selected on an ad hoc basis, and all it does is to reinforce a preconceived notion.

Monument construction, another variable measuring purchasing power, is also questionable. Van Oss and Slicher found that the construction of churches and monasteries reflected the economic environment of the time. However, that is not a solid reason to include monument construction as a proxy for the purchasing power of the poor. It could be just the opposite. In periods of economic bonanza in the market sector there was more pressure on labor, and therefore the Indian poor had less time to devote to their subsistence plots. This may be the reason why the coefficient of the "monument construction" variable is negative in all the correlations.

Because of the weak theoretical basis for the selection of some variables, it is impossible to interpret the meaning of the coefficients obtained through the various statistical exercises. A statistical correlation does not mean causality (this argument has been abused by the Tobacco Institute, but is nonetheless true); its role is to strengthen a solid argument. The meaning of the correlation is blurred when solid variables chosen on the basis of sound theory are combined with more questionable ones. In such cases, one may wonder, is this the "Volvo effect," or is it causality?

Ouweneel and Bijleveld's main points remain valid. The usefulness of tithes as an index of agrarian production is weakened by the presence of inflation and of the bureaucratic factor. On the other hand, tithe studies at the parish level contain all the data that are missing at the bishopric level and deserve more attention.

IV. ARIJ OUWENEEL AND CATRIEN C. J. H. BIJLEVELD

During the period of evaluation preceding the publication of our article, one of the anonymous readers advised us to change the title. It was too modest. Why not change it, he asked, to "Economic Cycles in Bourbon Mexico: A Reassessment"? Obviously this reader thought that we should give more attention to our discussion of the Real Caja data and their relationship with other variables. Apparently, our commentators have had similar thoughts. John Coatsworth even thinks that our critique on the *recaudación del diezmo líquido* is trivial. We have the impression that, in a way, both Coatsworth and Brading continue their debate on the use of Real Caja data and the possibilities of deflating the series of silver produc-

tion, expressed in the articles cited in the opening part of our critique.[1] Nevertheless, to criticize the use of tithe data in cash really was the basic aim of writing our piece, because it *is* being used as a reliable indicator of real agricultural output. First Claude Morin and later Juan Carlos Garavaglia and Juan Carlos Grosso introduced the tithe in cash, without much criticism, in the economic history of Bourbon Mexico, relating it to silver production.[2] The latter even concluded, after referring to a graph which depicted the curves of both silver production and the Michoacán tithe (seeing the second of these once again as a clear reflection of agrarian production): "Ante estas dos gráficas, cualquier discusión acerca del problema que nos ocupa ahora es ociosa si no coloca como un tema central la minería novohispana del XVIII."[3]

Repeating the words of Lindo Fuentes in his 1980 review of the Oaxacan tithe data did not seem sufficient. Apart from confirming that the tithe in cash is not a reliable indicator of agrarian output, we wanted to find out what could explain the similar trend in the curves of the tithe in cash, the output of silver production, and the curve of the Real Caja data. Our answer, obviously, would repeat the arguments expressed in several studies on the late eighteenth-century economy published recently which spoke of inflation. So far, nothing new. But, in applying multivariate research methods, the main objective became to highlight the background of the inflationary development. Was the trend of tithe data related to this development, or—as might be inferred from recent essays on the Real Caja data—by what we have called the *bureaucratic component*? Our analyses tended to confirm the first option, leaving the bureaucratic component as a bias of the Real Caja data.

In the comments there are a number of minor misconceptions and reading errors to which we will not respond. For example, Héctor Lindo Fuentes supposes that we discussed a correlation between the tithe data and the bureaucratic component and that we used the relative deprivation theory of Gurr. Coatsworth argues we used correlations among the tithe series to indicate the existence of a colonywide market for basic commodi-

1. John Coatsworth, "The Limits of Colonial Absolutism: The State in Eighteenth-Century Mexico," in *Essays in the Political, Economic and Social History of Colonial Latin America*, Karen Spalding, ed. (Newark, DE, 1982), 25–51; followed by David A. Brading, "Facts and Figments in Bourbon Mexico," *Bulletin of Latin American Research*, 4:1 (1985), 61–64; and Coatsworth, "The Mexican Mining Industry in the Eighteenth Century," in *The Economies of Mexico and Peru During the Late Colonial Period, 1760–1810*, Nils Jacobsen and Hans-Jürgen Puhle, eds. (Berlin, 1986), 26–45.

