Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

KWOKA'S *MERGERS, MERGER CONTROL, AND REMEDIES*: REJOINDER TO KWOKA ☆

Michael Vita

ABSTRACT

John Kwoka's Mergers, Merger Control, and Remedies *is a meta-analysis of "retrospective" academic studies of consummated mergers and other horizontal arrangements. Based on this meta-analysis, Kwoka strongly criticizes federal enforcement policies, claiming that the agencies permit far too many anticompetitive mergers to go unchallenged, and are far too willing to accept remedies that fail to prevent a significant loss of competition. Kwoka claims further that this excessive leniency is the culmination of a trend reflecting deliberate policy choices made over the last several decades.*

In a forthcoming critique, Vita and Osinski challenge Kwoka's analysis and his conclusions, identifying serious flaws in the size, construction, and composition of his sample, and in the statistical analysis of the data drawn from that sample. In a published response to Vita and Osinski, Professor

☆ The views expressed here are the author's, not those of the Federal Trade Commission or any individual Commissioner.

Healthcare Antitrust, Settlements, and the Federal Trade Commission
Research in Law and Economics, Volume 28, 433–444
Copyright © 2018 by Emerald Publishing Limited
All rights of reproduction in any form reserved
ISSN: 0193-5895/doi:10.1108/S0193-589520180000028010

Kwoka offers a number of objections and counter-arguments. In this rejoinder, I respond to Professor Kwoka.

Keywords: Mergers; merger remedies; meta-analysis; antitrust policy; competition policy; antitrust enforcement

JEL classifications: C18; C81; L4; L41

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

INTRODUCTION

John Kwoka's recently published *Mergers, Merger Control, and Remedies* has received considerable attention from antitrust practitioners, academics, and the news media.[1] The book's core is a meta-analysis of "retrospective" academic studies of consummated mergers, joint ventures, and other horizontal arrangements. Kwoka creates a database of the estimated price effects reported in these academic studies, and uses it to address important questions about contemporary federal antitrust enforcement such as Do the agencies successfully identify anticompetitive mergers? Do merger remedies effectively prevent the creation and exercise of market power? Have the agencies applied consistent standards over time for identifying and challenging anticompetitive mergers?

Based on this meta-analysis, Kwoka strongly criticizes federal enforcement policies, claiming (among other things) that the agencies (i.e., the the Federal Trade Commission (FTC) and the Department of Justice (DOJ)) permit far too many anticompetitive mergers to go unchallenged; and that even when they do challenge anticompetitive transactions, they are far too willing to accept negotiated relief (frequently "conduct" rather than "structural" remedies) that fails to prevent a significant loss of competition. Kwoka claims further that this excessive leniency is culmination of a trend reflecting deliberate policy choices made over the last several decades.

In a forthcoming critique, Vita and Osinski challenge Kwoka's analysis and his conclusions, identifying serious flaws in the size, construction, and composition of his sample. Further, they show that Kwoka draws his conclusions using flawed and poorly documented empirical methods that do not permit valid statistical inference. They conclude that Kwoka's sweeping critique of contemporary federal merger enforcement policy is empirically unfounded.

In his published response to Vita and Osinski,[2] Professor Kwoka offers a number of objections and counter-arguments. I address these briefly below.

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

DID VITA AND OSINSKI RELY EXCESSIVELY ON CHAPTER 7, WHILE IGNORING RELEVANT ANALYSES IN OTHER CHAPTERS?

Kwoka (2017) objects that Vita and Osinski focused on Chapter 7 ("Merger Policy and Remedies"), while ignoring other parts of his meta-analysis, such as that contained in Chapter 9 ("Retrospective Studies of Groups of Mergers"). We focused on Chapter 7 precisely because it is the analysis in Chapter 7 (and only the analysis in Chapter 7) that addresses the efficacy of merger remedies negotiated by the FTC and the DOJ. It is in Chapter 7 that one finds conclusions like this (p. 120):

> For mergers in particular, the corresponding price changes were equally stark − 7.05 percent for those subject to divestitures, and fully 16.03 percent for those resolved with conduct/conditions remedies. Thus, while neither type of remedy for competitively problematic transactions seems to have been especially effective in restraining postmerger price increases, conduct remedies were by far the weaker of the two.

