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Comment on "The Empirical Basis for Antitrust: Cartels, Mergers, and Remedies"

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JOHN M. CONNOR and ROBERT H. LANDE

ABSTRACT *In this journal, James Langenfeld critically reviewed four of the present authors' articles that analyze the size of cartel overcharges and their antitrust policy implications. In this comment, we explain why we believe Langenfeld errs in his criticism of our work. In particular, this comment discusses the variation in research quality of the sources used to compile a large sample of historical cartel overcharges; the advisability of trimming outliers or large estimates from the sample; alleged publication bias; why our 25% median estimate is much more likely to be correct than the US Sentencing Guideline's 10% presumption; and the implications of the average cartel overcharges results for optimal deterrence and antitrust policy.*

Key Words: Cartel; Collusion; Overcharges; Price Effects; Antitrust; Competition Law; Price Fixing.

JEL Classifications: K14; K21; L11; L13; L4.

Dr. James Langenfeld, who has a distinguished record in government, academia, and economic consulting, published an article in this journal critically assessing research on empirical estimates of cartel overcharges.[1] Specifically, Langenfeld reviews four publications by the present authors. The first is a lengthy law-review article that lays out in some detail the sources, methods, and descriptive patterns of overcharges of cartels by type, place, and time period (Connor 2014a).[2] He also comments on three antitrust policy results of the overcharges findings in Connor and Lande (2008, 2012, 2015). Langenfeld gives two reasons for choosing these four articles: (1) they illustrate "the state of the art" in empirical cartel studies, and (2) they tend to be cited in support of "more aggressive antitrust enforcement" and "stronger monetary penalties for antitrust conspiracies" (Langenfeld 2017, 1–2, 15). After offering these remarks, Langenfeld then tends to focus on a number of putative shortcomings in these papers.

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We agree with Langenfeld that some of these issues may be conceptually troubling. However, in this comment, we show that the alleged imperfections in the overcharges estimates are not empirically significant for the purpose of drawing policy implications. In the comment, we address Langenfeld's erroneous reasoning and his lack of empirical support for his assertions that his critiques undermine the importance of this research.

Criticisms of the Overcharges Data

One repeated point made by Langenfeld is that the "quality" of the cartel overcharge estimates collected vary significantly.[3] This follows from a conscious research decision we made at the outset for the following reasons. First, the statistical training to which all economists adhere demands that no sample point ought to be lightly discarded. An outlier in a data set can be rejected for a statistical analysis only after initial modeling is performed and the data point has characteristic that proves that it comes from a different universe. Had we picked and chosen only the "best" cartel studies, we would have been open to the criticism that the cartel studies we included were not typical or representative. So we chose the opposite approach: an all-inclusive catholic approach, which of course comes with its own set of problems.

Second, it became apparent that book-length studies of the archives of cartel secretariats by historians or the occasional journalist, although methodologically dated, were worthy of respect.[4] How can a researcher judge what was worthy or unworthy from a scholar from a different era or from a different tradition? And which is worse: omitting the principal work of some contemporary economist focusing on the mastery of a dubious cutting-edge method, or including an analysis of some great but superannuated 19th century scholar?

Third, there are ways to adjust for variable quality of data points in analytically approved ways. As discussed below, meta-analysis is one such method. Similarly, we included as a rough double-check on the results of our sample of economics studies a data sample that Langenfeld completely ignored: final verdicts in US cartel cases that were not overturned on appeal.[5] For whatever reason, Langenfeld discounts this sample. Many policy makers are lawyers, however, and they might well think more highly of court verdicts.

Cartel scholars have adopted many different criteria to compile their literature reviews. For example, Connor and Lande (2005, table 1) present mean and median cartel overcharges contained in several prominent researchers' literature reviews, the following three of which Langenfeld discusses. One fairly could ask, for example, how, out of the hundreds of cartel studies available, did Judge Posner chose those 12 to include in his study, how Dr. Werden chose a different 13, or how Professors Levenstein and Suslow chose a different 22?[6] One might ask whether they had surveyed every available study before including only the "best" cartel studies, and, if this was their criterion, how they decided which studies were "best". These authors are, however, silent as to how they chose their studies and, perhaps for this reason, make no explicit claims of representativeness of their compilations.

