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To Silvia, Gunther and Luigi

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Table of Contents

List of Tables

List of Figures

"Felix, qui potest rerum cognoscere causas"
Publius Vergilius Maro

Preamble

Economics is all about incentives. Incentives are a key driver of human behaviour, and are used to align the behaviour of agents with the interests of the principal that designed the scheme. Providing the "right" incentives might be of invaluable help in many situations, but incentives come at a cost too. Sometimes agents respond to them in unintended ways. For example, the introduction of programs that use student test scores to incentivize schools to improve students' performance induced unexpected distortions such as cheating. Jacob and Levitt (2003) found that teachers and schools cheated on test scores by suggesting the right answer to students in at least 4-5% of elementary school classrooms annually.

The field of economics that emerged, analyzing hidden or even criminal activity and detecting the "footprints" that wrongdoers' actions have left in the data, is generally referred to as "forensic economics", reviewed by Zitzewitz (2012). Prominent examples include violations of U.N. sanctions (DellaVigna and La Ferrara, 2010), detection of collusion by market makers in the Nasdaq stock exchange (Christie and Schultz, 1994), executive stock option backdating (Heron and Lie, 2007) and racial bias in employment decisions (Bertrand et al., 2005). Because of the hidden or even illegal nature of some actions, individuals attempt to hide their trails. By doing so they tend to systematically distort the outcome, e.g. test scores, they are interested in. Economists make use of their own specialized knowledge of the institutional setting and incentives to derive a test hypothesis to distinguish between "normal" and "suspicious" behaviour. I use the word "suspicious" rather than "illegal" for a precise reason. The question as to what courts regard as proof of illegal behaviour is a legal one. While economists can provide statistical evidence of a specific behaviour in line with incentives, only the law has the power to determine what can and what cannot be used as proof of illegal activity. For example "economic evidence" of collusion is not generally regarded as sufficient to prove collusion in courts. In the most famous case of empirical detection of collusion, Christie and Schultz (1994) provided strong economics evidence that brokerage firms making markets in Nasdaq stocks *implicitly* colluded to maintain profits at supra-competitive levels. In contrast to stocks in the New York Stock Exchange that were quoted both in odd and even eighths of a dollar, Nasdaq stocks were quoted exclusively in even-eighths, which increased the bid-ask spread, i.e. the brokers' trading margins. Just after their findings were released, the Nasdaq market making firms changed their pricing practices abruptly, producing lower trading costs for investors. Was this "enough" legal evidence to convict firms? No. The findings triggered a series of class-action antitrust lawsuits and the Antitrust Division of the U.S. Department of Justice and the Security and Exchange Commission (SEC) started to investigate the Nasdaq market makers. Even though defendants did not acknowledge wrongdoing, they settled the case and paid over $1 billion in fines. This case nicely summarizes the benefits and limits of economic evidence. The results of Christie and Schultz (1994) “uncovered” a pricing behavior which was consistent with collusion and led to class-actions settled for over $1 billion, but their economic evidence would have not been sufficient to prove collusion in courts. In a similar spirit Abrantes-Metz et al. (2012) analyzed manipulation of the Libor rate and found “suspicious” patterns, but only the subsequent investigations by the SEC, which found evidence of direct communication between banks, could prove collusion in court. While these two studies had a strong impact, many other studies might be confined to journal archives. For this reason it is important to highlight the need to fit economic evidence within the current legal framework, an issue Jens-Uwe Franck and I discuss in detail in chapter 2 with respect to using econometric evidence as proof of collusion.

Methodology

Before proceeding to a more detailed summary of the four chapters, I would like to highlight the methodology I have consistently used across studies. Having been deeply influenced by my econometric professor at LSE, Steve Pischke, I have always tried to be very clear on first, what is the causal relationship of interest; second, what is the identification strategy I use to approximate an experiment using observational data, and third, what is my mode of statistical inference. As Angrist and Pischke (2008, p. 83) write: “Causal inference has always been the name of the game in applied econometrics”. In my search for causality I have noted that the more interesting a question is the less likely it is that we have experimental data. Due to their intrinsic nature most questions on illegal activity cannot be analyzed using an experiment. Still, conditional on a set of assumptions, recent econometric techniques such as instrumental variables and differences-in-differences can be used to study causal effects using observational data.

In the following chapters I have used the three main estimation techniques discussed in Angrist and Pischke (2008). In Chapter 4 I use a natural experiment to evaluate the “fairness” of a coin toss in a two-stage tournament. In Chapter 3 I use an instrumental variable approach to decompose the mismeasured variation of insider trading related odds changes, and in chapter 1 I use a difference-in-differences framework to test whether a specific pricing strategy unilaterally implemented by the market leader caused a price increase compared to EU prices.

Even though I have been profoundly shaped by Angrist and Pischke's (2008, 2010) approach on “How Better Research Design is Taking the Con out of Econometrics”, I also recognize some limits of their approach. In particular Angrist and Pischke (2008, 2010) seem to leave very little space for theory. In contrast I think economic theory provides a powerful tool to think about the behavior of individuals and firms. For example Article 101 of the Treaty on the Functioning of the European Union prohibits inter alia “all agreements between undertakings, decisions by associations of undertakings and concerted practices […] which: (a) directly or indirectly fix purchase or selling prices or any other trading conditions. […]". The ratio of this law stems from a general microeconomic analysis of the welfare effects of price fixing. It is difficult to imagine whether industry-specific quasi-experiments would have been such a powerful policy driver as were the general theoretical results on monopoly pricing. In addition, while questions such as the effect of education on wage do not require much "structure", whenever we need to analyze strategic interaction between oligopolistic firms, we need an underlying model to test whether the observed behavior is consistent with a competitive or collusive model. In addition, without a formal model, as for example the simultaneous equations describing supply and demand, it would not be possible to perform a welfare analysis which is the basis of many important policy decisions and, for example, is used to calculate damages in antitrust litigations.

In all the following chapters I have derived a test hypothesis from different theoretical models. For example, a series of papers by Athey and Bagwell (2001, 2008) and Athey et al. (2004) provide a direct connection between price stickiness and collusion, while the work by Rotemberg and Saloner (1990) and Mouraviev and Rey (2011) provides a theoretical justification for the association between price leadership and tacit collusion. In the insider-trading paper I rely on the theory of efficient financial markets popularized by Fama (1970) for the null hypothesis that allows me to identify whether the last odds before the game incorporate all the past-price information. In addition I use the insights of Becker's (1968) theory of rational crime to find valid instruments that shift the likelihood of cheating. Finally, even in the natural experiment analyzed in chapter 4, where internal validity is guaranteed, I test two different theories: leading versus risk-taking effect. The finding that the order of an advantage in a two stage game does not significantly increase the winning probabilities might be caused by one effect balancing out the other. Thus the identification strategy aimed at taking apart these two effects.

Summary of the Main Findings

The interplay between the high explanatory power of statistical instruments to detect collusion and the need to fit these instruments within a regulatory framework is the subject of the first two chapters of this dissertation respectively. In these two chapters Jens-Uwe Franck and I analyze the incidents around the unilateral commitment by the market leader in the Italian gasoline market to adopt a sticky pricing policy.

The focus of the first chapter is on dynamic pricing strategies. Using daily firm level prices in Italy and weekly average EU prices, we show that ENI's unilateral commitment to sticky pricing had a twofold effect: first, it facilitated price alignment and coordination on price changes, and second, it led to a significant increase in prices. In the first part of the empirical analysis we compare the interdependence of the leader's and competitors' price changes before and after the implementation of the new pricing policy. Our main finding is that after the leader implemented its sticky pricing policy, the observed pricing pattern shifted from cost-based to leadership pricing. In the second part we relate the new pricing behavior to the level of prices, and show that sticky-leadership pricing had a positive and significant effect on prices. Because unobserved shocks continuously hit markets, we compare Italian and European prices before and after the policy, using a difference-in-differences approach. Our central identifying assumption is that "market trends" would have been the same in the treatment (Italy) and control (EU) group in the absence of a treatment (ENI's price policy change). As one might question the subjective choice of a control group we also use a "synthetic control group" approach developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010), which constructs an optimal data-driven control group. Our results are consistent and highly significant across specifications. After the introduction of sticky pricing, competitors followed the leader's price and average Italian prices grew by about €8 to 12 per 1000 liters, compared to EU prices.

In the second chapter we discuss how such an econometric detection of tacit collusion might be fitted within a legal framework. The main issue we address is the incoherence between the current legal instruments to tackle collusion, which are mostly based on explicit evidence of collusion, i.e. communication, and the incentive-based economic perspective that "talk is cheap" in the absence of effective enforcement mechanisms. While communication might serve to coordinate on a specific equilibrium, it is not a necessary condition to collude. At the heart of collusion lies the incentive of firms to cooperate rather than to compete. An unintended consequence of a legal framework based on explicit evidence is that firms are incentivized to find other, tacit, means of collusion. From a theoretical perspective, tacit and explicit collusion bring about the same negative welfare effects, but only explicit collusion entails the risks of detection for firms. This problem might be more of an issue precisely in those industries where the cartelization rate is presumably the highest, and communication is least needed to sustain collusion, i.e. oligopolies. We propose to use specific strategies played by firms to facilitate or bring about collusion as evidence of anticompetitive market conduct. For our policy to work effectively, we first need to clearly differentiate between (legal) oligopolistic interdependence and tacit collusion. Tacit collusion arises from decisions endogenous to the market by one or several firms which aim at reducing or eliminating competition. In contrast, oligopolistic interdependence stems from (passive) best response to market conditions (including other firms' behavior) which might favor non-competitive performance. Economic theory provides a very good guide to distinguish between the (legal) exercise of unilateral market power and the active promotion of collusion. We conclude by pointing out the necessity for stronger legal instruments that target unilateral conduct that aims at bringing about collusion.

The third chapter is my single-authored paper and deals with the identification of insider trading in a professional betting market. I look at the "informational content" of price changes, i.e. whether price changes convey public and/or private information or simply noise. As stock markets are bombarded by news and insider trading is not directly observable, its analysis has proved to be difficult in standard security exchanges.

In contrast, betting markets provide a natural environment to test theories about price formation and corruption. I exploit the fact that on average the first odds are released 24 hours before a tennis game, and not much information arrives to that market except for incoming order flows, which themselves might contain information. As bookmakers tend to keep balanced books and earn money through their bid-ask spread, if the amount bet on an event increases relative to its likelihood, the odds (implied probability) on that event decrease (increase) until they reach a new equilibrium. In the empirical analysis I use three different specifications to relate pre-game odds changes to returns on bets.

First, I run a linear probability regression of a binary variable being one if that player won the game on his implied probability of winning, calculated as the multiplicative inverse of his odds and on the pre-game changes of that implied probability. The weak form efficient market hypothesis requires that current odds reflect all information contained in historical odds. The null hypothesis can be rejected at very low p-values. Past probability changes contain viable information. A 10% increase in the ex-ante probability of winning increases the ex-post winning probability of a player by .91%, controlling for the last winning probability before the game. Rejecting the null of weak form market efficiency does not tell us where the inefficiency comes from. It might stem from not fully incorporated public information or private information. Thus in the main specification I first use an instrumental variable approach to decompose the insider-trading from non-insider-trading variation of pre-game odds changes, and then test whether the insider-trading variation of odds changes significantly predicts future returns on bets. I model odds changes as the mismeasured proxy for order flows which contain private information. To solve the error in variable problem which leads to attenuation bias, I use the interaction of two terms as an instrument: first, the exogenous variation in the incentives to cheat provided by tournament draws which randomly match players with different relative abilities in the bracket conditional on their seed, and second, a player's time-invariant cultural norms on corruption, calculated using the Corruption Perception Index. Intuitively, the first variable captures exogenous shifts in the incentive to cheat because, conditional on a player's seed, the odds on his opponent, and thus the returns on losing on purpose, vary according to the tournament draws. In contrast, the second variable reflects cultural norms with respect to corruption, which tend to be highly persistent over time. The IV regression results show that a 10% decrease in instrumented pre-game odds increases returns on bets by 1%. This effect duplicates in low stake tournaments where the incentives to cheat are higher. In the last approach I decompose the variation of odds changes in two parts: first, the one related to insider trading using the fitted values of the first stage without covariates, and second, the remaining unexplained variation, i.e. the residuals, of the first stage. The residuals might contain public information, and thus in the last specification I include both the insider-trading variation and the remaining unexplained part of odds changes. The results show that both variables are highly significant, but the coefficient on the insider-trading related part is 4.8 times larger than the non-insider-trading related part. These results indicate that price changes convey private information and that tennis players respond to incentives by cheating more in unimportant games.

In the last chapter Frank Müller-Langer and I use a natural experiment in professional soccer tournaments to test whether the ex-ante fair toss of a coin, which determines the order of an advantage in a dynamic tournament, is ex-post unfair due to two "order effects" between rounds. First, the "leading-effect" predicts that teams taking the lead at the beginning of the tournament might experience an encouragement-effect and/or teams lagging behind might feel discouraged. This effect stems from participants adjusting effort across stages, which could advantage the leading participant who faces a larger "effective prize" after an initial victory (Konrad and Kovenock, 2009; Malueg and Yates, 2010). Second, teams lagging behind might increase risk-taking in the final stages of the tournament as they have "nothing to lose" (Cabral, 2003; Hvide, 2002). We take advantage of a natural experiment in professional soccer tournaments where teams are randomly drawn to have an advantage (home game) either in the first or second game. As the main concern of our setting is that strategies, especially effort and risk choices, are unobserved, we develop an identification strategy to test for leading-effect controlling for risk-taking. Intuitively, if risk taking increases in the last round of the tournament, we should observe "more extreme" results. Thus, we tested whether the sum of goals and the distribution of results (home/away win, draw) significantly changed between the first and second stage. We found little evidence that risk taking changes between rounds. Our results show that the order of the advantage does not significantly change the winning probabilities of teams and that at least in our environment selection, efficiency and fairness for participants is guaranteed.

Conclusion

Forensic economics seems to be a field in expansion, with a lot of potential and some limits. Understanding hidden behavior is an important part of the research agenda in many different fields, such as industrial organization and finance. It can help to affect policy for the better, detects potentially illegal conduct, and deters future illegal behavior by increasing the likelihood of detection. A limit of such an economic analysis is that it relies on recognizing systematic patterns emerging over large samples, but it is of little use in a specific case. While this limit surely applies to the insider-trading paper, it is less a concern in the case of the Italian petrol market, where significant changes in the price conduct by the leader, and responses by competitors, can be econometrically characterized and (causally) related to price increases, using a benchmark. Even in the tennis data, my economic analysis might be useful in selecting which players are most likely to have cheated. This permits authorities to focus their limited resources on cases with the highest ex-ante likelihood of wrongdoing. Another limit of this approach is that it heavily relies on data. If the data is disclosed by those who commit the crime, or by those who for some reason prefer to conceal it, then forensic economics might be limited by the unavailability of (reliable) data. Finally, economic evidence of illegal activity is not yet incorporated in a legal framework and is seldom used as direct proof of wrongdoing. The aim of this dissertation was to show how the predictions of economic theory can be used to derive a test hypothesis to uncover illegal behavior, and in the case of collusion, how this evidence might be used in courts.

Chapter 1: Endogenous Price Commitment, Sticky and Leadership Pricing: Evidence from the Italian Petrol Market

1.1 Introduction

Understanding the interdependence of pricing strategies in oligopolies is a fundamental issue. Firms with market power might use a specific pricing behavior to influence competitors' actions in order to facilitate price coordination and sustain (tacit) collusion.[1]

In this paper we provide empirical evidence of the role of unilateral price commitment to sustain (tacit) collusion and highlight the role of price-stickiness as an *endogenous* commitment device to collude. Infrequent and large price changes by the market leader may facilitate tacit coordination on the leader's focal price and result in higher prices. On 6th October 2004, ENI, the market leader on the Italian petrol market, publicly announced a new pricing policy which consisted of infrequent price variations (sticky pricing) and price changes larger in magnitude. ENI increased the time lag between price changes from 6 to 16 days and increased the mean price change from 1% to 5.8%. About five months later the Italian Truckers' Association complained to the Italian antitrust authority about collusion by the Italian petrol firms. Allegedly, firms maintained high and aligned prices which they changed simultaneously.[2] Because the antitrust authority had no evidence of firms' explicit communication, it ended its investigation without issuing a formal decision after the firms accepted to restrict pricing transparency.[3]

In the empirical analysis we document that the leader's unilateral sticky price commitment with larger price changes had two major effects: first, it facilitated price alignment and coordination on the leader's focal price with the observed pricing pattern shifting from cost-based to sticky-leadership pricing. Second, prices increased relatively to a control group. To our knowledge this is the first paper that empirically shows the role of endogenous price commitment by the market leader who acted both as the initiator of the new collusive pricing and as the coordinator of price changes.

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[1] While firms might also (illegally) communicate to collude, the benefits of communication might be smaller for oligopolies. In a meta-study of detected cartels Levenstein and Suslow (2006) show that there is no clear relation between the likelihood of collusion and concentration. Using a laboratory experiment Fonseca and Norman (2012) find that concentrated industries are able to collude irrespectively of communication. Thus, understanding the role of specific pricing strategies used to tacitly collude is of key importance for competition policy and regulators as evidence of collusion is mostly based on evidence of communication.

[2] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section I para. 1; available at http://www.agcm.it.

[3] The inability of antitrust authorities to deal with tacit collusion poses the question on whether and how antitrust policy should respond to tacit collusion in oligopolies, an issue we discuss in Andreoli-Versbach and Franck (2013a). For the final report by the Italian antitrust authority on this case see: Autorità Garante della Concorrenza e del Mercato, 20.12.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 17754; available at http://www.agcm.it.

Our research is related to two strands of literature: cartel detection and dynamic pricing. Economists have long used their knowledge of collusive behavior to derive testable hypotheses to distinguish between collusion and competition.[4] Porter and Zona (1999) use data from the Ohio school milk auction and find that bids by colluding firms *decreased* the further the distance from the schools which is inconsistent with a competitive model. Abrantes-Metz et al. (2006) analyze structural breaks in the pricing of firms supplying seafood in a bid-rigging conspiracy. They find that the price mean (variance) significantly decreased by 16% (increased by 200%) after the collapse of the cartel. While these studies use data on detected cartels, other papers build test hypotheses to uncover cases of collusion. Knittel and Stango (2003) use data from the credit card market during the 1980s and find that a non-binding price ceiling may serve as a focal point to facilitate tacit collusion. Duso et al. (2012) test whether upstream R&D cooperation leads to downstream collusion and find that large horizontal networks are most likely to collude. Finally, Christie and Schultz (1994) have documented "suspicious" bid-ask quotes by market makers in the Nasdaq who increased their margins by avoiding the use of "odd-eights".[5]

The second strand of literature related to our study examines dynamic pricing strategies. A series of empirical papers aimed at characterizing the properties of Maskin and Tirole (1988) Edgeworth cycles. These cycles have been observed in gasoline markets in the U.S. (Lewis, 2012, Eckert, 2003), Canada (Noel, 2007) and Australia (Wang, 2009). Noel (2007) analyzes dynamic pricing in 19 Canadian cities over 574 weeks. Using a Markow-switching regression he estimates both the prevalence and structural characteristics of the three pricing patterns he finds: cost-based pricing, sticky-pricing and price cycles. He finds that cycles (sticky-pricing) are more prevalent when there are many (few) small firms. Wang (2009) studies the Australian gasoline market in relation to a unique policy change which required firms to change their prices simultaneously and only once per day. This policy increased the "commitment costs" by firms to be the first to "relent" after prices fell in a cycle. Whereas before the law a single large firm appeared to be the price leader and was consistently the first to raise prices, after the policy change firms adopted mixed strategies and took turns at being the first to raise prices. Lewis (2012) studies the role of price leadership in coordinating price increases in cycling gasoline markets in the U.S. and finds that the first price increases tend to stem from retail chains that operate a large number of stations.

Our paper complements the literature in three important ways. First, we document the role of endogenous price commitment in switching from cost-based to sticky-focal pricing equilibrium. Second, while Wang (2009) and Lewis (2012) highlight the importance of leadership pricing in the relenting phase of Edgeworth cycles we show its importance in coordinating price changes in response to cost shocks during sticky-pricing.

[4] See Harrington (2008) for a review of empirical cartel detection and Zitzewitz (2012) for a general review of forensic economics.

[5] They found relevant evidence of collusion between brokerage firms making markets in Nasdaq stocks by looking at the bid-ask spread, the traders' margin. As soon as their results became public, this pricing behaviour ended and the subsequent investigation by the Securities and Exchange Commission led to settlements of over $1 billion.

Finally, our setting with a clear shift in the industry's pricing behaviour allows us to develop an identification strategy that quantifies the causal effect of sticky-focal pricing on the price level. This enables us to draw some conclusions on the welfare effects of sticky-focal pricing as compared to cost-based pricing.

The key difficulty in analyzing dynamic pricing strategies is the non-experimental nature of the data. Neither the new pricing policy by the market leader, ENI, nor the leader's (large) price changes which were matched by those of its competitors can be regarded as exogenous. In addition demand and firm-level cost shocks are unobservable. Using existing theoretical and empirical models on price leadership (Rotemberg and Saloner, 1990, and Mouraviev and Rey, 2011) and price stickiness (Athey and Bagwell, 2001, 2008, Athey et al., 2004, Abrantes-Metz et al., 2006, Blanckenburg et al., 2012, and Connor, 2005) we first characterize and then evaluate the effects of the different pricing patterns. First, we use daily firm-level wholesale prices in Italy to characterize the effect of the leader's sticky price commitment on the price interdependence within the Italian gasoline market. Second, using the weekly average wholesale prices of eight other European countries we test whether the sticky-leadership equilibrium led to a price increase.

In the first part of our analysis we compare the interdependence of the leader's and competitors' price changes before and after the market leader introduced its new pricing policy. Before the policy change, competitors adjusted prices every five days following short run cost changes, but after the policy change the time lag between their price changes increased to nine days and the price-cost correlation decreased from .89 to .73. Using a logit model with firms' fixed effect we show that the probability of a competitor aligning its price to the leader's in response to a leader's price change significantly increased after the policy change. In addition, as price alignment is defined narrowly (up to the third decimal) we also look at the percentage price difference between the leader and its competitors. Results are consistent across specifications and point out that competitors coordinated price changes following the leader's focal price after, but not before the new policy.

In the second part we show that sticky-leadership pricing had a positive and significant effect on prices. We use a difference-in-differences approach to compare Italian prices with European prices before and after the implementation of the policy. In addition, as researchers often select comparison groups on the basis of subjective measures of similarity between treated and untreated units, we employ a synthetic control group estimation as developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) to demonstrate that the choice of selected control groups does not drive our results. The synthetic control group is constructed using a data-driven weight of European prices that minimizes the pre-treatment differences between the Italian price and the resulting synthetic control group. This lowers the discretion of researchers in selecting control groups and forces them to show the relative weight of each individual control group.

Our results are consistent across specifications and show that Italian prices significantly increased when compared to a control group after the new sticky-leadership pricing was in place.

The paper proceeds as follows: in section 2 we describe the main features of the Italian petrol market and in section 3 we present the data. In section 4 we perform the empirical analysis and section 5 is a conclusion.

1.2 The Italian Petrol Industry

The Italian wholesale gasoline industry is characterized by many traits which facilitate collusion: a small number of vertically integrated firms, high entry costs, inelastic demand, frequent and small purchases by different consumers and a transparent cost and price structure.[6] There are nine firms operating in the market holding 95% of market share, while the rest is held by small independent retailers that purchase gasoline from one of the vertically integrated firms. The nine big players are ENI,[7] Esso, ERG, Shell, Q8, Total, API/IP[8], and Tamoil.

Market shares are asymmetric across firms. In 2004 ENI, the market leader, accounted for about 35% market share. The second largest player on the market was Esso with a market share of 16%, followed by Q8 with around 11%. The six other firms account for a market share that ranges between 5% and 8%. All firms are vertically integrated, i.e. they either have access to crude oil or they hold shares in companies that run refineries in Italy or Europe. Each firm operates a retailer network with exclusive contracts binding the retailers to the wholesaler on a long-term basis which makes it difficult for other companies to enter the market or to increase their market share. The retailers, petrol stations, can either be independent companies or are directly owned by the wholesalers. Half of these stations are owned by the oil companies, the other half are owned by small private companies, each of them managing on average 30 to 50 stations in one city or region.

Distribution and price setting works as follows: the oil companies communicate to the manager of the petrol station the so-called "suggested price", the price we observe in our data. This price is a non-binding indication of the final retail price petrol stations supplied by that company should charge to consumers. The final retail price of petrol stations with the same brand might vary slightly from region to region due to different fiscal regimes, storage and transportation costs. The owners of the petrol station receive a discount on the suggested price depending on the incentive contract they have negotiated with their respective wholesaler, and the owners are allowed to charge up to a certain percentage on top of the suggested price. Thus, the retail price varies between a minimum, the suggested price minus the discount, and a maximum that is fixed by the wholesaler, so even though the station managers fix the final retail price, their available range

[6] See Levenstein and Suslow (2006) for a discussion on the determinants of cartels' success.

[7] ENI acts on the Italian market under the name of "Agip", its "Refining and Marketing" division dealing with gasoline.

[8] API and IP merged in 2005, when ENI sold IP to API.

falls between their purchase price and the maximum price they are allowed to charge, as stipulated by the oil company. Thus, their freedom to set prices is low and the effect of managerial choice of petrol station owners does not bias this analysis which focuses on competition between the nine large wholesalers and not between the owners of petrol stations. The strong similarity between "suggested" and final prices has been confirmed by the Italian antitrust authority which stated that "the suggested price constitutes an extremely narrow measure in relation to what the consumer will pay for retail gasoline."[9]

With respect to the cost structure, the most important cost for oil companies is the Premium Unleaded Gasoline Mediterranean Price, which is reported by the Platts.[10] In Italy the reference cost for buying gasoline on the refinery markets based in Genoa (north-west Italy) and Lavera (southern France) is the Platts Cif Med (Platts). This price index is widely regarded as the major (opportunity) cost for wholesalers[11] and is used by market-specific newspapers and industry insiders to calculate industrial margins, commonly defined as the difference between the "suggested price" and the Platts. The "suggested price" has two components: a fiscal one and an industrial one. It has been estimated by the Italian Union of Petrol Producers that the Platts reflects 67 percent of the industrial price, while the other 33 percent is attributable to distribution, storage, administrative steps and the petrol stations' margin. Taxes account for approximately 58 percent of the final retail price in Italy and are the major component of the final price.

1.3 Data

We use two different datasets, the first of which is a dataset which consists of daily firm-specific pre-tax wholesale prices and industry level costs as reported by the Platts Cif Med. This data is summarized in **Table 1.1** and will be used to analyze the pricing strategies adopted by the leader and the reaction of the competitors. The wholesale prices, plotted in **Figure 1.1**, refer to the "suggested prices" of gasoline described above from the nine major companies ENI, Api, Erg, Esso, IP, Q8, Shell, Tamoil and Total from 1st January 2003 until 15th May 2005. As discussed above, the main source of costs for firms is the Platts, which represents the implicit opportunity cost to firms to sell their gasoline on the European wholesale market rather than to their petrol stations.

The second dataset consists of average aggregate retail gasoline prices for EU countries, which include taxes. This dataset is taken from the European Commission Oil Bulletin, which reports the prices of oil products

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[9] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section IV, para. 27, available at http://www.agcm.it.

[10] Platts is a division of the Information & Media Services group of McGraw-Hill and a leading global provider of energy information that collects and publishes on a daily basis details on the prices of bids and offers for specific oil products and regions from traders and exchange platforms.

[11] See for example the analysis of the composition of final retail prices into industrial and fiscal components by the Italian Petrol Union, available at http://www.unionepetrolifera.it/it/show/34/La%20struttura%20del%20prezzo or the Pöyry (2009) report on EU fuel prices.

across Europe on a weekly basis.[12] The countries we consider to be the EU benchmark are the Netherlands, France, Germany, Austria, Belgium, Greece, Spain and Portugal for the period from January 2003 to May 2005. These prices include taxes and were collected weekly. A summary of the EU data per country is reported in **Table 1.2**, while a plot of the Brent, Italian and EU prices can be seen in **Figure 1.2**.

1.4 Tacit Collusion through Sticky Leadership Pricing

We analyze the incidents relating to 6th October 2004 (first vertical line in **Figure 1.1**) when ENI publicly announced the adoption of a new pricing policy which consisted of fewer and larger price changes. ENI increased its average price change from 1% to 5.8% and increased the time lag between price changes from 6 days to 16 days. ENI declared that the purpose of this policy was to lower the short-term price-cost relation and to stabilize retail prices.[13] About five months later in March 2005 (third vertical line in **Figure 1.1**) the Italian Truckers' Association, FITA, complained to the Italian antitrust authority about high and aligned prices.[14] About two years later, in January 2007, the Italian antitrust authority started an investigation into price fixing. Due to the lack of evidence of direct communication between firms the antitrust authority decided to end the investigation in December 2007 without punishing ENI and its competitors for an antitrust violation. The authority could only achieve a commitment by the firms to reduce price transparency on the market.[15]

The aim of the empirical analysis is to describe the different pricing patterns which emerged after the leader's unilateral price commitment and to test whether this change caused a price increase. In the first part of the empirical analysis we focus on characterizing the main traits of the pricing behavior of firms and on the relation between the leader's and competitors' price changes. We test whether the standard deviation, competitors' alignment and the frequency and magnitude of price variations significantly changed after the policy and thus, whether competitors adopted the same pricing behavior as the leader. In addition we test for the emergence of leadership pricing by analyzing competitors' price reactions to price changes by the leader.

In the second part we test whether the new price pattern was pro-collusive and caused a price increase using a difference-in-differences method and a synthetic control group approach.

Throughout the empirical analysis we will use the date when ENI's competitors started to align to ENI (12th November 2004) as the beginning of the policy (second vertical line in **Figure 1.1**) and not the date on which

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[12] For an in-depth analysis of the European gasoline market we refer to the report commissioned by the EU Commission and edited by Pöyry Energy Consulting in 2009.

[13] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VI, para. 42, available at http://www.agcm.it.

[14] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section I, para. 1, available at http://www.agcm.it.

[15] Autorità Garante della Concorrenza e del Mercato, 20.12.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 17754, available at http://www.agcm.it.

ENI announced its new pricing policy (6th October 2004). This choice reflects the emergence of the new sticky-leadership pricing *after* the transition period characterized by ENI's price commitment and does not significantly affect our results.[16]

1.4.1 Policy Change and Competitors' Alignment

1.4.1.1 Sticky-pricing

Sticky pricing constitutes an important element in a strategy to sustain collusion. An advantage of sticky pricing is that it is straightforward to implement and that deviations can easily be detected and punished. In a series of theoretical papers Athey and Bagwell (2001, 2008), Athey et al. (2004) analyse dynamic collusive pricing. The key trade-off that emerges from the theoretical analysis of firms' incentives to engage in sticky pricing as collusive strategy is between productive efficiency that requires firms with lower costs to produce more and higher (aligned) prices under collusion. Under some parameter constellations the optimal equilibrium for firms is relatively simple: all firms adopt a sticky pricing scheme and charge the consumers' reserve price.[17] Thus, firms sacrifice productive efficiency to sustain a higher price level in the market. The theoretical prediction that collusion is linked to sticky pricing is confirmed by a series of empirical findings based on ex-post evidence of cartel pricing (Abrantes-Metz et al., 2006, Blanckenburg et al., 2011, Connor, 2005). In this section we provide statistical evidence according to which firms adopted sticky pricing after the new policy was introduced.

Table 1.3 reports the firms' absolute percentage price changes on days with price changes (columns 1 and 2) and the number of days between price changes (columns 4 and 5) before and after ENI's new policy respectively. In column 3 and 6 we report the difference (in *italics*) of the pre and post policy means of frequency and magnitude and the t-statistic (in **bold**). Before the policy firms' price changes were frequent and price changes were small. On average changes occurred every 5 days and the average price change was .8%. After the new pricing policy, price changes occurred less frequently, on average every 9 days, and their amount became larger, namely 2.9%. Performing the same analysis for each firm individually, we get the same results: all competitors significantly increased the magnitude of price changes and six out of eight competitors significantly increased the time lag between price changes which shows that competitors substantially adopted the leader's new pricing policy as can be seen in **Figure 1.1**. [18]

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[16] We ran all the regressions both including and excluding the "commitment" period (interval between the first two vertical lines in **Figure 1.1**). The inclusion of this period neither changes the sign nor the significance level of the estimated coefficients.

[17] As usual in supergames, many different equilibria might emerge. In this paper we focus on the simplest strategy to sustain collusion, i.e. sticky-pricing. With other parameter configurations other (more complex) type of equilibria are possible.

[18] Only one competitor, ERG, publicly *declared* that it would not directly follow ENI's new pricing strategy, see Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VI, para. 41.4, available at http://www.agcm.it.

In addition, we test whether the daily dispersion of prices across firms decreased after the policy, a common finding in cases of collusion. The right part of **Figure 1.3** displays the kernel density distribution of the daily price standard deviation[19] across firms before and after the policy was implemented. The dotted line indicates the price dispersion after the policy change and suggests a decrease in the dispersion after the policy. In contrast the mean price dispersion *increased* significantly during the period of sticky pricing. This result is explained by the increase in the magnitude of price changes, which caused huge spikes in price dispersion on days where ENI changed its prices. In fact while the mean price dispersion is significantly higher (.0022 versus .0015), the median is lower (.0012 versus .0015). While this might seem to contrast with our collusive hypothesis based on the positive relation between sticky-aligned prices and collusion, in **Table 1.5** specification 1, we build a regression model which controls for current and lagged price changes by the leader. We find that the mean absolute percentage price difference between the leader and competitors significantly *decreased* after the policy, which is in line with collusive leadership pricing. This result suggests that economic screens[20] based on price mean-variance tests might fail to sufficiently take into account variations in the *magnitude* of price changes caused by focal pricing during collusive periods.

A shortcoming of sticky pricing models is that they do not address *how* colluding firms react and coordinate on exogenous cost and demand changes. In the next section we will demonstrate that ENI's (focal) price was used by its competitors to coordinate price changes.

1.4.1.2 Leadership pricing

Price leadership is "one of the most important institutions facilitating tacitly collusive pricing behavior" (Scherer and Ross, 1990, p. 346). Rotemberg and Saloner (1990) examine a differentiated oligopoly and demonstrate that price leadership facilitates collusion under asymmetric information and that it increases price rigidity. They conclude that such a pricing scheme has many positive attributes: first, it is easy to implement, second, it doesn't require communication, and third, it is very easy to detect deviations. Mouraviev and Rey (2011) study the role of price or quantity leadership in circumstances where firms can act either simultaneously or sequentially in an infinitely repeated setting for both Bertrand and Cournot competition. In line with Rotemberg and Saloner (1990) they highlight that leadership facilitates collusion.

We test whether ENI's commitment led to leadership pricing. The left part of **Figure 1.3** shows the histogram of $sum\_aligned\_compet_t$, the sum of aligned competitors. While pre-policy alignment, which is a count variable, seems to follow a Poisson distribution, the post-policy alignment distribution is more skewed to the right and seems to have a larger number of aligned firms. As $sum\_aligned\_compet$ is an over-dispersed count variable that takes values from 0 (no competitor aligned) to 8 (all competitors aligned), we

[19] Using the coefficient of variation yields the same results.