Kwoka's protests notwithstanding, the assessment of merger remedy effectiveness is at the core of his book (which is, after all, titled *Mergers, Merger Control, and Remedies*) − he dedicates two of the book's 10 chapters (7 and 8) to it. Many reviewers of his book see its principal message as an indictment of (alleged) merger remedy ineffectiveness.[3] Most merger enforcement takes place through negotiated consents, not litigation, and as evidenced by the two FTC remedy studies (1999 and 2017), assessing the performance of these remedies is an issue of great interest to the antitrust community.

We therefore appropriately focused exclusively on those portions of his book that supported his conclusions on remedy effectiveness, and did not discuss his review of "grouped merger studies" (Chapter 9), because those studies are irrelevant to the analysis of agency actions. As Kwoka himself concedes in Chapter 9 (p. 143), "these grouped-merger studies cannot be used to analyze questions of agency actions and remedies since individual mergers are not identified and policies cannot be matched to them." All of Kwoka's conclusions about the putative ineffectiveness of merger remedies that we quoted − whether those quotations appeared in Chapter 7 or the concluding Chapter 10 − are based entirely on the analysis he presents in Chapter 7. The material in Chapter 9 is completely irrelevant to this discussion.

So let us focus on Kwoka's objections to Vita and Osinski's critique of the analysis and conclusions contained in Chapter 7. These include:

- We criticized Kwoka for including in his sample studies of extremely old cases (e.g., cases from the 1970's) and studies of cases that were not adjudicated by either the FTC or the DOJ (e.g., airline mergers enforced by DOT, and rail mergers enforced by the STB). Kwoka claims that such cases are perfectly appropriate targets for analysis.

What is the ultimate goal of Kwoka's book? If it is offered as a work of antitrust history, then perhaps including these cases in the meta-analysis is justified. But if the principal goal is to critically assess contemporary DOJ and FTC decision-making, and determine what (if anything) these agencies should be doing differently, then the relevance of cases from four decades ago (several of which were adjudicated by other federal agencies such as the Transportation Department) is mystifying.

- We criticized Kwoka for failing to employ standard meta-analytic techniques for computing his average merger effects from the underlying studies, and for failing to provide any estimate of the standard errors of these averages, without which statistical inference is impossible.

By "standard meta-analytic techniques," we meant specifically computing "inverse-variance weighted average" merger price effects (and their associated standard errors) from the underlying studies. This method places more weight on those underlying merger effects that are estimated (relatively) precisely, and less weight on those estimated effects that are not.

In response, Kwoka claims that while this technique might be standard and appropriate for meta-analyses conducted in disciplines such as medicine and physics, it is unsuitable for (and hence unused in) economics. Or as he contends, "despite the FTC critique's[4] blithe assertion that use of its procedure is 'straightforward', it is anything but straightforward in economics and the social sciences."

Kwoka's assertion will come as a great surprise to the editors of the numerous economic journals (including the *Journal of Economic Perspectives*, one of the American Economic Association's flagship journals) that have published articles on the methods and substance of meta-analysis, endorsing and adopting the same basic approach we sketched in our critique.[5] These articles are easily found, and all emphasize and reinforce our point that the fundamental building block of a meta-analysis is an inverse-variance-weighted average of the "treatment effects" (i.e., "merger effects," in this context) reported in the underlying studies.

