Calculating the probability of collusion based on observed price patterns

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Abstract: We present a method for estimating the probability of collusion using observed price patterns. Having these probabilities, we can also estimate the impact of the number of firms and other relevant variables on the probability of collusion, as well as the price increases and total expected overcharge caused by collusion. These estimates are essential to inform policies about how to best prevent collusion. We apply the method to 28,863 auctions in the Swedish generic pharmaceuticals markets and predict 64% of the auctions to be part of price patterns at least partly caused by collusion. We also find that nearly all collusions take the form of bid-rotation (i.e., firms taking turns in offering the lowest price) and that moving from competition to collusion increases average prices by 65%. Moreover, the results demonstrate that multimarket contact significantly increases the risk of collusion, and that increasing the number of firms from two to four reduces the risk of collusion by approximately one half. Still, we find collusion to be an important problem even with four or more firms.

Keywords: bid rigging, price coordination, cartels, collusion, competition.

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1. Introduction

Firms have an economic incentive to engage in collusion because it enables them to raise prices. This has been identified as a problem and successfully prosecuted in a wide range of markets[1], as well as suspected in many others. Regarding the markets under study in this paper—generic pharmaceuticals markets—collusion cases have recently been brought in Great Britain, Italy, and South Korea, and in the U.S. several generic pharmaceutical companies have been charged for antitrust violations in what has been called the largest corporate cartel in U.S. history (Cuddy, 2020).

It is likely that most of the methods currently used for collusion detection tend to underestimate both the prevalence of collusion as well as the impact that collusion has on price. This is the case because most methods ignore the possibility of tacit collusion, and the analysis of overcharges are often conducted under the assumption that non-indicted markets in cartel cases are not affected by collusion. In addition, recent events in the U.S. have shown that even if one is able to identify suspected cartels, the time and cost of prosecuting these cases is considerable. Moreover, given that antitrust authorities sometimes use post-cartel prices to determine cartel damages, firms have an incentive to maintain high prices even after the cartel has fallen (Harrington, 2004; Clark et al., 2022), something that was shown to be the case in the analysis of the recent U.S. generic pharmaceuticals cartel by Starc and Wollman (2022). As for tacit collusion, such events are not even considered illegal in many jurisdictions but could still incur considerable costs for society. As such, there is a need for a method that estimates the probability of collusion in different markets and that can also be used to estimate the total overcharges and how different factors affect the likelihood of collusion. These estimates can then inform policy on how to best prevent collusion, tacit or outright, to be established in the first place.

The main purpose of our research is thus to develop a method for calculating the probability of collusion, tacit or outright, based on observed price patterns, and we do this for markets where the firm with the lowest price captures most of the demand. We depart from the theoretical model proposed by Varian (1980), which implies that when the cheapest product does not capture the entire market, firms will randomize their prices. However, all theoretical models are simplifications of real life, so in our empirical work we need to account for factors that, according to theory and previous literature, could potentially affect the outcome of our analysis. As such, we adjust the probabilities of observing different price pattern under competition to account for entry, exit, the tendency of leaving prices unchanged, ties, as well as firms' incentives to set higher prices when they face high demand from returning consumers and when they have low quantities in stock. Then, using these probabilities and observed frequencies of different price patterns, we use Bayes' theorem to calculate the probability that a given price pattern is the result of collusion. Notably, the method does not require productions costs to be estimated and do not rely on assumptions of rationality being common knowledge nor that firms never make mistakes. Additionally, we also estimate the effect of the number of firms and other relevant variables on the probability of collusion as well as the effect of collusion on prices.

The literature on collusion and collusion screening (reviewed in detail in section 2) can be divided into papers studying how known cartels have affected the market and how collusion screens for the detection of such cartels can be constructed, and papers taking their starting point in economic theory to create

[1] Cartels have been indicted in the markets for asphalt, beer, bromine, cement, coal, coffee, copper tubes and fittings, detergents, diamonds, electrical equipment, gasoline, hydrogen peroxide, ocean shipping, oil, parcel post, plasterboard, plastic bags, railroad travel, rayon, rubber, sea food, steal, sugar, tea, and vitamins, to name a few (Abrantes-Metz et al., 2006; Levenstein and Suslow, 2006; von Blanckenburg et al., 2013).

screens for deviations from competitive behavior and then use data from markets where collusion has been suspected but never prosecuted to test the predictions from the models. The first variant is often called *ex post* models (Imhof et al., 2018; Imhof, 2019) and important contributions to this include Porter and Zona (1993), on cartels bidding for state highway construction contracts in the 1980s, and Porter and Zona (1999), on a cartel in public procurement auctions for school milk contracts in Ohio, also during the 1980s. Directly related to the generic pharmaceuticals markets are studies based on the collusion allegations brought by the U.S. DoJAD against several generic drug manufacturers by Cuddy (2020), Clark et al. (2022), and Starc and Wollman (2022). Important examples of the second variant, often called *ex ante* models, are studies by Baldwin et al. (1997) and Bajari and Ye (2003). Baldwin et al. (1997) studied timber auctions in the U.S. Pacific Northwest from 1975 until 1981 and found that bidding in the auctions under study did not conform to the bidding expected from a competitive model. Bajari and Ye (2003) investigated the behavior of firms bidding for almost 18,000 highway repair contracts in Minnesota, North Dakota, and South Dakota from 1994 to 1998 and found that most of the bidding for these contracts was consistent with competitive bidding.

Our research is in the *ex ante* tradition, using data from markets where we suspect collusion to be present due to market characteristics but where no cartels have been successfully prosecuted. Our work is related to that of Byrne and De Roos (2019) in that it studies a consumer product. It is also related to studies of the public procurement auctions by Porter and Zona (1993; 1999), Bajari and Ye (2003), and others. However, the markets we study differs from those analyzed by Porter and Zona and Bajari and Ye because in our case the lowest-priced product does not secure all sales. However, pharmacists are required to dispense the lowest-priced product for prices to be covered by the Swedish pharmaceutical benefit program (with some exceptions). Our research is inspired by Cletus (2016), who documented the existence of bid-rotation and parallel bidding patterns lasting at least eight months for a fifth of the 1173 Swedish pharmaceutical markets studied and found these suspicious patterns to be associated with 60% higher prices.[2]

Our primary contribution is that we, contrary to previous studies, use a method that do not require cost estimations or assumptions of rationality to calculate a probability that an observed series of prices are collusive. This is important for several reasons. Firstly, it makes it possible to estimate total overcharges and to study how the number of firms and other variables affects the probability of collusion, which in turn enables us to make informed decisions on how markets should be reformed in such a way as to make tacit and outright collusion difficult for potential colluders in the first place. That we can study the effect of the number of firms on the probability of collusion using field data is itself a major contribution of this research, as the existing knowledge about this effect primarily comes from either classroom experiments or studies of convicted cartels, both clearly suffering from selected samples.

[2] Cletus' abstract contains two typos. First, she wrote that 231 markets with suspicious patterns account for around 25% of the 1173 markets studied, when it is actually 20% rounded-up. Second, the coefficient of 0.47 for a suspicious pattern in logged prices was interpreted as indicating a price effect of 47% instead of an effect of 60%, which is obtained using the formula $100 * [\exp(0.47) - 1]$. Also, Janssen (2022) analysed dynamics of Swedish pharmaceutical prices, more precisely a type of Edgeworth cycle, which he did not find, and two other types of price cycles. His definitions of the latter two require that each firm reverts back to the price it had two months prior, which rules out more than two participating firms. Therefore, it is not surprising that the regression output indicates that, compared to during a monopoly, the price cycles were more common only with two firms, and with three firms of which one was an originator. Collusions in the latter category likely involve two generics while the originator constantly has a higher price.

Secondly, our method provides competition authorities with more detailed information regarding the probability of collusion compared to previous screening methods that only divide markets into one group where collusion can be suspected and another where it is not suspected. Because competition authorities have limited resources, it seems reasonable for them to start with the high-probability suspected price patterns before working their way down the probability estimates.

Thirdly, an ever-present problem is that outside observers can never know with certainty what observations are affected by collusion since even when studying convicted cartels, there is a chance that some colluding firms were not indicted. As such, the estimate of the likelihood of collusion will always be measured with some degree of error, and we argue that this measurement error is smaller when using our method, as we do not group observations below some arbitrary level (e.g., 90 or 95%) together and assume that none of these are the result of collusion. Note also that our method is designed to capture partial collusion as well (i.e., collusion that does not involve all firms or time periods in a specific market), a situation in which many of the classical methods for collusion detection will fail (Imhof et al., 2018).

The method developed in this paper can be applied in a wide set of different markets. It could, for example, be used to investigate collusive pricing in many procurement markets, repeated auction markets, or on online marketplaces such as Amazon, where the firms offering the lowest price receive a large share of the total demand.

The main results from our statistical analysis are as follows. The probabilities of collusion are estimated to exceed 90% when n firms have sold the lowest-priced product every n:th month for nine months or more and when there has been a tie between the same two or more firms for five months or more. In addition, in auctions with four or more bidders in the low-price segment, the probabilities of collusion are estimated to exceed 60% already when the duration of the suspicious pattern weakly exceeds seven months.

The results also show that an increase in the probability of collusion from zero to one increases average prices by 30%, according to conservative estimators, and by 65%, according to the preferred specification. Using the conservative estimates, two firms have gained more than 100 million SEK each in excess revenues because of collusions during the study-period, while 25 firms gained more than 10 million SEK (1 million USD) each. The price effects are larger than in most previous studies. For example, the meta-analyses by Connor and Bolotova (2006) and Connor (2014), with significant overlap in their samples, reported average price effects of cartels of 29% and 23%, respectively. Our findings are more in line with the findings of Clark et al. (2022) and Starc and Wollman (2022) who analyzed the impact of the alleged U.S. generics pharmaceuticals cartel on prices. Starch and Wollman reported price increases of 45–50% on average after two years, while Clark et al. reported increases of between 0% and 166%, depending on pharmaceutical substance.[3] Because the demand elasticity on market level is low for pharmaceuticals, it is not surprising that the highest price effects for collusion have been found for pharmaceutical markets.

We also confirm the qualitative predictions from theoretical analyses (Selten, 1973; Shapiro, 1989; Phlips, 1995; Ivaldi et al., 2003) and classroom experiments (see Huck et al., 2004; Fonseca and

[3] It should be noted that these papers use samples from non-indicted U.S. generic drug markets as their competitive counterfactuals, which might be a strong assumption. If we use an estimation approach like those adopted in many previous studies by grouping observations with a likelihood of collusion below 90% and treating them as competitive, we instead find a price effect of 13%. This indicates that the price effects can be underestimated if all markets that not with high certainty is found to be collusive are treated as competitive.

Normann, 2012, Horstmann et al., 2018, and the literature therein) that the number of firms has a significant negative effect on the probability of collusion. More precisely, we find that increasing the number of bidders from two to four reduces the probability of collusion by half. This is a small reduction compared to Huck et al. and Fonseca and Normann who found quantities and prices, respectively, to be close to competitive levels with four players that were not allowed to communicate. Davies et al. (2011) analyzed merger decisions taken by the European Commission 1990–2004 and concluded that—consistent with the findings of Huck et al. and Fonseca and Normann—the Commission held tacit collusion to rarely occur in markets with more than two firms. Our results question this conjecture as we find collusions in markets with up to seven firms of which five are likely participants of the collusions. In addition, it is likely that the collusions in the markets we study are tacit because they take the form of bid-rotation that, compared to parallel bidding, are less stable and, because of a price cap, yields lower profits but, arguably, should be easier to initiate without verbal communication.

Also, we confirm the results of Ciliberto and Williams (2014) that variation in multimarket contact matters most at low and moderate levels and find that reducing multimarket contact from its third quartile to its minimum value reduces the risk of collusion as much as increasing the number of bidders from two to four.

The reminder of this paper is structured as follows. In section 2, the literature regarding collusion and collusion detection is reviewed, before section 3 provides a detailed description of the Swedish generics pharmaceuticals markets. In section 4 we describe the data, while section 5 uses theory to analyze competitive bid behavior. Section 6 then presents the calculations of the probability of collusion given the observed winning patterns, and in section 7 we show that increases in the probability of collusion are associated with higher prices. In section 8, the impact of the number of bidders on the probability of collusion is estimated. Finally, section 9 summarizes our results, discusses them in relation to the existing literature, and offers policy advice for regulators attempting to prevent collusion.

2. The literature

The literature on collusion and collusion screening can be divided into four strands according to whether the analysis is done *ex ante* or *ex post* and whether it is done using complex or descriptive statistical methods (Imhof et al., 2018; Imhof, 2019). *Ex ante* analysis is done without the researcher knowing whether the data contain spells of collusion or not while *ex post* analysis uses data from markets with confirmed and often successfully prosecuted cartels. Complex statistical methods imply that researchers use techniques such as regression analysis or machine learning to analyze the data.

Collusion screening is mostly carried out using time series analysis techniques, often focusing on the first two moments—the mean and the variance—of the price distribution.[4] These methods have their theoretical foundation in the work done by Athey et al. (2004) and Harrington and Chen (2006), who in different theoretical settings both demonstrated that prices are higher and less volatile during periods of collusion. In Athey et al.'s (2004) model, lower volatility is a result of costs related to firms setting equilibrium prices, while Harrington and Chen (2006) found that lower volatility is a result of the colluding firms' reluctance to pass through cost increases because this increases the risk of detection.

[4] Early works instead used successfully prosecuted cartel cases to identify instances of collusion and then presented descriptive statistics on price, number of sellers, etc. before and during collusion. For a review of this literature, see Levenstein and Suslow (2006).

Another potential reason that price volatility falls during periods of collusion is that the colluding firms must solve an agency problem to determine which price to coordinate on, and this takes time during which the price remains fixed (Abrantes-Metz et al., 2006).

Some experimental studies confirm the theoretical findings on how collusion affects price and/or price dispersion (Gerther and Plott, 1984; Isaac et al., 1984; Davis and Holt, 1998), showing that prices are considerably higher under collusion, even reaching the monopoly level in Davis and Holt's study, while the studies by Gerther and Plott and Isaac et al. indicate both higher average prices and lower volatility under collusion. The experimental work most closely related to our study is that of Fonseca and Normann (2012), who found that the two most common types of collusion are parallel bidding and bid-rotation, and that collusion is easier to uphold in markets with fewer firms, irrespective of whether the firms can communicate or not.

Most empirical studies on collusion use *ex post* data from known or suspected cartels to study the impact on prices and price dispersion. Genesove and Mullin (2001), for instance, studied prices during the U.S. Sugar Institute cartel from 1927 until 1936, and found a somewhat higher price but a considerably lower price volatility during the cartel years. While the previous study used mostly descriptive statistics, Bolotova et al. (2008) studied pricing during the international lysine and citric acid cartels using time-series analysis and found that for lysine collusion increased the price per pound by 25 cents while decreasing price volatility. For citric acid price increased by 9 cents per pound, but in this case price volatility also increased during the collusive periods.

Collusion has also been found identified in the cement industries in Norway and Germany (Röller and Steen, 2006; von Blanckenburg and Geist, 2009). Röller and Steen (2006) analyzed the welfare effects of a Norwegian cement cartel in the 1960s and found that total welfare was reduced by 131 million Norwegian crowns due to the cartel, and that investigating only the loss for the consumer yields a loss of 237 million crowns compared to competition. von Blanckenburg and Geist (2009) investigated prices before, during, and after a cartel in the German cement industry during the period 1981–2001, finding that price volatility was considerably lower during collusion. Clark and Houde (2013) found that colluding firms in gasoline retailing in Canada used delays in price adjustments to keep prices high. It has also been suggested that studying price and price volatility might not be sufficient to find collusion, and that more suitable tests can be constructed using Kolmogorov-Smirnov tests on the whole price distribution. Doing so, von Blanckenburg et al. (2013) were able to identify 9 out of 11 prosecuted cartels during the period 1976–2009 using only price series data.

In a recent study, Igami and Sugaya (2022) uses data and evidence from U.S. courts and European competition agencies to investigate how the vitamin cartels during the 1990s affected firm profits to find out if the collusive agreement was enforceable in the sense that the short run profits from cheating were less than the discounted profits of adhering to the collusive agreement. They found that for one of the four studied cartels, there was no clear incentive to collude, while for the others there was. The cartels were also found to have reduced social welfare by 13%, while descriptive statistics suggest price increases during the cartel periods of, on average, 21%.

Turning to collusion in auction markets, Graham and Marshall (1987) demonstrated that in single-object second-price auctions, bidder coalitions are viable and increase the payoff of each member in the coalition, while McAfee and McMillan (1992) showed that collusion is also viable in first-price auctions. However, McAfee and McMillan's results suggest that without the possibility of side-transfers, the scope of bidder collusion is severely limited. For repeated auctions like those under study in this paper, Aoyagi (2003) and Skrzypacz and Hopenhayn (2004) showed that collusion is possible even without side payments and that elaborate dynamic bid-rotation schemes involving intertemporal adjustment of a firm's payoffs can be constructed to improve not only competition payoffs but also payoffs in traditional bid-rotation schemes.

For auction markets, we again find a preponderance of studies using statistical modelling of *ex post* data from prosecuted cartels to study collusion (e.g., Hendricks and Porter, 1988; Porter and Zona, 1993, 1999; Pesendorfer, 2000; Ishii, 2008, 2009; Asker, 2010; Conley and Decarolis, 2016; Wachs and Kertész, 2019; Kawai et al., 2023). In an early paper, Hendricks and Porter (1988) investigated data from the federal auctions for offshore oil- and gas lease sales during the 10-year period 1959–1969, finding that firms did indeed submit collusive bids—something that was legal at the time. Porter and Zona (1993) analyzed data from a known cartel for state highway construction contracts during the 1980s and showed that the ranking of competitive bids was predictably correlated with cost and other explanatory variables, while cover bids from cartel members were not.

Adopting a similar empirical strategy, Porter and Zona (1999) analyzed data from a known cartel in the Ohio school milk procurement auctions from 1980 to 1990. They found that for cartel members, the probability of submitting a bid increases with the distance from the school districts, while the bids themselves decrease—something that is unlikely to be a pattern from competitive bidding since transportation costs increase with distance. Moreover, they reported that cartel bidding increased the price of milk by, on average, 6.5% during the cartel years. School milk procurement auctions during the 1980s was also the subject of a study by Pesendorfer (2000), but in this case the locations were Florida and Texas, where firms were also convicted of bid rigging. Pesendorfer (2000) showed that one of the cartels coordinated its behavior by dividing the market, while the other used side payments to compensate cartel members who submitted fake bids or abstained from bidding at all. However, Pesendorfer (2000) did not present any estimates of the impact of these cartels on milk prices in the affected regions.

Asker (2010) studied knockout auctions in a bidding ring of stamp dealers that was active in the US from the late 1970s to the mid-1990s. Asker used data collected by the New York Attorney General's department during its investigation into the ring to analyze the impact on both sellers and non-ring member stamp dealers, finding that there were considerable costs for both sellers and non-ring members. Using a clustering method, Conley and Decarolis (2016) studied collusion in average bid procurement auctions in Italy from 2000 to 2010. In these auctions, firms could affect both who won the contract and the average price in the auction by submitting multiple bids. Since this was not allowed, firms have been shown to set up subsidiaries to circumvent these regulations, and bid rigging was found to affect as much as 30% of the auctions held. Wachs and Kertész (2019) used network topology analysis to screen for cartels in public procurement auctions in the Republic of Georgia from 2011 to 2016, after having validated their screening model using the Ohio milk procurement data first analyzed by Porter and Zona (1999). In doing this, they were able to identify groupings of firms whose bids were similar to those from the Ohio milk auctions, suggestive of cartel bidding.

Using a regression discontinuity approach, Kawai et al. (2023) argued that for closed bids during competition winning or losing an auction is "as-if-random" and should not be affected by backlog or incumbency. Using the Ohio milk procurement dataset of Porter and Zona (1999), they showed that backlog and incumbency did indeed affect the probability of winning, suggesting collusive behavior. After that, they applied the method to a dataset from Japanese procurement auctions with no proven, but suspected, collusion, and found a significant number of procurement auctions that were classified as collusive. Finally, some researchers have begun using machine learning methods to screen large datasets for collusive behavior based on learning data from known cartels. Imhof and Walliman (2021) used a supervised machine learning method applied to several datasets on prices and price dispersion that includes data from known cartels to predict which data comes from known cartels and which do not. In doing so, they achieved a 90% correct prediction rate for the known cartels.

Descriptive analysis of *ex post* data is rare, but there are a few studies that are worth mentioning (e.g., Abrantes-Metz et al., 2006; 2012; Imhof, 2019). Abrantes-Metz et al. (2006) studied the collapse of a bid-rigging cartel of fish-processing companies supplying fish to military installations in the U.S and abroad in June 1988. Comparing average price and price dispersion before and after the cartel, they found that when the cartel collapsed, the average price fell by 16%, while the standard deviation of the price increased by 263%. However, when using these methods on a dataset of retail gasoline prices in Louisville, Kentucky from 1996 until 2002, when there were suspicions of collusive pricing, these suspicions could not be verified using the price and price dispersion screens. In a similar fashion, Imhof (2019) investigated how a cartel of road construction firms in the Ticino region of Switzerland active from January 1999 until March 2005 affected prices and price dispersion. Imhof also considered other statistics such as the skewness and kurtosis of the price distributions during and after the cartel and found that these measures can also be used to screen for collusive bid behavior. Based on their results, both Abrantes-Metz et al. (2006) and Imhof (2019) suggested using descriptive statistics for the price or the price distribution in order to screen for collusive behavior. Using such descriptive techniques, Abrantes-Metz et al. (2012) explored whether the London Interbank Offer Rate (LIBOR) had been manipulated, as suggested in an article in the *Wall Street Journal*. However, despite some findings of questionable patterns in the daily LIBOR quotes of the banks, Abrantes-Metz et al. (2012) concluded that their analysis did not indicate that the LIBOR was systematically manipulated by the banks.

We know of three recent *ex post* studies that are closely related to our research—those by Cuddy (2020), Clark et al. (2022), and Starc and Wollman (2022)—each of which studies collusion in the generic pharmaceuticals markets. Cuddy (2020) constructed a model of simultaneous retail procurement auctions for pharmaceuticals in the U.S. and investigated pricing under both collusion and competition. In doing so, she found collusion overcharges of 2.2 billion USD in her sample of pharmaceuticals which, if the results are representative of the market as a whole, would indicate total cartel overcharges of 12 billion USD.

Clark et al. (2022) used a difference-in-differences approach to estimate the price effects of the same alleged U.S. generic pharmaceuticals cartel as that studied by Cuddy (2020) on a sample of six affected substances in the Medicaid market. They found that the cartel was successful in raising prices in three of these markets, and that the price increases in these three markets ranged from 13–166%, leading to total overcharges of approximately 5 million USD. However, this was for a sample of only six affected pharmaceuticals while a total of 112 were included in the indictments, and for the Medicaid market which accounts for approximately 10% of total prescription drug expenditure in the U.S.

Finally, Starc and Wollman (2022) began their research by recording four findings from reduced-form estimations: that collusion led to 45–50% price increases within two years after collusion was formed; that these price increases induced entry into the market; that the regulatory process regarding entry into the market created significant delays of up to four years; and that when entry did occur, it reduced prices. They then constructed a structural model of the generic pharmaceuticals market and estimated the relevant parameters required to forecast prices under both competition and collusion. By doing this, they were able to find total damages of 5.4 billion USD over the three-year period under study, amounting to overcharges of 13.7 million USD per drug per year.

It should, however, be noted that these papers treat non-indicted U.S. generic drug markets as competitive to find their counterfactuals, which, given our results below, might be a strong assumption.

This could then also perhaps explain the counterintuitive finding in Clark et al. (2022), that the cartel only raised prices in three out of six investigated cases.

Irrespective of whether they use descriptive or more sophisticated statistical analysis, all the collusion screening methods used in the literature referenced above have been designed and calibrated using data from known collusions. While showing that cartel firms bid differently than non-cartel firms might help to confirm a conspiracy if one already has suspicions about colluding firms, it serves less effectively as a collusion screen in general if known collusions differ systematically from as yet undetected collusions. Also, in some cases the models developed in an *ex post* context have been unable to detect collusion in markets other than the one the model was originally created to study, even when there were strong suspicions of collusion (see, e.g., Abrantes-Metz et al., 2006). As such, collusion screens created using *ex post* data from known cartels are likely to under-detect collusive behavior.

Turning to articles that are categorized as *ex ante* studies (i.e., studies that are not based on data from convicted cartels), we find that this strand of literature is more heterogeneous than the *ex post* studies listed above. However, they all have in common that the suggested collusion screens are constructed and calibrated to identify deviations from specific competition models rather than explicit cartel behavior. This strand of literature includes studies by Baldwin et al (1997), Bajari and Ye (2003), Brannon (2003), Knittel and Stango (2003), Ishii (2008, 2009), Athey et al. (2011), Aryal and Gabrelli (2013), Imhof et al. (2018), Chassang and Ortner (2019), Schurter (2020), Chassang et al. (2022), and Kawai and Nakabayashi (2022).

Baldwin et al. (1997) studied Forest Service timber auctions in the Pacific Northwest during the period 1975–1981. These auctions had been suspected of playing host to bidder collusion, and so congressional hearings were held in 1977. However, despite much effort, the justice department failed to successfully prosecute the alleged cartels, other than in a few special cases. Baldwin et al. (1997) empirically determined whether the observed price variation in these auctions was more likely explained by collusive bidder behavior or demand conditions, finding that the observed price patterns were most likely caused by collusive bidder behavior. The U.S. Forest Service timber auctions were also the focus of Athey et al.'s (2011) research, who investigated potential collusion in forest auctions in a region on the Idaho-Montana border and in California from 1982 to 1990. They compared the prices from the actual auction to their predictions from a competitive model, revealing that while bidding in the California region is consistent with the predictions from the competitive bidding model, the prices obtained in the Idaho-Montana auctions are not. The authors chose to interpret the discrepancy as evidence of "a mild degree of cooperative bidding" in these areas and reported timber price increases of 5–10% due to collusion. Timber auctions in British Columbia, Canada from 1996 until 2000 were analyzed by Price (2008) and Schurter (2020) using statistical methods designed to detect discrepancies in competitive bidding. They both found that not all firms bid competitively, and Schurter (2020) also estimated that collusion reduced the expected revenue of the sellers of timber by 3.2%.

Brannon (2003), meanwhile, argued that the introduction of the Wisconsin Unfair Sales Act facilitated tacit collusion in retail petrol markets and found that, compared to a non-affected control market in Minnesota, prices were higher in the two studied Wisconsin markets and volatility lower in one of them after the introduction of the Act.

Bajari and Ye (2003) studied suspected cartel bidding among construction firms for nearly 18,000 highway repair contracts in Minnesota, North Dakota, and South Dakota from 1994 until 1998. Using a theoretical auction model with asymmetric bidders, they constructed a model of competitive bidding to show that if bidding is competitive the bids should be (1) conditionally independent (meaning that after controlling for all relevant information concerning costs, competitive bids should be independent) and

2) exchangeable (meaning that costs alone, rather than the identity of other bidders, should determine the size of firm bids). The exchangeability and conditional independence tests by Bajari and Ye (2003) both showed that most of the bidding behavior for the 18,000 contracts was consistent with competitive bidding. However, if a sophisticated cartel was in operation, it could perhaps generate phony bids that would pass the exchangeability and conditional independence tests used. As such, Bajari and Ye (2003) also elicited a prior distribution over the parameters that enter their model and, given this prior distribution, they used Bayes' theorem to choose between competitive and collusive models. The results from the estimation of these models clearly show that the model of competitive bidding strongly dominates the alternatives, suggesting that bidding was not collusive for these contracts.

Aryal and Gabrelli (2013) also studied road construction and maintenance work auctions, partly using the methods suggested by Bajari and Ye (2003). The region under study was California and the period ran from January 2002 to January 2008. The screening procedure used was conducted in two steps. First, using the exchangeability and conditional independence tests of Bajari and Ye (2003), Aryal and Gabrelli (2013) shortlisted bidders whose behavior was at odds with competitive bidding. Then, in a second step, the estimated cost distributions under collusion and competition were compared using the Kolmogorov-Smirnov and Wilcoxon-Mann-Whitney tests, the results showing that there was no evidence of collusion, even when the analysis was carried out on firms whose bidding behavior failed the exchangeability and conditional independence tests of Bajari and Ye (2003).

Imhof et al. (2018) studied possible collusion in construction procurement auctions in a non-disclosed Swiss canton from 2004–2010. The collusion screening method they used was based on price (bid) data and focused on two commonly used measures in the literature—the coefficient of variation of the bids and the relative distance in bids—which were defined as the distance between the winning bid and the first loser divided by the standard deviation of all losing bids. The results showed no indications of widespread collusion in the market as a whole, so the authors moved on to create a method for partial collusion detection. The suggested method consists of four steps. First, contracts that exhibit simultaneously low coefficient of variation and high relative distance in bids are flagged as suspicious. Second, a search for groups of firms regularly bidding for the same suspicious contracts is conducted. Third, an analysis of geographical bidding behavior is conducted to investigate whether there are signs of collusion by dividing the market. Finally, a method for the detection of bid-rotation schemes is applied. Using this multi-step procedure, a local bid-rigging cartel with cover bids and a bid-rotation scheme was identified and sanctioned by the Swiss Competition Commission in 2016.

The empirical literature on bid-rotation as a collusion device is, however, relatively scarce. This is surprising given the experimental findings of Fonseca and Normann (2012) that bid-rotation is one of the most common forms of collusion. Two studies by Ishii (2008; 2009) investigated suspected, but not prosecuted, bid rigging in public procurement auctions in Ibaraki city, Osaka, and Naha city, Okinawa during the period 2001–2005. These markets were suspected of being affected by bid rigging due to consistently high winning bids, with a few examples of intense bidding at low prices suggesting a price war among previous cartel members. Ishii (2008) investigated what determined the winning bid in auctions for road-paving works in Ibaraki city and obtained data that was consistent with a bidding ring that allocated contracts to the ring member whose duration since the last winning bid was long and total recent winning contract amounts were low. Ishii (2009) studied procurement auctions of compensation consulting works in Naha city, Okinawa. The analysis was based on a study of how a score variable based on the size of previous wins for other firms likely to be in the bidding ring affected the likelihood of winning the contract. The results indicate that firms with a high score variable (i.e., firms that were owed a favor by other ring members due to losing previous contracts) were considerably more likely to win the auctions, suggesting collusion using a form of bid-rotation to coordinate ring member behavior.

Thus far, much focus has been on the possibility of collusive bidding in auctions, which would constitute examples of outright cartel behavior. Knittel and Stango (2003), however, investigated whether credit card issuers in the U.S. used nonbinding state-level price ceilings as a focal point for tacit collusion from 1979 to 1989. Their estimates show a statistically significant probability of tacit collusion using the price ceiling as a focal point. This probability was high in the first years of the study period, but it fell during the final years due to the entry of several large credit card issuers nationwide in the U.S. A recent study by Chassang and Ortner (2019) used the introduction of a minimum price at the bottom of the distribution of observed winning bids to create a screen for collusion when such policy changes are made. Theoretically, the introduction of a minimum price at the bottom of the distribution will have a limited impact on competitive bidding, but under collusion it will make the punishment phase of cartel enforcement less effective, thus reducing the winning bid distribution if collusion is present. These predictions were tested on a dataset of 10,533 Japanese public procurement auctions in the Ibaraki prefecture from May 2007 until March 2016 using difference-in-difference analysis of prices from auctions in cities introducing the type of minimum price discussed above to those from cities in a control group not implementing minimum prices. The results from the empirical analysis show that there was collusion in these auctions and that the introduction of the minimum price limited the scope of collusion by reducing cartel discipline as the potential punishments became weaker with the minimum price in place.

Japanese procurement data was also used by Chassang et al. (2022) and Kawai and Nakabayashi (2022) to identify bid patterns not consistent with competitive bidding. Chassang et al. (2022) noticed that in first-price sealed bid auctions in Japan, the density of the bid distribution just above the winning bid is lower than expected from competitive bidding and develop a test using this observation. Kawai and Nakabayashi (2022) instead focus on auctions that require re-bidding as no bid reached the reserve price. Under these circumstances, if a specific firm has been designated as the winner by a bidding ring, this firm will submit the lowest bid in both the first- and second round of the auction, while this is not necessarily the case under competitive bidding. As such, collusion creates a higher level of persistence in the identity of the lowest bidder than competition, and after accounting for non-collusive reasons for persistence (e.g., cost heterogeneity among competitive winners), Kawai and Nakabayashi (2022) found that 37% of the auctions in their sample were affected by collusive bidding.

Finally, in a mostly descriptive analysis, Byrne and De Roos (2019) showed how Australian gasoline retailers learned how to tacitly collude to keep prices above competitive levels over a period of several years, due to price signaling from the dominant firm, BP. Their analysis found increases in retailer margins of between 19 and 64%, amounting to a price effect of less than 5%.

Our study is closely related to that of Byrne and De Roos (2019) in that it uses *ex ante* data from markets that, based on theory and market characteristics, should be prone to collusion. The setup of the auctions in the Swedish markets for generic pharmaceuticals, together with general market characteristics, implies that collusion could be present. There is frequent interaction among firms, prices are set simultaneously for the period in the auction procedure, firms can monitor the prices of all other firms, demand on the market level is inelastic and, in most cases, stable, and costs of operations are well known both in regard to the bidder themselves and their competitors. However, while Byrne and De Roos conduct a descriptive analysis, we conduct an advanced statistical analysis of the data to ensure that our model does not overestimate the prevalence of collusion in the market.

Entry by additional firms into the market has often been suggested as a remedy for collusion, theoretically (Selten, 1973), experimentally (Huck et al., 2004; Fonseca and Normann, 2012), and empirically (Levenstein and Suslow, 2007; Davies and Olczak, 2008; Cuddy, 2020; Starc and Wollman, 2022). The reason for the strong focus on entry as a remedy to collusion is that the rules and regulations related to entry are determined by the policymakers, and, as such, they can be revised when necessary (Starc and Wollman, 2022). As an example, in 2017, the U.S. Food and Drug Administration implemented its Drug Competition Action Plan to increase generic entry, and among the measures taken was to create a list of off-patent pharmaceuticals without generic competition (Cuddy, 2020).

As such, we will also determine how the likelihood of collusion is affected by the number of firms in the market, thus giving competition authorities and others more precise knowledge of how changes in regulation designed to increase entry and the number of bidders in the market might affect the likelihood of collusion. This will also be helpful for competition authorities when considering whether to allow mergers, as well as for firms or government organizations when setting criteria for procurement auctions in other markets.

Theory suggests that collusion will be more common in markets with a few participating firms (Selten, 1973; Shapiro, 1989; Phlips, 1995; Ivaldi et al., 2003).[5] Following Fonseca and Normann (2012), let us assume that the collusive profits (*cp)* are divided equally among members of the coalition (*n)*; that is, each firm gets *cp/n* if collusion occurs. By defecting, the firm can get (slightly less than) *cp*, while in competition no firms earn any profits. Under these conditions, Fonseca and Normann (2012) showed that collusion can be upheld if firms have a discount factor $\delta \geq \frac{n-1}{n}$, which increases with the number of firms, thus making collusion more difficult to uphold. This discount factor is applicable to situations in which all firms have the same price (i.e., parallel bidding), while the discount factor necessary for upholding a bid-rotation scheme is, according to Fonseca and Normann (2012), slightly above the discount rate necessary to uphold a common price collusion. In addition, theory suggests that parallel bidding is slightly more profitable than bid-rotation, and that they are both considerably more profitable than competition, all else being equal.

In a different setting, Selten (1973) has shown that in markets with four firms, they will always be able to collude, whereas in markets with six they are unlikely to do so. The intuition behind this result is that in the model, the position of being an outsider to the collusive agreement becomes relatively more attractive as the number of firms increases, and as the model is set up, the cut-off point will be set at five firms in the market. The theoretical predictions from the Selten model have been investigated by Huck et al. (2004), first by means of a meta-analysis of previous experiments and then using their own set of experiments on how the likelihood of collusion is affected by the number of firms. Both investigations show that more firms reduces collusion, and that when adding the practicalities of human interaction in the experiments to the Selten (1973) model, collusion was not even possible when there were four firms in the market, leading the authors to conclude that “two are few and four are many”. In an experiment allowing communication between firms, Fonseca and Normann (2012) found that collusion was facilitated in markets with fewer firms, and that this result holds irrespective of whether the subjects were allowed to communicate or not (the first thus resembling a formal cartel and the latter a tacit collusive agreement). Finally, in their experiments Horstman et al. (2018) found that tacit collusion decreases in the number of firms in an almost linear fashion when going from two to four firms.