[20] For a review of screens and their multiple applicability see Abrantes-Metz and Bajari (2009).

run a Negative Binomial Regression model to test if the number of aligned firms is higher after the policy. Specification 1 in **Table 1.4** shows the result. The coefficient on $PolicyChange$ is positive and highly significant, and computing the marginal effect shows that while the average number of perfectly aligned competitors is 1.95, after the policy change it increases to 3.2.

We now turn to dynamic price alignment and run two regression models to test whether the dynamic price response of competitors to price changes by the leader changed after the new policy. The key challenge when estimating competitors' responses is the endogeneity of ENI's price changes which might cause reverse causality. Thus, rather than claiming a causal interpretation of the regression coefficients we focus on testing a break in the leader-competitor pricing behavior. In particular, we are interested in testing whether the infrequent (but large) price changes by the leader served as a focal price to coordinate competitors' price changes. In the first model we use a firm fixed effect logit regression to relate the binary decision by a firm to perfectly align its price to the leader's price to current and past price changes by the leader:

$$P(Align_{i,t} = 1|\boldsymbol{Z}) = h(\beta_0 + \beta_1 policy_t + \sum_{k=0}^{6} \rho_K \, (policy_t * ENIchangeP_{t-k}) + \sum_{k=0}^{6} \pi_K \, ENIchangeP_{t-k} + \gamma_i) \quad (1)$$

Where $Align_{i,t}$ is a binary indicating whether competitor *i* charges the same price as the leader at day *t*, $policy_t$ is a dummy being 1 after the policy was introduced and $ENIchangeP_{t-k}$ is a dummy being 1 if the leader changed its price on day *t-k*, γ_i are time-invariant firm fixed effects and $h(\cdot)$ is the logistic distribution. The estimation coefficients of the logit model and their marginal effects are in **Table 1.4**, specification 2.1 and 2.2 respectively, while in specification 2.3 we report the odds ratios. The key parameters of interest are ρ_K which capture the competitors' dynamic price response to a price change by the leader after the policy. The average likelihood of a competitor aligning its price to the leader after the policy increased significantly by about 10%. Most importantly, after the policy, on days where the leader changed its price, the average likelihood of a price alignment decreases by 16.1% and then gradually grows until it reaches 16.4 to 17.9% from the fourth to the sixth lag. The same results can be seen by looking at odds ratios in specification 3. This regression analysis confirms the "visual" dynamic price alignment presented in **Figure 1.1**, where all major price changes by competitors where preceded by ENI's price change after the policy.

Because $Align_{i,t}$ is binary and narrowly defined (i.e. up to three decimal places) we also consider a continuous measure as the dependent variable, i.e. the absolute percentage price difference with respect to the leader ($\left|(Price_{i,t} - Price_{ENI,t})/Price_{i,t}\right|$). To take into account possible asymmetries between positive and negative price changes we also run the regression distinguishing between positive and negative price changes. A problem with dividing the samples is that after the policy ENI changed its price only 10 times, including 6 negative and 4 positive price changes. The coefficients of the three OLS regressions using all, only positive, and only negative price changes is shown in **Table 1.5**, specifications 1, 3, and 5, respectively. The regression coefficients of specification 1 for the two time periods, before and after the policy, are plotted in **Figure 1.4** as a graphical analysis best depicts the average change in the dynamic alignment between periods. After the new policy, the average absolute percentage price difference to the leader was 4.59% on days where the leader changed its prices and then this difference decreases to 1.94% (1.38%) [.04%] one (two) [three] day(s) after the leader changes its prices. Finally, this difference becomes insignificant on the fourth price-change lag. This relation is not present before the policy, where the competitors' absolute percentage price difference to the leader is mostly constant. In specification 2 and 3 we use the percentage price difference to the leader and consider positive and negative price changes respectively. The coefficients present a similar pattern as in specification 1. After the policy, the average difference to the leader after positive (negative) price changes is -6.52% (2.69%) indicating that the magnitude of the leader's positive changes is larger than negative changes. Similar to specification 1, the absolute value of these differences decreases after a few lags but the coefficients after the 4^{th} lag become insignificant only after positive price changes. For negative changes competitors' prices slightly but significantly undercut the leader's price by -.05%.

The estimates presented above show that the competitors' price reactions with respect to the leader's price changes changed significantly after the policy. Confirming the graphical evidence in **Figure 1.1,** competitors changed and aligned their prices within a few days after the leader changed its prices, a common pricing scheme referred to as leadership pricing, adopted to coordinate price changes and facilitate alignment.

1.4.2 The Effect of the New Pricing Policy on the Price Level

The previous section focused on the coordination mechanism represented by the sticky-leadership pricing which emerged after the announced new pricing policy and its relation to theoretical and empirical literature on how cartels work. We now turn to the pro-collusive effect of the new pricing behavior. The aim of this section is to causally evaluate the effect of ENI's sticky-leadership pricing policy on Italian prices.

The fundamental problem is that we can at most observe one treatment group (Italy) and have no information as to what would have happened without the introduction of the policy.[21] As markets and firms are simultaneously hit by a multiplicity of shocks our main concern is that ENI might be responding to shocks which are unobservable to the econometrician. Thus, the change in the post-policy price level in Italy might have been as well caused by omitted variables. To control for such unobserved shocks we need to relate Italian prices to a control group which received no treatment, the standard procedure in the literature on cartel detection.[22] In the case of the Italian petrol market, given that prices respond to the same cost

[21] For a discussion on problems and methods of evaluating different kind of policies see Imbens and Wooldridge (2009).

[22] The cartel detection literature takes comparable industries to detect "suspicious" pricing patterns or to evaluate the ex-post effects of illegal price coordination. For example Christie and Schultz (1994) compare the dealers' bid-ask spread in the Nasdaq to its equivalent in the Dow Jones, while Porter and Zona (1999) compare bidding behavior of colluding firms with non-colluding firms.

shocks across national markets, and that the goods are homogeneous and traded in the same currency, we can use the gasoline prices of EU Member States as a benchmark. This permits us to causally link ENI's new pricing policy to the industry's profits. Using panel data from nine EU Member States we estimate the effect of ENI's price policy using a dif-in-dif model. ENI's policy change in Italy induces a deviation from this common trend, and although the treatment country (Italy) and control countries (EU) can differ substantially, all the time invariant country level differences[23] are captured by the (EU countries) fixed effects. The key identifying assumption is that "market trends" would be the same in each of the selected EU Member States in the absence of a treatment (price policy change). This means that we assume that ENI's policy and the subsequent new pricing pattern were not correlated to any unobservable market shock in Italy. This assumption is justified by the fact that ENI declared that the reason they were introducing sticky pricing was the increased volatility of the major cost factor, the Platts.[24] The Platts is not an Italy-specific cost index and its volatility ultimately depends on the international price of crude oil. Thus, exogenous shocks to the Platts are not limited to the Italian petrol market but impact on other countries as well.

A common shortcoming of the dif-in-dif model has been the sensitivity of its results to estimation assumptions. In our case one might question a sufficient "similarity" between the Italian and the control group gasoline market. The selection of a control group is usually done on the basis of *subjective* measures of similarity between affected and unaffected groups. We address this issue using an "optimal" weighted average of the available control units. This estimation technique called "synthetic control group" was developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). This inferential method constructs a data-driven synthetic control group using weights of European prices in order to minimize the pre-treatment differences between the Italian price and that of the synthetic control group. Intuitively, the synthetic control group represents a "better" or "more similar" comparison group for Italian prices than any single EU price.[25] In addition this method illustrates the similarities and the relative contribution of each control group in forming a benchmark. Thus, it lowers the discretion in selecting a control group and forces the researchers to show the data-driven weights of each group. This estimation procedure allows us to construct a data-driven, and therefore, more objective control group and to compare the estimates for the effect of the new policy on Italian prices across specifications.

In the main regression model we estimate a dif-in-dif using panel data with weekly price observations from 9 EU countries over a time period of 29 months. The main regression equation is:

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[23] The Italian gasoline market differs in some respect from other EU countries as summarized by the report of Pöyry (2009). Italy has the lowest throughput per site and hypermarkets own considerably more stations in the rest of the EU than in Italy.

[24] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VI para. 42; available at http://www.agcm.it.

[25] Formally, the weights are constructed to minimize the difference between $price_{IT,t<policy}$ and $\sum_{j=2}^{9} w_j price_{j,t<policy}$.

$$price_{i,t} = \beta_0 + \beta_1(IT * policy_t) + \beta_2 policy_t + \gamma_i + X_t\beta + \varepsilon_{i,t} \quad (2)$$

The dependent variable, $price_{i,t}$, refers to the price of country i at week t. $IT * policy_t$ is an interaction term between two dummy variables indicating Italy and the new pricing policy respectively. $policy_t$ is a time dummy that switches to 1 after the policy change. γ_i are country fixed effects and X_t is a vector of control variables that vary over time but not across countries. In the full specification X_t contains lagged values of the Brent (crude oil), a linear time trend, month and year fixed effects. In some specifications we will add only a dummy for the Italian price so that $\gamma_i = Italy$ in order to estimate the "Italian mark-up," while in other specifications we will add all other country dummies and leave out Italy. As already pointed out this "country mark-up" reflects structural, time-invariant differences across countries, e.g. wages and taxes.

The key parameter of interest is β_1, the interaction between the time after the policy change and a dummy indicating Italy's price. β_1 captures the pre and post policy price difference between the treated country (Italy) and the control group (EU), controlling for cost changes (Brent) and seasonal effects. If sticky and leadership pricing were used as a facilitating device to sustain a supra-competitive price level, we would expect β_1 to be positive and significant.

The firms' cost structure across countries depends on three cost sources that can be considered separately. The main source of costs, crude oil, is the same for all countries and using the standard SBIC and AIC criteria, in line with previous literature, we added four weeks lags to account for dynamic price adjustment to costs. The second source of costs are time independent (unobserved) country-specific costs, such as wages and transport costs, that will be captured by the country fixed effect. Finally there are unobserved time varying firm-level cost shocks which we assume to be uncorrelated with ENI's new policy.

Table 1.6 reports the estimated coefficients for the fixed effect model specified by (2) with standard errors clustered at country level in parenthesis. Specifications 1 to 3 use EU countries fixed effects and thus show the average price difference of each EU country as compared to Italy, while specifications 4 to 6 use a dummy for Italy and leave the other EU countries out. All but one country, the Netherlands, have a significantly lower price level than Italy. Greece (France) is the country with the lowest (largest) price difference to Italy, namely about -4.6€ (-77.9€) per 1000 liters. From specifications 4 to 6 it emerges that Italy has a structural price difference of about 30€ per 1000 liters with respect to the other eight EU countries.

The parameter of the key variable of interest, $IT * policy_t$, is positive and highly significant across specifications. The inclusion of current and lagged costs, i.e. the Brent, and month and year fixed effects does not affect the estimate of β_1. The effect of ENI's policy was to increase prices by about 9.8€ per 1000 liters, which corresponds to a 3% price increase when controlling for costs, seasonality and a time trend. This is in line with the collusive hypothesis based on theoretical and empirical literature discussed above. Sticky and leadership pricing were used as a means to coordinate and raise prices.

We compare these findings with the synthetic control group approach by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). In **Figure 1.5** we plot the Italian and the synthetic control price (left part) and their difference (right part). The vertical line represents the date of ENI's policy change. The synthetic control (by construction) tracks the Italian price well before the policy change. After the policy change the lines seem to diverge more and the difference between the Italian price and the synthetic control increased. **Table 1.7** reports the estimates of the weights, pre and post treatment average prices and their difference. The Netherlands's price most closely resembles the Italian one, and thus has a weight of 81%, while all other countries oscillate between weights of 5.8% (Greece) and 1.4% (Germany). Whereas the pre-treatment synthetic price is by construction very close to the Italian price, the post-treatment price differences are large. After the policy change, Italian prices rose by 8.88€ per 1000 Liters with a standard error of 2.84. This positive price difference is significant at the 5% level with a p-value of .014 and is in line with the dif-in-dif estimation discussed above. This confirms that our findings of a significant and positive price increase are not the result of a subjective choice of control groups.

One concern with the dif-in-dif model is that prices follow an AR(1) process and thus the error terms are correlated over time. Performing an augmented Dickey-Fuller test on the Italian and European prices and the Brent, we cannot reject the null hypothesis of a unit root at conventional confidence levels; in contrast the first difference between the aforementioned variables is stationary. To account for the error term correlation we estimate a model in which all variables are stationary and the dependent variable is defined as the price difference between the Italian price and that of the synthetic control group in week t. We regress this stationary difference on the current and lagged first differences of the crude oil, $\Delta Brent_{t-j}$, a time trend, τ_t, and our key variable of interest, $policy_t$. The time series regression model we estimate is:

$$\Delta price_{IT-SynthCont,t} = \beta_0 + \beta_1 policy_t + \tau_t + \sum_{j=0}^{4} \theta_j \, \Delta Brent_{t-j} + \epsilon_t \quad (3)$$

The synthetic control group discussed above represents an "optimal benchmark" and $\Delta price_{IT-SynthCont,t}$ can be thought of as the daily price deviation of the Italian price relative to its pre-treatment optimal benchmark. The effect of the new price policy controlling for current and lagged cost differences will be captured by β_1. Due to the weekly level of the time series and the relatively short time horizon, 29 months with some gaps due to public holidays, we cannot add year and month fixed effects in (3) as we have insufficient data, 94 observations, but instead maintain the time trend. **Table 1.8** reports the estimated coefficients. Moving from specification 1 to 3 we first add current and lagged cost differences and finally a time trend. In line with the dif-in-dif results all the coefficients on $policy_t$ are significant at the 1% level and positive. Once we control for current and lagged costs, first differences and a trend the coefficient grows from 8.8 to 12.5.

Our regression analysis confirms that after the introduction of the sticky pricing policy Italian prices rose with respect to the period before the policy change and controlling for a synthetic control group or EU prices, costs and month and year fixed effects. The regression results confirm that the new policy had a positive and significant effect on Italy's gasoline prices. Through sticky and leadership pricing firms have coordinated their price changes and significantly increased their price levels relative to the EU.

1.4.3 Robustness Check: Productive Efficiency vs. High (Rigid) Prices

At least since the first theoretical models of the kinked demand curve there has been a long-standing feeling that collusion is associated with price rigidity. Intuitively, "to collude" means attributing a higher weight to keeping high prices rather than setting own prices in accordance with demand and firm-level costs. This result has been confirmed in different dynamic settings by a series of papers (Athey and Bagwell, 2001, 2008, Athey et al., 2004). While none of these papers perfectly match the setting of the Italian petrol market, the common prediction of the theoretical literature is that the *best* collusive scheme consists of rigid prices at the expense of productive efficiency. Thus, firms price independently of their own cost type and charge the consumers' reservation price. The key trade-off is between productive efficiency, whereby the firm with the lowest costs serves the market more, and high (rigid) prices that do not reflect firm-level costs.[26]

Our collusive hypothesis states that firms adopted sticky leadership pricing to increase their margins at the expense of productive efficiency. As a robustness check we test whether our hypothesis can be rejected using daily Italian firm level prices. We test two predictions of our sticky leadership collusive hypothesis: first, margin differences with respect to the price leader, ENI, must become insignificant after the policy change and second, margins must increase. The intuition behind this test is that firms aim at increasing their margins by colluding. Firms with higher (lower) costs must have higher (lower) prices in a non-collusive equilibrium, but have the *same* profit-maximizing (rigid) price under a collusive scheme.[27] Thus, at least some of the competitors' fixed effects should be significant if they price independently, but insignificant if they follow the leader's price. In regression model (4) β_2 captures the effect of the policy on margins, while the competitors' fixed effects capture cost differences with respect to the leader. Because of the asymmetric market shares this

[26] Interestingly, Marshall et al. (2008) analyse the role of price announcements in the vitamins cartel and found that during the cartel phase the likelihood of a price announcement is driven by the length of time between announcements, rather than by cost or demand changes. Their evidence provides empirical support for the hypothesis that during a cartel firms don't price following their own costs and thus the price difference across firms should decrease. Slade (1992) analysed dynamic models of tacit collusion in Vancouver's gasoline market and concluded that between price wars prices were very stable and uniform across firms.

[27] Note that this is equivalent to having higher margins as the major source of costs, i.e. the Platts, is the same for all firms. In addition note that firms are capacity constrained and consumers face search costs, thus the low cost firm cannot serve the whole market even though it charges the lowest price.

difference should be positive compared to the market leader (the low cost firm) in a non-optimal collusive equilibrium and insignificant in an optimal collusive sticky pricing equilibrium.[28] As we showed that ENI was the price leader we test whether firms increased their margins but decreased the average margin difference with respect to ENI.[29]

$$margin_{i,t} = \beta_0 + \beta_i(\gamma_i * policy_t) + \beta_2 policy_t + \gamma_i + \tau_t + \varepsilon_{i,t} \tag{4}$$

γ_i are firm fixed effects, $policy_t$ is a dummy that switches to one after the policy was implemented and τ_t is a time trend. The key parameters of interest are β_2 and β_i (see specification 3) that test whether margins were higher and whether the competitors' margin differences changed with respect to the leader after the policy respectively. The results are reported in **Table 1.9**. The dependent variable, $margin_{i,t}$, is stationary and defined as the daily difference between firm's i price and the Platts.[30] In the first (second) specification we perform the regression for the pre (post) policy period only. In specification 3 we report the results of model (4) and thus include both periods, (time invariant) fixed effects and their interaction with the policy dummy. Specification 3 tests for a structural break in the margin differences compared to the leader after the policy.

In line with the previous regressions firms' average margins significantly increased after the new policy by about 22€ per 1000 liters.[31] More importantly, the estimates seem to confirm that firms exchanged productive efficiency to maintain higher prices. As ENI acted as the price leader and has the largest market shares we can reasonably assume that it is the low-cost firm and accordingly it should have the lowest margins in the market. In fact, competitors' margins were significantly higher than ENI's margins before the policy (specification 1) with the exception of one firm, ERG. This difference changes sign (from positive to negative) and becomes insignificant for *all* firms after the policy implementation (specification 2). This result is confirmed in specification 3 where we include both time periods. The difference between the leader's and its competitors' margins becomes insignificant as firms adopt sticky-leadership pricing which lowers productive efficiency but increases the level of margins in the industry, as showed by the positive and significant estimate on $policy_t$ in specification 3. These results provide further evidence that the nature of ENI's new pricing policy was pro-collusive.

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28 Even though we don't have information on firm level costs, market shares are very asymmetric. ENI has about 35% market shares while the second (third) largest firm has 16% (11%), and all other firms range between 5% and 8%.

29 We obtain similar results if we test margin differences across all firms (results not shown).

30 We also ran a similar regression (results not shown) using firms' prices as the dependent variable and controlling for current and lagged costs. The findings are unchanged with respect to model (5).

31 If we leave out the trend (results not shown) the coefficient on the policy dummy is smaller, 16€ per 1000 liters, but still significant at the 1% level.

1.5 Conclusion

How firms set prices and coordinate price changes in order to tacitly collude in oligopolistic markets has been a perennial topic both for economics and antitrust policy. This paper examines dynamic pricing in the Italian wholesale gasoline market and highlights the importance of *endogenous* sticky price commitment and leadership pricing in tacit collusion.

We investigate the role of sticky-leadership pricing as a coordination mechanism to bring about and sustain (tacit) collusion. After its unilateral sticky-pricing commitment the market leader, ENI, did not change its price for 57 days irrespective of cost changes while competitors kept cost-based pricing. Sticky and leadership pricing emerged as the new pricing equilibrium and was adopted by all firms. Firms coordinated price changes through the leader's (focal) price and this coordination resulted in a price increase relative to EU prices.

In the first part of the empirical analysis we characterize the main traits of firms' pricing and the leader-competitor pricing interdependence. We show that after the new policy was implemented, firms increased the magnitude and the time lag between price changes, and thus adopted the same pricing policy as the leader had announced. In addition, we demonstrate that competitors adjusted their prices following the leader's price changes after but not before the implementation of the new policy. In the second part of the empirical analysis we focus on the effects of the newly emerged sticky-leadership pricing on the level of Italian prices with respect to a control group. We use a dif-in-dif and a synthetic control group approach to evaluate whether this sticky leadership pricing resulted in higher prices. In all specifications we find that prices significantly increased, with estimates ranging from 8 to 12€ per 1000 liters. Combined, this price coordination mechanism and the subsequent price increase show that the effect of the unilateral price commitment was to tacitly collude through facilitating price coordination.

These findings cast serious doubts on the effectiveness of cartel enforcement that depends on evidence of (explicit) communication. Tacit collusion appears to be a "natural" way in which oligopolistic markets work. Firms in oligopolistic markets can use their market power to influence competitors' conduct and collude through specific pricing strategies. How to address such unilateral conduct with welfare-decreasing effects without unduly limiting the freedom of price setting in oligopolies remains an unanswered question though.

1.6 Appendix: Tables and Figures

Table 1.1 Summary Statistics Italian Prices

Variable	Mean	St. Dev.	Min	Max	Obs.
ENI	0.375	0.0400	0.310	0.476	866
API	0.378	0.0397	0.313	0.476	866
ERG	0.376	0.0401	0.312	0.481	866
ESSO	0.376	0.0398	0.313	0.476	866
IP	0.377	0.0396	0.313	0.476	866
Q8	0.377	0.0399	0.312	0.476	866
SHELL	0.377	0.0402	0.313	0.476	866
TAMOIL	0.377	0.0399	0.310	0.476	866
TOTAL	0.378	0.0398	0.313	0.476	866
Platts Cif. Med.	0.228	0.0391	0.159	0.340	866
Nr. Aligned Firms	2.206	2.446	0	8	866
Aver. Price Dif.	0.00250	0.00346	0	0.0404	866
St. Dev. Prices	0.00174	0.00165	0	0.0205	866

Table 1.1 reports the summary statistics of "suggested" daily firm level pre-tax prices in the Italian gasoline market from January 2003 to May 2005. The units of observation are Euro per liter.

Table 1.2 Summary Statistics EU Prices

Variable	Mean	St. Dev.	Min	Max	Obs.
Italy	368.9	39.40	308.1	466.3	119
Belgium	328.3	39.18	255.6	421.1	119
Germany	312.6	39.14	254.8	387.5	119
Spain	338.0	37.54	280.3	410.5	119
France	288.9	39.11	231.8	383.6	119
Greece	362.0	39.61	296.2	453.8	119
Netherlands	371.9	40.29	310	464.5	119
Portugal	334.0	38.01	280.7	414.5	119
Austria	348.6	38.52	290.3	431.5	119
Mean EU Price	335.5	37.46	285.3	414.1	119
Brent	187.6	34.46	132.3	273.0	119

Table 1.2 reports summary statistics of weekly EU Prices from January 2003 to May 2005. The units of observation are Euros per 1000 liters.

Table 1.3 Frequency and Magnitude of Price Changes

	(1)	(2)	(3)	(4)	(5)	(6)
	Abs. % Price Change			Days between price changes		
Time period	Pre	Post		Pre	Post	
	Mean	Mean	*Difference*	Mean	Mean	*Difference*
	(St. Dev.)	(St. Dev.)	**t-stat**	(St. Dev.)	(St. Dev.)	**t-stat**
	[Obs.]	[Obs.]	[Obs.]	[Obs.]	[Obs.]	[Obs.]
All Firms	0.0088	0.0293	*0.0205****	5.3	9.47	*4.16****
	(0.0065)	(0.0319)	**19.27**	(5.43)	(7.67)	**8.81**
	[1143]	[172]	[1315]	[1143]	[172]	[1315]
ENI	0.0103	0.0586	*0.0483****	6.63	16.4	*9.76****
	(0.0071)	(0.0377)	**11.45**	(7.81)	(10.95)	**3.64**
	[104]	[10]	[114]	[104]	[10]	[114]
API	0.0081	0.0254	*0.0173****	4.94	8	*3.05***
	(0.006)	(0.03)	**6.01**	(4.69)	(6.93)	**2.67**
	[135]	[23]	[158]	[135]	[23]	[158]
ERG	0.0111	0.028	*0.0169****	6.61	8.8	*2.19*
	(0.007)	(0.0311)	**4.95**	(5.67)	(6.17)	**1.59**
	[101]	[21]	[122]	[101]	[21]	[122]
ESSO	0.0083	0.0268	*0.0185****	5.2	9.25	*4.04****
	(0.0061)	(0.0299)	**6.29**	(5.08)	(7.3)	**3.1**
	[129]	[20]	[149]	[129]	[20]	[149]
IP	0.0089	0.0372	*0.0283****	5.6	12.92	*7.32****
	(0.0067)	(0.034)	**8.03**	(5.16)	(9.88)	**4.47**
	[120]	[14]	[134]	[120]	[14]	[134]
Q8	0.0118	0.0277	*0.0159****	6.76	9.1	*2.34*
	(0.0067)	(0.0332)	**4.4**	(7.33)	(6.4)	**1.33**
	[100]	[20]	[120]	[100]	[20]	[120]
SHELL	0.0074	0.0333	*0.0259****	4.39	11.25	*6.85****
	(0.0058)	(0.036)	**8.11**	(4.14)	(9.77)	**5.31**
	[153]	[16]	[169]	[153]	[16]	[169]
TAMOIL	0.0068	0.0229	*0.0161****	4.12	7.38	*3.25****
	(0.0058)	(0.0298)	**6.27**	(4.21)	(5.32)	**3.45**
	[165]	[25]	[190]	[165]	[25]	[190]
TOTAL	0.0088	0.025	*0.0162****	4.9	8	*3.09***
	(0.0059)	(0.0273)	**6.18**	(4.7)	(7.01)	**2.69**
	[136]	[23]	[159]	[136]	[23]	[159]

Table 1.3 summarizes two key features of firms' pricing strategies before and after the leader's pricing policy change. Columns 1 and 2 report the absolute mean price change on days with price changes for all firms (first row) and at the firm level. Columns 4 and 5 report the average days between price changes. In Columns 3 and 6 we calculate the pre and post policy differences of these two variables and the t-statistic testing whether the difference is significantly different. In square brackets we report the number of observations.

Table 1.4 Price Leadership (1)

Dependent Variable	(1) Number of aligned firms	(2.1) $Firm_{j,t}$ aligned	(2.2) $Firm_{j,t}$ aligned	(2.3) $Firm_{j,t}$ aligned
Regression Model	Negative Binomial	FE Logit / Coef.	Marg. Effects	FE Logit / Odds Ratios
Policy Change	0.539***	0.547***	.1008***	1.728***
	(0.0898)	(0.0794)	(.016)	(.137)
Policy*Leader changes price dummy				
Lag 0		-1.512***	-.161***	.220***
		(0.393)	(.023)	(.086)
Lag 1		-0.0321	-.0054	.968
		(0.262)	(.043)	(.253)
Lag 2		0.158	.027	1.171
		(0.265)	(.048)	(.31)
Lag 3		0.560**	.109*	1.751**
		(0.272)	(.059)	(.475)
Lag 4		0.801***	.164***	2.23***
		(0.265)	(.062)	(.592)
Lag 5		0.762***	.155**	2.144***
		(0.265)	(.061)	(.569)
Lag 6		0.864***	.179***	2.375***
		(0.267)	(.063)	(.634)
Leader changes price dummy				
Lag 0		-0.432***	-.066***	.648***
		(0.101)	(.014)	(.065)
Lag 1		-0.276***	-.044***	.758***
		(0.0959)	(.014)	(.072)
Lag 2		-0.117	-.019	.889
		(0.0926)	(.014)	(.082)
Lag 3		-0.292***	-.046***	.746***
		(0.0945)	(.014)	(.07)
Lag 4		-0.337***	-.053***	.713***
		(0.0966)	(.014)	(.069)
Lag 5		-0.336***	-.053***	.714***
		(0.0969)	(.014)	(.069)
Lag 6		-0.463***	-.071***	.628***
		(0.101)	(.014)	(.063)
Firms Fixed Effects		Yes	Yes	Yes
Constant	0.554***			.325***
	(0.0447)			(.029)
Observations	866	6,928	6,872	6,872
R-squared				

Table 1.4 reports the estimation results of regression model (1). It tests whether ENI's competitors changed their pricing behaviour in response to the sticky pricing policy. Note that $policy_t$ turns to 1 after 12th November, the date on which most competitors started to follow ENI's new policy and not on the date the policy was announced, 6th October. The choice of the beginning of the treatment period does not change the results significantly. The first specification uses a negative binomial model to test whether the number of competitors perfectly aligned to the leader increased after the policy change. Results are in line with the t-test presented in Section V.3. In the second and third column we report the regression coefficients and marginal effects of the fixed-effects logit regression model (1). In the last column we perform the same regression using a different variable, the absolute percentage price difference with respect to the leader. For a graphical representation of the regression coefficients in column (3) see Figure 1.4. (Robust) standard errors are reported in parentheses for columns (3) 1, 2.1 and 2.2. The stars are defined as follows: * (**) and [***] refer to p-values below 10% (5%) and [1%].

Table 1.5 Price Leadership (2)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Absolute % Price Difference To Leader		% Price Difference To Leader			
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Leader's price changes:	Pos & Neg		Positive		Negative	
Policy Change	-0.00239***	(0.000290)	-0.00464***	(0.000423)	0.00178***	(0.000447)
Policy*Leader changes price dummy						
Lag 0	0.0459***	(0.000951)	-0.0652***	(0.00167)	0.0269***	(0.00119)
Lag 1	0.0194***	(0.000950)	-0.0309***	(0.00167)	0.00832***	(0.00118)
Lag 2	0.0138***	(0.000994)	-0.0250***	(0.00167)	0.00237*	(0.00127)
Lag 3	0.00409***	(0.000993)	-0.00377**	(0.00191)	0.00326***	(0.00118)
Lag 4	-0.00109	(0.000993)	-0.00108	(0.00191)	-0.00331***	(0.00118)
Lag 5	-0.00260***	(0.000994)	-0.00181	(0.00191)	-0.00539***	(0.00119)
Lag 6	-0.00170*	(0.000995)	-0.00216	(0.00191)	-0.00536***	(0.00118)
Leader changes price dummy						
Lag 0	0.00197***	(0.000298)	-0.00361***	(0.000425)	0.00775***	(0.000464)
Lag 1	0.000650**	(0.000296)	-0.00120***	(0.000424)	0.00342***	(0.000457)
Lag 2	4.87e-05	(0.000295)	-0.000518	(0.000424)	0.00287***	(0.000459)
Lag 3	0.000121	(0.000294)	0.000435	(0.000421)	0.00204***	(0.000459)
Lag 4	0.000216	(0.000295)	0.000147	(0.000421)	0.00169***	(0.000465)
Lag 5	0.000398	(0.000296)	0.000879**	(0.000421)	0.00181***	(0.000463)
Lag 6	0.000534*	(0.000298)	0.00123***	(0.000430)	0.00171***	(0.000452)
Firms Fixed Effects	Yes		Yes		Yes	
Constant	0.00573***	(0.000157)	0.00504***	(0.000247)	0.00362***	(0.000221)
Observations	6,872		3,960		2,872	
R-squared	0.338		0.440		0.376	

Table 1.5 reports the estimation results of regression model (1) using a different dependent variable and OLS. It is an additional test to the results reported in Table 1.4 of whether ENI's competitors changed their pricing behaviour in response to the sticky pricing policy. The dependent variable in specification 1 is the absolute percentage price difference of competitor *j* with respect to the leader in day *t*. In specifications 3 and 5 we consider only positive and negative price changes by the leader respectively. The dependent variable in these specifications is the percentage price difference with respect to the leader. Standard errors are reported in parentheses, the stars on the coefficients are defined as follows: * (**) and [***] refer to p-values below 10% (5%) and [1%].

Table 1.6 Dif-in-Dif Model

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable			Price of country i ant time t			
Model:	FE	FE	FE	FE	FE	FE
IT*Pol_Change	9.877***	9.863***	9.863***	9.877***	9.863***	9.863***
	(1.970)	(2.110)	(2.126)	(1.964)	(2.101)	(2.117)
Italy				30.75**	30.39**	30.39**
				(9.292)	(9.311)	(9.381)
Policy Change	0.245	-31.35***	-18.53***	0.245	-31.35***	-18.53***
	(3.196)	(3.634)	(2.778)	(3.185)	(3.619)	(2.767)
Time Trend	0.706***	0.152***	0.475	0.706***	0.152***	0.475*
	(0.0340)	(0.0396)	(0.284)	(0.0339)	(0.0394)	(0.283)
Belgium	-37.98***	-37.84***	-37.84***			
	(0.513)	(0.490)	(0.494)			
Germany	-53.68***	-52.78***	-52.78***			
	(0.513)	(0.490)	(0.494)			
Spain	-28.29***	-27.78***	-27.78***			
	(0.513)	(0.490)	(0.494)			
France	-77.39***	-77.90***	-77.90***			
	(0.513)	(0.490)	(0.494)			
Greece	-4.272***	-4.656***	-4.656***			
	(0.513)	(0.490)	(0.494)			
Netherlande	5.597***	4.751***	4.751***			
	(0.513)	(0.490)	(0.494)			
Portugal	-32.33***	-29.94***	-29.94***			
	(0.513)	(0.490)	(0.494)			
Austria	-17.69***	-17.01***	-17.01***			
	(0.513)	(0.490)	(0.494)			
Brent (Lag 0-4)		Y	Y		Y	Y
Year Fixed Effects			Y			Y
Month Fixed Effects			Y			Y
Constant	322.3***	139.2***	130.7***	291.6***	108.8***	100.3***
	(1.568)	(5.832)	(5.304)	(7.925)	(11.51)	(12.49)
Observations	1,071	891	891	1,071	891	891
R-squared	0.612	0.880	0.925	0.35	0.62	0.66
Number of Groups	9	9	9	9	9	9

Table 1.6 reports the estimation results of the dif-in-dif regression model in (2). It tests whether Italian prices increased after the introduction of the sticky pricing policy compared to a benchmark, EU countries. Specification 1 to 3 reports the coefficients on the benchmark countries while specification 4 to 6 reports Italy's fixed effect. In the first three specifications the coefficients represent the country specific price difference compared to Italy, while the last three specifications show Italy's price level compared to the benchmark. Specification 1 to 3 is symmetric to specification 4 to 6, respectively. In all specifications Italy's price significantly increases after the policy was introduced. Standard errors clustered at country level are in parentheses while the stars on the coefficients are defined as follows: * (**) and [***] refer to p-values below 10% (5%) and [1%].

Table 1.7 Synthetic Control Method

State weight to compute "synthetic control group"		Mean Italian price	Mean synthetic control	Difference	t-statistic	p-value	Obs.
Belgium	0.021	Entire Sample					
Germany	0.014	368.85	366.51	2.33*	1.8	0.073	119
Spain	0.027	Pre Sticky Pricing					
France	0.004	354.85	354.82	0.02	0.02	0.981	88
Greece	0.058	Sticky Pricing					
Netherlands	0.814	408.60	399.72	8.88**	2.6	0.014	31
Portugal	0.024						
Austria	0.038						

The left part of the table shows the weights that have been used to construct the synthetic control group. These weights are estimated by minimizing the difference between the pre-treatment (price policy change) Italian price and the other EU countries. The EU price which most closely resembles the Italian one is the Dutch price with a weight of .814. In the right part of the table we compare the Italian and synthetic control price in three different time periods: entire sample, pre and post treatment. By construction the weights are chosen to maximize the similarity of the Italian and synthetic price before the treatment, and thus their difference is small and insignificant. This guarantees that the synthetic control group more closely resembles Italian price movements *before* the policy and allows us to estimate the causal effect of ENI's new pricing policy on prices. "Difference" shows the difference between Italian and synthetic price, while t-statistic and p-value report the estimates of the test: H_0: IT_Price-Synth=0. The estimated price difference in the sticky pricing period is 8.88€ per 1000 liters which is statistically significant at the 5% level.