Kwoka disapproves of the meta-analysis text we cited, and claims that he "in fact relied on and cited a different text on meta-analyses, one that is specifically oriented to the social sciences."[6] So, what does his "preferred text" have to say about the issue of basing the meta-analysis on inverse-variance weighted averages of the underlying effects?[7]

> The problem is that every effect size value in such an analysis is not equal with regard to the reliability of the information that it carries. For a given relationship, for instance, an effect size based on a sample of five subjects is not as good as an estimate as one based on a sample of 500 subjects. If we simply average these two values together, the smaller sample contributes as much to the resulting mean as the larger sample despite its much greater sampling error. ***This actually makes the combined estimate worse than if we simple took the one based on the larger sample by itself.***
>
> The way that this problem is handled in meta-analysis is to weight each effect size value by a term that represents its precision … the optimal weights are based on the standard error of the effect size … because a larger standard error corresponds to a less precise effect size

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

> value, the actual weights are computed as the inverse of the squared standard error value – called the inverse variance weight in meta-analysis. Thus, when an effect size statistic is selected for meta-analysis, the formula for computing the associated standard error must also be identified. The coding of each research finding must be designed to yield both the value of the effect size and the inverse variance weight. [emphasis added]

Kwoka may be surprised to find that his "preferred" text actually endorses the identical technique recommended by every other meta-analysis text (including the one we cited); namely, average effects should be computed as inverse-variance-weighted averages.

Notwithstanding Kwoka's assertion (p. 15) that "the weighting technique from Borenstein [i.e., the inverse variance weighting method] … is infeasible for these types of studies," it is easy to find meta-analyses of economic research using precisely the inverse-variance-weighting method described by Borenstein (and Lipsey & Wilson, Kwoka's "preferred text"), and which is characterized in those sources as "best practice." A few recent examples include:

- the summary effect size is the weighted mean of the respective effect sizes … the weight assigned to each study is the sum of the inverse of the variance of its effect size and the between-study variance of the effect sizes. (Arnold, Rathgeber, & Stöckl, 2014, p. 446)
- we estimate (1) by [weighted least squares], using the inverse of the error term variances, i.e., the inverse of the squared standard error of the parameter estimate. (Lichter, Peichl, & Siegloch, 2015)
- In practice, meta-analysts [use] … weighted least squares to gain efficiency, and they require that specification (3) be divided by [the standard error], the measure of heteroscedasticity … [this] gives more weight to more precise results, which represents a common approach in meta-analysis. (Rusnak, Havranek, & Horvath, 2013)
- Weighting by the inverse of the variance is standard practice among meta-analysts. (Doucouliagos & Stanley, 2009)
- a random effects model was used to conduct the meta-analysis that was based on mean WTP weighted by the inverse of the variance of each included study. (Trapero-Bertran, Mistry, Shen, & Fox-Rushby, 2013)

Additional studies easily could be listed, but one suspects that by now the reader gets the point: best practice meta-analyses are based on inverse-variance-weighted averages of the effects obtained from the underlying studies. There is an abundance of such meta-analyses in peer-reviewed economics journals.

Kwoka complains that our critique fails to provide him with guidance on some difficult questions that would have arisen had he attempted to implement this method (e.g., what does one do when a given study provides multiple estimates of a merger effect?). Our brief discussion of meta-analyses was not intended to be a comprehensive guide to conducting a meta-analysis (there is a reason why most meta-analysis texts are book-length); rather, our goal was to introduce readers to the topic so that they could understand the extent to which Kwoka departed from well-established best practices. The particular question Kwoka raises is ubiquitous in meta-analyses of economic research (as well as

other disciplines). It is challenging but hardly insoluble, and econometricians have devoted considerable time and attention to addressing it. For example, as Nelson and Kennedy (2009, p. 354) note:

> The previous models assumed that each primary study produced only one estimate of the effect-size. However, due to specification searches and sensitivity analyses, most primary studies in economics produce more than one econometric estimate of the effect-size. Hence, the analyst is faced with the problem of using some or all of the available information.
>
> We recommend that meta-analysts consider employing [generalized least squares] whenever they have reason to believe that effect-size estimates are correlated. Computational difficulties associated with doing this in the meta-analysis context require that practitioners exploit available software. Fortunately, by viewing estimates from the same primary study as hierarchical groups, panels, or clusters, estimation procedures and associated software from the hierarchical, panel data, and clustering literature can be employed.

But put aside Kwoka's failure to use standard meta-analytic techniques to derive his average merger price effects. A far bigger issue, as we noted in our critique, was his failure to compute *any* standard errors for these averages. The *raison d'être* for the quantitative meta-analyses of a literature (as opposed to a more traditional narrative review) is to bring the power of formal statistical analysis to bear on the question of "what does an entire literature have to say about a particular research question?"