The economic intuitions behind these results are as follows. Firstly, identifying focal points in terms of prices (or quantities) becomes more complicated the more firms that are involved (Ivaldi et al., 2003). Secondly, if the collusive profits are shared by many firms, the takings might not be that impressive and

[5] As discussed in Feuerstein (2005), this is the case both for price and quantity competition, although the impact of the number of firms is in most cases more pronounced for quantity competition. Also, a potential caveat is that even with many firms, collusion might be possible if they are capacity-constrained, making cheating on the collusive agreement less profitable (Feuerstein, 2005).

the profit of attaining the whole market by undercutting the other firms relative to the share of collusive profits will be larger the more firms that are in the market (Ivaldi et al., 2003; Feuerstein, 2005). Thirdly, when there are many firms in the market, some firms might opt to stay outside the collusive agreement and free-ride on the other firms, as this will maximize profits (Selten, 1973). As such, having many firms creates problems both in the initiation and the implementation phases of the collusive agreement. The conventional wisdom that collusion is difficult in markets with many firms has also carried over to the legal profession, where the ability of firms to co-ordinate is often assumed to be closely related to the number of firms (Dick, 2003).

The empirical literature related to this issue is scarcer and often uses *ex post* data from situations where tacit or explicit collusion was suspected to infer the relationship between the number of firms and the likelihood of collusion (Levenstein and Suslow, 2007; Davies and Olczak, 2008). Levenstein and Suslow (2007) compiled evidence from previous studies of prosecuted cartels and presented how many firms were included in the different cartels. Their results show that while most of the cartels in their study are found in concentrated industries with fewer than 10 firms, three of the cartels have involved more than 100 firms. Davies et al. (2011) instead used information from European Commission merger and cartel cases to conclude that the Commission believes that tacit collusion rarely involves more than two firms, and that overt collusion is more common if there are 4–7 firms in the market. We are not aware of any similar analyzes for other competition authorities, but Bergman et al. (2019) found the U:S Federal Trade Commission to be slightly more permissive with respect to the related variable, post-merger market share.

In a reduced-form analysis, Starc and Wollman (2022) found that collusion in the U.S. generic pharmaceuticals markets raised prices, which in turn increases the number of applications for entry in the collusive markets. Two years after a pharmaceutical product market being cartelized, there were an additional 0.4 applications for market approval. However, due to significant delays in market approval, a significant increase in entry took place first four years after the market being cartelized. Using their structural model, they then simulated the impact of decreasing sunk entry costs from 4.3 million USD to 3.9 and 3.5 million USD, respectively. A decrease in costs by 400,000 USD leads to additional entry in 42 out of 113 markets, resulting in increases in consumer compensating variation of 142 million dollars, on average, with foregone revenues for the regulator of 61 million, while reductions in costs by 800,000 USD induces entry in 59 additional markets, compensating variation of 374 million USD, and forgone revenues of 122 million. Starc and Wollman also reported that reducing the approval times would create even larger effects, with compensating variation reaching almost 600 million USD for a 1-year reduction and over 1,500 million USD for a 2-year reduction.

3. The markets

Based on the literature, we know that markets that are affected by collusion often share many, or at least some, vital characteristics. They in most cases only have a few firms, firms have similar cost functions that are well-known to all firms in the market, competition is one-dimensional in price (or at least restricted to a few dimensions), the firm with the lowest price secures a large share of the market, the products sold are homogenous, demand is predictable, and, finally, prices and other important characteristics of the good or service are easily observable. Such characteristics were, for example, prevalent in the Ohio milk procurement markets examined by Porter and Zona (1999), the Norwegian cement market analyzed by Röller and Steen (2006), the Australian market for gasoline studied by Byrne and DeRoos (2019), and the U.S. generics pharmaceuticals markets studied by Cuddy (2020), Clark et al. (2022), and Starc and Wollman (2022).

Pharmaceutical markets often have features that increase the risk of collusion. One is that many pharmaceuticals' markets have substitution systems where the insurer only reimburses the lowest-priced generic alternative, creating markets with high market shares for the products with the lowest price. In the U.S., most state laws require substitution toward generics, which occurs automatically unless the prescriber indicates on the prescription that the branded drug must be dispensed as written (Complaint, 2019[6], p. 21). Furthermore, when marketing their generic drugs, manufacturers often do not attempt to differentiate their products because a generic drug is primarily considered a commodity (Complaint, 2019, p. 23). This means that firms can focus on coordinating prices, and not on product characteristics and advertising, to achieve an effective and stable collusion. Also, while pharmaceutical benefit schemes can maintain price sensitivity in choices between exchangeable products, they make consumers less price sensitive regarding the decision to buy a pharmaceutical at all, thus increasing the monopoly price and making the gain from collusion higher.

In Sweden, a government-funded benefit scheme covers 75–80% of the cost of prescription drugs for consumers. The generic substitution law from 2002 requires pharmacists to inform consumers whether less costly substitutes are available and to dispense the lowest-priced pharmaceutical unless: (i) the consumer choose to pay the price difference themselves to get the prescribed product; (ii) the prescribing physician has prohibited an exchange for medical reasons; or (iii) the pharmacist has reason to believe that the consumer would be adversely affected by substitution, for example, if the low-cost alternative has a package that would be difficult for the consumer to open.

Only products within narrowly defined exchange groups, which have the same combination of active ingredient, form of administration, strength, and packet size[7], are considered substitutes. When the physician or pharmacist prevents a switch, the entire cost of the prescription is included in the benefit scheme. Otherwise, only the cost of the cheapest available alternative—called the product-of-the-month (PM)—will be reimbursed according to the benefit scheme.[8] Demand for off-patent drugs is hence steered toward the PM, and becoming the PM is found to be associated with a 70-percentage-points-larger market share, on average (Granlund, 2021).

To decide who becomes PM, prices for off-patent pharmaceuticals are set in monthly sealed-bid first-price sell auctions for each exchange group, where the pharmaceutical providers place bids that, by regulatory fiat, are binding on the manufacturers and pharmacies during the period covered by the auctions. Pharmaceutical firms are free to set their prices in these auctions, but the Dental and Pharmaceuticals Benefits Agency (DPBA) must approve the price for the product to be included in the pharmaceutical benefit scheme. The DPBA approves prices not exceeding a price cap equal to the highest existing price of exchangeable products. The price cap is dynamic in the sense that it changes as the highest existing price is adjusted over time.

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[6] The complaint against Teva and alleged co-conspirators can be found at https://portal.ct.gov/-/media/AG/Press_Releases/2019/FINAL-UNREDACTED-Teva-Complaint-for-CT-District-Court.pdf, accessed 2023-09-28.

[7] Packet size is allowed to vary slightly—for example, substitution can be made from a 30-pill package to a package in the 28–32-pill range.

[8] To allow pharmacies to clear excess inventory, the previous month's PM can be sold without additional cost to the consumer during the first 15 days of a month. No PM is declared before generic competition is established or during the first two months of generic competition.

Pharmacies are not allowed to negotiate discounts from the national prices determined by the auctions or to give discounts to consumers.[9] As for pharmacies' retail margin, it is set in a regulatory process and can be expressed mathematically, so that the wholesale prices offered by the providers completely determine the retail prices as well.

The rules determining what information is available to the firms when setting the prices can be summarized as follows:

(i) At the end of Month 1, all firms that wish to sell a particular product during Month 3 submit a price bid to the DPBA.

(ii) During Month 2, the DPBA will announce the winner for Month 3. The winner's product is called "product-of-the-month" (PM) for that exchange group and month. For a product to be a PM, it must be the cheapest product within the exchange group (in terms of pharmacies' sales prices per smallest unit, e.g., per pill) and, since November 2014, the pharmaceutical firm must have actively guaranteed it to be available across the entirety of Sweden throughout the month.[10] If two or more firms submit identical bids, they will all be called PM.

(iii) Sales during Month 3 will be paid for according to the bids submitted in Month 1.

Note that when firms submit their bids for Month 3, the prices that will apply in Month 2 have already been announced. Consequently, a firm that deviates from a collusion can be punished by others with only one month's delay, which increases the stability of collusions.

Several other characteristics of the market structure are also worth noting from a collusion perspective. First, there are only a few firms bidding to become PM.[11] Although empirical studies are scarce (Ivaldi et al., 2003; Davies and Olczak, 2008; Davies et al, 2011), theoretical contributions regarding the impact of the number of firms on the likelihood of collusion suggest that collusion is facilitated in markets with few firms (Chamberlin, 1929; Stigler, 1964; Selten, 1973; Shapiro, 1989; Huck et al, 2004; Garrod and Olczak, 2018), and for the U.S. pharmaceuticals markets specifically, this has been suggested to be the case by Cuddy (2020), Clark et al. (2022), and Starc and Wollman (2022).[12]

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[9] For on-patent pharmaceuticals, discounts for pharmacies are legal and occur and pharmacies can also give consumers discounts on parallel imported products (Granlund, 2015).

[10] According to TLVFS 2011:4, firms needed to guarantee availability from October 2011, but until November 2014, the DPBA assumed that firms guarantee availability unless they have notified them otherwise (email from the DPBA August 31, 2021).

[11] This is also the case for the U.S. pharmaceuticals markets. Berndt et al. (2017) reported that most U.S. generic pharmaceutical markets are served by 2–3 firms and that there is a considerable share of monopolies even after patent expiration, while Cuddy (2022) reported less than 5 bidders per pharmaceutical substance on average in her sample of pharmaceuticals, and that markets with more than 10 bidders are rare.

[12] The facilitating impact of a low number of firms on collusion can to some extent be mitigated if there are low barriers to entry (Cuddy, 2020; Starc and Wollman 2022). However, as shown by Friedman and Thisse (1994), collusion might be sustainable even in markets with free entry. In the Swedish pharmaceuticals market, there is a 300,000 SEK (29,200 USD) licensing fee to market a generic where the originator drug is authorized in Sweden, accompanied by a 60,000 SEK (5,840 USD) yearly fee. Additional combinations of form or strength for a previously licensed product are subject to an additional fee of 30,000 SEK (2,920 USD), (https://www.lakemedelsverket.se/en/permission-approval-and-control/marketing-authorisation/fees#hmainbody3), accessed May 5, 2023.

Second, both theoretical (Stigler, 1964; Green and Porter, 1984; Abreu et al, 1986) and empirical work (Kühn, 2001) suggest that transparent markets with detailed and frequently updated information on prices and quantities are more susceptible to collusion. The bidding process used to determine the PM in Sweden provides firms with data on competitors' prices, while data on quantities sold are also easily obtainable. Again, there are similarities with the U.S. market, where generic manufacturers report the average wholesale price and wholesale acquisition cost for each generic product that they offer. In addition, generic manufacturers that enter into a Medicaid agreement must report their average manufacturer prices to the federal Centers for Medicare and Medicaid Services on a monthly and quarterly basis (Complaint, 2019, pp. 23 and 24).

Third, most sellers of generics are active in several exchange groups (i.e., markets), making multimarket contact a strong feature of these markets. As pointed out by Bernheim and Whinston (1990), increasing the number of markets might just raise the cost and benefits of deviation from collusion proportionally, in which case the likelihood of collusion is unaltered by multimarket contact. However, Bernheim and Whinston (1990) also show that if there is one market where collusion is stable and profitable, the threat of retaliation in that market will facilitate collusion in other markets as well. Empirically, a multitude of papers analyzing different markets have shown that multimarket contact increase prices, which has in general been seen as an indication that it facilitates collusion (Bilotkach, 2011; Ciliberto and Williams, 2014).[13]

Research regarding how regulations on the consumer side of the market affect the likelihood of collusion is scarcer, with most of the focus being on how growth or decline in demand affects collusion. Given that an increase in future demand also increases the severity of any punishment for defection from the collusive agreement, this will facilitate collusion. A corollary to this is that markets with pronounced demand fluctuations are less likely to sustain collusion because all firms know that the gains from deviation are at their largest when the market is at a peak, while the impact of future punishments is at a low (Rotemberg and Saloner, 1986; Haltiwanger and Harrington, 1991). These are, however, both special cases of Stigler's (1964) more general observation that collusion is facilitated by homogenous consumer behavior and that homogeneous size and predictable timing of purchases are important factors affecting the likelihood of collusion.

Consumers in the Swedish markets (with chronic conditions) often get prescriptions with four fillings per year. In these cases, the consumer could opt to have all four fillings dispensed at one time, but doing so will have a large impact on the re-imbursement from the government-funded benefit scheme. Under the benefit scheme, pharmacies can only dispense pharmaceuticals for the next 90-day period, and new fillings can at the earliest be made after two-thirds of the 90-day period has elapsed. In addition, advertising aimed at consumers is prohibited by law and neither producer nor pharmacy is allowed to grant discounts or make other offers directly to consumers. As such, much of the competition in these markets is one-dimensional regarding price, and the set-up of the insurance program makes the size and timing of purchases predictable for the firms, thus further facilitating collusion.

[13] Multimarket contact was also a common feature in the markets affected by the U.S. generic pharmaceuticals cartel. The "fair share" agreements in the cartel were not limited to any one market; rather; the agreements guided the market actions of generic drug manufacturers both within and across product markets (Complaint, 2019, p. 45).

4. Data[14]

This study is based on a panel dataset obtained by merging different datasets compiled by the DPBA. The dataset contains all products included in the PM-system from March 2010 through September 2020. Most importantly, it includes information regarding which exchange groups products belong to, identity variables of the products and information of the price and PM status of each product each month. It also lists the active ingredient(s), strength, administrative form, and package size of each product, and for each month it identifies the seller of the product and the quantity sold. We complemented the datasets compiled by the DPBA with datasets prepared by the company IQVIA (formerly IMS), which contains similar information. The IQVIA datasets are used to follow exchange groups over time (the DPBA have changed the numbering of exchange groups several times), to check for consistency across the two data sources, and to generate some control variables. In addition, a dataset provided by the County Council of Västerbotten and described in Granlund (2021) is used to generate one variable.[15]

Because firms must guarantee availability for products to be eligible to be a PM since November 2014, we use data from November 2014 through September 2020. We include exchange group and month combinations with positive sales and at least one product marketed by a potential bidder being declared a PM.[16] Also, because our purpose is to study interactions between firms, we exclude exchange groups and months with just one potential bidder. In addition, 0.2% of the product by month observations are excluded because they may be affected by errors in the exchange group variable.

4.1. Definition of potential bidders and potential low-price bidders

Because colluding firms can choose not to bid when it is not their turn to win, we cannot restrict our focus solely to actual bidders, but rather must define who the potential bidders are. In the main analysis, we consider a firm as a potential bidder month t if it for some month between month $t-2$ to $t+2$ for exchange group e has placed a bid and sold at least one package. Including potential bidders increases the number of product-by-month observations from 272,282 with a current bid and positive sales in the current month to 293,352 product-by-month observations. If the quantity requirement had not been added, we had obtained 368,553 observations. Of the observations not fulfilling the quantity requirement, 48% (35,878) had no sales any time during the study period.

Our choice of a five-month period (from $t-2$ to $t+2$) is partly because preliminary evaluations of the data indicated bid-rotations involving up to five firms. Applying our definition of potential bidders, we avoid excluding a firm that does not bid in between the months it was the designated winner. Of course, it is possible that we over- or underestimate the number of potential bidders close to entry and exit. For example, if the number of potential bidders increases from four to five in March, we might set the number of bidders as equal to five already in January. What is more, a firm that has been part of a five-

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[14] In Appendix D, a list of definitions and descriptive statistics for the variables used in each section below is provided.

[15] The variable $DiffHarv_{fet}$ is defined in section 5.3.

[16] No PM is declared before generic competition is established or during the first two months of generic competition. Of the observations within exchange group × month combinations without any PM, 57% are from an exchange group and month with no, or just one, locally sourced generic product. The corresponding figure for the data that is kept is only 11%.

firm bid-rotation might have been a potential bidder three or four months consecutively without placing a bid but then choose to leave the market because the collusion broke down. Due to such possibilities, we also conduct a sensitivity analysis when only including active bidders, defined as having placed a bid for month t and sold at least on package from $t - 1$ through $t + 1$. In practice, including potential bidders does not matter a great deal. In the estimation sample, the mean number of potential ($nbid_{et}$) and active bidders ($nbida_{et}$) are 4.8 and 4.7, respectively, and the two variables have a correlation of 0.99.

Some parallel importers submit prices on more than one product per exchange group and month because parallel imports from different countries are classed as different products. Moreover, some originators (and less often sellers of generics) submit prices on more than one product per exchange group and month, for example, because they want to sell both blisters and tins or both a 98-pill package and a 100-pill package. As a consequence, the number of unique firm × exchange group × month combinations fall short of 293,352, and more specifically amounts to 274,147 observations. Of these, firms had not submitted any bid for the current month in the exchange group for 3,268 observations and an additional 13,804 observations was for firms that had no sales in the exchange group × month combination.

If a firm sells two products with different prices per pill in an exchange group, it knows that the more expensive product cannot become a PM. Hence, submitting prices for several products per exchange group and month does not affect the likelihood that at least one of the firm's products will be a PM. Because of this, and given that the focus of the paper is to study collusion and not the choices of product portfolios, we aggregated observations to firm × exchange group × month combinations when studying winning patterns, and define the variable PM_{fem} to take the value one if at least one of firm f's products in exchange group e is PM in year-month m.

Of the firms that sell the cheapest products, 80% also sell a PM, but 28% of the cases when $PM_{fem} = 1$ apply to firms that do not sell the cheapest product. The correlation between selling the cheapest product and being a PM is 0.68. It is 0.73 in exchange groups × month combinations without parallel imports, but only 0.57 in exchange groups × month combination with parallel imports, because parallel importers relatively often cannot guarantee that they will meet all demand during a month.

Most exchange groups are vertically separated in the sense that many sellers of originators, including parallel imported ones, consistently sell at prices exceeding the price of the cheapest product. Also, for some generics, prices consistently exceed the lowest price in the exchange group. Instead of aiming to become the PM, the sellers of these products seem to focus on selling to consumers who are prepared to pay extra for their products. Column 4 of Table 1 provides evidence of this vertical separation by showing that for originators and parallel importers the share that sell a PM is much lower than for generics.

When calculating the probability that the price pattern is caused by collusion in section 5, we use the number of potential low-price bidders in each exchange group by month observation. We have classified firms as currently being in the low-price segment in an exchange group if (a) at least once in the current month, or in the preceding or following two months, one of its products was a PM, or (b) in the current month it sells a product that is declared to be available and has a price that is equal to or below the price of the PM in the last or the second-to-last month. Criteria (a) is primarily motivated by the notion that firms currently involved in a collusion in the low-price segment should be considered to belong to this segment (see the discussion in the first two paragraphs of this subsection).[17] Criteria (a) also ensures that most firms that have aspired to be a seller of a PM in the exchange group for several subsequent months are classified as being in the low-price segment. Still, some firms might fail to become a PM seller even when attempting. This is particularly important when the number of competitors is high, so the probability of losing when attempting to win is large. Therefore, criteria (b) is added. Products that fulfill criteria (b) have a 67% chance of becoming a PM, but products that fail to fulfill the criteria only have a 7% chance.

In the last column of Table 1, we present descriptive statistics for the low-price segment classification. Two-thirds of the observations are classified as belonging to the low-price segment. As expected, the share is highest for generics firms and lowest for originators.[18]

Table 1. Descriptive statistics for firm categories

Category	Number of obs.	Share of obs.	Share of PM	Share of category that is PM	Share of category in low-price segment
Generics	212,634	77.56%	88.27%	29.13%	75.41%
Originators	38,102	13.90%	6.81%	12.54%	27.67%
Parallel imp.	23,411	8.54%	4.92%	14.75%	45.97%
All	274,147	100%	100%	25.60%	66.26%

Note: Firms are categorized by which category of products they sell in each exchange group each month. No firm sells both locally sourced originators and parallel imports in the same exchange group and month. Nine percent of originators and 3% of parallel imports are for firms that sell both originators and generics and both parallel imports and generics, respectively, in the same exchange group and month.

Figure 1 and Figure 2 display histograms for the number of low-price bidders and all bidders, respectively. According to our classification, there are at most nine low-price bidders, but there are only three exchange group by month observations with eight or nine low-price bidders.

[17] All firms involved in collusion between two or three firms, in addition to most of the firms involved in the relatively rare collusions involving four or five firms, will be considered as being in the low-price segment because of criteria (a). Still, in the first (first two) month(s) of bid-rotation involving four (five) firms, a firm might not be classified as being in the low-price segment if it has not recently sold a PM. The same holds for the last and last two months, respectively, and this might cause the number of low-price bidders to fall short of the number of participants in a possible collusive scheme. However, Figures 7 and 8 reveal that the number of low-price bidders only falls short of the number of participants for a single observation when the number of participants in a possible collusive pattern is four or five and it is possible that it was no collusion in that case.

[18] Sellers sell a PM in 39% of the months when they are classified as being in the low-price segment. On average, we obtain 36 observations per exchange group and firm. Over all months, sellers that are currently in the low-price segment belong to this segment for 83% of the months and sell a PM in 34% of the months. Sellers currently not in this segment belong to this segment in 34% of the months but only sell a PM in 10% of the months.

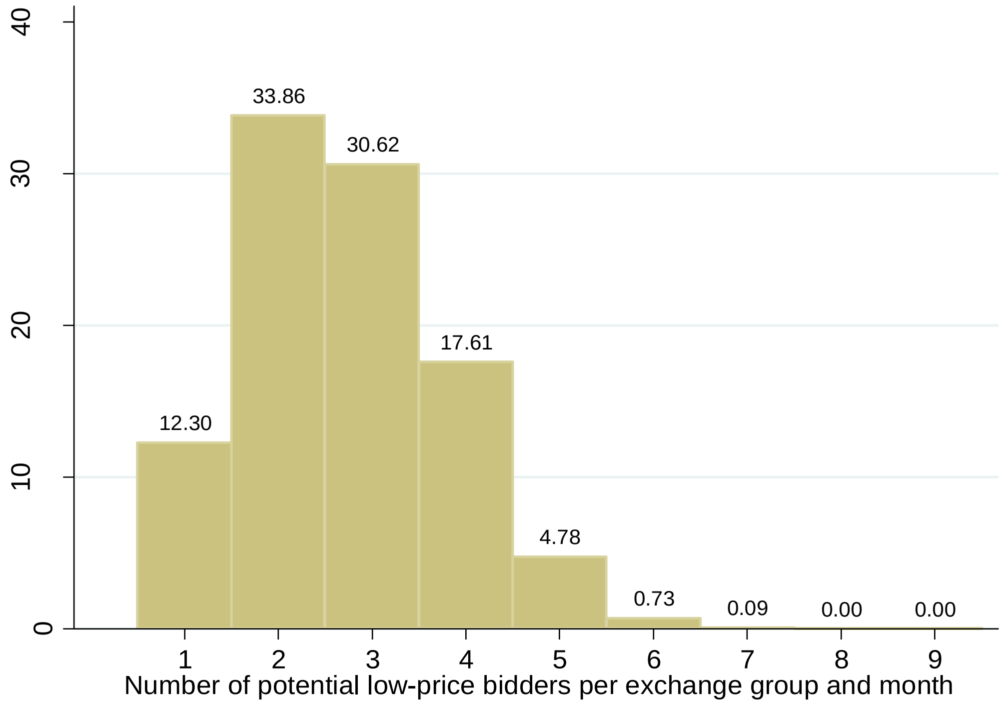


Figure 1. *Histogram of potential low-price bidders per exchange group × month combination.*

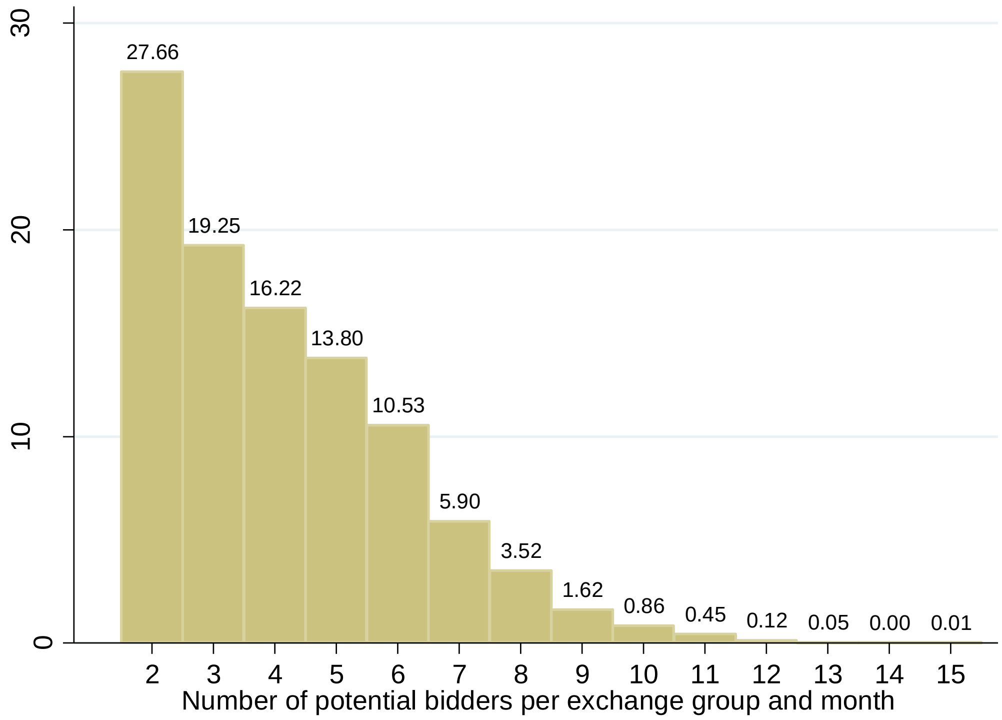


Figure 2. *Histogram of all potential bidders per exchange group × month combination.*

Figure 3 shows that only one firm sells a PM in 94.81% of the exchange groups by month observations, there are ties between two firms in 5.11% of the observations, and ties between three firms in 0.09% of the observations.

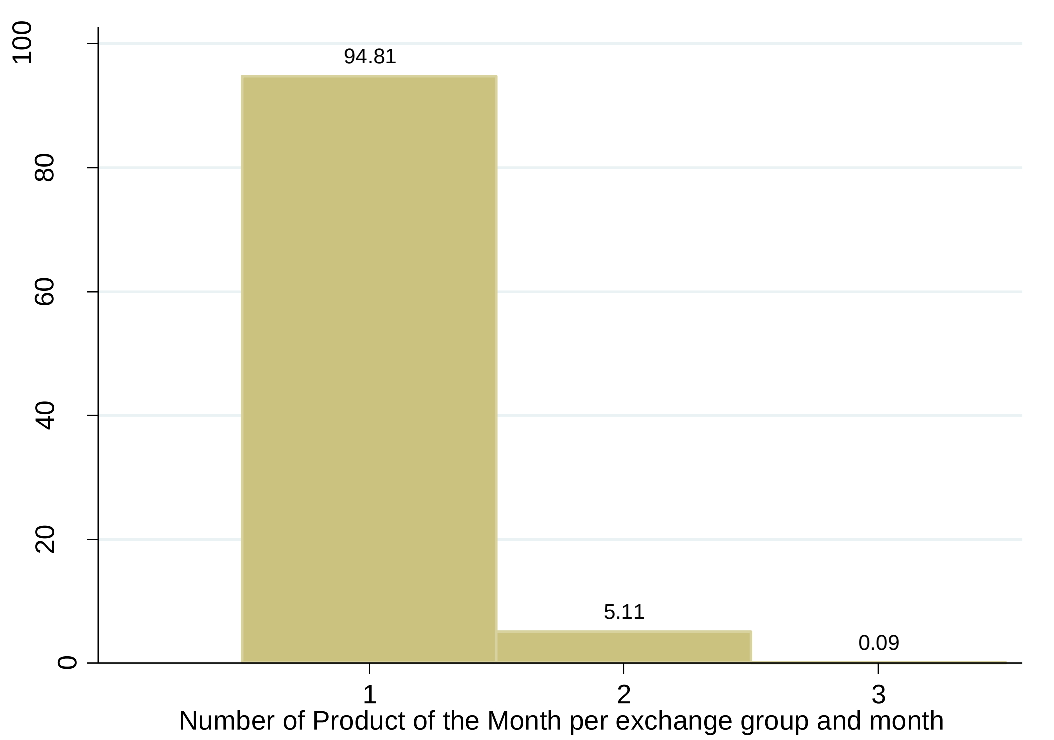
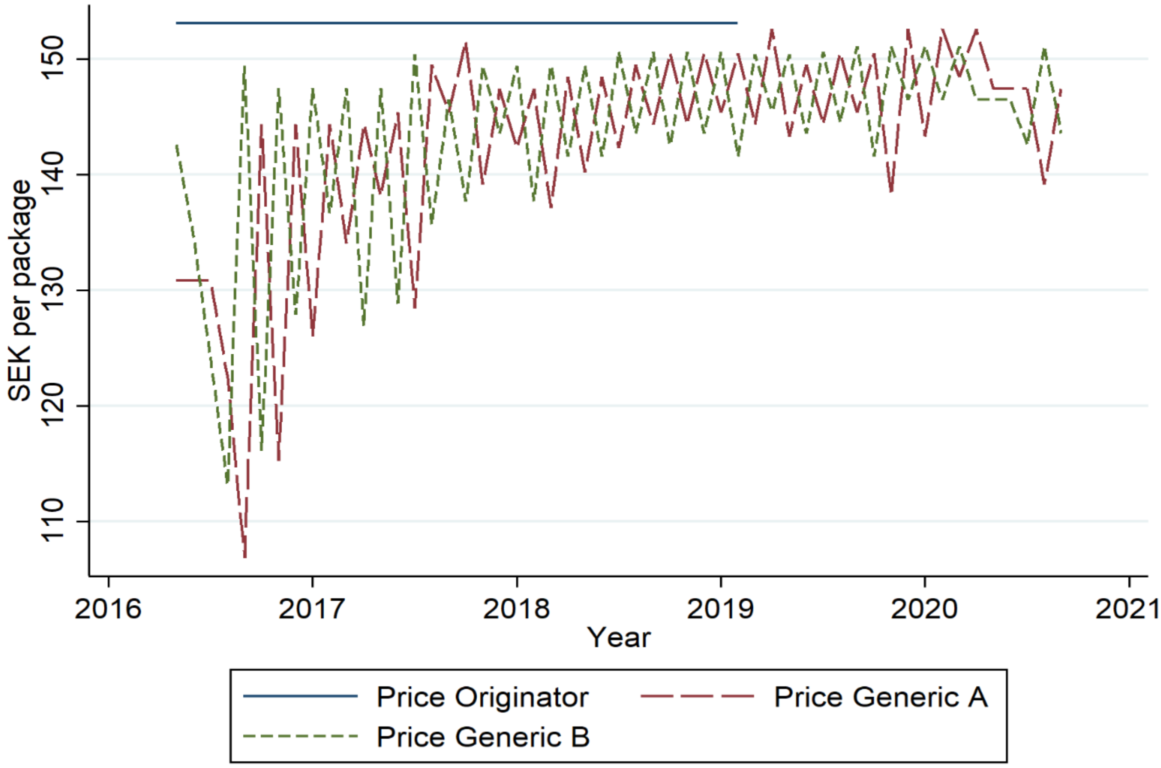


Figure 3. *Empirical distribution of exchange group by month observations of number of firms that have a product declared to be the PM at the beginning of a month.*

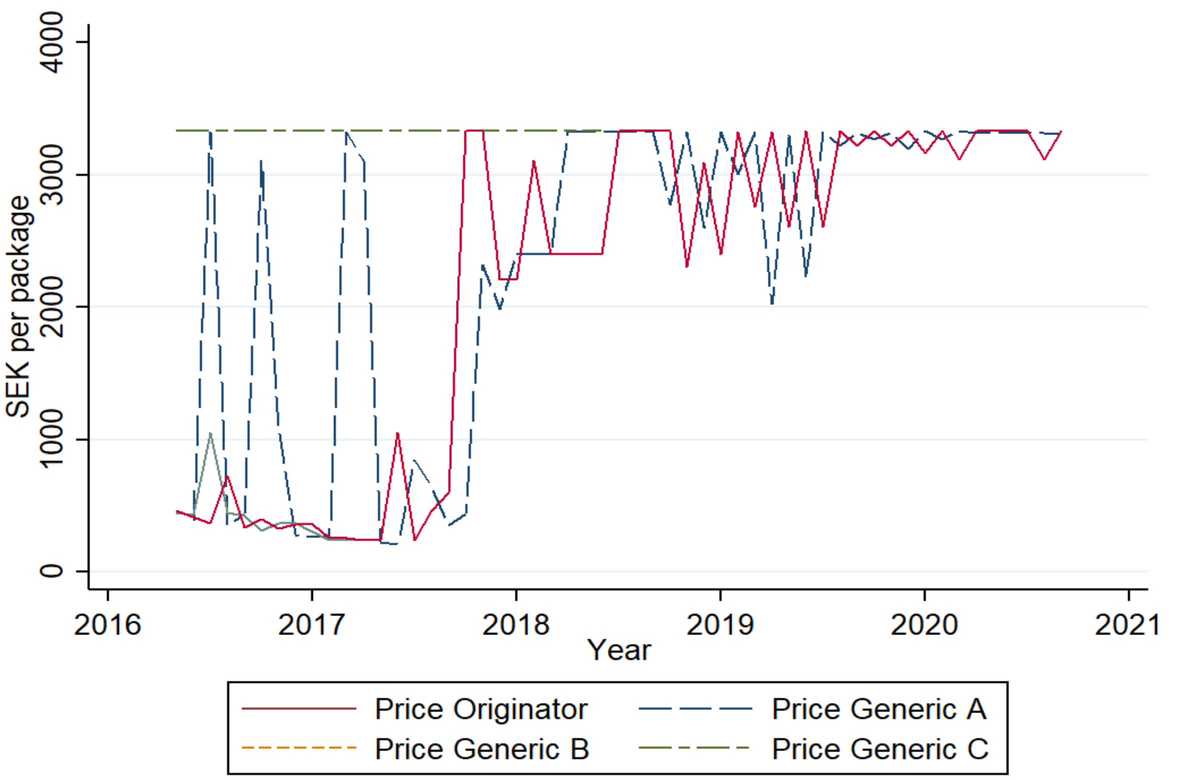
4.2. Winning patterns

Figures 4(a) and (b) display two empirical examples of firms' prices in exchange groups where one could suspect that the low-price bidders colluded during parts of the study period. In Figure 4(a), the low-price bidders start bid-rotating in the middle of 2016, continuing until 2020, with the price of the low-price bidders consistently increasing toward the price cap of SEK 153. In Figure 4(b), we find a similar pattern that also suggests a link between the number of firms and the likelihood of collusion.

Figure 4(b) shows an exchange group with a clear negative trend in the total number of packages sold and where some generics left the market prior to June 2016, which is the first month included in the figure. In 2017, Generic C left and in 2018 the originator also exited the market. The remaining two firms won every other month from October 2018 through April 2020. During the first part of this potentially collusive period, the winning firm set a price that was significantly lower than the losing bid, but in the end the winning bids were also close to the price cap of SEK 3333. Moreover, when there were three generics firms active in the market, Generic A set very high prices in some months, which can indicate a desire to establish a bid-rotation, but the stable bid-rotation was achieved first more than a year after generic C left the market (even though the price patterns during 2016 are consistent with three sequences of three-firms bid-rotations lasting 3–4 months). Hence, this exchange group seems to be an example of when collusion is facilitated by exits. It is more difficult, though, to determine why the bid-rotation ended in 2020, with Generic A winning at a high price four months in a row.



(a)



(b)

Figure 4(a) and (b). *Examples of two-firm bid-rotation.*

What we would like to establish is the probability that observed patterns like the ones in Figures 4(a) and (b) are the result of collusion. In section 5, we describe the reasons why firms relatively frequently win every other or every third month also during competition, and we will account for this when we in section 6 calculate the probability that collusion has occurred during the observed winning pattern. For now, we simply state that *f* firms winning every *f*th month are winning patterns that are consistent with collusion using bid-rotation, even though these patterns could also be the result of competition. Similarly, we classify patterns with the same two or more firms being shared as winner for at least three months as patterns consistent with collusion using parallel bidding.