Each method of inclusion or exclusion has advantages and disadvantages. We contend that a method that selects nearly every cartel study is also valid – even though some of the covered studies will of course be considered superior to others by various evaluators.

Finally, a point not mentioned by Langenfeld is that the present authors have served transparency by repeatedly laying bare their assumptions, sources, methods of analysis, and raw data (Connor 2014b).[7] This level of detail permits users to accept or reject any data point they want to, which is exactly what Oxera (2009) did for the European Commission. A spreadsheet containing overcharges estimates and several other quantitative characteristics has been made available to scores of economists, consultancies, and regulators.[8]

Yes, Some Overcharges Are Shockingly High, But So What?

On page 6, Langenfeld seems to be concerned that the Cartel Overcharges data set "contains so many large outliers."[9] He suggest five possible reasons for the reporting of excessively large cartel overcharges.

First, Langenfeld cites with approval a working paper by Boyer and Kotchoni (2011) that examines the issue of how to correct for alleged statistical bias in the Cartel Overcharges data set.[10] They originally corrected for bias by arbitrarily eliminating all overcharges >50%. Obviously, if one eliminates the largest 5% of the overcharges, the mean average overcharge drops significantly.[11] This technique is no more valid than it would be if a researcher decided to try to estimate the mean income in the US by first eliminating the top 5% of earners from the sample. In light of their original decision to omit the top 5% of results, it is curious that Langenfeld embraces their analysis as if it were sound methodologically.

No modern economic model of collusion supports the assumption that there is an upper limit to cartel overcharges; nor is there an upper limit in natural markets for a percentage price-fixing overcharge.[12] Rather, economic models of cartels predict *smooth* increases in overcharges as economic factors such as numbers of cartel members or elasticity of demand vary. Choosing 50% as a break point is completely arbitrary. High overcharge rates are not uncommon in theory or in practice.[13] Under the right conditions, many monopolies achieve overcharges well above 100%, and under the right conditions, cartels with few members mimic the price effects of monopolies.[14]

Second, the 2011 working paper cited by Langenfeld as key support for the idea of dropping large overcharge estimates (Boyer and Kotchoni 2011) was superseded in 2014 by a drastically altered working paper by the same authors which Langenfeld unaccountably does not cite.[15] In the latter version, Boyer and Kotchoni (2014, 3) admit that "…the trimming of the sample at 50% has not been well motivated." After substituting a superior statistical method for trimming the sample, they perform a meta-analysis that virtually replicates the factors responsible for statistical bias discovered by Connor and Bolotova (2006).[16]

Third, on page 6, Langenfeld raises an issue about the appropriate formula to calculate the percentage overcharge: if one has a dollar overcharge, should one divide by the cartel-bloated *actual* affected commerce or by the *but-for* affected sales? The latter approach does result in reporting higher overcharge percentages, if they are positive. We believe that the latter definition comes closest to the legal concept of damages. However, as a matter of practice, lawyers, judges, juries, and most economists opt to report the former percentages, and these are what is reported in Connor (2014a).[17]

Fourth, about 7% of the studies in our sample reported that cartels were ineffective in raising prices. Langenfeld suggests that this low percentage may be due to a bias against publishing such results. We have admitted that a "publication bias" may be present, though its extent and direction are highly speculative. That is, it is possible that authors and publishers may be reluctant to report zero overcharges and that this bias may inflate the reported average.[18] However, publication bias could easily work in the reverse fashion. For example, it could be considered significant or noteworthy if a severely punished cartel was shown not to have raised prices. Further, an economist who testified that even though his or her client allegedly tried to raise prices but failed to do so might have a strong incentive to publish his or her analysis either as a *post hoc* defense of their testimony or as an implicit form of an advertisement directed at potential defendants in price-fixing actions. Regardless, there is no objective way to adjust overcharge averages for such possible bias or to estimate the extent, if any, of this subjective bias.

