and gunpowder, the lottery, the customs house, and the mint; but each branch, including tributes, had to submit annual statements of receipts. Such information, however, did not go to the central treasury. It was the task of the Tribunal de Cuentas, the court of audit, and not of the *caja matriz*, to frame a statement of the annual revenue and expenditure of New Spain. There is nothing new or obscure about these documents: printed versions can be seen in the *Historia general de real hacienda*, compiled in 1791 by Fabián de Fonseca and Carlos de Urrutia, and in Humboldt's own *Geographical and Political Tables*. Manuscript specimens can be found in the Archivo General de Indias and no doubt also exist in the Archivo General de la Nación in Mexico.[11] In effect, a patient, critical exegesis of fiscal records, all readily available in the main archives, will yield a wealth of material which can be used to interpret and measure the ebbs and flows of economic activity and of bureaucratic competence. But computerized correlation, no matter how canonical, is no substitute for the hard work of the historian's art, the critical analysis of sources.

II. JOHN H. COATSWORTH

Ouweneel and Bijleveld have produced an interesting if somewhat eccentric paper that raises three important issues. The issues are:

(1) Can tithe data expressed in pesos be used as a plausible indicator of agricultural output?

(2) Was there a colonywide market for basic commodities in eighteenth-century New Spain?

(3) What caused the upward trend in domestic prices in the late Bourbon era?

The first of these questions is essentially trivial, since the answer has been known for centuries. The matter was laid to rest, yet again, in Héctor Lindo's 1980 review of the Oaxacan tithe data compiled by Rodolfo Pastor and his colleagues. Ouweneel and Bijleveld cite Lindo and refer as well to Joseph Goy's 1982 methodological essay on European tithe data. They might as well have cited any good introductory economics or business textbook.

The second question is more interesting because it raises a conceptual

11. Fonseca and Urrutia, *Historia general*, I, xxxviii; Humboldt's tables have been printed several times: see Florescano and Isabel Gil, *Descripciones económicas generales de Nueva España, 1784–1817* (Mexico City, 1973), 207–214; also Iturrigaray to Madrid, Aug. 27, 1803 (AGI, México 1617), where the statement for 1799 was enclosed.

problem that, in one form or another, has plagued economic historians of Mexico for some time. The treatment Ouweneel and Bijleveld give to this question is historiographically acute but theoretically deficient.

The two authors do not deny that colonywide markets did function for some commodities, services, and factors of production. They merely argue that such a market for "basic commodities" did not exist in the late eighteenth century. Clearly, they understand that there was a colonywide market (indeed, a world market) for silver coin. Otherwise, it would make no sense to argue that inflation was a colonywide phenomenon, demonstrated (if indirectly and imperfectly) by the high correlation between (most) tithe revenue series collected at diverse and distant locations over long periods of time. A colonywide market also existed for a series of domestic products traded over long distance; our authors cite "sheep, cattle, and repartimiento products (basically textiles like cotton cloth, *jícaras*, cochineal, and cattle and mules for Indians). . . ." They also mention long-distance trade of other manufactured goods produced in the colony and of luxury articles imported from Europe. The authors would even argue, I infer, for the existence of a colonywide (indeed, international) market for wheat flour, else they would not have thought to mention "decreasing export markets (Cuba was lost to sellers from Louisiana) . . ." as part of their explanation for the sharp decline in flour prices in the 1760s.

If we exempt flour from the list of "basic commodities" for which there was no colonywide market, what is left? The authors refer to wheat (from which the flour was made), maize, and other (unspecified) products whose bulk-to-value ratio was so high as to make transportation over long distances prohibitively expensive. This brings me to the key conceptual point, *a common market may exist in a given geographical area even where transport costs are so high as to prevent any transactions from actually taking place.*

Markets are not defined by the geographic space in which transactions *actually* occur, but by the space in which they may *potentially* occur given appropriate price signals. For some purposes, it may be convenient to distinguish between the "market" for maize and the "market" for flour —that is, to define markets as discrete functional entities, one for each commodity, service, or factor. For others, as in this case, it is also important to keep in mind the larger picture. New Spain's diverse regions were tied to each other by a common legal and political framework, by continuous communication and exchange, by a common lingua franca, by common customs and practices, and by common techniques and organization of production. Ouweneel and Bijleveld know this. That is why they occasionally modify their argument to take into account degrees of market "integration" or the "intensity" of trade in certain commodities. Some commodity markets were well integrated and involved intense trading across big spaces, while others were more like "the trans-Saharan caravan system. . . ." Thus, as they correctly argue, the European grain market was more integrated than that of New Spain because of cheap water transport between regions along with the continent's "well-developed infrastructure."