Table 1.8 Synthetic Control Regressions

	(1)	(2)	(3)
Dependent variable	IT-Synthetic Control price at time *t*		
Model	OLS	OLS	OLS
Policy Change	8.856***	9.746***	12.551***
	(3.59)	(3.059)	(4.224)
Time Trend			-.048
			(.0369)
Brent (Lag Dif 0-4)		Yes	Yes
Constant	.028	.561	2.918*
	(1.197)	(1.064)	(1.752)
Observations	119	94	94
R-squared	.076	.479	.486

In Table 1.8 we run an OLS regression of the policy change dummy, $policy_t$, on the price difference between Italy and the Synthetic Control, $\Delta Price_{IT\text{-}Synth,t}$. In specifications 2 and 3 we add the lagged first differences of the Brent. Results are consistent with the dif-in-dif model and show a significant positive change in the Italian price difference with respect to the synthetic control after the policy. Robust standard errors are in parentheses. * (**) and [***] refer to p-values below 10% (5%) and [1%].

Table 1.9 Robustness check

	(1)		(2)		(3)	
Dependent Variable			Margin of firm *i* at time *t*			
	Coef.	St. Err	Coef.	St. Err	Coef.	St. Err
Time period	Pre Sticky Pricing		Sticky Pricing		All	
Policy Change					22.95***	(2.036)
API	2.692***	(0.836)	-2.070	(2.844)	2.692***	(0.836)
ERG	1.372	(0.838)	-1.674	(2.847)	1.372	(0.839)
ESSO	1.460*	(0.848)	-1.281	(2.887)	1.460*	(0.848)
IP	1.840**	(0.854)	-0.635	(2.891)	1.840**	(0.854)
Q8	2.273***	(0.831)	-0.938	(2.905)	2.273***	(0.832)
SHELL	1.633*	(0.858)	-1.682	(2.880)	1.633*	(0.858)
TAMOIL	1.843**	(0.849)	-2.437	(2.829)	1.843**	(0.850)
TOTAL	2.692***	(0.843)	-1.270	(2.848)	2.692***	(0.843)
Policy*API					-2.070	(2.827)
Policy*ERG					-1.674	(2.831)
Policy*ESSO					-1.281	(2.870)
Policy*IP					-0.635	(2.872)
Policy*Q8					-0.938	(2.887)
Policy*SHELL					-1.682	(2.864)
Policy*TAMOIL					-2.437	(2.814)
Policy*TOTAL					-1.270	(2.830)
Time Trend	-0.0144***	(0.00103)	-0.0896***	(0.0115)	-0.0159***	(0.00104)
Constant	374.6***	(16.51)	1,637***	(190.2)	398.3***	(16.68)
Observations	6,129		1,665		7,794	
R-squared	0.034		0.033		0.115	

Table 1.9 reports the estimates of regression model (5). The dependent variable, margin, is expressed in Euro per 1000 liters. In specification 1 (2) we include only firm fixed effect for the pre (post) policy period. In specification (3) we include both the pre and post sticky pricing time period, firm fixed effects and their interaction with the policy dummy. The results show that while competitors' margins were significantly higher than ENI's margins before the policy, after the policy this difference becomes insignificant. In addition, the estimate on the policy dummy which captures the post policy difference in average industry margins is positive and significant. This shows that controlling for firms' fixed effects the policy had a positive impact on the profitability of the Italian gasoline industry. Robust standard errors are in parentheses while the stars on the coefficients are defined as follows: * (**) and [***] refer to p-values below 10% (5%) and [1%].

Figure 1.1 Cartel Formation

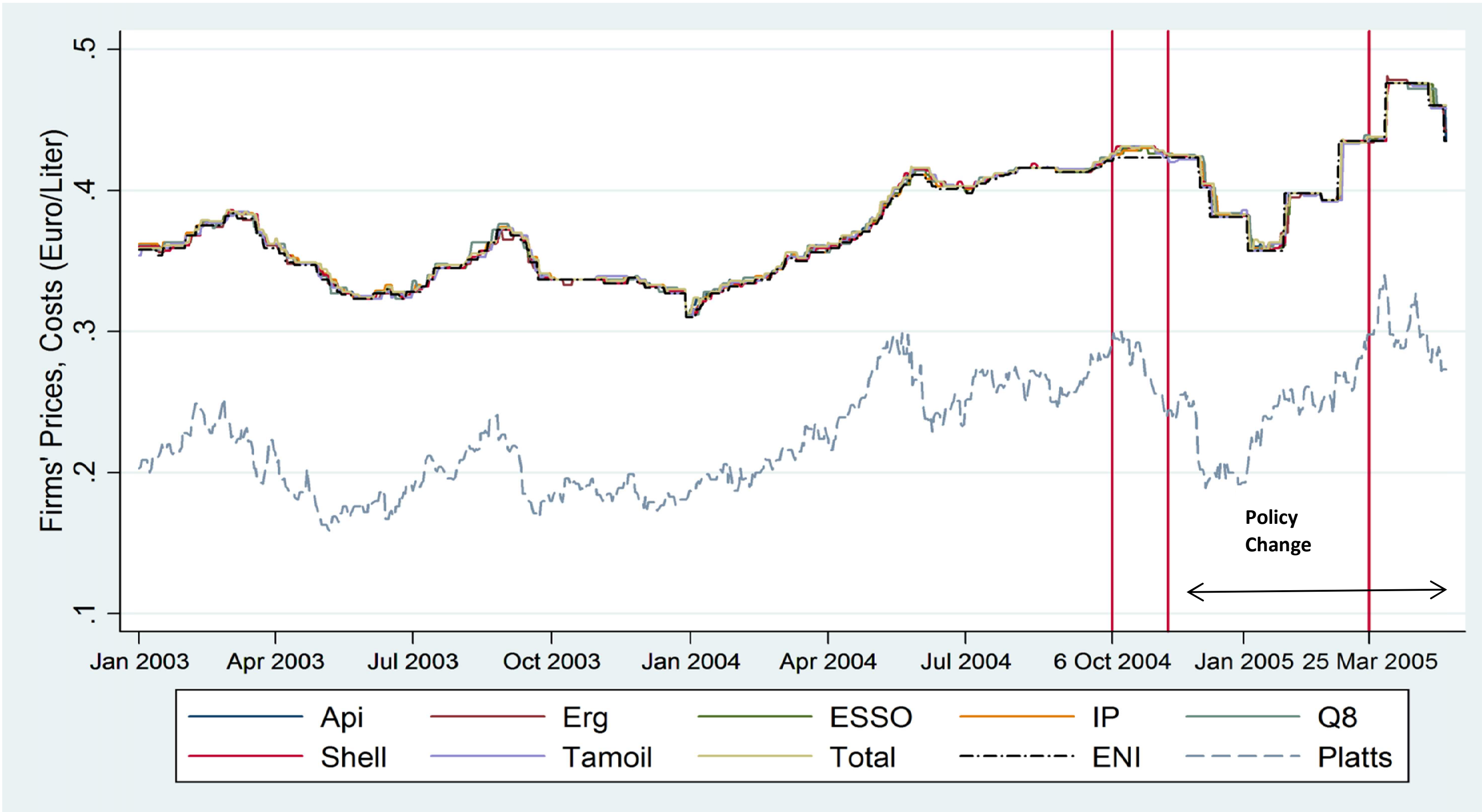

Figure 1.1 shows the daily "suggested" firm-level prices in the Italian gasoline market from January 2003 to 15th May 2005. These prices represent a very good approximation of final retail prices paid by consumers, see section 3.2. The dashed line represents the Platts Cif Med, the major source of cost for firms. The first vertical line denotes 6th October 2004, the date where ENI, the market leader, announced that it would adopt a new pricing policy consisting of sticky prices (i.e. infrequent price changes). The time span between the first two vertical lines constitutes the "commitment" time period. As prices respond to costs with about a month time lag costs were *increasing* just after the announcement by ENI contrary to what might seem from Figure 1.1. Competitors kept increasing their prices following short-run cost changes until the beginning of November when costs decreased and they started to align and follow the leader's price. The second vertical line is placed on the 12th of November, the date when most competitors aligned to the leader. Note that we will take this date as the starting date of the new equilibrium in the empirical analysis. The third vertical line shows the date when the Italian Truckers' Association (FITA) formally complained about "high and aligned prices" to the Italian antitrust authority.

Figure 1.2 Italian Price, EU Price and Brent

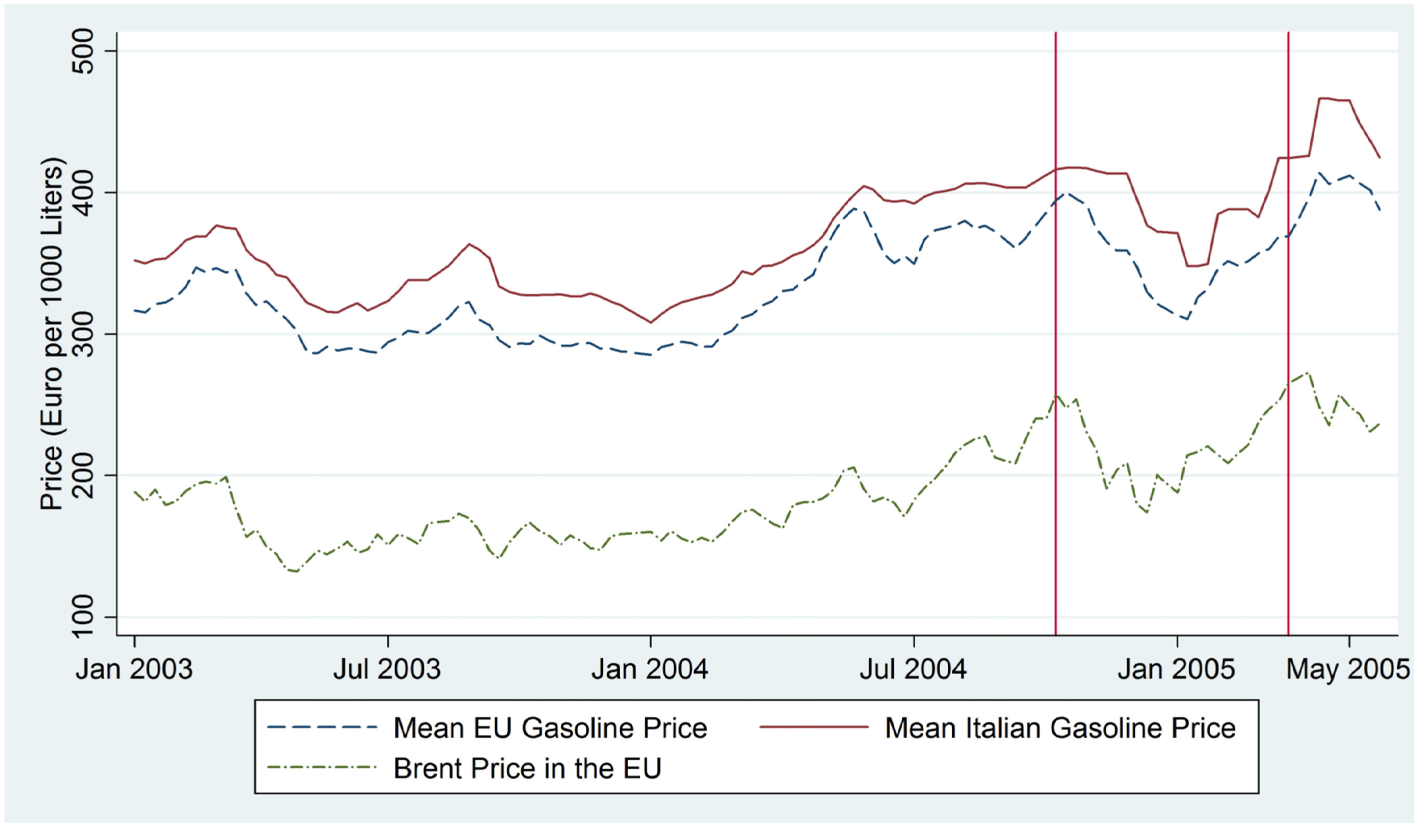

Figure 1.2 shows the average weekly Italian and EU price of gasoline and the European price of the Brent, i.e. crude oil. The continuous line represents the Italian price, while the dashed (dashed-dotted) line represents the EU price (Brent). The first vertical line denotes the date where the market leader announced that it would adopt a new pricing policy consisting of sticky pricing (i.e. infrequent price changes). The second vertical line shows the date when the Italian Truckers' Association (FITA) formally complained about "high and aligned prices" to the Italian Antitrust Authority on 25th March 2005.

Figure 1.3 Alignment and Price Dispersion

Figure 1.3 shows the distribution of the number of firms aligned to the leader (left graph) and the kernel density of the daily standard deviation of prices (right graph) both for the sticky and pre sticky time period. Alignment is a count variable that ranges from 0 (none of ENI's competitors charge exactly the same price as ENI) to 8 (all competitors are aligned). The right part of Figure 1.3 shows the kernel density of the daily price dispersion across firms. As ENI increased the magnitude and time interval of price changes the mean (median) price dispersion increased (decreased) during the sticky pricing period.

Figure 1.4 Dynamic Price Alignment to the leader

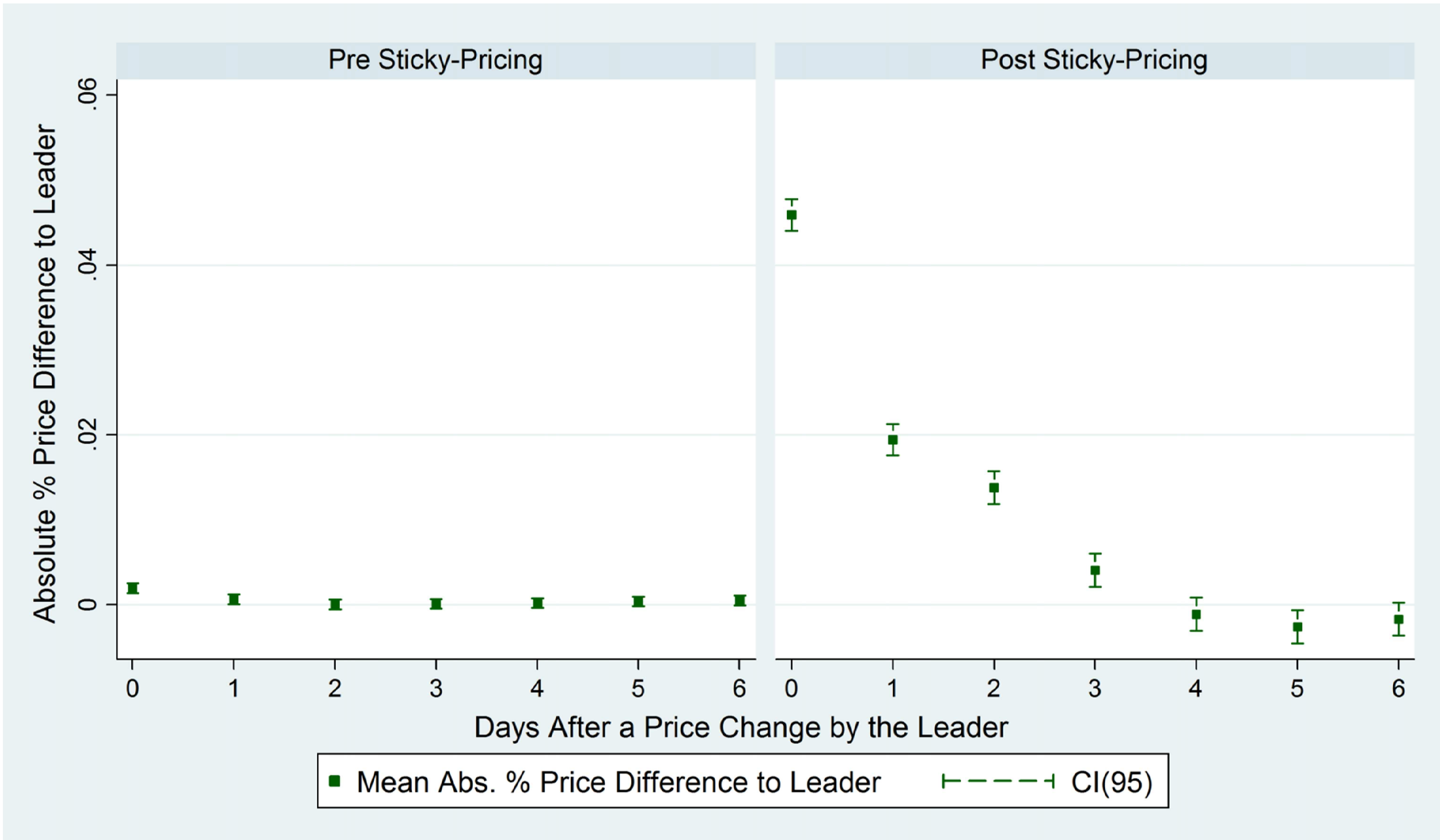

Figure 1.4 shows the coefficients of specification 3 in regression model (1), reported in Table 1.3. The coefficients describe the dynamic alignment of competitors after a price change by the leader both before the new policy (left graph) and after the new policy (right graph). Before the policy the average absolute percentage price difference to the leader did not significantly change in response to a price change by the leader. In contrast, after the policy change competitors significantly changed their pricing behaviour. They dynamically aligned their prices to the leader's price with two to three days lag after a price change by the leader. The absolute price difference is 4.5% on days where the leader changed its price and then quickly drops until it gets insignificant after the fourth day.

Figure 1.5 Italian Price and Synthetic Control

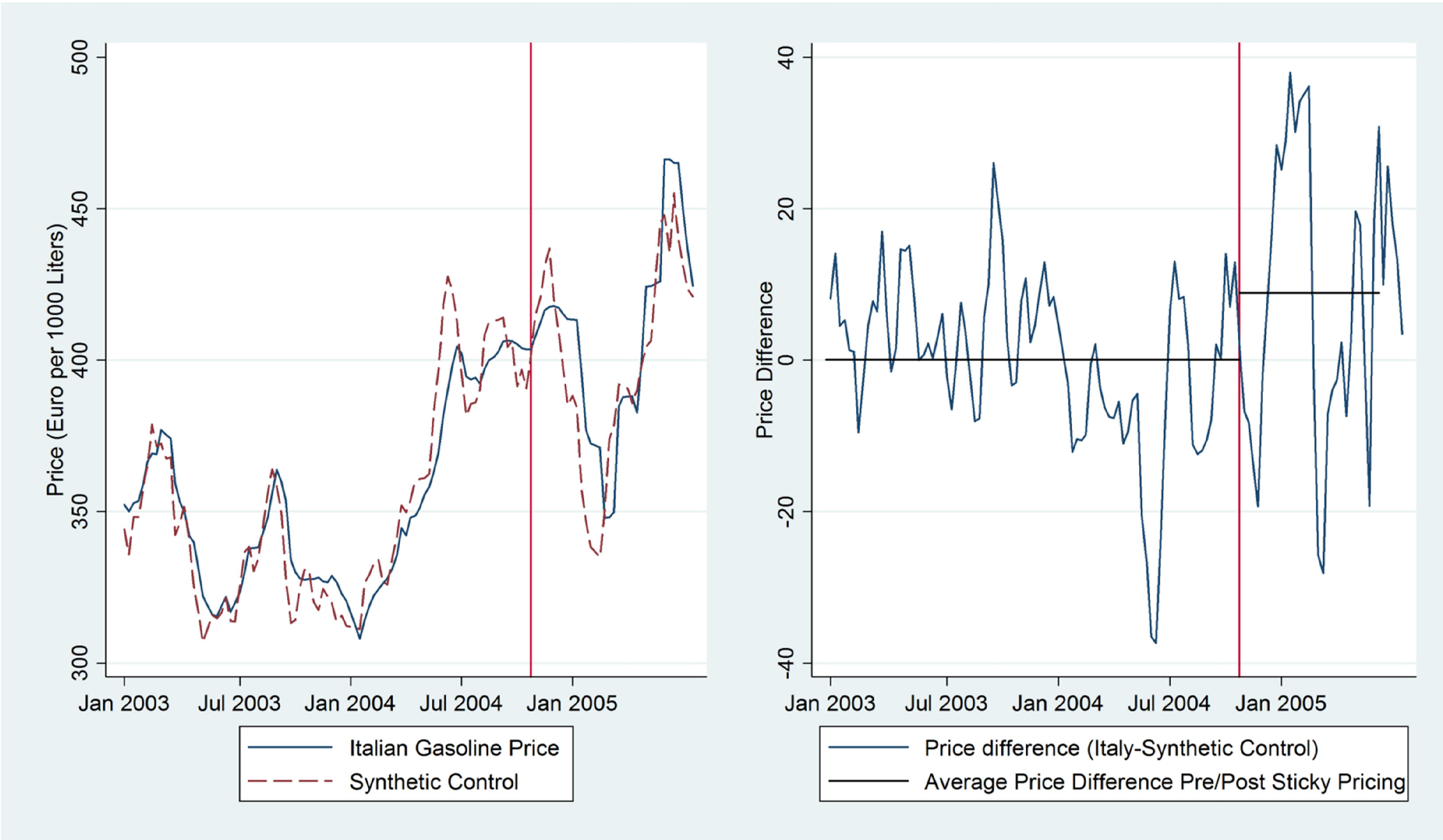

Figure 1.5 shows the mean Italian gasoline price and the synthetic control group on the left graph and their difference on the right graph. The weights to construct the synthetic control group are reported in Table 1.5. The vertical lines in both graphs denote the date where ENI announced that it would adopt a new pricing policy consisting of sticky prices (i.e. infrequent price changes).

Chapter 2: Actions Speak Louder than Words: Econometric Evidence to Target Tacit Collusion in Oligopolistic Markets

2.1 Introduction

In most markets firms quickly realize that they can earn supracompetitive profits by coordinating their market conduct. In response, antitrust policy seeks to foster "effective competition" by targeting collusive activities. The current legal framework to accomplish this goal has mainly evolved around communication as a means to reach a collusive agreement. In contrast, purely tacit collusion remains largely unaddressed by antitrust law though it may bring about the same negative welfare effects.

We argue that a crucial step forward in targeting tacit collusion could be taken through the forensic use of econometric evidence which may reveal collusive strategies. Theoretical and empirical findings on collusive behavior provide a basis for deriving clear test hypotheses to distinguish (lawful) oligopolistic interdependence from tacit collusion. Thus econometric analyses may provide quantitative evidence that firms strategically use specific elements of market conduct to (tacitly) collude. Antitrust remedies should in turn take up such instances of market behavior to tackle tacit collusion.

The paramount significance of evidence of explicit communication entails fundamental problems for the fight against cartels.[32] Communication is not a necessary condition to collude. At the heart of collusion lies the incentive of firms to cooperate rather than to compete.[33] In oligopolies firms can exercise their unilateral market power to facilitate anticompetitive coordination without engaging in communication. As firms weigh up the costs and benefits of explicit collusion, antitrust law's focus on communication incentivizes them to concentrate on tacit means of collusion. Legal instruments to counter collusion, the effectiveness of which depends on evidence of explicit communication, are least effective in concentrated industries,[34] i.e. precisely in those industries where the cartelization rate is presumably the highest and communication is least needed

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[32] Throughout this paper we use the term "cartel" to describe any kind of welfare-decreasing form of collusion, be it an explicit or a tacit one, and irrespective of whether or not we consider it an infringement of antitrust law.

[33] Much of the theoretical discussion on tacit collusion is based on the supergame approach. The best known result describing firms' incentives to collude is the "Folk Theorem" which states that for sufficiently low discount rates almost any price may be sustained as the equilibrium outcome of a repeated game. While the "Folk Theorem" provides fairly general conditions under which tacit collusion may be sustained as an equilibrium, it says nothing about how firms behave in reality. The strategies used in the "Folk Theorem" are chosen because of their analytical ease and not because they describe firms' collusive behaviour. See Fudenberg and Tirole (1991) for a discussion of the "Folk Theorem".

[34] While economic theory shows that concentration facilitates collusion, and thus predicts a positive relation between cartelization rate and market concentration, empirical evidence seems to contradict this result (Levenstein and Suslow, 2006). This gap between the number of cartels predicted from a theoretical perspective and the number of cartels that appear in the empirical analysis may plausibly be explained by a sample-selection bias. Only cartels which, first, have been detected and which, secondly, were regarded as illegal by antitrust authorities or courts are contained in the sample.

to sustain collusion.[35] Any economic approach to support the enforcement of antitrust law[36] is challenged by a legal significance of evidence of communication. Economists can use observable variables such as prices, and their knowledge of the strategies employed by firms to infer collusion[37] but have no instruments to prove whether firms collude with or without communication. From an incentive-based perspective, (illegal) communication appears to be of relative unimportance: While non-enforceable communication might facilitate coordination on a *particular* collusive equilibrium,[38] "talk is cheap" in the absence of effective enforcement mechanisms.[39]

It is, however, not out of economic naivety that antitrust law concentrates so much on evidence of communication in its struggle against collusion. Firstly, this reflects skepticism about whether instances of tacit collusion may be distinguished from oligopolistic competition with a degree of precision that suffices for forensic purposes. This concern may be associated with the so-called "indistinguishability problem" as put forward by Phlips (1996). He suggested that game theoretic arguments combined with the unavailability of some key data can make an economic based proof of collusion very difficult as something that looks like collusion might stem from a multiplicity of (indistinguishable) equilibria. Hence, the application of any legal instrument that addresses tacit collusion faces the challenge to prevent an unacceptable high number of false positives. Secondly, for purposes of antitrust enforcement it does not suffice to show that an observable market outcome emerged as the result of a collusive strategy. Antitrust remedies may not straightforwardly tackle firms because they charge "collusive", i.e. supracompetitive, prices but must address specific elements of firms' market conduct which may be characterized as collusive. Without taking into account these issues, antitrust enforcement that tackles tacit collusion risks either unduly restricting market operators' leeway to compete or to ultimately amounting to an instrument of price control.

In the following, we outline an approach that addresses both these concerns, and hence provides the basis for an expansion of the law's ambition to tackle tacit collusion. Oligopolistic interdependence *as such* and oligopolistic collusion are conceptually distinct. Tacit collusion arises from decisions endogenous to the market by one or several firms which aim at reducing or eliminating competition. In contrast, oligopolistic interdependence stems from best response to market conditions (including other firms' behavior) which favor non-competitive performance. Thus, while the market outcome might appear to be "indistinguishable,"

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35 Fonseca and Normann (2012) use a laboratory experiment to investigate the role of communication in sustaining collusion. They show that highly concentrated industries collude irrespective of communication.

36 See for comprehensive analyses of the use of economics to support cartel enforcement Werden (2004) and Kaplow (2011a).

37 One of the best known examples of economic detection of collusion is provided by the work of Christie and Schultz (1994). They detected collusion between Nasdaq market makers by comparing their bid-ask spread to the equivalent spread on the New York Stock Exchange. Christie and Schultz' (1994) work had an impressive impact as it led to regulatory investigations by the Securities and Exchange Commission (SEC) and class action lawsuits that were settled for over $1 billion.

38 Genovese and Mullin (2001) provide narrative evidence of the role of communication for collusion in the Sugar Institute Case. They find that one key missing aspect in formal theories of collusion is the role for rich communication within the collusive agreement.

39 To use the words of Thomas Hobbes (1651/1959, chap. 14, p. 71), author of the Leviathan, "[…] the bonds of words are too weak to bridle mens ambition, avarice, anger, and other Passions, without the fear of some coercive Power […]."

the specific strategies that lead to the outcome differ significantly. The gist of our approach to identify collusive behavior lies in an identification of patterns of *behavior* used by firms to bring about or facilitate (tacit) collusion.[40] Yet antitrust law must not simply infer the existence of a punishable (tacit) agreement from the insight that a certain market outcome is the result of a collusive strategy. Rather, it is essential to distinguish the active promotion of a collusive strategy by one firm from the passive (best response) alignment of competing firms. Consequently, antitrust enforcement should not conceptualize such instances of collusive leader-follower behavior as an illegal *coordination* which would – with regard to the "followers" – result in punishing oligopolistic interdependence. Rather, antitrust law should capture such instances of "unilateral collusion" only through considering as illegal the *unilateral* conduct that actively promotes the implementation of a collusive strategy. To effectively fight tacit collusion it appears therefore to be necessary to strengthen legal instruments that target the unilateral conduct that firms strategically employ to promote collusion.

To illustrate our behavioral approach to tackling tacit collusion and to demonstrate the capacity of econometric evidence we refer to incidents on the Italian gasoline market. In Andreoli-Versbach and Franck (2013b), hereafter AVF, we provide quantitative evidence of the means, i.e. specific pricing strategies, and the effects, i.e. higher prices, caused by the unilateral public announcement of ENI, the market leader. On 6th October 2004 ENI announced a new pricing policy which consisted of infrequent price variations (sticky pricing) and large price changes. Using daily firm level prices of gasoline in Italy and average weekly EU prices over the time period from January 2003 to May 2005, AVF show the effect of the new pricing policy. ENI increased the time lag between price changes from 6 to 16 days and increased the mean price change from 1% to 5.8%. After the policy change ENI did not change its price for 57 days irrespective of cost changes. Initially ENI's competitors kept their short-run cost-based pricing and thus increased their prices following (lagged) cost increases.[41] Once competitors started to align to ENI in mid-November 2004 a different pricing pattern emerged: sticky-leadership pricing. Each large price variation was matched by competitors and ENI endogenously emerged as the price leader in the market and coordinated price changes. While the first effect of the policy was to change the price interdependence in the Italian gasoline market this newly emerged tacit coordination had an additional effect: a significant price increase. Using several estimation techniques AVF show that Italian prices rose compared to EU prices after the new sticky leadership pricing emerged. Thus, the econometric analysis used to characterize pre and post policy pricing

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[40] In this respect, our approach is conceptually in line with Hay (2000, p. 128) who argues that "if there is to be a category of unlawful tacit collusion which is to be distinguished from classic oligopoly, the difference must lie […] on the specific elements of behavior that brought about that state of mind".

[41] Firms respond to cost shocks with some lags. While current costs decreased immediately after ENI's policy, lagged costs increased and thus competitors increased their prices. See **Figure 2.1** for a plot of daily prices and costs, i.e. Platts Cif. Med., around ENI's new price policy announcement (first vertical line).

behavior and evaluate the effect of the new market conduct on the price level might provide solid "statistical" evidence that ENI's unilateral commitment to a policy of sticky pricing has to be characterized as collusive.

Against the background of these incidents on the Italian gasoline market we suggest that an implementation of sticky pricing along with large price changes should be prohibited under market conditions such as highly asymmetric market shares and high concentration where it may be expected that price leadership will emerge as a price coordination mechanism and, thus, where such a pricing strategy will bring about collusion. Such an expansion of the legal tools to counter cartels seems especially relevant for oligopolies where the structural market features favor collusion and at the same time communication might be less needed because of price and cost transparency.

The structure of the paper is as follows: Section 2 discusses the status quo of cartel enforcement which focuses on firms' communication and the law's difficulties with tackling tacit collusion. In section 3 we outline incidents on the Italian gasoline market as an illustration for how our approach might be applied for purposes of antitrust enforcement. Section 4 describes the way to integrate quantitative evidence of collusion with antitrust law. Section 5 concludes.

2.2 On Collusion as a Legal Concept, its Limits in the Absence of Evidence of Collusive Communication, and the Reasons therefor

Collusion allows competing firms to charge supra-competitive prices and entails negative welfare effects. Meta-studies on cartel overcharges show that the median cartel-price increase ranges between 20 and 30 percent (Bolotova, 2009, and Connor, 2007). This is why antitrust law aims at inhibiting collusion and why the horizontal coordination of prices and quantities is considered a per-se violation of Section 1 Sherman Act or Article 101 Treaty on the Functioning of the European Union (TFEU), respectively. Successful collusion requires *inter alia* an underlying – tacit or explicit – consensus on the terms of the cooperation. Thus, in order to counter collusion, it seems a logical step to regard such underlying understanding as illegal.

However, the economic conception of a collusive agreement diverges significantly from the corresponding legal concepts of "conspiracy" according to Section 1 Sherman Act or "agreement" and "concerted practice" according to Article 101(1) TFEU.[42] While the former focuses on firms' incentives to engage in collusion and their strategies for sustaining a collusive equilibrium, the latter centers around the means to reach an understanding between firms. This divergent perspective on collusion becomes apparent with regard to instances of tacit collusion, i.e. under circumstances where no direct evidence of consensus between competing firms is available, such as written records or insider testimony. Though, as a matter of principle,

[42] This conceptual divergence may also give rise to *terminological* misunderstandings between economists and lawyers. Throughout this paper we will indicate when we use terms such as "collusion" or "agreement" in their technical economic or legal meaning.

both under the Sherman Act and the TFEU circumstantial evidence may suffice to demonstrate the existence of a "conspiracy"[43] or an "agreement"[44] respectively, there are doctrinal limits in this regard if it comes to (supposedly) tacit collusion between competitors.

In the words of the U.S. Supreme Court, "conspiracy" requires "that [the defendants] had a conscious commitment to a common scheme designed to achieve an unlawful objective."[45] Reasonably, this may not be inferred from conscious parallelism alone.[46] Rather a plaintiff has to produce additional evidence to prove that an observed parallel market conduct may not be considered the result of oligopolistic interdependence, but indeed forms part of a collusive strategy. Such so-called "plus factors" may encompass first, elements of industry structure that indicate that an industry is conducive to collaboration, second, conduct that appears irrational or inefficient absent collusion, and third, additional factors such as industry performance (e.g. stabile market shares over time, supra-competitive pricing) or facilitating practices (e.g. exchange of information).[47] While the U.S. Supreme Court has stated that plaintiffs can only survive summary judgment by presenting circumstantial evidence "that tends to exclude the possibility that the alleged conspirators acted independently,"[48] the case law so far does not provide a taxonomy of plus factors which would allow us to determine which elements of evidence are required to infer an agreement. Thus, Gavil et al. (2008) concluded that "[…] decisions analyzing plus factors generally have failed to establish a clear boundary between tacit agreements – to which Section 1 applies – and parallel pricing stemming from oligopolistic interdependence […]. This condition makes judgments about future litigation outcomes unpredictable."[49]

While the European Court of Justice (ECJ) considers it generally conceivable that consent to an agreement may be inferred from circumstantial evidence,[50] the Court is reluctant to infer an "agreement" between competitors from their market conduct alone, notwithstanding the presence of certain "plus factors." Given the current state of the jurisprudence, it appears that in the absence of direct evidence of collusion the Court

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[43] *American Tobacco Co. v. United States,* 328 U.S. 781, 809 (1946) ("No formal agreement is necessary to constitute an unlawful conspiracy"); *Norfolk Monument Co. v. Woodlawn Memorial Gardens, Inc.,* 394 U.S. 700, 704 (1969) ("business behavior is admissible circumstantial evidence from which the fact finder may infer agreement").

[44] CFI, 26.10.2000, Case T-41/96 *Bayer v Commission* [2000] ECR II-3383 para. 69; confirmed on appeal by the ECJ, 6.1.2004, Joined Cases C-2/01 P and C-3/01 P *Bundesverband der Arzneimittel-Importeure and Commission v Bayer* [2004] ECR I-23, para. 97.

[45] *Monsanto Co. v. Spray-Rite Servs. Corp.,* 465 U.S. 752, 768 (1984). While *Monsanto* involved a vertical collaboration, the Court soon after adopted the same reasoning also in a horizontal case, see *Matsushita Electronics Industries Co. v. Zenith Radio Corp.,* 475 U.S. 574, 588 (1986).

