

CARTEL OVERCHARGES: SURVEY AND META-ANALYSIS

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Abstract

The article presents a unique regression analysis of social-science estimates of the size of cartel overcharges. More than 800 overcharge rates were collected from a variety of published sources that have appeared in the literature during the last 125 years. A meta-analysis of overcharges from 395 cartel episodes finds that duration, legal environment, and organizational characteristics of cartels explain the variation on overcharge rates to a greater extent than the type of publication or the method of overcharge analysis. In particular, overcharges are significantly higher for durable international cartels and are declining as antitrust enforcement regimes have stiffened. Historical case studies and government reports calculate lower overcharges, whereas antitrust authorities arrive at relatively high rates.

Key words: antitrust, cartel, collusion, meta-analysis, overcharges.

INTRODUCTION

Since at least 1888, hundreds of economists, historians, commissioners, and jurists have labored mightily to assess the effectiveness of cartels. Various criteria have been applied to evaluate cartel performance, including longevity, stability, social welfare, and efficiency, but by far the greatest attention has been lavished on market price effects.¹ The increase in transaction prices relative to a more competitive benchmark price caused by a sellers' cartel is commonly called an overcharge by economists.² A price-fixing overcharge is a transfer of income or wealth from buyers to the members of the cartel that occurs as a result of a collusive agreement. The overcharge rate is calculated by comparing actual cartel-enhanced prices to some competitive benchmark (Connor 2004a). When a cartel achieves high levels of effectiveness (i.e., longevity, stability, and high overcharges), it generates large customer welfare losses.³

The size of cartel overcharges is an issue at the empirical heart of a number of legal and economic controversies. First, knowing the size and distribution of cartel overcharges is necessary to justify the underpinnings of U.S. and foreign sanctions for illegal cartel conduct. Second, there is evidence in the economic literature widely varying opinions among experts on the critical legal-economic issue of optimal cartel deterrence.

¹ Longevity, also called duration, measures the lifespan of a cartel or, if it has more than one, the length of time of one episode. Some researchers use the term stability synonymously with duration, but more commonly it refers to the absence of price wars or other reversions to competitive conduct during a cartel's time span. Stability is perhaps equivalent to low variation in a cartel's "discipline," where discipline may be measured by how close a cartel's selling prices are to its desired target price or the theoretical monopoly price. In the context of commodity agreements or marketing orders, stability will show up as lower variation in prices compared to the absence of such an agreement. Efficiency can refer to static allocative efficiency (low net social welfare loss) or, rarely, to technical efficiency or dynamic efficiency (rates of technological change). Allocative inefficiency is smaller than but closely correlated with the overcharge.

² Legal writers refer to the monetary value of the overcharge as damages. The price effect of a buyers' cartel is an undercharge.

³ Customers are direct buyers, usually industrial buyers, but overcharge pass-on will transfer the losses in whole or in part to final consumers as indirect buyers. If cartels improve technical or dynamic efficiency, this may offset the buyers' losses.

A century has passed since the last dedicated survey of the cartel literature (Bullock 1901).⁴ Yet, the vast majority of all quantitative analyses of cartel price effects have been published since 1901. To our knowledge no one has since published a work aimed principally at surveying and analyzing cartel overcharges. We intend to eliminate this gap.

The first objective of our paper is to describe the large-scale sample of cartel overcharges assembled for this paper. The estimates are from studies, some more than a century old, of private, hard-core cartels from all regions and eras. However, our sample may not be free from variation due to different publication types or methodologies employed. Therefore, the second objective of our paper is to evaluate the impact of these factors on variability in the overcharge estimates. To accomplish this objective we apply a regression meta-analysis to a large sample of quantitative estimates of overcharges. We believe that our meta-analysis is the first to be applied in the field of industrial economics. These results ought to be of interest to empirical researchers of cartels, to forensic economists testifying in horizontal conspiracy cases, and to policy makers interested in cartel deterrence.

The paper is organized as follows. A literature review discussing the economic and legal aspects of cartels' overcharges and six published traditional surveys of overcharges is followed by a summary of our unique survey of price-fixing overcharge estimates. Then, a description of our methodology and results of a meta-analysis are presented.

OVERCHARGES, CARTEL FINES, AND CARTEL DETERRENCE

⁴ Bullock's survey covers only a few years at the end of the 1890s; in common with nearly all economists of the time, he believed that the price effects of cartels could not be adequately estimated.

Overcharges and Cartel Fines

Beliefs about the average height of overcharges are at the center of modern policies on appropriate antitrust fines for hard-core cartels.

In the United States, the Sentencing Reform Act of 1984 created the U.S. Sentencing Commission, which was charged with devising guidelines for criminal sentencing for the federal judiciary (USSG Advisory Group 2003). The first set of guidelines was published in 1987, and after nearly three years of study and public comment was made law in 1989. The guidelines included sanctions for organizations guilty of horizontal price fixing and bid rigging (Cohen and Scheffman 1989:332). Although the Sherman Act of 1890 is a criminal statute that encompasses other types of restrictive business practices, by long tradition only horizontal price fixing and market-sharing agreements have triggered criminal indictments by the Department of Justice (DOJ).⁵

The issue of how high cartels typically raise prices was crucial when the U.S. Sentencing Commission (USSC) established the fine levels for cartels.⁶ The USSC's cartel fine levels followed from its famous conclusion: "It is estimated that the average gain from price-fixing is

⁵ Criminal filings are made in cases of *per se*, covert, intentional conspiracies by participants who are aware of the probable anticompetitive consequences (Hovenkamp 1999:585-586). While there are a few exceptions, potentially illegal anticompetitive conduct such as information-sharing, signaling, refusals to deal, resale minimum-price maintenance, tied sales, exclusive dealing, patent or trademark pooling, mergers, monopolization, and attempts to monopolize are treated as civil matters. More than 90% of all naked cartel cases are brought as criminal actions, but a small number of such cases are, at the discretion of the DOJ, filed as civil matters.

⁶ The USSC Guidelines start with a *base fine* double the 10% presumed overcharge and use it in conjunction with the assigned base Offense Level for antitrust offenses. They adjust this offense level by a number of factors, such as whether bid rigging and other aggravating factors were involved, and by mitigating factors as well. This adjustment results a pair of "*culpability multipliers*" that are between 0.75 and 4.0. The product of the base fine (20% of the affected commerce) and the culpability multipliers results in the fine range that is to be imposed on a cartel member. Thus, the fine range recommended for convicted cartelists is at its lowest 15% and at its highest 80% of affected sales. These fines usually are adjusted downwards for cooperation or as a part of the Division's leniency program. The USSC's Commentary also notes that "In cases in which the actual monopoly overcharge appears to be either substantially more or substantially less than 10%" it might not employ the 20% base fine. But in practice the DOJ almost always uses the figure of 20% of affected commerce as their starting point in their criminal fine calculations.

10 percent of the selling price.” The Commission added: “The purpose for specifying a percent of the volume of commerce is to avoid the time and expense that would be required for the court to determine actual gain or loss.”⁷ As the Sixth Circuit noted, the Sentencing Commission “opted for greater administrative convenience” instead of undertaking a specific inquiry into the actual loss in each case.”⁸ The USSC appears to have adopted the 10% presumption because its use was advocated by the then head of the DOJ’s Antitrust Division.⁹ A prominent analysis of the issue by Cohen & Scheffman (1989), published shortly after the antitrust sentencing Guidelines were promulgated, states that the economic evaluation of only three price-fixing conspiracies was particularly important in shaping the DOJ’s views.

The USSC’s 10% presumption was attacked as unreliable and overstated almost as soon as it was issued. For example, Cohen and Scheffman (1989) concluded that “...there is little credible statistical evidence that would justify the Commission’s assumptions which underlie the Antitrust Guidelines (p. 333).” “At least in price fixing cases involving a substantial volume of commerce, ten percent is almost certainly too high (p. 343).” Moreover, the specific data that the Commission used was criticized: “later research has cast considerable doubt on ... these estimates, concluding that the markups, if they existed, were quite small (p. 345).”

From 1990 to 1999, a series of record corporate fines were imposed for criminal price fixing by U.S. courts; a similar upswing may be noted for fines imposed by the European Commission from 1995 to 2001 (Connor 2005). Civil treble-damages cases in the United States

⁷ See U.S. Sentencing Commission Guidelines For the United States Courts, 18 U.S.C. Section 2R1.1, Bid-Rigging, Price Fixing or Market-Allocation Agreements Among Competitors, Application Note 3.

⁸ See *United States v. Hayter Oil Co.*, 51 F.3d 1265, 1277 (1995). The court noted: “The offense levels are not based directly on the damage caused or profit made by the defendant because damages are difficult and time consuming to establish. The volume of commerce is an acceptable and more readily measurable substitute...”

⁹ In a statement to the Commission, Assistant Attorney General Ginsburg stated that “the optimal fine for any given act of price-fixing is equal to the damage caused by the violation divided by the probability of conviction . . . such a fine would result in the socially optimal level of price-fixing, which in this case is zero”(USSG 1986:14). He stated his judgment that “price fixing typically results in price increases that has harmed the consumers in a range of 10 percent of the price...” and that these violations had no more than 10% chance of detection (*ibid.* p.15).

have seen a parallel response in the size of settlements. Attorneys who have defended convicted cartel members in a number of highly publicized international antitrust conspiracies have claimed that the Guidelines have resulted in excessive penalties. For example, just as the DOJ's campaign against international cartels was gathering steam, Adler and Laing (1997) assert that "the fines being imposed against corporate members of international cartels are staggering (p.1)", placing the blame on the "uniquely punitive" requirements of the U.S. Sentencing Guidelines. After viewing an intensification of this trend for another two years, Adler and Laing (1999) were even more alarmed.