The core of such an analysis – indeed, of *any* formal statistical or econometric analysis – is the estimation of some parameter, along with its corresponding standard error. Without both pieces of information, statistical inference is impossible. Without standard errors, the reader cannot infer whether the average price effects reported in, for example, Table 7.9 likely reflect real economic effects or are merely sampling error (i.e., noise). Computing the average of a set of noisy (i.e., "statistically insignificant") estimated merger effects will often yield a noisy, statistically insignificant overall average. The magnitude of the point estimate of this average may well differ from zero (perhaps even substantially so), but depending on the size of the corresponding standard error, the analyst might be unable to reject the null hypothesis that the true average effect is zero.

Or to see this point somewhat differently: suppose a researcher conducted a merger retrospective, but reported only the regression coefficients, but not the corresponding standard errors, and concluded on the basis of the former alone that the merger was unequivocally procompetitive. Would this analysis persuade an audience of professional economists?

Without a measure of the precision of the estimated effect, we think few would be persuaded. However, this is perfectly analogous to what Kwoka did.

For example, in discussing (p. 15) the effect of removing an incorrectly included data point (the *Xidex/Kalvar* merger) in his analysis of divestiture remedies, Kwoka argues that:

> the effect of omitting this one erroneous data point is to reduce the average price effect from all other cases of divestiture from 7.0 percent to 5.6 percent. While this is still quite different

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

from zero – which would be expected if the remedies were altogether effective – my error did result in a regrettable initial overstatement of the effect.

The point is that neither the reader nor Kwoka knows how precise are either of those estimates. Both might be nothing but noise, and their difference might be nothing but more noise.

On this point Kwoka is silent, other than to note (fn. 10) that "some findings are based on small numbers of observations. Some do not permit standard statistical testing." Recall just how small are his samples for his analysis of remedy effectiveness. Even before eliminating the three cases that Kwoka now concedes did not belong in his sample (because they did not actually analyze the impact of the remedy),[8] Kwoka only had six cases with divestiture remedies, and four cases with conduct remedies.

The response to Kwoka's rationalization seems obvious: any sample that is too small to permit "standard statistical testing" also is too small to support his sweeping conclusions in Chapter 7 such as: "while neither type of remedy for competitively problematic transactions seems to have been especially effective in restraining post-merger price increases, conduct remedies were by far the weaker of the two" (p. 120); or "it is clear that agency actions did not preserve or restore the price competition otherwise lost as a result of these mergers" (p. 119).

INTERTEMPORAL CHANGES IN ENFORCEMENT POLICY

Apart from his statistically unsupported conclusion about the ineffectiveness of negotiated remedies, Kwoka also takes issue with our assessment of his statistical analysis of changes in enforcement policy since the 1980s. His response is limited to a technical dispute about which statistical test is most appropriate for testing hypotheses about the equality of proportions (in this particular case, the proportion of merger investigations cleared in the 1980s and the 2000s, respectively).

To recall briefly, all the cases in Kwoka's sample (42 mergers, and 49 cases in total) fall into one of the following (mutually exclusive) categories: (1) challenged; (2) settled by consent; (3) cleared explicitly (i.e., the agency publicly explained its reason for clearing the transaction; or (4) cleared without public explanation. For his statistical analysis, he created a subsample consisting only of categories 1, 2, and 3, and conducted a formal statistical test of the hypothesis that the proportion of all cases *explicitly cleared* in the 1980s was equal to the proportion *explicitly cleared* in the 2000s (a hypothesis he claimed was rejected by his test, leading him to conclude (p. 117) that "it can therefore be reasonably concluded that, based on these studied transactions, the numerical shift toward a more accommodating policy across these decades reflects a bona fide policy change").

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

While we stand by our original conclusions on the technical aspects of his statistical test – given the small size of his sample, he used the wrong statistical test, and he computed the corresponding test statistic incorrectly in the bargain – the choice of which statistical test to use is of secondary importance. Far more important, as we noted explicitly, was Kwoka's (still) unexplained decision to perform his statistical analysis only on a very small and limited subsample of his data – that is, categories (1), (2), and (3) only, instead of all the data – categories (1)–(4).