[46] See e.g., *Theatre Enters., Inc. v. Paramount Film Distrib. Corp.,* 346 U.S. 537, 541 (1954).

[47] See for an overview Gavil et al. (2008), pp. 310-311.

[48] *Matsushita Electronics Industries Co. v. Zenith Radio Corp.,* 475 U.S. 574, 588 (1986) (quoting *Monsanto Co. v. Spray-Rite Servs. Corp.,* 465 U.S. 752, 764 (1984)).

[49] See also Kaplow (2011b), p. 816, who concludes after an extensive analysis of the concept of agreement in antitrust law: "[…] this Article does not come close to demonstrating that it would be good policy to proscribe and highly penalize all instances in which interdependent oligopolistic behavior appears to occur. The design of optimal policy is not dictated by definitions but rather by direct assessment of the consequences of different regulatory approaches."

[50] Accordingly, the Court infers a tacit approval of a collusive initiative from the attendance of a meeting where an anticompetitive agreement was concluded, see ECJ, 28.6.2005, Joined Cases C-189/02 P, C-202/02 P, C-205/02 P to C-208/02 P and C-213/02 P *Dansk Rørindustrie A/S and others v Commission* [2005] ECR I-5425 para. 143: "That complicity constitutes a passive mode of participation in the infringement which is therefore capable of rendering the undertaking liable in the context of a single agreement […]."

does not presume the existence of an "agreement" even if one has proved that observed parallel market conduct was an expression of (tacit) collusion rather than of oligopolistic interdependence as such. This has been reaffirmed by a decision on the doctrine of "collective dominance" under Article 102 TFEU where the ECJ implicitly approved that tacit collusion *per se* may not fall under Article 101(1) TFEU: "[u]nless they can form a shared tacit understanding of the terms of the coordination, competitors might resort to practices that are prohibited by Article [101 TFEU] in order to be able to adopt a common policy on the market."[51] However, where tacit collusion has been induced by facilitating practices such as, for example, an exchange of information, it may come under Article 101(1) TFEU as an illegal "concerted practice". In this regard, the ECJ drew a line: On the one hand, by assigning market operators the legal leeway to "adapt themselves intelligently to the […] conduct of their competitors" the Court signaled that mere *passive alignment* would not be treated as an illegal form of coordination. On the other hand, the Court submitted that a strategy that *actively aims at aligning* competitors' market conduct may fall under Article 101(1) TFEU.[52] Thus, to implement this standard it is essential to identify elements of behavior that promote (tacit) collusion.

This insight into legal concepts of coordination reveals ambiguities and restrictions with regard to tacit collusion. It raises the question why the law finds it so difficult to cope with this phenomenon, given that it seems uncontroversial in terms of competition policy that tacit collusion on prices and quantities should be prevented as rigorously as collusion based on explicit consensus. To begin with, the respective judicial definitions of "conspiracy" and "agreement" do not restrict these concepts in a way that would exclude collusion which has been sustained tacitly. Whatever the rhetoric of the courts might be when they characterize the requirements of an agreement – typically they refer to a need to show a "meeting of minds,"[53] a "joint intention"[54] or a "concurrence of wills"[55] –, the respective antitrust law concepts have to be defined strictly instrumentally. Hence it is, first, the underlying policy to contain as far as possible any kind of welfare-reducing collusion and, second, the role a legal intervention and, in particular, a prohibition of agreements between competitors may feasibly play in this regard, that determine which behavior should be regarded as illegal.

Part of the law's problem in coping with tacit collusion lies with the difficulty to distinguish collusion from oligopolistic interdependence as the latter may also result in suspiciously parallel market conduct and supra-competitive prices. This problem is addressed by the requirement of "plus factors" which – in addition to parallel pricing – are meant to indicate collusion, such as market conduct which may reasonably only be

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[51] ECJ, 10.7.2008, Case C-413/06 P *Bertelsmann and Sony Corporation of America v Impala* [2008] I-4951, para. 123.

[52] ECJ, 16.12.1975, Joined Cases 40 to 48, 50, 54 to 56, 111, 113 and 114/73, *Suiker Unie and others v Commission* [1975] ECR 1663 paras. 173-174; ECJ, 14.7.1981, Case 172/80 *Züchner v Bayerische Volksbank* [1981] ECR 2021, paras. 12-14.

[53] *American Tobacco Co. v. United States,* 328 U.S. 781, 810 (1946); *Copperweld Corp. v. Independence Tube Corp.,* 467 U.S. 752, 771 (1984).

[54] ECJ, 15.7.1970, Case 41/69 *ACF Chemiefarma* [1970] ECR 661 para. 112.

[55] CFI, 8.7.2008 *AC-Treuhand* [2008] ECR II-1501, para. 118.

explained as part of a collusive strategy.[56] From this perspective, the problem of distinguishing oligopolistic collusion from oligopolistic competition comes essentially down to a question of error costs: by defining the "critical mass" of plus factors required to infer an illegal coordination, courts strike a balance between the ambition to contain (tacit) collusion and the risk of producing false positives.[57]

However, in particular the ECJ's categorical reluctance to infer an agreement in cases of mere tacit collusion suggests that there is more to the law's difficulties to cope with tacit collusion than the problem of multiple (indistinguishable) equilibria and the issue of reaching an acceptable degree of error costs in this regard. Legal standards and remedies that are supposed to influence market conduct in order to guarantee effective competition may not simply prohibit an undesired economic condition such as a collusive equilibrium and punish firms because they charge "collusive" prices. Such a policy effectively meant nothing other than price control. This unwelcome consequence is prevented as antitrust standards and remedies relate to individual behavior and define which acts or omissions are required or prohibited. When authorities or private plaintiffs order a firm to bring an infringement to an end or seek to obtain injunctions before a court, it is already the remedy's behavioral nature that requires a specification of elements of conduct that violate antitrust law. The intended deterrent effect of concurring remedies such as imposing fines or damages likewise depends on whether market operators are in a position to foresee what conduct they may be sanctioned for, and how they are expected to behave to avoid sanctions. This appears particularly challenging where an undesired economic effect or market condition is the consequence of the interdependent behavior of several market actors.[58] But once again: if the elements of behavior that bring about a collusive equilibrium remain unclear, any legal intervention may ultimately amount to a price control by antitrust authorities or courts. Furthermore, with regard to criminal and quasi-criminal sanctions it is required by the principle of culpability[59] and the need to prove intent[60] or negligence,[61] respectively, that antitrust enforcement ensures that market operators may

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[56] This is presumed if, for example, a certain conduct "is so perilous when not imitated and imitation so uncertain that no reasonable actor would so act, then parallel action does imply some exchange of commitments or at least some comforting assurances connoting a traditional conspiracy", Areeda and Hovenkamp (2010), §1415c, p. 107 with reference to *Blomkest Fertilizer v. Potash Corp.,* 203 F.3d 1028, 1037 (8th Cir. 2000).

[57] See Posner (2001), p. 99: "[…] a damages judgment in a tacit collusion case would promote competition at a tolerable cost in legal uncertainty and judicial supervision."

[58] Cf., e.g., *E.I. Du Pont De Nemours & Co. v. FTC (Ethyl),* 729 F.2d 128, 139 (2d Cir. 1984): "In view of this patent uncertainty the [Federal Trade] Commission owes a duty to define the conditions under which conduct claimed to facilitate price uniformity would be unfair so that businesses will have an inkling as to what they can lawfully do […]. The Commission's decision in the present case does not provide any guidelines; it would require each producer not only to assess the general conduct of the antiknock business but also that of each of its competitors and the reaction of each to the other, which would be virtually impossible."

[59] Under European law, Article 7(1) ECHR enshrines the principle that offences and punishments are to be strictly defined by law, see on the relevance of this norm as to fines in EU Competition Law ECJ, 28.6.2005, Joined Cases C-189/02 P, C-202/02 P, C-205/02 P to C-208/02 P and C-213/02 P *Dansk Rørindustrie A/S and others v Commission* [2005] ECR I-5425 para. 202.

[60] Cf. 438 U.S. 422, 435 (1978): "We agree with the Court of Appeals that an effect on prices, without more, will not support a criminal conviction under the Sherman Act […]. [A] defendant's state of mind or intent is an element of a criminal antitrust offense which must be established by evidence and inferences drawn therefrom, and cannot be taken from the trier of fact through reliance on a legal presumption of wrongful intent from proof of an effect on prices." As to the required standard of intent the Court concluded id., at 444, "that action undertaken with knowledge of its probable consequences and having the requisite anticompetitive effects can be a sufficient predicate for a finding of criminal liability under the antitrust laws."

[61] See Article 23(2)(a) Council Regulation (EC) No 1/2003 of 16 December 2002 on the implementation of the rules on competition laid down in Articles 81 and 82 of the Treaty, Official Journal L 1, 04.01.2003, p. 1-25.

anticipate their legal leeway and addresses certain modes of behavior rather than an economic effect or condition.

Thus, the key to overcoming the law's difficulties to counter tacit collusion lies in an approach which identifies specific elements of behavior whose object or effect it is to bring about or facilitate collusion. Such an approach has a chance for success as market operators that seek to implement a collusive strategy need to adjust their market conduct to reach an optimal and stable collusive equilibrium. Even in oligopolistic markets that are characterized by features that facilitate tacit collusion, prices and other parameters have to be adjusted according to an underlying (tacit) agreement, and the need for such adjustments may lead firms to resort to a certain behavior that may be identified as serving a collusive strategy. Empirical and theoretical research[62] on how cartels behave provides solid test hypotheses to identify such elements of collusive behavior. Precisely these elements of behavior are the focus of our approach to provide evidence of anticompetitive behavior.

2.3 Empirical Evidence

Academic forensic economics and finance[63] has long applied its tools in a number of areas to reveal conduct that agents strive to conceal. Some of the most prominent examples include teachers cheating in exams (Jacob and Levitt, 2003), violations of U.N. sanctions (DellaVigna and La Ferrara, 2010), and racial biases in employment decisions (Bertrand et al., 2005). This research is methodologically related to our topic of empirical cartel detection as econometrics is employed to provide evidence of hidden wrongdoings. In academic forensic economics and finance researchers use their knowledge about incentive schemes on observable variables, e.g. prices, in order to derive statistical tests to compare distinct hypotheses, e.g. collusion versus competition.

While a test hypothesis for teachers to raise students' test scores or employment discrimination on the basis of race can be clearly defined, what should constitute an appropriate test for collusion? In line with the literature on economic screens (see Abrantes-Metz and Bajari, 2009) we believe that the answer lies in economic theory and empirical evidence on cartel behavior.[64]

Since the foundational work by Stigler (1964) who highlighted firms' incentive to cheat as the preeminent challenge faced by cartels, much research has been carried out on "pricing structures" which can sustain a collusive outcome.[65] The two key strategic aspects that are relevant for our analysis are the use of sticky and

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[62] For a meta-study on cartels' features see Harrington (2006) and Levenstein and Suslow (2006). For a survey on price fixing in particular see Hay and Kelley (1974). For an analysis of the determinants of cartel duration see Levenstein and Suslow (2011).

[63] For a review of forensic economics and finance see Zitzewitz (2012) and Ritter (2008), respectively.

[64] See for example Bajari and Ye (2003) who develop an approach to identify and test for bid rigging in procurement auctions. For a general discussion of methods to detect collusion see Porter (2005), Harrington (2008b) and Rey (2007).

[65] See, for example, Green and Porter (1984), Rotemberg and Saloner (1986) and Maskin and Tirole (1988).

leadership pricing as a facilitating device to sustain collusion. With respect to price leadership we base our analysis on the models developed by Rotemberg and Saloner (1990) and Mouraviev and Rey (2011). With respect to price stickiness we rely on theoretical findings by Athey and Bagwell (2001, 2008), Athey et al. (2004), Hanazono and Yang (2007) and Garrod (2012), and empirical insights by Abrantes-Metz et al. (2006), Blanckenburg et al. (2012) and Connor (2005) who show that price stickiness is associated with collusive behavior.

2.3.1 The Facts of the Case

On 6th October 2004, ENI, the market leader in the Italian gasoline market, publicly announced the adoption of a new pricing policy. ENI declared that the purpose of this policy was to lower the short-term price-cost relation and to stabilize retail prices.[66] As the volatility of crude oil was increasing, ENI claimed that the policy aimed at lowering the retail price variability and would benefit customers. The new policy consisted in a reduction of the number of price changes (i.e. sticky pricing), and increased the magnitude of each variation. ENI increased the average time lag between price changes from 6 to 16 days and increased the mean price change from 1% to 5.8%. The result of this declaration can be seen in **Figure 2.1**, which shows the daily price per company over time before and after the new pricing policy was introduced. Before the policy firms' price changes were frequent. On average, firms changed their prices every five days. The average price change was .8% before the policy change. After the new pricing policy was introduced, price changes occurred infrequently, on average every 9 days, but their amount became larger, namely 2.9% on average.

As a result, all but one competitor, ERG,[67] followed ENI's new pricing strategy. About five months later in March 2005, the Italian Truckers' Association, FITA, complained to the Italian antitrust authority about high and aligned prices.[68] This eventually led to an investigation by the antitrust authority for price fixing under Article 14 of Law 287/90 of 10 October 1990, the Italian legislation which restates Article 101 TFEU. The Italian antitrust authority claimed that the petrol firms' conduct of adapting their prices to the leader's price had to be considered a collusion to stabilize prices and to coordinate price changes.[69] The high transparency in the market facilitated an exchange of price information. Firms may easily observe their competitors' prices at each gas station and Italian law required weekly price communications to the Ministry of Industry which subsequently published the data. In addition and more importantly, companies communicated future price

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[66] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VI, para. 42, available at http://www.agcm.it.

[67] ERG publicly declared that it would not follow ENI's new pricing and stick to their own method which it did not further specify, see Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VI, para. 41.4, available at http://www.agcm.it.

[68] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section I, para. 1, available at http://www.agcm.it.

[69] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VIII, paras. 58 and 59, available at http://www.agcm.it.

changes to a specialist Italian magazine, "Staffetta Quotidiana," which published all price change announcements on its website. Cost transparency also facilitated coordination. The major source of cost is the Platts Cif Med,[70] the wholesale price refineries charge in the Mediterranean area for gasoline. This price can be thought of as the opportunity cost of companies to sell their gasoline on the Mediterranean wholesale market rather than to gas stations. It thus constitutes industry practice[71] to compute firms' margins as the difference between their (suggested) consumer price and the Platts Cif Med.

It is important to note that the antitrust authority had no proof of direct communication between the firms, other than the price changes the firms communicated via the aforementioned online magazines. The authority claimed that ENI's policy created a focal price used to facilitate coordination. ENI's sticky pricing lowered competitors' uncertainty about the future pricing while the large price variations helped to coordinate price changes.[72]

2.3.2 Sticky Pricing

Sticky pricing constitutes an important element in a strategy to sustain collusion. An advantage of rigid pricing is that it is straightforward to implement and that deviations can be easily detected and punished. A series of studies (Athey and Bagwell, 2001, 2008, Athey et al., 2004, Hanazono and Yang, 2007, and Garrod, 2012) analyze the profit maximizing scheme of cartels under different settings and find a direct relation between optimal collusive schemes and rigid pricing.[73] For example, Athey and Bagwell (2008) show that when firms are moderately patient the equilibrium that maximizes ex-ante profits is relatively simple: all firms adopt a sticky pricing scheme and charge the consumers' reserve.[74] In this equilibrium colluding firms adjust their prices infrequently, and thus sacrifice productive efficiency to sustain a higher price level in the market.

In fact most empirical studies conclude that prices are more rigid when the industry is in a collusive phase (Abrantes-Metz et al., 2006, Blanckenburg et al., 2012, and Connor, 2005). A key example is the study by Abrantes-Metz et al. (2006) on the frozen perch market. Using ex-post evidence of collusion the authors find that the price variance during collusion was indeed distinctly lower than the price variance in the period after the end of the cartel. In a meta-study Blanckenburg et al. (2012) compare the distribution of price changes between competition and collusion for 11 cartels. They find that the price variance decreased significantly in 8 out of 11 examined cartels.

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[70] The Platts company is a leading global provider of energy information that collects and publishes on a daily basis details on the prices of bids and offers for specialized oil products and regions from traders and exchange platforms.

[71] See, for example, the definition of the gross margin by the Italian Petrol Union who defines it as the difference between the retail price net of taxes and the Platts Cif Med, available at http://www.unionepetrolifera.it/it/show/34/La%20struttura%20del%20prezzo.

[72] Autorità Garante della Concorrenza e del Mercato, 18.1.2007, Case I681 – *Prezzi dei carburanti in rete,* Provvedimento no. 16370, Section VIII paras. 60-63; available at http://www.agcm.it.

[73] Rigid pricing is defined as firms pricing independently of their current cost position.

[74] With other parameter configuration other (more complex) type of equilibria are possible.

A shortcoming of sticky pricing models is that they do not address *how* colluding firms react and coordinate to exogenous cost and demand changes. In section 3.3 we will describe how firms in our case used the leader's price as the focal price.

2.3.3 Leadership Pricing

Price leadership is "one of the most important institutions facilitating tacitly collusive pricing behavior" (Scherer and Ross, 1990, p. 346). Theoretical evidence has been presented by Rotemberg and Saloner (1990) who demonstrate that price leadership facilitates collusion under asymmetric information and that it increases price rigidity. The authors conclude that such a pricing scheme has many positive attributes: First, it is easy to implement, second, it doesn't require communication and third, it is very easy to detect (and punish) deviations.

In line with these findings Mouraviev and Rey (2011) study the role of price or quantity leadership under circumstances where firms can act either simultaneously or sequentially in an infinitely repeated setting for both Bertrand and Cournot competition. They highlight that leadership facilitates collusion. Firms competing on prices a la Bertrand can use price leadership to sustain (perfect) collusion for any value of the discount factor while leadership is less effective with quantity competition a la Cournot.

Both papers convey an important implication for antitrust policy: if firms are able to tacitly collude using price or quantity leadership, the negative effects on welfare are essentially the same compared with cases of explicit collusion. The way firms collude is not decisive for the negative effect collusion has on consumers' welfare. In addition, both papers show how leadership pricing can be used to implement an anticompetitive strategy in the market as it facilitates coordination and makes deviation more visible.

2.3.4 Key empirical findings

Based on the previous finding of the role of sticky and leadership pricing to sustain collusion and on the effects of collusive agreements on prices, we show that ENI's pricing behavior facilitated price coordination and led to a price increase.

Table 2.1 shows the different pricing conduct firms adopted after ENI's price commitment. In Panel A we summarize the frequency and magnitude of price changes. Columns 3 and 5 show the differences in the pre and post mean of these variables and thus test whether the pricing behavior significantly changed after ENI's policy. ENI significantly increased the time lag between price changes from one every 6 days to one every 16 days. This difference is significant at the 1% level and shows that ENI did hold its price commitment as publicly announced on 4th October 2004. In addition, the leader increased the absolute mean price change from 1% to 5.8%. This 4.8% increase is statistically significant at the 1% level. Similar results hold true for all firms. The average time lag between price changes increased from five to nine days, while absolute price changes increased from .8% to 2.9%, both significant at the 1% level. Theoretical literature discussed above suggests that large price changes might have been used to coordinate price changes on the leader's focal price. Panel B tests this hypothesis and shows whether the average number of perfectly aligned competitors (i.e. up to three digits) to the leader and the average price difference of competitors to ENI significantly changed after ENI's new pricing policy. In line with the collusive hypothesis the number of aligned competitors significantly increased and the average price difference to the leader significantly decreased after the policy.

In addition to the price coordination adopted by firms we report the key coefficients on the causal effect of the policy on prices and margins from AVF in **Table 2.2**. Specification 1 shows the result of the dif-in-dif model with standard errors clustered at the country level. In this regression weekly prices of eight EU countries[75] were used as a control group.[76] The estimate on the dif-in-dif effect of the policy on Italian prices is positive and highly significant. As one might question the subjective selection and the sufficient similarity of the control group, in specification 2 AVF first construct an "optimal" data-driven benchmark (i.e. a synthetic control group) and then take the weekly difference between the Italian price and the "optimal benchmark" as the (stationary) dependent variable. The synthetic control group estimation was developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) and is constructed using a data-driven weight of European prices that minimizes the pre-treatment differences between the Italian price and the resulting synthetic control group. Consistent with specification 1 we find a positive and significant effect of the policy on prices. Finally, specification 3 shows the within market regression of firm-level margins (i.e. without benchmark) which also points to a positive and significant effect of the new policy on firms' profits.[77]

The results of the econometric analysis show that ENI's policy had two effects: first, it facilitated price coordination and second, it increased average prices.

2.3.5 Discussion and Robustness of the Empirical Results

In oligopolistic markets the way firms interact with their competitors determines their profits. Our empirical analysis shows that the *ex-post* effect of the leader's (credible) commitment to sticky pricing was an equilibrium with higher prices.

ENI's success in the implementation of a collusive scheme depended on the individual incentives for its competitors to adhere. The first issue that arises, therefore, is whether it is reasonable to think that the leader could expect *ex ante* that its competitors would adopt its pricing and that this would cause an increase in prices.

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[75] EU countries differ with respect to Italy, e.g., in the number of gas stations owned by hypermarkets that compete aggressively to attract customers to their stores. Using state-level data of U.S. gasoline prices, Zimmerman (2012) shows the positive competitive impact of hypermarket retailers. The dif-in-dif analysis as carried out in AVF assumes that "market trends" would be the same in the treatment and control group while structural country specific market differences are captured by the fixed effects.

[76] For a plot of average weekly prices in Italy and the EU and the Brent see **Figure 2.2**.

[77] Both specification (2) and (3) were performed using robust standard errors.

Firms' behavior is a key element of managerial choice. Spagnolo (2005) shows that typical compensation schemes for CEOs are designed to incentivize tacit collusion at the cost of "income smoothing".[78] In addition, managers are aware of or are at least well-advised of strategic behavior that favours collusion.[79] Since the seminal work by Schelling (1960) it is common knowledge that commitment lies at the heart of strategic behavior.[80] If competing firms could write enforceable contracts on prices, most industries would collude. However, as explicit collusion is illegal and the decision to communicate is endogenous, firms may opt for tacit collusion instead. Yet any collusive strategy must be incentive compatible, irrespective of whether it is implemented explicitly or tacitly. After its announcement on 6th October 2004, ENI kept prices fixed until 3rd December 2004 (57 days), see **Figure 2.1**. This means that ENI kept sticky prices for almost 10 times the usual price-change interval (6 days) irrespective of cost changes. Just after ENI's announcement costs increased and its competitors kept cost-based pricing. As costs fell again competitors started to align to ENI at the beginning of November, i.e. about a month after ENI's change in pricing policy. We can only speculate about what would have happened if costs *had risen* after ENI's announcement. However, it clearly emerges both from **Figure 2.1** and from the price-interdependence analysis that ENI strongly committed itself to sticky pricing. As can be inferred from **Table 2.2**, specification (3), ENI's competitors' behaved in their best interest as industry margins increased. ENI emerged *endogenously* as the price leader through its use of market power and then used its position to coordinate the price changes of its competitors, which ultimately caused a price increase. While each market has its traits and results from an individual market cannot be easily generalized, leadership pricing has been consistently associated with collusion. The empirical results of AVF provide large evidence that ENI's strategy aimed at coordinating and increasing prices at the expense of consumers.

A second concern which arises is where to set the boundaries between a firm's freedom to set its profit-maximizing price on the one hand, and antitrust authorities' power to prevent certain behaviour that results in supra-competitive pricing on the other. To address this issue we need to distinguish the "source" of market power which made that market outcome possible. In this respect it is helpful to compare our empirical results with Borenstein et al. (2002) who analyse inefficiencies in the restructured Californian electricity market. They find that wholesale electricity expenditures increased in the summer of 2000 with respect to the summer of

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[78] Spagnolo (2005) focuses on the role of observable CEO compensation schemes with regard to tacit collusion. He concludes that "a strong pro-collusive effect may well outweigh agency costs and transform apparently puzzling compensation practices into profitable 'governance' instruments."

[79] One of the standard textbooks used in MBA courses that deal with competitive strategy is "Economics of Strategy" by Besanko et al. (2010). Chapters 9 and 10 extensively deal with the issues of "Strategic Commitment" and "The Dynamics of Pricing Rivalry", respectively, which are key elements to sustain collusion. Under the heading "The golden age of micro", the journal "The Economist" discussed in its issue of 19th October 2012 why leading academic microeconomists are top advisers at firms such as Microsoft and Amazon.

[80] Maskin and Tirole (1988) build commitment in a dynamic Bertrand model through exogenous costs such as menu costs. They show that sticky prices can serve as a commitment device to sustain higher prices than under static Bertrand. Recently, Wang (2009) studied firms' pricing strategies in a gasoline market before and after the introduction of a law which regulated firms' timing of price changes. As a result, he highlighted the importance of short-run price commitment in tacit collusion.

1999 from $2.04 billion to $8.98 billion and that about 59% of this increase was caused by the exercise of unilateral market power.

Both the Italian gasoline market and the Californian electricity market suffered from higher prices. However, there is a key difference: in California market power stemmed from exogenous shocks. Electricity prices were relatively low compared to a benchmark in 1998 and 1999 but dramatically increased in the summer of 2000. While there are many structural factors that make it easy for electricity firms to exercise market power, such as binding constraints at peak times or difficulties to forecast demand and high storage costs, firms did not actively implement a new strategy to coordinate and increase their prices but rather *individually* best-responded to shocks which favored the exercise of market power. Among many factors Borenstein (2002) identifies that 2000 was a very dry year which reduced hydroelectric production, economic growth throughout the western United States increased demand for energy, and the price of nitrogen oxide pollution permits increased from about $1 per pound to over $30 per pound which increased the price of gas.

In the Californian electricity market regulation should address the structural problems which have been revealed by the incidents in the summer of 2000. However, insofar as the firms only best-responded to exogenous shocks, their conduct should not be addressed by cartel enforcement. In contrast, our analysis reveals the active implementation of a collusive strategy by one firm which resulted in an anticompetitive market outcome and thus, should be targeted by antitrust enforcement.

2.4 Integrating Economic Insights on Collusive Strategies into the Legal Framework

As any collusion between competitors may result in welfare losses, it is essential to strive to contain collusive behavior irrespective of direct evidence of a "meeting of minds" or explicit communication between firms. It remains, however, an outstanding question how economics may be integrated with the legal framework and how antitrust law should be developed to counter tacit collusion.

There are several reasons to believe that this challenge deserves more attention than ever. First of all, prevalence of tacit collusion may increase in times of globalization. Information on competitors' actions as capacity choices, prices and transactions are widely reported by international media and thus, transparency increases. Firms interact on many markets which increases their scope to collude. Secondly, market players must not be regarded as naïve, but as professionally advised and capable of employing economic know-how strategically to avoid price wars, and to reach collusive equilibria instead. Thirdly, the introduction of leniency or other types of immunity programs increased the capability of antitrust authorities to produce direct evidence of collusion such as documents or insider testimony, and thus has significantly strengthened the effectiveness of the law to counter collusive behavior that occurs via explicit communication.[81] As the decision to communicate is endogenous to market players, leniency programs have increased firms' cost of following such a strategy. This is likely to cause or to have already caused a shift from explicit to tacit collusion.

These are grounds to expect that social welfare damage caused by tacit collusion will increase. Legal instruments that are supposed to work preventively against collusion such as merger control or (quasi-)regulatory mechanisms which address the unwanted effects of collusion will hardly suffice to counter tacit collusion effectively. It appears to be crucial, therefore, that antitrust law finds a way to target those elements of behavior that are employed by firms to implement a collusive strategy and whose collusive character may be demonstrated by the kind of analysis as suggested in this article. Inasmuch as it appears inadequate to regard such behavior as an illegal *coordination*, this calls for a development of the law against *unilateral* anticompetitive conduct.

2.4.1 "Unilateral Collusion" and Unlawful Coordination

Price leadership may serve as a mechanism to find a consensus about the collusive price, a challenge any cartel faces. However, since leader-follower behavior may equally be the result of oligopolistic competition, its mere observation must not suffice to infer a collusive agreement. This raises the question of whether under circumstances such as those in the present case, i.e. where it may be demonstrated that leader-follower behavior sustained a collusive equilibrium, such conduct should be considered illegal. In other words, should the kind of evidence presented herein be regarded a "super plus factor"[82] that allows courts to infer an illegal (tacit) agreement?

If certain conduct of two or more firms is conceptualized as an unlawful *coordination*, i.e. a violation of, for example, Section 1 Sherman Act or Article 101(1) TFEU, this implies that the law regards the behavior of these firms as a wrongdoing which may be punished. In other words, where a certain collusive equilibrium has been brought about by the unilateral collusive conduct of one firm, one should only infer a punishable agreement if one also considered the competitors' reactions as inappropriate behavior. Turning again to the general regulatory and legal requirements we formulated above with regard to antitrust enforcement,[83] we may recall that antitrust standards and remedies should address specific elements of behavior and that market operators should be provided with an idea of which conduct may be regarded as acceptable or not acceptable under defined market conditions. Such standards of conduct must be in line with the general purpose of

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[81] For a theoretical discussion of leniency see Motta and Polo (2003) and Harrington (2008a). Empirical evidence on the effects of leniency is provided by Miller (2009).

[82] Kovacic et al. (2011), p. 435, offer a list of "super plus factors" which includes *inter alia* "[a] reliable predictive econometric model that accounts for all material noncollusive effects on price, estimated using benchmark data where conduct was presumed noncollusive, produces predictions of prices that do not explain the path of actual prices in the period or region of potential collusion, at a specified high confidence level."

[83] See supra section 2.

antitrust law to foster effective competition. Thus, if tacit price alignment in response to unilateral collusive conduct ought to be prohibited, the law has to define how firms should behave once a competitor's conduct may be interpreted as a (tacit) invitation to engage in (tacit) collusion. When ENI held prices constant despite of cost increases and thereby signaled its commitment to a policy of sticky pricing, this might be viewed as a "suggestion" to its competitors to align their pricing policy and as an "offer" to take on the role as price leader. Should ENI's competitors have been legally obliged to refrain from any market conduct that ultimately could have been regarded as having brought about a collusive equilibrium and thus proof of an underlying illegal agreement?

It seems not feasible to define any meaningful and administrable legal standard of conduct in this respect. Should it have been forbidden for ENI's competitors to tacitly align their prices to ENI's policy of sticky pricing? Should they have been obliged to stick to their higher prices and with open eyes to put up with losing market share? And even if an alignment of pricing to the strategy of a price leader such as ENI was prohibited, the question would remain *how closely* and *how quickly* a competitor would be allowed to adjust its market parameters. In the absence of any clear standard of behavior, a legal intervention in situations of (supposedly) collusive pricing may ultimately amount to judicial price regulation. In addition, under such a legal regime a market player could strategically restrict the competitive room for manœuvre of its competitors: if it was prohibited for ENI's competitors to align its pricing to ENI's strategy because such an alignment would be regarded an illegal coordination, ENI could have restricted the price-setting freedom of its competitors by implementing its strategy of sticky pricing.

These considerations point to the heart of the regulatory problem with regard to "unilateral collusion." The reaction of ENI's competitors to ENI's pricing policy must be regarded as mere best response. Their behavior is an expression of mere oligopolistic interdependence, even though they benefitted from the higher price level in the market. Consequently, a passive adaptation to collusive market conduct should not be considered illegal but part of functioning oligopolistic competition. Thus, collusive leader-follower behavior must not be conceptualized as a form of unlawful *coordination,* and thus illegal according to Section 1 Sherman Act or Article 101(1) TFEU. Antitrust law should instead target *unilateral* collusive behavior that facilitates "best response" which leads ultimately to collusion.

This appraisal of collusive leader-follower behavior appears to be in line with the treatment of non-conspiring firms that adjust their prices in reaction to a price increase by cartelizing competitors. Such a constellation is generally referred to as "umbrella pricing" since the nonparticipant benefits from the "price umbrella" spread by its cartelizing rivals.[84] This metaphor somewhat obscures the interdependence between the optimal cartel price and the behavior of the firms outside the circle of cartel participants. Nevertheless, even if the conduct

[84] Areeda and Hovenkamp (2007), §347, p. 198.

of a non-cartelist is in fact in accordance with the collusive strategy of the cartel, "umbrella pricing" is generally regarded as being innocent per se, and the legal discussion circles only around the question of whether customers of nonparticipants may recover damages from the cartelists.[85] Thus, notwithstanding that "umbrella pricing" contributes to sustain collusive equilibria, antitrust law does not require market operators to abstain from a best-response strategy in reaction to their competitors' pricing. The law refrains from imposing on non-cartelists a duty to keep prices constant (or at least at a lower level than the cartel price) which would effectively amount to a duty to increase output to offset the cutback of conspiring competitors.

If we accept therefore that there are valid economic and regulatory reasons why collusive leader-follower behavior such as the pricing alignment by ENI's competitors should not be considered as participation in an illegal coordination, it seems consequent that the law should instead target ENI's decision to implement a collusive strategy.

2.4.2 Developing the Legal Framework: Targeting Unilateral Conduct with Collusive Impetus

Unilateral conduct that has as its object or effect to promote (tacit) collusion ought to be prevented. Based on findings of the collusive potential of sticky pricing we have proved empirically that ENI employed such a pricing policy successfully to bring about a collusive equilibrium in the Italian gasoline market. But is there a feasible way of legal intervention? Should we ban a firm from implementing a policy of sticky pricing because it may facilitate collusion and punish the firm in case of an infringement?

There would be nothing inherently new in prohibiting a certain pricing behavior. Market dominant firms are not allowed to engage in predatory pricing. And just as it has to be defined with regard to a specific industry whether a certain pricing policy has to be considered "predatory," courts would also have to define "sticky pricing" industry-specifically as infrequent price changes in response to changes of input costs or demand patterns. Thus, we propose to adopt a doctrine according to which *inter alia* the implementation of sticky pricing along with large price changes would be prohibited under market conditions where it may be expected that price leadership will emerge as a price coordinating mechanism and thus, such a pricing strategy will bring about collusion. This is particularly relevant for oligopolies with price and cost transparency where structural market features favor collusion and at the same time communication might be less needed.

These requirements would have been fulfilled in ENI's case. The Italian gasoline market[86] was characterized by features that indicate its conduciveness to tacit collusion, such as its concentrated oligopolistic market structure, a high price transparency and entry barriers etc. More specifically, due to its market share of about 35 percent and the asymmetric distribution of market shares in the Italian petrol industry, ENI clearly held

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[85] Several courts have recognized such claims for "umbrella damages," see, for example, *Loeb Indus., Inc. v. Sumitomo Corp.,* 306 F.3d 469 (7th Cir. 2002); *In re Beef Indus. Antitrust Litig.,* 600 F.2d 1148 (5th Cir. 1979).

[86] See supra section 3.1 and 3.2.

the position as market leader. Thus, its commitment to a strategy of sticky pricing resulted in a credible signal to its competitors and entailed a strong potential to encourage them to align their pricing in order to bring about a collusive equilibrium.