We define 24 different winning patterns, *W*, that are consistent with collusion. Table 2 shows descriptive statistics for these winning patterns for the 28,863 exchange group by month observations that have at least two low-price bidders for all months from $t-10$ to $t+10$. These observations are for 891 exchange groups from September 2015–November 2019 and their sales account for 67% of the sales (measured in pharmacies' purchase prices) within exchange groups and month with at least two low-price bidders and for 55% of all sales within the PM-system during this period. Note that there is no bid-rotation among more than five firms (that last more than five months) and no parallel bidding involving more than two firms (and lasting three or more months). The descriptive statistics also show the cumulative share of observations with patterns weakly exceeding 3 and 5 months, respectively. In total, 89% (≈90.85% – 2.13%) of the observations are consistent with some sort of bid-rotation lasting at least three months. This is not surprising given that all three-month sequences with just a single winner each month are consistent with bid-rotation, except if a firm wins twice in a row. As we will return to in the following section, this high share of short episodes consistent with bid-rotation is to a large extent explained by winning patterns that are consistent with bid-rotations for 3–4 months occurring by chance also when firms actively compete. It is more interesting to note that 15% of the exchange group by month observations are part of patterns that are consistent with a collusion that has lasted at least 11 months. Table 2 also indicates that collusion seldom takes the form of parallel bidding, and in this sample, we observe no parallel bidding lasting three months or more that involve more than two firms.[19]

Table 2 Share of exchange group × month combinations that are part of different possible collusive patterns

	3–4 months	5–6 months	7–8 months	9–10 months	≥11 months	**≥3 months**	**≥5 months**
2 firms bid-rotation	8.12%	4.19%	1.74%	1.45%	9.06%	24.56%	16.44%
3 firms bid-rotation	21.99%	9.91%	3.30%	1.65%	3.82%	40.67%	18.67%
4 firms bid-rotation	9.16%	7.71%	1.36%	0.26%	0.07%	18.56%	9.40%
5 firms bid-rotation	-	4.38%	0.53%	0.01%	0%	4.92%	4.92%
2 firms parallel bidding	0.10%	0.07%	0.06%	0.07%	1.83%	2.13%	2.03%
SUM	39.38%	26.26%	7.00%	3.44%	14.78%	90.85%	51.47%

Note: The entry of 0% indicates that no observation is part of 5 firms bid-rotation weakly exceeding eleven months. The number of observations is 28,863.

[19] Appendix A provides histograms of the duration of possible collusive patterns using also older data.

In later sections, winning patterns consistent with collusions, W, are denoted *b2m34* when two firms win every other month for 3–4 months, where *b* indicates bid-rotation, *2* indicates the number of firms involved in the pattern, and *m34* indicates that the duration is 3–4 months. Parallel bidding patterns are denoted with an initial *p* instead of an initial *b*. More precisely, W equals *b2m34* for exchange group by month observations that are part of winning patterns consistent with two firms bid-rotation for at least three months, but not consistent with two firms bid-rotation for five months or more, or with more than two firms bid-rotating three months or more when $n_{et} > 2$. The latter condition is set to avoid double classification and it means that if the sequence of winners is A, B, A, C, then W do not equal *b2m34* for the middle two observations, which can be the last months of two-firms bid-rotation and also the first months of three-firms bid-rotation. Instead, W = *b3m34* for these observations.[20] Similarly, W equals *b2m56* for exchange group by month observations that are part of winning patterns consistent with two firms bid-rotation for at least five months, but not consistent with two firms bid-rotation for seven months or more, or with more than two firms bid-rotating five months when $n_{et} > 2$. Corresponding criteria apply when W equals *b2m78* and *b2m910*. Lastly, W = *b2m11* for exchange group by month observations that are part of winning patterns consistent with two firms bid-rotation for at least eleven months, and not consistent with more than two firms bid-rotating eleven months or more when $n_{et} > 2$.

We use corresponding definitions for winning patterns consistent with bid-rotation among 3, 4, or 5 firms, as well as winning patterns consistent with parallel bidding among 2 firms. However, the minimum duration of bid-rotation among 4 or 5 firms are 4 and 5 months, respectively. Like the definitions for two firms bid-rotations, the indicator variables for three or four firms bid-rotation take the value zero for observations that are also consistent with bid-rotation among more firms for, at least, as many months.

Figure 5 below summarizes the share of observations that belong to different possible collusive patterns. Unlike in the tables, the shares of patterns consistent with bid-rotation and parallel bidding are added together and, to better illustrate the survival probability of patterns, the figure presents the share of observations in patterns that weakly last more than 3, 5, 7, 9, and 11 months. For patterns weakly exceeding seven months, the figure illustrates the large difference between exchange groups with two low-price bidders and exchange groups with more low-price bidders. In exchange groups with two low-price bidders, 55% of exchange group by month observations belong to possible collusive patterns lasting seven months or longer. The corresponding numbers for exchange groups with three, four, or five and six or more low-price bidders are 21%, 9%, and 6%, respectively. However, the short patterns are so much more common in exchange groups with three or more low-price bidders that the share of observations in patterns lasting at least three months is slightly smaller for exchange groups that currently have two low-price bidders. A possible explanation for this is that for short patterns, the effect

[20] Whether observations are classified as being part of a bid-rotation between two or more than two firms only affects the descriptive statistics, as we group bid-rotation patterns of the same duration (e.g. $\geq$ 11 months) together irrespective of the number of firms involved when calculating the probability of collusion, as explained at the end of section 6.1. That observations that could be part of either bid-rotation involving two or three firms are only classified as being part of a three-firm rotation when $n_{et} > 2$ seldom affects the classification. One example of how this can affect classification is if first $n_{et} = 3$ and the three firms win one month each, and then a fourth firm enters and wins, because then the pattern changes classification from *b3m34* to *b4m34* when n_e changes from 3 to 4. This classification choice increases the share with W = *b3m34* by four percentage points, reduces the share of observations with W = *b5m56* by five percentage points, and also has minor effects on the shares of other short bid-rotation patterns.

that collusion has on the number of bidders, by attracting new firms and delaying exits, dominates the causal effect of the number of firms on the probability of collusion.

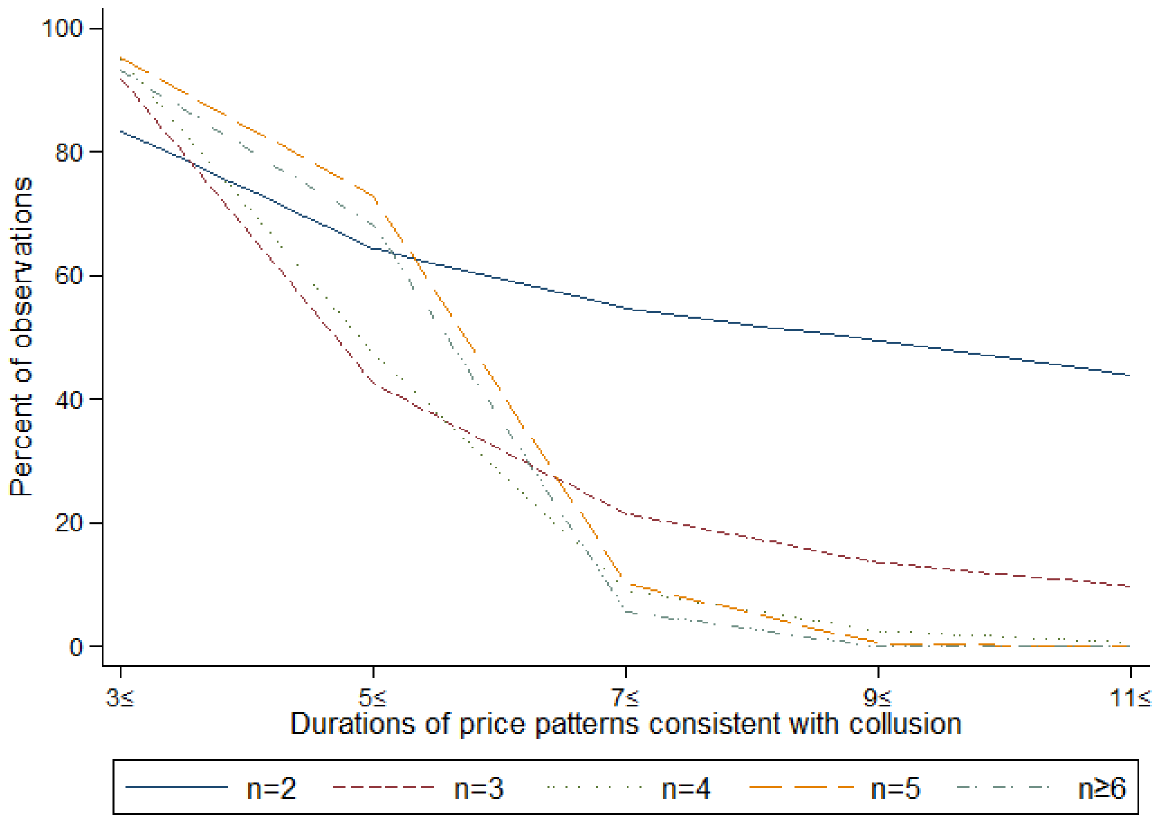


Figure 5. *Share of observations in patterns consistent with bid-rotation or parallel bidding lasting at least 3, 5, 7, 9, or 11 months for different numbers of low-price bidders.*

Figure 6 displays the empirical distributions of low-price bidders and all bidders separately for exchange group by month observations that are, and are not, part of a bid-rotation or parallel bidding that lasted at least nine months. Our calculations described in the following sections suggest that 99% of bid-rotations or parallel bidding that weakly exceed nine months are at least partly caused by collusion, while over a third of the shorter patterns arose during competition.

Figure 6 shows that the longer possible collusive patterns are highly over-represented in exchange groups with two low-price bidders, which is consistent with theoretical and experimental findings suggesting that collusion is most likely to occur when there are few bidders in a market (Selten, 1973; Huck et al., 2004; Fonseca and Normann, 2012; Horstman et al., 2018). The right panel shows a large over-representation of long possible collusive patterns for exchange groups with three bidders. This is expected given the pattern shown in the left panel and the fact that it is common that the originator sells branded products to loyal consumers at high prices.

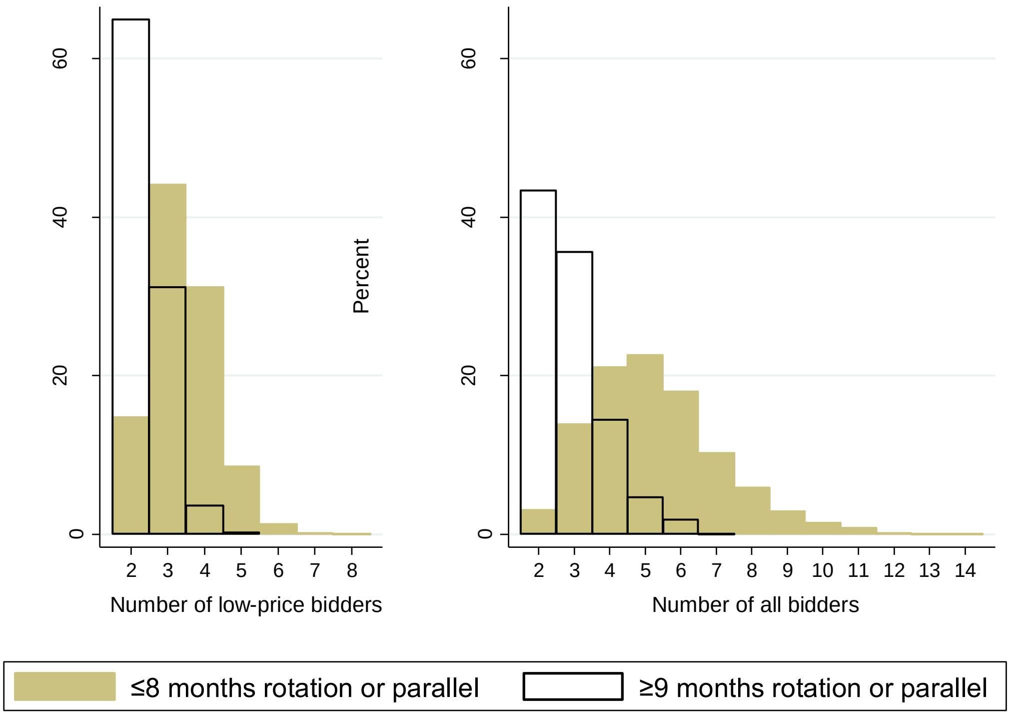


Figure 6. *Histogram of low-price bidders (left panel) and all bidders (right panel) per exchange group × month combination: separate for observations that are, and are not, part of any bid-rotations or parallel bidding weakly exceeding nine months. The number of observations is 28,863.*

Figure 7 shows that for short possible collusive patterns there are significant discrepancies between the number of potential low-price bidders and the number of participants in possible collusive patterns, while Figure 8 illustrates that these two numbers are identical in 95% of cases for patterns weakly exceeding 9 months. A large percentage of the discrepancies for the short patterns could be for patterns that occurred by chance during competition, for example, that four firms actively competed in the low-price segment, but that only two firms won and that they won every other month for a few months. Another explanation is that we include potential bidders and that firms can therefore be classified as low-price bidders two months before their first bid and two months after their last bid. This leads to an overestimation of the number of firms directly prior to entry and directly after exit, which we address in a robustness analysis by including only active bidders. However, we use potential bidders in the main analysis because firms can choose to not place any bids when it is another firm's turn to win.[21] In Figure 7 we also note that there are a few cases where the number of low-price bidders falls short of the number of participants in the pattern; more precisely, there are nine cases with three participants, which can occur if a low-price bidder is bought by, or merges with, another firm during a possible collusive pattern, and one with five participants, which is discussed in footnote 17.

[21] Of the exchange group by month observations with a predicted probability of collusion exceeding 0.90, 1.36% have more participants in the likely collusive pattern than the number of active bidders, while the number of potential bidders never exceeds the number of participants when the probability of collusion exceeds 0.90.

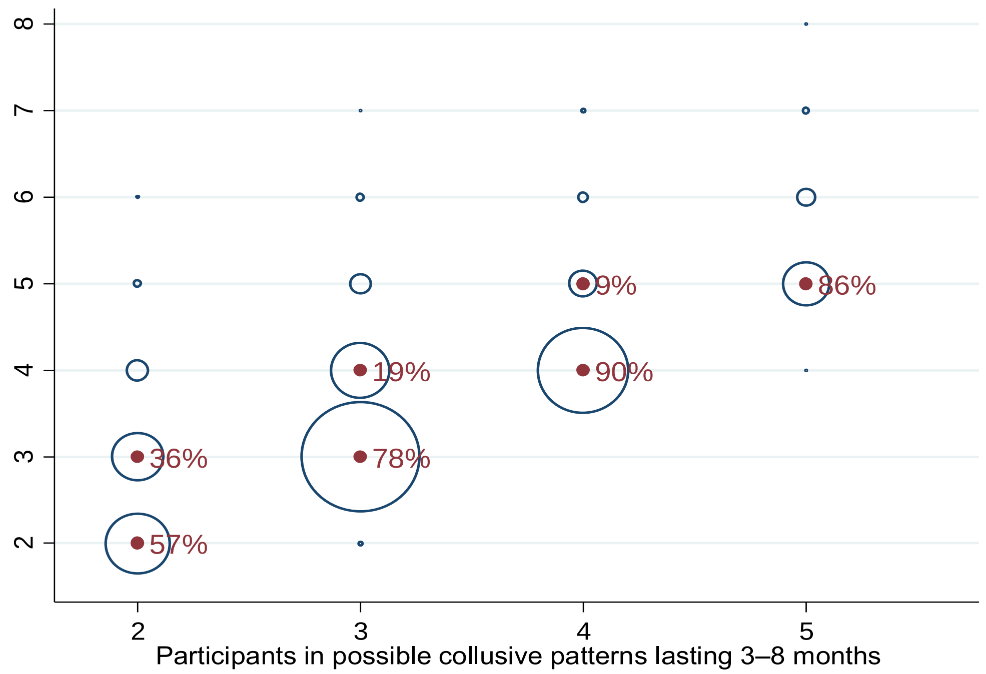


Figure 7. *Scatterplot for possible collusive patterns lasting 3–8 months. The size of the circles indicates each combination's share of all observations represented in the figure. The percentage figures show the share of the observations with a given number of participants in possible collusive patterns, for patterns lasting 3–8 months.*

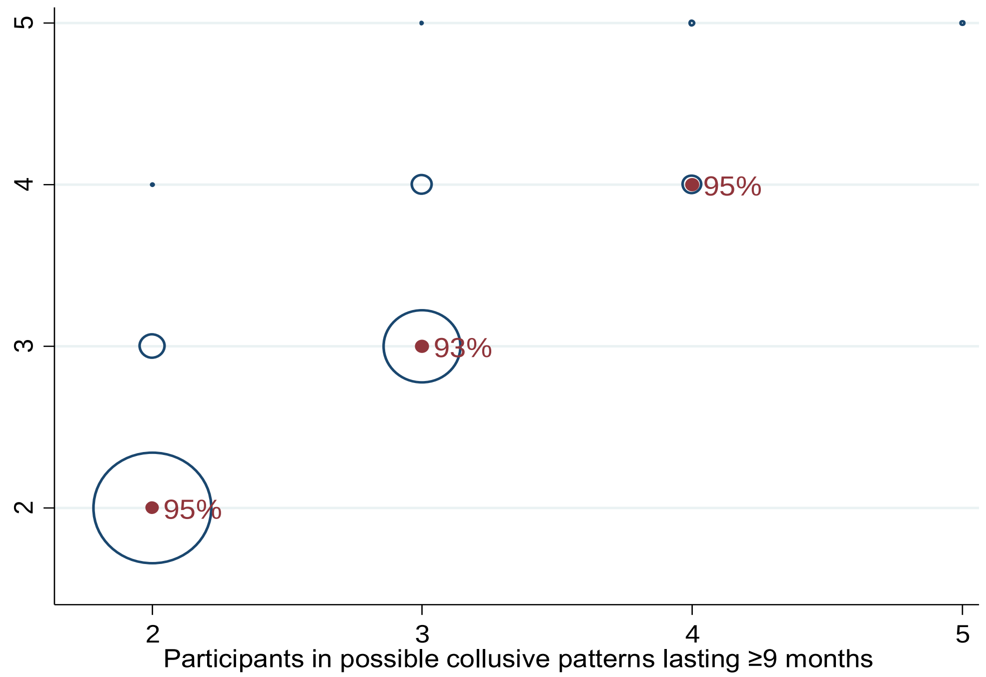


Figure 8. *Scatterplot for possible collusive patterns lasting 9 months or more. The size of the circles indicates each combination's share of all observations represented in the figure. The percentage figures show the share of the observations with a given number of participants in possible collusive patterns, for patterns lasting nine months or longer.*

5. Competitive bid behavior

5.1. A mixed strategy equilibrium

During competition, firms classified as low-price bidders are expected to apply mixed pricing strategies. One reason for this is that they face demand from some price-insensitive consumers to whom they can sell even if they charge the maximum price permitted within the benefit scheme, P_O. These price-insensitive consumers include those for whom the prescriber or pharmacist has disallowed substitution, and can also include consumers with strong preferences for specific products and consumers who do not trust that exchangeable products are medically equivalent and therefore buy the prescribed product. Even though the number of price-insensitive consumers is relatively small, they imply that firms can secure revenues that exceed their variable cost by setting high prices. Therefore, a pure strategy in which the price is set as being equal to the marginal cost is a strictly dominated strategy for firms with some price-insensitive consumers.

The price-setting incentives for competing low-price bidders can be understood using the model of sales proposed by Varian (1980). The model states that when some consumers' choices are not affected by relative prices, bidders will randomize their bids. Varian focused on cases where some consumers are uninformed of the relative prices, but the same results are obtained if the consumers whose choices are not affected by relative prices are loyal to some sellers, which could be because the prescriber disallowed substitution. With some loyal consumers and marginal costs normalized to zero, low-price bidders in the generics markets will randomize their bid over the interval [P_{min}, P_O] (Bergman et al., 2017), where

$$P_{min} = P_O \frac{1 - S_w - S_H}{S_w(n-1)}. \tag{5.1}$$

In the equation, S_w is the market share of the winner, S_H is the joint market share of all high-price bidders, and *n* is the number of low-price bidders.[22] The logic here is that a firm can secure a revenue of $P_O \frac{1-S_w-S_H}{(n-1)}$ by setting the highest permissible price, and that $P_{min}S_w$ must be equally large. The exact result builds on the simplifying assumption that the choices among low-price products that are not the cheapest are not affected by relative prices. However, even if this result is relaxed, firms will randomize their prices (Barut and Kovenock, 1998).

Of course, the result rests on more simplifying assumptions. For example, that the marginal cost is normalized to zero also implies that it is constant and equal across all firms. We argue that constant marginal cost is a good approximation in the long run. Firms should be able to meet all demand from the small Swedish markets with unaffected marginal cost if they can foresee the level of demand when making their production and purchase decisions. Unforeseen sudden changes in demand can, however, affect marginal cost in the short run. For example, firms might need to use costlier transportation to meet an unforeseen sudden increase in demand—in section 5.3 we describe how we account for this. Another

[22] High-price bidders are firms that have so many loyal consumers that the profit they gain from selling only to these strictly exceeds the profit they would gain by setting a price that gives them a chance to also capture the sales to consumers who choose the cheapest product.

issue is that marginal cost in the long run might vary across firms. If the difference is sufficiently large,[23] the equilibrium could be that the firm with the lowest marginal cost always win. Markets where this is the case will have only one low-price bidder according to our definitions, and will therefore be excluded from the empirical analysis where we study collusion among low-price bidders.

5.2. Probability of ties during competition

Another simplifying assumption in the model just described is that prices are assumed to be continuous. In practice, prices are discrete because only two decimal places are allowed when prices are submitted to the DPBA. More importantly, 42% of the price bids are in whole crowns, which further increases the possibility of ties when firms compete by using mixed pricing strategies.

The probability of ties should depend on the number of discrete prices within the interval over which firms randomize their bids, the bid distribution functions, and on the number of bidders. The latter can be formalized as the probability of a single winner should equal x^{n-1}, $x \in (0{,}1)$. The logic of this function is that with certainty (at least) one firm will have the lowest price and therefore the number of trials for a tie is $n-1$.[24] The function x^{n-1} reveals that the probability of a single winner should fall in n, for a given width of the bid-interval and given bid distribution functions.

The parameter x should be a function of $1-1/w$, where $w = P_O\left(1-\frac{1-S_W-S_H}{S_W(n-1)}\right)$ is the width of the bid-interval. In fact, if P_O happens to be equal to the number of possible prices above the marginal cost and the firms have uniform bid distributions, x should equal $1-1/w$. We do not assume uniform bid densities, but assume that $x = 1-1/\left[P_p\left(1-\frac{1-S_W-S_H}{S_W(n-1)}\right)\right]$ and set the parameter P_p to equalize the predicted share of single winners with the observed share among observations that are not part of a possible collusive pattern lasting three months or longer. Still, the predicted share of ties during competition can be affected by attempts to collude, but this is partly balanced by the exclusion of all possible collusive patterns weakly exceeding three months, because this also excludes some patterns that occur by chance during competition.

We let one minus x^{n-1} be the probability of a tie between two or more firms and denote it by PT_{et}. With the values $S_W = 0.794$, $S_H = 0.095$, which are the mean values in the estimation sample, we get $P_P = 12.531$, x ranging from 0.907 to 0.919, and the predicted share of ties between two or more firms is 17.6%. Specifically, the share of ties is predicted to increase from 9.3% when the number of bidders in exchange group e and month t (n_{et}) is equal to 2, to 16.4%, 23.1%, and 29.2% when n_{et} is increased to 3, 4, and 5, respectively.

We follow this approach rather than, for each value of n_{et}, setting the share of single winners equal to the observed frequency, as the stringency of inclusion criteria like "*that the observations that are not part of a possible collusive pattern lasting three months or more*" will differ depending on n_{et}.

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[23] The pharmaceutical firms have some fixed costs for being active in the Swedish pharmaceutical markets, including yearly fees to be paid to the Swedish Medical Products Agency and costs for contact with Swedish wholesalers. Hence, if the difference in marginal cost is so low that the firm with the lowest marginal cost cannot recover its fixed cost by setting a constant price that the competitors cannot match, it is better for this firm to exit the market or apply a mixed pricing strategy.

[24] Note that x^{n-1} is identical to the binomial probability function $\frac{(n-1)!}{0!(n-1)!}(1-x)^0 x^{n-1}$.

5.3. Autocorrelation in winning price bids during competition

When calculating the probability that different winning patterns have occurred during competition, it is also important to quantify autocorrelation in winning probabilities during competition. We identify three reasons why, during competition, the probability of a product becoming a PM in the current month could depend on the PM-status of the product in previous months. The first is that the winner(s) will be the same as the previous month if all firms in an exchange group submit the same bid, there is no entry, and the last month's winner(s) did not exit. This is particularly important when calculating the probability of repeated ties between the same firms. The second reason relates to state dependence, i.e., that some consumers are prepared to pay extra to get the same brand they bought last time, and the third relates to the effect of previous winnings on the quantity in stock. As shown by Zona (1986) and Lang and Rosenthal (1991), bid-rotation-like price patterns might arise under competition if the firms are capacity constrained, and a naïve model would then over-estimate the likelihood of collusion for a given price pattern.

We begin by addressing the issue that the winner(s) will be the same as in the previous month if all firms in an exchange group submit the same bid, there is no entry, and the last month's winner(s) did not exit. We estimate the probability that a low-price bidder will submit the same bid as in the previous month, *U*, to 0.2838 using observed frequencies for products from exchange groups with at least three low-price bidders, which is not part of a possible collusive winning pattern weakly exceeding three months. We estimate this parameter only using exchange groups with at least three low-price bidders to reduce the probability of *U* being affected by attempts to form parallel bidding collusions. As reported in section 4.2, we see no such collusions weakly exceeding three months when $n_{et} \geq 3$.[25]

Then, we estimate the probability that all bids by low-price bidders are the same in months *t+1* and *t* as $U^{n_{et}}E^{n_{et}}S1_{et}$, where $E = 0.9366$ is the probability that a low-price bidder month *t* was this also month $t-1$, so that $E^{n_{et}}$ is the probability that there was no entry to the low-price segment, and $S1_{et}$, which equals 0.9676 when $n_{et} = 3$, is the probability that a winner continues to be a low-price bidder in the next month.[26] Hence, when $n_{et} = 3$, $U^{n_{et}}E^{n_{et}}S1_{et} = (0.2838^3 \times 0.9366^3 \times 0.9676) \approx 0.018$. The values of *E* and $S1_{et}$ are the observed frequencies in the sample, and a reason why $S1_{et} < 1$ is that the winner is sometimes bought by, or merges with, another pharmaceutical company.

To address the remaining two issues, we calculate autocorrelation in winning probabilities related to state dependence and stock quantity by first estimating models including some variables related to these mechanisms that depend on whether the firm has won in previous months. As explained below, we then calculate the contribution of state dependence and variation in stock quantity to autocorrelation in winning probabilities during competition based on the estimation results and the correlation between relevant variables and previous winnings. We cannot simply estimate autocorrelation coefficients for PM_{fet} (i.e., an indicator for firm *f*'s product in exchange group *e* to be a PM month *t*) because these coefficients would also capture autocorrelation caused by collusive behavior. For these estimations we restrict the sample to low-price bidders from exchange groups that have existed for at least six months and the current month having at least two low-price bidders and a single PM. In addition, we include

[25] If exchange groups with $n_{et} = 2$ are also included, U becomes 0.4156. This is likely an overestimation of the probability during competition because it can be affected by attempts to achieve parallel bidding collusion. A bid consists of both a price and a declaration regarding whether the product will be available for the entire month or not. Robustness analyses reported at the end of section 6.2 indicate that the value of U has only minor effects on the estimated probability of collusion.

[26] The probability of a winner continuing to be a low-price bidder varies by n_{et}, as illustrated in Table 6.

only exchange group by month observations where the winner can be another than the previous month, either because of entry, the changed bid of at least one low-price bidder, or due to the exit of the previous month's winner. The latter restriction is made to be able to add together all sources of autocorrelation during competition without risking a double count of any part.

State dependence implies that firms can partly predict month-to-month variation in demand for their products. For example, if a product containing pills for three months was sold in large quantities in January because it was a PM at the time, this increases the demand for the product in April when many consumers return to get a refill. It is therefore profitable for the seller to harvest this increased demand by setting a higher price in April. For this reason, we expect PM_{fet} (i.e., an indicator for firm *f*'s product in exchange group *e* to be a PM month *t*) to be negatively correlated with $PM_{fe,t-3}$ if consumers buy the drug every third month.

The time between drug fills differs across exchange groups due to differences in package sizes and type of drug (e.g., whether the drug is for a chronic or acute condition). Also, within exchange groups the time between fillings differs across consumers. Therefore, to account for the autocorrelation caused by state dependence in combination with repeated purchases, we create variables for time between purchases using the 1.9 million drug fills that are carried out after the first six months of the data set described in Granlund (2021). More specifically, we generate $Harv_{fet} = \sum_{\mathrm{m=t-1}}^{t-6} Share_e^m \, PM_{fem} / nPM_{em}$ where $Share_e^m$ equals the share that made a filling $t - m$ months before their current filling in the exchange group. The variable PM_{fem} is an indicator for that firm *f* in exchange group *e* sold a PM month *m*, and this is divided by the number of products (usually one) that was a PM in the exchange group this month (nPM_{em}). It is the difference between $Harv_{fet}$ and the values for this variable for competing firms that affect the probability of firm *f* selling a PM. Therefore, we define $DiffHarv_{fet} = Harv_{fet} - \overline{Harv_{et}}$, where $\overline{Harv_{et}}$ is the mean of $Harv_{fet}$ for low-price bidders in exchange group *e* month *t*.

A high value of $Harv_{fet}$, and hence $DiffHarv_{fet}$, means that a high share of the consumers making purchases in exchange group *e* month *t* made their most recent purchase in that exchange group when firm *f*'s product was a PM. Given that being a PM has a large effect on a product's market share, most of these likely bought firm *f*'s product and some of them are prepared to pay extra to secure firm *f*'s product again. To harvest this increased demand, firm *f* is expected to set a higher-than-average price in month *t*. Therefore, we expect $DiffHarv_{fet}$ to have a negative effect on the probability of selling a PM.

On average, 69% of consumers made their filling one to six months after their previous filling. Five percent made the fill the same month as the previous fill, and the share of consumers that had not purchased a product from the same exchange group over the last six months was 26%. Most common was that the filling was done three months (22%) or four months (21%) after the previous filling, and only 6% and 3% made their filling five or six months, respectively, after the previous filling.[27]

[27] Six and twelve percent of the fillings were done one and two months, respectively, after the consumers' previous filling within the exchange group. When calculating the number of months since the previous drug fill, we assume that the previous fill was done on the 25th of the month. This is because the 25th is the median day of additional purchases caused by a product being the PM according to the results presented in Table II in Granlund (2021). That it is such a late date is explained by the pharmacies' option of selling the previous month's PM during the first half of a month, for the previous month's price. Because the same prices apply for these 15 days, refills made during these days are considered to be carried out zero months (price periods) after the previous filling.

For 12% of the firm × exchange group × month observations, we lack data on drug fills (e.g., because the exchange group only existed at the beginning or end of the study period, which the filling data do not cover). For these, we assume that the distribution of months between fillings equals the mean for all drug fillings (e.g., we assume that 22% of fillings were done three months after the previous filling etc.). The variable $DiffHarv_{fet}$ has a mean of zero by definition and, as reported in Table D1, ranges from -0.50 to 0.80.

As stated above, stock quantities can also lead to autocorrelation in the probability of a product being a PM. The reason for this is that the drugs have limited durability, which implies that firms holding excessively large quantities in stock will face the risk of having to dispose of drugs or price very aggressively when drugs are approaching their expiration date. At the same time, firms must hold some quantities in stock because the delivery time of drugs is often a few months. Because of this, there should exist some optimal stock quantity. When a firm's stock falls below this level, perhaps because its product has recently been a PM for a greater number of months than expected, we expect it to raise its price, which will in turn reduce its expected sales and the probability that its product becomes a PM. To capture this effect, we create

$$PrevPM_{fet} = \sum_{m=t-6}^{t-1} \delta^{t-m} \left(PM_{fem}/nPM_{em} - 1/n_{em}\right). \tag{5.2}$$

For products being a PM month *m*, the parenthesis is intended to capture higher-than-average sales that month, and it will take higher values for exchange groups with a high number of low-price bidders, as the probability of becoming a PM is lower in these exchange groups. The parameter $\delta \in (0,1)$ is included so that there is room for higher-than-expected sales in the more distant past to matter less. The PM status more than six months in the past is assumed to not matter at all because firms should be able to restore their stock to the optimal level within six months by adjusting orders or their own production. For all values of δ, $PrevPM_{fet}$ has a mean close to zero (not identical because it is based on past values and some firms exit), and, for example, when $\delta = 0.298$ (which we estimate it to be below) $PrevPM_{fet}$ ranges from -0.42 to 0.34.

We estimate the following equation:

$$P\left(PM_{fet} = 1\right) = F\left(\alpha_1 DiffHarv_{fet} + \alpha_2 PrevPM_{fet}(\delta) + \alpha_3 \frac{1}{n_{et}} + \mu_{fe} + \varepsilon_{fet}\right), \tag{5.3}$$

where μ_{fe} is firm × exchange group fixed effects. The model is estimated for observations that are not from exchange groups that three (five) months earlier were part of any bid-rotation or parallel-bidding pattern weakly exceeding three (five) months.[28] As a result, we restrict the sample to observations where the identity of the winner month *t* should not be a function of preexisting collusive behavior. Hence, the parameters are identified, almost exclusively, using observations from competitive regimes; more precisely, they are identified using subsets of observations from competitive regimes because some longer patterns consistent with collusion arise during competition.

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[28] If the sample was based on if bid-rotation occurred more recently, we would get an endogenous sample that depends on the identity of the winner month *t*. For example, if firm *f* neither sold a PM month *t – 1* or *t – 2*, the probability that the exchange group two months ago was part of a bid-rotation of at least three months would be higher if it sold the PM month *t*. (It would be a bid-rotation including *t – 2* to *t* if two different firms sold the only PM month *t – 1* and *t – 2*.) Hence, basing the exclusion criteria on the existence of collusive patterns two months ago would imply that winning sequences consistent with $\alpha_2 < 0$ would be more likely to be excluded, which could result in biased estimators for this and other parameters.

In the larger sample, which includes observations with up to four months of bid-rotations, we also include three dummy variables PM_ll_{fet} that for l = 2, 3, 4 take the value one if $PM_{fe,t-l} = 1$ and $n_{fe,t-l} = 1$. That is, PM_22_{fet} takes the value 1 for firm *f* if it has sold a PM two months ago and there were two low-price bidders in the exchange group that month. These dummy variables are included to control for the dynamics caused by bid-rotations among 2–4 firms which could otherwise bias the estimators.

We assume that the parameters α_1, α_2, and δ are identical across competitive observations excluded from the estimations and those included in the estimations. As the model is nonlinear because of only one parameter, δ, it is convenient to estimate equation (5.3) using a grid-search estimation strategy. We employ this method by setting δ to values ranging from 0 to 1 and estimating the other parameters using xtreg (specifications 5.1 and 5.2) and xtlogit (specifications 5.3–5.4). Finally, likelihood values are used to discriminate between the different values of δ.

Specification 5.1 is the preferred specification because it is estimated on the smaller sample, which reduces the risk of estimates being affected by collusions, and because it is a linear probability model. We consider the latter an advantage because $1/n_{et}$ should have a linear effect on the probability of winning.[29]

The signs of all parameter estimates reported in Table 3 are what we expected to see, meaning they support the hypothesis that a firm sets a higher price, which reduces the probability of their product becoming the PM, the larger the expected demand caused by state dependence is and the more it has recently sold. The effects of $DiffHarv_{fet}$ and $PrevPM_{fet}$ are larger in specification 5.1 than in specification 5.2. Because the sample used to estimate specification 5.2 includes some short possible collusive patterns, this might be explained by that $DiffHarv_{fet}$ and $PrevPM_{fet}$ have no (or smaller) effects during collusions.