This choice is baffling – if one is concerned about an increasing tendency of the agencies to close investigations, should not one look at *all* the data on closed investigations by using the larger of these two samples?

It is unproductive to speculate about Kwoka's decision to conduct his analysis only on the smaller limited subsample, but it is worth restating the key observation from our original critique: using all of the data in his sample, the proportion of "cleared" cases in the respective decades is *identical* (50 percent) when one examines "all transaction" (i.e., mergers, joint ventures, and code shares); and almost identical in the sample of mergers only (5/10 vs 4/7). One can use any statistical test one wishes – the conclusion will be the same: in these data, there is no meaningful difference between the two decades in the proportions of cleared cases.

Kwoka objects (p. 12) that this particular piece of analysis is not the only evidence in his book that bears on the question of possible intertemporal changes in enforcement standards. I agree – there is other evidence in his book that bear on this question of changes in enforcement policy, and we discussed this evidence in some detail (see pp. 20–22 of our critique), concluding that this other evidence also fails to support Kwoka's conclusion. On this point, Kwoka is silent.

WHICH STUDIES SHOULD HAVE BEEN INCLUDED IN KWOKA'S META-ANALYSIS?

I have little to add to our previous discussion of Kwoka's analysis of divestiture and conduct remedies. Once one deletes from his sample the studies that clearly do not belong (i.e., those that he now concedes do not test the effects of the remedies), the remaining number of studies is small, and most report a mix of positive and negative results (as well as a mix of large and small variances, which a properly conducted meta-analysis would have taken into account, but which Kwoka did not). From this large number of estimated merger effects, he distills a very small set of average effects, using a procedure that (he reassures us) is "clearly stated, fully practical, and entirely evenhanded" (p. 15).

Consider first whether his protocol is "clearly stated." The test of whether a research protocol is "clearly stated" is whether another researcher could

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

replicate the study in question using the description provided in the original publication.[9] Readers are invited to review pp. 92–93 of Kwoka's book, where he describes his research procedure, and ask themselves if they could reproduce his estimates based on this loose sketch of a procedure. Most would find it impossible.

What about "entirely evenhanded"? One cannot discuss this claim without revisiting our (and Kwoka's) discussion of Karikari, Agbara, Dezhbakhsh, and El-Osta (2007); the original General Accountability Office (GAO) study (2004) study on which Karikari et al. (2007) was based; and the FTC Bureau of Economics (BE) Staff Technical Report (2004), which reproduced, critiqued, and re-estimated the GAO report (hence Karikari), but which was excluded from (indeed, not even mentioned) in Kwoka.

Let us first dispose of an easily resolved controversy. Kwoka objects (p. 16) to our claim that he included the GAO Report but not the FTC report:

> In another inexplicable passage, the FTC critique asserts that I "cite[s] a study conducted by the GAO (2004), ultimately published as Karikari (2007)." In fact, I never once cite "GAO (2004)" – it is not in the text or references. It is not used in any fashion in MMCR since it (a) was not published in a peer-reviewed journal or in one of the listed working paper series, and (b) had some methodological limitations that might have disqualified it anyway. *Karikari's 2007 study was based on, and in some ways improved on, the original GAO study, and consequently Karikari qualified for inclusion*. The FTC nonetheless claims – in the face of all evidence to the contrary – that I rely on the earlier version. The FTC critique of MMCR in this connection is altogether mistaken and irrelevant.

It is true that Kwoka does not list the 2004 GAO Report in his bibliography. That aside, everything else in this paragraph is false – in particular, the claim that Karikari et al. (2007) "improved upon" the GAO report. As Karikari et al. themselves note (see Table 3, fn iii, p. 52), their reported results are taken from GAO report; and indeed, an inspection of Tables 21, 22, and 23 of the GAO report shows the *identical* (not similar; *identical*) estimated merger effects found in Karikari et al.'s Table 3. It is an odd "improved" estimation procedure indeed that yields identical parameter estimates to the "unimproved" version. Thus, in terms of its estimated merger effects, Karikari et al. (2007) are *identical* to GAO (2004); and therefore any criticism of the latter obviously applies to the former.