2. Claude Morin, *Michoacán en la Nueva España del siglo XVIII. Crecimiento de una economía colonial* (Mexico City, 1979), 102–121; Juan Carlos Garavaglia and Juan Carlos Grosso, "La región de Puebla/Tlaxcala y la economía novohispana (1670–1821)," *Historia Mexicana*, 35:4 (1986), 549–600.

3. Garavaglia and Grosso, "Región de Puebla/Tlaxcala," 591–592.

ties and to present a discussion of inflation as a colonywide phenomenon. He even assumes that we applied our techniques to confirm the "existence of a market or the absence of a market," and he thinks we linked the presentation of the bureaucratic component with the analysis of rising prices. David Brading suggests that we wanted to substitute computer analyses for "the hard work of the historian's art." All these things we did not do. A number of more serious comments focus on the conceptual background of our analyses and on the introduction of two European historical concepts. Some further clarification is obviously needed here.

A large part of the commentary seems to center around the issue of spurious correlations. This is most clearly present in the comments of Lindo Fuentes. He argues that the correlation between liberal ideas and ownership of Volvos is well established, but that data on the sales of these cars cannot predict the outcome of political elections. This is the traditional historians' comment on correlation analysis: here quantities might have been involved that cannot be compared theoretically. A correlation can never prove causality; a correlation is a correlation is a correlation, and we wholeheartedly agree. As we stated in Appendix II of the article, a correlation is an index of simultaneous occurring of events, and of nothing more (and nothing less) than that. This simply follows from the formula of the correlation coefficient. It is further illustrated by the fact that the correlation between A and B is the same as the correlation between B and A. In canonical correlation analysis the same results are also obtained if the variables/sets are switched. Therefore, this technique can also never prove causality. What it can do, however, is "strengthen a solid argument." This is exactly what happened: the correlations we found with this technique underscored our reasoning. If another model had underscored our data, these correlation coefficients would be hard to explain. Thus, in using these techniques, we were looking for empirical support of our arguments.

A number of misunderstandings about the techniques should be clarified. Any elementary statistics textbook will tell Coatsworth that canonical correlation analysis does not separate variables within sets (the separation "*into 'inflationary,' 'bureaucratic,' and 'purchasing-power' . . . subgroups*"). For cosmetic reasons we have separated the variables in the tables of our text; submitting the variables in whatever sequence gives exactly the same results. The way in which the canonical correlation analysis maximized the correlation between sets led to the interpretation of the results: we find it hard to discern what conceptual apparatus we built into the data. Also, we would like to emphasize that we have in fact prejudiced both the canonical correlation analysis and the linear dynamic system analysis *away* from, rather than *toward*, a solution that corresponds with our hypothesis: when using only three inflationary variables and a total of

nine bureaucratic and purchasing-power variables, the analysis technique would have improved the fit (and the canonical correlations) by weighting the nine other variables more heavily than it actually did.

It is rightly remarked that the first linear dynamic systems analysis produced the same result as the canonical correlation analysis. As we are dealing with time-series data, we expected the linear dynamic system to approximate reality better than a cross-sectional model. It is reassuring when a better-suited technique gives one the same results as a less well-suited technique, but one should not conclude that the better technique was therefore superfluous, or that the same will happen in all cases; there are many instances where dynamic analyses give different solutions than cross-sectional analyses. We merely added the canonical correlation analysis to show that anyone could have obtained our results with a well-established old-fashioned technique like this. Because of the time dependency of the latent variable in year t on the latent variable in year t-1, the analysis incorporates the time dependency in the data and the analysis is for that reason nearer to true causal analysis, although of course there is never a cure for misspecification.