The estimates for $PM\_ll_{fe,t-l}$ (l = 2, 3, 4) indicate that the identity of the PM in the larger sample in some cases is affected by bid-rotations among 2–4 firms. However, the results not reported in tables show that none of these parameters are significantly positive for the smaller sample, which is expected given that this sample should not include observations from exchange groups with preexisting collusive behavior.

We use the estimates from specification 1 to calculate values for al_{et} (l = 2, 3, 4, 5). These parameters describe the likelihood of selling a PM in exchange group *e* month *t* for a seller that sold a PM *l* months ago, and have sold no PM in the exchange group between this month and month *t*, relative to the average probability for a low-price bidder in exchange group *e* month *t* of selling a PM. We calculate separate values for each pair of l and $\breve{n}_{et}$, where $\breve{n}_{et} = n_{et}$ for $n_{et} \leq 5$ and $\breve{n}_{et} = 6$ for $n_{et} \geq 6$. More specifically, for each $\breve{n}$ we first replace the values of $DiffHarv_{fet}$ and $PrevPM_{fet}$ with the mean values for all low-price bidders (including those excluded from the estimation samples), and predict the probability of $PM_{fet} = 1$, called $p\_\breve{n}_{fet}$, for all observation. After that, we replace the values of $DiffHarv_{fet}$ and $PrevPM_{fet}$ with their mean values for each $\breve{n}$ and *l*, and predict the probability of

[29] In a model without any other explanatory variables, the effect of $1/n_{et}$ on the probability of winning should equal 1. A linear model also guarantees that $DiffHarv_{fet}$ has a symmetric effect on the probability of winning. For example, for an exchange group with two low-price bidders bidders (*f* = *A*, *B*) it holds by construction that $DiffHarv_{Aet} = -DiffHarv_{Bet}$ and because the average probability of winning in this sample must be $1/n_{et}$, the marginal effect of $Diff$ must be equal for all values of the variable within the exchange group the current month. In exchange groups with no entry and exit, the same holds for $PrevPM_{fet}$.

$PM_{fet} = 1$, called $p\_\breve{n}l_{fet}$. Lastly, for each pair of $\breve{n}$ and l, we divide the mean of $p\_\breve{n}l_{fet}$ by the mean of $p\_\breve{n}_{fet}$ and define al_{et} as the maximum of this quotient and $1/\breve{n}_{et}$. Restricting al_{et} so that it never falls below $1/\breve{n}_{et}$ is done primarily to avoid negative values, which otherwise could follow from a linear probability model, and we set the lower bound to $1/\breve{n}_{et}$ because the correct adjustment factor can be expected to be decreasing in $\breve{n}_{et}$. The restriction is only binding for $a1_{et}$ for $\breve{n}_{et} \geq 4$.

Table 3. Estimation result of the probability of winning during competition

Specification	1	2	3	4	3	4
Estimator	OLS	OLS	Logistic	Logistic	Logistic	Logistic
					Marginal	effects
$DiffHarv_{fet}$ (α_1)	-0.052*	-0.032**	-0.266**	-0.135*	-0.037*	-0.018*
	(0.025)	(0.013)	(0.119)	(0.073)	(0.017)	(0.010)
$PrevPM_{fet}$ (α_2)	-1.156***	-0.720***	-5.329***	-4.214***	-0.740***	-0.565***
	(0.025)	(0.008)	(0.125)	(0.051)	(0.034)	(0.011)
$PrevPM_{fet}$ (δ)	0.298***	0.460***	0.300***	0.434***		
	(0.021)	(0.009)	(0.024)	(0.010)		
$1/n_{et}$ (α_3)	0.881***	0.925***	4.149***	4.791***	0.576***	0.642***
	(0.050)	(0.025)	(0.237)	(0.119)	(0.007)	(0.004)
$PM\_22_{fe,t-l}$		0.150***		0.652***		0.087***
		(0.007)		(0.030)		(0.005)
$PM\_33_{fe,t-l}$		0.068***		0.318***		0.043***
		(0.005)		(0.026)		(0.004)
$PM\_44_{fe,t-l}$		0.033***		0.168***		0.023***
		(0.007)		(0.037)		(0.005)
Within R^2	0.133	0.130				
Log-l	-12,706.950	-49,883.216	-8,498.840	-40,087.184		
Observations	24,395	90,111	21,525	88,537		

Note: See Table D1 for variable definitions and descriptive statistics. Standard errors are reported in parentheses. For the linear probability specifications, the standard errors are robust to correlation within exchange group × firm combinations. *, **, and *** indicate statistical significance at the 5%, 1%, and 0.1% level according to one-sided tests.

Table 4 reports the mean values for $DiffHarv_{fet}$ and $PrevPM_{fet}$ (for δ =0.298) by $\breve{n}_{et}$ for all low-price bidders and for each value of l, while the values for al_{et} are given in Table 5. Table 4 shows that $DiffHarv_{fet}$ has positive values for $l \leq \min(\breve{n}_{et}, 4)$. The positive values for l = 1 might seem surprising, but they are explained by the ones selling a PM one month ago are also overrepresented among those selling a PM 3 and 4 months ago, and, as stated above, most consumers make their refills at 3–4 months intervals. For $PrevPM_{fet}$ we see, as expected, the highest numbers for l = 1. The descriptive statistics presented in Table 4, together with the estimates of Table 3, reveal that the probability of a firm winning every n:th month during a competitive regime will be underestimated if one does not account for the incentives captured by the variable $PrevPM_{fet}$. However, accounting for the harvest motive ultimately proves to be less important because the parameter α_1 is close to zero and because the values of $DiffHarv_{fet}$ are close to zero for $l = \breve{n}_{et}$ (these values are the most important because, in most cases, $\breve{n}_{et}$ equals the number of firms winning every l:th month; see Figures 7 and 8).

Table 4 Mean values for $DiffHarv_{fet}$ and $PrevPM_{fet}$ by l and $\check{n}_{et}$

	$DiffHarv_{fet}$				
All or l\\$\check{n}_{et}$	2	3	4	5	6
All	0.00	0.00	0.00	0.00	0.00
l = 1	0.04	0.05	0.05	0.04	0.07
l = 2	0.09	0.12	0.12	0.13	0.13
l = 3	0.02	0.04	0.06	0.07	0.09
l = 4	-0.06	-0.03	0.01	0.07	0.05
l = 5	-0.15	-0.11	-0.08	-0.08	-0.07
	$PrevPM_{fet}$ for	δ =0.298			
All	-0.01	-0.00	-0.02	-0.02	-0.02
l = 1	0.12	0.16	0.18	0.18	0.20
l = 2	-0.08	-0.04	-0.02	-0.01	-0.00
l = 3	-0.15	-0.11	-0.09	-0.07	-0.07
l = 4	-0.17	-0.13	-0.10	-0.08	-0.10
l = 5	-0.19	-0.15	-0.12	-0.10	-0.09

Note: The values are for the estimation sample of specification 5.1, which comprises a total of 24,395 observations.

Table 5 The relative probability during competition of selling a PM for a seller that sold a PM l months ago and that has sold no PM between this month and month t

al_{et}\\$\check{n}_{et}$	2	3	4	5	6
$a1_{et}$	0.682	0.365	1/4	1/5	1/6
$a2_{et}$	1.242	1.141	1.038	0.944	0.853
$a3_{et}$	1.400	1.383	1.329	1.269	1.203
$a4_{et}$	1.459	1.452	1.411	1.373	1.341
$a5_{et}$	1.500	1.490	1.463	1.436	1.409

Note: These parameter values only vary by $\check{n}_{et}$.

The adjustment parameters al_{et} are multiplied by $(1 - PT_{et})/n_{et}$ to obtain Al_{et}, where Al_{et} is the probability of being a single winner, conditional on selling a low-price product, for a firm that last won l months ago. Because these probabilities are conditional on selling a low-price product, we also define survival parameters. In addition to $S1_n$ defined above, we also for each value of $\check{n}_{et}$ define Sl_{et} for l = 2, 3, 4, 5 as the probability of a firm selling a low-price product in exchange group e month t for those with $PM_{fe,t-l} = 1$ and with no product being PM between $t - l$ and t. These survival probabilities are listed in Table 6. The parameters Al_{et} and Sl_{et} are included in the formulas used to calculate $P(W_{et}|K, \check{n}_{et})$, defined as the probability of the price patterns, W_{et}, conditioned on competition, K, and the truncated number of low-price bidders, $\check{n}_{et}$.

Table 6 Probability of selling a PM again after l months during competition

Sl_{et}\\$\check{n}_{et}$	2	3	4	5	6
$S1_{et}$	0.9390	0.9676	0.9712	0.9754	0.9803
$S2_{et}$	0.8511	0.9300	0.9408	0.9499	0.9571
$S3_{et}$	0.2674	0.6515	0.6891	0.7286	0.7840
$S4_{et}$	0.1302	0.4815	0.6261	0.6890	0.7319
$S5_{et}$	0.0605	0.3716	0.5598	0.6447	0.6716

Note: These parameter values only vary by $\check{n}_{et}$.

6. Calculating probabilities of collusion based on winning patterns

6.1. A method based on Bayes' theorem

Probability theory tells us that the probability of collusion, S, given an observed winning pattern, W_{et}, can be calculated using Bayes' theorem:

$$P(K|W_{et}, \check{n}_{et}) = \frac{P(W_{et}|K, \check{n}_{et})P(K|\check{n}_{et})}{P(W_{et}|\check{n}_{et})} \quad (6.1),$$

where K denotes competition and $P(S|W_{et}, \check{n}_{et}) \equiv 1 - P(K|W_{et}, \check{n}_{et})$. The identity follows from the fact that competition and collusion are mutually exclusive and the only two market regimes. Recall that we only focus on collusions involving the winner(s) in each exchange group by month observation. Therefore, there will either be a collusion involving the winner, or not be a collusion involving the winner, no third possibility exists.[30]

We calculate probabilities conditioned on $\check{n}_{et}$ (recall that $\check{n}_{et} = n_{et}$ for $n_{et} \leq 5$ and $\check{n}_{et} = 6$ for $n_{et} \geq 6$), instead of n_{et}, to prevent the frequencies from being heavily influenced by just a few observations, which otherwise would be the case because few observations have a value of n_{et} above 6. . Note that we calculate the probability that collusion has occurred sometime during the winning pattern, and not the probability that an individual bid is the result of collusion.[31]

We proxy the denominator of equation (6.1), $P(W_{et}|\check{n}_{et})$, with the observed frequencies in the data. We set $P(K|W_{et}, \check{n}_{et}) = 1$ for the 9% of the observations that are not part of any of the possible collusion patterns in Table 2. Then, we note that $P(K|\check{n}_{et})$ is the weighted probability of competition for all winning patterns. That is:

$$P(K|\check{n}_{et}) = \sum_{W=1}^{\overline{W}} P(K|W_{et}, \check{n}_{et})P(W_{et}|\check{n}_{et}) + 1 - \sum_{W=1}^{\overline{W}} P(W_{et}|\check{n}_{et}) \quad (6.2),$$

where $\overline{W}$ is the number of patterns consistent with collusion, so $1 - \sum_{W=1}^{\overline{W}} P(W_{et})$ is the share of observations with $P(K|W_{et}, \check{n}_{et}) = 1$. By substituting $(K|W_{et}, \check{n}_{et})$ using equation (6.1) and rearranging, we get the following:

$$P(K|\check{n}_{et}) = \frac{1 - \sum_{W=1}^{\overline{W}} P(W_{et}|\check{n}_{et})}{1 - \sum_{W=1}^{\overline{W}} P(W_{et}|K, \check{n}_{et})} \quad (6.3).$$

What remains to be determined is $P(W_{et}|K, \check{n}_{et})$. We calculate these probabilities using the results regarding survival probabilities, ties, and autocorrelation in winning patterns described in subsections

[30] It is possible to build on the work of this paper to develop a method to calculate the probabilities for collusions not involving the winner. Due to the large number of possible collusive groups in markets with numerous bidders, this will be relatively complex and likely produce less robust results. Because of this, and because collusions involving the winner are of primary economic concern in markets where the winner secures most of the sales, we restrict the focus of this paper to collusion involving the winner and leave generalizations of the method to future research.

[31] For patterns lasting 11 months or longer, the probability is that it was collusion sometime during the 11 months closest to month t of the winning pattern.

5.2 and 5.3. However, as an example, let us start by explaining how $P(W_{et}|K, \check{n}_{et})$ is calculated for $W_{et} = b2m11$ (i.e., two firms winning every other month for at least 11 months) when $\check{n}_{et}$= 2 for all relevant months and under the hypothetical assumption that the probability of being a single winner is constant at 0.5 for each firm. In this hypothetical case, $P(b2m11|K,2) = 1 - (1 - 2 \times 0.5^{11})^{11} \approx 0.01$. The first exponent is explained by the fact that the bid-rotation must last 11 months, and the multiplication by two is because a pattern can start with either one of the two firms winning. Note that $2 \times 0.5^{11} = 1/1024$ is the probability of observing an 11-month bid-rotation in a given 11-month window. As we use moving windows so that $W_{et} = b2m11$ regardless of whether the pattern is observed from $t - 10$ through t, or from $t - 9$ through $t + 1$, and so on up to t through $t + 10$, $P(b2m11|K,2)$ equals one minus the probability that exactly zero 11-month bid-rotations have occurred during the eleven possible time periods, which explains the rest of the formula.

When applying the method to the data, we must account for the fact that n_{et} varies over time. We do this by defining $P(W_{et}|K, \check{n}_{et})$ as the mean value of $P(W_{et}|K, \boldsymbol{n_{et}})$ for each value of $\check{n}_{et}$, where, $\boldsymbol{n_{et}}$ denotes a vector of n_{et}-values affecting the probability of observing the pattern during competition. For example, for $b2m11$, $\boldsymbol{n_{et}}$ includes the parameters $n_{e,t-10}$ through $n_{e,t+10}$. Because we need to know these values and collusion among low-price bidders cannot occur when $n_{et} = 1$, we restrict the calculations to observations that are both preceded and followed by ten months in the data with at least two low-price bidders in the exchange group.

Also, we must account for autocorrelation in winning probabilities during competition and, for the patterns with an upper limit of the duration of the pattern, we must subtract the probability that a winning sequence exceeds this limit. All this complicates the calculation of $P(W_{et}|K, \boldsymbol{n}_{et})$. Equation (6.4) shows the formula for calculating $P(b2m11|K, \boldsymbol{n}_{et})$ when accounting for this:

$$P(b2m11_{et}|K, \boldsymbol{n_{et}}) = 1 - \prod_{T=t}^{t+10} (1 - Pb2m11_{eT}) \tag{6.4}$$,

where

$$Pb2m11_{eT} = PS1_{e,T-10} PS2_{e,T-9} \prod_{m=T-8}^{T} C2_{em} \tag{6.5}$$.

For $b2m11$, the variable T denotes the 11th or later month with two alternating winners during at least 11 months, while $Pb2m11_{eT}$ denotes the probability during competition of observing two alternating winners from month $T - 10$, or earlier, to month T, or later. A difference between this and the case with constant $n_{et} = 2$ is that the probabilities vary across months and, therefore, we must multiply different probabilities instead of raising one probability to a power.

We define $PS1_{et} = (1 - PT_{et})$, i.e., the probability that any firm is the single winner in month t is equal to one minus the probability of a tie. For the first observation in a winning pattern (i.e., in $T - 10$ in eq. (6.4)), the winning probabilities are not conditioned on any winning pattern other months. However, the winner in the second month in a bid-rotation must be a different winner to the winner in the first month. Therefore, we define $PS2_{et} = (1 - PT_{et} - C1_{et})$ as the probability that a second firm (i.e., a firm other than the winner in $t - 1$) is the single winner, where $C1_{et} = S1_{et}[U^{n_{et}}E^{n_{et}} + (1 - U^{n_{et}}E^{n_{et}})A1_{et}]$ is the probability that the winner in the previous month is also the winner in this month.[32] The equation reveals that $C1_{et}$ depends on the probability that the winner in the previous month markets a low-price product this month, $S1_{et}$, the probability that all low-price bidders submit the same bid as they did in the previous month ($U^{n_{et}}$) and were also low-price bidders in the exchange group in the previous month ($E^{n_{et}}$) and the probability that someone has entered or changed their bid, $(1 - U^{n_{et}}E^{n_{et}})$, so that there could be a new winner, and the probability, $A1_{et}$, that the winner in the previous month is the single winner this month conditional on it being a low-price bidder and there being a possibility for another winner to emerge. Recall from section 5.3 that $A1_{et} = a1_{et}(1 - PT_{et})/n_{et}$ where $a1_{et}$ corrects for autocorrelation caused by state dependence and stock quantity. We also define $C2_{et} = (1 - S1_{et}U^{n_{et}}E^{n_{et}})S2_{et}A2_{et}$ as the probability that a firm that sold a PM two months earlier, but not one month earlier, is a single winner in month *t*.

As shown in Figures 7 and 8, it is possible for the number of possible collusive participants to differ from the current number of potential low-price bidders, n_{et}. A consequence of this is that the observed frequencies for some combinations of winning patterns and $\check{n}_{et}$ are very low and therefore heavily influenced by just a few observations. To prevent this from causing imprecise estimates of $P(K|W_{et}, \check{n}_{et})$ when applying Bayes' theorem (equation 6.1), we group bid-rotation patterns of the same duration (e.g., ≥ 11 months) together irrespective of the number of firms involved, and we do the same for patterns of parallel bidding. For example, we group $b2m11$, $b3m11$, $b4m11$, and $b5m11$ together in the winning pattern b_m11, and calculate the following:

$$P(b\_m11_{et}|K, \boldsymbol{n}_{et}) = 1 - \prod_{T=t}^{t+10}\left(1 - \sum_{F=2}^{5} PbFm11_{eT}\right) \quad (6.6).$$

The probabilities $PbFm11_{eT}$ for the number of firms participating in the possible collusion, F = 3, 4, and 5, are defined in equations (B.25)–(B.27) in Appendix B. A description of how $P(W_{et}|K, \check{n}_{et})$ is calculated for the other winning patterns can also be found in Appendix B. As detailed in the following section, we also group the winning patterns of different durations together when $P(W_{et}|\, \check{n}_{et})$ would otherwise take the value zero. In addition, we impose the restriction $P(W_{et}|K, \check{n}_{et}) \leq \frac{P(W_{et}|\check{n}_{et})}{P(K|\check{n}_{et})}$, which ensures that $P(K|W_{et}, \check{n}_{et})$ never exceeds one.[33]

6.2. Predicted probabilities

When interpreting the results presented below, note that these are only for observations that are both preceded and followed by at least ten months with at least two low-price bidders within the exchange group. Hence, the results might not be representative for new exchange groups. Also, recall that *$P(S|W_{et}, \check{n}_{et})$* is the probability that the winning pattern is at least partly caused by collusion, not the probability that it was collusion in each individual auction.

[32] Note that absent entry and exit, $S1_{et} = E = 1$, and if all randomize a new price each month, $U = 0$, and there is no autocorrelation in winning probabilities, meaning that $A1_{et} = (1 - PT_{et})/n_{et}$, and $PS2_{et}$ would simply be $(1 - PT_{et})(1 - 1/n_{et})$.

[33] This restriction follows from $P(W_{et}|K, \check{n}_{et})P(K|\check{n}_{et}) = P(W_{et} \cap K|\check{n}_{et}) \leq P(W_{et} \cap K|\check{n}_{et}) + P(W_{et} \cap S|\check{n}_{et}) = P(W_{et}|\check{n}_{et})$. When binding, this restriction implies that an iterative method must be used to solve for $P(K|W_{et}, \check{n}_{et})$ and $P(K|\check{n}_{et})$ using equations (6.1) and (6.3).

The results for $\check{n}_{et} = 2$, reported in Table 7, indicate that the majority of bid-rotation patterns lasting 3–6 months arose in competitive regimes. However, 92% (99.7%) of bid-rotation patterns lasting 9–10 (11≤) months arose, at least partly, during collusive regimes. Table 7 shows that $P(W|K,\check{n}_{et})$ takes values close to zero also for short patterns of parallel winning, and, for all durations, parallel winning patterns are far more frequent than would be expected under competition, resulting in high values of $P(S|W,\check{n}_{et})$. Note also that $P(S|W,\check{n}_{et})$ is always strictly less than 100%, and that $P(W|K,\check{n}_{et})$ is always strictly positive.

Table 7 Probability of collusion conditioned on winning patterns for $n_{et} = 2$, in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\check{n}_{et})$	0	28.19	66.08	92.25	99.67	
	S.e.	-	2.89	1.92	0.44	9.09×10^{-3}	
	$P(W_{et}\|K,\check{n}_{et})$	42.17	15.26	3.84	0.92	0.27	62.45
	% of obs.	18.73	9.44	5.02	5.25	36.27	74.70
Parallel bidding	$P(S\|W_{et},\check{n}_{et})$	62.93	99.34	99.99	100	100	
	S.e.	7.93	0.15	2.41×10^{-3}	1.94×10^{-5}	1.74×10^{-9}	
	$P(W_{et}\|K,\check{n}_{et})$	0.27	4.06×10^{-3}	5.00×10^{-5}	5.62×10^{-7}	6.08×10^{-9}	0.27
	% of obs.	0.32	0.27	0.23	0.29	7.63	8.74

Note: $P(S|\check{n}_{et} = 2)$ = 55.59% and the number of observations is 6,910. S.e. denotes analytic standard errors for $P(S|W_{et},\check{n}_{et})$ reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. For bid-rotations lasting 3–4 months, instead of reporting a standard error, we note that $P(S|W_{et},\check{n}_{et})$ would remain at zero unless the share of observations in this category would increase from 18.73% to 20.82% as $P(W_{et}|K,\check{n}_{et})$= 46.89% if the restriction $P(W_{et}|K,\check{n}_{et}) \leq \frac{P(W_{et}|\check{n}_{et})}{P(K|\check{n}_{et})}$ is not imposed. All probabilities are strictly less than 100%.

By comparing $P(W|K,\check{n}_{et})$ to the observed frequencies (% of obs.), we see that in total for n_{et} = 2, bid-rotation patterns are more common than predicted during competition, so observing such a pattern is an indication of collusion, $P(S|W,)$, but it is only a weak indication. However, observing that the pattern stopped within six months is instead an indication of competition. A possible explanation for this is that it is easy to continue a bid-rotation collusion when it has started, so if a bid-rotation pattern is caused by collusion, we would expect it to last for a long time, whereas if it lasts for only a few months, it is evidence for that the pattern arose by chance during competition.

Table 7 also reports standard errors for $P(S|W_{et},\check{n}_{et})$, reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. The large standard error for parallel bidding patterns lasting 3–4 months is caused by the fact that the square of the derivative $dP(S|W_{et},\check{n}_{et})/d\,P(W_{et}|\check{n}_{et})$, by which the variance for $P(W_{et}|\check{n}_{et})$ should be multiplied when calculating the variance $P(S|W_{et},\check{n}_{et})$, takes a high value.[34]

[34] The standard errors are the square root of $d_a^2 Var[P(W_{et}|\check{n}_{et})] + d_b^2 Var[P(K|\check{n}_{et})] + 2d_a d_b \times Cov[P(W_{et}|\check{n}_{et}), P(K|\check{n}_{et})]$, where d_a and d_b are the partial derivative of $P(S|W_{et},\check{n}_{et})$, defined as one minus eq. 6.1, with respect to $P(W_{et}|\check{n}_{et})$ and $P(K|\check{n}_{et})$. The covariances and $Var[P(K|\check{n}_{et})]$ are calculated using the relationship described in equation (6.2) and by assuming that a change in the number of observations belonging to a possible collusive pattern is balanced by a change in the number of observations that do not belong to any possible collusive pattern. That is, we do not account for the fact that a reduction in the frequency of one pattern can increase the frequency of another. This leads to an overestimation of the covariances and $Var[P(K|\check{n}_{et})]$ but has only negligible effects on the standard errors because these are almost entirely determined by $d_a^2 Var[P(W_{et}|\check{n}_{et})]$.

Table 8 shows that the probability that either two firms win every other month or three firms win every third month during competition when n_{et} = 3 ($P(W|K,\breve{n}_{et})$) is as high as 62%. The observed frequency is less than this (49%), resulting in that the conditional probability of bid-collusion for such short patterns, $P(S|b\_m34,\breve{3}_{et})$, is only 46%.

Figure 9 and Tables 8–11 show that $P(W|S,\breve{n}_{et})$ already exceeds 90% for bid-rotation patterns lasting 7–8 months when n_{et} equals 3 or 4, and for patterns lasting 5–6 months when $n_{et} \geq 5$, compared to 9–10 months for n_{et} = 2. Figure 9 also shows that for both bid-rotation and parallel-bidding and for all values of $\breve{n}_{et}$, the probability of collusion is increasing in the duration of the pattern.

Together the estimates of $P(S|W_{et},\breve{n}_{et})$ reported in Tables 7–11 and the observed number of observations in each pattern suggest that 2% of the 28,863 auctions (exchange group by month observations) studied are part of parallel bidding patterns which, at least partly, are caused by collusion, while 62% of the auctions are part of bid-rotation patterns which, at least partly, are caused by collusion. Put differently, 97% of the observations in collusive patterns are part of bid-rotation patterns. Of the 64% of the auctions expected to be part of collusive patterns, 56% have a value of $P(S|W_{et},\breve{n}_{et})$ below 0.90 while 70% have a value of $P(S|W_{et},\breve{n}_{et})$ below 95%. This implies that 19% (= 64% × [1 – 0.70]) of the auctions are part of patterns that significantly deviate from competitive price pattern. Therefore, even though the expected number of auctions to be part of collusive patterns are high, the share of actions that are part of patterns that significantly deviate from competitive behavior fall show of the estimate on 37% obtained by Kawaai and Nakabayashi (2022) for construction contracts in Japan.

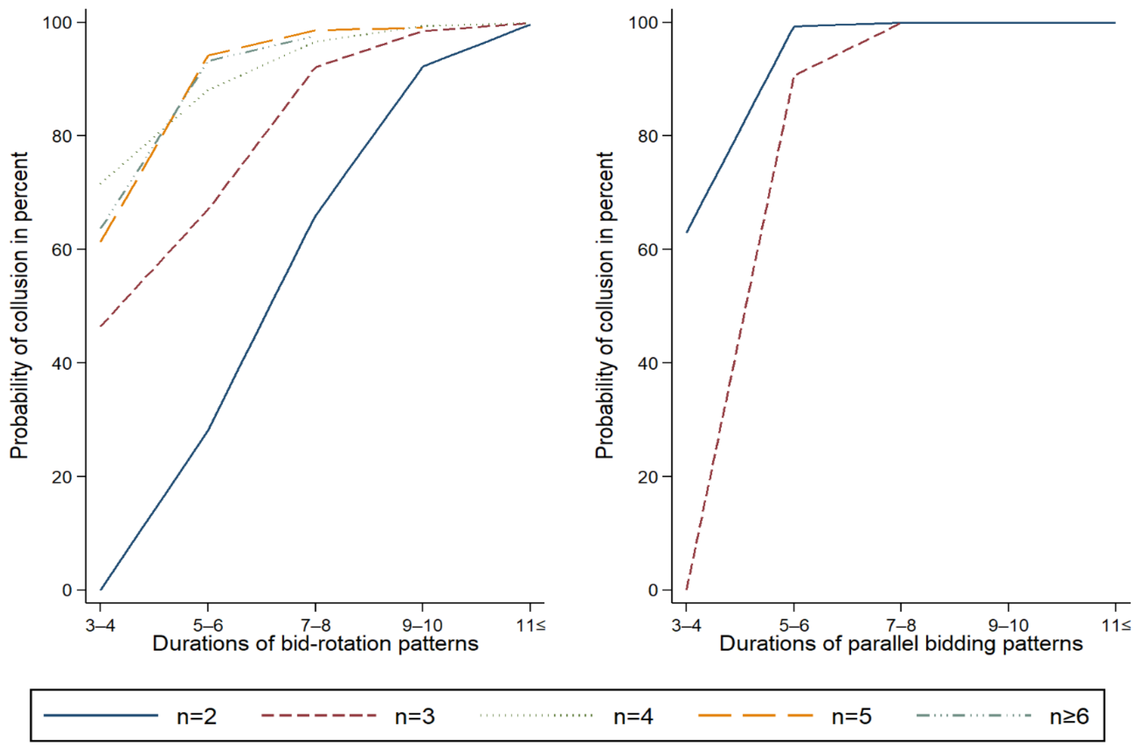

Figure 9. *Probability of collusion for bid-rotation (left panel) and parallel bidding patterns (right panel) of different lengths and for different number of low-price bidders, n.*

Specifically, $Var[P(W_{et}|\breve{n}_{et})]$ equals $P(W_{et}|\breve{n}_{et})[1 - P(W_{et}|\breve{n}_{et})]/(6{,}910 - 1)$. Note that the standard errors do not reflect uncertainty in the various assumptions made when calculating $P(W_{et}|K,\breve{n}_{et})$.

Table 8. Probability of collusion conditioned on winning patterns for n_{et} = 3, in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\check{n}_{et})$	46.45	67.08	92.18	98.46	99.92	
	S.e.	0.77	0.77	0.29	0.08	2.70×10^{-3}	
	$P(W_{et}\|K,\check{n}_{et})$	62.36	16.47	1.49	0.14	1.85×10^{-2}	80.44
	% of obs.	49.16	21.12	7.82	3.83	9.79	91.71
Parallel bidding	$P(S\|W_{et},\check{n}_{et})$	0	90.66	99.91	-	-	
	S.e.	-	6.60	6.47×10^{-2}			
	$P(W_{et}\|K,\check{n}_{et})$	0.12	3.67×10^{-3}	3.60×10^{-5}	-	-	0.12
	% of obs.	0.05	0.02	0.02	-	-	0.08

Note: $P(S|\check{n}_{et}=3)$ = 57.78% and the number of observations is 12,038. S.e. denotes analytic standard errors for $P(S|W_{et},\check{n}_{et})$, reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. For parallel bidding lasting 3–4 months, instead of reporting a standard error, we note that $P(S|W_{et},\check{n}_{et})$ would remain at zero unless the share of observations in this category increases to 0.14% as $P(W_{et}|K,\check{n}_{et})$= 0.32% if the restriction $P(W_{et}|K,\check{n}_{et}) \leq \frac{P(W_{et}|\check{n}_{et})}{P(K|\check{n}_{et})}$ is not imposed. Parallel winning patterns exceeding seven months are grouped together.

Table 9. Probability of collusion conditioned on winning patterns for n_{et} = 4, in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\check{n}_{et})$	71.61	88.08	96.62	99.40	99.90	
	S.e.	0.85	0.40	0.19	5.53×10^{-2}	1.46×10^{-2}	
	$P(W_{et}\|K,\check{n}_{et})$	57.48	19.41	0.95	4.67×10^{-2}	3.18×10^{-3}	77.90
	% of obs.	47.48	38.22	6.57	1.83	0.72	94.82
Parallel bidding	$P(S\|W_{et},\check{n}_{et})$	0	-	-	-	-	
	S.e.	-					
	$P(W_{et}\|K,\check{n}_{et})$	5.65×10^{-2}	-	-	-	-	5.65×10^{-2}
	% of obs.	1.33×10^{-2}	-	-	-	-	1.33×10^{-2}

Note: $P(S|\check{n}_{et}=4)$ = 76.55% and the number of observations is 7,544. S.e. denotes analytic standard errors for $P(S|W_{et},\check{n}_{et})$, reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. For parallel bidding lasting 3–4 months, instead of reporting a standard error, we note that $P(S|W_{et},\check{n}_{et})$ would remain at zero unless the share of observations in this category increases to 0.10%—that is, from 1 to 8 observations—as $P(W_{et}|K,\check{n}_{et})$= 0.42% if the restriction $P(W_{et}|K,\check{n}_{et}) \leq \frac{P(W_{et}|\check{n}_{et})}{P(K|\check{n}_{et})}$ is not imposed. Parallel winning patterns exceeding three months are grouped together.

Table 10. Probability of collusion conditioned on winning patterns for n_{et} = 5, in percent

Winning pattern W	Variable	3–4	5–6	7–8	9≤	11≤	sum
Bid-rotation	$P(S\|W_{et},\check{n}_{et})$	61.31	94.24	98.63	99.12	-	
	S.e.	3.23	0.40	0.15	0.28		
	$P(W_{et}\|K,\check{n}_{et})$	50.79	20.95	0.77	2.77×10^{-2}	-	72.54
	% of obs.	22.54	62.48	9.72	0.54	-	95.28

Note: $P(S|\check{n}_{et}=5)$ = 82.83% and the number of observations is 2,036. S.e. denotes analytic standard errors for $P(S|W_{et},\check{n}_{et})$, reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. Bid-rotation patterns exceeding nine months are grouped together. There is no observation of parallel winning patterns exceeding three when n_{et} = 5.

Table 11. Probability of collusion conditioned on winning patterns for $\breve{n}_{et}$= 6, in percent

Winning pattern W	Variable	3–4	5–6	7≤	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\breve{n}_{et})$	63.73	93.25	97.63	-	-	
	S.e.	6.68	1.02	0.70			
	$P(W_{et}\|K,\breve{n}_{et})$	44.79	20.73	0.66	-	-	66.18
	% of obs.	25.07	62.39	5.67	-	-	93.13

Note: $P(S|\breve{n}_{et}= 6)$ = 79.70% and the number of observations is 335. S.e. denotes analytic standard errors for $P(S|W_{et},\breve{n}_{et})$, reflecting the uncertainty caused by the sampling variability in the share of observations belonging to each pattern. Bid-rotation patterns exceeding nine months are grouped together. We observe no parallel winning patterns exceeding three when $n_{et} > 5$.

Note that the method presented in this section does not require productions cost to be estimated and neither rest on assumptions of rationality being common knowledge nor that firms never make mistakes. We consider this as advantages as imposing assumption that are not accurate can give biased estimates of the prevalence of collusion. However, the estimated probabilities depend on the parameter values derived in section 5 and we therefore investigate how sensitive the results are to these values. First, we make naive calculations where the effects of state dependence and stock balance on the probability of selling the cheapest product are ignored—that is, setting $al_{et} = 1$ for l = 1–5. These naïve calculations produce considerably higher values for $P(S|W_{et},\breve{n}_{et})$, especially for short bid-rotation patterns. For example, for n_{et} = 2 and bid-rotation patterns lasting 3–4 and 5–6 months, $P(S|W_{et},\breve{n}_{et})$ increases by 37 and 48 percentage points, respectively, and the mean value for $P(S|W_{et},\breve{n}_{et})$ over all 28,863 observations increases from 64% to 80%. This demonstrates that it is important to account for the autocorrelation in the probability of selling the cheapest product that arises under competition.

If, in addition to setting $al_{et} = 1$ for l = 1– 5, we also ignore the propensity to leave prices unchanged by setting U = 0, we obtain a similar mean value for $P(S|W_{et},\breve{n}_{et})$ of 79%. Furthermore, by disregarding changes in the identity of the low-price bidders by setting E = 1, and $Sl_{et} = 1$ for l = 1– 5, we again obtain a mean value of $P(S|W_{et},\breve{n}_{et})$ of 80%, even though individual values of $P(S|W_{et},\breve{n}_{et})$ are affected. That is, when $al_{et} = 1$, the values of U, E Sl_{et} seem to only have minor effects on the mean value of $P(S|W_{et},\breve{n}_{et})$. If we instead set al_{et} to the preferred values, setting U = 0, E = 1, and $Sl_{et} = 1$, for l = 1–5, reduces the mean value of $P(S|W_{et},\breve{n}_{et})$ from 64%, in the main analyses, to 61%, which indicates that there is an interaction between the effect of the different parameters. In Appendix F, we also show that we obtain similar results, with the same mean value for the probability of collusion, when using active, instead of potential, low-price bidders in the calculations.

7. A higher probability of collusion leads to higher and less variable prices

7.1. The price effects of collusion

We start by estimating a simple model to demonstrate that prices are positively associated with the probability of collusion, $P(S|W_{et},\breve{n}_{et})$. Then, we add control variables and interaction variables to obtain estimates of the causal effect of collusion. In all estimations, we use the average price per unit (e.g., pill or gram), weighted by the number of units sold, within an exchange group and month, P_{et}, as the dependent variable.