Fifth, Langenfeld ignores the fact that Connor (2014a) directly addresses the issue of possible publication bias. For example, on pages 299–304, Connor subjectively "grades" the quality of the economic research used to generate very large estimates and finds no research-quality reasons to devalue them.[19] In any case, publication bias is unlikely to affect the soundless of any policy implications of our research because at every turn in those papers we chose conservative (i.e., low-overcharge) options.

Policy Implications Should Depend Upon Overcharges Estimates

Langenfeld critiques three policy analyses by Connor and Lande (2005, 2012, 2015) that depend in part on overcharges estimates derived from the compilation reported in Connor (2014a).[20]

We are puzzled why on page 3 Langenfeld shows so much deference to the 10% overcharge presumption. Connor and Lande (2005) discuss in detail the very thin evidence upon which it was established approximately 30 years ago, and point out the discrepancy between the 10% overcharge presumption enshrined in the 1987 US Sentencing Guidelines[21] and the large body of current evidence showing that median overcharges for all cartels ending since 1973 were much higher: 25% of affected commerce (Connor and Lande 2005, Table 2).

Langenfeld has four specific criticisms with regard to overcharges. First, he again raises the "quality variability" issue. For example, Langenfeld seems more impressed that a respected European economic consultancy picked apart the data in Connor (2014a) by removing many estimates it regarded as derived from low-quality research, and yet, as Langenfeld writes:

> In all, the pattern of overcharges Oxera estimates is similar to what Connor and Lande (2005) and Connor (2014a) find: an overall mean of 22% compared to our overall mean of 25% (Langenfeld 2017, 5–6).

Again, we note that any researcher who eliminates a significant number of studies from our universe should give readers pause: were they selectively removing "unreliable" estimates to bias the results in a particular direction? Perhaps not, but this possibility is a significant downside that our method avoids.

Langenfeld also fails to mention that as a check on the quality of the data in the economic studies, Connor and Lande (2005) presented the above-referenced separate study of what every available final verdict in US courts concluded about cartel overcharges. These final verdicts contained specific determinations of the dollar overcharges and affected commerce of hard-core cartels. The 25 verdicts yielded median overcharges of 22%, which is a very similar average to the median figure of 25% derived from the economic studies analyzed in the same article.[22]

Second, Langenfeld wants to discard estimates for cartels connected with US private price-fixing cases presented in Connor and Lande (2015)[23] because "Almost all of overcharges estimates come from cases involving settlements" (Langenfeld 2017, 4).[24] Langenfeld's concerns about estimates derived from settlements in damages cases are, however, misplaced, and are likely to flow from a misunderstanding of our methodology. It certainly is true that almost every private cartel case settles (as noted, however, we did compile a separate sample of the private cases that yielded final verdicts). But in no case did we rely upon overcharge estimates derived from settlement amounts, as if they are surrogates for the actual amounts of damages.[25] All of the overcharge estimates we used in our analysis came from neutral studies of the affected cartels, not from the settlements themselves.

Langenfeld further elaborates his concerns about cartel settlements, saying that:

> Connor and Lande's calculations, however, do not necessarily show inadequate deterrence. The level of any settlement presumably reflects the strength of the allegations in that case and the probability of the plaintiffs winning. The evidentiary basis for many of these cases may be weak, and the value of the settlement and the estimates of overcharges should be appropriately discounted.

This appears to be a principal reason why Langenfeld is dismissive of our results and believes they should not guide antitrust policies or enforcement. He argues that private cases settle for the "right" amount, a figure determined by the strength of the evidence showing that the cartel raised prices by a particular amount. This could be true, but it is irrelevant for making conclusions about cartel deterrence.

The most important policy issue involving cartels is whether *cumulative* cartel sanctions are at the optimal level.[26] This can best be analyzed by comparing the total sanctions paid by cartels to the actual size of their illegal overcharges divided by the probability the cartel will be detected and sanctioned.[27] The focus of policy makers should be on the actual size of the overcharges as calculated by a neutral analyst, not the overcharges claimed by the alleged victims or agreed to as part of a settlement.