In short, instead of simply looking at the curves, or working with ordinary or canonical correlation coefficients, DYNAMALS gave us the opportunity of identifying three variables out of twelve that might have "caused" the trend in tithe income: *tributarios*, silver production, and pulque revenues, and not any other variable. To determine why these variables performed that way is part of, to repeat Brading, the hard work of the historian's art, the critical evaluation of sources (this critical evaluation, by the way, consumes more than a third of our essay). But again, empirical verification of our reasoning is possible with the techniques used, and we found support for our hypothesis that the Real Caja data we have used—and that did not correlate with other data very well—were blurred by a bureaucratic component and that the *conjunto* of *tributarios*, pulque revenues, and silver production may have been part of an inflationary development, identified with Irving Fisher's equation of exchange.

The problem of the correlation between data returns in the question of regions. We introduced a scheme of four regions, based on work of Adriaan van Oss and Bernard Slicher van Bath: Oaxaca, Central Mexico, Michoacán, and Guadalajara.[4] These regions were not part of an *integrated*

4. Most revealing in this respect are A. C. van Oss, "Architectural Activity, Demography and Economic Diversification: Regional Economies of Colonial Mexico," *Jahrbuch und Gesellschaft Lateinamerikas* (hereafter *JbLA*), 16 (1979), 97–145; and B. H. Slicher van Bath, "Dos modelos referidos a la relación entre población y economía en Nueva España y Perú durante la época colonial," in *Empresarios, indios y estado. Perfil de la economía mexicana (siglo XVIII)*, Arij Ouweneel and Cristina Torales Pacheco, eds. (Amsterdam,

commodity market. Coatsworth's two "methodological" points, which he places in italics, are mainly meant to criticize this scheme. We would like to question his "methodological" points. First, he states that if there is a positive correlation between price series of different regions, we may conclude that an integrated market existed. Next, he states that there could be a colonywide market for commodities and foodstuffs even when such a correlation cannot be found. If we understand him well, we must conclude that, whatever the value of the correlation that is found, an integrated commodity market existed and that regional differences were not important. The first remark that needs to be made is that when prices display similar long-run trends, they *are* correlated. The correlation coefficient is the very index of the similarity of those long-run trends, and Coatsworth is invited to produce two series of data that display similar long-run trends and are uncorrelated. When two events have a correlation, there is no reason to assume that they are functionally related. Here the Volvos and the liberals return. Mexican maize prices could have been influenced by common weather conditions and international warfare, instead of by colonywide economic growth, stagnation, or inflation. When two events are uncorrelated, as those of Mexico City and the Bajío, we may assume that there is *no* functional relation, although for longitudinal data caution is required: the interval of measurement might be unsuited to reveal the dependency (the maize prices of the Bajío, for example, might have followed the Mexico City prices with an interval of several months or years —but we have checked that in this case).

However, we never used statistical analyses to measure the integration of markets, nor did we argue that high correlations among the tithe series could indicate the existence of a colonywide market for basic commodities. As is affirmed by Coatsworth, in Mexican historiography the necessary data for measuring such integration are not available. Our reasoning was qualitative, based on the intensification of the economy, along a Von Thünen model. Several criteria were used: population density, the road system, the formation of cities and towns, the building of religious monuments, the production of agricultural and artisanal commodities, etc. In Slicher van Bath's model 62 economic activities were included.[5] As can be seen in the surviving documents of the repartimiento trade, not only the

1988), 15–44, partly based on a book-length study, *Bevolking en economie in Nieuw-Spanje (ca. 1570–1800)* (Amsterdam, 1981). Ouweneel's work is an extended elaboration of this, see his *Onderbroken groei in Anáhuac. De ecologische achtergrond van ontwikkeling en armoede op het platteland van Centraal-Mexico (1730–1810)* (Amsterdam, 1989), 23–33, 47–66.