In the first specification, we only control for fixed effects for each active ingredient and year × month fixed effects. Because the estimation sample includes only exchange groups with generics alternatives that have had at least two low-price bidders for at least ten months, this simple regression can provide us with some idea of the price effect of collusion. We do, however, identify four potentially important drawbacks with this model, which we then address in turn. The first is that prices are correlated with the number of bidders, both because of the effect of prices on entry and exit and the causal price effects of the number of bidders, and that, at the same time, the number of bidders is correlated with the probability of collusion. We address this by controlling for the number of active bidders ($lnnbida_{et}$) and the number of active bidders in the low-price segment ($lnna_{et}$).[35] We also control for the natural logarithm of the number of available different active ingredients within the same therapeutic group, $lnThAlt_{at}$, where the subindex a indicates that the variables each month are identical for all exchange groups with products containing the same active ingredient. More therapeutic alternatives can increase price competition across drugs with different active ingredients because recommendations to physicians regarding which drug to consider first for different patient groups are partly determined by relative prices.[36] Moreover, given Granlund and Bergman's (2018) finding that prices fall faster the shorter the time from patent expiration is, we control for $lnMonths\_PatG_{et}$, defined as the natural logarithm of the number of months since patent expiration or, when this data is missing, from generic entry in Sweden.

The second concern is that prices vary across exchange groups due to the differences in administrative forms, strengths, and package sizes. We address this in two different ways. The first is by including indicator variables for administrative form ($form_e$), the natural logarithms of the strength ($lnStrength_e$), and the package size ($lnSize_{et}$), as defined in Appendix D. The second is by replacing the fixed effects for active ingredients with exchange group fixed effects, exploiting the fact that we have variation over time in $P(S|W_{et}, \breve{n}_{et})$ for exchange groups accounting for 97% of the exchange group by month observations, and that the mean spread in $P(S|W_{et}, \breve{n}_{et})$ within these exchange groups is as high as 0.88. With fixed effects for both exchange groups and year × month-combinations, we thus study differences in price changes across exchange groups with different changes in the probability of collusion.

The third concern is that prices do not adjust immediately to new competitive environments. One reason for this is that firms' expectations regarding competitors' cost, or more generally their expectations regarding the distribution of their future bids, can depend on past bids. Another reason is that the dynamic price cap gives firms selling the most expensive alternative within an exchange group an incentive to gradually adjust prices to new market environments,[37] and the price of the most expensive

[35] The correlation between the number of potential bidders ($nbid_{et}$) and the number of active bidders ($nbida_{et}$) is high (0.99), and also the correlation between the number of active bidders (na_{et}) and the number of potential low-price bidders (n_{et}) is high (0.98). We control for active bidders because this should affect the expected minimum price even if all firms randomize prices from the same distribution, and because—among the instruments that according to logic and tests should be valid—we find stronger instruments for $lnnbida_{et}$ and $lnna_{et}$. For the OLS specifications, we obtain similar results when using potential bidders. For the IV specification 7.4, we have calculated that the bias caused by the difference between the measures of potential and active bidders on average being larger for observations with $P(S|Wet, \breve{n}_{et}) \geq 0.5$ would underestimate the short-run effect of collusion with no more than 0.001 or a twentieth of the standard error for $P(S|W_{et}, \breve{n}_{et})$, given the estimates for $lnnbida_{et}$ and $lnna_{et}$.

[36] In Sweden, 21 county councils are responsible for providing health care and, together with the central government, they finance the pharmaceutical benefit scheme. Each county has at least one drug- and therapeutic committee that issue recommendations to prescribers to promote the safe and cost-effective use of pharmaceuticals.

[37] If a seller of the most expensive alternative is uncertain about how much its optimal price is reduced when, for example, a new firm enters the exchange group, it is because of the price cap better off starting with a small price

alternative can in turn affect the prices of cheaper alternatives. For the specifications with fixed effects for active ingredients, we address this autocorrelation simply by allowing the error term to be correlated across observations with the same active ingredient. We do not include lags of the dependent variable in these specifications, as the estimators for these in specifications without exchange group fixed effects would be biased by unexplained time-invariant variation across exchange groups. In specifications with exchange group fixed effects, the parameters of interest are only identified using variation over time, and it is therefore important to model price dynamics to prevent the parameters from describing something in between the short- and long-term effects of the variables.[38] Therefore, we use the Akaike information criterion (AIC) to determine how many lags of the dependent variable should be included. The AIC is minimized by including four lags for both OLS and IV specifications. That more than one lag should be included is expected since the value of P_{et} is influenced by the randomization of prices during competition and the temporary variations in how market shares relate to the relative prices.[39]

The reason we also estimate IV specifications is that the variables $lnnbida_{et}$ and $lnna_{et}$ can be endogenous because high prices can increase entry and reduce exit. However, it is also possible that $lnnbida_{et}$ and $lnna_{et}$ are exogenous because the decision to be an active bidder month *t*—that is, having a price for this month—must be taken at the latest in month *t – 2* when prices must be submitted. If there is no autocorrelation in the error term, firms are unlikely to be able to predict the value of ε_{et} when they make the entry and exit decisions that affect the values of $lnnbida_{et}$ and $lnna_{et}$. Therefore, we test for autocorrelation up to four months in the true regression error using the test proposed by Cumby and Huizinga (1992), which allows for some regressors (e.g., lags of the dependent variable) to be only weakly exogenous. The test, implemented in STATA by Baum and Schaffer (2013), rejected the null hypothesis of no autocorrelation of order two at the 5% level for the first dynamic OLS specification. Therefore, we also estimated a model accounting for first- and second-order serial correlation using generalized least squares, which indicates that the estimated correlation between ε_{et}

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reduction and then complementing it with an additional price cut if—when it learned more about the behavior of the entrant or consumer's preferences for the product of the entrant—found the first price cut to be small. If it instead started with a price cut that in retrospect is found to be too large, it cannot be reversed because of the dynamic price cap, which is described in greater detail in section 3. Because of this, dynamic specifications have been used before when analyzing determinants of prices in this market (Bergman et al., 2017; Granlund and Bergman, 2018).

[38] In an extreme case with an explanatory indicator variable that takes the values 0 and 1 every other month, the estimator would only capture the short-term effect in a static model with exchange group fixed effects. In general, the more persistent the explanatory variable is, the closer will the estimate from such a model be to the long-run effect.

[39] Simultaneously including lagged dependent variables and fixed effects can cause Nickell bias, but as we have many time periods per exchange group, this bias is expected to be small. According to Nickell (1981), the limit of the bias for the parameter θ as N̰ approaches infinity can be approximated by –(1+ θ)/(T-1), where N̰ and T are the number of fixed effects and time periods, respectively. With T = 32.4 on average, and the estimates for the lagged dependent variable, we expect biases of approximately -0.03 to -0.04. Nickell notes that for low values of θ, his more exact formula for the bias, and hence also this approximation, corresponds well to the Monte Carlo results of Nerlove (1967). On the other hand, for large values of θ, and a large share of the total variance being caused by fixed effects, Nerlove found considerably lower biases. Because the sum of the coefficients for the lags of the dependent variable equals around 0.7, and because the fraction of variance caused by the fixed effect is around 0.7, Nerlove's result indicates that the bias should be smaller than the above-mentioned figures of -0.03 to -0.04. The estimators could also be biased by autocorrelation, but Keele and Kelly (2006) report biases of less than 1% (3%) for both the short- and long-term effect when using OLS with a lagged dependent variable—with a true coefficient of 0.75—when the correlation coefficient is 0.1 (0.2). When the true effect of the lagged dependent variable is smaller, the bias is also smaller.

and $\varepsilon_{e,t-1}$ is -0.17, while it is -0.12 for ε_{et} and $\varepsilon_{e,t-2}$. The low absolute value for the correlation between ε_{et} and $\varepsilon_{e,t-2}$ suggests that the endogeneity problem is largely resolved by the fact that the entry and exit decision affecting $lnnbida_{et}$ and $lnna_{et}$ must be made in month $t-2$ instead of in month t. Nevertheless, we also estimate IV-specifications using $lnnbida_{e,t-1}$, $lnn_{e,t-1}$, and $lnQ_{e,t-3}$ as instruments, and in Appendix C we discuss the relevance and validity of these instruments. Using predicted values of $lnnbida_{et}$ and $lnna_{et}$ to resemble the IV estimation, we cannot at the 5% level reject any of the null hypotheses of no autocorrelation of order one to four.

Lastly, we include interaction terms between $P(S|W_{et},\breve{n}_{et})$ and $lnnbida_{et}$ and $lnna_{et}$ to allow the effect of collusion to differ depending on the number of bidders and to allow prices to fall in the number of bidders at different speeds in competitive and collusive regimes. The equation with interactions is written as follows:

$$lnP_{et} = \sum_{l=1}^{4} \theta_l lnP_{e,t-l} + \beta_1 P(S|W_{et},\breve{n}_{et}) + \beta_2 P(S|W_{et},\breve{n}_{et}) lnnbida_{et} \quad (7.1),$$
$$+\beta_3 P(S|W_{et},\breve{n}_{et}) lnna_{et} + \beta_4 lnnbida_{et} + \beta_5 lnna_{et} + \beta_6 lnThAlt_{at}$$
$$+ \beta_7 lnMonths\_PatG_{at} + \eta_t + \mu_e + \varepsilon_{et}$$

where η_t and μ_e are year × month and exchange group fixed effects (that is, separate intercepts for each exchange group) and ε_{it} is the error term, which is allowed to be correlated among observations for products with the same active ingredient.[40] We estimate equation (7.1) with OLS (spec. 7.5) and with IV (spec. 7.6), where $lnnbida_{et}$ and $lnna_{et}$ and their two interactions are treated as endogenous and $P(S|W_{et},\breve{n}_{et}) lnnbida_{e,t-1}$ and $P(S|W_{et},\breve{n}_{et}) lnna_{e,t-1}$ are used as additional instruments. Note, however, that the interaction variables $P(S|W_{et},\breve{n}_{et}) lnnbida_{et}$ and $P(S|W_{et},\breve{n}_{et}) lnna_{et}$ must not be endogenous even if $lnnbida_{et}$ and $lnna_{et}$ are endogenous (Bun and Harrison, 2019).

Irrespective of instrumentation, note that the estimator of the effect of $P(S|W_{et},\breve{n}_{et})$ might not be an unbiased estimator of the causal price effect of collusion because collusion is a binary variable, whereas $P(S|W_{et},\breve{n}_{et})$ is a continuous variable. In other words, $P(S|W_{et},\breve{n}_{et})$ can be seen as a variable with a measurement error of the binary variable of interest. This problem exists in all studies that have focused on the effect of collusion on prices because researchers have never established with certainty which observations are affected by collusion. In studies of convicted cartels, researchers know with great certainty that some firms have been involved in collusion, but do not know if some non-convicted firms have also engaged in said collusion. We argue that this measurement problem is smaller in our case than in previous research because we do not group all observations with a probability of collusion below a certain level (e.g., 90%) together and implicitly assume that none of these are the result of collusion.

The derivative $dlnP_e^*/dP(S|W_e^*,\breve{n}_e^*)$ reported in Table 12 shows the long-term effect of collusion on logarithmic prices, which for the specifications with interactions (7.5 and 7.6) are calculated at within sample means for $lnnbida_{et}$, and $lnna_{et}$. Using the formula $100 * [\exp\big(dlnP_e^*/dP(S|W_e^*,\breve{n}_e^*)\big) - 1]$, this derivative is translated into percentage effects on prices of going from competition to collusion, $dP_e^*/dP(S|W_e^*,\breve{n}_e^*)$ *in percent*. Calculations that are not reported in the tables, meanwhile, show that,

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[40] Exchange group fixed effects control for time-invariant differences in demand across products. Including lnQ_{et} as an additional endogenous regressor produces almost identical results and indicates that prices increase by approximately 0.03% in the short term (s.e. 0.03%) when the quantity demanded increases by 1%, but this estimate is not significantly different from zero.

on average, 67–68% of the long-term effect is realized within 3 months and 87% within 6 months for specifications 7.5–7.6.

According to the static specifications 7.1 and 7.2, collusion increases prices by 30–31% (95% CI 17%–43% and 19%–42%), but as these specifications do not differentiate between short- and long-run effects, this is likely an overestimation of the short-run effect and an underestimation of the long-run effect. Interestingly, adding control variables has negligible effects on the estimated effect. More important than this is to add exchange group fixed effects and lags of the dependent variable, as we do in specifications 7.3–7.6. According to these specifications, collusions increase prices by 49–65% in the long run. The point estimates for the collusion effects are approximately one standard error larger in the instrumental variable regressions (except for $P(S|W_{et}, \check{n}_{et})$ in the specification with interactions). This may just be a coincidence, but may also be explained by the fact that the estimates for $lnnbida_{et}$ and $lnna_{et}$ are slightly more negative in the IV-regression (which is consistent with these variables being endogenous) and by the fact that $lnna_{et}$ is positively correlated with $P(S|W_{et}, \check{n}_{et})$.

The average collusion-effect also becomes slightly larger when interactions between $P(S|W_{et}, \check{n}_{et})$ and $lnnbida_{et}$ and $lnna_{et}$ are added, and the results show that the collusion-effect is increasing in $lnnbida_{et}$ and, according to the estimates for $P(S|W_{et}, \check{n}_{et})lnna_{et}$, possibly increasing at a lower speed when increases in this variable are caused by more low-price bidders rather than by more high-price bidders. For specification 7.6 and with one high-price bidder so that $nbida_{et} = na_{et} + 1$, the estimated coefficients imply that, in the long term, collusion increases prices by 37%, 57%, and 75% when the number of low-price bidders equals 2, 3, and 4, respectively. Hence, in relative terms, prices are increased more by collusion the higher the number of bidders is, given that the bidders manage to form and uphold a collusion. However, the price effect of collusion in SEK is relatively stable because the competitive prices fall fast in $nbida_{et}$ and na_{et}. Also, note that the collusive prices fall in $nbida_{et}$ as the sum of the coefficients for $P(S|W_{et}, \check{n}_{et})lnnbida_{et}$ and $lnnbida_{et}$ is negative. Because the sum of the coefficients for $lnna_{et}$ and $P(S|W_{et}, \check{n}_{et})lnna_{et}$ is also negative, the collusive prices falls faster in na_{et} than in Na_{et} (the other source of variation in $nbida_{et} = na_{et} + Na_{et}$).

Price effects reported in *ex ante* studies like ours (Baldwin et al., 1997; Price, 2008; Athey et al., 2011; Byrne and DeRoos, 2019; and Schurter, 2020) lie below 20%, while the meta-analyses of Connor and Bolotova (2006) and Connor (2014) report average price effects in convicted cartels of 29% and 23%, respectively. Our findings are thus more in line with the results from the cartel literature, and especially the findings of Clark et al. (2022) and Starc and Wollman (2022) when analyzing the impact of the U.S. generics pharmaceuticals cartel on prices. Starch and Wollman (2022) observed price increases of 45–50% on average, while Clark et al. (2022) reported increases of between 0% and 166%, depending on the pharmaceutical substance. Price effect of collusion can be expected to be larger for pharmaceuticals than for most other products given that demand on the market level for pharmaceutical is relatively inelastic.[41]

The estimates for $lnna_{et}$ show that the competitive prices fall more if $nbida_{et}$ is increased by an additional low-price bidder as opposed to by an additional high-price bidder. This is, at least partly, explained by the fact that low-price bidders have higher market shares than high-price bidders, meaning that a percentage price decrease for low-price bidders mechanically has a larger impact on the average price (P_{et}) than an equally large reduction among high-price bidders. It is also interesting to note that the estimate for $lnnbida_{et}$ for specification 7.2 (–0.724) is close to the long-term estimate for this

[41] Estimates on the demand elasticity on the market level for prescription pharmaceuticals range from close to 0 to − 0.33 (Kanavos and Costa-Font, 2005).

variable for specification 7.3 (–0.726 ≈ –0.210/(1-0.366-0.206-0.096-0.061)), while the estimate for $lnna_{et}$ is close to the short-run estimate for this variable for specification 7.3. A partial explanation for this is that $lnnbida_{et}$ is more stable over time; its partial correlation with its first lag, conditioned on the other explanatory variables of specification 7.2 is 0.95, while the corresponding correlation for na_{et} is 0.53.

We find no significant evidence that prices are falling in the number of therapeutic alternatives ($lnThAlt_{at}$) and only weak evidence that prices fall fastest during the first months after patent expiration ($lnMonths\_PatG_{at}$). Prices are found to increase by 0.24% for each percentage increase in the strength ($lnStrength_{e}$) and to fall by 0.20% for each percentage increase in package size ($lnSize_{et}$). The signs for $lnStrength_{e}$ and $lnSize_{et}$ are expected because the active ingredients are costly and the cost for packaging per pill should fall in the number of pills. The 20 indicator variables for administrative forms included in specifications 7.2, but not presented in tables, are jointly significant at the 0.1% level. Compared to the control group of ordinary tablets and capsules, to which 71% of the observations belong, the next most frequent administrative form, which is tablet and capsules with extended release (16%), has a positive effect on prices. Lastly, both the time fixed effects and the active ingredient or exchange group fixed effects are jointly significant at the 0.1% level across all specifications.

In addition to the specifications presented in Table 12, we have also estimated a version of specification 7.2 where $P(S|W_{et}, \check{n}_{et})$ is replaced by an indicator variable that takes the value one when $P(S|W_{et}, \check{n}_{et})$ exceeds 0.90. The effect of this indicator variable is that the price increases by 13% (s.e. 3%), which indicates that the price effects of collusions can be significantly underestimated if all the observations that are not considered to be collusive with 90% certainty are treated as competitive. A key reason for this, of course, is that many observations with $P(S|W_{et}, \check{n}_{et}) < 0.90$ are likely to be affected by collusions, thus raising prices in this group as well. In fact, 56% of the observations predicted to be affected by collusion, and an equally large share of the sales, have a value of $P(S|W_{et}, \check{n}_{et})$ below 0.90.We have also investigated whether the price effects of collusion differ depending on whether the collusion is achieved through bid-rotation or parallel bidding by including an interaction term between $P(S|W_{et}, \check{n}_{et})$ and an indicator for parallel bidding in versions of specifications 7.3–7.6. The coefficients for this variable are -0.077 (s.e. 0.019) and -0.091 (s.e. 0.023) in the specifications without interaction variables and -0.005 (s.e. 0.017) and -0.005 (s.e. 0.018) in the specifications without interaction variables. That is, for a given number of bidders, we find no significant differences in the price effects of bid-rotation and parallel bidding, but, on average, the percentage price effects of parallel bidding patterns are smaller because these patterns more often take place in exchange groups with fewer bidders.

Table 12. Estimation results for the effect of $P(S|W_{et}, n_{et})$ on lnP_{et}

Specification Estimator	7.1 OLS	7.2 OLS	7.3 OLS	7.4 IV	7.5 OLS	7.6 IV
$lnP_{e,t-1}$			0.366*** (0.019)	0.363*** (0.019)	0.364*** (0.019)	0.360*** (0.018)
$lnP_{e,t-2}$			0.206*** (0.013)	0.204*** (0.013)	0.206*** (0.013)	0.205*** (0.013)
$lnP_{e,t-3}$			0.096*** (0.013)	0.095*** (0.013)	0.096*** (0.013)	0.095*** (0.013)
$lnP_{e,t-4}$			0.062*** (0.013)	0.061*** (0.013)	0.062*** (0.013)	0.061*** (0.013)
$P(S\|W_{et}, \breve{n}_{et})$	0.264*** (0.051)	0.268*** (0.045)	0.108*** (0.012)	0.122*** (0.018)	-0.061* (0.027)	-0.094* (0.037)
$P(S\|W_{et}, \breve{n}_{et})lnnbida_{et}$					0.178*** (0.028)	0.196*** (0.034)
$P(S\|W_{et}, \breve{n}_{et})lnna_{et}$					-0.071* (0.034)	-0.047 (0.051)
$lnnbida_{et}$		-0.724*** (0.078)	-0.210*** (0.030)	-0.247*** (0.039)	-0.304*** (0.036)	-0.352*** (0.044)
$lnna_{et}$		-0.099 (0.065)	-0.099*** (0.017)	-0.140*** (0.040)	-0.095*** (0.027)	-0.156* (0.061)
$lnThAlt_{at}$		-0.064 (0.089)	-0.039 (0.026)	-0.039 (0.026)	-0.036 (0.026)	-0.034 (0.026)
$lnMonths\_PatG_{at}$		-0.499* (0.226)	-0.094 (0.060)	-0.102 (0.059)	-0.085 (0.058)	-0.092 (0.057)
$lnStrength_e$		0.239* (0.094)				
$lnSize_{et}$		-0.199*** (0.033)				
$dlnP_e^*/dP(S\|W_e^*, \breve{n}_e^*)$	0.264*** (0.051)	0.268*** (0.045)	0.399*** (0.053)	0.441*** (0.077)	0.444*** (0.054)	0.503*** (0.088)
$dP_e^*/dP(S\|W_e^*, \breve{n}_e^*)$ *in percent*	30.150*** (6.584)	30.676*** (5.853)	49.076*** (7.945)	55.459*** (11.974)	55.879*** (8.480)	65.344*** (14.608)
Active Ingre. FE	yes	yes	no	no	no	no
Form FE	no	yes	no	no	no	no
Exchange group FE	no	no	yes	yes	yes	yes
Year × month FE	yes	yes	yes	yes	yes	yes
R^2	0.803	0.866	0.458	0.457	0.460	0.459
Log-l	-27,917	-22,358	-3,328	-3,352	-3,280	-3,303
N	28,863	28,863	28,851	28,851	28,851	28,851
K-P rk LM				96.229		89.790
K-P rk LM, p-v.				0.000		0.000
Hansen J, p-v.				0.084		0.080

Note: See Table D1 for variable definitions and descriptive statistics. Standard errors robust to correlation across observations for products with the same active ingredient are reported in parentheses. The derivative $dlnP_e^*/dP(S|W_e^*, \breve{n}_e^*)$ shows the long-term effect of collusion on logarithmic prices calculated at within sample means for $lnnbida_{et}$, and $lnna_{et}$, when all exchange group by month observations are weighted equally, and $dP_e^*/dP(S|W_e^*, \breve{n}_e^*)$ *in percent* equals $100\times\left(exp\left(dlnP_e^*/dP(S|W_e^*, \breve{n}_e^*)\right) - 1\right)$. Twelve exchange groups have only one observation each and are therefore dropped in specification 7.3–7.6. For specifications with exchange group fixed effects, within R-squared values are reported. K-P rk LM refers to the Kleibergen-Paap rk LM statistic, which indicates the strength of the instruments. The null hypothesis in the K-P test is that the model is under-identified. The null hypothesis for the Hansen J test is that the instruments are valid, i.e., uncorrelated with the error term. *, **, and *** indicate a statistically significant difference from zero at the 5%, 1%, and 0.1% significance level.

7.2. The variability of prices

Collusion can reduce variability in prices for several reasons. One reason is that it is difficult for colluding firms to coordinate on price changes in response to changes in cost, demand, or competition from firms not participating in the collusion. Without communication, a price cut can trigger a price war while a firm that increases its price might lose most of its sales. For the markets we study, this problem would be especially pronounced for parallel-bidding collusion. During bid-rotation, the designated winner should be able to reduce its price without risking a price war, but it might choose not to do so because of the difficulty of increasing prices without losing market shares if the optimal price reverts later. However, bid-rotating firms could signal their desire to increase the price of the winner by increasing the price they set when another firm is the designated winner. Such behavior can be observed in Figure 4(a), where the generic that is not winning consistently sets a price close to the price cap equal to the highest existing price of exchangeable products.

To determine whether collusion reduces price changes, we follow Abrantes-Metz et al. (2006) by analyzing how the variation of prices differs over time across competitive and collusive regimes. Specifically, we do this by using two different dependent variables, the coefficient of variation (i.e., the standard deviation of the price divided by the mean price) in exchange group *e* during price sequence *p* of the mean price per unit of the PM (CVp_PV_{ep}) and of all low-price bidders' products ($CVp\_meanLOW_{ep}$), respectively. Price sequences consist of succeeding months in which there is the same W_{et}, e.g., b_m34_{et} or p_m11_{et}. The length of the price sequences is 1 for 1,319 of the 28,863 exchange group by month observations—and for these observations, the two variables just mentioned are not defined.

Collusion can also reduce the variance of bids at a given action by truncating the prices from below, as described by researchers such as Imhof (2019). To study this, we use CVt_LOW_{et}, defined as the coefficient of variation of the prices of low bidders in exchange group *e* month *t*, as the dependent variable. In all regressions, $P(S|W_{et}, \check{n}_{et})$ is the main variable of interest, and specifications 7.7–7.12 include indicator variables for 3, 4, 5, and 6 or more active[42] bidders ($nbida_{et}$) and low-price bidders (na_{et}), respectively, as well as for exchange group and year × month fixed effects. In three specifications, we also include an interaction variable between the probability of collusion and an indicator variable for parallel bidding weakly exceeding three months, $P(S|W_{et}, \check{n}_{et})Par_{et}$. In addition, we estimate specification 7.13, which is identical to specification 7.4 presented in section 7.1, except that lnP_{et} and its lags are replaced by CVt_LOW_{et} and lags of it. The motivation for this is that, according to the reasoning of Imhof (2019), the coefficient of variation should be smaller the higher the prices are.

The results presented in Table 13 and Table 14 show that all three measures of price variability decrease in line with the probability of collusion, $P(S|W_{et}, \check{n}_{et})$, although for the dependent variable CVp_PV_{ep} it is only the sum of the coefficients for $P(S|W_{et}, \check{n}_{et})$ and $P(S|W_{et}, \check{n}_{et})Par_{et}$, denoted by ($dY_{et}/dP(S|W_{et}, \check{n}_{et})$ if Par_{et} = 1), that are significantly negative. Considering that the means of CVp_PV_{ep}, $CVp\_meanLOW_{ep}$, and CVt_LOW_{et} are 0.19, 0.20, and 0.39, respectively, the point estimates for $P(S|W, n)_{et}$ for specifications 7.7–7.12 are relatively small. Specification 7.13, however, indicates that an increase in the probability of collusion has a relatively large effect on CVt_LOW_{et}. As expected, the signs of the estimates for the number of bidders in specification 13 are the opposite of those presented in Table 12 for lnP_{et} as the dependent variable.

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[42] Appendix E presents the results obtained when controlling for potential bidders instead, which are almost identical.

The coefficients for $P(S|W_{et}, \breve{n}_{et})Par_{et}$ are measured less precisely because of the few observations of parallel bidding, but still the results indicate that a collusion has a smaller impact on *CVp_meanLOW*$_{ep}$ if it takes the form of parallel bidding instead of bid-rotation. Based on the arguments in the first paragraph of this subsection, we expected negative point estimates for $P(S|W_{et}, \breve{n}_{et})Par_{et}$for specification 7.8, at least. That we obtained a positive result for specification 7.10 may indicate that non-colluding low-price bidders happen to change prices more frequently during parallel-bidding collusions. Turning to the control variables, all measures of price variability is increasing in the number of bidders, and for a given number of bidders, *CVp_PV*$_{ep}$ and *CVt_LOW*$_{et}$ are increasing more rapidly in the number of low-price bidders (though not monotonically for *CVp_PV*$_{ep}$). Both the year × month fixed effects and the exchange group fixed effects are jointly significant at the 0.1% level.

Table 13. Price variability results for static specifications

Specification	7.7	7.8	7.9	7.10	7.11	7.12
Dependent variable	*CVp_PV*$_{ep}$	*CVp_PV*$_{ep}$	*CVp_mean LOW*$_{ep}$	*CVp_mean LOW*$_{ep}$	*CVt_LOW*$_{et}$	*CVt_LOW*$_{et}$
Estimator	OLS	OLS	OLS	OLS	OLS	OLS
$P(S\|W_{et}, \breve{n}_{et})$	-0.011	-0.010	-0.046***	-0.047***	-0.043***	-0.043***
	(0.007)	(0.007)	(0.009)	(0.010)	(0.011)	(0.012)
$P(S\|W_{et}, \breve{n}_{et})Par_{et}$		-0.018		0.028*		0.019
		(0.012)		(0.012)		(0.014)
$nbida_{et} = 3$	0.032*	0.033*	0.020	0.020	0.079**	0.079**
	(0.014)	(0.014)	(0.011)	(0.011)	(0.026)	(0.026)
$nbida_{et} = 4$	0.081***	0.081***	0.063***	0.062***	0.151***	0.151***
	(0.017)	(0.017)	(0.013)	(0.013)	(0.029)	(0.029)
$nbida_{et} = 5$	0.096***	0.097***	0.100***	0.099***	0.215***	0.215***
	(0.019)	(0.018)	(0.016)	(0.016)	(0.031)	(0.031)
$nbida_{et} \geq 6$	0.106***	0.106***	0.141***	0.140***	0.286***	0.286***
	(0.021)	(0.021)	(0.019)	(0.019)	(0.035)	(0.035)
$na_{et} = 3$	0.026***	0.026***	0.008	0.008	0.050***	0.050***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)	(0.009)
$na_{et} = 4$	0.026***	0.026***	0.009	0.010	0.103***	0.103***
	(0.008)	(0.008)	(0.011)	(0.011)	(0.014)	(0.014)
$na_{et} = 5$	0.013	0.012	-0.010	-0.009	0.153***	0.154***
	(0.009)	(0.009)	(0.012)	(0.012)	(0.018)	(0.018)
$na_{et} \geq 6$	0.049***	0.049***	-0.000	0.000	0.227***	0.227***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.029)	(0.029)
dY_{et}/d$P(S\|W_{et}, \breve{n}_{et})$		-0.028***		-0.020**		-0.024**
if Par_{et} = 1		(0. 011)		(0.009)		(0.010)
Exchange group FE	yes	yes	yes	yes	yes	yes
Year × month FE	yes	yes	yes	yes	yes	yes
Within R^2	0.038	0.038	0.054	0.054	0.052	0.052
Log-l	18459.251	18459.878	21800.823	21802.693	1133.452	1133.668
N	27,544	27,544	27,544	27,544	28,851	28,851

Note: dY_{et}/d$P(S|W_e^*, \breve{n}_e^*)$ if Par_{et} = 1 is the total effect of $P(S|W_e^*, \breve{n}_e^*)$ for parallel bidding patterns, that is, the sum of the coefficients for $P(S|W_{et}, \breve{n}_{et})$ and $P(S|W_{et}, \breve{n}_{et})Par_{et}$. See Table D1 for variable definitions and descriptive statistics. Twelve exchange groups have only one observation each and are therefore dropped and, for specifications 7.7–7.10, an additional 1,307 observations are not used because the dependent variable is not defined. Standard errors robust to correlation within exchange groups are reported in parentheses. *, **, and *** indicate a statistically significant difference from zero at the 5%, 1%, and 0.1% significance level.

Table 14. Price-variability results for a dynamic specification

Specification	7.13
Dependent variable	CVt_LOW_{et}
Estimator	IV
$CVt\_LOW_{e,t-1}$	0.263***
	(0.013)
$CVt\_LOW_{e,t-2}$	0.035**
	(0.011)
$CVt\_LOW_{e,t-3}$	0.087***
	(0.012)
$CVt\_LOW_{e,t-4}$	0.043***
	(0.011)
$P(S\|W_{et}, \breve{n}_{et})$	-0.060***
	(0.010)
$lnnbida_{et}$	0.102***
	(0.027)
$lnna_{et}$	0.252***
	(0.029)
$lnThAlt_{at}$	0.002
	(0.022)
$lnMonths\_PatG_{at}$	0.061
	(0.048)
$dCVt\_LOW_e^*/$ $dP(S\|W_e^*, \breve{n}_e^*)$	–0.104***
	(0.017)
$dCVt\_LOW_e^*/dP(S\|W_e^*, \breve{n}_e^*)$ in percent	–9.928
	(1.570)
Exchange group FE	yes
Year × month FE	yes
Within R^2	0.148
Log-l	2660.062
N	28,804
K-P rk LM	97.191
K-P rk LM, p-v.	0.000
Hansen J, p-v.	0.896

Note: See Table D1 for variable definitions and descriptive statistics. Standard errors that are robust to correlation within exchange groups are reported in parentheses. The derivative $dCVt\_LOW_e^*/dP(S|W_e^*, \breve{n}_e^*)$ shows the long-term effect. K-P rk LM refers to the Kleibergen-Paap rk LM statistic, which indicates the strength of the instruments. The null hypothesis in the K-P test is that the model is under-identified. The null hypothesis for the Hansen J test is that the instruments are valid, i.e., uncorrelated with the error term. *, **, and *** indicate a statistically significant difference from zero at the 5%, 1%, and 0.1% significance level.

7.3. The cost of collusion

In this section, we calculate how much lower the cost of the drugs bought would have been in the absence of collusion, holding the quantities bought and variables other than $P(S|W_{et}, \check{n}_{et})$, its interactions, and lnP_{et} and its lags constant. We do this for the period September 2015–November 2019.

We form conservative calculations of the overcharge using the results of specification 7.2 and form calculations that we judge to be closer to the true overcharge using the results of specification 7.6. First, we calculate $\overline{dP_e^*/dP(S|W_e^*, \check{n}_e^*)\%}$, which for specification 7.6 differs from the derivative reported in Table 12 by being a weighted average over all observations of the long-term price effect of collusion in percentages, using the sales values in current SEK for each exchange group by month observations as weight. Because markets with larger sales attract more bidders for which we found a larger relative effect of collusion and a percent-on-percent effect, the value of this derivative exceeds the percentage price effect of the derivative reported in Table 12. Specifically, it equals 83.397 (s.e. 19.216) for specifications 7.6, while for specification 7.2, the relevant estimate remains 30.676 (s.e. 5.853) as the percentage price effect of collusion by assumption is equal for all observations according to this specification. After that, we note that the average of $P(S|W_{et}, \check{n}_{et})$, again using sales as weights, equals 0.668 (i.e., it is just slightly higher than the raw average of 0.642), implying that the average price increase caused by collusion in this sample is approximately 55.674% (s.e. 12.828%) and 20.479% (s.e. 3.907%), respectively, according to the two specifications.

For the dynamic specification 7.6, 55.674% is not an exact point estimate because the value of $P(S|W_{et}, \check{n}_{et})$ changes over time and the long-term effect of the current value of $P(S|W_{et}, \check{n}_{et})$ is never fully realized. Therefore, we also simulate what the prices would have been if $P(S|W_{et}, \check{n}_{et})$ had equaled zero from September 2015 onwards using specification 7.6. The simulations indicate that the weighted average price increase caused by collusion during the estimation period is 51.007% (s.e. 3.271%).[43,44]

The pharmacies' total purchase costs for the drugs in the estimation sample amounted to 10,182 million SEK during the 51 months studied—that is, on average 2,396 million SEK per year. If it had not been for price increases of 51.007% because of collusion, the cost for these would have been 28.961%[45] (s.e. 1.167), or 694 (s.e. 28) million SEK, lower per year. If we instead use the conservative estimate obtained using specification 7.2, the overcharge of 15.671% (= [1-1/1.30676] × 0.668) (s.e. 2.288%) amounts to 375 (s.e. 55) million SEK per year. Using the conservative estimates, two firms have gained more than 100 million SEK (10 million USD) each in excess revenues because of collusions during the study-period, while 25 firms gained more than 10 million SEK (1 million USD) each.

[43] The standard error is obtained by making 1000 draws from the distribution of the parameter estimates for the three variables including $P(S|W_{et}, \check{n}_{et})$ and the four lags of the dependent variable, and for each draw simulating the weighted average price increase during the estimation period.

[44] If, for each exchange group, we exclude the first six (twelve) months after entering the estimation sample, the estimate rises to 58.248% (60.655%), which is partly explained by the fact that less of the long-term effect is realized during the first months and partly by there being different distributions of $P(S|W_{et}, \check{n}_{et})$, $lnnbida_{et}$, and $lnna_{et}$ across months.

[45] Note that this is not identical to 1-1/1.51007 because this expression is a non-linear function of the denominator, which varies across observations.