"What is...troubling is that the company fines...have risen astronomically – to levels far higher than the fines for other serious economic crimes and in amounts that can be unrelated to the economic harm caused by the violations (p.1)."

More recently, Denger (2003) too decries the prevalence of excessive price-fixing fines and private settlements. He places the blame for excessive fines on the Corporate Guidelines base fine calculation (p. 3). This approach, he notes, unlike all other white-collar federal crimes, means that the actual degree of direct harm caused does not have to be proven by prosecutors.¹⁰ Denger blames this state of affairs on a gap in the economic-legal literature: "...we have little information on what level of criminal or civil exposure is needed to deter most cartels (p.4)."

The lack of empirical evidence on the actual harm caused by price fixing is also of concern to prosecutors. DOJ official Graubert (2003) notes that the controversy over whether antitrust payments are excessive¹¹ is largely attributable to the "...difficulty of gathering useful data." A critic of the effectiveness of antitrust enforcement, Klawiter (2001) expresses

¹⁰ Denger appeals primarily to an increase in settlement rates in treble-damage direct-purchaser suits to establish the unfairness of the high fines imposed on corporate price fixers, an increase that, he believes, cannot be explained by increases in overcharge rates. He cites about 8 domestic U.S. law cases that settled for 2 to 4 % of sales in the 1970s and one international case in 2001 that settled for 18 to 20% (pp. 3-4). It is argued below that settlements are inappropriate evidence in this context.

¹¹ On p. 7 Graubert defines payouts greater than reasonable damage estimates as excessive.

skepticism as to whether the severe monetary penalties imposed on cartelists in the late 1990s will in fact deter illegal price fixing.

In response to numerous issues on the effectiveness of the antitrust enforcement, the Antitrust Modernization Commission was created in 2003. The Commission is to recommend changes in U.S. federal antitrust laws. One issue to be addressed is whether the existing U.S. guidelines for criminal punishment of price fixing ought to be amended (AMC 2004).

U.S. antitrust enforcement has been a model for many other countries that have more recently adopted such laws (Wells 2000). After four years of confidential political discussions within the EEC's Commission, Regulation 17 was passed in 1962; it lays out the powers of the Competition Directorate General (DG-COMP) to fine companies for competition-law infringements (Goyder 1998: 45). That rule sets a maximum corporate fine of 10% of the company's total sales in the year prior to the Commission's decision and specifies that the specific fine will depend on the duration and seriousness of the offense.¹² Methods of calculating EU cartel fines are further explained in a 1998 Notice (Connor 2005:14-15). The EC considers the "gravity" of the offense. EU cartel fines are loosely related to overcharges because cartels with large damages that are geographically widespread add to the gravity. Also, relatively large companies are fined more than smaller participants: in several global cartels, companies in the upper half of the cartel's size distribution had their fines doubled. After applying a number of other factors, the Commission ensures that fine amount does not exceed 10% of global sales in the year prior to the date of the decision. Rarely does the EC need to worry about breaching the 10% cap (Connor 2003).

Canada is another jurisdiction with relatively tough sentencing for cartels. The Canadian Competition Bureau (CCB) uses a fairly simple standard for setting fines. Although not spelled

¹² Rule 17 was amended in 2004, but these provisions were unaffected.

out in any administrative guidelines, decisions of Canadian courts have, in the absence of aggravating and mitigating circumstances, imposed fines close to 20% of Canadian affected sales (Low 2004, Connor 2003).¹³ A former Canadian cartel prosecutor comments that “there has not been any economic or judicial analysis of the assumptions behind this proxy for harm that this represents...” (Low 2004:19). The Canadian 20% rule seems to mimic the base fine of the USSGs. If Canada intends to punish cartels, then the presumed overcharge may also be 10%; if only compensation is the aim, then a 20% overcharge would be appropriate.

Overcharges and Cartel Deterrence

Concerns about the inadequacy or excessiveness of antitrust sanctions are part of the larger issue of the effectiveness of antitrust interventions. To make any headway in assessing empirically the adequacy of anticartel enforcement, it is necessary to have reliable information about the degree of harm generated by private cartels. Cartel injuries to purchasers are positively related to three economic factors: the size of the cartel’s market, the duration of the conspiracy, and the percentage overcharge. Antitrust sanctions should be calibrated to a cartel’s affected sales, overcharge rate, and the probability of cartel formation or the duration of cartels.

The U.S. Sentencing Guidelines’ are consistent with the standard optimal deterrence standard promulgated by William Landes (1983). Landes showed that to achieve optimal deterrence the damages from an antitrust violation should be equal to the violation’s “net harm to others”, divided by the probability of detection¹⁴ and proof (Landes 1983:666-68). Cohen and

¹³ Under Section 45 of Canada’s Competition Act, fines are limited to C\$10 million, but foreign price-fixing conspiracies can be prosecuted under Section 46, which has no fine limit (Low 2004:17).

¹⁴ In 1986 the Assistant Attorney General for Antitrust, Douglas Ginsburg, estimated that the enforcers catch less than 10% of all cartels. See USSG (1986: 15). If he is correct, optimal fines for cartels should be tenfold damages! The percentage of cartels that are caught and proven is probably much higher today. See Spratling (2001). There is, however, neither evidence nor speculation that it exceeds 33%, so there is no reason to believe that the treble damage remedy should be lowered. See also the discussion in Landes (1983: 115 fn. 1).

Scheffman (1989) argue that U.S. sentencing guidelines, when coupled with civil and marketplace sanctions, cause “a serious overdeterrence problem” (p. 334).¹⁵ During recent years this criticism has been repeated with perhaps even more intensity. In a provocative essay that quickly drew rebuttals¹⁶, Crandall and Winston (2003) argue that extant empirical evidence demonstrates that U.S. antitrust policy has been ineffective in either raising consumer welfare or in deterring anticompetitive conduct: “We find little empirical evidence that past [antitrust] interventions have provided much direct benefit to consumers or significantly deterred anticompetitive behavior” (p. 4). To support their view that the prosecution of overt price fixing is misdirected, they cite five empirical studies of overt collusion that find no upward effects on prices of conspiracies convicted in U.S. courts¹⁷. While Crandall and Winston later admit that there are some “examples” of successful collusion, they cite no studies that support a positive effect of cartels on prices¹⁸. As for deterrence, Crandall and Winston rather grudgingly admit that the large DOJ fines meted out to cartels in recent years possibly deterred the most harmful cartels.¹⁹ In his comment on Crandall and Winston, Kwoka (2003) faults them for their

¹⁵ Those critical of aggressive antitrust policy have often embraced the comforting notion that cartels are fragile coalitions. When the OPEC cartel began to have an impact on petroleum prices in the early 1970s, several leading economists predicted its imminent demise. Morris Adelman (1972) wrote “Every cartel has in time been destroyed by one and then some members chiseling and cheating...” (p.71). In a now infamous 1974 news-magazine article, Milton Freedman predicted OPEC’s imminent collapse. OPEC may be less powerful than in the 1970s, but its production decisions continued to roil the petroleum market through at least 2004.

¹⁶ See Baker (2003), Werden (2003), and Kwoka (2003). According to Kwoka (2003: note 2), Crandall and Winston’s earlier drafts “... endorsed consideration of outright appeal of the antitrust laws”.

¹⁷ Space constraints do not appear to be responsible for such a skimpy treatment of this topic, for they list 59 references. The choice of two of the articles is unfortunate, because both are methodologically deeply flawed. Newman (1988) is discussed later in this paper; Sproul (1993) is criticized by Werden (2003). Both articles appear in journals managed by University of Chicago economists. Two other studies focus on an odd alleged episode of price fixing, the so-called Overlap group of 23 elite U.S. universities that met regularly to allocate needs-based graduate scholarships; this practice was permitted to continue under a consent decree that limited the degree of detail shared.

¹⁸ They say that the lysine, citric acid, and vitamins cases are “well known,” but provide no citation for this assertion. There appears to be only one publication that covers the price effects of all three of these three cases with a degree of depth, viz., Connor (2001).

¹⁹ Their reasoning is obscure. Perhaps they are referring to international cartels, cartels with absolutely large overcharges, or conspiracies with high percentage overcharges. In any case, why they expect the probability of discovery or relative size of expected sanctions to be greater in such cases is not clear.

“startlingly selective” body of evidence. He suggests that they should have included “... studies from any source with appropriate evaluation of their credibility” (p. 4).

The majority of the overcharges generated by cartels in the past 15 years have been international, even global in membership and geographic spread (Connor 2001, 2003). To assess deterrence in the context of international schemes, non-U.S. monetary sanctions must be considered. To be effective, cartel sanctions must be somewhat punitive. Harding and Joshua (2003) state that EU fines are supposed to incorporate both compensatory and punitive components, the latter to serve deterrence (p. 240). It is clear that for a single-product firm that participates in a cartel with a 10% overcharge for one year, there can be no punitive component solely with EU fines. For more effective cartels, an EU fine cannot even be compensatory. Moreover, if the probability of detection and conviction is less than 20%, then any specialized member of a one-year cartel with an expected 2% overcharge or bigger will not be deterred.²⁰ EU and Canadian fines together are usually less than those imposed by U.S. courts for the same violations, and penalties in other parts of the world are practically zero. In general, global monetary sanctions have amounted to less than 10% of estimated global overcharges (Connor 2003). Thus, punitive sanctions are the exception not the rule for illegal international price fixing.