In suggesting that a certain market practice should be prohibited depending on its *potential* to restrict competition *under particular market conditions* we propose nothing revolutionary. An exchange of information between competitors, for example, does not necessarily restrict competition and may even be regarded as procompetitive. However, under particular market conditions it may seriously endanger the competitive process as it allows firms to coordinate their behavior and thus, may be considered an illegal facilitative practice.[87] A corresponding regulatory response should be conceivable in cases of unilateral practices which entail an equal potential to facilitate collusion. Turning to antitrust provisions which address firms' unilateral behavior, we need to recognize, however, that the law appears to be fragmented – to say the least – when it comes to conduct whose object or effect it is to promote collusion. Neither Section 1 Sherman Act nor Article 101 TFEU embodies an offense of *attempted* coordination. Section 2 Sherman Act and Article 102 TFEU, the essential provisions on unilateral conduct, apply generally[88] only to firms with monopoly power or to firms that dominate a market, respectively, and thus based on criteria which typically exclude single oligopolists.

In line with the approach suggested in this article, the Federal Trade Commission (FTC) strove already to tackle unilaterally adopted (supposedly) facilitating practices under Section 5 FTC Act.[89] This ambition received a decisive blow from the decision of the Court of Appeals for the Second Circuit in the *Du Pont (Ethyl)* case.[90] In *Ethyl* the FTC blamed four producers of gasoline antiknock compounds of having unilaterally adopted practices that were aimed at facilitating parallel pricing at a supra-competitive level. These practices included 30-day advance announcements of price changes, "most favored nations" clauses in sales contracts, and uniform delivered prices.[91] The Court, however, held that the evidence presented by the FTC did not sufficiently support the view that these practices did indeed have an anticompetitive purpose or

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87 See, for example, ECJ, 23.11.2006, Case C-238/05 *Asnef-Equifax,* [2006] ECR I-11125, para. 54: "[…] the compatibility of an information exchange system […] with the [EU] competition rules cannot be assessed in the abstract. It depends on the economic conditions on the relevant markets […] as well as the type of information exchanged […] and its importance for the fixing of prices, volumes or conditions of service."

88 Unilateral use of facilitative practices to sustain collusion by a firm that is not individually market dominant could be regarded as an abuse of collective dominance under Article 102 TFEU. But there is no established doctrine to that effect. Under Section 2 Sherman Act it is the prohibition of any "attempt to monopolize" which broadens the scope and which may allow catching unilateral conduct of firms that individually do not hold a monopoly position. Thus, in *United States v. American Airlines, Inc.,* 743 F.2d 1114 (5th Cir. 1984), an explicit invitation to collude was considered an infringement of Section 2 Sherman Act as the court considered the aggregate market share of offeror and offeree. Besides, explicit attempts to initiate collusion have been charged as violation of the wire fraud or mail fraud statutes, see, e.g., *United States v. Ames Sintering Co.,* 927 F.2d 232 (6th Cir. 1990).

89 The U.S. Supreme Court had recognized that this provision may comprise anticompetitive conduct beyond the Sherman Act, see, e.g., *FTC v. Indiana Federation of Dentists,* 476 U.S. 447, 454 (1986); *FTC v. Sperry & Hutchinson,* 405 U.S. 233, 244 (1972); FTC v. Brown Shoe Co., 384 U.S. 316, 320-321 (1966).

90 *E.I. Du Pont De Nemours & Co. v. FTC (Ethyl),* 729 F.2d 128 (2d Cir. 1984).

91 Id. at 133.

effect.[92] Econometric evidence as suggested in this article could fill such gaps by relating a specific practice with a certain market outcome. With adequate firm level data and a benchmark an antitrust authority or a court may test whether or not (supposedly) facilitative practices contributed to a supra-competitive price level.

This shows on the one hand that advanced economic methods may support an effective use of available legal instruments to counter unilateral behavior which has as its object or effect to promote collusion. On the other hand, the analysis reveals a significant gap in the arsenal of antitrust enforcement when it comes to targeting unilateral conduct that serves a collusive strategy. Thus, under the current legal framework the potential of advanced economics to identify the collusive character of specific elements of behavior may not be fully realized. It seems therefore essential to strengthen legal instruments that frustrate unilateral conduct through which firms strive to promote or sustain collusion.

2.5 Conclusion

Collusion in oligopolistic markets has been a perennial topic both for economics and antitrust law. Antitrust law rests on economic welfare analysis which shows that collusion inflicts substantial negative welfare effects. However, antitrust authorities and private plaintiffs are substantially restricted in their fight against collusion as they much depend on evidence of explicit communication between competitors. The mild reaction of the Italian antitrust authority to the incidents on the Italian gasoline market illustrates the limits of antitrust enforcement in the absence of such evidence.

The crucial role attributed to explicit communication in the practice of antitrust enforcement hinders the detection and punishment of cartels precisely in those industries where the collusion rate is expected to be relatively high and communication appears to be less needed. Theoretical and empirical findings on cartel behavior provide a basis to derive clear test hypotheses to distinguish (lawful) oligopolistic interdependence from (tacit) collusion. On that basis, econometric evidence may step in and reveal collusive strategies behind firms' actions. Thus, it entails the potential to decisively increase the effectiveness of cartel enforcement in oligopolistic markets.

Analyzing the incidents on the Italian gasoline market where the market leader announced it was changing its pricing strategy reveals how firms might use their market power to facilitate price alignment and coordinate price changes. To be more specific, the econometric analysis by Andreoli-Versbach and Franck (2013b) reveals just *how* the leader's sticky pricing policy coordinated prices, and its *effect* on the price levels with respect to a benchmark. After the new policy was implemented, all competitors adjusted their prices following the leader's price changes. In addition the new pricing behavior resulted in a significant price

[92] Id. at 139-140.

increase. Combined, this price coordination mechanism and its effect show that it was the object and effect of the introduced pricing policy to collude through facilitating price coordination and to raise prices.

Whilst antitrust enforcement may certainly benefit from an enhanced economic methodology to identify tacit collusion, antitrust law cannot straightforwardly prohibit the participation in tacit collusion as a form of illegal coordination. The active promotion of collusive pricing by ENI and the passive (best response) alignment of its competitors must not be normatively equated. Thus, antitrust law should not infer a punishable (tacit) agreement between ENI and its competitors from the collusive market outcome, but should instead consider conduct such as ENI's pricing strategy as being a unilateral anti-competitive practice. To effectively fight tacit collusion it appears therefore to be necessary to strengthen legal instruments that target unilateral conduct which firms strategically employ to promote or sustain collusion.

2.6 Appendix: Tables and Figures

Table 2.1: Pre and post policy pricing

Panel A: Frequency and Magnitude of Price Changes	(1)	(2)	(3)	(4)	(5)	(5)
Time period	Pre	Post		Pre	Post	
	Mean	Mean	*Difference*	Mean	Mean	*Difference*
	(St. Dev.)	(St. Dev.)	**t-stat**	(St. Dev.)	(St. Dev.)	**t-stat**
	[Obs.]	[Obs.]	[Obs.]	[Obs.]	[Obs.]	[Obs.]
	Abs. % Price Change			Days between price changes		
All Firms	0.0088	0.0293	*0.0205****	5.3	9.47	*4.16****
	(0.0065)	(0.0319)	**19.27**	(5.43)	(7.67)	**8.81**
	[1143]	[172]	[1315]	[1143]	[172]	[1315]
ENI	0.0103	0.0586	*0.0483****	6.63	16.4	*9.76****
	(0.0071)	(0.0377)	**11.45**	(7.81)	(10.95)	**3.64**
	[104]	[10]	[114]	[104]	[10]	[114]
Panel B: Average Alignment to the Market Leader	(1)	(2)	(3)	(4)	(5)	(5)
	Sum of aligned firms			Price difference to ENI		
ENI's Competitors	1.74	2.98	*-1.24****	.00527	.00115	*.00439****
	(2.07)	(2.06)	**-7.23**	(.00656)	(.01986)	**12.94**
	[681]	[185]	[866]	[5448]	[1480]	[6928]

Table 2.1 summarizes the pre and post policy pricing behaviour of the nine firms acting in the Italian wholesale gasoline market. Panel A shows the frequency and magnitude of price changes. ENI increased the mean price change from 1% to 5.8%, while the average price change increased from .8% to 2.9%. Similarly, ENI increased the average time lag between price changes from one every six days to one every 16 days. The same time lag increase holds across firms, where the time lag between changes increased from five to nine days. Panel B shows the sum of aligned firms to ENI (specification 1 and 2) and the average price difference to the leader (specification 4 and 5). The number of aligned competitors significantly increased after the policy, while the average price difference to the leader significantly decreased after the policy. All changes in the pricing behaviour are significant at the 1% level.

Table 2.2: Effect of the policy on prices

	(1)	(2)	(3)
Dependent Variable	Price EU Country *j* at week *t*	Price Difference Italy-Synthetic Control week *t*	Margin firm *i* day *t*
Type of Data	Panel Data	Time Series	Panel Data
Regression Model	Dif-in-Dif	OLS	Firm Fixed Effect
Policy*Itlay	9.863***		
	(2.117)		
Policy		12.551***	22.95***
		(4.224)	(2.036)
Controls	Crude oil (4 Lags), Year and Month FE	Crude oil (4 Lags), Time trend	Time trend
Observations	891	94	7,794
R-squared	0.66	.486	0.115

Table 2.2 reports the coefficients on the full specification regression models which capture the effect of the new pricing policy. For the details of the regression analysis we refer to Andreoli-Versbach and Franck (2013). *Policy*Italy* is the intersection between two dummies (Italian price after the policy), while *Policy* is a dummy being one after the 12th of November 2004 when most competitors adopted ENI's pricing behaviour. FE stands for fixed effects. Prices and margins are expressed in € per 1000 liters. In specification (1) standard errors are clustered at country level, while in specification (2) and (3) robust standard errors are reported. In all specifications prices/margins significantly increased after the competitors adopted the same pricing behaviour as the market leader.

Figure 2.1: Cartel Formation

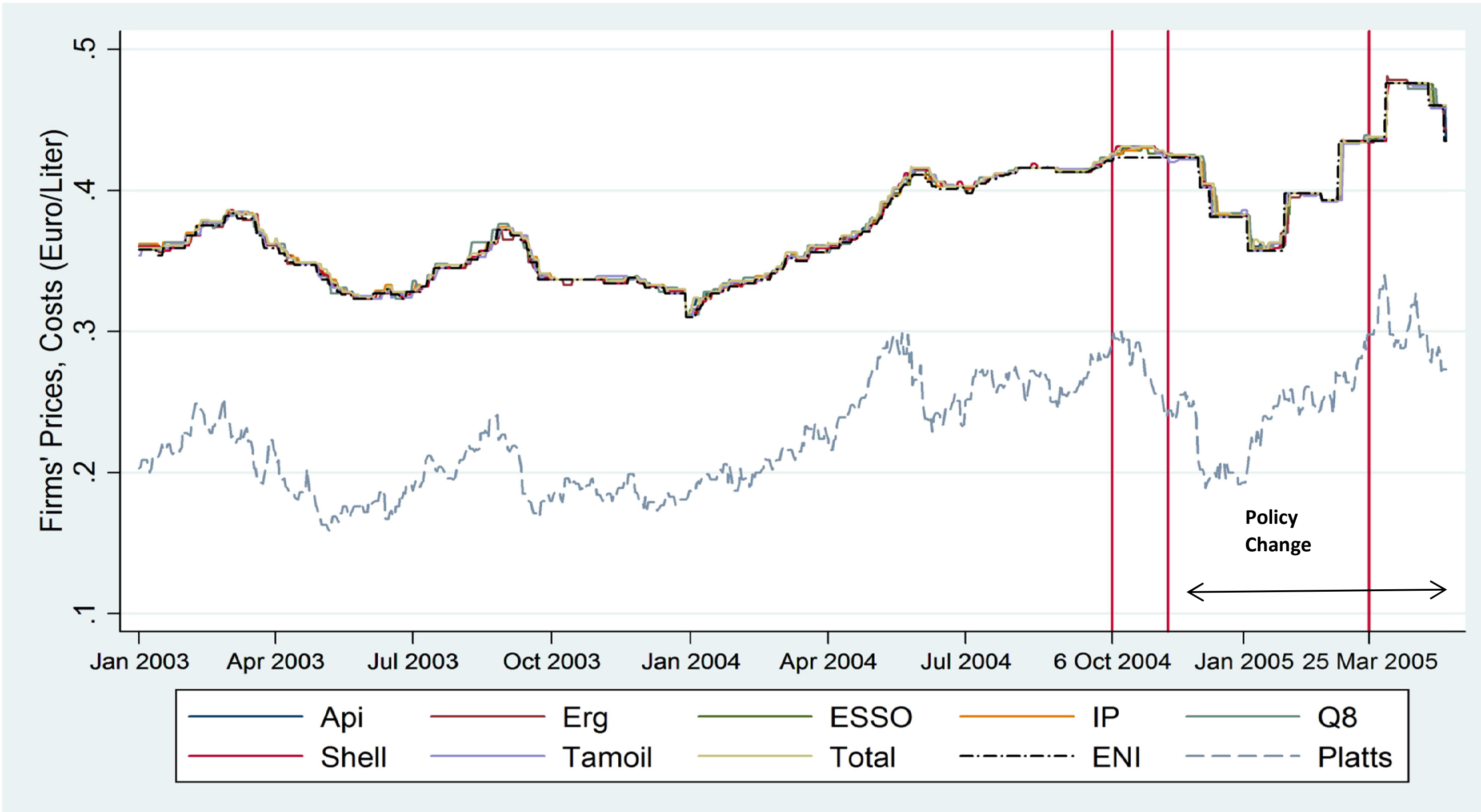

Figure 2.1 shows the daily "suggested" firm-level prices in the Italian gasoline market from January 2003 to 15th May 2005. These prices represent a very good approximation of final retail prices paid by consumers, see section 3.2. The dashed line represents the Platts Cif Med, the major source of cost for firms. The first vertical line denotes 6th October 2004, the date where ENI, the market leader, announced that it would adopt a new pricing policy consisting of sticky prices (i.e. infrequent price changes). The time span between the first two vertical lines constitutes the "commitment" time period. As prices respond to costs with about a month time lag costs were *increasing* just after the announcement by ENI contrary to what might seem from Figure 2.1. Competitors kept increasing their prices following short-run cost changes until the beginning of November when costs decreased and they started to align and follow the leader's price. The second vertical line is placed on the 12th of November, the date when most competitors aligned to the leader. Note that we will take this date as the starting date of the new equilibrium in the empirical analysis. The third vertical line shows the date when the Italian Truckers' Association (FITA) formally complained about "high and aligned prices" to the Italian antitrust authority.

Figure 2.2: Italian Price, EU Price and Brent

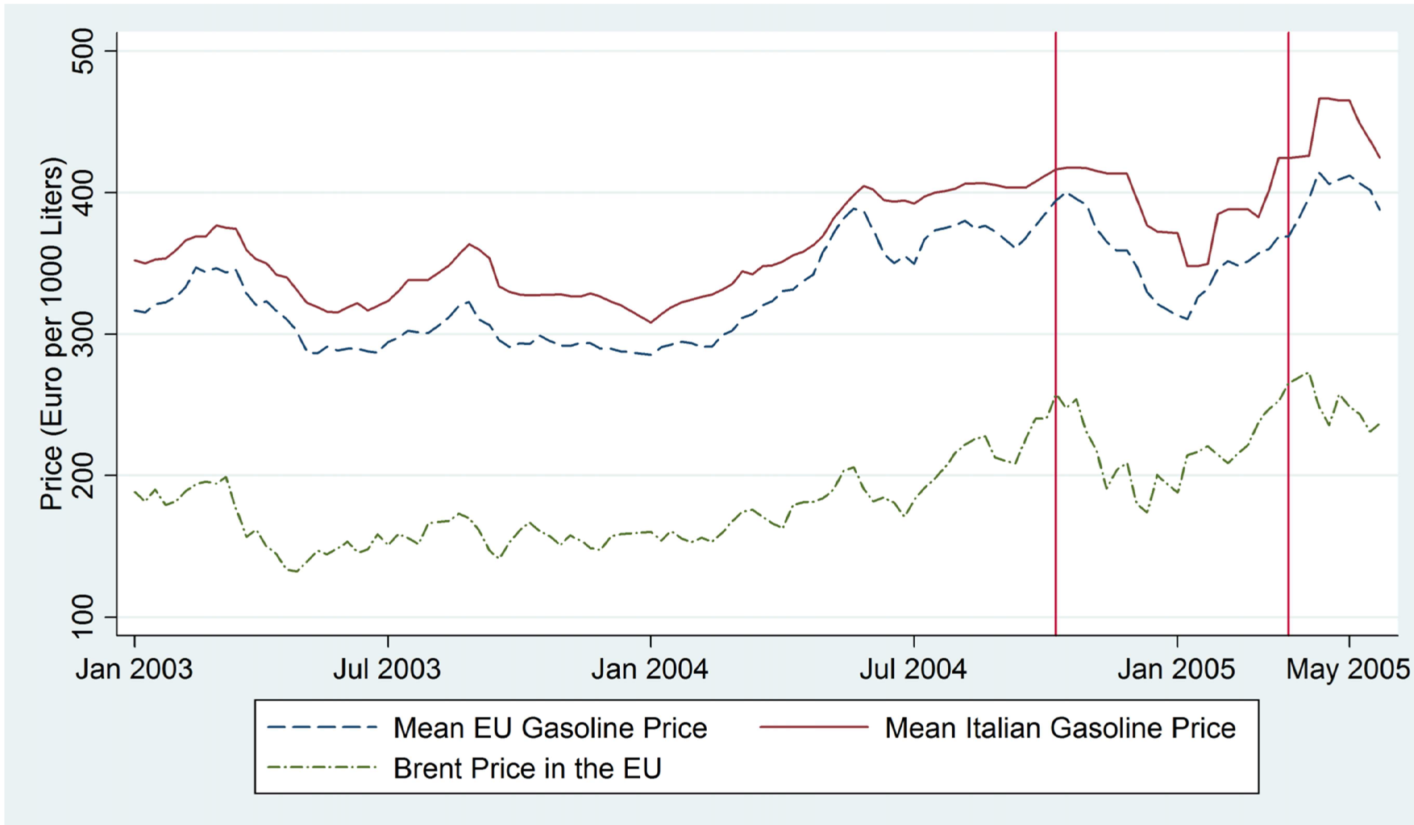

Figure 2.2 shows the average weekly Italian and EU price of gasoline and the European price of the Brent, i.e. crude oil. The continuous line represents the Italian price, while the dashed (dashed-dotted) line represents the EU price (Brent). The first vertical line denotes the date where the market leader announced that it would adopt a new pricing policy consisting of sticky pricing (i.e. infrequent price changes). The second vertical line shows the date when the Italian Truckers' Association (FITA) formally complained about "high and aligned prices" to the Italian Antitrust Authority on 25th March 2005.

Chapter 3: The Informational Content of Price Changes: Evidence from Professional Tennis Betting

3.1 Introduction

In cases of insider trading market participants use their non-public information to earn extra-profits. The high profile illegal insider trading[93] cases in recent years indicate that the gains of trading with privileged information can be substantial and that regulatory authorities worldwide continue to expend large resources trying to address this problem. A central issue for regulators and market participants is whether price changes convey private information. However, this question is difficult to answer because the extent of private information is not directly observable. In addition financial markets are continuously hit by the arrival of new information and to decode insider-trading related information is problematic because of its hidden nature. Most scholars[94] agree that insider trading is detrimental for financial markets. Guiso et al. (2008) show that the lack of trust negatively affects participation in stock markets. Du and Wei (2004) show that insider trading raises stocks' volatility. Bhattacharya and Daouk (2002) use data on the existence and the enforcement of insider trading laws in 103 countries and show that the enforcement, rather than mere existence, of insider trading laws is associated with a reduction in the cost of equity. Finally, Easley et al. (2002) and Kelly and Ljunqvist (2012) show that investors require a higher return when trading in stocks that have a higher risk of asymmetric information.[95]

In this paper we provide a new framework for thinking about whether price changes reflect private information and whether they convey viable information about future returns. In the empirical analysis we use three different specifications to test whether price changes, especially those most likely conveying private information, have a predictive power on future returns. We use a large and unique dataset of odds on every tennis game of the main tennis world tour composed by Association of Tennis Players (ATP) and Grand Slam events from 2008 to 2012, where professional tennis players repeatedly compete in about 63 tournaments in 30 countries each year.[96] The dataset consists of decimal odds of two large bookmakers, Pinnacle Sports and Marathonbet, and incorporates more than 16,000 individual games. The key feature of this data is that it contains observations on players' odds at two different time periods: the first odds released before the game and the last odds observed just before the beginning of the game. The odds show how many

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93 Recent cases include the $156 million paid by Galleon Group LLC after conviction or the $615 million paid by SAC Capital Advisors to settle their case with the SEC. Note that trading on privileged information is not always illegal. U.S. Senators were allowed to trade on the regulatory decisions they take and made substantial gains (Ziobrovski et al. 2004).

94 This view is not unanimous. For example, Leland (1992) argues that insider trading might be beneficial for markets as stocks incorporate information more quickly.

95 See Easley and O'Hara (2004) for a theoretical justification for this link.

96 These numbers might slightly vary from year to year. For details on the ATP tennis world tour see http://www.atpworldtour.com/.

units the bookmaker pays out per unit staked on an event, in case that event happens. On average the first odds are released 24 hours before the game, when the two players that won their previous round are matched according to the tournament brackets. In principle, odds change infrequently. Little information is revealed since odds are formed after both players have finished their previous games and move on in the tournament. However, odds can still move due to incoming orders, which might convey private information. When privately-informed investors trade, their order flow will convey non-public information to the rest of the market and thus, market participants revise their estimates of the probability that a player wins.[97]

The aim of this paper is to disentangle the odds variation caused by insider trading from other sources using an exogenous variation in the incentives to cheat. Cheating by a player is defined as losing a game on purpose and selling this information to bettors. In the report commissioned by the tennis authorities on the integrity in tennis Gunn and Rees (2008, par. 5.2) conclude that "[t]here are strong indications that some players are vulnerable to corrupt approaches and others outside of tennis are using them to make corrupt games on betting from professional tennis," an issue which is not confined to tennis.[98]

The hidden nature of insider trading and cheating has made its identification difficult. With the exception of Meulbroek (1992) who studies the impact on stock prices of ex-post detected illegal insider trading most of the data used to analyze the forecasting ability of company insiders is based on self-reported trading filed with government regulators.[99] A potential problem with this data is that corporate insiders might trade from numerous accounts, e.g. accounts of children (Berkman et al. 2013) or sell inside information to their social networks, e.g. class mates working at hedge funds (Cohen et al. 2008 and 2010). In addition company insiders have an unknown time horizon for their investment which makes it difficult to calculate the cumulative abnormal returns.

Betting markets provide a series of advantages to study insider trading: first, a very large number of observations is available. Second, each game has a clear beginning and end which provides an objective time interval to calculate odds changes and returns. Third, players compete repeatedly in different competitions facing exogenous changes in their incentives to cheat. Fourth, the rules, the odds and the outcomes of games are observable and clear. Fifth, betting is a global and highly competitive business where both betting platforms and bookmakers compete to attract bets and bettors can place bets worldwide through the internet at very low transaction costs. Gunn and Rees (2008, par. 3.117) estimate a total of about 562 on-line betting resources in 2008. Finally, the sport betting market is large and constantly increasing. The Remote Gambling Association Report (2010, p.9) estimates that the global (legal) amount bet in 2008 in sports events was equal

97 Adverse selection in securities markets was formalized by Glosten and Milgrom (1985).

98 An investigation by Europol into match-fixing in soccer revealed widespread occurrences of match-fixing in recent years, with 680 games globally deemed suspicious. Europol's chief, Rob Wainwright, stated that "match-fixing activity [is] on a scale we have not seen before".

99 See for example Lakonishok and Lee (2001), Jeng et al. (2003), Marin and Olivier(2008) and Fidrmuc et al. (2006).

to \$46.5 billion. While there are no official figures on the total tennis betting market, in 2007 a total of \$566 million was placed on men's Wimbledon tournament only on the largest internet based betting platform, Betfair.[100]

In this paper we analyze the informational content of odds changes before professional tennis games and test whether odds changes, i.e. changes in the implied probability[101] of winning the game, have a significant forecasting ability on a game's outcome. To test whether odds changes are a significant predictor of future returns on bets we develop three identification strategies derived from the efficient market hypothesis (EMH) popularized by Fama (1970) and the rational crime theory formalized by Becker (1968).

In any financial market future asset returns should be difficult (or even impossible) to forecast using public information. As bookmakers start quoting an event 24 hours before that game one can exploit the variation in a player's implied winning probability, i.e. the difference between his implied winning probabilities calculated using the last and first odds, to test whether changes in that probability have a predictive power on a player's ex-post result. The null hypothesis testing the weak-form EMH is that controlling for a player's last winning probability before a game, past probability changes should not significantly increase the accuracy of the prediction. Just before the start of the game odds should equal the best possible estimate of the winning probability incorporating all available information. An increase (decrease) of the probability before the game should not translate into a significant increase (decrease) in the likelihood of winning controlling for the last probability. Even though this approach is directly derived from for the EMH, rejecting the null of no informational content of past probability changes does not provide an indication of the *underlying* cause of this market inefficiency. A player's winning probability might change for a variety of reasons which are unobserved to the econometrician. Markets might fail to fully incorporate *public* and/or *private* information. The main specification aims at taking apart these two confounding effects using an instrumental variable approach which uses an exogenous shift in the incentives of a player to lose on purpose.[102] Odds changes represent changes in the market's expectation of the result of a tennis game. These observed changes can be thought of as the sum of three components: insider trading, public news and noise. We model odds changes as a mismeasured variable of the true insider-trading related changes in odds. Assuming that, first, news and noise are uncorrelated and add together to the random "non-insider-trading" component of odds changes, that is uncorrelated with the true insider-trading component and second, the "non-insider-trading" component is uncorrelated with the stochastic disturbance in the regression specification, then, we are dealing

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[100] See the Tennis Integrity Report (2008, p. 64).

[101] Note that the implied probability of winning is defined as the multiplicative inverse of a player's odds, i.e. $implied\ probability_i = 1/odds_i$. For the theoretical foundations of why prediction market prices correspond with mean beliefs and are quite accurate predictors of an event's probabilities see Wolfers and Zitewitz (2006).

[102] This approach goes in the direction of recent attempts to analyse insider trading such as Cohen et al. (2012). They exploit the fact that insiders might trade for a variety of reasons, e.g. to yield extra-profits from their advantaged position or simply diversify or hedge risk, and find that "opportunistic" trading yields value-weighted abnormal returns of 82 basis points per month.

with a "classical" measurement error problem.[103] The coefficient of the regression of betting returns on odds changes will be smaller than the true coefficient, i.e. it will suffer from attenuation bias.[104] The most common solution to mismeasurement is the use of instrumental variables.[105] Our research design exploits an exogenous shift in the incentives to cheat caused by random tournament draws and a player's time-invariant corruption norms to decompose the "insider-trading" related variation of odds changes from the "non-insider-trading" component.

At least since Becker (1968) seminal economic analysis of criminal behavior it is well known that incentives are an important determinant of criminal activity.[106] In tournament draws, conditional on a player's seed, players are randomly matched with opponents.[107] The weaker a player's opponent is the higher the odds, and thus the returns on cheating, on the opponent. While monetary and ATP-ranking incentives are constant within a round of a tournament, the incentives to cheat might vary substantially depending on the relative strength of the randomly matched opponent. Players might (rationally) decide to cheat, i.e. lose on purpose and sell this information. Losing a game is uncomplicated and just requires the will of one player.[108] Assuming $Player_j$ cheats, insiders with privileged information will start buying bets on the opponent of $Player_j$. As bookmakers act as market makers and tend to keep balanced books by making money on the transaction costs rather than by taking a position in the game, odds adjust. Bookmakers attract money on a player by making bets on that player (his opponent) more (less) favorable. As a consequence the odds (implied probability) on the cheating player will increase (decrease). Conversely, the odds (implied probability) on the cheater's opponent will decrease (increase). However, players' incentives to cheat do not just change according to the random draws of a tournament, but also across tournaments. In the empirical analysis we will run the regression for two different types of tournaments with high and low stakes. Because of the sharp increase in both ATP-ranking points and prize money between the two lowest, i.e. ATP 250 and ATP 500, and the two highest, i.e. Master Series and Grand Slam, types of tournaments, players face higher incentives to compete in the latter tournament type but similar returns from cheating, i.e. odds on the opponent, in both types of tournament.[109]

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103 As estimations with fixed effects typically increase the variance of the noise relative to the variance of the signal, the mismeasurement problem might be even larger in panel data as is the case of this paper where the observational units are players who repeatedly compete at different tournaments over time. See Griliches and Hausman (1986) for a discussion on this issue.

104 This result is also known as the "Iron law of econometrics", see Hausman (2001) for a general discussion on mismeasurement error and its consequences.

105 See Angrist and Pischke (2008) and Angrist and Krueger (2001) for a general discussion of IV.

106 See for example the work by Olken (2007) and Björkman and Svensson (2009) on the effects of monitoring on corruption.

107 See section 2 for a more detail account of how the tournament draws work.

108 This might be a reason why tennis has experienced fewer betting related scandals than soccer or cricket where more players are involved.

109 A different strategy might be to use rounds within a tournament. As all tournaments in the data are single-elimination tournaments a player must win initial rounds to compete for high stakes in final rounds and thus all rounds are necessary to advance.

While incentives to cheat vary over time, there might be also some strong, persistent, personal preferences towards cheating. As shown by Fisman and Miguel (2007) the behavior of individuals that face the same incentives might differ substantially with respect to cultural norms.[110] Thus, we use a player's Corruption Perception Index (CPI) as a proxy for his time-invariant[111] likelihood to cheat. In the IV approach we instrument odds changes with the interaction between a player's time-invariant CPI and his opponent's initial odds. Thus, we exploit the variation with respect to *preferences* and *chances* to cheat to uncover the insider-trading related variation in odds changes and relate it to future returns on bets.

In the final specification odds changes will be split in two parts: an insider-trading and a non-insider-trading related one. In a similar spirit as in the IV case we begin by running a regression of odds changes on the instrument (excluding the other covariates). Instead of keeping only the insider-trading related variation of odds changes we also keep the other part, i.e. the fitted values of the residual. If this part is simply noise we would expect it not to be informative. In contrast if it represents not fully incorporated news it might have some predictive power if betting markets don't fully incorporate public news.[112] In the final step we regress returns on bets on the fitted values of the first stage, the error term of the first stage and other covariates. In all three specifications we find significant evidence of insider trading. Probability and odds changes are a significant predictor of a game's outcome and returns, respectively. The effects are larger for low stake tournaments where the incentives to compete are lower. We also find that non-insider-trading variation has significant predictive power over future returns, but the insider-trading effect is about five times larger.

To better understand how cheating might affect odds changes we report the key facts of the most famous case of "highly suspicious" betting patterns which took place in the second-round match at the ATP-250 Prokom Open 2007 in Sopot, Poland, between the number four-ranked Russian Nikolay Davydenko and number 87-ranked Martin Vassallo Arguello of Argentina. The match was scheduled on the 2nd of August 14:00 CET. The first quotes by Pinnacle (Marathon) came out on the 1st of August at 9:42am (8:00am) and in line with the rank difference between these players quotes saw Davydenko as the strong favorite. Pinnacle's odds on Davydenko (Vassallo Arguello) were 1.206 (5.45), while Marathon's odds on Davydenko (Vassallo Arguello) were 1.10 (6). These odds significantly changed over time and just before the match Pinnacle's odds on Davydenko (Vassallo Arguello) were 1.350 (3.66), while Marathon's odds on Davydenko (Vassallo Arguello) were 1.90 (1.8). This large change implies a large change between the returns[113] on bets over time.

[110] Fisman and Miguel (2007) show that diplomats working at the UN from high-corruption countries accumulated significantly more unpaid parking violations even though they face the same incentives as low-corruption country diplomats.

[111] Cultural norms seem to be highly persistent over time. Using data on anti-Semitism in Germany Voigtländer and Voth (2012) show that cultural traits persisted for over 600 years.

[112] Gandar et al. (1998) test whether odds changes were related to fundamentals or were pure noise before NBA games. The authors show that odds changes significantly improve the accuracy of forecasts of actual game outcomes. We bring this analysis one step further by including the possibility that odds changes are caused by private rather than public information.

[113] Returns on the winning player are calculated as odds-1. Returns on the losing player are -1.

A winning bet using the first odds on the underdog, Vassallo Arguello, would have yielded 500% profit with Marathon's odds and 445% with Pinnacle's odds. Performing the same calculation using the last odds, the profit would have been much lower, i.e. 266% and 80%. Similarly, as the implied probability of winning is the multiplicative inverse of a player's odds, Vassallo Arguello's winning probability increased from 18% (=1/5.45) to 27% (=1/3.66) using Pinnacls's odds while Davydenko's winning probability decreased from 90% (=1/1.206) to 74% (=1/1.35).[114] The effect is even larger using Marathon's odds, whereby Vassallo Arguello's winning probability increased from 16% (=1/6) to 52% (=1/1.9) while Davydenko's winning probability decreased from 90% =(1/1.1) to 55% (=1/1.8). When the match started Davydenko won the first set and then retired because of an injury[115] in the third set. More than $7millions were placed on this match only on the largest internet-based trading exchange, Betfair, 10 times the usual amount for a similar-level match.[116] In a subsequent confidential report commissioned by the ATP it emerged that three Russia-based Betfair accounts risked a total of more than $1.1 million on Vassallo Arguello to win the match despite his low winning chances.[117] Eventually the ATP cleared Davydenko for match fixing as investigators were unable to review phone records and no direct connection between Davydenko and the betting accounts could be established. This case received massive media attention and brought the ATP to found an internal agency, the Tennis Integrity Unit (TIU), to watch over match fixing and other betting related threats to tennis. While the extent of odds changes in this case is abnormal, the intuition behind it can be generalized. A player coming from a high-corruption level country facing high odds on his opponent in a low stake tournament might decide to lose on purpose and sell this information. Informed bettors then start buying bets on that player's opponent and the odds on the opponent decrease.

The reminder of the paper is structured as follows: section 2 describes the data and the setting. Section 3 presents the empirical analysis on the informational content of odds changes. In section 4 we discuss the results while section 5 concludes.

3.2 Data and Setting

3.2.1 Institutional Background

The setting used to examine the extent to which odds changes reflect private information is the professional tennis betting market. Sport betting markets are also known as "prediction markets," "information markets,"

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[114] Because of bookmakers' overround, i.e. spread, probabilities sum to a number greater than 1. If bookmakers keep balanced books the difference between the sum of implied probabilities and 1 is a bookmaker per bet margin.

[115] Davydenko's injury turned out to be short-lasting as on the 8th of August 2007, 5 days after he retired, he played at the Rogers Masters in Montreal, where he reached the quarter finals.

[116] Betfair, the online betting company, took the unprecedented step of voiding all bets on the match. See http://www.thetennisspace.com/the-inside-story-of-the-davydenko-controversy/

[117] See http://sports.espn.go.com/sports/tennis/news/story?id=3235411

or "event futures".[118] These markets allow participants to trade contracts whose payoffs are tied to a future event, as for example the victory of a player. The odds on a player are equal to the per unit bet return on a player in case of victory. Because of the probabilistic nature of a game's outcome odds also represent the market-aggregated forecasts of a game. This market structure offers an ideal environment to test price formation theories. There is a large amount of data available, for each game there is an objective start and end, results are observable, the distribution of prizes is known in advance and initial tournament draws are random. In the main tour, professional tennis players compete in about 63 tournaments in 30 countries for prize money and for ATP-ranking points. The data contains all games from the main tennis tour from 2008 to 2012 which include Grand Slams, ATP World Tour Masters 1000, ATP World Tour 500 series and ATP World Tour 250 series (listed in decreasing order of importance). The total prize money of each type of tournament varies greatly. In 2012, Grand Slams such as Wimbledon (US Open) had total prize money of $10.5 million ($11.7 million). ATP 250 tournaments have much lower prize money, typically ranging from $450 to 500 thousand. The same holds true for ranking points, plotted in **Figure 3.1**. For example, the winner of a Grand Slam earns eight times as many ATP-ranking points as the winner of an ATP 250 tournament.