5. See Slicher van Bath, "Dos modelos," 27, Diagrama 1: "Concentración de población y actividades económicas en Nueva España, ap. 1743 y 1800."

maize markets were highly localized, as the thousands of mules carrying goods across the country mainly remained *within* the four regional entities. Therefore, we identified domestic textiles, livestock, sheep, tobacco, and sugar as part of a kind of caravan system, to stress the *intra*regional trade.[6] All this was necessary to limit ourselves to only *one* of these regions: Central Mexico. The conclusions of our essay are thus valid only for this region. Economic—or demographic—development in Michoacán, Oaxaca, or Guadalajara is not discussed in our article, and might have followed a different course (see Brading's analysis, or that of Rabell referred to in the comments).

Of course, the exact boundaries of the regional entities are open to research. That there was considerable overlap between the valley around the city of Valladolid, the Bajío, and the hinterland of Guadalajara is correct. There was also some overlap between the eastern Bajío and the valley of Toluca in Central Mexico. But this does not disqualify the Von Thünen criteria to distinguish regions. In fact, because of population growth, regions tended to unite, and, therefore, we identified the hinterland of Valladolid and the Bajío as one region, and Guadalajara, which was not united to this complex, as a separate one. Because such criteria provide a way of looking at data, they have a heuristic value of considerable importance: to document intraregional trade and production for local and intraregional markets, instead of stressing interregional trade and production for interregional markets. This intraregional accent must not be misunderstood, however. We never can nor will state that interregional trade was unimportant. Even in the Sahara a flourishing interregional trade existed (and still does).

As Europeans we are well acquainted with the theory of protoindustrialization, now founded on dozens of monographs, but our commentators are obviously not: they link protoindustry with industrialization. Coatsworth discusses elements like increased productivity and economic modernization, and Brading mentions the *obrajes* in Querétaro and the coming of the tobacco "factories." There are no theoretical arguments that link protoindustrialization with these elements. In fact, theory suggests that economic modernization and "factories" like *obrajes* had, in prin-

6. This point is extensively documented by Ouweneel, *Onderbroken groei in Anáhuac*, 108–148. See also Horst Pietschmann, "Agricultura e industria rural indígena en el México de la segunda mitad del siglo XVIII," in *Empresarios, indios y estado*, 71–85; and Thomas Gerst, "Die wirtschaftliche Entwicklung Mexikos und das Problem der Proto-Industrialisierung am Ausgang der Kolonialzeit," in *Lateinamerika Studien*, 24 (1988), 7–135. Reviewing recent historical literature, Eric Van Young labeled the late eighteenth-century economy even as a "*mercado desarticulado*"; see his "A modo de conclusión: El siglo paradójico," in *Empresarios, indios y estado*, 206–231.

ciple, nothing to do with it.[7] As is known by now, protoindustry sometimes even *hindered* the development of features usually associated with the industrialization process.

Protoindustry is the putting out of commodity production, mainly of cotton textiles and certain artisan handicrafts, by merchants from urban centers into the countryside. The merchants reclaimed the completed articles for finishing and sale. Unlike small-scale cottage industries that produced for subsistence needs and local markets, protoindustries employed large numbers of rural residents, and the goods were sold in intra- and interregional, national, or sometimes even international markets. The rural workers were unlikely to know the putting-out merchants who remained in the towns and cities, for assignments and money were distributed by traveling porters or local merchants, who themselves became mere employees of the urban putting-out merchants.[8] The rural workers can be considered to work for piece rates: the men in periods of little agricultural work, but the women almost on a daily basis. Running through the documentation of the repartimiento trade we find precisely this sequence of putting out, giving credit, and reclaiming the articles. Besides this we can identify local merchants, mostly all in a circle around Mexico City and Antequera (Oaxaca)—sometimes called *repartidores particulares*—and in more remote areas *alcaldes mayores*, who operated as the merchants' local "employees." In Michoacán, with the exception of some villages in the Bajío, and in Guadalajara repartimiento trade hardly existed.[9]