In sum, there does indeed seem to be a broad consensus among legal and economic writers that the question of the optimality of price-fixing penalties turns mightily on the actual degree of harm caused by cartel conduct, and that not enough is known about this issue.

Moreover, even if the creators of the USSC Guidelines were correct that in the 1980s cartels

²⁰ However, most companies that engage in cartel behavior are large diversified firms; for them, EU fines can come closer to optimal deterrence levels. If the cartelized product line accounts for 10% of total company sales, then the duration or the overcharge level can be 10 times greater to achieve compensation or deterrence.

generally raised prices by 10%, the harsher cartel sanctions imposed more recently could mean that this presumption is no longer justified.

Traditional Surveys of Cartel Overcharges

Given the importance of the topic for legal-economic discourse, there have been surprisingly few compilations of empirical findings about cartel overcharges.²¹ We have been unable to find any research that has as its principal aim collecting or analyzing information on the price effects of overt collusion.²² However, there are six works that mention a significant number of studies of mark-ups due to overt collusion. None claims to be a comprehensive survey.

Cohen and Scheffman (1989) recognize that the average size of price-fixing overcharges generated by overt collusion is a critical issue in evaluating cartel fines. Their paper cites five to seven estimates for price-fixing cases.²³ A working paper by Werden (2003) cites 14 studies of cartel overcharges. All of his sampled studies were published since 1991, because he wished to study conspiracies that operated after 1974, the first year in which cartels could be prosecuted as felonies; three studies examined international cartels prosecuted by the DOJ in 1996-97.

Posner's (1975, 2001) treatise on antitrust law is an avowedly economic treatment of the subject. To illustrate the social costs of cartelization, Posner assembles data on 12 "cartel price increases"

²¹ Of the leading textbooks in industrial organization, Carlton and Perloff (1990) devote considerable space to cartels – almost 50 pages out of 852 total pages. This work mentions by name 60 cartels, most of them interwar, international cartels. Other textbooks have far fewer numbers of cartels cited.

²² Hay and Kelley (1974) authored a classic review of 65 U.S. price fixing conspiracies, which Fraas and Greer (1977) extended to 606 cases from 1910 to 1972. Both studies contain a wealth of information about the number of conspirators, duration, industry, and specific collusive methods employed. However, neither survey covered the topic of price effects, presumably because of the paucity of such data.

²³ One of them (Block *et al.* 1981) is irrelevant because it quotes the ratio of out-of-court settlements to *annual sales* for several U.S. bread price-fixing cases. As Cohen and Scheffman recognize in a footnote, both the numerator and denominator of this ratio are inappropriate indicators of an overcharge; nevertheless in the text of their article, they persist in citing this ratio.

in "...industries having well-organized (mainly international) private cartels" (Posner 2001:303), which he admits are "crude and probably exaggerated" (*ibid.* p.304).²⁴

Table 1. Summary of Six Economic Surveys of Cartel Overcharges

Reference	Number of Cartels	Average Overcharge	
		Mean	Median
		<i>Percent</i>	
1. Cohen and Scheffman (1989)	5-7	7.7-10.8	7.8-14.0
2. Werden (2003)	13	21	18
3. Posner (2001)	12	49	38
4. Levenstein and Suslow (2002)	22	43	44.5
5. Griffin (1989), private cartels	38	46	44
6. OECD (2003), excluding peaks	12	15.75	12.75
Total, simple average	102-104	30.7	28.1
Total, weighted average	102-104	36.7	34.6

Levenstein and Suslow (2002) focus on the determinants of success for both the interwar and more modern cartels. Although the authors are modest about their accomplishment,²⁵ this paper contains the fullest accounting of overcharges of any source. This paper provides a total of 21 estimates of price effects for international cartel episodes. The OECD (2003) report on private "hard-core" cartels contains a summary of a 2001-2002 survey of its government-members on the economic harm caused by cartels recently prosecuted by the European Commission and national antitrust authorities.²⁶ While not all of the survey responses can be

²⁴ Given that Posner is an avatar of the Chicago School of economics, it is noteworthy that his estimates are among the highest of the six studies.

²⁵ "I have very little evidence on the excess profits ... [from] cartelization. For fifteen cartels ... I have anecdotal evidence of price increases..." (p. 20).

²⁶ A few non-members that participated in an OECD-sponsored "Global Forum on Competition" also submitted responses to the survey. "Hard-core" is a European term that refers to conspiracies that fix prices and/or quantities.

converted to overcharge percentages, the usable responses represent an unusually authoritative compilation of data on mark-ups by contemporary cartels that have been prosecuted by courts or commissions.²⁷ The six surveys just discussed are summarized in Table 1.

DATA SET DESCRIPTION

Sources and Collection Methods

We have made every attempt to identify and collect all useful information on *private, hard-core* cartel overcharges available from public sources. Hundreds of books, book chapters, journal articles, working papers, and other analyses of cartel price effects were examined.²⁸ Many of the books were written primarily as historical case studies and mention price effects only in passing. The majority of the shorter works were written by economists. Nearly all economic articles are written by North American academics using cartel episodes that affected commerce in the United States or Canada

A private cartel is one that by contemporary U.S. standards could be criminally indicted under the Sherman Act.²⁹ Hard-core or “naked” cartels are those that made explicit agreements to control prices or limit quantities to be produced or sold. Price agreements may cover list prices or transaction prices; the transactions prices may be floor prices, target prices, or, if a common sales agency is employed, actual transactions prices. Prices may refer to sales of goods

Other cartels (soft core?) cooperate on information, technology, marketing, and the like. The distinction seems roughly to correspond to criminal versus civil violations under U.S. law.

²⁷ In a few cases the harm was reported as a monetary value and the size of affected commerce was missing, but I was able to find a reasonable estimate of the affected commerce from an alternative source. For example, the U.S. DOJ provided a monetary estimate of the U.S. harm caused by the international lysine cartel of 1992-1995, and I found the value of affected commerce in a sentencing opinion written by a federal judge in a criminal jury trial that convicted three of the cartel’s managers. I was able to derive 16 overcharge percentages, of which 12 were long-run and 4 were peak.

²⁸ For a more complete description of the literature consulted, see Connor (2004b:8-24).

²⁹ Criminal indictments for only hard-core cartels is a matter of custom, not law. The 5 to 10% of U.S. DOJ horizontal or vertical conspiracy cases handled through civil indictments could be criminally actionable.

or services, procurement of inputs, or bids in auctions or tenders. Quantity restrictions most commonly involve fixed market shares for each participant, but may also include territorial exclusivity, customer allocations, or production-capacity agreements. Cartels that focused exclusively on advertising, patent pooling, setting technical standards, R & D, and the like are excluded.

Identifying which cartels are private and hard-core at times requires judgment. Some cartels operated prior to 1890 when passage of the Sherman Act made participation by U.S. companies illegal, but many cartels headquartered in Europe predate the beginnings of effective European anticartel laws. If these cartels were not formed by means of a legally enforced government monopoly, they are generally considered *private* schemes.³⁰ However, if a government simply required registration or chartering of a cartel but left its management in corporate hands, they are included in the data set. Beginning in 1918 in the United States and in most European countries in the interwar period, domestic producers were permitted to register and operate export cartels with no or minimal supervision; we consider these private cartels. Similarly, if a government-owned national monopoly or commodity association voluntarily joins an international cartel, that too may be a private cartel. Thus, the mere fact that governments tolerated or turned a blind eye to cartels does not disqualify them from inclusion in the data set. However, commodity agreements known to have been initiated, actively sponsored, or overtly protected by national sovereignty are not included.³¹ In these “public” cartels the active involvement of governments is signaled by the signing of a treaty, government ownership of

³⁰ Wallace and Edminster (1930: Appendix A) provide a convenient chronology of most government-sponsored export-control monopolies: the Japanese camphor monopoly of 1899, the Italian citric acid monopoly of 1910, the Greek currant monopoly of 1895, and the New Zealand kauri-gum monopoly of 1927 are examples of clearly public cartels.

³¹ In some cases particularly in the early 1930s, the earlier phases of an international cartel were controlled by national producers’ organizations that negotiated voluntary quota reductions; when cheating threatened the effectiveness of the cartel, colonial or metropolitan governments stepped in to pass mandatory supply-control legislation. The early phase of the cartel we deem private, but not the latter.

stocks, or the appointment of civil servants to cartel-management positions. There are many fine studies of such agreements, but the inclusion of government-sponsored or -enforced cartels would tend to bias upward the overcharges in the sample (Suslow 2001). Where judgment was required procedures were followed that would result in conservative overcharge statistics.

Besides the overcharge estimates themselves, we have information on cartel characteristics. In most cases each observation has information on the beginning and the termination dates of the conspiracy; whether a cartel is international in membership or not; whether primarily a bid-rigging conspiracy or not; whether found guilty by an antitrust authority or not; the type of overcharge estimate (average low, average high, peak low, peak high); method of overcharge estimation; name of the author; year of publication; and the type of publication.