The empirical analysis exploits the institutional setting of tennis tournaments and in particular the random nature of the initial tournament draw. Tournaments accept players on the basis of their world rankings. The higher a player's ranking, the better his chance of being accepted into the draw. For example a two-week Grand Slam has a draw of 128 players. The top 32 players are seeded and the rest, 96, are unseeded. Out of the 96 unseeded players, 72 made it into the draw based on their world ranking, 16 reached the draw through the qualifying rounds and 8 are selected as wild cards.[119] The seeding of the top 32 players works as follows: the number one (two) seeded player is put into the top (bottom) slot of the bracket, the other seeded players are put into groups and randomly placed in a way that ensures that no seeded player can play against another seeded player in the first or second round. After the top 32 players are seeded the remaining players are then randomly placed in the draw. For example in the first round of Wimbledon in 2012 the 207 ranked qualifier Jimmy Wang was drawn to play the 16 world-ranked and 17 seeded Fernando Verdasco. The odds (implied probability) on Wang were 12.12 (8.2%). Another player who started from the qualifications as well, Michael Russell, ranked 112, had a "luckier" draw and played against Adrian Menendez-Maceiras, a qualifier as well, ranked 212. The odds (implied probability) on Russell were 1.5 (66.6%). In contrast the seeded players always face weaker opponents in the first two rounds, but also face great variation in the relative ability of their opponents. A seeded player might for example play against number 35, 70, 100 or 200 of the world ranking.

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[118] For a general discussion on these markets see Wolfers and Zitzewitz (2004).

[119] Wild cards are given at the discretion of the tournament organizers. Usually they are given to "home players", i.e. players of the same nationality where the tournament takes place or promising junior players.

The exogenous variation of the relative ability of players shifts the incentives to cheat. Sometimes the odds on a player's opponent will be higher than usual and thus provide a higher incentive to cheat. In the IV specification of the empirical analysis we use the interaction between the opponent's odds and that player's cultural norms measured by the Corruption Perception Index (CPI) to identify insider-trading related variation in odds changes before games.

3.2.2 Data

The data used in this analysis come from OnCourt,[120] a large provider of statistical information on tennis players, tournaments and odds. **Table 3.1** presents summary statistics of the data, consisting of over 16,000 games played from 2008 to 2012. The data includes information about players like nationality, age and ATP-rank. For every tournament of the main tour we observe games for all rounds going from the qualification[121] to the final. Tournaments differ in many dimensions such as the prize money, size, duration, location and ATP-ranking points. The largest (smallest) tournaments have 128 (28) players excluding the qualification round. All tournaments are single-elimination events where players compete over multiple rounds over several days. The main tour starts beginning of January and ends in mid-November.

The key variables used in this analysis are: a game's result, returns on bets, initial and last odds, Corruption Perception Index, tournament and round dummies, a player's seeding and the bookmakers' margins. The OnCourt data includes odds from two major bookmakers: Pinnacle Sports and Marathonbet. Bookmakers start quoting a game as soon as the winners of the two matches in the previous round are determined. On average the first quotes are available 24 hours before the match, while the last odds are collected just before the game starts. On average odds tend to increase over time. The mean (median) odds change is .21 (0). In the regression analysis returns on bets is always calculated using the last odds and defined as:

$$retruns_{i,T+1,G,BM} = \begin{cases} odds_{i,T,G,BM} - 1; & \text{if } Player_i \text{ wins game G} \\ -1; & \text{if } Player_i \text{ loses game G} \end{cases}$$

$retruns_{i,T+1,G,BM}$ are the returns on bets on $Player_i$ after game G using the last available odds of bookmaker BM. Throughout the empirical analysis we will refer to t (T) as the time when the first (last) odds are observed, and $T+1$ to the period when the uncertainty is resolved and the result of the game is known. Returns can be thought of as the per-$ bet percentage return on bets. For example odds of 3.4 on $Player_i$ imply a 240% (3.4-1) return if he wins and a 100% loss (-1) if he loses. The implied probability of $Player_i$ is calculated as $1/odds_i$. An important feature of betting markets is that bookmakers tend to keep balanced books and thus tend to attract money in relation to odds and seldom take positions in a game. If an outcome

[120] See www.oncourt.info for further details on the level of provided data.

[121] The qualification rounds start in the week preceding the start of the main tournament which begins after the draws are done.

attracts too many bets bookmakers try to balance their books by offering higher odds and thus attracting more money on the under-betted outcome.[122] With balanced books the per-bet bookmakers' margin is determined by the "overround", i.e. the spread, which is calculated as the sum of implied probabilities minus one, i.e. $margin_{BM} = 1/odds_i + 1/odds_j - 1$. The average spread in this sample is 3.9%.

The only variable that was not generated using OnCourt data is the Corruption Perception Index (CPI). The CPI has been published every year since 1995 from Transparency International and ranks countries "based on how corrupt a country's public sector is perceived to be. It is a composite index, a combination of surveys and assessments of corruption, collected by a variety of reputable institutions."[123] In the empirical analysis we use the CPI in 2011[124] which ranges from 0 (low corruption) to 10 (high corruption). In order to ease interpretation in the regression analysis we generate $InvCPI_i = -1 * CPI_i$ so that an increase in $InvCPI_i$ represents an increase in $Player_i$'s country corruption level. During the following analysis I will refer to $InvCPI_i$ as CPI_i.

3.3 Preliminary Analysis: Prize Money, Cumulative Returns and CPI

3.3.1 Betting-related Corruption in Tennis

After the suspicious betting patterns observed during the game between Davydenko and Vassallo Arguello, also known as the "Sopot Match", concerns about the integrity and credibility of tennis rose. Asked about cheating in tennis during the Kremlin Cup in Moscow in October 2007, 2 month after the "Sopot Match", top British player Andy Murray stated that: "It doesn't really surprise me. Some guys have to come to tournaments like this every single week and the first-round loser's cheque is sort of 2,500 euros and they have got to pay for their air fares and, you know, it's only a 10 or 12-year career so you have to make all your money while you're still playing. It's pretty disappointing for all the players but everyone knows cheating goes on." The growing concerns about the status quo of integrity in professional tennis led the tennis authorities to establish a permanent unit called The Tennis Integrity Unit (TIU). The aim of the TIU, established in September 2008, is to "protect the sport from all forms of betting-related corrupt practices."[125] The establishment of the TIU was preceded by an independent review commissioned by tennis authorities into betting-related corruption, Gunn and Rees (2008). The report consulted and interviewed with numerous stakeholders to understand the status quo of betting related corruption in tennis. Gunn and Rees (2008, par. 2.28) state that: "A large majority of current and former players we interviewed claimed to 'know of'

[122] The goal for most bookmakers is to balance their books. This practice ensures a guaranteed per-bet margin and eliminates the risk of exposure to a game's outcome. See Harris (2003) for an analysis on the market microstructure of bookmakers.

[123] For more details see: http://cpi.transparency.org/cpi2012/in_detail/#myAnchor1

[124] As discussed in the introduction, cultural norms tend to be highly persistent over time. We use the 2011 estimate of CPI as proxy for the time-invariant preferences of a player with respect to corruption.

[125] See http://www.tennisintegrityunit.com/about-us/

approaches to players being invited to 'throw matches' presumably for corrupt betting purposes." While strong attempts have been made to watch over betting-related misconduct, there have been only three life bans of minor players[126] since 2008. As is always the case in the analysis of criminal activity, detection is endogenous and using a sample of ex-post detected players might not be informative about the true underlying population. The detection of these three minor players was possible because they had to rely on stronger players throwing the match.

3.3.2 Preliminary Graphical Evidence: Incentives and Norms

Before proceeding to the empirical analysis we provide graphical evidence that players' incentives to compete are very high for Grand Slams and Masters, where top-players compete for most of the ranking points and prize money. In contrast lower ranked players face relatively flat incentives. The right part of **Figure 3.1** shows the ATP-Ranking points a player can earn conditional on the round he achieves for the four different types of tournaments. ATP-Ranking points are convex in the rounds of a tournament and greatly differ across different tournament types. Players are admitted at tournaments on the basis of their rank. The better a player's rank the best is his seeding and thus his chance to proceed to the final stages of the tournament. In the empirical analysis we split the sample in two: high vs. low stake tournaments. We define Grand Slams and Masters to be the high stake tournaments, while ATP 500 and 250 will be considered low stakes.[127]

An additional incentive to cheat is the low variation among top players. A few top players seem to systematically outperform the others and get most of the prize money. The left part of **Figure 3.1** depicts the cumulative prize money of 422 tennis players with a mean rank under 200 over the time period 2005 and 2012. Cumulative prize money is calculated by summing up all the prize money a player earned in the tournaments he played over the aforementioned time period. The total prize money is highly convex in players' rank. The slope is relatively flat from rank 200 to rank 20 and very steep below rank 20. For example, the difference in total prize money between Roger Federer, the top-earner, and Rafael Nada, the second best earner, was $12.6million. The difference between the 100th best earner, Pablo Cuevas, who earned $1.73millions from 2005 to 2012 and the 101st, Gilles Muller, was $40k. Only ten players consistently ranked among the top-20 over that time period. Out of about $753 million in total prize money the top 5 (10) [20] earning players received 26% (34%) [44%]. Out of 422 players with a mean rank below 200 the best 27 players got 50% of the total prize money between 2005 and 2012.[128]

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126 Austrian player Daniel Koellerer, Serbian player David Savic and Russian player Sergei Krotiouk have been banned for life from professional tennis tournaments for betting-related purposes. The investigations concluded that these players had made invitations to another tennis player to fix the outcome of tennis matches. These players were low ranked and mostly played in low-stake Challanger tournaments, which are not part of the tennis main tour.

127 A different strategy might be to consider specific rounds. The problem with such an approach is that in single-elimination tournaments only winning players advance to successive stages. Thus, players must win initial low-stake rounds to access the final rounds.

128 These numbers also include prize money earned in Challenger events which are not part of the main tour.

The second source of variation is a player's CPI. Criminal behavior is not entirely driven by incentives. Faced with the same incentives people behave in significantly different ways. For example, Fisman and Miguel (2007) analyze unpaid tickets by diplomats working at the UN in New York, USA, and show that home country corruption norms are an important predictor of propensity to behave corruptly.[129] **Figure 3.2** shows preliminary evidence on the relation between a player's home-country CPI and the cumulative returns on bets on his opponent, calculated as the sum of the returns of each bet on that player's opponents. The fitted line points to an increasing relation between the CPI and cumulative returns on opponents.[130] Running a regression of the total returns on bets on a player's opponents on that player's CPI using the total number of games as weights yields a highly significant and positive coefficient of .94 with a t-statistic of 10.2. This result is robust to the inclusion of rank[131] and bookmakers' margin (results not shown). On average moving from a low corruption country like Finland (CPI=-9.4) to a high corruption country like Ukraine (CPI=-2.3) increases the cumulative returns by 620%, from -1700% to -1080%. Thus, an investor who consistently betted the same amount on opponents of players from high corruption countries would have gained 620% more than if he betted the same amount on opponents of players with low CPI.[132] This graph provides preliminary evidence on the relation between abnormal returns on opponents and a player's CPI.

3.4 Empirical Analysis

In this section we examine the forecasting ability of odds changes on betting returns and of changes in the implied probability on the outcome of a game. We employ three different specifications to identify whether odds changes convey information about the results of games. The first specification tests the weak-form efficient market hypothesis (EMH) popularized by Fama (1970). In this specification we will not distinguish between private and public information but rather test whether pre-game implied probability changes are fully reflected into a player's last winning probability. The second and main specification deals with disentangling insider-trading from non-insider-trading related variation in odds. We use an IV approach to split the mismeasured insider-trading related content of odds changes to solve the attenuation bias. In the third specification we run the first stage IV regression without covariates to obtain the fitted values of the insider-trading related variation in odds changes and the fitted values of the residuals. In the final step we regress returns on bets on both the insider-trading and non-insider-trading related variation in odds changes.

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129 See Guiso et al. (2006) for a general discussion on the role of culture in economic outcomes.

130 These results were constructed using Pinnacle's odds. Similar results hold using Marathon's odds.

131 If we control for a player's rank (results not shown) the coefficient [t-statistic] increases [increases] to 1.09 [12.45]. The rank is highly significant and positive with a coefficient of .115.

132 The reason why the *level* of cumulative returns is so low is that bookmakers charge a transaction cost for each game. If we control for a game's spread (results not shown) both the coefficient on CPI and its t-statistic are unchanged. Thus, the results reported in **Figure 3.2** are not driven by higher spreads charged on players with a low CPI.

3.4.1 Implied Probability Changes and Ex-post Game Outcome

A common approach in forensic economics and finance[133] is to exploit the predictions of the EMH to generate test hypothesis to detect illicit actions. The general idea of the EMH is that security prices at any time fully reflect all available information [Fama (1970)]. The first and most lenient test is for weak-form efficiency. It requires current prices to reflect all information contained in historical prices. This can be directly tested in betting markets. If the market correctly forecasts the probability of an event, then, on average, the ex-post frequency of an event should not be significantly different from the ex-ante implied probability.[134] This hypothesis can be tested using a linear probability model in the form of equation (1):

$$win_{i,G,T+1} = \beta_0 + \beta_1 probability_{i,G,T} + u_{i,G,T+1} \tag{1}$$

$win_{i,G,T+1}$ is a binary variable that equals 1 (0) if $Player_i$ won (lost) game G. $probability_{i,G,T}$ is last winning probability of $Player_i$ calculated as the inverse of the last odds before the game, i.e. $probability_{i,G,T} = 1/odd_{i,G,T}$. In all specifications $T+1$ refers to the time period after the end of the game, T to the period just before the game and t to the period where the first odds are released. The first specification of **Table 3.2** tests whether the current probability correctly reflects the ex-post result. The joint test hypothesis is that $\beta_0 = 0$ and $\beta_1 = 1$, which can be rejected with a p-value below .01. One reason to reject efficiency might be the bookmakers' spreads, which (artificially) increase the implied probability of winning.[135] In addition we are interested to test whether past price changes are a predictor of future game outcome. In specification (2) we test whether the direction of probability changes has a significant predictive power on the ex-post result.

$$win_{i,G,T+1} = \beta_0 + \beta_1 probability_{i,G,T} + \beta_2 \Delta probability_{i,G,T-t} + \beta_3 spread_{G,T} + \gamma_i + u_{i,G,T+1} \tag{2}$$

$\Delta probability_{i,G,T-t}$ is the change in probability calculated using the last (T) and first (t) odds. $spread_{G,T}$ is the spread, i.e. margin, charged by the bookmaker. γ_i are time-invariant player fixed effects that will be added in **Table 3.2** Panel B. In contrast to specification (1) the joint test hypothesis in the pooled-OLS regression is that $\beta_0 = 0$, $\beta_1 = 1$and $\beta_2 = 0$. The regression results in **Table 3.2** Panel A (B) are estimated using robust standard errors (clustered standard errors at player ID). We first discuss the results of the pooled-OLS regression in Panel A and then the players' fixed effects results reported in Panel B.

Adding the spread to the regression, specification (2.2), takes away the negative effect on the constant which turns insignificant while it has a minor effect on the coefficient of the implied probability. Performing the

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[133] For a review of forensic economics and finance see Zitzewitz (2012) and Ritter (2008), respectively.

[134] This test was used in a number of empirical paper testing for MEH in betting markets, see for example Woodland and Woodland (1994).

[135] Consider for example that typical odds in a game between two equal players would be 1.9, rather than 2. This implies that a player's implied probability would be 52.6% rather than 50% and the bookmaker's margin assuming balanced books would be 52.6%+52.6%-100%=5.2%.

same joint test as in specification (2.1) we still reject the null of efficient markets. Interestingly, the p-value increases substantially to .048. In the third specification we add the change in probability and leave the spread out. The coefficient on $\Delta probability$ is highly significant and positive. A 10% increase in the ex-ante probability increase the ex-post winning probability of a player by .91% controlling for his last winning probability. This is the first evidence that changes in the probability of a player are not fully reflected in the last pre-game probability. The coefficient on $\Delta probability$ does not change in specification (2.4) when $spread_{G,T}$ is added. Interestingly, in specification (4) the *previous* test for market efficiency cannot be rejected. *Conditional* on the spread and on probability changes we cannot reject that $\beta_0 = 0$ and $\beta_1 = 1$. As $\Delta probability$ is positive and has a p-value below 1% we do reject the weak EMH that current prices reflect all information contained in historical prices. This result provides an interesting insight in understanding the source of inefficiency. The last odds correctly forecast future events, i.e. the coefficient is not significantly different from 1, once we condition on the bookmakers' spread *and* pre-game probability changes. Thus, the market inefficiency seems to stem from order flows before the game starts which seem to contain viable information not fully reflected in the final probability.

In specification (2.5) and (2.6) the sample is split in two: high versus low stake tournaments. The incentives to compete vary greatly across tournaments as discussed in the previous section. Both the prize money and ATP-ranking points are substantially higher in Grand Slams and Masters than in ATP 500 and 250 events. In low stake tournaments we would expect *more* insider trading, as ceteris paribus players have similar odds on their opponents but face lower incentives to compete. In specification (2.5) [(2.6)] we use only games played in ATP 250 and 500 [Grand Slams and Masters] to evaluate whether the inefficiency caused by the positive coefficient on $\Delta probability$ is the same for high and low stake events. Both coefficients on $\Delta probability$ are positive and not statistically significant from each other.[136] The main difference between the two coefficients is the dispersion around the estimate. In low stake events probability changes have a lower dispersion and are significant at the 5%.

One concern of regression model 2 is that we used pooled OLS which do not take into account player-specific unobservable heterogeneity. In **Table 3.2** Panel B we perform the same regression using player fixed effects to obtain the within-player estimate of probability changes. In all six specifications the null that all players' fixed effects, γ_i, are jointly insignificant can be rejected. The regression coefficients are similar as in Panel A. The within-player EMH[137] can always be rejected at p-values below .05 and $\Delta probability$ has the same magnitude as in Panel A and is significant at the 5% level in specification (3) and (4). In specification (5)

[136] I ran a separate regression (results not shown) where I included an interaction term between probability changes and the high-stake tournament dummy which was highly insignificant.

[137] The fact that the joint hypothesis that players' fixed effects are insignificant cannot be rejected shows that the null of the weak EMH can be rejected (results not shown). The hypothesis we tested was whether we could reject the weak EMH conditional on players' fixed effects.

and (6) the regressions are run for high and low stakes tournaments. As in Panel A the coefficients on $\Delta probability$ are not statistically different from each other between tournaments. The within-player effect of $\Delta probability$ is larger for low stake tournaments and significant at the 10% level, while it turns insignificant in high stake events.

Controlling for fixed effects increases the standard error of $\Delta probability$ in all specifications but the effect is still significant at the 5% level in the full specification regression (2.4) in Panel B. Thus, even though the fixed effects results seem weaker, they confirm that betting markets do not incorporate all information from past odds changes. In the next section we consider returns on bets rather than games' outcomes because this is the key variable of interest of traders using private information to earn abnormal returns.

3.4.2 Instrumental Variable Approach

In a perfect natural experiment one would randomly assign players to cheat in a match. Knowing in which games cheating occurred one could run a regression of returns on bets or odds changes on a dummy being one for corrupt games and observe the causal effect of cheating on odds changes and returns. In reality cheating is illegal and thus unobserved. What we observe are odds changes, returns on bets, incentives, game results and a set of players' and tournaments' characteristics. In a standard case of forensic economics a researcher uses his knowledge on the institutional setting and observable variables to derive test hypothesis on whether observed outcomes are the result of a "normal" situation or whether it is the result of hidden behavior. Prominent examples of this approach include stock option backdating [(Narayanan and Seyhun (2008); Heron and Lie (2007)], allocation of hot shares in initial public offerings to corporate executives (Massa et al. (2010); Liu and Ritter (2010)), violations of U.N. sanctions [DellaVigna and La Ferrara (2010)] or the manipulation of test scores by teachers [Jacob and Levitt (2003)]. The common theme of these papers is that they uncover the "footprints" that wrongdoers' actions have left in the data. In this case informed bettors aim at earning abnormal returns from games where a player is corrupt and loses on purpose. To do so they bet money on an event, this in turn has an impact on the odds of that event.

In the tennis betting market odds are set on average 24 hours before a match and little information about the players is revealed before the match. Odds might still change due to incoming order flows. Specification (3) test whether odds changes significantly predict future returns.

$$returns_{i,G,T+1,BM} = \alpha_0 + \alpha_1 \Delta odds_{i,G,T-t,BM} + \boldsymbol{X}_G \delta + \boldsymbol{Z}_i \theta + \gamma_i + u_{i,G,T+1,BM} \tag{3}$$

$returns_{i,G,T+1,BM}$ is defined as $odds_{i,G,T,BM}$-1 (-1) if $Player_i$ wins (loses) match G using odds from bookmaker BM. $\boldsymbol{X}_G$ is a set of control variables that vary at game level such at round, tournament type and year. $\boldsymbol{Z}_i$ are player specific control variables such as a player's seeds in a tournament or his implied probability of winning. γ_i are players fixed effects and $u_{i,G,T+1,BM}$ is the error term. In the following discussion we denote $Player_j$ as the cheating player while $Player_i$ is his opponent. The key variable of interest, $\Delta odds_{i,G,T-t,BM}$, is defined as the pre-game odds changes of $Player_i$ using the odds of bookmaker BM. This difference is observed by bettor and reported in many webpages for different bookmakers.[138] Traders care about returns on bets and if they possess private information on who is going to win the game they will buy (sell) odds on the winning (losing) player. These order flows will affect odds changes which in turn will have a predictive power over who will win the game if the private information is not fully incorporated in the odds. One problem with specification (3) is that it assumes that odds changes reflect just private information rather than incoming public information or simply noise. In any financial market it is impossible to distinguish whether a particular order conveys private information. What is observed is the average change in odds caused by the aggregate amount of orders. Thus, odds changes represent a mismeasured proxy for the true informational content of insider-trading driven order flows. In general, if an explanatory variable is measured with an additive random error its coefficient suffers from attenuation bias, i.e. its coefficient is biased towards zero. The higher the proportion of noise that is due to errors the greater the bias. Assuming that the measurement error formed by the sum of public information and noise is uncorrelated with the true variable of interest and with the disturbance in the regression, then we are in the case of the "classical" measurement error. In these cases an IV approach might be used to solve the attenuation bias. We need an instrument that is uncorrelated with the measurement error and the error in the regression of interest but correlated with insider trading. Let odds changes be defined as the sum of insider trading, news and noise:

$$\Delta odds_{i,G,T-t,BM} = IT_{i,G,T-t,BM} + news_{i,G,T-t,BM} + \varepsilon_{i,G,T-t,BM} \tag{4}$$

We assume that public news, $news$, and noise, ε, are uncorrelated with insider trading, IT, and rewrite (4) as:

$$\Delta odds_{i,G,T-t,BM} = IT_{i,G,T-t,BM} + \omega_{i,G,T-t,BM} \tag{5}$$

In equation (5) ω is the sum of two independent and normally distributed random variables with mean zero, news and noise. Using equation (5) we can rewrite our equation of interest in (3) as:

$$returns_{i,G,T+1,BM} = \alpha_0 + \alpha_1\left(IT_{i,G,T-t,BM} + \omega_{i,G,T-t,BM}\right) + \boldsymbol{X}_G\delta + \boldsymbol{Z}_i\theta + \gamma_i + u_{i,G,T+1,BM} \tag{6}$$

The key variable of interest is IT which measures the level of private information driving the odds changes of $Player_i$. Instead we observe a noisy signal of IT, $\Delta odds$. Running a regression of returns on odds changes

[138] Many betting webpages report both odds and odds changes for tennis games. For example http://www.oddsportal.com/dropping-odds/ has a section dedicated to odds changes for many sport events worldwide including tennis.

will lead to a downward biased coefficient. Assuming that we are in the case of classical measurement error, a way to solve the attenuation bias of α_1 is to find an instrument which satisfies the exclusion restriction and is correlated with the mismeasured variable IT. Considering the discussion on incentives and cultural norms in affecting the likelihood to cheat we use the interaction between $Player_j$'s CPI and $Player_i$'s initial odds as an instrument. Because tournament draws are random conditional on a player's seeding, we also add seeding fixed effects, $s_{j,G}$. The intuition behind the choice of instruments is as follows: Conditional on a player's seed the relative strength of its opponent, and thus his odds, is randomly drawn. Higher odds on an opponent imply a higher return on cheating. In addition conditional on the incentives to cheat some players might have stronger cultural norms towards cheating measured by players' CPI. The first and second stage of our IV approach are:

$$\Delta odds_{i,G,T-t,BM} = \alpha_0 + \alpha_1 CPI_j * odd_{i,G,t,BM} + s_{j,G} + \boldsymbol{X}_G\pi + \boldsymbol{Z}_i\omega + \gamma_i + v_{i,G,T-t,BM} \tag{7}$$

$$returns_{i,G,T+1,BM} = \alpha_0 + \alpha_1 \widehat{\Delta odds}_{i,G,BM,T-t} + \boldsymbol{X}_G\delta + \boldsymbol{Z}_i\theta + \gamma_i + u_{i,G,T+1,BM} \tag{8}$$

Table 3.3 reports the results of the first stage while **Table 3.4** reports the results of the second stage along with OLS estimates. In both Tables we run the regressions both with and without players' fixed effects. In addition in specifications (4.3) to (4.6) we split the sample in two using Grand Slams and Masters as high stake tournaments and ATP 500 and 250 as low stake tournaments. Ceteris paribus the incentives to cheat in lower ranked tournaments are higher than in high stake tournaments and thus we expect the magnitude of odds changes driven by insider trading to be larger.

$CPI_j * odd_{i,G,t,BM}$ is negative and highly significant in all specifications of **Table 3.3**. The higher $Player_j$'s CPI *and* the higher the initial odds on $Player_i$, the larger the decrease in $Player_i$'s odds over time. Conditional on $Player_j$'s seeding, the higher CPI_j and the higher the incentive to cheat by $Player_j$, measured by $odd_{i,G,t,BM}$, the larger the demand for $Player_i$'s odds and thus the larger the decrease in $Player_i$'s odds before the game. Adding players' fixed effects does not significantly change the results. The first stage R^2 using (not using) players' fixed effects is 14.4 (14.5). The F-statistic of the first stage using (not using) players' fixed effects is 121 (47), far above the usual rule of thumb of 10. This provides evidence that the instruments are not weak and explain a significant part of the variation of odds changes.

Panel A in **Table 3.4** reports the OLS and 2SLS estimates of the informational content of past odds changes on future returns on bets. Odd (even) specifications are estimated with OLS (2SLS). Panel A (B) presents the results excluding (including) players' fixed effects. In all specifications we report heteroskedasticity-robust standard errors which we cluster at player ID in Panel B.

The effect of odds changes on returns is negative and highly significant in all specifications. In line with our hypothesis of insider trading being mismeasured, the coefficients in all IV regressions are larger than in the OLS regressions. Using the entire tournament sample (columns 1 and 2) the estimated IV effect is about 4 times larger than the OLS estimate. A 10% decrease in $Player_i$'s odds before the game instrumented by the ex-ante incentives to cheat increases the ex-post returns on $Player_i$'s bets by 1%. In comparison the OLS estimate points to a .24% change. We get an even larger effect by splitting the sample in low (columns 3 and 4) and high (columns 5 and 6) stake tournaments. The IV estimate of the effect of odds changes is twice as large in tournaments with lower incentives to compete as in tournaments with high incentives. A 10% decrease in $Player_i$'s odds before the game results in a 1.9% (.85%) increase in ex-post returns in low (high) stake tournaments as estimated by IV. While the OLS result show that a 10% decrease in $Player_i$'s odds before the game results in a .3% (.23%) increase in ex-post returns in low (high) stake tournaments. The IV estimates are clearly larger and in particular they are larger in those tournaments where we would expect insider trading to happen more often. As these results might suffer from unobserved time-invariant heterogeneity between players we perform the same analysis using players' fixed in **Table 3.4** Panel B. Testing whether all fixed effects are jointly insignificant we cannot reject the null of joint insignificance at a very high p-value in all specifications of Panel B. Since the fixed effects were jointly significant in the first stage we report both estimates with and without fixed effects. The results are in line with the estimates of the pooled regression. The coefficients of odds changes in the players' fixed effect regression model are very similar to the results of the pooled regression. All the coefficients are negative and significant at the 1% level.

3.4.3 Disentangling Insider-Trading Variation and News

In the last approach we decompose the variations of odds changes in two parts. The first type is the one related to insider trading and the second is the remaining unexplained variation orthogonal to insider trading. In the previous IV analysis this variation was modeled to be the error in measurement of the true insider-trading related variation in pre-game odds. We now take a different perspective and regard this non-insider-trading related variation as potentially informative. Most importantly it might contain news-related variation in odds. We first run the following regression:

$$\Delta odds_{i,G,T-t,BM} = \alpha_0 + \alpha_1 CPI_j * odd_{i,G,t,BM} + s_{j,G} + u_{i,G,T-t,BM} \tag{9}$$

In the next step we estimate the fitted values and residuals of (9). Finally we run a pooled and a player fixed effect OLS regression of returns on $\widehat{\Delta odds}_{i,BM,T-t}$, $\hat{u}_{i,G,T-t,BM}$ and the other covariates. The final regression model we estimate is:

$$returns_{i,G,T+1,BM} = \alpha_0 + \mu_1 \Delta\widehat{odds}_{i,G,T-t,BM} + \mu_2 \hat{u}_{i,T-t,G,BM} + \boldsymbol{X}_G \delta + \boldsymbol{Z}_i \theta + \gamma_i + v_{i,G,T+1,BM} \tag{10}$$

In contrast to the first stage of the IV model we do not include covariates when disentangling the variation of odds changes and we use the estimated residuals as explanatory variables in the regression of interest. The intuition behind this regression is to use all the variation of odds changes to explain returns rather than just the insider-trading related part. In all fixed effects regressions the F-statistic testing for the joint significance of the γ_i is very low and the estimated coefficients are very similar to the ones in the pooled regression. Thus, we report only the estimates of the pooled OLS model. In **Table 3.5** we report the estimates of the pooled OLS regression model (10) with heteroskedasticity-robust standard errors in parenthesis.

The coefficient on $\Delta\widehat{odds}$ is negative and significant at the 1% level in all specifications. Using the entire sample of tournaments a 10% decrease in the fitted values of $Player_i$'s odds changes increases the ex-post returns on $Player_i$'s bets by .89% controlling for non-insider-trading related variation. In contrast a 10% decrease in the non-insider-trading related variation of $Player_i$'s odds changes increases the ex-post returns on $Player_i$'s bets by .18%. This effect is stable and significant across specifications and confirms that the non-insider-trading related variation in odds changes has a significant predictive power over future returns even though the effect is not as strong as the insider-trading related one. The coefficient on $\Delta\widehat{odds}$ is larger in low stake tournaments than in high stake ones but not significantly so. A 10% decrease in $\Delta\widehat{odds}$ in low (high) stake events increases returns by 1.13% (.85%), both significant at the 1% level.

These results point out that both insider-trading and non-insider-trading related variation have significant predictive power over future returns. Because we would expect simple noise not to be predictive the non-insider-trading related variations seems to rather stem from (mismeasured) news which has not been fully incorporated in the final odds. Even though news might be mismeasured which leads μ_2 to be downward biased the magnitude of the two effects differs greatly. The coefficient on the insider-trading related part is 4.8 times larger than the non-insider-trading related part. Using an F-test we can easily reject the null of equal effect at a p-value below .01. Thus, both variations of odds changes convey information but the insider-trading related effect dominates.

3.5 Discussion

The aim of the empirical analysis was to test whether pre-game odds changes convey private information about the ex-post returns on bets. Tennis players with low cultural norms on corruption facing high odds on their opponent might rationally decide to cheat and sell this inside information. As a consequence the odds (implied probability) on the cheater increase (decrease) and the odds (implied probability) on his opponent decrease (increase). The empirical results provide strong evidence in favor of this hypothesis. Odds changes instrumented by the interaction of the incentives to cheat and time-invariant cultural norms on corruption are a highly significant predictor of a bet's returns. In line with the results of the rational crime theory this effect is larger in low stakes events, when stronger players face similar odds on their opponent but have less incentives to compete.

The first step involved testing the weak-form EMH, i.e. whether current prices reflect all available past price information. As the implied probability of a player winning a game is the multiplicative inverse of his odds we first tested whether betting markets efficiently forecast the probability of an event. Once we control for past probability changes and the bookmaker's spread, the coefficient on the implied winning probability is not statistically significant from 1 and the constant is not significantly different from 0 as we would expect in the case the weak-form EMH holds true.

The larger market inefficiency seems to stem from past odds changes and players' fixed effects, both of which should be insignificant but in fact are highly significant. This gives some indication that the "true" market inefficiency is hidden in pre-game odds changes rather than in the final odds.

The core part of the empirical analysis used an IV approach to relate past odds changes, instrumented with incentives to cheat and time-invariant cultural norms on cheating, on returns of bets. Once we isolated the insider-trading variation the results pointed out that a decrease of 10% of a player's pre-game odds is associated with an increase in 1% of his returns. This effect duplicates in low stake tournaments.

In the last part of the analysis we decomposed odds changes in two parts: insider-trading and non-insider-trading related variation. The results show that non-insider-trading related variation has also significant predictive power on future returns but the coefficient is about one fifth of the insider-trading-related one. Thus, betting markets might not be fully efficient in incorporating the arrival of public news, but the largest inefficiency seems to be driven by the insider-trading related variation.

These results show that odds changes convey private information and are a significant predictor of future returns. This is especially the case in low stake tournaments where players face lower incentives to compete. A question which arises is how liquid betting markets actually are and thus how much money bettors can place on a single event. While there are no official statistics on the total money betted on an event, in the example of the "Sopot match" discussed in the introduction all $7 million bets were matched on Betfair's trading platform. Odds on Davydenko were relatively high compared to what they *should have* been. This might have incentivized many bettors to place bets on Davydenko. Similarly as in a betting exchange, as long as bookmakers can balance their books they are indifferent on the market equilibrium odds as their per-bet margin is fixed.

3.6 Conclusion

In this paper we tested whether odds changes before professional tennis games are driven by private information and whether they are a significant predictor of future returns on bets. An important question for securities markets is the extent to which price changes reflect the arrival of private information and whether prices fully reflect all past price information. Securities markets are bombarded by news and disentangling the reason of price changes is a tedious task. In addition stocks don't have a definite end, i.e. they potentially live forever. This makes it difficult to establish an objective time interval to calculate the cumulative excessive returns. Betting markets provide a natural environment to test theories of price formation. They are large, most information is directly observable, incentives for players are clear and events have a defined start and end. Using a large and detailed data set consisting of odds changes for more than 16,000 unique games we provide evidence that pre-game odds changes are partially driven by private information and significantly predict returns on bets. This effect is larger in games where incentives to compete are lower. The test hypotheses in the empirical analysis are derived from the efficient market hypothesis (EMH) and the theory of rational crime. In professional tennis betting markets the first odds before games are released on average 24 hours prior to the game start and little information is revealed between initial and last odds before the game start. Still, odds might change due to order flows which themselves contain information. A corrupt tennis player facing high odds on his opponent might rationally decide to throw the match and sell this information to bettors who start buying odds on that player's opponent. Losing a game is uncomplicated and requires the will to lose of only one player. Over time markets react to incoming order flows by adjusting the expectations on the event and consequently the odds on the cheating player (his opponent) increase (decrease). Alternatively, observed odds changes might be driven by the arrival of public information or simply noise. The aim of the empirical analysis was to disentangle insider-trading from non-insider-trading related odds changes and test whether insider-trading driven odds changes are a significant predictor of future returns.