The 2004 FTC Bureau of Economics Technical Staff Report contains many trenchant criticisms of the GAO Report (Karikari et al., 2007); it (essentially) reproduces the GAO findings, and shows how they change when alternative (arguably better) econometric practices are used. We did not ask Kwoka to take sides in the debate between the FTC and the GAO, but we contend that it would have been more "evenhanded" to include the FTC's findings in the meta-analysis; at bare minimum, he at least should have alerted his readers to the existence of the FTC Technical Report, and let them decide for themselves whether its criticisms were valid.[10] The power of a critique resides in the strength of its evidence and arguments, not in whether it was published in an academic journal.

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

So why did Kwoka ignore the FTC Bureau of Economics Staff Technical Report? Kwoka claims that he was obediently adhering to his protocol of using only published papers, or those appearing in "respected" working paper series. Moreover, he claims that the FTC's critique of the GAO study was not substantive, and was instead "manufactured" to serve the FTC's "parochial interests" (p. 16). In short, Kwoka claims that the 2004 BE Technical Report was propaganda, not scholarship.

Given this disparaging view of BE's research quality, one then wonders why Kwoka included in his sample the BE Staff Report authored by Schumann, Rogers, and Reitzes (1992)? It does not satisfy his "evenhanded" research protocol, as it was neither published in a peer-reviewed journal, nor included in a "respected" working paper series (whatever that means). Rather, it was an ordinary FTC Bureau of Economics Staff report, produced and reviewed using the same standards as the 2004 FTC Staff Technical Report.

More generally, if Kwoka regards the FTC's Bureau of Economics Staff Reports as inherently untrustworthy, why should *any* of BE's research output be trusted? The danger for Kwoka of carrying his view to its logical conclusion is that without BE's research output – 14 of the 27 studies of individual mergers in his meta-analysis were authored or co-authored by BE economists – there is no Kwoka book. A cynic might infer that Kwoka's gerrymandered standards for deciding which studies to include in his meta-analysis were designed and implemented to serve his own "parochial interests."

A WAY FORWARD

Ultimately, a fundamental problem with Kwoka's book is its utter lack of transparency. While he is clear about which studies were included in his analysis, one hopes that by this point readers are aware that the process by which he took the multiplicity of published merger effects in those studies, and distilled them into a handful of averages, is opaque. Empirical findings and policy conclusions emerging from methodological black boxes are not credible.

Rather than continuing to stridently insist on the intellectual integrity of his research methods and findings, Kwoka would be more credible were he to make public his data and the details of his analysis, so that readers could see exactly how the numbers in his tables were produced, and assess for themselves the robustness of those calculations to alternative empirical choices.[11] In doing so, he merely would be adhering to the standards established by our best peer-reviewed professional journals, as exemplified by the *American Economic Review*:

> It is the policy of the *American Economic Review* to publish papers only if the data used in the analysis are clearly and precisely documented and are readily available to any researcher for purposes of replication. Details of the computations sufficient to permit replication must be provided.

Given his professed reverence for the process of peer review, one expects no less from Professor Kwoka.

NOTES

1. Kwoka (2014).
2. Kwoka (2017).
3. Skitol (2015).
4. Notwithstanding Kwoka's absurd statement in footnote 6 of his response (claiming that our review was a collective "FTC Critique" because we thanked numerous FTC colleagues for comments), that review, like this rejoinder, is not an "FTC Critique" – it is the work of the Vita and Osinski alone.
5. Stanley (2001) and Stanley and Doucouliagos (2012).
6. Kwoka (2017, p, 11).
7. Lipsey and Wilson (2001, p. 36).
8. These cases were Kalvar/Xidex, Scott Graphics/Xidex, and Evanston/Highland Park.
9. See the research standards required of articles submitted to the *American Economic Review*, below.
10. In Stanley et al.'s (2013) reporting guidelines for economics meta-analyses, they strongly urge (p. 392) that the analyst "provide a list of all studies included and a description of why others were excluded."
11. As one reviewer (Skitol, 2015, p. 3) observes, "I would assume, and certainly hope, that economists at both of the enforcement agencies will take the time to satisfy themselves on the validity of Kwoka's econometrics and methodologies."