Types of Publications

The data were collected from peer-reviewed academic journals, dissertations, court and commission decisions, OECD reports, books, government publication, working papers, and other sources. In general, we aimed at collecting the largest possible body of quantitative estimates of monopoly overcharges, and avoided applying some sort of quality screening. In the vast majority of cases, the writers themselves provided the overcharge calculations. In a small minority of cases, it was necessary to make inferences from price data shown in the works.³² A substantial number of estimates are drawn from trial decisions, announcements by antitrust authorities, or statements submitted by governments to the OECD or other multilateral

³² The bases for the inferences are briefly outlined in Appendix Table 2 of Connor (2004b). If a credible study of a cartel concludes that it was “ineffective,” we have coded this as a zero price effect and included this observation in the averages. Likewise, conclusions that the impact of collusion was “overwhelmed” by natural market forces are interpreted as a zero overcharge. However, vague conclusions that a cartel episode was “effective” are not tabulated.

organizations.³³ Few overcharge claims appearing in newspapers, magazines, and newsletters are included because such assertions are usually from anonymous sources who may not be disinterested parties in an ongoing law suit or in some public policy debate, roles that may color their assertions.³⁴ In some cases, overcharge estimates may originate from articles in industry trade journals, but if they were cited by economists, historians, or legal scholars with some background in cartel studies, such estimates are reported in the present survey. We did include estimates appearing in a few book-length cartel studies by journalists, public servants, or professional writers of nonfiction.³⁵

Newer publications were located by using various bibliographic search engines, by noting the references cited by authors in the works themselves, and by searching on-line library catalogs. These studies vary substantially in terms of depth and the degree of professional commitment to the study of cartels. Some economists and historians have spent substantial portions of their careers specialized in cartel analysis, but most of the publications with useful overcharge estimates are by social scientists for whom cartels were just a passing interest. Other sources of information include the Web pages of scores of antitrust agencies, court and commission decisions, and reports multilateral organizations.

³³ Cartel fines are frequently announced in press releases by competition-law authorities, but only rarely do such announcements contain overcharge data. Final decisions in price-fixing trials with sufficient information to calculate overcharges are even more uncommon (Connor and Lande 2004). However, the OECD has undertaken a program of annual reports on competition policy and occasional special reports in which governments frequently report authoritative estimates of cartel harm (OECD 1974, 1976, 2001, 2002, 2002-2003). Similarly, beginning in the 1950s the UK Monopolies Commission published scores of detailed investigations of alleged cartels.

³⁴ Some scholars may have relied on what they judged to be credible journalistic reports of overcharges.

³⁵ We have confined journalists' accounts of cartels primarily to book-length treatments of cartels, in the belief that such monographs are in-depth accounts of a cartel collected from many sources, some of them anonymous, over a period of time sufficient for the author to provide a balanced account of conflicting claims. Books by journalists typically do not focus on the quantitative economic aspects of the case at hand, so in practice there are relatively few overcharges drawn from these sources in the present study. We rarely include overcharge estimates embedded in newspaper or magazine articles, though some specialists may judge such assertions to be sufficiently reliable to include in their published studies. For example, Elzinga (1984) cites Demaree (1969), and Carlton and Perloff (1990) cite Smith (1963).

Methods of Overcharge Estimation

Our data set identifies eight categories of estimation approaches used to identify a benchmark price distinguished in our data set. They are: price before the conspiracy began, price during an intra-conspiracy price war, price after the conspiracy ended, total economic cost or normal profit, other yardsticks, econometrics, legal decisions, and historical case studies with no method specified by the author.³⁶

Several are court-approved methods. Older social science studies tended to use a rather informal method of price analysis that now comes under the rubric of the “before-and-after method” (Connor 2004a). That is, armed with knowledge of when overt collusion occurred, the author would compare prices during the affected period with prices before the cartel began or after it ended; in some cases, the basis of comparison would be a price war that erupted during the affected period. The base price was typically assumed to be the long-run competitive equilibrium benchmark price (now rather succinctly, if inelegantly, termed the “but-for price”). Although some were careful to take such factors into account, in many cases the possibility that shifts in demand or supply conditions could have caused the benchmark price during the affected period to depart systematically from the before or after price was ignored; moreover, the idea that price wars could generate unsustainably low prices was not often recognized. Some economists of the time realized the importance of averaging before or after prices for periods long enough to eliminate the influence of transitory disturbances in markets, but others were satisfied to identify one month’s or one day’s price as the but-for price.

A second way of calculating a benchmark price is the yardstick method. In this type of analysis, an economist would collect prices for analogous markets that were believed to be free

³⁶ Historical case studies tend to rely on a deep evaluation of original cartel documents (minutes of meetings, memoirs of conspirators, hearing reports, and the like). A ninth residual category is “method unknown.”

from cartelization. For a localized conspiracy, the competitive yardstick could be prices in a nearby city or an adjacent state with similar demand or cost conditions; the trend in cartel prices could then be compared to the trend in the yardstick during the collusive period. Yardstick price movements can also be constructed for a noncartelized product made in the same region that is made with the same inputs, utilizes a similar technology, and is consumed by the same customers.³⁷ If a cartel colludes against only some of its customers, then the discounts offered to other similarly situated customers could yield a yardstick.

Third, sometimes the costs of production and the margins earned by firms in the relevant lines of business may provide collateral indicators of variations in the degree of competitiveness of a firm or market. Cost-based estimates are relatively uncommon because detailed internal business records are needed. Both the before-and-after and yardstick methods require expert judgments about the market in question, but both remain the leading methods used in courts of law or commission hearings to determine the fact of injury or the amount of damages.

Fourth, since the 1970s the rigor and precision displayed in deriving estimates of cartel overcharges have made several advances. Driven by developments in oligopoly theory and the increasing availability of detailed company and market data, increasingly it is econometric models of the alleged collusive market that are specified and fitted to the available data.³⁸ Game theory has influenced contemporary concepts of collusion, the design of competition policies, and empirical modeling of oligopolies (Werden 2004). In a sense, econometric modeling is an elaboration of the before-and-after method.³⁹ These models usually specify the demand and

³⁷ The danger with this method is that the product yardstick may be a substitute for the cartelized product, and, hence, price-responsive to a cartel overcharge.

³⁸ These data are often proprietary facts revealed during the discovery phase of litigation or submitted to an antitrust authority under compulsory legal processes. In addition to transaction prices of the defendants, production and marketing costs of details of business contracts may be handed over on a confidential basis.

³⁹ Both approaches require prior information on the terminal dates of a conspiracy as parameters for analysis. Depending totally on price patterns to decide on the duration of a conspiracy is tautological.

supply conditions in the relevant market, and then investigate through statistical tests whether and to what extent changes in prices or output fail to be explained by normal, competitive market forces. Because these models can simultaneously incorporate multitudinous factors, economists tend to regard overcharge estimates from such models as more credible than analyses that depend on more informal ways of accounting for such factors.⁴⁰

The remaining categories probably used one or a combination of the four methods just discussed. Legal decisions are typically reached by courts, juries, or commissions after listening to testimony of participants in a cartel or studying expert opinions. Historical case studies rely on weighing original sources: minutes of cartel meetings, participants' memoirs, interviews, government investigations, journalistic reports, and the like.

General Description of the Sample⁴¹

The data are organized according to three levels of analysis: markets, episodes, and overcharge estimates. By "market" is meant the industry or product that was subject to price fixing. *Markets* are precisely self-identified by the participants in the conspiracy, though occasionally there are alternative names for the same market.⁴² The name of the market is eponymous for the cartel. *Episodes*, discussed more fully below, are distinct periods of collusion separated by price wars, temporary lapses in agreements, or changes in cartel membership or methods. Episodes may be

⁴⁰ On the other hand, if a cartel operated during a span in which cost conditions (input prices, expansion of capacity, inventories, and technology) were steady and demand conditions (consumer preferences, disposable income, available substitutes, and the like) did not shift, then fancy econometric models and the more traditional methods will yield the same overcharges. For durable cartels, constancy of all these factors is unlikely.

⁴¹ The subsequent tables in this report are constructed from spreadsheets that incorporate data collected as of October 10, 2004. Connor (2004b: Appendix Tables 1 and 2) contains a few observations added after that date.

⁴² For example, the "nitrogen" cartel is in fact dry salts of nitrogen used as fertilizer, not the gaseous form. The hugely successful "vitamins" cartel is best regarded as a series of overlapping ventures, each of which focused on one of 15 unique products.

adjacent in time or may be separated by significant gaps of time.⁴³ The markets marked by adjacent multiple episodes will typically be regarded by antitrust law as one infraction, but by economists as multiple cartels. The analyses in this paper will use *overcharges* as the units of observation. Each episode will in principle have one true “average” (episode-long) overcharge and one “peak” overcharge.⁴⁴ However, because there are sometimes multiple publications about the same episode and because a single analyst will sometimes apply alternative methods of estimation, this paper often records several estimates or a range for a single episode.

Publications from economists, historians, and related sources yielded useful overcharge or undercharge information on cartels that operated in 237 markets (Table 2). If one group of sellers decided to fix prices of a product in one geographical region and another group colluded on the same product in a separate geographical region, these will be viewed as two markets. Of the 237 markets, 37% were cartelized by international agreements, where “international” describes the membership composition of the cartel and not necessarily the geographic spread of the cartel’s effects. Some international cartels affected directly the commerce of only one nation, though the vast majority was international in both senses. National cartels account for the remaining 63% of the cartelized markets.

⁴³ Episodes are in principle different from phases of cartels that give rise cartels instability. Episodes mark changes in cartel *organization*, whereas stability is measured by changes in the degree of cartel *discipline or cohesiveness*.

⁴⁴ In the rare instances where a cartel kept the market price absolutely constant for the whole episode, the two overcharge concepts will be the same number.