Overcharges estimates by disinterested experts are, of course, the figures that we employed for our optimal deterrence analysis (Connor and Lande 2012). We totaled the actual amounts of the settlements in the private cases, the criminal fines paid, and a surrogate for the (dis)value of prison time and house arrest. Then we compared the total antitrust penalties paid to the overcharges each cartel received. On average, the total sanctions were only

9–21% of their optimal amount. We submit that we have asked and given a good answer to the most important policy question involving cartels – that of optimal deterrence.

Third, Langenfeld complains: "Some overcharge estimates in Lande and Connor's data appear to cover more products and longer time periods than the conspiracies identified by the DOJ."[28] It is true that we sometimes adopted more expansive definitions of affected commerce in class actions, but only if the supervising judge accepted the plaintiffs' claims in her order approving the settlement.[29] This can perhaps be explained by DOJ's tendency to "plea bargain" away products or time periods. Again, Langenfeld presents no evidence that this happens systematically or that, if it does, how it might bias our results.

Fourth, Langenfeld asserts that cartel fines should be based upon the size of the overcharges or monopoly profits achieved by *each cartel*, rather than an overall assessment as to how high cartels raise prices on average. In effect, Langenfeld rejects the use of fining guidelines by antitrust authorities, which are designed both for administrative convenience and for the goal of *general* deterrence. Most of the world's antitrust authorities aim for general not specific deterrence, and their cartel-fining guidelines use affected commerce of a participant in the authority's jurisdiction as a surrogate for the harm caused to customers of cartels (OECD 2016).

Langenfeld's suggestion to employ actual monopoly profits is inconsistent with the goal of the general optimal deterrence of cartels, which is based upon the expectations of potential price fixers, not the actual impacts of their cartels. It would seriously undermine the relatively clear signal that the current level of sanctions sends to prospective cartelists. Implementing a regime of specific deterrence is burdensome and generally impractical: it would require prosecutors to incur heavy costs to calculate the magnitude of cartel overcharges for each defendant, something they currently do not do. In effect, this approach would reduce the number of cartel cases initiated and lower the rate of successful convictions, further undermining the ideal of optimal deterrence.

Replication of Experiments

Langenfeld ends his essay with the counsel of perfection: an appeal to replicate the surveyed "experiments." Of course, one may argue whether the typical empirical work of economists may be classified as experiments in the classical sense because testing in the bench sciences ideally requires a comparison of a control group and a treatment group.[30] In our own experience, obtaining the underlying data used in social-science publications more than ten years old is rarely successful, and >90% of the studies surveyed are, as of this writing, more than a decade old. His suggestion about replication is impractical.

A Contribution to Legal-Economic Scholarship

Amassing the world's biggest data set on any phenomenon is in itself a pointless exercise.[31] The ultimate aim was to facilitate testing IO theories, models of collusive conduct, and assessment of antitrust enforcement. Assembling a large data set enables scholars to demonstrate to the antitrust legal community that the statistical approaches that are second nature to industrial economists could provide new and valuable insights into the policy making that should underlay the logic of competition, competition laws, and the applicable remedies.[32] We believe that our scholarship on cartel overcharges has withstood the tests of time.[33] We further submit that our policy suggestions are well grounded in standard optimal-deterrence principles and the current state of knowledge as to empirical facts about cartel effects. Nearly all of Langenfeld's assertions about the shortcomings of the overcharges data set have previously been acknowledged by the present authors as being theoretically possible, but none has been shown by Langenfeld to be so empirically significant that the competition-policy implications of our study are likely to be erroneous.