8. Market characteristics and the probability of collusion

The purpose of this section is to estimate the effect of the number of bidders and other variables on the probability of collusion, $P(S|W_{et}, \check{n}_{et})$. Many bidders makes it more difficult to coordinate on a bidding pattern. It can also increase the gain from deviating because each bidder receives a smaller share of the collusive profits. This is clearly the case for parallel bidding collusions. To demonstrate this, we assume that all consumers buy the cheapest product, implying that *nbid* = *n*· and that the quantity bought, Q, is not affected by the price of the cheapest product. Given that, a bidder can increase its revenue during one month from $p^C Q/n$ to $(p^C\text{-}e)Q$ by undercutting the parallel bidding price, p^C, by e. However, for bid-rotation, the one-month revenue increase by deviating is $(p^C\text{-}e)Q$ irrespective of *n*, because bidders do not make any of the sales in the months that it is others' turn to be the cheapest, given the assumption in this paragraph. (This is also the reason why parallel bidding should be a more stable form of collusion). A third factor—common for both bid-rotation and parallel bidding—is that more firms reduce the long-term benefit from maintaining the collusion.[46]

We start by estimating a simple specification (8.1) with the number of bidders serving as the only explanatory variables and no fixed effects.[47] In specification 8.2, we add indictor variables for year × month-combinations and other explanatory variables, as described below, while in specification 8.3 fixed effects for exchange groups are also added. Thus, in specification 8.3 we study differences in changes in the probability of collusions across exchange groups with different changes in the number of bidders. If firms have been able to coordinate on a collusive behavior and uphold the collusion to, at least, month *t – 1*, it is likely that they also collude in month *t*. Therefore, we add $P(S|W_{e,t-1}, \check{n}_{e,t-1})$ as an explanatory variable in specifications 8.4–8.6.[48]

We use two bidders as our reference category, and include indicator variables for three, four, and five or more active bidders (I$\{nbida_{et} = m\}, for\ m = 3, 4, or\ \geq 5$), as well as the continuous variable $nbida6_{et} = max\{0, nbida_{et} - 5\}$. In the estimation sample, $nbida_{et}$ equals six for 14% of the observation and seven or more for 16%. We use a continuous variable to capture this variation instead of indicator variables to reduce the number of endogenous variables, and because estimation results (not

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[46] According to the theoretical model in section 5.1, each firm's profit during collusion, $\pi^S/n = P_O(1 - S_H)/n$, and each firm's expected profit during competition $\pi^K/n = P_O \frac{1-S_W-S_H}{(n-1)}$ are both decreasing in *n*, but still more firms reduces the long-term benefit from maintaining the collusion because $(\pi^S - \pi^K)/n$ is decreasing in *n*. To demonstrate this, we use $\frac{\partial \pi^K}{\partial n} = \frac{-P_O(1-S_W-S_H)}{(n-1)^2}$, which gives $\frac{\partial(\pi^S-\pi^K)/n}{\partial n} = \left[\frac{P_O(1-S_W-S_H)}{(n-1)^2} - P_O(1 - S_H)\right]/n^2$, which is negative because $(1 - S_H) > \frac{(1-S_W-S_H)}{(n-1)^2}$.

[47] The fact we estimate a specification without fixed effects here while we did not in section 7, is explained by the different dependent variables. The dependent variable in section 7.1, lnP_{et}, varies across active ingredients due to variation in the health benefit per smallest unit (e.g., pill or gram) which affects the price during patent protection which in turn affects the price cap, and because of variation in marginal cost per smallest unit. Also, prices in SEK should vary over time (e.g., due to variation in different exchange rates). We see no reason to expect variation across markets and time in $P(S|W_{et}, \check{n}_{et})$ that would *ex ante* make estimations without fixed effects meaningless. Moreover, only 29% (13%) of the variance in the dependent variable $P(S|W_{et}, \check{n}_{et})$ is explained by fixed effects for exchange groups (active ingredient), while these fixed effects explain 93% (80%) of the variance of the dependent variable of section 7.1, lnP_{et}.

[48] We have estimated models with up to four lags for the sample that can be estimated with four lags, and the Akaike's Information Criteria is minimized by including only one lag. Also, the coefficient estimates for the second to fourth lags are below 0.02 in absolute value.

reported in tables) suggest that the additional effect of an additional bidder is small already at $nbida_{et} = 6$. We use $nbida6_{et}$ instead of $nbida_{et}$ because otherwise the estimates for $nbida_{et}$ must also be included when interpreting what the results say about the effect of, for example, a third bidder.

The three dummy variables $\mathrm{I}\{nbida_{et} = m\}, for\ m = 3, 4, or\ \geq 5$ and $nbida6_{et}$ can all be endogenous because higher prices caused by collusion can increase entry and reduce exit (Starc and Wollman, 2022). In specifications 8.4–8.6, we address this by using the possibly endogenous variables' three-month lags and $lnQ_{e,t-3}$ as an instrument, but for the static specifications (8.1–8.3) we find no strong and valid instruments and therefore present only OLS results. In Appendix C we discuss the relevance and validity of the instruments we use. OLS results for specifications 8.4–8.6 are presented in Appendix E together with IV results for specification 8.4 when one-month or six-month lags of the endogenous variables are used as instruments instead.

Specification 8.4 is written as follows:

$$P(S|W_{et}, \breve{n}_{et}) = \theta P(S|W_{e,t-1}, \breve{n}_{e,t-1}) + \sum_{m=3}^{\geq 5} \tau_m \mathrm{I}\{nbida_{et} = m\} + \beta nbida6_{et} + \boldsymbol{X_{et}\gamma} + \eta_t + \mu_e + \varepsilon_{et,} \quad (8.4),$$

where $\boldsymbol{X_{et}}$ is a set of market characteristics described below, which according to theory should affect the probability of collusion; θ, τ, β and $\boldsymbol{\gamma}$ are parameters to be estimated; η_t and μ_e are year × month and exchange group fixed effects; and ε_{it} is the error term, which is allowed to be correlated among observations for products with the same active ingredient.

Specification 8.5 differs from specification 8.4 in that it uses potential bidders instead of active bidders. To determine whether the different types of bidders affect the risk of collusion differently, specification 8.6 also includes indicator variables for the number of bidders marketing locally sourced generics, $nlsGena_{et}$, and uses their third lags as instruments.[49]

Lower entry barriers will decrease the risk of collusion (Kreps, 1994). The barriers to enter an exchange group are often lower for firms that sell other products with the same active ingredient in Sweden, because when applying for marketing authorization the same fee covers all administrative forms and strengths of a pharmaceutical that the firm applies for at the same time.[50] The entry barriers should be especially low for firms selling products with the same active ingredient, strength, and administrative form, because they will not need to pay any additional application or yearly fee to sell an additional package size and will only need to make changes in the packing stage to do this. Therefore, we proxy time-variant difference in entry barriers with the number of active bidders marketing products with the same active ingredient, strength, and administrative form as in exchange group *e*, but not marketing products in exchange group *e* ($add\_bida\_drug_{et}$). When controlling for the number of bidders in the exchange groups, the variation in $add\_bida\_drug_{et}$ is thus caused by entry and exit in other exchange groups, which should be exogenous.

Cost and quality differences can make it more difficult to coordinate (Feuerstein, 2005) and can also increase the probability that high-quality low-cost firms deviate from collusion if they have a larger price-cost margin (Ivaldi et al., 2003). We proxy cost and quality differences with variation across firms in market share that are not explained by whether the firm sells a PM. The motivation is that products

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[49] We use these indicator variables for $nlsGena_{et}$ instead of for na_{et} because we are unable to find instruments for na_{et} that are both strong and valid.

[50] The application fee for a generic is currently SEK 300,000 (https://www.lakemedelsverket.se/en/permission-approval-and-control/marketing-authorisation/fees#hmainbody3), accessed May 5, 2023.

with higher perceived quality will secure a larger market share at equal prices, and that low-cost products can secure a larger market share by having lower prices. We do not use variation caused by the share of the months in which different products sell a PM because these shares can be affected by whether firms collude or not. More specifically, we define the proxy $QualityDiffSD_{et}$ as the standard deviation over low-price bidders in exchange group e of $\sum_{m=t-7}^{t}\sum_{pm=0}^{1}\left(Share\_pm_{fem} - \overline{Share\_pm_{e,t-7,t}}\right)I\_pm_{fem}$. Here, pm = 0, 1/3, ½, 1 denotes whether a firm did not sell a PM in a given month, sold it but there was a tie between three or more firms (pm = 1/3) or between two firms (pm = 1/2), or the firm sold the only PM. I_pm_{fem} is an indicator variable taking the value one if $pm = PM_{fet}/nPM_{et}$ and zero otherwise. The expression within the parentheses shows for each value of pm how a low-price bidder's market share differs from this average for all low-price bidders in the exchange group from the months $t-7$ through t. We use an eight-month average because we want to capture variation that firms think can persist and therefore affect the probability of collusion in month t.[51]

By making it more difficult to attract customers from other firms, product heterogeneity makes deviation less profitable, but it also makes the possible punishment less severe. For price-setting games, Deneckere (1983) showed that the first effect dominates if the products are sufficiently differentiated to guarantee that the deviating firm does not capture the whole market. We proxy product heterogeneity with $Hetrogeneity_{et}$ which is the mean for exchange group e from month $t-7$ through month t of the share of packets sold of low-price bidders in exchange group e which is not of the cheapest product (i.e., a PM or (one of) the last month's PM during the first half of the month) minus the average of this for all exchange group by month observations in the main dataset with the same value of pm. For observations in the estimations sample (i.e., observations for which $P(S|W_{et}, \check{n}_{et})$ is calculated, see section 6), $Hetrogeneity_{et}$ has a mean of 0.02 (not zero because the averages in the estimation sample differ slightly from those in the main dataset), a standard deviation of 0.08, and a range of -0.13–0.66.

Differences in capacity constraints can hinder collusion because of the limited possibility of low-capacity firms to punish deviation. However, the Swedish markets are small and therefore it is likely that all sellers of locally sourced products can meet the entire demand if they receive a few months' notice. Hence, we expect asymmetry in capacity constraints to be a relatively unimportant determinant of collusion in our setting, but for sellers of parallel imports the capacity constraints are more likely to be binding because they compete in buying excess packages in countries with lower prices. What is important for maintaining collusion is that at least one other firm can punish each potential deviator sufficiently strongly, e.g., that A can punish B and C, and B can punish A and C. Therefore, we control for this mechanism by including the indicator variable $Over2ls_{et}$ which takes the value one if two or more of the low-price bidders are selling locally sourced products. For a given value of the number of active bidders in exchange group e at time t, $nbida_{et}$, variation in $Over2ls_{et}$ is caused by variation in the share of $nbida_{et}$ that only sells parallel imports. We expect this to be largely driven by prices in the exporting countries and hence to be exogenous. The variable $Over2ls_{et}$ equals one for 97.16% of the observations in the estimation sample.[52]

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[51] Note that when firms decide on their prices for month t, they are only aware of the market shares for months up to $t-3$ and have only some information for month $t-2$. Still, we let $QualityDiffrences_{et}$ include values up to month t because the probability of collusion month t can also depend on decisions taken in months t and later as these decisions can affect the length of a possible collusive scheme involving month t.

[52] Note that in specifications where we control for the number of bidders marketing generics, the parameter for $Over2ls_{et}$ is still identified so long as not all generics firms are low-price bidders, or if an originator is also a low-price bidder.

As Bernheim and Whinston (1990) have observed, multimarket contact serves to pool the incentive constraints from all the markets served by the firms and can therefore increase the probability of collusion. We defined $MultiM\_low_{et}$ as the average over each pair of low-price bidders in an exchange group of the total number of exchange groups, E, the bidders currently have contact in as low-price bidders. This definition follows the definition of Evans and Kessides (1994), except that we only look at contact between low-price bidders as we study collusion involving low-price bidders. To be exact, we define

$$alow_{klt} = \sum_{e=1}^{E} Dlow_{ket} Dlow_{let} \qquad k = 1, 2, \dots \underline{F} - 1; l = k + 1, k + 2, \dots \underline{F}, \tag{8.2),}$$

where $Dlow_{fet}$ for $f = k, l$ denotes dummy variables taking the value one if low-price bidder f marketed a product in exchange group e month t, and $\underline{F}$ is the number of low-price bidders marketing products within the PM-system during month t. Then,

$$MultiM\_low_{et} = \frac{1}{[n_{et}(n_{et} - 1)/2]} \sum_{k=1}^{\underline{F}-1} \sum_{l=k+1}^{\underline{F}} alow_{klt} Dlow_{ket} Dlow_{let}. \tag{8.3),}$$

As described by Ciliberto and Williams (2014), measures of multimarket contact depend on the identity of bidders in a market and can also be correlated with unobservables that affect prices and can therefore be endogenous. In our setting, the dependent variable is not prices but rather the probability of collusion, and we are primarily concerned that firms active in more markets might be more able to collude, purely because of experience. To prevent this from being captured by the multimarket variables, we include $Markets\_low_{et}$, which is the average over low-price bidders of the number of exchange groups the bidders market products and is defined to be a low-price bidder in. Following Ciliberto and Williams, we include the squares and cubes of the multimarket contact variables because we expect the marginal effect of multimarket contact to decline, and we also include squares and cubes of $Markets\_low_{et}$.

We control for the fact that demand fluctuations can hinder collusion with the variables $QSeason_{et}$, $QTrend_{et}$, and QSD_{et}. The first is intended to capture the fact that, during months with large sales compared to the closest following months, there is an increased incentive to deviate from a collusion and a reduced incentive to initiate a collusion by letting another firm win, and $QSeason_{et}$ is defined as the average number of packages sold within the exchange group during the calendar months of t during 2015–2019, divided by the average number of packages sold within the exchange group during the calendar months of $t + 2$ and $t + 3$ during the same years. For example, if the current month is January, the numerator is the average over January months during 2015–2019, while the denominator is the average number of packages sold in exchange group e during the months of March and April from 2015–2019. Hence, we expect $QSeason_{et}$ to have a negative effect on the likelihood of collusion. We use averages over five years because seasonal patterns in pharmaceutical sales are relatively stable, and firms have not observed the actual sales for month t or the coming month when they set the price for month t. Of course, this is only a proxy because the future sales that matter differs depending on the type of collusive pattern firms follow or are considering following; the sales in months $t + 2$ and $t + 3$ are most relevant for two- and three-firm bid-rotations, respectively, which are the most common collusive patterns in our data (see Table 2).

$QTrend_{et}$ is defined as the number of packages in exchange group *e* sold from $t-5$ through $t-3$ divided by the number of packages sold five months earlier; $t-10$ through $t-8$.[53] It controls for the fact that an increased demand should reduce the likelihood that firms deviate from a collusion, holding the current and expected number of firms constant, but also that increased demand can increase the expected number of bidders, which in turn can reduce the probability of collusion. Depending on which effect dominates, $QTrend_{et}$ can either have a negative or a positive effect on the probability of collusion.

Lastly, QSD_{et} is defined as the standard deviation of $Q_{e,t-10}$ through $Q_{e,t-3}$. This variable is intended to capture the fact that variability in demand, all else being equal, can reduce the risk of collusion.

One concern is that $QTrend_{et}$ can be endogenous because a higher price due to collusion could reduce the quantity sold. However, in the absence of autocorrelation in the error term, this should not be the case because we control for the one-month lag of the dependent variable, meaning the error term reflects unexplained changes in the probability of collusion between month t and month $t-1$, and this should not affect lagged quantities. For the same reason, QSD_{et} should not be endogenous either. It is more likely that $QSeason_{et}$ is endogenous because a positive error term through its effect on prices could reduce the quantity sold in the current month and hence increase $QSeason_{et}$. For this reason, we instrument $QSeason_{et}$ with the leaving-one-out instruments $QSeasonIns_{et}$. For example, if *t* is January 2018, the sales values for exchange group *e* for all January months in the data except January 2018 is used to calculate $QSeasonIns_{et}$. $QSeasonIns_{et}$ is set as equal to 1 for 53 observations for which it otherwise would be missing.

Several factors known to affect the probability of collusion are either constant over all the markets we study, or constant over time, and therefore captured by the exchange group fixed effect. For example, for all markets, firms interact each month, and information about prices and quantities are publicly available. Other factors likely have too little variation over time to have any significant effect on the probability of collusion. We believe this to be the case for the price elasticity of demand at the exchange group level. This is likely small across all exchange groups and months because the same pharmaceutical benefit scheme covers all exchange groups; plus, the cut-offs for the coinsurance tiers have not been changed, only adjusted according to inflation, since 2012. In addition, firms are prohibited from setting the monopoly price because the DPBA have declared a price cap for each markets. This price cap can itself act as a focal price and hence facilitate collusion, but in markets with many bidders or low entry cost, the colluding firms might have to choose lower prices to avoid being under-cut by others and to increase the stability of the collusion. That is, there can be variation in the existence of a focal price, but that variation should be captured by variables already included in the estimations such as $nbida_{et}$, $add\_bida\_drug_{et}$, and the fixed effects, and we therefore do not add a separate variable for the potential existence of a focal price.

When discussing the results presented in Table 15, we refer to the dependent variables $P(S|W_{et}, \check{n}_{et})$ simply as the probability of collusion but recall that the exact definition of $P(S|W_{et}, \check{n}_{et})$ is the probability that the bidding pattern, which exchange group *e* was part of in month *t*, was at least partly caused by collusion. Hence, $P(S|W_{et}, \check{n}_{et})$ exceeds the probability that it was collusion this month if,

[53] To reduce the correlation with $QSeason_{et}$, we have also estimated one specification with $QTrendY_{et}$ (defined as the number of packages in exchange group *e* sold $t-5$ through $t-3$ divided by the number of packages sold one year earlier; $t-17$ through $t-15$) instead of $QTrend_{et}$. In that specification, we also use $QSDY_{et}$, defined as the standard deviation in Q_{et} of the values from month $t-14$ through $t-3$ in exchange group *e*, instead of QSD_{et}, which is the standard deviation from month $t-10$ through $t-3$. This alternative specification produces nearly identical results but reduces the number of observations by 1,492.

for example, the winner month t just so happened to be equal to the winner month $t + 2$ in some cases when two firms have a bid-rotation collusion from month $t + 1$ and onwards. This definition of $P(S|W_{et}, \check{n}_{et})$ can affect its average value and therefore also the magnitude of estimated coefficients.

The results show that all statistically significant estimates have the expected signs. Let us start by describing the effect of the number of bidders, which is estimated using indicator variables for 3, 4, or 5 or more bidders and the linear variable $nbida6_{et}$.[54] All specifications show that a third bidder significantly reduces the probability of collusion. According to the point estimates for the IV specifications, the incremental effect of a fourth bidder—that is, the difference between the point estimates for the fourth and the third bidder—is approximately half as large as the effect of the third bidder. For specifications 8.4 and 8.5, it also holds that the incremental effect of the fifth bidder is around half as large as the incremental effect of the fourth bidder, but it is only for specification 8.5 that the incremental effect of the fifth bidder is significantly different from zero. For the other specifications, the incremental effects of the fourth and fifth bidder are smaller, and for no specification do we find a significant reduction in the probability of collusion when the number of bidders increases from five or a larger number (i.e., the effect of $nbida6_{et}$ is not significantly negative). Thus, the results indicate that the effects of additional bidders decline rapidly.

How strong, then, is the effect of the number of bidders on the likelihood of collusion? According to specification 8.4, a third bidder reduces the probability of collusion by 10.4 percentage points in the short run and by 26 percentage points $[\approx (-0.104)/(1 - 0.595)]$ in the long run (standard error 7.7 percentage points; s.e. 7.7 pp). The effect is similar for specifications 8.5 and 8.6 and, despite endogeneity, the estimate for the static OLS specification 8.2 of –22 percentage points is comparable to the long-term estimates of the dynamic IV-specifications. Comparing the long-term estimates of specification 8.4 for four and five or more bidders, which are –39 (s.e. 8.3) and –46 (s.e. 7.8) percentage points, respectively, with the corresponding estimates for specification 8.2 (–27 and –28 percentage points), indicates that variations in a higher number of bidders might be more endogenous. This is supported by the significant positive estimate for $nbida6_{et}$ for two of the OLS specifications.

The estimated effects can be compared with the fact that the probability of collusion, on average, is 79% for the 11% of the estimation sample with only two bidders. Thus, according to specification 8.4, the risk of collusion is half as large for four bidders as it is for two bidders. Then, the probability falls by 6 percentage points more if a fifth bidder enters, and thereafter by 2.7 percentage points $[\approx (-0.011)/(1 - 0.595)]$ for each additional bidder, but the latter effect is not significantly different from zero.

The similarities between the estimates of specifications 8.1 and 8.2 indicate that the correlations between the number of bidders and the other explanatory variables are relatively small. That the estimates for the number of bidders are closer to zero in specification 8.3 than in specification 8.2 is expected, as introducing exchange group fixed effects should move the estimates towards the short-term effects. Comparing specifications 8.4 and 8.5 reveals that the number of actual and potential bidders has a similar effect on the risk of collusion, even though the point estimates for potential bidders generally is a half to a third standard error closer to zero than the corresponding estimate for active bidders. These similarities are also expected because of the high correlation between these two variables.

[54] Having indicator variables for five bidders and six or more bidders, instead of the variable for five or more bidders, produces almost identical results.

Specification 8.6 shows significant negative effects for the indicator variables for 2, 3, 4, or 5 or more bidders marketing locally sourced generics, and, compared to specification 8.4, shows that adding these variables reduces the estimates for the indicator variables of $nbida_{et}$. This implies that, all else being equal, an additional bidder reduces the risk of collusion more if it markets locally sourced generics. To be specific, compared to a situation with two bidders—of which only one market a locally sourced generic—adding two more bidders reduces the risk of collusion by 76 percentage points $[\approx (-0.125 - 0.184)/(1 - 0.593)]$ (s.e. 16 pp.) if both market locally sourced generics but only by 31 pp. $[\approx -0.125/(1 - 0.593)]$ (s.e. 10 pp.) if none of the new bidders do this. These estimates are obtained holding all other variables constant and although it is possible that the number of generics bidders changes independently of the other variables, the variable is positively correlated with, e.g., $Over2ls_{et}$ and $MultiM\_low_{et}$, which have a positive effect on the probability of collusion. Including changes in the other explanatory variables, except market and year × month fixed effects, adding two bidders reduces the risk of collusion by 51 pp. (s.e. 16 pp.) if both market generic and by 43 pp. (s.e. 10 pp.) if none of the new bidders do this. The larger effect of generics is expected as Table 1 indicates that 75% of firms marketing generics belong to the low-price segment—and it is collusion in this segment that we study. One may expect a new bidder in the high-price segment to only affect the probability of collusion by making firms contemplating to collude in the low-price segment fearing that the new high-price bidder may switch to the low-price segment.

When interpreting the effects of the other explanatory variables, we focus on the result for specification 8.4. The variable $add\_bida\_drug_{et}$, which indicates that there are firms not presently active in the market that have low entry costs, has a negative effect on the probability of collusion, as expected. In absolute size, the effect is larger than that of a sixth bidder, but smaller than the effect of $nbida_{et}$ being five or more instead of four.

Furthermore, our proxy for consumers' perceptions of quality differences has a negative effect on the probability of collusion, as expected. The point estimates imply that an increase by a standard deviation for $QualityDiffSD_{et}$, which equals 0.078, reduces the risk of collusion by 4.5 (s.e. 0.8) percentage points in the long run. $Hetrogeneity_{et}$ has a similar standard deviation on 0.082, but the estimated effect of this variable is smaller and not significantly different from zero at the 5% level. The proxy for at least two low-price bidders having sufficient capacity to punish deviators swiftly and severely, $Over2ls_{et}$, is estimated to increase the risk of collusion by 37 percentage points (s.e. 7.6 pp.) in the long term.

Our results also confirm previous findings that multimarket contact increases the risk of collusion and, like Ciliberto and Williams (2014), we find that this matters most for low and moderate levels. Figure 10 illustrates the empirical distribution of $MultiM\_low_{et}$ and its short-term marginal effects obtained from specification 8.4, that is, the derivative of $P(S|W_{et}, \check{n}_{et})$ with respect to $MultiM\_low_{et}$ evaluated at different values of $MultiM\_low_{et}$ and holding the lag of $P(S|W_{et}, \check{n}_{et})$ fixed. Specifically, Figure 10 shows that an increase in $MultiM\_low_{et}$ from one to two increases the risk of collusion by around one percentage point in the short run, but the marginal effect then decreases and becomes indistinguishable from zero according to the confidence interval when $MultiM\_low_{et}$ reaches 56. The effects obtained from specifications 8.5 and 8.6 are almost identical to these, and those from specification 8.2 are also close to these short-term effects. In addition, the marginal effects obtained from specification 8.3 are similar and reach zero at $MultiM\_low_{et} = 67$, but they start out with a predicted marginal effect of 1.8 pp compared to 1.2 pp for specification 8.4.

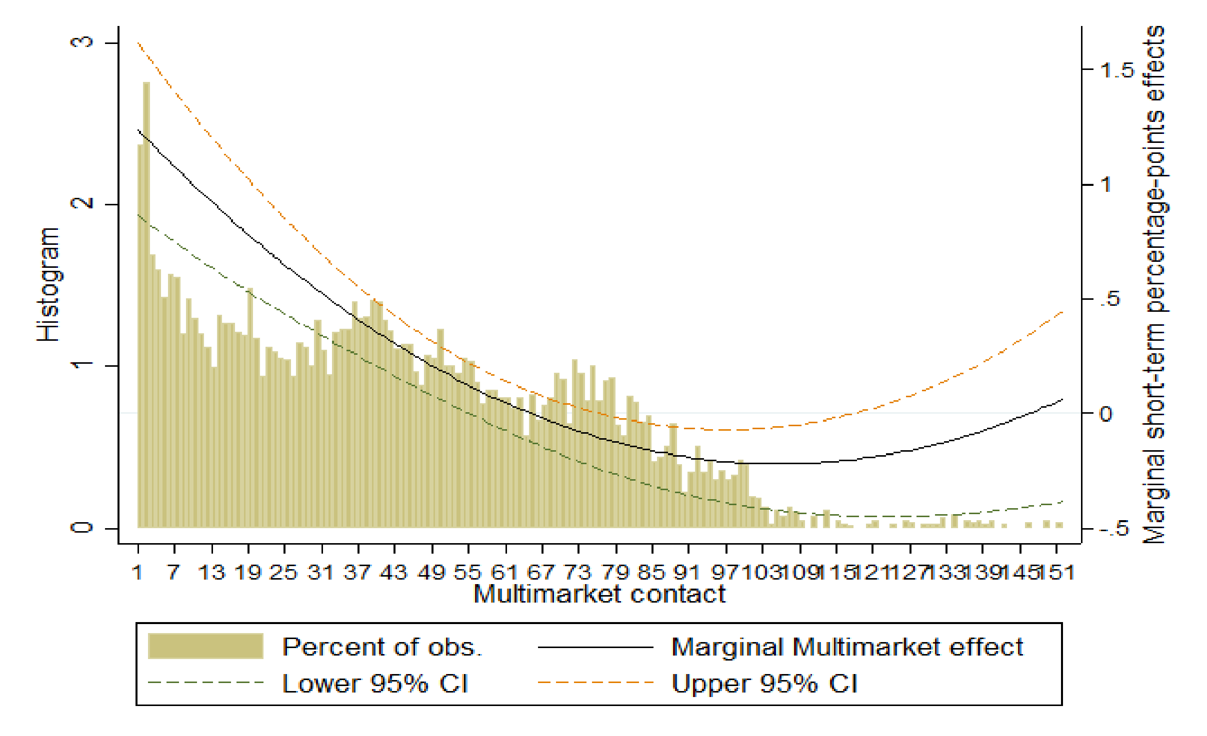

Figure 10. Histogram and marginal short-term percentage-points effects of multimarket contact on the probability of collusion according to specification 8.4.

Regarding the accumulated effect, increasing multimarket contact with one standard deviation, 28.90, increases the probability of collusion by 26 percentage points in the short term if it is increased from its lowest value of one, but increasing it equally much from its median value increases the probability by only 4 percentage points. The total effect of multimarket contact reaches its maximum at $MultiM\_low_{et} = 65$, when it increases the risk of collusion by 35 percentage points in the short run. Thereafter, the effect is reduced to a local-minimum short-term effect of 23 percentage points. Comparing the estimates for multimarket contact with those for number of bidders reveal that that reducing multimarket contact from its third quartile of 75 to its minimum value of 1 reduces the risk of collusion as much as increasing the number of bidders from 2 to 4.

The control variables for the average number of markets in which the low-price bidders market their low-price products ($Markets\_low_{et}$, $Markets\_low^2_{et}$, $Markets\_low^3_{et}$) have the same signs as the multimarket polynomials, and estimations not presented in tables reveal that not including these controls leads to an underestimation of the multimarket-contact effects.

None of the three proxies for demand fluctuations, $QSeason_{et}$, $QTrend_{et}$, and QSD_{et}, has a significant effect on the probability of collusion, except for $QTrend_{et}$ in specification 8.2. The negative estimates for $QTrend_{et}$ indicate that the effect through an expectation of more firms in the future dominates the direct effect of increased demand for a given number of firms in the current month and in the future. That the estimates for $QSeason_{et}$ are not more negative could be seen in the light of many collusions lasting for many years, meaning that even if the temptation to deviate is stronger during months with peak demand, it still might not be sufficiently strong for a firm that otherwise would expect to benefit from collusion during many months. A similar argument might hold for QSD_{et}, but a partial explanation for the non-significant effect of QSD_{et} is that prices are set in advance, so that when a firm realizes that the demand is especially large for month *t*, it is already too late to deviate for the collusion that month. Lastly, both the time fixed effects and exchange groups fixed effect are jointly significant at the 0.1% level.

Table 15. Estimation results for the determinants of the probability of collusion

Specification	8.1	8.2	8.3	8.4	8.5	8.6
Estimator	OLS	OLS	OLS	IV	IV	IV
$P(S\|W_{e,t-1}, \check{n}_{e,t-1})$				0.595***	0.594***	0.593***
				(0.009)	(0.009)	(0.009)
$nbida_{et} = 3$	-0.169***	-0.219***	-0.150***	-0.104***	-0.091**	-0.089*
	(0.029)	(0.029)	(0.034)	(0.031)	(0.029)	(0.039)
$nbida_{et} = 4$	-0.192***	-0.266***	-0.194***	-0.159***	-0.142***	-0.125**
	(0.025)	(0.028)	(0.041)	(0.034)	(0.033)	(0.040)
$nbida_{et} \geq 5$	-0.182***	-0.278***	-0.195***	-0.187***	-0.176***	-0.130**
	(0.026)	(0.028)	(0.038)	(0.032)	(0.030)	(0.043)
$nbida6_{et}$	0.017**	0.011*	0.003	-0.011	-0.008	-0.008
	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(0.006)
$nlsGena_{et} = 2$						-0.196***
						(0.054)
$nlsGena_{et} = 3$						-0.184**
						(0.061)
$nlsGena_{et} = 4$						-0.235***
						(0.062)
$nlsGena_{et} \geq 5$						-0.243***
						(0.065)
$add\_bida\_drug_{et}$		-0.013*	-0.017*	-0.013**	-0.015**	-0.013**
		(0.005)	(0.007)	(0.004)	(0.005)	(0.004)
$QualityDiffSD_{et}$		-0.290**	-0.442***	-0.236***	-0.222***	-0.256***
		(0.093)	(0.079)	(0.042)	(0.042)	(0.043)
$Hetrogeneity_{et}$		-0.052	0.156	0.082	0.078	0.077
		(0.088)	(0.121)	(0.057)	(0.057)	(0.056)
$Over2ls_{et}$		0.043	0.235***	0.151***	0.150***	0.191***
		(0.041)	(0.049)	(0.031)	(0.031)	(0.023)
$MultiM\_low_{et}$		0.014***	0.019***	0.013***	0.013***	0.013***
		(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$MultiM\_low^2_{et}$		-1.65e-4***	-2.11e-4***	-1.42e-4***	-1.42e-4***	-1.40e-4***
		(3.39e-5)	(3.73e-05)	(2.33e-5)	(2.34e-5)	(2.27e-5)
$MultiM\_low^3_{et}$		5.37e-7***	6.60e-7***	4.42e-7***	4.48e-7***	4.38e-7***
		(1.49e-7)	(1.42e-7)	(9.16e-8)	(9.23e-8)	(8.94e-8)
$Markets\_low_{et}$		1.11e-3	2.06e-3	1.73e-3	1.68e-3	2.21e-3*
		(1.88e-3)	(1.67e-3)	(9.26e-4)	(9.39e-4)	(9.55e-4)
$Markets\_low^2_{et}$		-1.89e-5	-3.45e-5*	-2.64e-5**	-2.60e-5**	-2.85e-5**
		(1.93e-5)	(1.63e-5)	(8.77e-6)	(8.89e-6)	(8.81e-6)
$Markets\_low^3_{et}$		5.11e-8	8.17e-8	6.07e-8**	5.97e-8**	6.37e-8**
		(5.01e-8)	(4.25e-8)	(2.27e-8)	(2.30e-8)	(2.27e-8)
$QSeason_{et}$		-0.024	-0.016	-0.013	-0.012	-0.015
		(0.017)	(0.017)	(0.011)	(0.011)	(0.011)
$QTrend_{et}$		-0.008**	-0.003	-0.003	-0.005	-0.002
		(0.003)	(0.002)	(0.009)	(0.009)	(0.009)
QSD_{et}		-3.37e-6	2.44e-6	2.35e-6	2.38e-6	2.49e-6
		(5.26e-6)	(3.75e-6)	(1.83e-6)	(1.76e-6)	(1.82e-6)
Exchange group FE	no	no	yes	yes	yes	yes
Year × month FE	no	yes	yes	yes	yes	yes
R^2	0.031	0.077	0.056	0.399	0.399	0.399
Log-l	-7,899	-7,193	-2,685	3,759	3,743	3,763
N	28,863	28,863	28,851	27,888	27,888	27,888
K-P rk LM				51.696	54.054	58.363
K-P rk LM, p-v.				0.000	0.000	0.000
Hansen J, p-v.				0.718	0.767	0.669

Note: See Table D1 for variable definitions and descriptive statistics. When an IV-estimator is used. the variables $nbida_{et} = 3$ through $nlsGena_{et} \geq 5$ and $QSeason_{et}$ are instrumented, using as excluded instruments their third lags, except for $QSeason_{et}$, $QSeasonIns_{et}$, and $lnQ_{e,t-3}$. In specification 8.5, variables for potential bidders are included instead of corresponding variables for active bidders. Standard errors robust to correlation among products with the same active ingredient are reported in parentheses. Also, see notes for Table 12.

9. Discussion

The purpose of this research has been to develop a method for calculating the probability that observed price patterns are caused by collusion, tacit or outright. We apply the method to the Swedish generics markets and also estimate how changes in the number of bidders and other relevant variables affect the likelihood of collusion and how collusion affects the prices of pharmaceuticals.

To analyze competitive bid behavior in a market setting like ours, we depart from Varian's (1980) model. This model implies that when the cheapest product does not capture the entire market, firms will randomize their prices. As shown by Zona (1986) and Lang and Rosenthal (1991), patterns similar to bid-rotation could also emanate from purely competitive behavior, such as when firms have short-run quantity constraints. Ignoring the possibility of other causes of bid-rotation patterns in the empirical analysis would lead to an overestimation of the prevalence of collusion. We account for this, firms' incentives to set higher prices when they face high demand from returning consumers, and for entry, exit, ties, and the probability of leaving prices unchanged. After that, we calculate the probability during competition of observing different price patterns—e.g., that three firms win every third month during 9–10 months. Thereafter, Bayes' theorem is used to calculate the probability that observed price patterns stem from collusive behavior, using data from pharmaceutical firms bidding to be product-of-the-month in Sweden. The product-of-the-month is the lowest-priced product available across all of Sweden for the whole month, and the product that will be reimbursed according to the Swedish pharmaceuticals insurance program. We investigate bidding in these markets from September 2015 until November 2019 in a total of 28,863 auctions (i.e., the number of pharmaceutical exchange groups × months).

Descriptive statistics (section 4.2, Table 2) indicates that firms prefer to use bid-rotation to collude and this is supported by the empirical results (section 6.2) which show that 97% of the auctions predicted to be part of collusions are part of bid-rotation schemes. This is in contrast with the experimental results of Fonseca and Normann (2012) that parallel bidding was more common than bid-rotation for treatments in which potential colluders were allowed to communicate. Compared to bid-rotation, parallel bidding yields higher profits in markets with price caps and do not require equally high discount rate to be stable (Fonseca and Normann). Therefore, it is not surprising that they found parallel bidding to be the most common form of collusion in treatments with communication. The precise reason why we find bid-rotation to be the most common form of collusion requires further investigation, but it should be noted that while parallel bidding requires firms to agree on a focal price, bid-rotation only requires the firms to agree on the period winner of the auctions. Also, absent verbal communication, it might be easier to clearly demonstrate a willingness to collude using bid-rotation than to initiate parallel bidding.