These conceptual observations lead me to two methodological points. First, *correlation analysis may confirm the existence of a market, but cannot be used to confirm the absence of a market.* If the price of a commodity sold in one place is highly correlated with the price of the same commodity in another place, one may conclude that a single market exists. However, if prices in the two distant localities are not correlated, this may be due to the absence of a market uniting the two locations, or it may be due to the existence of a market that is not well communicated. In the latter case, local demand and supply conditions cause price fluctuations in the two locations such that price differentials are usually too small to induce trade between them. Trade only takes place in the infrequent years when harvest failure in one place is matched by relative abundance in another. But local trade between adjacent localities strung out along roads between our distant towns may be taking place all the time. Maize prices in Silao may be correlated with those in Irapuato, those in Irapuato with Salamanca, Salamanca with Querétaro, Querétaro with San Juan del Río, and so on to Mexico City. And this may be occurring even when the statistical tests show low correlations between the end points in the chain.

Second, *commodity prices in distant locations may display similar long-run trends even when they are not highly correlated.* Maize prices in Silao and Mexico City are not well correlated, but they trend upward (or downward) in roughly the same periods because the two towns are affected by the same monetary disturbances in the colonywide money market. The larger point is worth making explicitly. The "price" of maize was merely the rate at which maize was exchanged for silver. That is, the price of maize was necessarily affected not only by supply and demand conditions in the local maize market, but by supply and demand conditions in the colonywide silver market. In sum, it is illogical to argue that there was no colonywide market for basic commodities and, at the same time, observe that basic commodity prices were rising all over the colony at the same time.

So was there a colonywide market for basic commodities? Of course there was. Was it as well integrated as that of Western Europe at the same time? Certainly not.

The third issue raised by Ouweneel and Bijleveld consumes most of their essay. They specify their principal hypothesis succinctly. Inflation in late eighteenth-century New Spain was caused by "population growth (demand), expanding silver production (stock of money), and nascent protoindustrialization (industrial activity and turnover of money). . . ." Unfortunately, this hypothesis is not coherent, nor is it actually tested.

Population growth alone cannot cause inflation. Indeed, population growth accompanied by urban expansion has been held responsible, among other factors, for increasing the size (and through economies of scale in transactions, the efficiency) of markets. Increasing demand from a larger population can fuel inflation only if productivity is falling and supplies become inelastic. Moreover, it is not so clear that Mexico's population growth was accelerating in the late eighteenth century. Rabell and others have concluded that the rate of population growth declined after the 1770s.

Protoindustrialization did not cause inflation. In the first place, there is little evidence that this phenomenon was actually taking place in late Bourbon Mexico. (Nor was it occurring in Europe if, by protoindustrialization, one is referring to a phenomenon linked to later stages of real industrial growth.) Even if the industrial sector of the Mexican economy (however defined) was growing, prices (*ceteris paribus*) should have been falling, not rising, as productivity increased, as they did during the industrial revolution in Europe. Moreover, economic modernization should have reduced the velocity ("turnover") of circulation of the money supply as it did everywhere else, and would thus have exerted an additional downward pressure on prices.

We seem to be left with the money supply, which was increasing at the end of the colonial era. I hesitate, however, to encourage the view that everyone at Chicago takes a similar view of the power of money, so I will add two additional factors which may have exerted some influence. First, agricultural productivity was probably declining, at least after 1780, in much of the colony; this is a view I have already expressed elsewhere. Second, inflationary pressures, especially during periods of international warfare (1778–83, 1793–1815), were probably imported from outside the colony.

In any case, the statistical techniques deployed by Ouweneel and Bijleveld have nothing to do with testing their hypothesis about the causes of inflation. As they introduce these tests, they state that their purpose is "to unmask the bureaucratic component and to prove the inflationary bias of the late colonial economy. This brings us back to the tithe data and to our hypothesis that these [tithe data] do not reflect agrarian production but inflation."