Notes

1. Langenfeld (2017) (hereinafter "Langenfeld") is cited in the References below. Half of Langenfeld's article is concerned with assessing important research by Professor John Kwoka on mergers, but here we focus only on the cartel-overcharge topics.
2. It is curious to note that James Langenfeld was an editor of the peer-reviewed *Review of Law and Economics* when Connor (2014a) was reviewed. Connor (2014a) is a shorter, less detailed version of a companion working paper (Connor 2014b). A similar predecessor version, Connor (2007), was published by a different editor and referees.
3. However, all estimates met the tests of scholarly seriousness and disinterestedness. Hallmarks of seriousness are the levels of detail mastered by the author, the use of appropriate methods of analysis, and a balanced presentation of the results.
4. Langenfeld seems to rather dismissive of "ancient" cartels of the type habitually studied by Suslow and Levenstein, whose work he seems to respect.
5. Connor and Lande (2005, 551–559).
6. Langenfeld incorrectly states the number to be 16.
7. This working paper is the third edition of postings of the raw overcharges data; the observational details require appendix tables that take up 153 of the paper's 316 pages. Moreover, Connor (2014a, 38–42, 49–50, 51–52) spends several pages in self-critiques on the same issues raised by Langenfeld, namely, variable quality of the estimates, the motives of various types of authors and publishers in choosing to write about overcharges, the reliability of alternative methods of overcharge computations, the unexpectedly small number of zero observations, why central tendency is reported using medians, and the effects of very high overcharges on asymmetry.
8. Use of the spreadsheet is gained through sale, thus satisfying a "market test," or through a gratis transfer. As several times in the past, I again invite interested researchers to contact the author should they have a need for these data.
9. One of our referees wrote, "It is an oxymoron to say that there are many outliers. By definition, an outlier is rare." We agree.
10. This is a rather technical point that arises from statistical science. Such bias can arise when the frequency of the data set is not normally distributed (i.e., graphs of a data set do not conform to the "bell-shaped curve" seen in statistics' textbooks. Wooldridge (2009, 126). Without a normally distributed data set, statistical estimates from some types of econometric models can be distorted and will not produce accurate conclusions. Similarly, extreme observations can sometimes prove to be outliers (data points that are drawn from a different population the one of interest) and that systematically distort tests. For example, if the experiment is designed to investigate only *effective* cartels, then omitting all zero overcharges is a wise decision.
11. However, Langenfeld is incorrect in suggesting that dropping large overcharges *before* performing an ordinary least squares regression model can be justified by the science of statistics. Outliers can only be properly identified using techniques that require applying an econometric model first and *then* looking for outliers that are not well predicted by the model

employed. Woolridge (2009, 325–331). That is, outliers should not be initially identified by their distance from the sample mean, but rather by their distance (conventionally two or three standard deviations) from the regression plane.

12. The formula for overcharges divides the change in market price due to collusion by the but-for price. A but-for price can be exceedingly small for high-tech services. For example, studies have shown that the marginal cost for a debit-card transaction in the 2000s approaches zero, which implies an infinite overcharge for banks charging a few pennies for each use of a debit card. We note that most economists are schooled in measuring market power with the Lerner Index, which does have an upper bound of 1.0 or 100%. The overcharge measure of market power is a creature of antitrust-law scholarship, with which few economists are familiar.
13. Numbers of firms matter. In their leading undergraduate IO textbook, Carlton and Perloff (2005, 165) provide an empirical example of oligopoly pricing. In a market for homogeneous goods and linear inelastic demand (–38.9 at the competitive price), ten identical Cournot firms achieve an overcharge of 35%, and a duopoly gets 86% above the competitive price. Cournot firms do not communicate; under cartel conditions, i.e., when communication is permitted, the duopoly overcharge is 129%.
14. See Tirole (1988) for a survey of game-theoretic models that apply to monopolies and oligopolies. The major conditions that affect the ability cartels to achieve positive overcharges include the shapes of the marginal cost and demand curves, product homogeneity, inelastic demand, storage conditions, participants' planning horizons and degree of cooperation, and blockaded industry entry.
15. This 2014 working paper shortly thereafter reappeared in a slightly revised version as a peer-reviewed journal article (Boyer and Kotchoni 2015). It is possible that this latter version was not available to Langenfeld when he began to write his critique.
16. The bias-corrected median overcharge for all cartels ending after 1973 is 17.4% (domestic cartels 15.0% and international 21.1%). If correct, these medians are lower than those we reported, but they are still well above the US presumptive 10% level.
17. In any case, when the sources report a percentage, we have no way of determining which method was employed. Similarly, several authors presented Lerner Indexes as measures of the market power of cartels, which are always percentages lower than or equal to an overcharges percentage. As Langenfeld notes, in recent years, to be conservative, Connor has not converted these indexes to overcharges.
18. Connor (2014a) mentions this limitation.
19. Connor (2014b, 72–76) also anticipates this issue by examining controlled laboratory market experiments that ought to be free of publication bias. We recognize that not all economists are convinced that market experiments are good replicas of natural markets. Nevertheless, the outcomes of controlled laboratory cartel experiments broadly support the pattern of overcharges reported in Connor (2014a).
20. In each case, the authors employed the latest available version of the overcharges spreadsheet, which grew over time, or its companion data set, Private International Cartels. In the 2005 article, 674 worldwide episodic estimates up to 2004 were employed, while the 2015 article focused on just 71 US-fined cartels.
21. Because as a group *international* cartels generated markedly higher overcharges (around a 35% median), the authors proposed that competition-law authorities ought to consider such collusion an aggravating factor in their fining guidelines. Furthermore, we note that the United States' 10% figure is rapidly becoming obsolete. The European Commission since late 2006 has used 15–30% of affected sales as its "starting point" for computing cartel fines. Many other antitrust authorities have raised their presumed or starting points well above 10% in the past decade. Also, the fact that Langenfeld supports the notion that cartelized markets have elastic demands is contrary to the vast body of cartel theory and studies.
22. Langenfeld also omits mention of a section of Connor (2014a, 75–80) that examines an interesting subset of overcharge estimates developed from trial records, plea agreements, or the reports of competition commissions. Connor (2014, table 14) contains an analysis of a subset of episodic overcharges estimates that directly flow from 485 cartel decisions of courts or other antitrust authorities in 36 jurisdictions. Most of these estimates are studies of economists (none with known interests in the cases) who used dates of collusion or affected commerce reported in the decisions to calculate an overcharge estimate. While a minority of the decisions contained sufficient temporal price data, most overcharges estimates required collecting prices from market transactions. This analysis of legal decisions concludes that their median