The descriptive analysis also indicates that while short price patterns consistent with collusion can also be observed in markets with up to eight low-price bidders (Figure 7), longer patterns are highly over-represented in auctions with only two firms bidding to be PM (Figure 6). Short patterns could, for example, be observed if two, by chance, win every other month for a few months when competing. We therefore conduct a formal statistical analysis of the winning patterns to investigate the probability of collusion given the observed patterns and the number of firms. The result from this analysis shows that for bid-rotation schemes lasting 9 months or more, the probabilities that they are at least partly caused by collusion are above 90%. For markets with three or more low-price bidders (n), the probability of winning every n:th month given competition falls faster in the number of months than in the two-firm scenario. As a result, for these the probabilities of collusion exceed 90% already for 7–8-months long rotation patterns. For parallel bidding, the likelihood of two firms winning at the same price given competition is very low, so we find that the probability of collusion given parallel biding is high, including for parallel-bid patterns lasting only 5–6 months.

Having estimated the probability for collusion for a large number of markets and months enables us to empirically estimate how the number of bidders affects the risk of collusion. This is important because the existing knowledge on this effect primarily stems from classroom experiments. We confirm the qualitative predictions from theoretical and experimental research that the number of bidders has a significant negative effect on the probability of collusion. Specifically, we find that increasing the number of bidders from two to four reduces the probability of collusion by one half. The probability is further reduced when more of the new bidders sell generics, which can be explained by the vertical separation in the markets. Still, also in markets with seven bidders of which five are low-price bidders we observe price patterns lasting over nine months (Figure 6), and the probability that such patterns are caused by collusion is estimated to 99%. This demonstrates that Huck et al.'s (2004) findings that four bidders are sufficient to avoid collusion does not necessarily hold for professional price setters in the field. Our results are more in line with Selten's (1973) theoretical prediction that four bidders are sufficiently few for collusion to take place while six are unlike to collude, although this is qualified by the suggestion that, to avoid collusion, the six bidders must belong to the same price segment in narrowly defined markets.

There is also the question of how harmful collusion is for the consumers and society at large. As such, we also estimate how pharmaceutical prices are affected by collusion. The results of the preferred specification indicate that an increase in the probability of collusion from zero to one raises the average prices by 65% and simulations suggest that the yearly overcharge by colluding firms was 694 million SEK (68 million USD). The large effect on prices can be understood in the light of that the demand elasticity at market levels is low for pharmaceuticals, but this, in turn, implies that each percentage increase in prices only causes small welfare costs by reducing consumption below optimal levels.

Estimates of overcharges and effects of market characteristics on the risk for collusion are needed to take informed decisions on how to best reform markets to reduce the prevalence of collusions. However, to provide specific policy suggestions one also needs estimates of the costs of possible policies, which is beyond the scope of this paper, and to take a stand on if each dollar in revenue for colluding firms should be valued less—and if so, how much less—than each dollar saved by consumers and their benefit schemes. Still, we want to mention that the high overcharges and large effect of number of bidders on the risk for collusion provide a much stronger case for lowering entry and annual fees for bidders than would be the case in absence of collusion. These results are also an argument for considering the effect on the risk of collusion in merger decision also when the number of firms in the relevant price segment of the market would be three to five post-merger. Davies et al. (2011) report that the European Commission's interventions in mergers due to the risk tacit collusion have almost always been confined to markets with only two firms post-merger.

Second, theory shows that frequent interaction facilitates collusion, and we find it likely that the high prevalence of collusion in our data is, at least in part, due to the short time needed for price reactions. The results regarding the number of bidders can also be suggestive for the effect of this time, because both the number of bidders and the time before a deviator can be punished affects how high the probability-adjusted discount rate must be for an efficient collusion to be stable. With grim trigger strategies, the monthly probability-adjusted discount rate must weakly exceed 0.75 with four bidders and monthly interactions, while it must exceed 0.70 and 0.79 with two bidders and two- and three-months long periods for price reactions, respectively. If instead, the probability of restoring the collusion is 50% after one period of punishment, the monthly discount rate must exceed 0.83 with four bidders and monthly interactions, while it must exceed 0.81 and 0.85 with two bidders and three- and four-months long periods for price reactions, respectively. Therefore, it is perceivable that increasing the length of price periods to three months would reduce the stability of bid-rotation collusions about as much as increasing the number of bidders from 2 to 4. This is an argument for increasing the length of the price periods from the current length to, for example, three months, which would increase the time from defection to punishment in a collusion threefold. Longer periods could also be considered, but, at some point, this must be combined with increasing the time between auctions and sales so that firms can place orders and make production decisions when they know if they have won the auction. Otherwise, long price periods could cause some firms to refrain from bidding, thus reducing competition and inducing higher prices.

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Appendix A. Duration of possible collusive patterns

In this appendix we describe the duration of patterns consisting with bid-rotation and parallel bidding using histograms and numbers in the text based on all the available data from March 2010–January 2021. We use all the data instead of for only those in the sample we use for calculation of probabilities and estimations (November 2014–September 2020), to reduce the truncations of pattern. This increases the number of firm × exchange group × month observations from 274,147 to 425,943, and the number of exchange groups from 1,302 to 1,515.

We report the number of patterns by their exact duration, and not the number of exchange group by month observations that are part of a pattern as in Table 2 and the following sections in the main text. The histograms only display durations exceeding seven months because including the high frequencies of shorter patterns would make it more difficult to see the differences among the longer patterns.

Figure 11 shows the length of bid-rotations between two firms, demonstrating that there are 405 bid-rotations lasting 8 to 12 months and 280 bid-rotations exceeding one year. In addition, 2 firms win every other month for 3 months in 1,643 cases and for 4, 5, 6, and 7 months in 673, 648, 310, and 190 cases, respectively. The rapid drop in frequencies as the length increases is partly a consequence of the fact that more bid-rotation sequences can be observed in exchange groups with short bid-rotation sequences. Data not shown in figures reveal that the longest bid-rotation between two firms is 8 to 12 months for 215 out of the 1,515 exchange groups and that nearly as many (205) have a bid-rotation exceeding one year. The observed frequencies are also affected by the fact that patterns are truncated by the study period and by re-groupings of exchange groups and, of course, that some collusions ceased, for example, because of the entry of new firms, and that in particular some short patterns occurred by chance during competition.

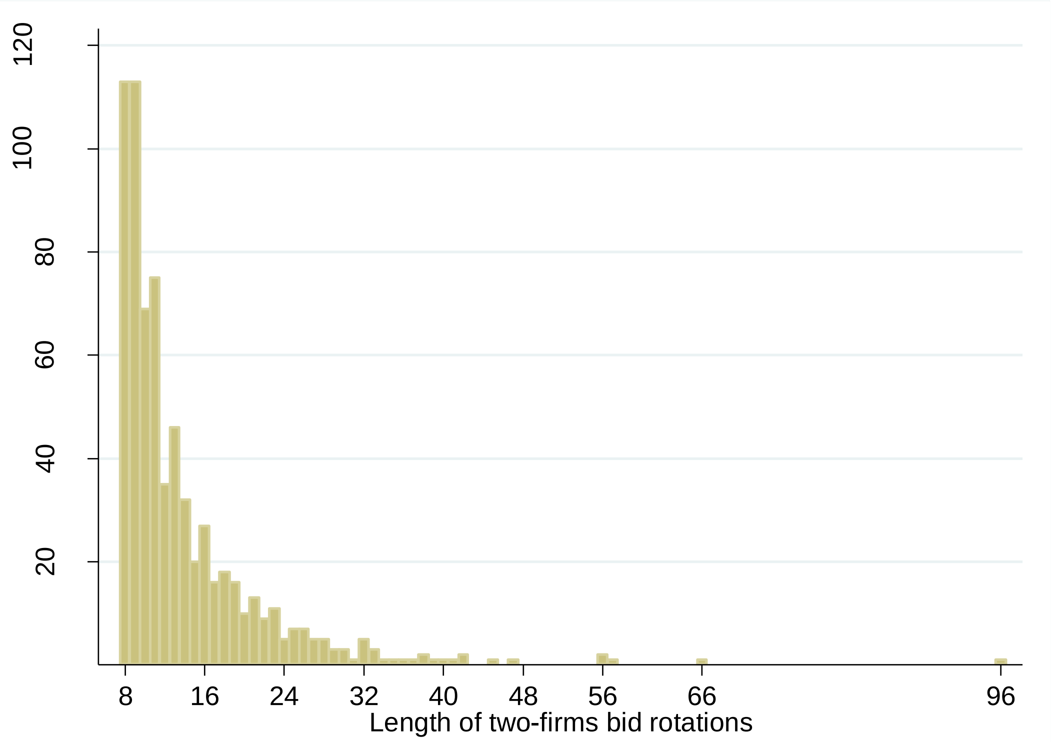

Figure 11. Number of sequences with two firms winning every other month for 8 months or more. The durations are truncated by the study period and by re-groupings of exchange groups.

Figure 12 shows that there are 222 bid-rotations between three firms that last 8 to 12 months and 70 such bid-rotations exceeding one year. In addition, three firms win every third month for 3 months in 2,717 cases and for 4, 5, 6, and 7 months in 2,640, 1,009, 286, and 159 cases, respectively. Data not shown in figures reveal that the longest bid-rotation between three firms is 8 to 12 months for 132 exchange groups, while 58 have a bid-rotation between three firms exceeding one year.

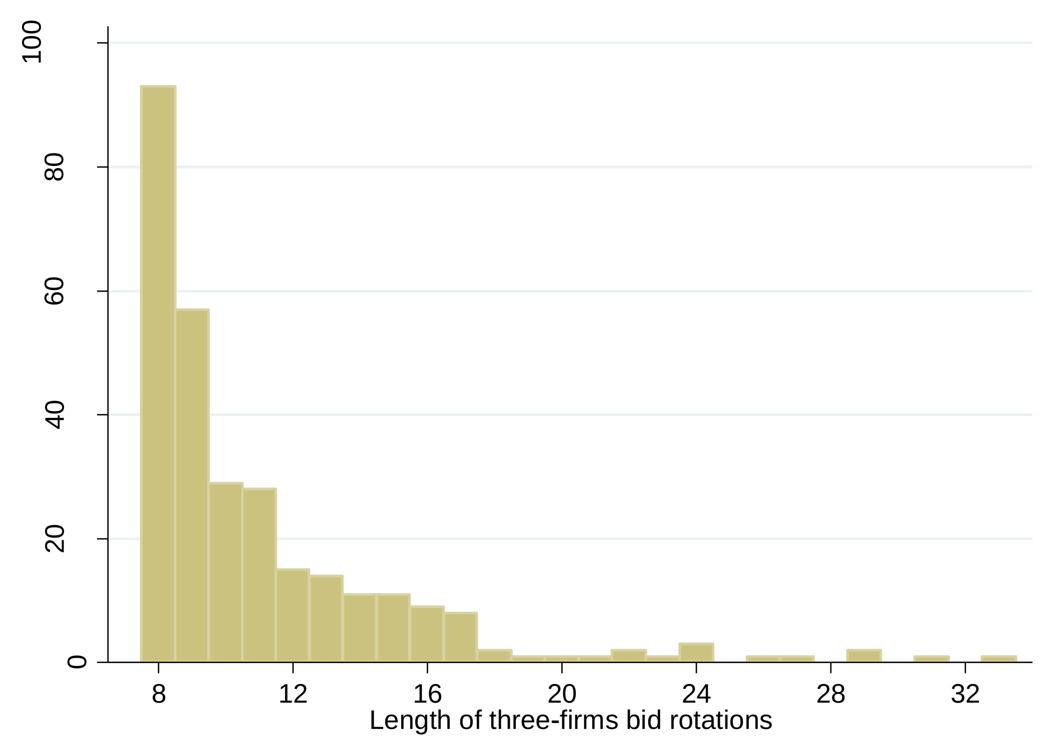

Figure 12. Number of sequences with three firms winning every third month for 8 months or more. The durations are truncated by the study period and by re-groupings of exchange groups.

Figure 13 shows that there are 39 bid-rotations between four firms lasting 8 months or more. In addition, four firms win every fourth month for 4 months in 1,440 cases and for 5, 6, and 7 months in 668, 209, and 83 cases, respectively. Data not shown in figures reveal that the longest bid-rotation between four firms is 8 to 12 months for 33 exchange groups while 3 have a bid-rotation between four firms exceeding one year.

In addition, there are 508, 407, 38, 7, and 1 sequences when five firms win every fifth month for 5, 6, 7, 8, and 10 months, respectively. As mentioned earlier, there is no sequence of six or more firms taking turns to win for six months or more. Note, however, that patters consistent with five firms winning every fifth month during five months, are also consistent with six firms winning every sixth month during 5 months. Therefore, it is possible that some firms believed that they were part of a six-firm rotation, but that the collusion ceased before the last firm became the winner.

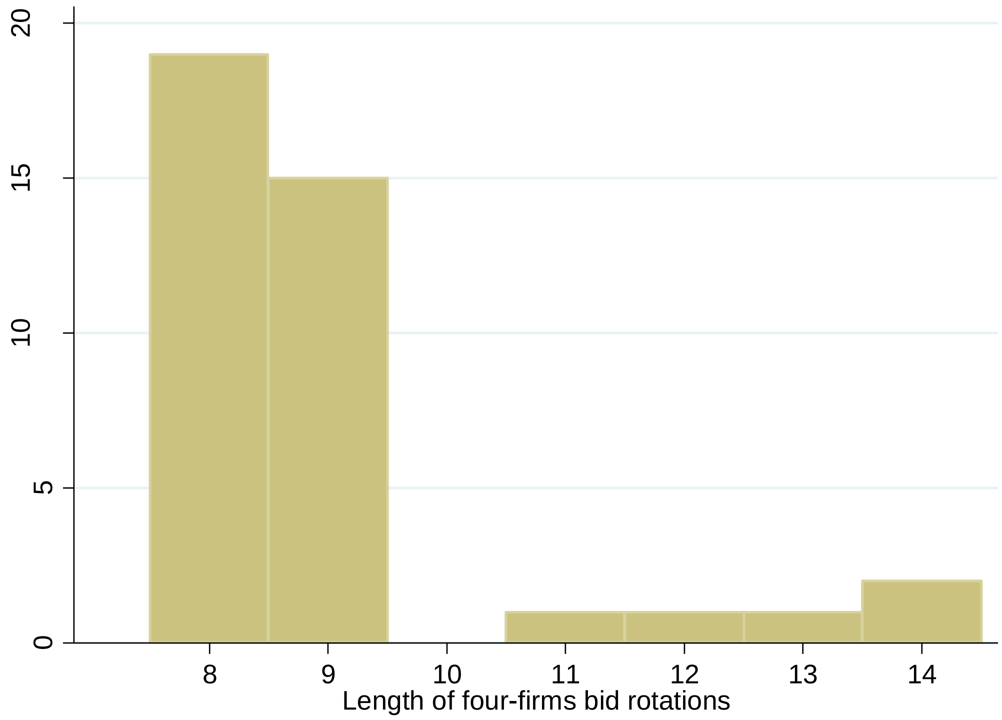

Figure13. Number of sequences with four firms winning every fourth month for 8 months or more. The durations are truncated by the study period and by the re-groupings of exchange groups.

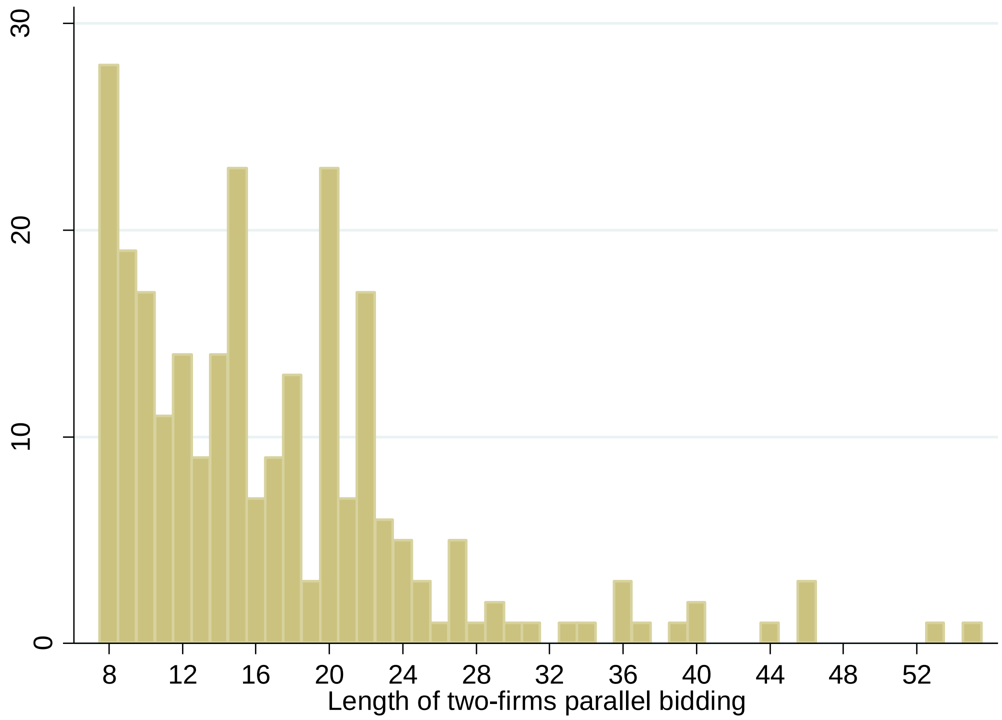

Figure 14. Number of sequences with two firms both winning every month for 8 months or more, and no additional firm winning during these months. The durations are truncated by the study period and by the re-groupings of exchange groups.

Figure 14 shows that there are 89 parallel bidding sequences between two firms that lasted 8–12 months and 165 that exceeded one year. In addition, two firms won every month for 3 months in 83 cases and for 4, 5, 6, and 7 months in 76, 51, 46, and 44 cases, respectively. Data not shown in figures reveal that the longest parallel bidding between two firms is 8 to 12 months for 38 out of the 1,515 exchange groups, while 107 have a parallel bidding between two firms exceeding one year.

It is far less likely to see parallel bidding between three firms. There are 5 sequences when three firms were split winners for 3 months, and 8, 1, 2, and 1 when three firms were split winners for 4, 5, 6, and 7 months, respectively, and there was no case of longer parallel bidding between three firms. None of these cases are for any of the 28,863 exchange group by month observations with at least two low-price bidders for all month from t-10 to t+10 for which we calculate the probability of collusion. Also note that there are no cases of four or more firms being split winners for three months or more.

Appendix B. Calculating probabilities of observing bid-rotation and parallel winning patterns during competition

The formula for calculating $P(b\_m34_{et}|K, \boldsymbol{n}_{et})$ (the probability of observing bid-rotation among two or more firms for 3–4 months during competition and conditioned on $\boldsymbol{n}_{et}$), is written as follows:

$$P(b\_m34_{et}|K,\boldsymbol{n}_{et}) = 1 - \prod_{\mathrm{T=t}}^{2}\left(1 - \sum_{F=2}^{3} PbFm3_{eT}\right) - \left[1 - \prod_{\mathrm{T=t}}^{4}\left(1 - \sum_{F=2}^{5} PbFm5_{eT}\right)\right] \qquad \text{(B.1)},$$

$$= -\prod_{\mathrm{T=t}}^{2}\left(1 - \sum_{F=2}^{3} PbFm3_{eT}\right) + \prod_{\mathrm{T=t}}^{4}\left(1 - \sum_{F=2}^{5} PbFm5_{eT}\right)$$

where $PbFm3_{eT}$ and $NoFm5_{eT}$ for F = 2, 3, 4, 5 denotes the probabilities that there are bid-rotations involving F firms lasting at least three and five months, respectively. Hence, the equation gives the probability that there is one bid-rotation pattern involving exchange group e month t that lasts 3–4 months. Note that including $PbFm3_{eT}$ for F = 4, 5 would not change the value of $P(b\_m34_{et}|K,\boldsymbol{n}_{et})$ because at most three firm can be winners during the first three months of a bid-rotation. The entries in equation (B.1) are defined as follows:

$$Pb2m3_{eT} = PS1_{e,\mathrm{T}-2}PS2_{e,T-1}C2_{eT} \qquad \text{(B.2)},$$

$$Pb3m3_{eT} = PS1_{e,T-2}PS2_{e,T-1}PS3_{eT} \qquad \text{(B.3)},$$

$$Pb2m5_{eT} = PS1_{e,T-4}PS2_{e,T-3}\prod_{m=T-2}^{T} C2_{em} \qquad \text{(B.4)},$$

$$Pb3m5_{eT} = PS1_{e,T-4}PS2_{e,T-3}PS3_{e,T-2}\prod_{m=T-1}^{T} C3_{em} \qquad \text{(B.5)},$$

$$Pb4m5_{eT} = PS1_{e,T-4}PS2_{e,T-3}PS3_{e,T-2}PS4_{e,T-1}C4_{eT} \qquad \text{(B.6)},$$

and

$$Pb5m5_{eT} = PS1_{e,T-4}PS2_{e,T-3}PS3_{e,T-2}PS4_{e,T-1}PS5_{eT} \qquad \text{(B.7)},$$

where $PS1_{et}$, $PS2_{et}$, $C1_{et}$, and $C2_{et}$ are defined in section 6.1 and

$$PS3_{et} = max\{0, (1 - PT_{et} - C1_{et} - C2_{et})\} = max\{0, (PS2_{et} - C2_{et})\} \quad \text{(B.8)},$$

$$PS4_{et} = max\{0, (PS3_{et} - C3_{et})\} \quad \text{(B.9)},$$

$$PS5_{et} = max\{0, (PS4_{et} - C4_{et})\} \quad \text{(B.10)},$$

$$C3_{et} = (1 - S1_{et}U^{n_{et}}E^{n_{et}})S3_{et}A3_{et} \quad \text{(B.11)},$$

and

$$C4_{et} = (1 - S1_{et}U^{n_{et}}E^{n_{et}})S4_{et}A4_{et} \quad \text{(B.12)}.$$

Note that we also include $Pb4m5_{eT}$ and $Pb5m5_{eT}$ when calculating $P(b\_m34|K, \boldsymbol{n}_{et})$. That is, we include the probability of no bid-rotation involving four or five firms, even though these cannot be identified solely by looking on three months. The reason for this is that we have classified the empirical bidding patterns based on their maximum duration, meaning that if the winner from month *t – 1* and onwards are the following firms *a a b c d a e*, it is classified as a four-firm rotation from *t* to *t + 4*, and not as a three-firm rotation.

The formulas for calculating the probability of observing bid-rotation among two or more firms for 5–6, 7–8, and 9–10 months, are written as follows:

$$P(b\_m56|K, \boldsymbol{n}_{et}) = -\prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm5_{eT}\right) + \prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm7_{eT}\right) \quad \text{(B.13)},$$

$$P(b\_m78|K, \boldsymbol{n}_{et}) = -\prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm7_{eT}\right) + \prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm9_{eT}\right) \quad \text{(B.14)},$$

and

$$P(b\_m910|K, \boldsymbol{n}_{et}) = -\prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm9_{eT}\right) + \prod_{\mathrm{F}=2}^{5}\left(1 - \sum_{F=2}^{3} PbFm11_{eT}\right) \quad \text{(B.15)},$$

where $PbFm5_{eT}$ for F = 2, 3, 4, 5 is defined in equations (B.4)–(B.7) and

$$Pb2m7_{eT} = PS1_{e,T-6}PS2_{e,T-5}\prod_{m=T-4}^{T} C2_{em} \quad \text{(B.16)},$$

$$Pb3m7_{eT} = PS1_{e,T-6}PS2_{e,T-5}PS3_{e,T-4}\prod_{m=T-3}^{T} C3_{em} \quad \text{(B.17)},$$

$$Pb4m7_{eT} = PS1_{e,T-6}PS2_{e,T-5}PS3_{e,T-4}PS4_{e,T-3}\prod_{m=T-2}^{T} C4_{em} \quad \text{(B.18)},$$

$$Pb5m7_{eT} = PS1_{e,T-6}PS2_{e,T-5}PS3_{e,T-4}PS4_{e,T-3}PS5_{e,T-2}\prod_{m=T-1}^{T} C5_{em} \quad \text{(B.19)},$$

$$Pb2m9_{eT} = PS1_{e,T-8}PS2_{e,T-7}\prod_{m=T-6}^{T} C2_{em} \quad \text{(B.20)},$$

$$Pb3m9_{eT} = PS1_{e,T-8}PS2_{e,T-7}PS3_{e,T-6}\prod_{m=T-5}^{T} C3_{em} \quad \text{(B.21)},$$

$$Pb4m9_{eT} = PS1_{e,T-8}PS2_{e,T-7}PS3_{e,T-6}PS4_{e,T-5} \prod_{m=T-4}^{T} C4_{em} \quad \text{(B.22)},$$

$$Pb5m9_{eT} = PS1_{e,T-8}PS2_{e,T-7}PS3_{e,T-6}PS4_{e,T-5}PS5_{e,T-4} \prod_{m=T-3}^{T} C5_{em} \quad \text{(B.23)},$$

$$Pb2m11_{eT} = PS1_{e,T-10}PS2_{e,T-9} \prod_{m=T-8}^{T} C2_{em} \quad \text{(B.24)},$$

$$Pb3m11_{eT} = PS1_{e,T-10}PS2_{e,T-9}PS3_{e,T-8} \prod_{m=T-7}^{T} C3_{em} \quad \text{(B.25)},$$

$$Pb4m11_{eT} = PS1_{e,T-10}PS2_{e,T-9}PS3_{e,T-8}PS4_{e,T-7} \prod_{m=T-6}^{T} C4_{em} \quad \text{(B.26)},$$

and

$$Pb5m11_{eT} = PS1_{e,T-10}PS2_{e,T-9}PS3_{e,T-8}PS4_{e,T-7}PS5_{e,T-6} \prod_{m=T-5}^{T} C5_{em} \quad \text{(B.27)}.$$

As shown in Table 2, we do not observe any bid-rotation involving more than five firms. Still, the possibility of observing bid-rotation involving more than five firms during competition is strictly positive, and the definitions above imply that this probability is included in $P(b\_m56|K, \boldsymbol{n}_{et})$.

For parallel bidding, the probabilities are calculated as follows:

$$P(p_{m34}|K, \boldsymbol{n}_{et}) = -\prod_{T=t}^{t+2}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-2} \prod_{m=T-1}^{T} \mathrm{CF}T_{em}\right) + \prod_{T=t}^{t+4}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-4} \prod_{m=T-3}^{T} \mathrm{CF}T_{em}\right) \quad \text{(B.28)},$$

$$P(p\_m56|K, \boldsymbol{n}_{et}) = -\prod_{T=t}^{t+4}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-4} \prod_{m=T-3}^{T} \mathrm{CF}T_{em}\right) + \prod_{T=t}^{t+6}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-6} \prod_{m=T-5}^{T} \mathrm{CF}T_{em}\right) \quad \text{(B.29)},$$

$$P(p\_m78|K, \boldsymbol{n}_{et}) = -\prod_{T=t}^{t+6}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-6} \prod_{m=T-5}^{T} \mathrm{CF}T_{em}\right) + \prod_{T=t}^{t+8}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-8} \prod_{m=T-7}^{T} \mathrm{CF}T_{em}\right) \quad \text{(B.30)},$$

$$P(p\_m910|K, \boldsymbol{n}_{et}) = -\prod_{T=t}^{t+8}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-8} \prod_{m=T-7}^{T} \mathrm{CF}T_{em}\right) + \prod_{T=t}^{t+10}\left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-10} \prod_{m=T-9}^{T} \mathrm{CF}T_{em}\right) \quad \text{(B.31)},$$

and

$$P(p\_m11|K, \boldsymbol{n}_{et}) = 1 - \prod_{T=t}^{t+10} \left(1 - \sum_{F=2}^{3} \mathrm{PF}T_{e,T-10} \prod_{m=T-9}^{T} \mathrm{CF}T_{em}\right) \quad \text{(B.32).}$$

Here, PFT_{et} for F = 2 and 3 equals the probability of a tie between two and three or more firms, respectively, during competition. Based on binomial probability theory, these are defined using the parameter *x* described in section 5.2, as follows

$$P2T_{et} = \frac{(n_{et}-1)!}{(n_{et}-2)!}(1-x)^1 x^{n_{et}-2}, \quad \text{(B.33),}$$

$$P3T_{et} = 1 - P2T_{et} - x^{n_{et}-1}. \quad \text{(B.34),}$$

The other entries in equations (B.28)–(B.32) are defined as follows:

$$C2T_{em} = S1_{et}^2(1 - P3AfterTie_{em})[U^2 + (1-U)^2 P2T_{em}] \quad \text{(B.34),}$$

$$C3T_{em} = S1_{et}^3(1 - P4AfterTie_{em})[U^3 + (1-U)^{23} P3T_{em}] \quad \text{(B.35),}$$

$$P3AfterTie_{em} = P3T_{em} + PS1_{et}\frac{n_{em} - a1_{et} - 1}{n_{em}} \quad \text{(B.36),}$$

and

$$P4AfterTie_{em} = max\left\{0, \left(PS1_{et}\frac{n_{em} - a1_{et} - 2}{n_{em}}\right)\right\} \quad \text{(B.37).}$$

The term $C2T_{em}$ is used to capture the probability during competition that there is a tie between the same two firms as in the previous month. This will happen if both winners continue selling a low-price product, $S1_{et}^2$, no other firm sets a weakly lower price, ($1 - P3AfterTie_{em}$), and the firms either both leave their prices unchanged, U^2, or both change their prices by an equal amount, $(1-U)^2 P2T_{em}$. Note that if one, but not both, of the winners in month *t* changes its price, which occurs with probability *2U(1-U)*, the tie between the two firms cannot prevail. The term $C3T_{em}$ has a corresponding explanation.

The probability that a third firm sets a weakly lower price after a tie between two firms, $P3AfterTie_{em}$ (eq. B.37), of course, depends on the probability that a third firm sets a price that is equal to the price of the two previous winners, $P3T_{em}$, and the probability that it sets a lower price. Without autocorrelation in winning probabilities, the latter would have equaled $PS1_{et}(n_{em}-2)/n_{em}$. Instead, we set it as being equal to $PS1_{et}(n_{em} - a1_{et} - 1)/n_{em}$, where $a1_{et}$ (defined in section 5.3) describes how the probability of being a single winner is affected by having won in the previous month. The adjustment is consistent with our assumption in section 5.3 that the effect on future winning probabilities of being one of two winners is half the effect of being the single winner.[55]

The definition of $P4AfterTie_{em}$ (eq. B.38) differs from that of $P3AfterTie_{em}$ (eq. B.37) in some key respects. First, it does not include a term, corresponding to $P3T_{em}$, for an additional firm setting the same price as the previous winners. The reason for this is that a tie between four or more firms should

[55] Recall that this effect works through the increase in sales when being the PM and that this increase in sales is divided by the number of products that are simultaneously the PM.

be extremely rare according to the model laid out in section 5.2 and is never observed in the data (Figure 3), so we defined $P3T_{em}$ in such a way as to include ties between three or more firms. Second, we adjust the probability of a fourth firm setting a lower price by $(n_{em} - a1_{et} - 2)/n_{em}$, because the effect of being a winner in the previous month should only be a third as strong when a firm was one of three previous winners compared to if it was the sole winner, as the effects of being a previous winner goes through quantities sold. Third, we specify that $P4AfterTie_{em}$ is never strictly negative, which would otherwise have been the case for n_{em}= 2. We did not have to specify this for $P3AfterTie_{em}$ as $(n_{em} - a1_{et} - 1)$ is always positive because we have restricted the sample to observations where $n_{em} \geq 2$ and $a1_{et}$ is always below one.

Appendix C. Relevance and validity of instruments

In sections 7 and 8, we instrument the number of bidders in month *t* with lagged values of these variables and $lnQ_{e,t-3}$. Lagged values of the number of bidders are expected to be relevant instruments because entry- and fixed costs increase the likelihood that bidders that are already active in a market will continue to be active; indeed, assuming that firms are rational profit maximizers, previous bidders will remain in a market if their revenue exceeds their variable costs, but new bidders will only enter if the present value of their expected revenue is expected to exceed the present value of all their costs. In the markets we study, the cost of firms consists of the entry cost (which includes the cost of preparing the application to the Swedish Medical Products Agency (SMPA) or by the European Medicines Agency (EMA) and the agency's fee for handling the application), the fixed yearly cost (including a small yearly fee to the SMPA), and the variable cost. The values of the Kleibergen-Paap rk LM statistic confirm that the instruments are strong for all specifications; hence, we do not discuss the relevance and strengths of the instruments any further.

The Hansen J test demonstrates that for none of the specifications can we reject the null hypothesis that the instruments are valid, but, since this in itself does not prove that the instruments are indeed valid, we describe in more detail below why we think this is the case. A requirement for the instruments to be valid is that they are uncorrelated with the error term ε_{et}. However, if firms know ε_{et} when they make entry and exit decisions that affect $lnnbida_{e,t-1}$ and $lnna_{e,t-1}$, these instruments (which are used in section 7) will likely be invalid. As bids must be submitted two months in advance, the values of $nbid_{e,t-1}$ and $n_{e,t-1}$ are affected by information in *t – 3*. Also recall that bids are made public nearly a month before they become effective, so when submitting bids in *t – 3*, firms know competitors' prices for month *t – 2*, and even though they do not yet know the exact market shares for this month, this means that they have at least some information about $\varepsilon_{e,t-2}$.

Thus, in section 7, where $lnnbida_{e,t-1}$ and $lnna_{e,t-1}$ are used as instruments, the relevant question is whether firms in *t – 3*, knowing $\varepsilon_{e,t-3}$ and other variables and having some information about $\varepsilon_{e,t-2}$, can predict the value of ε_{et}. If we had estimated a static model, the answer would likely be yes, because there is some persistence in prices. For example, the originator seldom changes its price because if it has the highest price in the exchange group and reduces its price, the highest permissible price within the benefit scheme can be lowered accordingly. However, we estimate dynamic models including four lags of the dependent variable in section 7 and one lag in section 8. Therefore, in section 7, ε_{et} could be described as unexplained price shocks in month *t* (e.g., those caused by low-price bidders during competition randomly draw their prices from a distribution). Similarly, in section 8, the error term should reflect changes in the probability of collusion.

Still, a potential problem for the price regressions is that serially correlated error terms could enable firms to forecast ε_{et} based on $\varepsilon_{e,t-3}$ and their information about $\varepsilon_{e,t-2}$ because this could make lagged values of $lnnbida_{et}$ and $lnna_{et}$ endogenous. Autocorrelation in the error term could also bias the estimators for the lags of the dependent variables. Therefore, we tested for serial correlation up to four months by using the test proposed by Cumby and Huizinga (1992), as implemented in STATA by Baum and Schaffer (2013). Cumby and Huizinga showed how a consistent estimate of the covariance matrix can be used to test for serial correlation in the true regression error in models in which some regressors (e.g., the lag of the dependent variable) are only weakly exogenous. Using predicted values of $lnnbida_{et}$ and $lnna_{et}$ to resemble the IV estimation, we cannot at the 5% level reject any of the null hypotheses of no autocorrelation of order one to four for the price specifications.

For the collusion regressions, we can reject the null hypotheses of no autocorrelation of order one or two using the test proposed by Cumby and Huizinga (1992), but we find no evidence of autocorrelation of order three or four. To establish a sense of the size of the autocorrelation, we also estimate a specification accounting for first- and second-order serial correlation using generalized least squares (GLS), which suggests that the estimated correlation between ε_{et} and $\varepsilon_{e,t-1}$ is 0.39, while it is 0.25 for ε_{et} and $\varepsilon_{e,t-2}$. These correlations are likely exaggerated because the generalized linear estimator of the parameter for the lagged dependent variable have a bias of –0.19 when its true parameter is 0.5 and the explanatory variable is trending like the US's real GDP (Maeshiro, 1980).[56] Consistent with a negative bias for the GLS estimator, we estimate the coefficient for the lagged dependent variable to 0.27 using GLS compared to 0.60 using OLS or 2SLS. Also, the estimated first-order autocorrelation is close to the value where OLS with a lagged dependent variable will be unbiased according to Maeshiro.