Table 2. Number of Cartel Markets, by Type

Type	Number	Percent
International membership	88	37.1
National or regional	149	62.9
Bid-rigging schemes	73	30.4
Classic cartels	164	69.6
Cartel found guilty or liable	140	59.1
Currently under investigation (presumed “illegal”)	6	2.5
Known to have been operating legally	54	22.8
No record of sanctions (presumed “legal”)	37	15.6
Total	237	100.0

Source: Connor (2004 b: Appendix Table 1) (version of 10/14/04)

Almost one-third of the sample consists of markets affected by bid-rigging cartels.⁴⁵ Although most cartels have some sales to government entities or industrial customers that purchase by tenders, these cartels are explicitly indicated by the authors to have substantially or exclusively engaged in bid rigging. This proportion is certainly an underestimate because the sources did not always provide enough detail on the cartels to be certain of the degree of bid rigging. It is widely believed that bid rigging leads to higher overcharges than otherwise identical conspiracies.⁴⁶ The remaining 70% of the cartelized markets may be called “classic” cartels, those that set market selling prices and/or market quotas for each or its members.⁴⁷

Three-fifths of the cartels were found to be in violation of antitrust laws by at least one legal body.⁴⁸ Sometimes these are called “discovered” cartels. The determination of guilt or liability may take the form of guilty pleas (or *nolo contendere* in U.S. courts up until the 1960s),

⁴⁵ In Europe, bid rigging is generally referred to as collusion involving “tenders.”

⁴⁶ The U.S. Sentencing Guidelines automatically raise fines on bid rigging.

⁴⁷ Only a few cartels were oligopsonies.

⁴⁸ Counted in this category are criminal convictions; adverse decisions of the UK Monopolies Commission, which made recommendations to the government similar to consent decrees; adverse decisions of the European Commission and similar civil authorities; and those cartels that paid court-approved damages. Also a few unfinished probes by antitrust authorities are placed in this category.

of a decision at trial by judge or jury, of a commission decision to impose fines or other sanctions, of the payments of civil penalties, or of negotiated settlements by defendants in a suit. The remaining 39% of the cartelized markets are known or believed to be “legal,” because they operated prior to the enactment of antitrust laws in the jurisdictions in which they functioned, or at the least extra-legal, because they were never discovered by an antitrust authority. Other legal cartels were organized and registered under antitrust exemptions, such as export cartels or ocean shipping conferences.

Consistent with most previous economic studies of cartel effectiveness, each cartel episode can be treated as a unique observation. Most cartels are organized and fall apart only once; not counting brief disciplinary price wars, this describes one episode. However, many cartels are formed, disband, reform, and disband several times; each cycle may be an episode. Episodes are marked by changes in membership composition, the terms of the collusive agreement, method of management, geographic focus, or other major changes. Each time a new collusive episode begins, chances are that the methods and membership composition have changed; pauses between episodes are often quite lengthy. In some of the cartels the interregnum is a period of contract renegotiation. Because the agreement and the players are different, in effect a new cartel is launched. Our sample contains at least 512 episodes.⁴⁹

In general, the distribution of episodes across types of cartels is quite similar to the distribution of cartelized markets. The major difference is that international cartels tended to have a larger number of multiple episodes than did domestic ones. The 88 international markets in the sample that were cartelized had on average 1.6 episodes, whereas national cartels had only

⁴⁹ One study from which we obtained a dozen observations summarized the results of 109 bid-rigging convictions in the fluid milk markets of the Southeastern United States within a few years (Lanzillotti 1996). We count each conviction as an episode. If one prefers to count the Lanzillotti summary and two other “group studies” as a three episodes, then the episode total becomes 332.

1.3 episodes on average. As a result, a larger share (44%) of the cartel episodes had international membership. The number of episodes per market does not vary significantly across other type categories.

Table 3. Number of Average Overcharge Observations, by Type of Cartel

Type	Number	Percent
International membership	365	54.2
National or regional	309	45.8
Bid-rigging schemes	185	27.4
Classic cartels	489	72.6
Cartels found guilty or liable ^a	384	57.0
No record of sanctions (“legal”)	290	43.0
Total	674	100.0

Source: Connor (2004b: Appendix Table 1) (spreadsheet dated 10-14-04).

^a Included are six cartels still being investigated by authorities.

Researchers usually report the *average* price increases over the whole episode (Table 3). This is the measure most relevant for forensic purposes and is the one that will be the focus of most analyses in this paper.⁵⁰ We have collected 635 of these estimates; 94% of all episodes report average overcharges. In some cases, the averages are carefully weighted by the sales in each year or month of the episode, but in most cases the authors give equal weights to the price changes in each sub period during the total affected period. Sometimes it is not clear from the source whether the averages are weighted or unweighted; if the conspiracy period is marked by steady slow market growth, it matters little which is reported. Some of the overcharge estimates

⁵⁰ One-fourth (210) of the 845 overcharge figures that were assembled, are *peak* price effects. Thirty-one percent of the episodes have peak estimates. In some cases the peak price was reached for only one day during a cartel period of several years; in other cases, the peak may be the highest one of several years. Peak price changes indicate the potential for maximum harm when a cartel is at its most disciplined. Classifying a particular estimate as an average or peak figure in a minority of cases required judgment. If the original source is unclear about which type of estimate is being presented, in order to be conservative we have assumed it is a peak estimate.

are said to be *minimum* estimates. To be conservative, all such minimum estimates are recorded as averages.⁵¹

A META-ANALYSIS OF THE OVERCHARGES

Our catholic approach to data-gathering might well create concerns in the minds of many readers about the reliability and precision of the overcharges. There may be substantial variation in the quality of the price data, the methods used, degrees of judicial scrutiny, the professional orientation of the authors, and the degree of peer review that could affect the reliability of the estimates. Connor (2004b) notes a lack of clarity among professional economists about the essential characteristics of cartels until at least the 1920s. Consequently, some readers may wish to dismiss scholarship before that decade, while others will be untroubled by semantic differences. Economists may well give greater weight to writings by professionals in their own field than to opinions reached by judges, commissions, or juries, whereas legal scholars will often give greater credence to the latter. Similarly, many economists might trust results published in refereed scientific journals more than other publication outlets that receive less peer scrutiny, prefer modern quantitative methods to deep historical case studies, or express skepticism about the analyses of economists writing before the Age of Game Theory. We will address these concerns by applying a meta-analysis to our data set in subsequent section.

Methodology and Hypotheses

Meta-analysis may be described as the analysis of analyses, specifically a statistical analysis of a large collection of individual studies for the purpose of synthesis and integration of findings

⁵¹ Some averages are given as ranges, but we have used the midpoints of the ranges for this paper.

(Wolf 1986: 10-11). It is a way of formally examining a sample of previous empirical studies that contain an identical “effect-size statistic.” The most common meta-analyses focus on measures of central tendency, but some also examine responses across groups of respondents, types of publications, or methods of analysis.⁵² Meta-analysis differs from traditional literature reviews because the latter are selective samples, typically rely on the reviewer’s impressions of quality, often weight the included studies subjectively, and usually fail to consider relevant methodological variation in the sample.

Meta-analysis is a statistical procedure applied mainly in the social and medical sciences (Lipsey and Wilson 2001: 12-17).⁵³ Meta-analysis has been used by economists since about 1985 to evaluate previous research on estimates of size effects.⁵⁴ Several economic studies have adopted meta-analysis to investigate the impact of study characteristics, such as sample size, model structure, and estimator choice on an economic variable of interest. As Smith and Kaoru (1990) note, economics does not usually employ data collected from controlled experiments. Therefore, meta-analysis is used differently in economics than in psychology or the medical sciences. Stanley and Jarrell (1989) named the use of meta-analysis in economics “meta-regression analysis,” because such analyses are typically applied to data collected from the studies employing econometric techniques. Meta-regression analysis specifies variables (“meta-independent variables”) that represent various characteristics of the studies from which the estimates of a variable of interest are obtained. Meta-independent variables usually include specifications of the estimation procedure, publication source, the year of the study and others.

⁵² Perhaps the earliest meta-analysis that focused on variation in modeling choices is the examination of consumer surplus estimates derived from travel-cost demand models by Smith and Kaoru (1990).

⁵³ Meta-analysis was originally applied to size effects from samples of controlled experiments but is now extended to samples of quasi-experimental results common in economics.

⁵⁴ The first reference to the term in statistics dates from 1976 (*Oxford English Dictionary Online*), and the first article using meta-analysis to appear in the bibliographic search engine ECONLIT is Peterson *et al.* (1985). About 150 meta-analyses of empirical studies have since been published, of which consumer demand (34 studies) and labor economics (23 studies) are the most common fields.

Examples of met-regression analysis cited by Stanley and Jarrell (1989) are: analysis of the Ricardian equivalence theorem (Stanley, 1998), farmer education and farmer efficiency (Phillips, 1994), the effect of state and local taxes on economic development (Phillips and Goss, 1995), minimum wage (Card and Krueger, 1995), and multinational companies and productivity spillovers (Görg and Stroble, 2001).

Smith and Huang (1995) and Smith and Kaoru (1990) apply meta-analysis to hedonic models of value air quality and variability in recreation benefits. While they also used regression analysis, their studies differ from classic meta-regression analysis because their explanatory variables included characteristics of the *dependent* variables along with the usual meta-independent variables. For example, in analyzing the variability in the estimates of the real consumer surplus from using recreation sites, Smith and Kaoru (1990) include variables characterizing type of recreation and type of site. Our paper follows the example of Smith and Kaoru (1990).