ACKNOWLEDGMENTS

I thank James Frost, Ginger Jin, Dan Hosken, Alison Oldale, Dave Osinski, Dave Schmidt, Lou Silvia, and Chris Taylor for helpful comments. Remaining errors are mine.

REFERENCES

Arnold, M. M., Rathgeber, A. W., & Stöckl, S. (2014). Determinants of corporate hedging: A (statistical) meta-analysis. *The Quarterly Review of Economics and Finance*, *54*, 443–458.

Doucouliagos, H., & Stanley, T. D. (June 2009). Publication selection bias in minimum-wage research? A meta-regression analysis. *British Journal of Industrial Relations*, *47*(2), 406–428.

Federal Trade Commission. (2004). *Robustness of the results in GAO's 2004 report considering the price effects of mergers and concentration changes in the petroleum industry*. Technical Report. Bureau of Economics Staff.

General Accountability Office. (2004). *Effects of mergers and market concentration in the U.S. Petroleum Industry*. GAO Report No. GAO-04-96.

Karikari, J., Agbara, G., Dezhbakhsh, H., & El-Osta, B. (2007). The impact of mergers in the U.S. Petroleum Industry on wholesale gasoline prices. *Contemporary Economic Policy*, *24*, 46–56.

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)

Kwoka, J. (2014). *Mergers, merger control, and remedies*. Cambridge, MA: MIT Press.
Kwoka, J. (2017). Mergers, merger control, and remedies − A response to the FTC critique. *Social Science Research Network*. Retrieved from https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2947814. Accessed on April 19, 2017. Also forthcoming in a 2018 volume of *Antitrust Law Journal*.
Lichter, A., Peichl, A., & Siegloch, S. (2015). The own-wage elasticity of labor demand: A meta-regression analysis. *European Economic Review*, *80*, 94−119.
Lipsey, M. W., & Wilson, D. B. (2001). *Practical meta-analysis*. Thousand Oaks, CA: Sage.
Nelson, J. P., & Kennedy, P. E. (2009). The use (and abuse) of meta-analysis in environmental and natural resource economics: An assessment. *Environmental & Resource Economics*, *42*(345), 346.
Rusnak, M., Havranek, T., & Horvath, R. (February 2013). How to solve the price puzzle? A meta-analysis. *Journal of Money, Credit & Banking*, *45*(1), 37−70.
Schumann, L., Rogers, R., & Reitzes, J. (1992). *Case studies of horizontal mergers*. FTC Bureau of Economics Staff Report.
Skitol, R. A. (2015). A harsh report card on the merger enforcement process. *The Antitrust Source*, *14*, 1−5.
Stanley, T. D. (2001). Wheat from chaff: Meta-analysis as quantitative literature review. *Journal of Economic Perspectives*, *15*(3), 131−150.
Stanley, T. D., Doucouliagos, H., Giles, M., Heckemeyer, J. H., Johnston, R. J., Laroche, P., Nelson, J. P., Paldam, M., Poot, J., & Pugh, G. (2013). Meta-analysis of economics research reporting guidelines. *Journal of Economic Surveys*, *27*(2), 390−394.
Stanley, T. D., & Doucouliagos, H. (2012). *Meta regression analysis in economics and business*. Oxford: Routledge.
Trapero-Bertran, M., Mistry, H., Shen, J., & Fox-Rushby, J. (2013). A systematic review and meta-analysis of willingness-to-pay values: The case of malaria control interventions. *Health Economics*, *22*, 428−450.
Vita, M., & Osinski, F. D. (forthcoming). John Kwoka's mergers, merger control, and remedies: A critical review. *Antitrust Law Journal*.

Downloaded by Federal Trade Commision, Doctor Michael Vita At 06:53 20 August 2018 (PT)