The markets for these Mexican protoindustries were international (cochineal), interregional (cattle, cotton textiles), and, above all, the cities, towns, haciendas, and the *tianguiz* in the villages within the regional entities. These markets were growing during most of the eighteenth century, but collapsed in the last decades of Spanish rule. Several factors played a role, no doubt: harvest failures and rising foodstuffs prices (reducing the purchasing power to buy repartimiento goods), rising and better collected taxes, and the consequences of international warfare. The precise development of protoindustrial growth can only be measured when the precise alcabala data are known. We have our doubts about Brading's statement

7. The best review of the state of research on protoindustry is provided by Gay L. Gullickson, *Spinners and Weavers of Auffay. Rural Industry and the Sexual Division of Labor in a French Village, 1750–1850* (Cambridge, 1986), esp. 38–46.

8. Gullickson, *Spinners and Weavers*, 2.

9. See AGN, Subdelegados 34, SG 1752; Pietschmann, "Alcaldes Mayores, Corregidores und Subdelegados. Zum Problem der Distriktsbeamtenschaft im Vizekönigreich Neuspanien," *JbLA*, 9 (1972), App. 1; Brian Hamnett, *Politics and Trade in Southern Mexico, 1750–1821* (Cambridge, 1971), passim; and Ouweneel, *Onderbroken groei in Anáhuac*, 123–148.

that the alcabala cannot be used for this; in Chalco villages almost a third of the sales-tax revenues came from local commodities sold at the Friday *tianguiz*.

Protoindustries most often occurred as a result of *relative overpopulation*, when the agrarian subsistence base of a certain region grew smaller because of population growth, or because of agrarian crises like harvest failures. If population growth and protoindustrialization go hand in hand, inflation, of course, is not likely to occur. But in a period of growing—or not falling—silver production, when the output of protoindustry falls and demand outgrows supply, it does. The variables reflecting this were suited to be discussed using Fisher's equation. Apart from investigating our hypotheses on inflation, we identified possible indicators of pauperization that were related to the consequences of a situation of relative overpopulation and demand outgrowing supply in the last decades of the colonial period.

A common element in the comments of Lindo Fuentes, Coatsworth, and Brading is their doubt about the usefulness of multivariate statistical techniques: are we not shooting at a mouse with a nuclear missile? What, then, were the benefits of using the linear dynamic systems analyses? First of all, the two-dimensional analyses underscored the well-known relationship of the tithe in cash with silver production, showing that the agrarian variables performed differently in the solutions. We wanted to discourage further use of tithe data in cash, and in our view the results of our analyses mean that the cash tithe is *out*. Second, we found a clustering of variables that requires more research: maize prices, flour prices, *tributarios*, land disputes, and—inversely—monument building. On known theoretical grounds this clustering could be identified as a reflection of the pauperization of the poor. Although this is not expressed in our article, we wanted to encourage the kind of work that shifts the focus of inquiry away from macroeconomics of the Royal Treasury or silver production to the microeconomics of the poor. Third, the *tributos reales* variable turned up in an odd-one-out position, which seemed to be an indication of economic problems in the villages. Letters, reports, and inquiries present in Mexican and Spanish archives form evidence of widespread extension of payments of tribute or even remissions in the years between 1785 and 1821.[10] Fourth, as can be read in the text of the article, we were aware of the bureaucratic bias in the Real Caja data. Linear dynamic systems analysis filtered out this common bias in the treasury data. Even the disputable

10. See Ouweneel, *Onderbroken groei in Anáhuac*, passim, examples based on tribute data found in the AGI and AGN. The results of our analyses inspired the return to the archives for some *qualitative* research; but more of it, above all in a systematic way, is necessary.

inclusion in our analyses of the pulque and *tributos reales* variables proved meaningful. In all, we think that one may conclude that both a "patient, critical exegesis of records" and multivariate analyses can bring the researcher to generalizations on the ebbs and flows of economic activity in the past.