To the best of our knowledge our meta-analysis of cartel overcharges presented in our paper is the first meta-analysis of cartel overcharges and appears to be the first meta-analysis in the field of industrial economics. To investigate the variability in overcharge estimates appeared in the literature we use meta-independent variables along with variables representing different characteristics of cartels and market environment of their operation. Our analysis may be unique because it includes meta-independent variables that capture alternative methods of overcharge estimation and different publication sources. This modeling decision is prompted by the fact that in our data set econometric modeling is only one of the eight estimation methods used to derive the overcharge estimates. Given our research objective, this specification of regression meta-analysis is an appropriate procedure.

Empirical Model

We specify seven models to evaluate econometrically three general sources of variation in overcharges: estimation methods, publication sources, and cartel characteristics. Cartel characteristics include duration, mode of organization, legal status, region of operation, and antitrust environment. The most general model is:

$$OVCHG_i = \alpha + \phi * DUR_i + \beta * C_i + \gamma * G_i + \varphi * P_i + \mu * M_i + \delta * S_i + \varepsilon_i.$$

All other models are special cases of this model.

The dependent variable in all models is the size of overcharge ($OVCHG_i$) imposed during cartel episode i . Each model is fitted to a sample consisting of 395 observations representing different cartel episodes. The explanatory variables of the general model are an intercept (α); a discrete variable representing duration of the cartel's episode (DUR_i); three vectors of binary variables representing the cartels' basic organizational characteristics (C_i), geographic markets of operation (G_i), different periods of antitrust law regimes (P_i); two sets of binary meta-independent variables characterizing eight methods of overcharge estimation (M_i), seven publication sources (S_i); and an error term (ε_i). A list of the explanatory variables and their expected signs is presented in Table 4.

Model [1], Model [2] and Model [3] include only meta-independent variables. Model [3] is the nested model of Model [1] and Model [2]. Model [4] includes only cartel characteristics and the legal environment. Model [5] and Model [6] are nested cases of Model [4] with Model [1] and Model [4] with Model [2], respectively. Finally, Model [7] is the most general model represented by the equation above.

Hypotheses

Given that some estimation methods can be related to each other, it is particularly difficult to predict the signs of the coefficients for the estimation methods.⁵⁵ For example, for many cartels PBEFORE, PWAR, and PAFTER could each be drawn from but-for periods that have unsustainably low non-equilibrium prices. Nevertheless, cartel histories seem to demonstrate a greater proclivity for large price collapses after termination than price wars before formation.⁵⁶ Therefore, one might not expect to observe statistically significant difference between PWAR and the reference category PAFTER, but PBEFORE ought to be positive. Because of the difficulties of accounting for all economic costs from accounting data, we expect COST methods to overstate OVCHG. Econometric methods and deep historical studies are better able to control for shifts in demand or supply than the before-and-after methods, which should result in more conservative estimates of OVCHG. Choosing a cartel-unaffected yardstick requires judgment, but is a potentially neutral method of calculation. On the other hand, there is a danger that analysts might choose substitute products or regions to serve as yardsticks that underestimate the umbrella effects on yardstick products or the geographic spillover effects of a conspiracy on the but-for price. We are uncertain about the signs of YARDST and OTHER.

Developing hypotheses for publication sources is also difficult. However, we believe that archival publications with supervisory review will generally produce more conservative overcharge estimates than fugitive publications driven by a need to sell. MONOGR (archival but not peer reviewed and sales-driven) is the reference category. Because GOVREP, JOURNAL, and EDBOOK are generally peer-reviewed and not sales-driven, OVCHG ought

⁵⁵ Some of our predictions are more fully developed in Connor (2004a).

⁵⁶ This was true for most of the vitamin cartels (Connor 2005).

Table 4. Definitions of Explanatory Variables

Explanatory Variable	Definition	Expected Sign
DURATION	Discrete variable in the range of 1 to 4, characterizing duration of episode: = 1 if duration is less or equal to 5 years, = 2 if duration is from 6 to 10 years, = 3 if duration is from 11 to 15 years, = 4 if duration is greater than 16 years.	+
<i>Binary variables representing organizational characteristics</i>		
DOMESTIC	= 1 if members of cartel from one country	-
BIDRIG	= 1 if a cartel is bid-rigging.	+
GUILTY	= 1 if a cartel is found or pleads guilty.	-
<i>Binary variables representing geographic markets</i>		
US	= 1 if overcharge is for the U.S. and Canadian markets.	-
EU	= 1 if overcharge is for the E.U. or any of European countries markets.	-
ASIA	= 1 if overcharge is for the any Asian country or Australia.	?
ROW	= 1 if overcharge is for ROW including Latin America.	?
<i>Binary variables representing different antitrust law regimes</i>		
P1	= 1 if cartel episode belongs to the period of 1770-1890.	+
P2	= 1 if cartel episode belongs to the period of 1891-1919.	reference
P3	= 1 if cartel episode belongs to the period of 1920-1945.	-
P4	= 1 if cartel episode belongs to the period of 1946-1973.	-
P5	= 1 if cartel episode belongs to the period of 1974-1990.	-
P6	= 1 if cartel episode belongs to the period of 1991-2004.	-
<i>Binary variables, representing overcharge estimation methods</i>		
OTHER	= 1 if no explanation, others.	?
HISTOR	= 1 if no explanation, historical case study.	-
PBEFORE	= 1 if price before conspiracy.	+
PWAR	= 1 if price during price war or laps of collusion.	?
PAFTER	= 1 if price after conspiracy.	reference
YARDST	= 1 if yardstick.	?
COST	= 1 if normal profit or total cost.	+
ECON	= 1 if econometric modeling.	-
<i>Binary variables, representing publication sources</i>		
JOURNAL	= 1 if peer reviewed journals, including academic journals.	-
EDBOOK	= 1 if chapters in edited books.	-
MONOGR	= 1 if monograph or books.	reference
GOVREP	= 1 if official government report.	-
COURT	= 1 if court or antitrust authorities source.	?
WORKP	= 1 if unpublished working paper.	+
SPEECH	= 1 if speech or conference presentation proceedings.	+

to be negative; the opposite is true for WORKP and SPEECH. We are uncertain about COURT.⁵⁷

We expect that longer duration of a conspiracy episode leads to a higher level of overcharge imposed by cartels.⁵⁸ If a cartel is successful in maintaining its cohesiveness for a long time, that may signal the likelihood that the conspirators can amiably negotiate and renegotiate price agreements. International cartels are expected to have a higher level of overcharges relative to domestic cartels because possible geographic price discrimination may provide an opportunity for increased overcharges. Also, international cartels do not have import competition that domestic cartels may face. We hypothesize that bid-rigging cartels have a higher level of overcharge than other types of cartels. This hypothesis follows from the U.S. Sentencing Guidelines, which increase the base offence level by 1 if a cartel submitted non-competitive bids (paragraph 2R1.1). We hypothesize that *ceteris paribus* incompetent cartels are more likely to be caught and punished. By analogy, illegal cartels are likely to be less competent in generating high overcharges than cartels that were not prosecuted. Therefore, we expect the estimated coefficients for DURATION and BIDRIG to be positive and those for DOMESTIC and GUILTY to be negative.

Given that the reference cartels are global ones⁵⁹, we hypothesize that if the cartels implement geographic third-degree price discrimination, then the estimated coefficients for US, EU, ASIA, and ROW are statistically significant. Low income countries may be discriminated against by cartels. At the same time we expect that the markets with strong and effective

⁵⁷ Judges and juries may tend to sympathize with prosecutors or allegedly injured plaintiffs, and prosecutors (especially in criminal cases) may bring to court only the most egregious cases; on the other hand corporate defendants are often able to pay for high quality representation. Judges and Commissions are likely to be cautious about overcharge claims out of fear of higher court reversals.

⁵⁸ Duration may be influenced by the expected and achieved overcharge rate (Zimmerman 2005).

⁵⁹ Recall that global cartels operated in two or more (usually three or more) continents.

antitrust law enforcement will have relatively low levels of overcharges. Clarke and Evenett (2003) found evidence of such price discrimination in the global vitamins cartel. In summary, we expect that the estimated coefficients for EU and US to be negative, and the estimated coefficients for ASIA and ROW are expected to be lower than the high income regions but of uncertain sign.

The data set we use in the study includes cartel episodes ranging from the 18th century to 2004. Connor (2005) distinguishes six historical antitrust law regimes.⁶⁰ It is assumed that each subsequent regime has tougher, more effective anticartel regulation and enforcement than the previous regime. Therefore, the estimated coefficients for P2...P6 are expected to be negative and increasing over time.

Meta-Analysis Data Set Description

To conduct a meta-analysis we compile a sub-set of the data set discussed above using the following procedure. First, two types of estimates were available: average (low and high) and peak (low and high) overcharges. We decided to analyze the average low level of overcharge to conduct the most conservative analysis. Second, some episodes were represented by more than one overcharge estimate. This happens because the same episode was analyzed in different studies and/or different methods of overcharge estimation were used. Finally, in addition to research reported in the academic literature, overcharge estimates became available from court decisions. So, we had to eliminate all redundant estimates to form an appropriate data set to be used in a meta-analysis. Again, to follow the most conservative approach, we included in the data set the lowest overcharge estimate among available alternatives for each episode⁶¹.

⁶⁰ These periods are admittedly somewhat judgmental, and they may be correlated with changes in or progress in social-science methods of analysis.

⁶¹ We exclude the three obvious outliers.

Descriptive statistics for the meta-analysis data set is presented in Table 5. The survey nature of the data set may introduce additional noise. The mean overcharge of the total sample is 28.88 percent and the median is 19 percent. The minimum value of overcharge is -10 percent, and the maximum value is 322 percent⁶².