Furthermore, when using the predicted values of the endogenous variables to resemble the IV estimation, we can for the collusion regressions reject the null hypotheses of no autocorrelation of order one or two and estimate the correlation between ε_{et} and $\varepsilon_{e,t-1}$ to 0.39 and between ε_{et} and $\varepsilon_{e,t-2}$ to 0.24. Together, these correlations imply that the second-order autocorrelation coefficient is 0.09 ($\approx 0.24 - 0.39^2$), which in turn implies that the correlation between the error term in $t-5$, when the entry and exit decisions determining the three months lags of endogenous variables are taken, and ε_{et} is 0.04. That is, according to these estimates, three-month lags should be nearly exogenous, which is consistent with the fact that the null hypotheses of exogenous instruments cannot be rejected by the Hansen J test.[57] Therefore, we used three-month lags as instruments when regressing the probability of collusion. In Appendix E, we report results for specification 8.4 when one-month or six-month lags of the endogenous variables are used as instruments instead.

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[56] Of course, the bias in our case will differ from –0.19 as we include several explanatory variables that are quite persistent and the true parameter for the lagged dependent variable likely differ from 0.5, but the sign of the bias should still be negative.

[57] Unless correlations arise by coincidence, the correlations between the lagged endogenous variables we use as instruments and ε_{et} should only be a fraction of 0.04 because the correlation between these instruments and $\varepsilon_{e,t-5}$ is less than one and, as discussed above, because the correlation between ε_{et} and $\varepsilon_{e,t-5}$ is likely to be less than the estimated 0.04. Berkowitz et al. (2008) simulated samples, with size 100, where the endogenous variables had no effect on the dependent variable. Using instruments with an absolute correlation with the error term of 0.10 and 0.06, they rejected the null hypothesis of no effect on the 5% significance level in 16.85% and 8.85% of the simulation. Thus, a reduction in the correlation by 40% reduced the over rejection compared to the significance level by two-thirds, from 11.85 to 3.85.

Appendix D. Variable list

Table D1. Definitions, descriptive statistics for variables, and parameter values used in the main analyses

Variable/ parameter	Definition (and comments)	Mean (std.dev.)	Min (max)
Section 4	Descriptive statics for 274,147 firm × exchange group × month observations		
Active bidder$_{et}$	A firm that has placed a bid for month *t* and sold at least on package from $t – 1$ through $t + 1$.		
Active low-price bidder$_{et}$	An active bidder$_{et}$ that in exchange *e*: a) sometime from $t – 2$ to $t + 2$ has sold a PM, or b) in month *t* marketed a product declared to be available that had price that was equal or below the price of the PM the last or the second last month.		
Potential bidder$_{et}$	A firm that some time from month $t – 2$ to $t + 2$ has placed a bid and sold at least one unit in exchange *e*.		
Potential low-price bidder$_{et}$	A potential bidder$_{et}$ that in exchange *e*: (a) some time from $t – 2$ to $t + 2$ has sold a PM, or (b) in month *t* marketed a product declared to be available that had a price that was equal to or below the price of the PM in the last or second-to-last month.		
Generics$_{fet}$	Dummy taking the value one for firms that only sell locally sourced generics in exchange group *e* month *t*.	0.776 (0.417)	0 (1)
$nbid_{et}$	Number of potential bidders (see section 4) in exchange group *e* month *t*. $nbidp_{et} \equiv np_{et} + Np_{et}$, where the latter are defined in section 5.	4.795 (1.924)	2 (14)
$nbida_{et}$	Number of active bidders in exchange group *e* month *t*. $nbida_{et} \equiv na_{et} + Na_{et}$, where the latter are defined in section 5.	4.691 (1.873)	1 (13)
Originators$_{fet}$	Dummy taking the value one for firms that sell originators in exchange group *e* month *t*.	0.139 (0.346)	0 (1)
Parallel imp.$_{fet}$	Dummy taking the value one for firms that sell parallel imports in exchange group *e* month *t*.	0.085 (0.279)	0 (1)
Winning patterns	A firm is considered a winner if it sells a PM. See section 4.2 for details regarding the definition of winning patterns.	See des.stat. in Table 2.	
b2m34	Two firms win every other month for 3–4 months.		
b2m56, …78, …910	Two firms win every other month for 5–6, 7–8, and 9–10 months, respectively.		
b2m11	Two firms win every other month for at least 11 months.		
b3…; b4…;b5…	Three firms win every third, four every fourth, and five every fifth, respectively.		
b5m9	Five firms win every fifth month for at least 9 months.		
p2m34; …; p2m910	Two firms both win every month for 3–4 months, 5–6, 7–8, and 9–10 months, respectively.		
p2m911	Two firms both win every month for at least 11 months.		

Section 5	Descriptive statistics for 28,863 exchange group × month observations, except for $DiffHarv_{fet}$ and $PrevPM_{fet}$ are for 24,395 and $PM\_22_{fe,t-l}$–$PM\_44_{fe,t-l}$ are for 90,111 firm × exchange group × month observations			
Al_{et}	Conditioned on competition, the probability of being a single winner, conditional on selling a low-price product, for a firm that last won l months ago. $Al_{et} = al_{et}(1 - PT_{et})/n_{et}$.	See next five rows		
$A1_{et}$	See Al_{et}.	0.131 (0.103)	0.011 (0.310)	
$A2_{et}$	See Al_{et}.	0.330 (0.145)	0.059 (0.563)	
$A3_{et}$	See Al_{et}.	0.394 (0.152)	0.083 (0.635)	
$A4_{et}$	See Al_{et}.	0.414 (0.156)	0.092 (0.662)	
$A5_{et}$	See Al_{et}.	0.426 (0.159)	0.097 (0.680)	
al_{et}	Conditioned on competition, the relative probability of selling a PM for a seller that sold a PM *l* months ago, and having sold no PM between this month and month *t*.	See Table 5		
$DiffHarv_{fet}$	$= Harv_{fet} - \overline{Harv_{et}}$, where $\overline{Harv_{et}}$ is the mean of $Harv_{fet}$ for low-price bidders in exchange group *e* month *t* and $Harv_{fet} = \sum_{\mathrm{m=t-1}}^{t-6} Share_e^m \, PM_{fem}/nPM_{em}$, where $Share_e^m$ equals the share that made a filling $t - m$ months before their current filling in the exchange group. A proxy of increased demand caused by consumers that have previously bought a product make another filling.	0.000 (0.172)	-0.500 (0.800)	
E	The probability that a low-price bidder month *t + 1* was this also month *t*.	0.9366		
$n \equiv n_{em}$ (n_{et})	Number of potential bidders in the low-price segment in exchange group *e* month *m* (month *t*).	3.199 (0.926)	2	(8)
$\breve{n}_{et}$	n_{et} truncated so that $\breve{n}_{et} = 6$ for $n_{et} \geq 6$.	3.198 (0.921)	2	(6)
N_{em} (N_{et})	Number of all potential bidders in the high-price segments in exchange group *e* month *m* (month *t*).	1.596 (1.520)	0	(10)
P_0	The highest permissible price allowed within the benefit scheme.			
P_{min}	The lowest price in the interval firms randomize over.			
PM_{fet}	Dummy taking the value one if firm *f*'s product in exchange group *e* is a product-of-the-month (PM) month *t*. A product is a PM if it has the lowest price per unit (e.g., pill) in the exchange group and is guaranteed to meet the demand.	0.371 (0.483)	0	(1)
P_P	Measure of the width of the bid interval.	12.531		
PM_22_{fet}	Dummy that takes the value one if $PM_{fe,t-2} = 1$ and $n_{fe,t-2} = 1$.	0.092 (0.289)	0	(1)
PM_33_{fet}	Dummy that takes the value one if $PM_{fe,t-3} = 1$ and $n_{fe,t-3} = 1$.	0.103 (0.305)	0	(1)
PM_44_{fet}	Dummy that takes the value one if $PM_{fe,t-4} = 1$ and $n_{fe,t-4} = 1$.	0.050 (0.217)	0	(1)
$PrevPM_{fet}$	$\sum_{m=t-6}^{t-1} \delta^{t-m} \left(PM_{fem}/nPM_{em} - 1/n_{em}\right)$, where $\delta \in (0,1)$ is estimated. A proxy of variation in stock levels caused by deviations from average sales.	For -0.015 (0.146)	δ=0.298 -0.424 (0.339)	

PT_{et}	Conditioned on competition, the probability of a tie, that is, several firms selling PM in exchange *e* month *t*.	0.176 (0.062)	0.093 (0.448)
S_w	Market share of the winner, i.e., the firm selling the PM.	0.794	
S_H	The joint market share of all high-price bidders.	0.095	
$Sl_{et}\ for\ l =$ 1,2,3,4,5	The probability of a firm selling a low-price product in exchange group *e* month *t* for those with $PM_{fe,t-l} = 1$ and with no product being PM between *t* – *l* and *t*. Values vary by $\breve{n}_{e,t-l}$.	See below	
$S1_{et}$	See $Sl_{et}\ for\ l = 1, 2, 3, 4, 5$.	0.962 (0.013)	0.939 (0.980)
$S2_{et}$	See $Sl_{et}\ for\ l = 1, 2, 3, 4, 5$.	0.916 (0.037)	0.851 (0.957)
$S3_{et}$	See $Sl_{et}\ for\ l = 1, 2, 3, 4, 5$.	0.567 (0.175)	0.267 (0.784)
$S4_{et}$	See $Sl_{et}\ for\ l = 1, 2, 3, 4, 5$.	0.453 (0.195)	0.130 (0.732)
$S5_{et}$	See $Sl_{et}\ for\ l = 1, 2, 3, 4, 5$.	0.369 (0.197)	0.061 (0.671)
U	Conditioned on competition, the probability that a low-price bidder submits the same bid as in the previous month.	0.2838	
x^{n-1}	The probability of a single winner during competition.	0.824 (0.062)	0.552 (0.907)
x	Parameter used to calculate the probability of a single winner and probabilities of ties during competition.	0.913 (0.004)	0.907 (0.919)
Section 6	Descriptive statistics for 28,863 exchange group × month observations		
$P(K\|\breve{n}_{et})$	Calculated probability of competition (K), conditioned on $\breve{n}_{et}$. Calculated using $P(W_{et}\|\breve{n}_{et})$ and $P(W_{et}\|K, \breve{n}_{et})$ (see eq. 6.3).	0.358 (0.101)	0.172 (0.444)
$P(K\|W_{et}, \breve{n}_{et})$	Calculated probability of competition (K), conditioned on winning pattern W_{et} (defined in section 4), and truncated number of low-price bidders $\breve{n}_{et}$ (defined in section 5).	0.358 (0.323)	3.54×10^{-10} (0)
$P(S\|W_{et}, \breve{n}_{et})$	Calculated probability of collusion (S), conditioned on W_{et} and $\breve{n}_{et}$. $P(S\|W_{et}, \breve{n}_{et}) \equiv 1 - P(K\|W_{et}, \breve{n}_{et})$.	0.642 (0.323)	0 (<1.000)
$P(W_{et}\|K, \breve{n}_{et})$	Calculated probability of winning pattern W_{et} conditioned on competition and $\breve{n}_{et}$.	See Tables 7–11	
$P(W_{et}\|\breve{n}_{et})$	Observed frequencies of winning pattern W_{et} conditioned on competition and $\breve{n}_{et}$.	See Tables 7–11	
Sections 7–8	Descriptive statistics for 28,863 exchange group × month observations, except for CVp_PV_{ep} and $CVp\_mean\ LOW_{ep}$ for which the number is 27,544		
$add\_bida\_drug_{et}$	The number of active bidders marketing products with the same active ingredient, strength, and administrative form as in exchange group *e*, but not marketing products in exchange group *e*.	0.843 (1.373)	0 (9)
CVp_PV_{ep}	Coefficient of variation in exchange group *e* during price sequence *p* of the mean price per unit (e.g., pill or gram) of the PM.	0.199 (0.177)	0 (1.510)
$CVp\_mean\ LOW_{ep}$	Coefficient of variation in exchange group *e* during price sequence *p* of the mean price per unit (e.g., pill or gram) of products sold by low-price bidders.	0.193 (0.190)	0 (1.543)
CVt_LOW_{et}	Coefficient of variation in exchange group *e* month *t* of the prices per unit (e.g., pill or gram) of products sold by low-price bidders.	0.390 (0.373)	0 (2.100)

$Form_e$	Indicators for twenty different administrative forms, e.g., ordinary tablet and capsules, tablet and capsules with extended release, solutions, eye preparations, and powders.		
$Hetrogen._{et}$	Mean for exchange group e from month $t-7$ through month t of the share of packets sold of low-price bidders in exchange group e which is not one of the cheapest products (i.e., a PM or (one of) the previous month's PMs during the first half of the month) minus the average of this for all exchange group by month observations in the main dataset with the same value of *pm*. *pm* = 0, 1/3, ½, 1 denotes whether a firm did not sell a PM in a given month, sold it but there was a tie between three or more firms (*pm* = 1/3) or between two firms (*pm* = 1/2), or the firm sold the only PM.	0.023 (0.082)	-0.101 (0.663)
$lnMonths\_PatG_{at}$	Natural logarithm of $Months\_PatG_{at}$.	4.961 (0.716)	3.761 (7.960)
$lnSize_{et}$	Natural logarithm of $Size_{et}$.	3.877 (0.097)	0 (6.908)
$lnStrength_e$	Natural logarithm of $Strength_e$.	3.239 (1.857)	-3.912 (6.908)
lnn_{et}	Natural logarithm of n_{et}, defined in section 5.	1.121 (0.291)	0.693 (2.079)
$lnna_{et}$	Natural logarithm of na_{et}, defined in section 7.	1.110 (0.292)	0 (2.079)
$lnnbid_{et}$	Natural logarithm of $nbid_{et}$, defined in section 4.	1.485 (0.416)	0.693 (2.639)
$lnnbida_{et}$	Natural logarithm of $nbida_{et}$, defined in section 4.	1.464 (0.412)	0 (2.565)
lnN_{et}	Natural logarithm of N_{et}, defined in section 5.	0.443 (0.562)	0 (2.303)
lnP_{et}	Natural logarithm of P_{et}.	0.593 (1.435)	-3.705 (8.626)
$lnThAlt_{at}$	Natural logarithm of $ThAlt_{at}$.	1.342 (0.618)	0 (3.761)
lnQ_{et}	Natural logarithm of Q_{et}.	14.263 (2.844)	4.025 (23.667)
$Markets\_low_{et}$	Average over low-price bidders in exchange group e month t of the number of exchange groups they as low-price bidders market products in.	149.761 (55.479)	1 (290.5)
$Months\_PatG_{at}$	Number of months since patent expiration or, when this data is missing, from generic entry in Sweden	212.942 (324.43)	43 (2865)
$MultiM\_low_{et}$	Average number of contacts among low-price bidders (see equations (8.2) and (8.3) in section 8 for details).	42.306 (28.895)	1 (152)
Na_{et}	Number of active bidders in the high-price segment (see section 4) in exchange group e month t.	1.525 (1.470)	0 (10)
na_{et}	Number of active bidders in the low-price segment (see section 4) in exchange group e month t. Equal to one in 0.08% of the observations.	3.166 (0.919)	1 (8)
$nbida6_{et}$	$= max\{0, nbida_{et} - 5\}$, where $nbida_{et}$ is defined in section 4.	0.601 (1.138)	0 (8)
$nlsGena_{et}$	Number of active bidders in exchange group e month t marketing locally sourced generics, including firms that in addition market parallel imports or originators.	3.926 (1.853)	0 (10)

$Over2ls_{et}$	A dummy variable taking the value one if two or more of the low-price bidders in exchange group e month t market locally sourced products. It serves as a proxy for at least two firms having the capacity to punish deviators by meeting all demand at the market.	0.972 (0.166)	0 (1)
P_{et}	The average pharmacy purchase price in current SEK per smallest unit (e.g., pill or gram) within exchange group e month t, weighted by the number of units sold.	11.716 (132.758)	0.025 (5572.320)
$P(S\|W_{et}, \check{n}_{et})Par_{et}$	Interaction variable between a dummy variable taking the value one for parallel bidding patterns weakly exceeding three months and the probability of collusion.	0.021 (0.142)	0 (<1.000)
$P(S\|W_{et}, \check{n}_{et})CVt\_LOW_{e,t-1}$	$\equiv P(S\|W_{et}, \check{n}_{et}) \times CVt\_LOW_{e,t-1}$	0.244 (0.282)	0 (1.983)
$P(S\|W_{et}, \check{n}_{et})lnna_{et}$	$\equiv P(S\|W_{et}, \check{n}_{et}) \times lnna_{et}$	0.738 (0.425)	0 (1.939)
$P(S\|W_{et}, \check{n}_{et})lnnbida_{et}$	$\equiv P(S\|W_{et}, \check{n}_{et}) \times lnnbida_{et}$	0.929 (0.528)	0 (2.365)
Q_{et}	Number of pills sold in exchange group e month t times the strength of each pill (e.g., times 10 for pills with 10 mg of active ingredient per pill).	7.08×10^{7} (6.00×10^{8})	56 (1.90×10^{10})
$QSeason_{et}$	Average during 2015–2019 of number of packages sold within exchange group e during the calendar months of t, divided by the corresponding average for the calendar months of $t + 2$ and $t + 3$.	1.007 (0.136)	0.226 (2.838)
$QSeasonIns_{et}$	A leaving-one-out instrument for $QSeason_{et}$. It differs from $QSeason_{et}$ in that the numerator is the average across all calendar months of t (e.g., all Aprils) except month t.	1.005 (0.154)	0.157 (3.686)
QSD_{et}	The standard deviation of $Q_{e,t-10}$ through $Q_{e,t-3}$.	367.971 (1,215.479)	1.414 (45,824.650)
$QSDY_{et}$	The standard deviation of $Q_{e,t-14}$ through $Q_{e,t-3}$.	419.507 (1,562.135)	1.945 (72,333.320)
$QTrend_{et}$	Number of packages in exchange group e sold $t-5$ through $t-3$ divided by the number of packages sold five months earlier; $t-10$ through $t-8$.	1.013 (0.517)	0.231 (82.487)
$QTrendY_{et}$	Number of packages in exchange group e sold $t-5$ through $t-3$ divided by the number of packages sold one year earlier; $t-17$ through $t-15$. Missing for 467 observations.	1.045 (1.353)	0.227 (132.835)
$QualityDifferencesSD_{et}$	Standard deviation over low-price bidders in exchange group e of difference from $t-7$ to t between the bidder's market share and average market share in the exchange group for a given PM status. See the text in section 8 for details.	0.094 (0.079)	0 (0.707)
$Size_{et}$	Average size in exchange group e month t of packages sold, e.g., number of pills per package, weighted with number of packages sold of each product. The variable can vary over time as the package sizes within an exchange group can differ slightly, e.g., from 28 to 32 pills.	69.953 (61.406)	1 (1000)
$Strength_{e}$	The amount of active ingredient per unit, e.g., milligram per pill.	102.029 (176.587)	0.020 (1000)

$ThAlt_{at}$	Number of different active ingredients available in Sweden which have the same five-digit ATC-code as the active ingredient (a) in exchange group *e*.	4.565 (2.962)	1	(43)

Appendix E. Robustness analyses for regressions

Table 16. Price-variability results for static specifications with controls for potential bidders

Specification	E.1	E.2	E.3	E.4	E.5	E.6
Dependent variable	CVp_PV_{ep}	CVp_PV_{ep}	$CVp\_mean LOW_{ep}$	$CVp\_mean LOW_{ep}$	CVt_LOW_{et}	CVt_LOW_{et}
$P(S\|W_{et},\check{n}_{et})$	-0.011	-0.011	-0.045***	-0.046***	-0.042***	-0.043***
	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)	(0.012)
$P(S\|W_{et},\check{n}_{et})\ Par_{et}$		-0.018		0.026*		0.017
		(0.012)		(0.012)		(0.014)
$nbid_{et}=3$	0.025	0.026	0.017	0.016	0.082**	0.082**
	(0.015)	(0.015)	(0.011)	(0.011)	(0.026)	(0.026)
$nbid_{et}=4$	0.077***	0.077***	0.061***	0.060***	0.151***	0.151***
	(0.017)	(0.017)	(0.012)	(0.012)	(0.029)	(0.029)
$nbid_{et}=5$	0.094***	0.094***	0.101***	0.100***	0.213***	0.213***
	(0.018)	(0.018)	(0.015)	(0.015)	(0.032)	(0.032)
$nbid_{et}\geq 6$	0.109***	0.109***	0.142***	0.142***	0.279***	0.278***
	(0.020)	(0.020)	(0.018)	(0.018)	(0.035)	(0.035)
$n_{et}=3$	0.026***	0.025***	0.006	0.006	0.049***	0.049***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.010)	(0.010)
$n_{et}=4$	0.026**	0.026**	0.007	0.007	0.098***	0.098***
	(0.008)	(0.008)	(0.010)	(0.010)	(0.014)	(0.014)
$n_{et}=5$	0.014	0.013	-0.013	-0.012	0.147***	0.148***
	(0.009)	(0.009)	(0.011)	(0.011)	(0.018)	(0.019)
$n_{et}\geq 6$	0.053***	0.052***	-0.004	-0.003	0.220***	0.220***
	(0.014)	(0.014)	(0.012)	(0.013)	(0.029)	(0.029)
$dY_{et}/dP(S\|W_{et},\check{n}_{et})$ if $Par_{et}=1$		-0.029***		-0.020**		-0.026**
		(0. 011)		(0.009)		(0.011)
Exchange group FE	yes	yes	yes	yes	yes	yes
Year × month FE	yes	yes	yes	yes	yes	yes
Within R^2	0.038	0.038	0.054	0.054	0.046	0.046
Log-l	18460.607	18461.255	21804.706	21806.395	1050.352	1050.529
N	27,544	27,544	27,544	27,544	28,851	28,851

Note: $dY_{et}/dP(S|W_e^*,\check{n}_e^*)$ if $Par_{et}=1$ is the total effect of $P(S|W_e^*,\check{n}_e^*)$ for parallel bidding patterns, that is, the sum of the coefficients for $P(S|W_{et},\check{n}_{et})$ and $P(S|W_{et},\check{n}_{et})Par_{et}$. See Table D1 for variable definitions and descriptive statistics. Standard errors robust to correlation within exchange groups are reported in parentheses. *, **, and *** indicate statistically significant differences from zero at the 5%, 1%, and 0.1% significance level.

We have also estimated specifications without interaction variables which include 2, 3, and 4 polynomials for $P(S|W_{et},\check{n}_{et})$, respectively. The linear specifications presented in Table 16 performed best according to AIC, while second best was the cubic specifications for CVp_PV_{ep} and

$CVp\_meanLOW_{ep}$ and the quadruple specification for CVt_LOW_{et}. According to these "second-best" specifications, the marginal effect of $P(S|W,n)_{et}$ is positive at small values and becomes negative at $P(S|W,n)_{et} = 0.44$ for CVp_PV_{ep}, 0.17 for $CVp\_meanLOW_{ep}$, and at 0.18 for CVt_LOW_{et} and thereafter becomes increasingly negative (despite small positive coefficients for the cubic term).

Alternative specifications including $lnThAlt_{at}$, and $lnMonths\_PatG_{at}$ produce nearly identical results for the other variables, and these three variables have no significant effects on CVp_PV_{ep} and reduce the AIC values for specifications E.3–E.6.

Specification E.7 in Table 17 differs from specifications 8.2 and 8.3 in that it includes fixed effects for active ingredients instead of no time invariant fixed effect and exchange group fixed effects, respectively. For the dummy variables for $nbida_{et}$ the point estimates for specification E.7 are in absolute values larger than those for specification 8.3 but smaller than those for specification 8.2.

Specifications E.8–E.10 contain OLS estimates for specifications 8.4–8.6. All estimates for the number of bidders are closer to zero in the OLS estimations than in IV estimations, which is consistent with the hypothesis that the OLS specifications suffer from endogeneity bias. The estimates for $QSeason_{et}$ are similar for the OLS and IV estimations.

Specifications E.11 and E.12 differ from specification 8.4 in that, instead of using three-month lags of the variables for the number of bidders as instruments, they use one-month lags (specification E.11) and six-month lags (specification E.12). Recall that the point estimates and standard error for specification 8.4 are -0.104 (0.031), -0.159 (0.034), and -0.187 (0.032) for 3, 4, and 5 or more bidders, respectively, and -0.011 (0.006) for $nbida6_{et}$. We note that using deeper lags produces more negative point estimates (except that the estimate for $nbida6_{et}$ is more negative with three than with six lags), which is consistent with the deeper lags being less endogenous. However, changing from three- to six-month lags affects the point estimates by only 0.1, 0.3, 0.8, and 0.8 standard errors (with the last change being in the opposite direction than the other three) and such differences are likely to arise by chance even if both tree- and six-months lags are exogenous and valid. Because of this and that using deeper lags gives weaker instruments and larger standard errors, the specification with three-months lags is the preferred specification.

Table 17. Additional estimation results for the determinants of the probability of collusion

Specification	E.7	E.8	E.9	E.10	E.11	E.12
Estimator	OLS	OLS	OLS	OLS	IV	IV
$P(S\|W_{e,t-1}, \check{n}_{e,t-1})$		0.596***	0.596***	0.595***	0.595***	0.595***
		(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
$nbida_{et} = 3$	-0.184***	-0.056***	-0.058**	-0.049*	-0.083***	-0.106*
	(0.034)	(0.017)	(0.021)	(0.021)	(0.020)	(0.044)
$nbida_{et} = 4$	-0.233***	-0.088***	-0.089***	-0.073**	-0.121***	-0.169***
	(0.035)	(0.020)	(0.024)	(0.024)	(0.023)	(0.047)
$nbida_{et} \geq 5$	-0.243***	-0.097***	-0.085***	-0.075**	-0.132***	-0.211***
	(0.039)	(0.018)	(0.022)	(0.026)	(0.022)	(0.044)
$nbida6_{et}$	0.017**	-0.002	0.000	-0.001	-0.005	-0.006
	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.007)
$nlsGena_{et} = 2$				-0.102***		
				(0.030)		
$nlsGena_{et} = 3$				-0.092*		
				(0.040)		
$nlsGena_{et} = 4$				-0.117**		
				(0.041)		
$nlsGena_{et} \geq 5$				-0.115**		
				(0.044)		
$add\_bida\_drug_{et}$	-0.006	-0.007	-0.009*	-0.007	-0.009*	-0.013**
	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
$QualityDiffSD_{et}$	-0.301***	-0.211***	-0.202***	-0.220***	-0.220***	-0.240***
	(0.080)	(0.041)	(0.041)	(0.040)	(0.041)	(0.042)
$Hetrogeneity_{et}$	0.011	0.072	0.072	0.070	0.078	0.084
	(0.096)	(0.053)	(0.054)	(0.053)	(0.055)	(0.059)
$Over2ls_{et}$	0.119*	0.154***	0.155***	0.174***	0.153***	0.148***
	(0.047)	(0.031)	(0.031)	(0.024)	(0.031)	(0.031)
$MultiM\_low_{et}$	0.015***	0.012***	0.012***	0.012***	0.012***	0.013***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$MultiM\_low^2_{et}$	-1.80e-	-1.36e-	-1.35e-	-1.35e-	-1.37e-	-1.42e-4***
	(3.14e-5)	(2.27e-5)	(2.28e-5)	(2.23e-5)	(2.29e-5)	(2.36e-5)
$MultiM\_low^3_{et}$	5.43e-	4.25e-	4.21e-	4.23e-	4.28e-	4.42e-7***
	(1.29e-7)	(9.11e-8)	(9.17e-8)	(9.00e-8)	(9.12e-8)	(9.21e-8)
$Markets\_low_{et}$	4.26e-3*	1.46e-3	1.39e-3	1.72e-3	1.55e-3	1.79e-3
	(2.00e-3)	(8.83e-4)	(8.98e-4)	(9.00e-4)	(8.98e-4)	(9.39e-4)
$Markets\_low^2_{et}$	-4.46e-5*	-2.39e-	-2.32e-	-2.51e-	-2.46e-	-2.70e-5**
	(1.97e-5)	(8.44e-6)	(8.51e-6)	(8.40e-6)	(8.53e-6)	(8.90e-6)
$Markets\_low^3_{et}$	1.06e-7*	5.49e-8*	5.33e-8*	5.66e-8**	5.67e-8*	6.20e-8**
	(4.95e-8)	(2.21e-8)	(2.22e-8)	(2.19e-8)	(2.22e-8)	(2.30e-8)
$QSeason_{et}$	-0.024	-0.016	-0.016	-0.016	-0.013	-0.014
	(0.017)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
$QTrend_{et}$	-0.001	-0.003	-0.004	-0.003	-0.003	-0.003
	(0.002)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
QSD_{et}	-5.51e-7	2.21e-6	2.10e-6	2.27e-6	2.21e-6	2.41e-6
	(3.84e-6)	(1.70e-6)	(1.71e-6)	(1.70e-6)	(1.73e-6)	(1.74e-6)
Active Ingre. FE	yes	no	no	no	no	no
Exchange group FE	no	yes	yes	yes	yes	yes
Year × month FE	yes	yes	yes	yes	yes	yes
R-squared	0.179	0.401	0.400	0.402	0.400	0.398
Log-l	-5510.783	3792.487	3787.983	3812.383	3787.902	3741.804
N	28,863	27,888	27,888	27,888	27,888	27,888
K-P rk LM					66.356	40.476
K-P rk LM, p-v.					0.000	0.000
Hansen J, p-v.					0.911	0.621

Note: See Table D1 for variable definitions and descriptive statistics. The variables $nbida_{et} = 3$ through $nlsGena_{et} \geq 5$ and $QSeason_{et}$ are instrumented, using as excluded instruments $lnQ_{e,t-3}$ and their first lags in spec. E.11 and their sixth lags in spec. E.12, except that $QSeasonIns_{et}$ is used instead of a lag for $QSeason_{et}$, Also, see notes for Table 12.

Appendix F. Robustness analyses for the probability of collusion

Here we report the probabilities obtained when using active, instead of potential, low-price bidders when performing the calculations described in section 6. For the 23 observations with na_{et} =1, $x = 1 - 1/\left[P_p\left(1 - \frac{1-S_W-S_H}{S_W(na-1)}\right)\right]$ is not defined and the probability of a tie is instead set as equal to zero. We define $\widetilde{na}_{et} = 2$ when $na_{et} = 1$, $\widetilde{na}_{et} = 6$ when $na_{et} > 6$, and $\widetilde{na}_{et} = na_{et}$ otherwise and use the same value of P_p and the adjustment parameters as in the main analysis.

The number of active low-price bidders falls short of the number of potential low-price bidders for only 946 of the 28,863 observations for which the probability of collusion is calculated,[58] and for 913 of the 19,417 additional observations from proceeding and following months which are used to calculate the probability of collusion. Therefore, it is not surprising to find the correlation between $P(S|W_{et},\breve{n}_{et})$ and $P(S|W_{et},\widetilde{na}_{et})$ to be as high as 0.99, nor that both have the same mean value. Still, we have estimated versions with specifications 7.4 and 8.4 with $P(S|W_{et},\widetilde{na}_{et})$ instead of $P(S|W_{et},\breve{n}_{et})$. In the price-regression, the coefficient for $P(S|W_{et},\widetilde{na}_{et})$ was estimated to be 0.124 as compared to 0.122 for $P(S|W_{et},\breve{n}_{et})$ in specification 7.4. Using $P(S|W,\widetilde{na}_{et})$ as dependent variable changes the estimates for the indicator variables for $nbida_e$ equaling 3, 4, and 5 or more to -0.090, -0.123, and -0.145, respectively, as compared to specification 8.4 where the corresponding estimates are -0.104, -0.159, and -0.187. The estimated long-term effects change less as the smaller short-term effects are partly offset by the coefficient for the lagged dependent variables increasing from 0.594 to 0.599. For the other variables, the estimated effects are even closer to those reported for specification 8.4.

Despite the high correlation between na_{et} and n_{et}, and the resulting high correlation between $P(S|W_{et},\widetilde{na}_{et})$ and $P(S|W_{et},\breve{n}_{et})$, we use $P(S|W_{et},\breve{n}_{et})$ in the main analyses as we find that the mean difference between na_{et} and n_{et} is twice as large (0.036) when $P(S|W_{et},\breve{n}_{et}) > 0$ as compared to when $P(S|W_{et},\breve{n}_{et}) = 0$, which is consistent with the notion that some colluding firms choose not to place a bid when another firm is the designated winner.

Table F1 Probability of collusion conditioned on winning patterns for $na_{et} \leq 2$. in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\widetilde{na}_{et})$	0	28.13	65.25	91.77	99.64	
	$P(W_{et}\|K,\breve{n}a_{et})$	42.24	15.45	3.91	0.94	0.28	63.81
	% of obs.	19.67	9.78	5.11	5.18	35.51	75.25
Parallel bidding	$P(S\|W_{et},\widetilde{na}_{et})$	60.39	99.36	99.99	100	100	
	$P(W_{et}\|K,\breve{n}a_{et})$	0.26	0.00	0.00	0.00	0.00	0.27
	% of obs.	0.31	0.29	0.22	0.28	7.32	8.42

Note: $P(S|\widetilde{na}_{et} = 2)=54.52\%$ and the number of observations is 7,200. All probabilities are strictly less than 100%.

[58] One less in 933 cases and two less in the other 13 cases.

Table F2. Probability of collusion conditioned on winning patterns for na_{et} = 3. in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\widetilde{na}_{et})$	48.29	68.84	92.18	98.44	99.91	
	$P(W_{et}\|K,\check{n}a_{et})$	62.25	16.52	1.48	0.14	0.02	80.41
	% of obs.	49.31	21.71	7.75	3.82	9.37	91.96
Parallel bidding	$P(S\| W_{et},\widetilde{na}_{et})$	0	-	99.16	-	-	
	$P(W\|K,\check{n}a_{et})$	0.12	0.00	0.00	-	-	0.12
	% of obs.	0.05	0.00	0.02	-	-	0.07

Note: $P(S|\widetilde{na}_{et}= 3) =49.04\%$ and the number of observations is 12,204. Parallel winning patterns exceeding seven months are grouped together.

Table 18. Probability of collusion conditioned on winning patterns for na_{et} = 4. in percent

Winning pattern W	Variable	3–4	5–6	7–8	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\widetilde{na}_{et})$	70.87	88.13	96.59	99.33	99.84	
	$P(W_{et}\|K,\check{n}a_{et})$	57.62	19.31	0.97	0.05	0.003	77.95
	% of obs.	47.05	38.70	6.74	1.74	0.52	94.76
Parallel bidding	$P(S\|W,\widetilde{na}_{et})$	0	-	-	-	-	
	$P(W\|K,\check{n}a_{et})$	0.06	-	-	-	-	0.06
	% of obs.	0.01	-	-	-	-	0.01

Note: $P(S|\check{n}_{et}= 4) =76.21\%$ and the number of observations is 7,245. Parallel winning patterns exceeding three months are grouped together.

Table 19. Probability of collusion conditioned on winning patterns for na_{et} = 5. in percent

Winning pattern W	Variable	3–4	5–6	7–8	9≤	11≤	sum
Bid-rotation	$P(S\|W_{et},\widetilde{na}_{et})$	59.79	94.25	98.55	98.80	-	
	$P(W_{et}\|K,\check{n}a_{et})$	50.99	20.84	0.79	0.03	-	72.65
	% of obs.	22.11	63.23	9.48	0.42	-	95.23

Note: $P(S|\check{n}_{et}= 5) =82.57\%$ and the number of observations is 1,909. Bid-rotation patterns exceeding nine months are grouped together. There is no observation of parallel winning patterns exceeding three when n_{et} = 5.

Table 20. Probability of collusion conditioned on winning patterns for $\widetilde{na}_{et}$= 6. in percent

Winning pattern W	Variable	3–4	5–6	7≤	9–10	11≤	sum
Bid-rotation	$P(S\|W_{et},\widetilde{na}_{et})$	58.93	93.00	97.55	-	-	
	$P(W_{et}\|K,\check{n}a_{et})$	45.13	20.63	0.65	-	-	66.40
	% of obs.	23.61	63.28	5.90	-	-	93.13

Note: $P(S|\check{n}_{et}= 6) =88.52\%$ and the number of observations is 305. Bid-rotation patterns exceeding seven months are grouped together. There is no observation of parallel winning patterns exceeding three when n_{et} > 5.