overcharge is 20.0% and the mean is 40.8% (Connor 2014a, 313). Both of these averages are only a few percentage points lower than the full sample averages.

23. These estimates comprise only 13% of the cartels in Connor (2014a, table 2).
24. He repeats this assertion on page 7: "…the observations in their data are virtually all based on settlements."
25. Referring to Connor and Lande (2015), a study of private recoveries from US cartels, on page 5, it states that "Almost all of the overcharges estimates come from cases involving settlements." On page 7, it is reiterated that "…the observations in their data are virtually all based on settlements."
26. That is, we interpret optimal deterrence theory to require empirical studies to sum all the monetary penalties imposed and all penalties that have monetary equivalents.
27. This standard formulation is of course subject to a large number of qualifications and caveats (Connor and Lande 2005, 516).
28. If private plaintiffs accepted the DOJ's concept of affected commerce, then we used the longest of the time periods in any of the cartelists' posted guilty plea agreements, which is a sensible protocol in cartel studies.
29. Plaintiffs in follow-on class-action suits can either accept the product mix and collusive period cited in a guilty plea, or they have the burden of proof in basing a settlement on a broader market definition. Market scope is not nearly so contentious an issue in damages suits as is the size of the overcharge rate.
30. Other branches of science, such as astrophysics or paleontology, do not follow this rubric; these branches are satisfied to apply predictability as the arbiter of truth, just like economics.
31. Langenfeld errs in stating that that the most recent edition of the overcharges data base includes "[more than] 2000 studies and judicial decisions…" (Langenfeld 2017, 4). Rather, Connor (2014a, abstract) cites "…more than 700…" such *sources*. Langenfeld must be referring to the >2000 quantitative *estimates* of overcharges.
32. For example, Connor and a student published the first meta-analysis in the field of industrial-organization economics precisely to test for systematic sources of quality bias in overcharges estimates (Connor and Bolotova 2006).
33. We note that the four articles scrutinized by Langenfeld are heavily cited by scholars. Including posted working papers, on 8th May 2017, Google Scholar had located 432 citations, and there were 5595 downloads on ssrn.com. Moreover, dozens of peer-reviewed journal papers in economics and finance have incorporated our donated cartel overcharges' data into their analyses (Connor 2016, 28–30).

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