Table 5. Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
OVCHG	28.88	1.874	-10	322
DURATION*	1.90	0.063	1	5
DURATION**	8.61	0.566	0.08	98
DOMESTIC	0.47	0.025	0	1
BIDRIG	0.18	0.020	0	1
GUILTY	0.65	0.024	0	1
US	0.38	0.025	0	1
EU	0.31	0.023	0	1
ASIA	0.09	0.014	0	1
ROW	0.04	0.010	0	1
P1	0.13	0.017	0	1
P3	0.23	0.021	0	1
P4	0.15	0.018	0	1
P5	0.17	0.019	0	1
P6	0.32	0.023	0	1
OTHER	0.21	0.021	0	1
HISTOR	0.01	0.005	0	1
PBEFORE	0.33	0.024	0	1
PWAR	0.02	0.007	0	1
YARDST	0.11	0.016	0	1
COST	0.06	0.012	0	1
ECON	0.15	0.018	0	1
JOURNAL	0.20	0.020	0	1
EDBOOK	0.08	0.013	0	1
GOVREP	0.03	0.009	0	1
COURT	0.24	0.022	0	1
WORKP	0.17	0.019	0	1
SPEECH	0.01	0.004	0	1

* *duration as discrete variable*

** *duration as continuous variable (in years)*

⁶² There are three negative overcharge estimates in the sample of 395 cartel episodes. These undercharges are -10, -5, and -1 percent. In these cases the authors interpreted the decline in price as failure to effectively collude.

The mean duration of cartel episodes is 8.61 years with the minimum duration of one month and the maximum duration of 98 years. International cartels represent 53 percent and bid-rigging cartels represent 18 percent of the total sample. Approximately 65 percent of cartels were found or pled guilty. Some of the cartels that were not guilty were represented by legal cartels and those not prosecuted.

There are few overcharge estimates available for cartels that operated in Asia and Latin America. This is because antitrust law has been enforced in the US, Canada, and the EU for a longer period of time than in other countries, thus making discovery more likely. Some Asian countries started enforcing antitrust regulation recently. However, many other countries either do not have antitrust law or similar regulation at all, or have it but do not enforce it. Therefore, overcharge estimates are very rare for these markets. As for the distribution of cartels episodes across the different antitrust regimes, total sample episodes are distributed relatively evenly across six periods covering 1770-2004 with 32 percent belonging to last 14 years.

Most of the overcharges included in the meta-analysis sample were estimated using PBEFORE, OTHER, ECON, YARDST, and PAFTER methods. These estimates represent 33, 21, 15, 11, and 11 percent of the sample, respectively. Most overcharge estimates were collected from MONOGR, COURT, JOURNAL, and WORKP. These estimates constitute 27, 24, 20, and 17 percent of the total sample, respectively.

Results

Given the survey nature of our data set, we do not make any strong assumptions about the error distribution, and we estimate the models with the ordinary least squares estimator (OLS) as semi-parametric linear regression models. We do not conduct any formal tests for the presence of

autocorrelation and heteroscedasticity. First, as the data come from widely different periods of time and the overcharges are estimated for different episodes with different length, we cannot organize the data to easily capture dynamics. Second, we assume that the errors are homoscedastic because we are not aware of a particular form of the heteroscedasticity associated with these data. Thus, the results of both specification tests may be misleading at this stage.

The ordinary least square estimation results for all models are represented in Table 6. The estimation results of Model [1] indicate that the variation in estimation methods does not have a statistically significant impact on the variability of overcharges. The reference (intercept) group is the “price after collusion” method. The Wald statistic tests whether all explanatory variables (except intercept) are jointly significant; this statistic rejects the null hypothesis of no joint effect of differences in the estimation methods on the variability of overcharge at the probability of type 1 error equal to 0.3778.⁶³ The average overcharge estimated using yardstick method is 15.71 percent higher than the average overcharge estimated using the reference group method and is statistically significant at alpha equal to 0.0490. At alpha equal to 0.1530 the overcharges estimated using “price war or lapse of the collusion” method are 21.71 percent higher than the overcharges estimated using the reference method. Using econometrics method results in the 8.37 percent higher overcharge estimates than those of reference group at alpha equal to 0.2600. The rest of the methods are not statistically significant from the intercept group at an acceptable level of alpha.⁶⁴

⁶³ Further in the analysis instead of “probability of type 1 error” we are using “alpha”. Discussing results, we present p-values to allow the readers to form their own opinions about statistical significance of the estimated coefficients or test statistics in each individual case.

⁶⁴ Given the large number of economically important omitted from our model, we feel that a statistical significance of 20% or better may be noteworthy.

Table 6. Cartel Overcharges, OLS Estimation Results

Estimated Coefficient	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Intercept	22.92*	31.33	26.50	39.47	35.05	43.45	40.21	39.42
	5.61	3.53	6.18	6.63	8.79	7.43	9.21	6.61
	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
DURATION				4.81	3.98	4.67	3.91	3.95
				1.81	1.93	1.80	1.91	1.78
				0.0080	0.0390	0.0100	0.0420	0.0270
DOMESTIC				-8.49	-9.68	-11.26	-12.34	-14.35
				5.04	5.15	5.17	5.28	4.42
				0.0930	0.0610	0.0300	0.0200	0.0010
BIDRIG				-2.53	-2.93	0.96	1.77	
				5.47	5.61	5.58	5.77	
				0.6440	0.6010	0.8630	0.7590	
GUILTY				-1.44	-1.63	-6.03	-6.49	-5.32
				4.71	4.81	4.99	5.04	4.78
				0.7600	0.7350	0.2280	0.1990	0.2670
US				-5.12	-5.49	-6.30	-6.32	-5.18
				6.03	6.14	6.28	6.32	4.74
				0.3960	0.3710	0.3160	0.3180	0.2750
EU				-6.87	-7.56	-9.60	-9.83	-9.31
				5.65	5.99	6.11	6.18	4.82
				0.2250	0.2080	0.1170	0.1130	0.0540
ASIA				1.24	0.35	-2.25	-2.16	
				8.68	8.86	9.20	9.27	
				0.8870	0.9680	0.8070	0.8160	
ROW				4.83	5.98	1.04	2.38	
				10.24	10.70	10.44	10.74	
				0.6380	0.5760	0.9210	0.8240	
P1				-11.18	-8.69	-8.31	-5.48	
				7.08	7.25	7.15	7.33	
				0.1150	0.2310	0.2460	0.4560	
P3				-8.26	-8.21	-8.11	-7.76	-6.56
				6.03	6.19	6.06	6.20	5.57
				0.1720	0.1860	0.1820	0.2120	0.2400
P4				-11.64	-11.54	-11.46	-11.72	-10.46
				6.66	6.88	6.73	6.92	6.31
				0.0810	0.0940	0.0900	0.0910	0.0980
P5				-7.64	-7.27	-9.92	-10.23	-8.07
				6.23	6.40	6.38	6.59	5.73
				0.2210	0.2570	0.1210	0.1220	0.1600

P6				-13.27	-12.97	-17.72	-17.85	-13.13
				5.92	6.08	6.88	7.02	5.36
				0.0260	0.0330	0.0100	0.0110	0.0150
OTHER	2.41	1.52			5.74		1.71	
	6.94	7.16			7.09		7.26	
	0.7290	0.8320			0.4190		0.8140	
HISTOR	-10.42	-15.33			-19.68		-23.74	-27.24
	19.44	19.54			19.75		19.64	18.73
	0.5920	0.4330			0.3200		0.2270	0.1470
PBEFORE	6.29	5.86			6.30		6.07	3.43
	6.49	6.47			6.61		6.56	4.49
	0.3330	0.3650			0.3410		0.3550	0.4460
PWAR	21.71	21.68			11.87		11.22	
	15.15	15.12			15.96		15.81	
	0.1530	0.1520			0.4570		0.4780	
YARDST	15.71	14.10			16.32		12.49	10.99
	7.94	8.05			8.14		8.18	6.42
	0.0490	0.0810			0.0460		0.1270	0.0880
COST	1.53	-1.99			6.71		0.35	
	9.58	9.90			9.80		10.11	
	0.8730	0.8400			0.4940		0.9720	
ECON	8.37	7.56			7.41		10.31	4.81
	7.42	8.68			8.10		9.12	6.36
	0.2600	0.3840			0.3610		0.2590	0.4500
JOURNAL		-8.02	-10.00			-2.01	-4.35	
		5.49	6.01			6.12	6.40	
		0.1450	0.0970			0.7430	0.4970	
EDBOOK		7.00	5.30			-1.59	-5.43	
		7.63	9.34			8.22	9.77	
		0.3600	0.5710			0.8470	0.5790	
GOVREP		-24.60	-23.17			-23.68	-22.17	-21.36
		11.27	11.47			11.53	11.71	11.24
		0.0300	0.0440			0.0410	0.0590	0.0580
COURT		1.41	1.78			14.33	15.59	14.24
		5.19	5.69			6.64	7.09	5.16
		0.7860	0.7540			0.0310	0.0290	0.0060
WORKP		-5.47	-4.87			4.61	5.34	
		5.74	5.84			7.85	7.95	
		0.3420	0.4050			0.5580	0.5030	
SPEECH		-8.93	-3.28			6.54	11.50	
		21.69	22.38			22.61	23.33	
		0.6810	0.8840			0.7730	0.6220	
R2	0.0190	0.0252	0.0443	0.0565	0.0716	0.0882	0.1038	0.0958
R2 adj.	0.0013	0.0102	0.0117	0.0244	0.0220	0.0420	0.0405	0.0600
Wald St.	7.51	10.04	17.66	22.83	28.86	36.26	42.62	40.17
p-value	0.3778	0.1229	0.1707	0.0437	0.0905	0.0098	0.0212	0.0004

* figures in the columns corresponding to each estimated coefficient represent an estimate, its standard error and p-value.