In our main specification we model odds changes as the sum of insider trading, news and noise. To solve the attenuation bias driven by mismeasured insider trading, we instrumented odds changes with the interaction between an exogenous incentive shift to cheat and a player's time-invariant cultural norms on corruption. Both the OLS and IV estimates of returns on bets on odds changes were highly significant and in the expected direction. In line with our error in variables approach, once the variation of odds is instrumented the coefficient quadruplicates. The average effect of a 10% decrease in pre-game odds using OLS (IV) is a .24% (1%) increase in returns. Finally, as incentives to compete for prize money and ATP-ranking points differ substantially across tournaments we split the sample in high and low stake events. In line with the predictions of the rational crime theory the instrumented effect of odds changes on returns is twice as large in law stake events as in high stake ones. In the IV specification a 10% decrease in pre-game odds causes a 1.9%

increase in returns in low stake and a .85% increase in returns in high stake tournaments both significant at the 1% level.

These results pose a serious threat to markets regulators. Insider-trading related odds changes seem to be a strong predictor of returns. While an econometric analysis can test whether there is significant evidence of insider trading, a shortcoming of this approach is that it relies on recognizing systematic patterns emerging over large samples but it is of little use in specific case. However, using repeated observations of the same players over time, the economic approach might be useful in selecting which players are most likely to have cheated which permits authorities to focus their limited resources on cases with the highest ex-ante likelihood of wrongdoing. An additional benefit of the economic approach is that it yields a good understanding of the incentives driving corrupt behavior. This analysis also confirms the strong link between incentives to cheat and cheating. Changing the ex-ante incentives to compete for tennis players might prove to be a successful strategy to fight match fixing. If ATP-ranking points and prize money are increased in initial rounds of tournaments and are more balanced across tournaments players will face higher incentives to compete and less incentives to cheat.

3.7 Appendix: Tables and Figures

Table 3.1: Summary Statistic

Variable	Mean	Median	St.Dev.	Min	Max	Obs.
Win	0.500	0.500	0.500	0	1	65,628
Returns on Bets	-0.0588	0	1.279	-1	40	59,030
Last Odds	2.996	1.920	4.105	1	121	59,030
First Odds	2.785	1.910	3.162	1	81	59,146
Implied Prob. Last Odds	0.518	0.521	0.240	0.00826	1	59,030
Implied Prob. First Odds	0.519	0.524	0.228	0.0123	1	59,146
Spread Last Odds	0.0362	0.0293	0.0200	0.00248	0.446	59,028
Spread First Odds	0.0373	0.0293	0.0213	0.00244	0.0916	59,146
Odds Changes	0.214	0	1.656	-30	91.84	59,030
Imp. Probability Changes	-0.000550	0	0.0558	-0.780	0.797	59,030
Rank	96.90	68	97.56	1	500	65,628
Age	26.26	26.16	3.383	15.94	44.06	65,608
Corruption Perc. Index (Inv.)	-5.786	-6.200	2.103	-9.500	-1.600	65,514
CPI*Fist Odds	-16.43	-11.13	22.26	-712.8	-1.600	59,071
Fitted non-IT variation	-0	-0.0201	1.543	-43.28	87.13	58,955
Fitted IT Variation	0.215	0.0689	0.607	-0.364	18.89	59,071
Year	2,010	2,010	1.399	2,008	2,012	65,628
Round Tournament	6.816	6	3.000	1	12	65,628
Rank Tournament	2.692	2	0.838	2	4	65,628
Total Games by Player	715.0	708	389.2	4	1,612	27,434
Total Returns on Opponent (Pinnacle)	-14.14	-5.644	29.20	-151.4	37.24	27,434
Seeds	2.290	0	5.226	0	32	65,628
Time lag First last Odds	25.06	19.17	19.39	0	133.5	59,086
Player ID	414.0	412	218.2	1	784	65,608
Bookmaker	0.536	1	0.499	0	1	59,146

The table shows summary statistics of the variables used. The level of observation is a player in a match using either the odds of Pinnacle Sports or Marathon Bet. The data consists of 16410 unique matches. The odds observations are not perfectly balanced across bookmakers. Out of the 59030 total observations 27340 come from Marathon Bet while 31690 come from Pinnacle.

Table 3.2: Game outcome, implied probability and probability changes

Panel A: Pooled Data						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Binary outcome: 1 if $Player_i$ wins the game; 0 loses the game					
Tournaments	All	All	All	All	Low Stake	High Stake
$\text{Probability}_{\text{Last Odd}}$	1.013***	1.015***	1.006***	1.008***	1.009***	1.008***
	(0.00616)	(0.00616)	(0.00662)	(0.00662)	(0.00985)	(0.00886)
$\text{Spread}_{\text{Last Odd}}$		-0.508***		-0.504***	-0.504***	-0.504***
		(0.0910)		(0.0912)	(0.124)	(0.134)
$\Delta\text{Probability}_{\text{T-t}}$			0.0915***	0.0884***	0.0938**	0.0821*
			(0.0339)	(0.0339)	(0.0477)	(0.0479)
Constant	-0.025***	-0.00757	-0.0214***	-0.00417	-0.00466	-0.00372
	(0.00366)	(0.00482)	(0.00388)	(0.00497)	(0.00705)	(0.00702)
Cons=0; Prob=1	<.01	0.048	<.01	0.44	0.63	0.69
Cons=0; Prob=1; ΔProb=0			<.01	<.01	0.069	0.13
Observations	59,030	59,028	59,030	59,028	33,132	25,896
R-squared	0.236	0.237	0.236	0.237	0.209	0.272

Panel B: Players' Fixed Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Binary outcome: 1 if $Player_i$ wins the game; 0 loses the game					
Tournaments	All	All	All	All	Low Stake	High Stake
$\text{Probability}_{\text{Last Odd}}$	0.990***	0.992***	0.982***	0.984***	0.983***	1.000***
	(0.0111)	(0.0112)	(0.0121)	(0.0123)	(0.0164)	(0.0162)
$\text{Spread}_{\text{Last Odd}}$		-0.191***		-0.183***	-0.194**	-0.0534
		(0.0648)		(0.0655)	(0.0813)	(0.108)
$\Delta\text{Probability}_{\text{T-t}}$			0.0901**	0.0879**	0.101*	0.0652
			(0.0387)	(0.0389)	(0.0574)	(0.0553)
Constant	-0.0127**	-0.00703	-0.00851	-0.00314	-0.00219	-0.0161*
	(0.00573)	(0.00579)	(0.00630)	(0.00625)	(0.00838)	(0.00839)
Cons=0; Prob=1	<.01	<.01	<.01	<.01	<.01	<.01
Cons=0; Prob=1; ΔProb=0			<.01	<.01	<.01	<.01
F-Test FE=0	<.01	<.01	<.01	<.01	<.01	<.01
Observations	59,015	59,013	59,015	59,013	33,123	25,890
R-squared	0.183	0.183	0.183	0.183	0.154	0.220
Number of Players' FE	730	730	730	730	650	570

Table 3.2 reports the OLS estimates of the weak Efficient Market Hypothesis (EMH) test. The dependent variable is binary and takes the value of one if the player won the match. Panel A uses pooled OLS while Panel B uses players' fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses for Panel A, while in Panel B the robust standard errors are clustered at player ID. In columns (1) to (4) all the data is used while in columns (5) and (6) the data is split in low and high stake tournaments respectively. Low stake refers to ATP-250 and ATP-500 tournaments, while high stake refers to Masters and Grand Slam tournaments. The rows following the constant report the p-values for the weak EMH test. P-values below 1% are reported as "<.01". All tests are joint test, ";" denotes "and". In Panel B the "F-Test FE" refers to the joint test of all player fixed effects being jointly insignificant. The regressors "Probability" and "Spread" are computed using the last odds.

Table 3.3: First Stage Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$\Delta odds_{i,G,T-t,BM}$: Difference in the pre-game odds of $Player_i$					
Regression Model	Pooled	FE	Pooled	FE	Pooled	FE
Tournaments	All	All	Low Stake	Low Stake	High Stake	High Stake
CPI_j*$Odd_{i,\text{ First Odd}}$	-0.023***	-0.0268***	-0.0134***	-0.0156**	-0.0252***	-0.0296***
	(0.00279)	(0.00357)	(0.00331)	(0.00709)	(0.00383)	(0.00446)
$Spread_{\text{First Odd}}$	2.723***	3.079***	2.894***	3.076***	1.949**	2.323
	(0.539)	(1.138)	(0.620)	(1.175)	(0.983)	(1.411)
$Probability_{i,\text{Last Odd}}$	0.685***	0.588***	0.689***	0.668***	0.739***	0.694***
	(0.0950)	(0.110)	(0.0931)	(0.195)	(0.134)	(0.156)
MarathonBet	-0.232***	-0.239***	-0.177***	-0.180***	-0.316***	-0.318***
	(0.0213)	(0.0769)	(0.0253)	(0.0576)	(0.0373)	(0.106)
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Tournament Rank FE	Yes	Yes				
$Seed_j$ FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Player FE		Yes		Yes		Yes
Constant	-0.622***	-0.579***	-0.484***	-0.484***	-0.864***	-0.731***
	(0.0391)	(0.0881)	(0.0326)	(0.0589)	(0.123)	(0.196)
Overall F-statistic	52	139	46	62	31	78
F-test FE		<.01		<.01		1
Observations	58,953	58,953	33,112	33,112	25,841	25,841
R-squared	0.146	0.113	0.096	0.063	0.169	0.135
Number of Players' FE		724		647		565

Table 3.3 reports the OLS estimate of the first stage IV regression. The dependent variable is defined as the pre-game difference in the odds of $Player_i$. To ease the interpretation of the coefficient we refer to $Player_j$ as the cheating player and $Player_i$ as his opponent. "FE" refers to fixed effects. Low stake refers to ATP-250 and ATP-500 tournaments, while high stake refers to Masters and Grand Slam tournaments. The overall F-statistic test for joint significance of all regressors is always above 10. The "F-Test FE" refers to the joint test of all player fixed effects being jointly insignificant. P-values below 1% are reported as "<.01". Heteroskedasticity-robust standard errors are reported in parentheses.

Table 3.4: OLS and IV regression of returns on bets on odds changes

	Panel A: Pooled Data					
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$returns_{i,G,T+1,BM}$: Return on bets on $Player_i$					
Regression Model	OLS	TSLS	OLS	TSLS	OLS	TSLS
Tournaments	All	All	Low Stake	Low Stake	High Stake	High Stake
$\Delta Odd_{i,T-t}$	-0.024***	-0.100***	-0.0307***	-0.196***	-0.0236***	-0.0855***
	(0.00703)	(0.0240)	(0.00945)	(0.0671)	(0.00890)	(0.0247)
MarathonBet	-0.00698	-0.0217	0.00112	-0.0258	-0.0214	-0.0373
	(0.0158)	(0.0166)	(0.0202)	(0.0228)	(0.0253)	(0.0264)
$Spread_{First\ Odd}$	-0.705*	-0.723*	-0.914*	-0.618	-0.307	-0.510
	(0.398)	(0.400)	(0.502)	(0.524)	(0.652)	(0.653)
$Probability_{j,Last\ Odd}$	-0.176***	-0.0451	-0.169***	0.0294	-0.179***	-0.0388
	(0.0274)	(0.0387)	(0.0355)	(0.0718)	(0.0414)	(0.0519)
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Tournament Rank FE	Yes	Yes				
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0977***	0.0396	0.0937**	0.00398	0.102	0.0477
	(0.0322)	(0.0349)	(0.0365)	(0.0479)	(0.0699)	(0.0716)
Instrument		CPI_j*$Odd_{i,t}$ $Seed_j$ FE		CPI_j*$Odd_{i,t}$ $Seed_j$ FE		CPI_j*$Odd_{i,t}$ $Seed_j$ FE
Observations	59,028	58,953	33,132	33,112	25,896	25,841
R-squared	0.003		0.003		0.004	
	Panel B: Players' Fixed Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	$returns_{i,G,T+1,BM}$: Return on bets on $Player_i$					
Regression Model	OLS	TSLS	OLS	TSLS	OLS	TSLS
Tournaments	All	All	Low Stake	Low Stake	High Stake	High Stake
$\Delta Odd_{i,T-t}$	-0.021***	-0.0854***	-0.0273***	-0.219***	-0.0204***	-0.0663***
	(0.00648)	(0.0130)	(0.0103)	(0.0434)	(0.00748)	(0.0144)
MarathonBet	-0.00189	-0.0159	0.00594	-0.0267	-0.0158	-0.0288
	(0.00820)	(0.0159)	(0.0104)	(0.0215)	(0.0130)	(0.0256)
$Spread_{First\ Odd}$	-0.929***	-0.890**	-1.130***	-0.721	-0.560	-0.652
	(0.269)	(0.407)	(0.360)	(0.520)	(0.408)	(0.677)
$Probability_{j,Last\ Odd}$	-0.0196	0.0721**	-0.0405	0.172***	-0.00514	0.0717
	(0.0413)	(0.0338)	(0.0500)	(0.0613)	(0.0616)	(0.0508)
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Tournament Rank FE	Yes	Yes				
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0672	0.0164	0.0718	-0.0331	0.182*	0.118
	(0.0513)	(0.0346)	(0.0472)	(0.0453)	(0.108)	(0.0718)

[Cont.]

Instrument		CPI_j*$Odd_{i,t}$ Seed$_j$ FE		CPI_j*$Odd_{i,t}$ Seed$_j$ FE		CPI_j*$Odd_{i,t}$ Seed$_j$ FE
Observations	59,013	58,953	33,123	33,112	25,890	25,841
R-squared	0.003		0.003		0.004	
Number of Players' FE	730	724	650	647	570	565

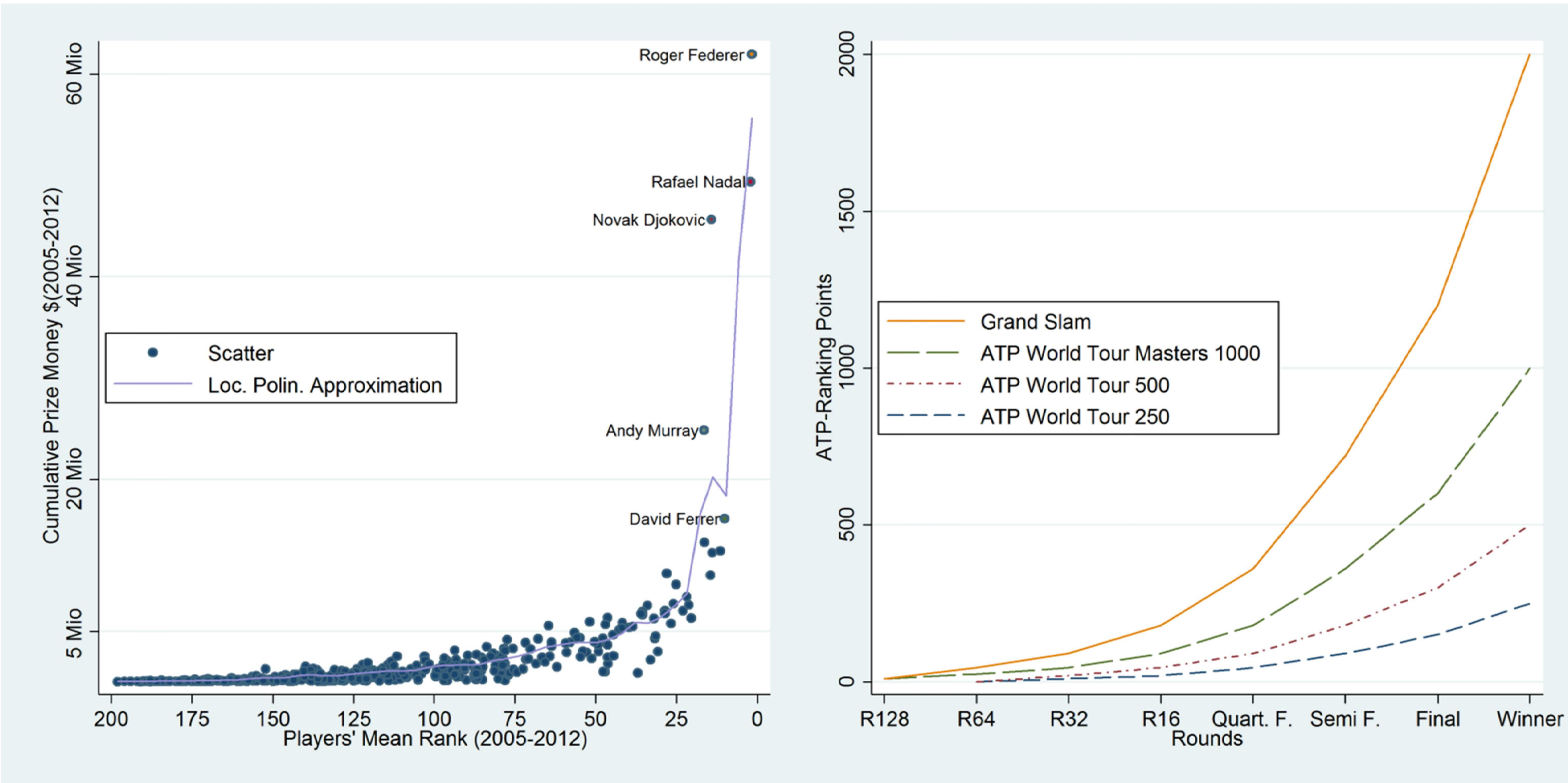
Table 3.4 reports the estimates of the main empirical specification testing whether insider-trading driven odds changes have a predictive power on future returns. The dependent variable is defined as the return on bets on $Player_i$ using odds of bookmaker BM in game G. Odd (even) columns report OLS (2SLS) estimates. The set of instruments used in the even columns is the interaction between the $Player_j$'s CPI and the initial odds on $Player_i$. The additional set of instruments consists of $Player_j$'s seed in game G. "FE" refers to fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. In the OLS regressions with players' FE standard errors are clustered at player ID.

Table 3.5: Returns, insider trading and news

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	$returns_{i,G,T+1,BM}$: Return on bets on $Player_i$				
Tournaments	All	All	All	Low Stake	High Stake
FV $\Delta Odd_{i,T-t}$	-0.0870***		-0.0897***	-0.113***	-0.0853***
	(0.0185)		(0.0187)	(0.0329)	(0.0221)
FV News		-0.0173**	-0.0187***	-0.0256***	-0.0172*
		(0.00713)	(0.00717)	(0.00899)	(0.00934)
MarathonBet	0.00170	-0.00627	-0.00266	0.00432	-0.0158
	(0.0161)	(0.0158)	(0.0158)	(0.0202)	(0.0253)
$Spread_{First\ Odd}$	-1.002**	-0.646	-0.950**	-1.116**	-0.646
	(0.401)	(0.396)	(0.398)	(0.501)	(0.653)
$Probability_{j,Last\ Odd}$	-0.103***	-0.212***	-0.0918***	-0.0766*	-0.0880**
	(0.0279)	(0.0277)	(0.0280)	(0.0393)	(0.0401)
Round FE	Yes	Yes	Yes	Yes	Yes
Tournament Rank FE	Yes	Yes	Yes		
Year FE	Yes	Yes	Yes	Yes	Yes
Constant	0.0824**	0.110***	0.0743**	0.0679*	0.0841
	(0.0323)	(0.0324)	(0.0321)	(0.0367)	(0.0698)
Observations	58,953	58,953	58,953	33,112	25,841
R-squared	0.003	0.003	0.004	0.003	0.005

Table 3.5 reports the estimates of the regression model that disentangles insider-trading related variation in odds changes from non-insider-trading related variation. The dependent variable is defined as the return on bets on $Player_i$ using odds of bookmaker BM in game G. "FV $\Delta Odd_{i,T-t}$" refers to the "fitted values" of regression model (9), whereas "FV News" are the "fitted values" of the residuals of regression model (9). Heteroskedasticity-robust standard errors are reported in parentheses.

Figure 3.1: Players' Prize Money and ATP-Ranking Points



The left part of Figure 3.1 shows the scatter plot between the cumulative prize money a player earned over the time period 2005 to 2012 and his mean rank over the same period. The right part shows the ATP-ranking points a player earns if he reaches a particular round of the tournament and the ATP-ranking point differences between the four different types of tournaments of the main world tour. The cumulative prize money was computed by adding the prize money received by a player over the time period 2005 to 2012. The x-axis shows the mean ATP-rank of a player over the same time period. Cumulative prize money is highly convex in rank. Out of about $753million in total prize money over the aforementioned time period the top-5 (10) [20] earners received 26% (34%) [44%]. The median earner had a mean rank of 27. The right side of Figure 3.1 shows the ranking points in different types of tournaments conditional on the round achieved by a player. Ranking points are convex within a tournament type and substantially differ across tournaments.

Figure 3.2: Cumulative Returns and CPI

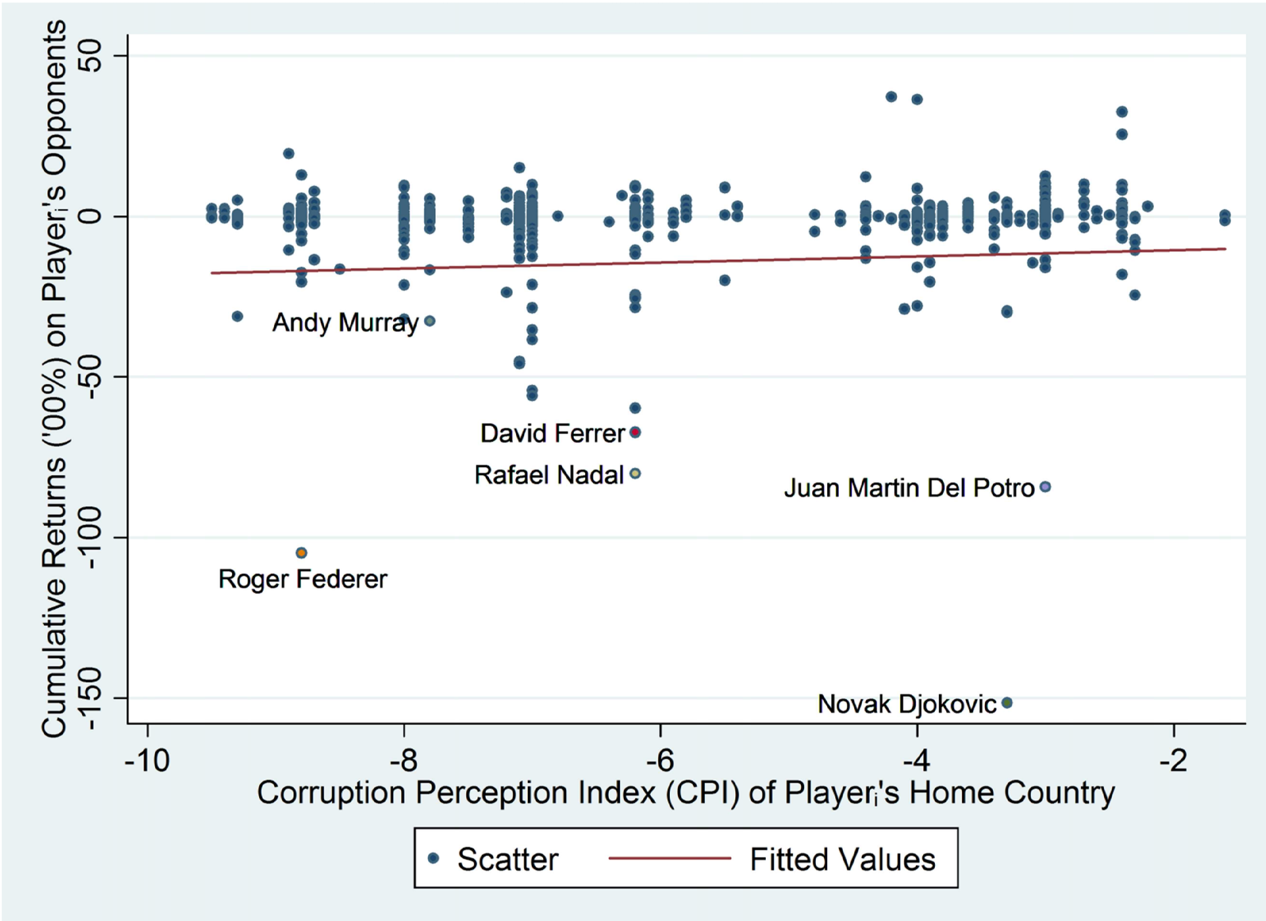


Figure 3.2 shows the scatter plot and fitted line between the cumulative returns on a player's opponent and his Corruption Perception Index (CPI). The cumulative returns are computed by summing up all the bets on the opponent of a player using Pinnacle's odds. Higher values of CPI correspond to higher corruption level in the country of origin of that player. The regression coefficient on CPI is .94 and significant at the 1% level. Moving from a low corruption country like Finland (CPI=-9.4) to a high corruption country like Ukraine (CPI=-2.3) increases the cumulative returns on that player's opponent from -1700% to -1080%. This positive relation is robust to including the spread charged by the bookmaker and a player's rank (results not shown).

Chapter 4: Leading-effect vs. Risk-taking in Dynamic Tournaments: Evidence from a Real-life Randomized Experiment

4.1 Introduction

Tournaments are widely used in corporations, politics and sports to provide incentives to work hard or to select the best agents. A key aspect of tournaments is that participants are rewarded on the basis of their relative rather than absolute performance. In addition participants often compete in a dynamic setting with information feedback and under asymmetric conditions. Two fund managers acting on different markets and competing to attract new funds might get intermediate feedback of performance and change their strategy before the investors' choice of asset allocation. In the US the major party candidates are determined through a sequence of state-level primary elections where candidates can constantly monitor their interim rank and change their strategies accordingly. Most of the literature on tournaments points out how incentives mitigate the conflicting objectives between principal and agents inducing higher levels of effort. However, little is known on the effect of revealing information on relative performance during a dynamic tournament. In this setting effort is not the only choice variable and risk-taking might cause `order effects'. By `order effects', we mean the advantage or disadvantage to a player when performing either in a given sequence or under different conditions that are determined by the regulation of the tournament. Two distinct order effects might arise in a dynamic setting with intermediate information feedback where both effort and risk-taking are relevant. First, there might be a leading-effect. Teams taking the lead at the beginning of the tournament might experience an encouragement-effect and/or teams lagging behind might feel discouraged. This effect is due to the fact that the leading (lagging) player has an incentive to exert more (less) effort as she faces a larger (smaller) `effective prize' from winning the second game (Konrad and Kovenock, 2009; Malueg and Yates, 2010). Second, teams lagging behind might increase risk-taking at final stages of the tournament as they have `nothing to lose' (Cabral, 2003; Hvide, 2002).

In this paper we take advantage of a unique natural experiment with 1,146 observations where highly paid professionals have strong incentives to compete and know the setting very well. In two-game soccer knock-out competitions, teams are randomly drawn to have an advantage (home game) either in the first or second game. The team randomly drawn to play the first game at home wins the first game more often (53% home win, 26% draw and 21% away win) and thus might benefit from a leading-effect. The team playing the second game at home is more likely to lag behind after the first game and thus might increase risk-taking in the second game. Using this real life situation that guarantees internal validity we investigate the selection efficiency of tournaments with information feedback and asymmetric initial conditions.

The main concern of using a natural experiment as opposed to an experiment in the laboratory is that strategies, and in specific effort and risk choices, are unobserved. By exploiting our rich dataset we develop an identification strategy capable of distinguishing the relevance of each effect, i.e. leading-effect and risk-taking, on the winning probability of teams.

Most of the literature focuses on how tournament design influences the behavior of participants and in particular on the incentive mechanism of tournaments (Ehrenberg and Bognanno, 1990; Knoeber and Thurman, 1994; Lazear and Rosen, 1981). Another strand of literature elaborates on the comparison between tournaments and other performance schemes (Baker, Gibbs and Holmstrom, 1994; Green and Stokey, 1983; Lazear, 2000; Oyer, 1998).

Less research is done on how the dynamic structure of tournaments affects the ex-ante winning probabilities of participants through order effects. This is of great importance for two reasons. First, from the perspective of the organizer, tournaments are often used as a selection mechanism to identify the best candidates, e.g. for job promotion or research grants. Better agents should win the tournament. This may not be the case when the regulation randomly attributes a considerable advantage to one player. Second, from the perspective of the participant, it is not fair if one player receives an advantage due to a randomized order of play. Thus, in dynamic tournaments, the order of interaction must be carefully designed.

We benefit from a randomized natural experiment in soccer knock-out competitions with two games in which each team is randomly drawn to play either the first or the second game at home. Given the robustly identified result that home teams have an advantage (Clarke and Norman, 1995; Ferrall and Smith, 1999; Neave and Wolfson, 2003; Pollard, 1986), we investigate whether the random allocation of this advantage in either the first or second game has an impact on the probability of winning the knock-out. The setting of a knock-out allows us to go beyond what can usually be done in empirical work on selection efficiency in corporate tournaments where many variables are not observable and the data is not available. Sport is in many ways the perfect environment for testing economic theories about decision-making.[139] There is an abundance of readily available data, the goals of participants in sporting contests are relatively uncomplicated and the outcomes are extremely clear. Szymanski (2003) concludes that sports data is a valuable source for economists trying to understand the relationship between tournament structure and effort choices and to test theoretical predictions against the data.

Our setting of a natural experiment allows us to study how the randomly assigned order of advantages might affect winning probabilities. Without order effects, teams' winning probabilities should be independent of whether they play the first or second game at home. We refer to this condition as `neutral structure' of the

[139] Sports contests have been successfully used to show the use of mixed strategies (Chiappori, Levitt and Groseclose, 2002; Walker and Wooders, 2001), risk aversion (Pope and Schweitzer, 2011), cheating (Duggan and Levitt, 2002) and labor market participation of women (Stevenson, 2010).

tournament. If, however, order effects play a relevant role, professional and experienced teams in high stake environments will exploit such favorable conditions by changing their strategy and increase their winning probability.

Our empirical analysis relates to two strands of literature on dynamic tournaments[140] that focus on two aspects of order effects: leading-effect and risk-taking. Regarding the leading-effect, Malueg and Yates (2010) and Ferrall and Smith (1999) study the existence of strategic effects in dynamic tournaments using individual and team level sports data, respectively. Malueg and Yates (2010) find that players in best-of-three tennis tournaments strategically adjust efforts across sets conditional on the intermediate score. Given equal ex-ante abilities, players that take the lead by winning the first set exert higher effort in the second set than the opponents as they face a larger `effective prize' from winning the second set.[141] Ferrall and Smith (1999) analyze data from team sports such as basketball, baseball and hockey and do not find evidence of leading-effects.[142] Klumpp and Polborn (2006) model US presidential primaries that consist of a sequence of elections within a political party in different districts between two candidates. They find evidence of strategic effects. Consistent with empirical evidence, the winner of early districts is endogenously more likely to win later districts than the loser. A possible explanation for this mixed evidence is that incentives within teams may attenuate incentive effects across games which might explain why strategic effects are present in settings where individuals rather than teams compete (Ferrall and Smith, 1999).

In addition to effort choices, in many situations agents may also choose risk to influence their performance. Fund managers could pick riskier assets and managers opt for riskier investments or production technologies if they are lagging behind. In such cases the latter results on leading-effects do not hold true in general. When players can choose both effort and risk, players choose riskier actions and lower effort in equilibrium as compared to the equilibrium effort without risk-taking (Hvide, 2002). Also, when risk is the only choice variable, the selection efficiency of tournaments deteriorates (Hvide and Kristiansen, 2003). Cabral (2003) sets up a model in which firms can choose between a safe, low-variance and a risky, high-variance research and development strategy. He provides sufficient conditions under which the firm lagging behind chooses a riskier strategy than the leader. As compared to a model without risk-taking, weaker players or players lagging behind choose riskier strategies because they have `nothing to lose'.

Due to the difficulty to analyze effort and risk choices separately the empirical literature confirming the theoretical result of `gambling for resurrection' is relatively underdeveloped. One notable exception are

140 See Konrad (2009) for a thorough overview of the literature on dynamic contests.

141 See also Konrad and Kovenock (2009) who analyze multi-battle, all-pay auctions and find that, with intermediate prizes, even a large lead by one player does not fully discourage the laggard. Without intermediate prizes, laggards may only drop out if they are lagging too far behind.

142 Apesteguia and Palacios-Huerta (2010) study penalty kicks with randomly assigned order of who shoots first and find that teams randomly allocated to take the first kick win 60.5% of the shoot-outs. They ascribe this effect of sequential moves to `psychological pressure' on the kicker of the second-kicking team. Note, however, that Kocher, Lenz and Sutter (2012) cannot replicate this positive effect on teams kicking first in a larger sample of shoot-outs with 540 observations.

Genakos and Pagliero (2012) who study the impact of interim rank on risk-taking and performance in weightlifting competitions. They find that risk-taking takes an inverted-U relationship with interim rank where competitors that are ranked just behind the leader take more risk. In addition Chevalier and Ellison (1997) find that mutual funds with relatively low mid-year performance increase fund volatility, relative to the funds with relatively high mid-year performance.

The view that the player lagging behind increases risk-taking is not unanimous. Kräkel and Sliwka (2004) analyze a two-player tournament where players have asymmetric abilities. They show that depending on the interplay of effort and probability of winning and the degree of asymmetry between agents diverse equilibria are possible. For example, both agents may choose a high or low risk strategy. Nieken and Sliwka (2010) analyze a static model in which a leading player and a lagging player decide between risky and safe strategies. They show that the decisions depend on the correlation between contestants' outcomes of risky strategies. If the correlation is low, the player lagging behind increases risk whereas the leader plays safe in order to protect her lead. However, if the correlation is high, it might be optimal for the leader to follow the laggard's risky strategy.[143] In a high-correlation environment it may well be attractive for the leading agent to imitate the competitor's risky strategy. Independent of whether the strategy fails or succeeds the relative position remains unchanged when the strategy can be exactly replicated. Thus, choosing the risky strategy becomes a means to protect the lead. If the outcomes of risky strategies are uncorrelated the imitation of risk-taking is ruled out from the outset.

Building on this theoretical framework on leading-effects and risk-taking we test two alternative predictions. First, teams playing the first game at home have an advantage at the beginning of the tournament and thus take the lead more often than teams playing the second game at home. This leading-effect might favor teams playing the first game at home by encouraging them to exert more effort or discourage the team lagging behind to exert effort.[144] Second, if the correlation between the contestants' outcomes of risky strategies is low, teams lagging behind at the end of the tournament might increase risk-taking as they have nothing to lose. This `gambling for resurrection' could advantage the team playing the second game at home. If, however, the correlation is high teams might copy the rival's risk-taking strategy and risk-taking may be constant across teams and games. As both effects might be simultaneously taking place, we will first analyze risk-taking both dependent and independent on past performance. As the correlation between contestants'

[143] Taylor (2003) sets up a model in which two heterogeneous fund managers in terms of midyear performance compete for new cash inflows at the end of the year. He shows that the outcome in which the lagging manager gambles and the leading manager indexes only holds if one of the managers is an exogenous benchmark. However, if both managers are active and the outcomes of their risky strategies are correlated, the leading manager is more likely to gamble.

