The World Oil Market is not "One Great Pool:" A Reply to Rodriguez and Williams

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There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy.

— Hamlet, act I, scene V

1. Introduction

It is always gratifying when one's research stimulates others to undertake work in similar areas. As pointed out in my original paper (Weiner, 1991), questions of market definition have received inadequate attention in the energy area, and more contributions are most welcome, as is debate over empirical methodology and interpretation of results. Unfortunately, the paper by Rodriguez and Williams (1993) in the preceding issue of *Energy Studies Review* contains serious flaws, rendering its conclusions of doubtful use for addressing energy policy questions. This brief reply explains why.

The reply is organized as follows. Section 2 examines Rodriguez and Williams's initial presumption, and its relevance to the questions raised in my original paper. Rodriguez and Williams's empirical work is analyzed in section 3. Section 4 offers some interpretations and thoughts about policy, which differ quite strikingly from theirs.

2. Economic vs. Antitrust Markets

My original paper was motivated by short-term policy issues related to supply disruptions and energy security; e.g., effectiveness of the IEA's emergency-sharing mechanism and supplier diversification. In contrast, the philosophy of the US Federal Trade Commission (FTC), employer of Messrs. Rodriguez and Williams, is based on concerns about the consequences of mergers, etc. for monopoly, and thus consumer welfare over the long term. It is therefore hardly surprising that their paper starts with the assumption that antitrust is the relevant metric for measuring the extent of geographic markets.

It is not. For energy security policy decisions, what matters is the *economic market*, i.e., an area wherein prices move together as a result of arbitrage, rather than the *antitrust market*, which reflects the monopoly power of producers serving the area, as assumed by the FTC economists. As demonstrated by Scheffman

and Spiller (1987), economic and antitrust markets need not coincide, nor is one always larger than the other. While indeed an interesting and important area of economic policy, antitrust is hardly the only motivation in heaven and earth for economic analysis.

The econometric approach adopted in their paper, cointegration, is appropriate for the types of long-term market-power issues of concern to the FTC. As demonstrated in the paper, spot prices of the main "marker" crude oils are cointegrated, indicating that *in the long run*, the world oil market is indeed unified, rather than regionalized. This should not be very surprising; we know that the prices of all types of crude oil rose in the 1970s, fell in the 1980s, and roughly speaking, tend to track each other. Given enough time, refineries can be reconfigured to process different types of crude oil, transportation capacity can be added to accommodate changing trade flows, etc.

In contrast, during a crisis, the oil transportation and production capacity that has not been disrupted is typically strained to the limit. Capacity constraints result in marginal-cost curves that are vertical in the short run, even if they are relatively flat in the long run, making antitrust analysis, not to mention the FTC's *Horizontal Merger Guidelines*, irrelevant for crisis policymaking.

The fact that the world market is unified in the long run tells us nothing about the short run, the relevant time-frame for energy security decisions, which is at most a few months. Before my (1991) paper, most economists simply *assumed* à la Adelman (1984) that even in the short term the market is "one great pool." Policymakers tended to hold the diametrically opposite opinion, in pursuing "secure sources of supply" or import diversification. Neither of these views was supported by any empirical evidence, which was provided in my (1991) paper.

3. Empirics: Analytically Correct but Irrelevant

Even if we wish to analyze antitrust, rather than economic, markets (e.g., for deciding about proposed mergers), Rodriguez and Williams's choice of data to analyze raises serious problems for interpretation of their empirical work. The cointegration analysis focuses on the spot prices of the "marker" crude oils, to which the prices of most other crudes are linked through formulae. This approach would be fine if the formulae remained fixed over time, and were the same for all export destinations (up to an adjustment for variation in transport cost).

Alas, the formulae change regularly. Moreover, the *way* in which they change is revealing for the question of market definition. Perhaps more strikingly, the components of the formulae themselves vary across export destinations. This can only be explained if the market is regionalized. In a unified market, differing formulae for different destinations would make no sense, because arbitrage would ensure that prices move together.

Let us examine Mexican Isthmus crude oil, the example discussed in their paper. According to the issue of *Petroleum Intelligence Weekly* (1990) they use, the price of this crude oil for sales to the United States and Europe was linked to those of Alaskan North Slope (ANS) and West Texas Sour (WTS) crude oils (and not to those of Brent and West Texas Intermediate, as incorrectly claimed in their paper). At the time, PEMEX did not employ formula pricing in its sales to East Asia, but later adopted a formula based on the average of Dubai and Oman.

The next month, Mexico **raised** its discount to the average price of WTS and ANS for exports to the United States from $0.10 to $0.25 per barrel, while at the same time it **reduced** its discount (relative to the same average) on sales to Europe from $0.20 to $0.10 per barrel (*Weekly Petroleum Argus*, 1990). This type of pricing decision would make no sense in a unified market. In a regionalized market, in contrast, such a decision would be rational if competition were intensifying in the first market and easing in the second.

Another example from the same page of *PIW* is also illuminating. Saudi Light, the world's highest-volume crude oil, is sold to Western Hemisphere customers at prices linked to ANS, to European customers at prices linked to Brent, and to East Asian customers at prices linked to the same average of Dubai and Oman used by Mexico. All of this is anecdotal evidence that would be hard to explain if the world oil market really were "one great pool," but consistent with the statistical evidence of regionalization presented in my earlier paper.