The differences in the publication sources have a statistically stronger impact on the variability of overcharge than the differences in the estimation methods. The intercept in Model [2] is represented by the overcharges collected from monographs and books. Using the results of the Wald test we reject the null hypothesis of no joint effect of the differences in the publication sources on the variability of the overcharge at alpha equal to 0.1229. Overcharge estimates appeared in the official government reports, 24.60 percentage points lower, on average, than overcharge estimates appeared in the publication reference groups and are statistically significant at alpha equal to 0.0300. Overcharge estimates presented in the peer-reviewed and academic journals are 8.02 percent lower than those of the reference group at alpha equal to 0.1450 percent. The rest of the publication sources not statistically significant from the intercept group at an acceptable level of alpha.

The joint effect of different estimation methods and publication sources on the variability of overcharge is investigated by Model [3]. Using the results of the Wald test we reject the null hypothesis of the absence of this joint effect on the variability of overcharge at alpha equal to 0.1707. In this model the magnitude and the statistical significance of most of the coefficients are approximately the same as in Models [1] and [2].

The estimation results of Model [4] show that different cartel characteristics along with market environment of cartel operation have a highly significant impact on the variability of overcharge. The Wald test rejects the null hypothesis of no joint effect of different cartel characteristics and market environment of cartel operation on the variability of overcharge at alpha equal 0.0437. After incorporating in this model explanatory variables characterizing either different estimation methods of publication sources (Model [5] and Model [6] respectively) the magnitudes, signs, and statistical significance of many coefficients remain at approximately the

same levels as in the special case models discussed so far. The estimated coefficients that were not statistically significant at acceptable alpha level in the special case model might change direction of the marginal effect.

To get the most complete picture of an impact of all available factors on the overcharges, we estimated Model [7]. The Wald test rejects the null hypothesis of no joint effect of all the independent determinants considered in our model at alpha equal to 0.0212. Next, using the results of Model [7] we eliminated the variables with estimated coefficients that were not statistically significant at alpha equal to 0.5 or higher for Model [8]. We reject the null hypothesis of no joint effect of all explanatory variables (excluding intercept) at alpha equal to 0.0004. Model [8] is a preferred model for our final conclusions.⁶⁵

The estimation results of Model [8] have important implication for antitrust policy. The signs of the most of the estimated coefficients are as expected (see Table 4), and many are statistically significant at an acceptable level of alpha. Each additional five years of cartel operation increases the overcharge by 3.95 percentage points on average. International cartels on average impose overcharges 14.35 percentage points higher than domestic cartels. The marginal effects of DURATION and DOMESTIC are statistically significant at alpha equal to 0.0270 and 0.0010 respectively. Those cartels that were found or pled guilty on average imposed 5.32 percent lower overcharges than those cartels that were not under prosecution and legal cartels, however, this effect is statistically significant only at alpha equal to 0.2670 percent. BIDRIG was not included in Model [8] because it had not shown a statistically significant impact at an acceptable alpha level in the other models. Therefore, bid-rigging cartels are no more harmful than the “classic” types.

⁶⁵ Again, those estimated coefficients that were significant in other models are significant in Model (8) and have approximately the same magnitude.

As for the hypothesis on geographic price discrimination, we found that only the estimated coefficient for EU is statistically significant at an acceptable level of alpha equal to 0.0540. US is weakly statistically significant at alpha equal to 0.2750 only. ASIA and ROW were not statistically significant in any of the models at an acceptable alpha level, which means that their overcharges were lower than global but higher than the North American and EU cartels. In comparison with the reference group of global cartels, the overcharges achieved by more localized cartels in North America and Western Europe are 5.18 and 9.31 percentage points lower. These results support the idea that there is some broad regional geographic price discrimination exercised by cartels. Cartels operating in the US, Canada, and EU tend to achieve lower overcharges in markets with strongly enforced antitrust laws. In addition, relative to the reference group, overcharge rates in the US and Canada are higher than those in EU countries.

The magnitudes of the coefficients of the most recent different antitrust-law regimes (P3, P4, P5 and P6) imply that the size of the overcharge were 6.56, 10.46, 8.07 and 13.13 percentage points lower in the most recent four regimes than two earliest periods of history. P4 and P6 are statistically significant at alpha equal to 10 percent, and P3 and P5 are statistically significant at a higher but acceptable alpha level. This suggests that antitrust regimes were increasingly effective after 1919 and most effective in 1990-2004 when anticartel sanctions were harshest.⁶⁶

As for an impact of the differences in the estimation methods on the level of overcharge, only HISTOR and YARDST significantly differ at acceptable alpha level from the reference group represented by PAFTER, OTHER, PWAR, and COST. As expected, the overcharge estimates from historical case studies were on average 27.24 percentage points lower than

⁶⁶ The extreme breadth of time covered by our sample would seem to argue against “globalization” as a force pressing down on cartel overcharges. Trade openness was higher in 1890-1930 than most other periods, and foreign direct investment high throughout 1945-2004. Nevertheless, the antitrust hypothesis awaits more formal testing.

overcharges estimated with any of the reference group methods. In contrast, the overcharge estimates recovered using YARDST are on average 10.99 percent higher than the overcharges estimated using reference group methods. PBEFORE and ECON are not statistically significant from the reference group at an acceptable alpha level. The nonsignificance of statistically computed estimates is rather surprising, because statistical methods have become the preferred approach since the 1970s. The ancient before-and-after and cost-based methods do not produce systematically different overcharge estimates.⁶⁷

Two publication sources, those represented by official government reports and court and antitrust authorities decisions, have a statistically significant impact on the overcharge rate relative to reference group of publication sources (MONOGR, JOURNAL, EDBOOK, WORKP, and SPEECH). Overcharge estimates gathered from official government reports are, on average, 21.36 percentage points lower than the overcharges appeared in any of the reference group publications.⁶⁸ In contrast, the overcharge estimates obtained from decisions of various antitrust authorities were, on average, 14.24 percentage points higher than those of the reference group.

CONCLUSIONS

This paper is the first large-scale survey of quantitative estimates of cartel overcharges to appear in the industrial-economics literature. It analyzes a sub sample of a survey of more than 800 observations of overcharges imposed by private price-fixing cartels that operated in diverse markets during the last 250 years. These overcharges, which were calculated using several methods and are drawn from various type of publication sources, under the most conservative evaluation approach have a mean value of 29 percent of affected sales and the median of 19

⁶⁷ Both of these methods were evident in late 19th century social-science analyses.

⁶⁸ A large number of these estimates were sponsored by the League of Nations in the 1930s.

percent. After an extensive description of the data set, this paper performs a unique regression meta-analysis of overcharge estimates. This analysis provides a rigorous evaluation of how much noise in the overcharge estimates comes from the alternatives methods of overcharge estimation and publication types. Also, it allows quantifying the impact of different characteristics of cartels and the market environment of their operation on the variability of the overcharge estimates appeared in the literature.

We have found that the differences in the alternative estimation methods do not have a statistically significant impact on the variability in overcharge estimates at an acceptable significance level. In contrast, the differences in the publication sources do have a statistically significant impact. An important finding is that the overcharges estimated using the yardstick method were on average 11 percentage points higher and the overcharges estimated as part of historical case studies were 27 percentage points lower than the overcharges estimated using any other method. Overcharges appearing in official government reports are 21 percentage points lower and the overcharges obtained from court and antitrust authority decisions 14 percentage points higher than the overcharge estimates found in any other publication source.

Our meta-analysis provides relevant antitrust-policy guidelines. We found that international cartels impose 14 percentage points higher overcharges than domestic cartels. This supports an idea that international cartels justify greater antitrust scrutiny and sanctions than domestic cartels. We also found out that overcharges achieved by local cartels North American and EU countries are lower than the overcharges of cartels operating in Asia or Latin America. In addition, overcharges imposed in EU member states are lower than those in North America. From an optimal-deterrence perspective, this result may justify lower fines (as a percentage of affected sales) on *local* cartels by the EU and its member states than *local* cartels in North

America. However, this result does not justify lower EU fines than the public and private sanctions placed by North American authorities on *global cartels*.⁶⁹

Cartels impose lower overcharges in jurisdictions with strongly enforced antitrust laws. To effectively deter international cartels, Asian and Latin American jurisdictions ought to become more active in prosecuting such cartels. However, we did not find any support to the fact that bid-rigging cartels imposed higher overcharges than other types of cartels. For each five additional years of cartel operation, the overcharge level rises by 4 percentage points. Thus, durable cartels ought to be fined at higher rates than short-lived conspiracies.

Finally, our results show that with time antitrust regulation seems to have become more effective. Overcharges imposed during the period 1920-2004 were lower than overcharges imposed during the period 1770-1919. Since 1919 overcharge estimates have declined. The overcharge estimates achieved by cartels during 1991-2004 were 13 percentage points lower than those imposed during 1770-1919.

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⁶⁹ Fines on the vitamins cartels by the European Commission totaled 20% of the fines and private settlements paid to North American governments and plaintiffs, even though affected sales were much larger in the European Economic Area (Connor 2005: Table 9).

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