[144] Note that both encouragement as well as discouragement-effect go in the same direction, i.e. they advantage the leading team. In this paper we therefore focus on measuring the impact of the random order of play on the selection efficiency of dynamic tournaments. The distinction between encouragement and discouragement-effect is beyond the scope of the paper.

outcomes of risky strategies is rather high in soccer[145] we show that risk choices are constant and do not depend on past performance. This result is in line with the theoretical predictions by Nieken and Sliwka (2010). We then test for leading-effects given constant risk choices and find no evidence of a significant effect.

The remainder of the paper is organized as follows: Section 2 describes the data. In Section 3, we analyze risk-taking in each game of the knock-out and conditional on the result of the first game. Section 4 provides evidence on the absence of leading-effects. Section 5 discusses the results and Section 6 concludes.

4.2 Data

We construct a dataset with 1,146 games and thus 573 knock-outs where the home advantage is randomly assigned by the regulation of the Union of European Football Associations (UEFA).[146] The data come from the UEFA. **Table 4.1** summarizes the data. The dataset consists of games played in the UEFA Champions League and UEFA Europa League[147] from 1955 until 2009. Observations from 564 games are from the period of 2005-2009 (80 from 2000-2004 and 502 from 1955-1999).

For each game, we observe the date, result, location, knock-out round, tournament, and whether the team passed the knock-out round by goal difference, away goals rule, extra time, or penalty kicks. **Table 4.2** summarizes the relative importance of each UEFA regulation.[148] The goal difference rule is by far the most important one. In the last column, we summarize the winning probability of a single game from the perspective of the home team. The data on home winning probabilities show that the place where the game is disputed significantly affects the outcome. This result is therefore in line with previous literature on the advantage of playing at home.[149]

4.3 Natural Experiment

We study a randomized natural experiment in which the order of an advantage, and thus treatment and control group, are determined via explicit randomization. One team is drawn to play the first game home, and the other to play the second game home. In this natural experiment professionals know exactly the

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[145] Grund and Gürtler (2005) analyze single soccer games. They show that as the opponent increases risk-taking it is easier for the other team to score a goal. Intuitively, in the extreme risk-taking case where a team is lagging behind and the goalkeeper joins the striker in the last few minutes of the game to try to equalize the result, it will be very easy for the opposing team to score a goal.

[146] The UEFA is the administrative and controlling body of the European soccer association. UEFA represents most of the national soccer associations of Europe, runs national and club competitions and controls the prize money, regulations and media rights for those competitions.

[147] UEFA Champions League replaced the European Champion Clubs' Cup after season 1991/1992. UEFA Europa League replaced the UEFA Cup after season 2008/2009.

[148] See the appendix for the structure of the knock-out and the four ways of winning the knock-out.

[149] The average goal difference between home and away team is .72 goals in our sample of 1,146 games. This positive difference is significantly different from zero (same number of home and away goals) at a p-value lower than .01. Other studies (Carmichael and Thomas, 2005; Greenhough et al., 2002; Clarke and Norman, 1995; Pollard, 1986) estimate this advantage to be between .43 and .66 goals in national soccer leagues. However, they also find that the home advantage increases significantly in the geographical distance between the two teams. Hence, our findings which are based on international games between more distant teams are in line with the existing literature on the home advantage.

tournament's setting, the payoffs and incentives are very high, and the process of allocating teams is random. Page and Page (2007) analyze the same knock-out setting. The most important difference between this paper and Page and Page (2007) is that we develop an identification strategy capable of distinguishing between the leading-effect and risk-taking. In addition Page and Page (2007) do not distinguish between random and non-random knock-out rounds. The key difference is that in the non-random knock-out rounds the regulation states that better teams play their last game home. Page and Page (2007) find a positive and significant effect of playing the second game at home (55% vs. 45%), but cannot distinguish between the risk-taking effect and the higher ability of teams playing the last game home. These two confounding variables would favor the teams playing the second game at home, but it is not clear which one, if any, should be the driver of the 45% vs. 55% advantage.

In contrast, we focus on knock-out rounds where the order of the advantage is randomly assigned. This allows us to exploit the properties of a natural experiment and specifically the fact that team characteristics as ability and the treatment effect, i.e. the order of home games, are independent.[150] In the final phase of the major European soccer tournaments, such as the Champions League and the Europa League, teams are randomly drawn to play against each other with a time interval of one to three weeks between the two games. There is a fundamental difference between knock-outs in the final rounds of the tournament, i.e. the quarter- and semi-finals, and the qualification rounds for the main tournament. For instance, Article 8.07 of the Regulations of the UEFA Champions League 2008/09 prescribes that "the ties are determined by means of a draw. The club drawn first plays the first leg of the tie at home". With respect to the qualification phase, Article 8.01 of the Regulations of the UEFA Champions League 2008/09 states that "the UEFA administration seeds clubs for the qualifying phase, the play-offs and the group stage (...) in accordance with the club coefficient ranking established at the beginning of the season (...)".[151] Thus, teams are not randomly drawn to play in a given order, but better teams are allocated to play the second game at home. In light of this fundamental difference, causal inference about the order can only be drawn from the final phase. Therefore, we analyze the 1,146 games where the home advantage is randomly assigned.

Because of the random draw, the Average Treatment Effect (ATE) is defined as the difference between the two groups' mean winning probabilities. Let y_S (y_F) denote the winning probability of a team playing the second (first) game at home, and let w denote the allocation of home and away games. The average treatment effect is $ATE_S = ATE_F = E(y|w = 1) - E(y|w = 0)$, if w is statistically independent of y_S and y_F.

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150 We also tried an identification strategy similar to Malueg and Yates (2010) and selected equally skilled teams using the smallest possible positive goal difference of the first game, i.e. a one goal lead by the home team. The results on leading-effect and risk-taking were unchanged.

151 Article 6.09 states that "the quarter-final pairings are determined by means of a draw. The quarter-finals are played under the cup (knock-out) system, on a home-and-away basis (two legs)." Article 6.10 prescribes that "the semi-final pairings are determined by means of a draw." The same rules apply to the Round of last 16 as well as the quarter- and semi-finals of the Europa League.

As the theoretical work on dynamic tournaments points out, two effects going in opposite directions may emerge from such a setting. First, teams may get discouraged (encouraged) after an initial loss (victory) which gives them information feedback on their opponent's ability. Second, teams lagging behind might choose riskier strategies.[152]

The econometric problem arising from these different strategies can be illustrated as follows. Let $\varepsilon_{S,t}$ ($\varepsilon_{F,t}$) be the risk-taking in the $t = first;\ second$ game of team S (F) that plays the second (first) game at home. Then $E(y|w = \{1,0\}) \neq E(y|w = \{1,0\}, \varepsilon_{F,t}, \varepsilon_{S,t})$, as both choice variables $\varepsilon_{F,t}$ and $\varepsilon_{S,t}$ are correlated with the random allocation of the advantage. If we do not control for them, we would have a biased estimator of order effects as the analysis would suffer from an omitted variable bias. Therefore, we must first understand what type of model drives teams' risk choices. As shown by Nieken and Sliwka (2010), if the correlation between contestants' outcomes of risky strategies is low, then there is an incentive for the team lagging behind to increase risk. If this correlation is high, teams tend to copy the rival's strategy more often. Specifically to our setting, if the correlation is high F anticipates the risk strategy that S chooses in the second game and chooses the same risk level in the first game. Under this hypothesis $E(y|w = \{1,0\}) = E(y|w = \{1,0\}, \varepsilon_{F,t}, \varepsilon_{S,t})$, as risk choices are identical across games of the knock-out and thus the ATE is the difference of the winning probabilities given the order of the home game. In the next section we provide indirect empirical evidence that the correlation between contestants' outcomes of risky strategies is rather high and thus risk-taking is indeed constant across games and unconditional on past performance.

4.4 Risk-taking Unconditional on Past Performance

The effects of risk-taking can be tested in two ways: first, by comparing the distribution of results (home win, draw, away win) in the first and second game, and second, by comparing the number of goals scored across games. These two measures are interrelated and capture whether there is evidence that risk-taking influences the results across games. Intuitively, if risk-taking differs across games, it should shift the distribution of results towards the extremes (more home/away wins) and increase the number of goals at the end of the knock-out, when one team is lagging behind. We start by analyzing the first measure, i.e. the distribution of results (home (win), draw and away (win)), in the two games.

If risk-taking plays an important role at the end of the knock-out, we should observe significantly fewer draws in the second game and more home/away wins as the effect of an increase in risk would be to shift probability from the median (draw) to the extremes (win/lose). Teams lagging behind towards the end of the knock-out have nothing to lose and thus might be indifferent between a draw and a defeat. Note that

[152] A natural way in which coaches of teams that are lagging behind might increase risk-taking is by substituting defensive players by more offensive ones. Using data on the German soccer league Grund and Gürtler (2005) provide evidence that coaches adopt this strategy. However, risk-taking does not pay off in this setting.

monetary incentives to win the knock-out are very high and often exceed € 3 million,[153] which points to the fact that winning the knock-out really matters. **Figure 4.1** shows the relative frequency of each outcome for 1,146 games where the home advantage is randomly assigned. To test the hypothesis of constant risk-taking in both games we perform a non-parametric Kolmogorov-Smirnov test on the distribution of outcomes. The test cannot reject equality of distribution across games with a p-value of .89. The distributions do not differ, a finding that is in line with our hypothesis that risk choices are constant over games of the knock-out. The second measure we use to evaluate the relevance of risk-taking is the distribution of the sum of the home and away team goals across games. If at the end of the knock-out one team is lagging behind and has an incentive to increase risk-taking, we should observe that the distribution of the sum of the goals is more skewed to the right (more goals) in the second game. The average number of goals in the first (second) game is 2.48 (2.7) implying a difference of .22 goals across games. Doing a two-sided t-test with 573 observations per sample, the .22 difference is statistically significant at a p-value of .031. While this might point to an increase in risk-taking at the end of the knock-out, the magnitude is very small, one goal more every five games, which points to a negligible effect of risk-taking on the outcome of the knock-out.

To confirm this assertion we plot the histogram of the sum of home and away team goals in the two games in **Figure 4.2** and perform a two-sample Kolmogorov-Smirnov test to evaluate whether the distributions are significantly different. Using our 1,146 games, 573 in the first game and 573 in the second game, we cannot reject equality of distribution functions at a p-value of .24. Thus, while there is some evidence that the number of goals increases in the second game, the magnitude is very small and the distribution of the sum of goals is not statistically different across games.

4.5 Risk-taking Conditional on Past Performance

Even though we cannot reject that the distributions of goals are equal across games, strategies across games might not be independent and thus teams might react to past performance. In particular, teams that lost the first game might increase their risk-taking. This strategy, if effective, might lower the selection efficiency of tournaments as predicted by Hvide and Kristiansen (2003). We test this by relating the sum of home and away goals in the second game to the goal difference (home-away) in the first game. If risk-taking depends on past performance we would expect that the sum of goals in the second game increases if the absolute goal difference in the first game increases, i.e. we should observe a U-shaped relation between past performance (goal difference) and the sum of goals.[154] A team lagging behind in the second game, i.e. with a

[153] For instance, in addition to the revenues generated from broadcasting, merchandising, sponsoring and tickets, each of the 32 teams that play in the Champions League receives a minimum payment of € 9.3 million plus rewards of reaching the round of last 16 (€ 3 million), the quarter-finals (€ 3.3 million), the semi-finals (€ 4.2 million) and the final (€ 5.6 million). UEFA (2012). Financial Report 2010/11. UEFA, Nyon: Switzerland.

[154] If S is lagging behind by a large goal difference after the first game it could increase risk-taking which in turn leads to a higher total number of goals in the second game. Symmetrically, if F is leading by a large number of goals it will be easier for F to score additional goals in the second

negative goal difference in the first game, might increase risk-taking as they have nothing to lose which should increase the number of goals in the second game. The team lagging behind might substitute a defensive player for a more offensive one as shown in Grund and Gürtler (2005) which increases the likelihood that the team lagging behind equalizes while at the same time makes it easier for the leading team to score. Both effects point to an increase of the sum of home and away goals in the second game.

If, however, the correlation between contestants' outcomes of risky strategies is relatively high, it might be optimal for both the leading and lagging team to choose the same risk level and thus the sum of goals in the second game should be independent from the goal difference of the first game.

In **Figure 4.3** we plot the mean, interquartile range and 95% confidence interval of the sum of home and away goals in the second game conditional on the goal difference (home-away) in the first game. We can test two alternative hypotheses regarding risk-taking across the two games. First, teams lagging behind increase their risk-taking. Thus there should be a positive relation between the number of goals that a team is lagging behind after the first game and the total number of goals scored in the second game. Second, risk-taking is constant across games and thus there is no relation between past goal difference and the sum of goals in the second game.

As **Figure 4.3** shows, the sum of goals in the second game is independent of the goal difference in the first game. In order to test the graphic relation we perform an OLS regression with robust standard errors as given by:

$$sumgoals_{H+A,2} = \beta_0 + \beta_1 goaldif_{H-A,1} + \beta_2 goaldif_{H-A,1}{}^2 + u_2 \qquad (1)$$

We define $sumgoals_{H+A,2}$ as the sum of home (H) and away (A) goals in the second game. u_2 is the error term. Let $goaldif_{H-A,1}$ ($goaldif_{H-A,1}{}^2$) be the difference (squared) between home and away goals in the first game. If risk-taking plays a role we should observe a U-shaped relation between past performance (goal difference) and the sum of goals. Thus, β_1 should be insignificant and β_2 should be positive and significant. Estimating the regression using OLS we find that the constant is positive, 2.69, and significant at the 1% level. β_1 and β_2 are highly insignificant with a p-value of .77 and .97, respectively. These results confirm the graphic evidence that there is no relation between past performance and current risk-taking.

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game as S increases its risk-taking. In contrast, if the goal difference between teams is rather low, we would expect risk-taking and thus the total number of goals in the second game to be low.

4.6 On the Absence of Leading-effects

We can test for leading-effects using a natural experiment in soccer knock-outs if risk-taking is constant across games and teams. As we show in the previous section, there is no evidence that risk-taking plays a significant role. Thus, we can exploit the properties of the natural experiment. The two-game structure in soccer competitions should be a neutral structure absent of order effects. The overall winning probability for S, the team playing second home, is 51.8%. While the point estimate is slightly above 50%, it is far from being significantly so. For preliminary evidence we perform a two-sided binomial test with the hypothesis that the order of play does not significantly influence the probability of winning. Performing the test with a sample size of 1,146 games and H_0: 50%, we get a p-value of .31 so that we cannot reject the hypothesis of the mean to be 50%.

In **Table 4.3**, we perform an in-depth analysis by adding various control variables. We use a logit model[155] where the dependent variable, win_i, is binary and equals 1 if the team i wins the knock-out. As our observations are not independently drawn from the same population, but one team winning the knock-out implies the other losing it, we cluster the standard errors by the knock-out ID.

In the first specification of **Table 4.3**, we regress win_i on SH_i, a dummy indicating whether team i plays the second game at home. As specification 1 shows, SH is indeed insignificant. In the second specification, we add an interaction term between SH and time dummy variables from 1955 to 2009 to test whether time fixed effects are present. In specification 3, we add an interaction between Champions League games and SH. As better teams play for higher stakes in this competition as opposed to the Europa League, it might be that these teams are more capable of exploiting their advantage deriving from order effects. As the regression results show, this is not the case. Statistically, there is no significant difference between SH in Champions League or Europa League. In specification 4, we include both time fixed effects and the Champions League dummy but results are unchanged. In specification 5, we include round dummies. As it might be that order effects are stronger in the final knock-out rounds of the tournaments when stakes are highest, we include four interactions between round dummies and SH. As specification 5 shows, none of them is individually significant at the usual confidence levels. In the last specification we add all control variables but none has a significant effect on the probability of winning given that the team played the second game home and SH is insignificant.

In this two-game tournament context, we find no evidence for leading-effects. On average, teams playing home second have a slight advantage, but this is not significantly different from no-advantage. The evidence provided in **Table 4.3** and in the previous section on risk-taking shows that allocating symmetric advantages

155 Using a probit model instead does not qualitatively change the results.

at different stages of tournaments is both fair from the players' perspectives and guarantees selection efficiency from the tournament organizer's perspective.

4.7 Discussion

Our empirical strategy rests on two testable predictions from the theoretical literature on tournaments (leading-effect and risk-taking). Even though our strategy addresses each prediction separately there might be some concerns on the strategies and beliefs teams have during the game which we do not observe. For example, we do not observe the beliefs of players or coaches. If they believe to be disadvantaged by their order of home play, this might cause an (unobservable) decrease in effort. While such beliefs are unobservable, we address the two main causes of order effects and provide evidence that neither of them plays a significant role. In soccer there might be additional unobserved strategies, as for example players might get substituted for the risk of injuries or the coach might change formation. While it is unquestionable that such decisions may play a role, the advantage of having a natural experiment is that the teams' characteristics are uncorrelated with the treatment effect. Thus, additional strategic behavior should be uncorrelated with the treatment effect, i.e. the order of the advantage, as we control for the two main potential causes of order effects. Furthermore, in other sports like National Basketball Association (NBA) basketball, teams might be more likely to intentionally lose games at the end of the regular season because of the incentives they face (Taylor and Trogdon, 2002). This is not a concern in our setting, where the seedings for the tournament depend on the end-of year rankings in the national leagues and (financial) incentives to compete in every game of the tournaments are very high.[156] An additional concern might be that our setting is not perfectly symmetric. Even though 93% of the knock-outs finish after the second game, a minority continues to supplementary times (4%) and eventually finishes after the penalty kicks (3%), as is shown in **Table 4.2**. In knock-outs where the competition is tight, S might adopt a strategy which increases the probability of reaching the extra time. Then S will be playing the extra time at home, where it has an advantage.[157] While these issues might be a concern, players are highly paid professionals who exactly know the setting of the game and have high incentives to pass the knock-out round. If they could get advantaged by their order of home play, they would exploit this advantage. At optimum, coaches and players maximize their winning probability given the random order of home play.

4.8 Conclusion

Tournaments are widely used for two main purposes: as a selection mechanism and to provide incentives to work hard. While many tournaments are dynamic, little is known about the effect of revealing information

[156] See footnote 15.

[157] We performed the leading-effect analysis on different sub-samples depending on when the knock-out ended (regular time, extra time and penalty kicks), but results were unchanged.

during the tournament on participants' effort and risk choices. We focus on the selection efficiency of dynamic tournaments. If the structure of multi-game tournaments systematically distorts winning probabilities because of leading-effects or risk-taking, tournaments may not be an efficient selection mechanism when multiple repetition under different conditions is needed. Some players could benefit from an advantaged position and the winner may not be the best participant, but the luckiest one. In this paper, we analyze the presence of order effects in dynamic tournaments with asymmetric conditions. We define order effects as whether the random order of a temporary advantage for a team in multi-game tournaments has an impact on its probability of winning. From a theoretical perspective two alternative and opposing hypotheses have been proposed. First, there might be a leading-effect. The winner of the first game faces a higher `effective prize' from winning the second game than the first-game loser. This encourages the leading team and/or discourages the team lagging behind to exert effort. Second, agents lagging behind might increase risk-taking as they have nothing to lose.

Using a natural experiment with 1,146 observations in professional sports competitions where highly paid professionals play in tournaments with strong (financial) incentives, we develop an empirical strategy to distinguish between the two order effects highlighted by theoretical work on tournaments: leading-effect and risk-taking. In two-game soccer knock-out competitions, teams are randomly drawn to play either the first or second game at home, and thus have an advantage either early in the knock-out round or later. Before analyzing the conditional winning probability on the randomly assigned order of the advantage we provide evidence that risk-taking is constant across games and does not increase in response to negative past performance. Our empirical evidence suggests that players anticipate the opponent's risk-taking across games and adapt their risk-taking behaviour accordingly. The team playing the first game at home, F, anticipates that the opponent might lag behind and increase risk-taking in the second game. Consequently, F increases risk-taking in the first game as well, and risk choices are constant across games.

As we do not find any evidence of risk-taking, we perform a series of regressions relating the winning probability to the order of the advantage and other covariates. We find that teams have statistically the same winning probability irrespective of whether they have an advantage in the first or second game. Using a unique and large dataset with 1,146 games where advantages are randomly assigned, we show that teams playing first (second) home win 48.2% (51.8%) of the knock-outs, not statistically different from 50% with a p-value of .31. In the regression analysis we confirm this finding using a logistic regression model with clustered standard errors at knock-out-ID. In addition, we add other control variables as time fixed effects, type of tournament and knock-out round dummy variables. The results are unchanged. In all specifications, the order of the advantage is never significant.

Our findings using team sport data are consistent with Ferrall and Smith (1999). In contrast, papers using data on individuals as Malueg and Yates (2010) and Genakos and Pagliero (2012) find evidence of strategic effects. A possible explanation for this difference between teams and individuals is provided by Ferrall and Smith (1999) who argue that incentives within teams may attenuate incentives between teams. Our results suggest that if the setting is known by participants and individuals are competing in teams the timing of symmetric advantages seems irrelevant. This guarantees selection efficiency and fairness for participants.

4.9 Appendix

4.9.1 Setting

There are four ways of winning the knock-out, which apply in the following order. First, the goal difference rule states that the team that scores more goals on aggregate in the two games qualifies for the next knock-out round. Second, the away goals rule prescribes that if the two teams score the same number of goals over the two games, the team that scores more away goals qualifies for the next knock-out round. Third, Article 7 of the Regulations of the UEFA Champions League 2008/09 states that "if both teams score the same number of goals at home and away, two 15-minute periods of extra time are played at the end of the second leg". Fourth, "if no goals are scored during extra time, kicks from the penalty mark (...) determine which club qualifies for the next stage."

The knock-out competition is structured in six steps.

1) Teams F and S are randomly allocated to play either the first or the second game at home.
2) The first game is played. Assume without loss of generality that F is the home team in the first game.
3) The second game is played (S is the home team now).
4) If the sum of the goals of S is strictly larger (smaller) than those of F, the game ends and S (F) wins the knock-out. If the sums are equal, the team that scored more away goals wins. If also the away goals are equal the game continues at S's venue.
5) Supplementary time is played. The team that scores more goals in the supplementary time wins.
6) If both teams score the same number of goals, penalty kicks are used to determine the winner.

The continuation of the game at S's venue could be the explanation for the higher point estimate of teams playing the second game home, 51.8%. As we show in **Table 4.3**, there is no significant advantage from playing second, but the fact that the game is played on the field of the team playing home second is certainly a small advantage. The percentage of games ending after the second game for the entire dataset with 1,146 observations is 93%, while 4% of the games end after supplementary times and 3% after penalty kicks. The probability of winning for S conditional on reaching supplementary time (penalty kicks) is 57% (55%), not significantly different from 50%.

4.9.2 Tables and Figures

Table 4.1: Summary of the data

Competition	Phase	Time Period			
		1955-1999	2000-2004	2005-2009	1955-2009
Champions League	Final KO Round	502	80	28	610
Europa League	Overall	0	0	536	536
	Final KO Round	0	0	456	456
	KO Round of last 16	0	0	80	80

Note: One observation is one game. Overall number of observations: 1,146. Final Knock-Out (KO) Round comprehends quarter- and semi-finals. We only consider games where the order of the home advantage is randomly assigned by the UEFA.

Table 4.2: UEFA regulation, passing the knock-out round and home advantage

UEFA Regulation	Observations	Frequency	Home Result	Observations	Frequency
Goal difference	929	0.81	Home win	611	0.53
Away goals rule	139	0.12	Home draw	300	0.26
Supplementary time	42	0.04	Home defeat	235	0.21
Penalty kicks	36	0.03			

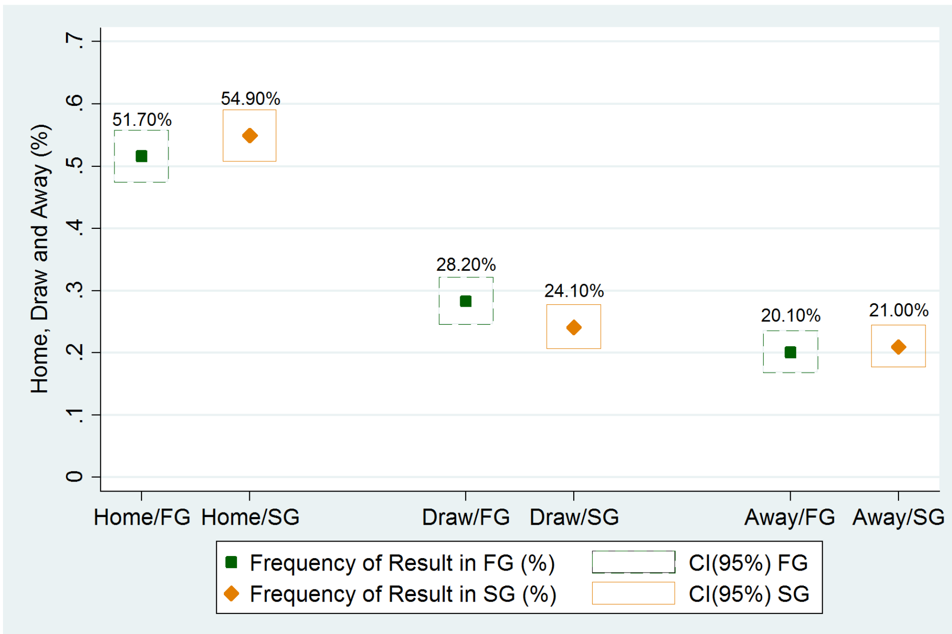
Note: There are several ways of passing the knock-out round as discussed in the appendix. This table summarizes the relative importance of each of them. The 'goal difference' regulation is the most important one. For a detailed description of the regulation see UEFA.com. 'Home Result' is defined as the result from the perspective of the home team, irrespective of whether it is the first or second game of the knock-out. One observation is one game. Overall number of observations: 1,146. We only consider games where the order of the home advantage is randomly assigned by the UEFA.

Table 4.3: The absence of order effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Binary Dependent Variable: Win the knock-out=1; Lose=0					
Second Home (SH)	0.175	0.0873	0.0413	0.177	0.108	-0.0288
	(0.167)	(0.296)	(0.300)	(0.190)	(0.203)	(0.315)
Round of last 16*SH					0.385	0.392
					(0.353)	(0.348)
Quarter Final*SH					0.123	0.172
					(0.195)	(0.424)
Semi Final*SH					-0.0382	-0.00463
					(0.237)	(0.448)
Champions League*SH			0.466	-0.00431		0.419
			(0.413)	(0.168)		(0.556)
Year Dummy		Yes	Yes			Yes
Constant	-0.0873	-0.0873	-0.0873	-0.0873	-0.0873	-0.0873
	(0.0837)	(0.0837)	(0.0837)	(0.0837)	(0.0837)	(0.0837)
Observations	1,146	1,134	1,134	1,146	1,146	1,134

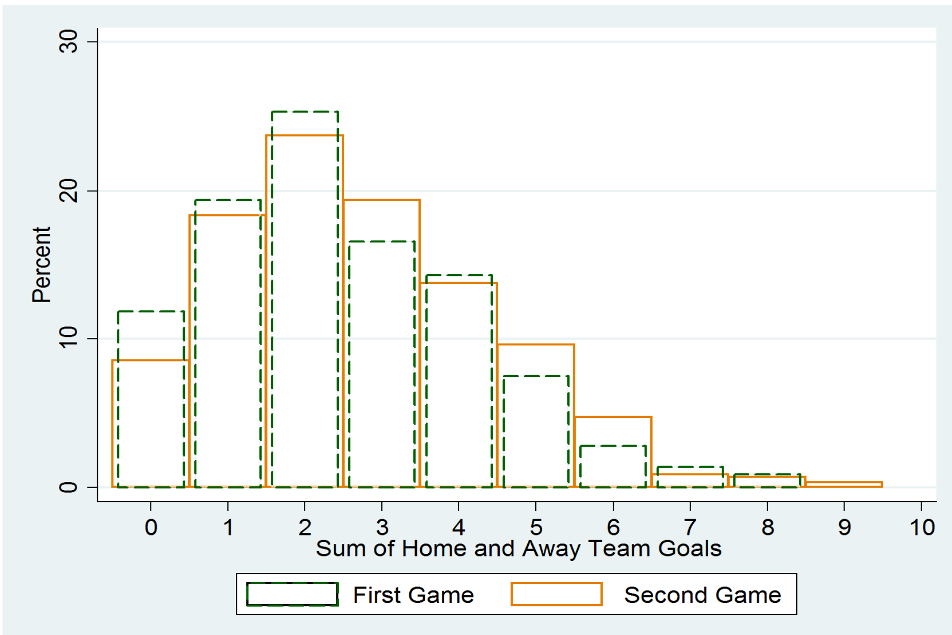
Note: Robust standard errors clustered by knock-out-ID in parentheses. The results do not change qualitatively if the probit regression model is used instead of the logit model. Year Dummy is a binary variable which indicates the year in which the knock-out is played. One observation is one game. We only consider games where the order of the home advantage is randomly assigned by the UEFA. 6 observations from Saison 1968 and 6 observations from Saison 2001 are dropped due to collinearity in specifications 2, 3 and 6.

Figure 4.1: Results in the First Game (FG) and Second Game (SG)



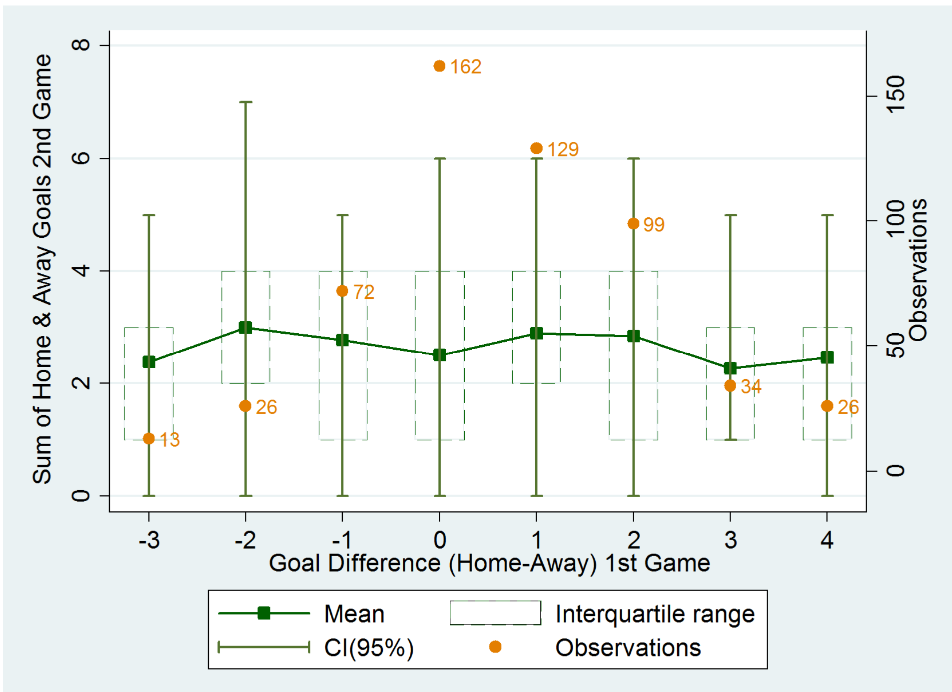
Note: Figure 4.1 shows the frequency of outcomes (home (win), draw, away (win)) in the first and second game of the knock-out. The square dot represents the percentage of each result, while the square around the dot represents the 95% confidence interval (CI) around the mean. The square (diamond) represents the results in the first (second) game. FG (SG) refers to the first (second) game of the knock-out. The main result shown in Figure 4.1 is that the distribution of outcomes is statistically the same across games, a result confirmed by a non-parametric Kolmogorov-Smirnov where we cannot reject the null of equal distributions across games at a *p*-value of .89.

Figure 4.2: Sum of home and away goals by game



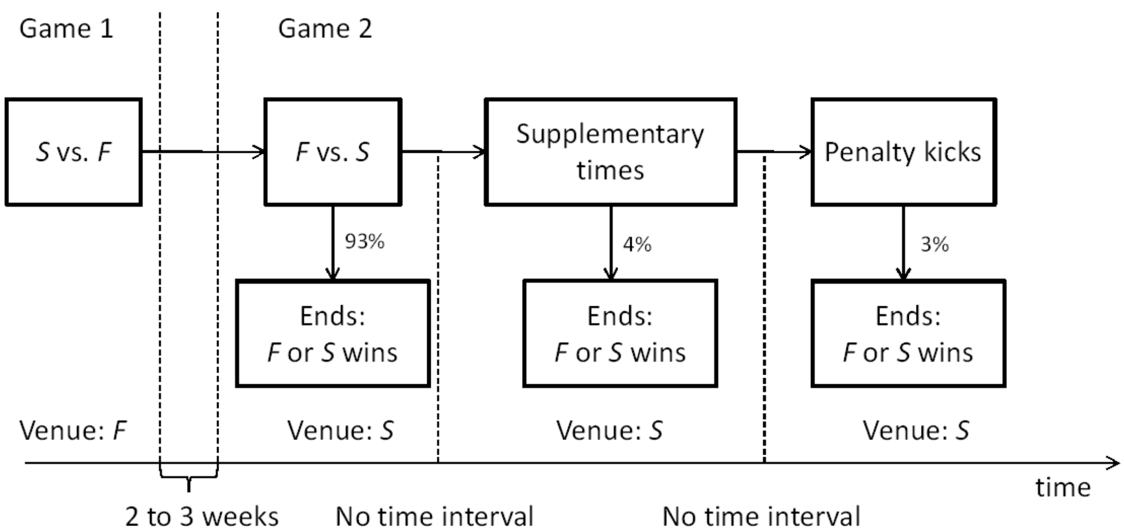
Note: Figure 4.2 shows the distribution of the sum of home and away team goals in the first and second game. The dotted (normal) bar represents the first (second) game. While the average number of goals in the second game is slightly higher than in the first game (2.7 vs. 2.48) a non-parametric Kolmogorov-Smirnov test cannot reject that the distributions are equal across games with a *p*-value of .24. This shows that risk-taking measured by the distribution of the sum of goals is constant across games.

Figure 4.3 Past performance and risk-taking



Note: Figure 4.3 shows the relation between the sum of home and away goals in the second game (left *y*-axis) and the goal difference (home-away) in the first game (*x*-axis). In addition, the observations (knock-outs; on the right *y*-axis) to compute the mean, the interquartile range and the 95% confidence interval (CI) are reported. One observation is one knock-out round consisting of the first and second game. We restrict the sample to goal-differences with more than 10 observations. A total of 12 knock-outs with goal-differences greater (lower) than 4 (-3) are dropped. The main result of Figure 4.3 is that the sum of home and away goals in the second game is independent from the goal difference in the first game. If teams lagging behind increase risk-taking we would expect the sum of goals to increase with the absolute goal difference in the first game. The independence between these variables provides further evidence that teams do not change risk-taking as a response to past performance.

Figure 4.4: Description of knock-out structure



Note: *F* (*S*) denotes the team playing the first (second) game at home. The arrows indicate the progress of the knock-out. 93% of the knock-outs end after the second game, 4% end after supplementary times and 3% end after penalty kicks.

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Eidesstattliche Versicherung

Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht.

Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht.

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