The "bureaucratic component" emphasized by the authors refers to the fact that the fiscal data compiled by TePaske and his collaborators contain various distortions introduced by officials who cooked the books in ways that tend to exaggerate revenues and expenditures, especially after the 1770s. Curiously, the authors do not mention a second distortion at least as serious. Like tithe data, fiscal revenues were also sensitive to inflation. Some taxes (like the alcabala) were ad valorem taxes on commodities; when prices rose, so did revenues, even if production of the taxed commodities remained stagnant or fell. In any case, the authors are at least partially correct when they argue that the raw fiscal data (like the tithe data) cannot be used as indicators of real economic activity.

However, the distorting effects of the "bureaucratic component" may be less serious than those introduced by inflation. For most of the colonial period, double counting and unexplained transfers of funds between accounts make it difficult to extract accurate data on each year's flow of new revenues into the cajas. "Errors" such as these affect many time series. However, as Klein and TePaske have argued, so long as officials do not abruptly change their practices so as to introduce new sources of error, the data may be quite useful for gauging long-term trends (though, I would add, they still need to be corrected for the effects of inflation).

In the case of late eighteenth-century New Spain, the fiscal data lose much of their utility. Not only does inflation accelerate, but accounting procedures change, new taxes and forced loans are introduced, and new cajas are opened in other parts of the colony. Some of these potential sources of distortion may be attenuated by deflating the series (though an entirely appropriate price index is not available), by disaggregating the data (that is, gauging trends on the basis of the income derived from taxes whose rates and objects did not change), and by summing across all the cajas rather than using the Mexico City data alone. Nonetheless, the utility of the fiscal data for making judgments about economic trends clearly diminishes toward the end of the century.

This does not mean, of course, that bureaucratic book cooking produced or even contributed to rising prices in New Spain's late eighteenth-century economy. The fiscal data exaggerate actual income and expenditure, and in this special sense the data are "inflated." But "inflated" figures can be produced even in periods of stable or falling prices. Oeweneel and Bijleveld appear at times to conflate the bureaucratic "inflation" produced by accounting and administrative practices with price inflation in the economy at large.

Finally, bureaucrats can contribute to inflation by expanding the money supply or by issuing bonds (or other tradeable "securities") when they borrow to pay for expenses they cannot cover out of current revenue. They can also dampen inflationary pressures by reducing the money supply or by raising taxes to reduce borrowing or redeem public debt. While the authors mention accounting procedures in defining the concept of a bureaucratic component, the actual indicators they use in their statistical work are data on tax revenues. Tax revenues were increasing in late colonial Mexico, and increasing quantities of silver coins were leaving the colony as a result. Thus, it is likely that what Ouweneel and Bijleveld describe as a bureaucratic component that contributed to raising prices actually served to dampen inflation.

None of this is relevant to the statistical manipulations developed by Ouweneel and Bijleveld, which are elaborated with the goal of proving that the tithe data do not reflect real trends in agricultural output. Since I have already pointed out that this is a trivial issue, I will not belabor the point here. Instead, I will address the question whether the statistical tests used by these authors accomplish the two objectives they specify. That is, do the tests prove that the tithe data are unreliable as indicators of real agricultural output (even if we know the answer already)? And do the tests help to identify the causes of inflation? The answer to both questions is no.

The first of the two tests performed on the data (canonical correlation analysis) may be summarized as follows. Into their computer the authors poured a century's worth of annual data on 14 "variables." The data were first divided by the authors into two groups or "sets." Set one included two tithe series, one from Puebla, the other from Michoacán. Set two contained everything else. The computer rearranged the data *within* each of the sets in such a way as to produce the highest possible "correlation" *between* the two sets. To do this, it assigned weights to each variable. If a variable in set two contributed to improving the set's correlation with set one, it was given a larger weight than a variable which did not contribute so much. After a few more turns, the computer produced a flurry of statistical measurements, including numbers (called "function coefficients" or "loadings") which measure how strongly each variable is correlated with the shape of the rearranged set it belongs to.

These loadings are the numbers reproduced in Table II and used by Ouweneel and Bijleveld for interpretive purposes. But the interpretation of the results depends more on the conceptual apparatus the authors have built into their data than on the loadings themselves. Thus, it is the authors (not the computer) that separated the variables into "inflationary," "bureaucratic," and "purchasing-power" (also occasionally called "agrarian") subgroups. The statistical relations among these subgroups and between any of them and the tithe set have no historical significance in themselves.