Of course, all of these subtleties are missed if one merely analyzes the spot prices of the marker crudes. Moreover, analyzing spot prices itself raises additional problems of interpretation. The "spot prices" used by Rodriguez and Williams, which are compiled by the US Energy Information Administration from trade press reports, are actually "assessments," i.e., averages of daily bid and ask prices quoted by dealers, rather than prices of actual transactions. Because the spot market is very thin, these assessments may not be closely related to prices in actual transactions.[1] Even worse, the bid/ask prices may be quoted in relation to other marker crudes (e.g., Dubai at Brent less $1.50/bbl), resulting in findings of "relationships" among them that are artifacts of the price-reporting process. In contrast, the data used in my original analysis are based on prices prevailing in actual transactions.

Furthermore, even if Rodriguez and Williams had used spot prices of actual transactions in their analysis, it is unclear how representative they would be of the oil market, and thus how

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1/ Verleger (forthcoming) estimates spot trade at three–four transactions per day for Brent, one transaction per week for Dubai and two–three transactions per week for ANS.

relevant their results would be for testing the "one great pool" theory. The 80-85% of the international oil market in 1985 cited by the authors from Razavi (1989) refers not to the spot market alone (as incorrectly claimed in their paper), but also includes the contract trade at spot-linked prices discussed above. Razavi estimates that the spot market alone accounted for 30-35% of the oil trade in 1985. Estimates covering longer periods of time, however, show 1985 as the peak year for the spot market. For example, Japan imported 5.4% of its crude oil on the spot market in 1981, rising to 31.2% in 1985, then falling steadily to 19.6% by 1991 (Koyama, 1993).

As discussed above, spot prices cannot be used as proxies for spot-linked prices when formulae differ across regions. In doing so, the authors miss the very mechanism that makes regionalization possible! The original (1991) paper did not commit this error because the prices used were average of all transactions for oil actually imported, both through spot purchases and ongoing contracts.

The story about the changing spot market points up a more general problem in Rodriguez and Williams's analysis — the 1982-1992 period it covers encompasses vast changes in the way oil has been priced. During the earlier half of their period, spot prices often differed substantially from contract prices. With the advent of formula pricing in 1987, the two series moved much closer together. It would have been illuminating to divide the sample into two subperiods, and then to test each separately.

Rodriguez and Williams's criticism of my empirical work is weakened by their unfamiliarity with the market they are analyzing. They offer the example of arbitraging price differences between the East Coast of the United States and Western Europe not only by European supply, but also by supply from Texas, Canada, North Africa, and South America. Alas, the transport system makes Canadian crude uneconomic on the US East Coast, Texas crude cannot be exported by law, South America already sends virtually all its oil exports to the United States, and North Africa is considerably further from the US than is Western Europe (and hence a costlier supplier). Similarly, the other pairs analyzed in the original paper, like those of Spiller and Huang (1986), make sense in an industry and geographic context.

The authors' discussion of refined products also suffers from their lack of familiarity with the oil market. They criticize the original paper for focusing on crude oil and ignoring refined products. But as can be seen from their own figures, trade in such products is extremely small; for example, in 1990 US imports of crude oil were over ten times greater than those of all refined products combined. Moreover, the fact that refineries typically are utilized near capacity implies little room for expansion of products trade in the short term. In short, refined products are simply not a close substitute for crude oil, to which their prices are only loosely linked, and are properly omitted from the analysis.

4. Policy Implications and Conclusions

Even if Rodriguez and Williams's statistical findings are accepted, their conclusions for policy do not follow. For example, they argue that the policies of focusing on "secure" vs."insecure" import sources and supplier diversification are wise, even in a unified market, citing reduction in risk of "costly supplier strategic behaviour or just bad luck." While at first glance this may appear reasonable, it fails to hold up to closer scrutiny.

In a unified market, bad luck that reduces or eliminates a supplier or customer (e.g., political or military turmoil, accidents, natural disasters) is shared by all market participants, as the trading partners of the disrupted country simply bid up prices and draw supply from other sources (in the case of a supply disruption), or bid down prices to take market-share from other sources (in the case of a demand disruption).

Strategic behaviour is an important consideration in industries where capital is tailored specifically to the particular trading partner, and is not easily movable or salvageable (e.g., natural gas pipelines, minemouth power-generating plants). In such cases, diversifying suppliers or customers can be a means of preventing "opportunistic behaviour," i.e., taking advantage of the vulnerability of one's trading partner after the specific capital investment has been sunk.[2] With crude oil easily transportable by ship, pipe, rail, and truck, it is not an issue here. The study cited by Wolak and Kolstad (1991) of supplier diversification in the Japanese steam coal market applies to industries characterized by long-term contracts. In contrast, sale contracts for crude oil are easily cancelled by either party if prices get out of line with the competition.

In light of the difficulties described above, the policy implications of Rodriguez and Williams's analysis, as well as their challenge to the original analysis, are very doubtful. Cointegration analysis is silent on short-term issues. In the long term, the key question for oil prices is the market power of OPEC, which the authors acknowledge that they are unable to address. Ironically, economists who have presumed that market power is what counts for regional market definition ignore perhaps the greatest example of such power in any market.

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2/ This idea is the basis of transaction-cost economics; see Williamson (1975) for the theory, and Hubbard and Weiner (1991) for an application to natural gas.

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