, the mean becomes:

$$\hat{\mu_t} = \frac{\sum_{i=1}^{n} ab_{it}}{n} = a\frac{\sum_{i=1}^{n} b_{it}}{n} = a\mu_t \tag{6.11}$$

And the standard deviation is expressed as:

$$\hat{\sigma_t} = \sqrt{\frac{1}{n}\sum_{i=1}^{n}(ab_{it}-a\mu_t)^2} \tag{6.12}$$

$$= \sqrt{a^2\frac{1}{n}\sum_{i=1}^{n}(b_{it}-\mu_t)^2} \tag{6.13}$$

$$= a\sqrt{\frac{1}{n}\sum_{i=1}^{n}(b_{it}-\mu_t)^2} \tag{6.14}$$

$$= a\sigma_t \tag{6.15}$$

Then the coefficient of variation do not differ because:

$$CV_t = \frac{\sigma_t}{\mu_t} = \frac{a\sigma_t}{a\mu_t} = \frac{\hat{\sigma}_t}{\hat{\mu}_t} = \widehat{CV_t} \tag{6.16}$$

Consequently, if the bids b_{it} are scaled with a common factor a, it is impossible to detect a change in the coefficient of variation. Note also that considering $\hat{\mu}_t = a\mu_t$ and $\hat{\sigma}_t = a\sigma_t$, it is also possible to proof that scaling through a common factor a does not affect the kurtosis statistic, the percent between the two lowest bids, the skewness statistic and the relative distance. Thus, we conclude that the simple screens can flag bid-rigging cartels only if cartel participants do not scale their bids.

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