This observation applies equally to the results of the more elaborate linear dynamic system approach to which the authors submit the same data. This statistical manipulation, in effect, adds a small complication to the calculation by building in the assumption that the value of each variable in each year is related to its values in past years. This enormously complicates the mathematics, and consumes considerable computer time, but the results turn out to be quite similar (Tables III and IV).

The authors reach one major conclusion from the canonical correlation loadings, that "the tithe series appear to be indicators of inflationary and bureaucratic rather than of agrarian development. . . ." They offer additional observations and conclusions after running their data with the linear dynamic systems model. However, the plausibility of their analysis is determined not by the statistical results themselves but by the extent to which the results make conceptual sense.

Take their first conclusion. The tithe series are "correlated" with years of crisis and silver production (the "inflationary" variables) and with revenues from the pulque tax, the alcabala, and the *novenos reales* (the "bureaucratic variables"). Tithe revenues are less strongly correlated with "agrarian" or "purchasing-power" variables—indigenous revolts, land disputes, and the prices of maize and flour. What does this mean? Very little, since all of the variables, except for tribute income and monument building, were highly intercorrelated for reasons well known to the authors before the statistics were run.

But the conceptual relations among these variables are the real problem. The statistical results show that tithe revenues in Puebla and Michoacán are only moderately correlated with Mexico City maize and flour prices. But to the extent that the tithes in these two bishoprics consisted of maize and wheat, one can be sure that the tithe series were highly correlated with the local prices of these commodities. On the other hand, tithe revenues were highly correlated with silver production. Of course. Rising silver production pushed up prices, so any series sensitive to inflation will be highly correlated with that variable—both tithe and fiscal revenues. And how are revolts and disputes linked conceptually to their (inflationary, bureaucratic, or purchasing-power/agrarian) set mates or to the tithes? In the end, Ouweneel and Bijleveld offer a bit of ad hoc commentary in their concluding pages that is interesting mainly when it ignores the statistical results altogether.

Needless to say, the statistical results have nothing to do with the utility of the tithe series as an indicator of agricultural production. Ouweneel and Bijleveld believe that because their results show that the tithe series is more weakly correlated with what they call "agrarian" variables and more strongly correlated with what they call inflationary and bureaucratic variables, they have demonstrated that the tithe data were more sensitive to inflation and bureaucracy than to agrarian "production." But the opposite is true. Two of their agrarian production variables are the prices of maize and flour, so their results show, if anything, that the tithe series were *less* sensitive to price changes than to other variables.

Nor do the results contribute to understanding the causes of late eighteenth-century Mexican inflation. Ouweneel and Bijleveld do reach the conclusion that "silver production was not the only variable related to this [inflationary] bias," and further that "population growth pushed prices up as commodity production in the end could not match demand." Indeed, they go even further by concluding that "demand-driven inflation" was occurring in late Bourbon Mexico. None of these conclusions can be derived from the statistical tests they describe in their essay.

III. HÉCTOR LINDO-FUENTES

Arij Ouweneel and Catrien C. J. H. Bijleveld question "the usefulness of the tithe data of colonial Mexican bishoprics as an index of agrarian production in New Spain." After reading their article, one is persuaded of the dangers of using tithes as a source. Tithes promise the key to information on agricultural production and fulfill their promise, but they mix valuable information with unwanted clutter. Tithes are like velcro, every change in the economy adheres to them. Each tithe figure is a blend of information on agricultural production, variations in relative prices, inflation, changes in the product mix, and changes in the capacity of the church to collect the tithes. Tithes say plenty about the economy, too much. As Ouweneel and Bijleveld make clear, before drawing conclusions from tithe data, it is necessary to find a methodology to separate the effects of all the variables that affect them. To follow our anachronistic metaphor, it is necessary to clean the velcro.

Ouweneel and Bijleveld are more successful in analyzing the problems of tithes than in developing a methodology to unscramble the puzzle. In the authors' view, two key problems deserve attention: the impact of inflation, and what they call the "bureaucratic component" (changes in the ability of the church to collect tithes). Since the data are incomplete, neither of these problems can be solved with standard methodologies. The orthodox way of dealing with inflation is to use a price index to deflate the data. The bureaucratic component cannot be measured so easily. With