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Sales Taxes and Prices: An Empirical Analysis

Abstract - *We employ a unique data source to examine the incidence of sales taxes. The main idea is to take information on the prices of specific commodities in different U.S. cities and to examine the extent to which differences in tax rates and bases are reflected in prices, controlling for other factors (such as costs). We find a surprising variety of shifting patterns. For some commodities, the after-tax price increases by exactly the amount of the tax, a result consistent with the standard competitive model. However, taxes on other commodities are overshifted—an increase in tax revenue of one dollar per unit increases the price by more than one dollar.*

INTRODUCTION

One of the most fundamental questions in public finance is who bears the burden of taxes—the "incidence of taxation." This question has received a great deal of attention, especially at the theoretical level. However, it seems fair to say that the empirical evidence on incidence is still quite meager. Indeed, there seems to be little evidence, even in the case that is theoretically the easiest—partial equilibrium commodity taxes. Are taxes levied on commodities completely shifted into their prices, or does the incidence also fall on firms? This question is just as important to policymakers as it is to academics. Current debates in Europe about the effects of tax harmonization hinge crucially on the way in which prices relate to taxes. In the United States, recent debates on whether to increase reliance on consumption-based taxes have revealed an intense concern over the distributional effects of such taxes. Technical staffs in both the Administration and the Congress have prepared detailed analyses of how the various taxes would be distributed among income classes. The differences in these technical analyses—which grow into important political disputes—are due in part to differences in assumptions about who would ultimately bear the various taxes. We stress the word "assumptions," because in the absence of empirical evidence, all the technicians can do is assume how the various taxes would be distributed.

In this paper, we employ a unique data source to examine the incidence of sales taxes. The main idea is to take information on the prices of specific commodities in different U.S.

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cities and to examine the extent to which they are affected by taxes, controlling for other factors (such as costs) that also affect prices. The next section discusses some previous work in this area. The third section provides a framework for thinking about how changes in commodity taxes may affect prices, particularly when markets are not competitive. We discuss the data in the fourth section and present the results in the fifth section. A major finding is that there is a surprising variety of shifting patterns. For some comodities, the after-tax price increases by just the amount of the tax, a result consistent with the standard competitive model. However, some taxes are overshifted, which is difficult to reconcile with the assumption of perfect competition. The final section concludes with a summary and discussion of the policy implications of this study.

BACKGROUND

A compendium of the theory of tax incidence can be found in Kotlikoff and Summers (1987). As they note, many factors determine how taxes are shifted in a particular industry, including the responsiveness of supply and demand to changes in price. In addition, recent work has paid much attention to the consequences of market structure. (See, e.g., Besley (1989), Delipalla and Keen (1992), Katz and Rosen (1985), Seade (1985), and Stern (1987).) An important implication of this literature is that in an imperfectly competitive market, varying degrees of shifting are possible in the long run. Indeed, even *overshifting* is a distinct possibility; i.e., the price of the taxed commodity can increase by more than the amount of the tax. These results contrast markedly with those that emerge from a competitive model. With competition, after-tax prices increase by just the amount of the tax if the long-run supply curve is horizontal, and by less than the amount of the tax if the supply curve is upward sloping.

While economists are now in command of a better understanding of the theory of tax incidence, knowledge at the empirical level has not progressed so easily. The government's technical staffs typically assume (1) that shifting is the same for all goods and (2) that shifting is full, i.e., consumers bear the full burden. This has also been the assumption in most academic studies of sales tax incidence, where it is assumed that prices fully reflect taxes, so that the only important empirical question is how these price increases affect members of different income groups. (See, e.g., Pechman and Okner (1974) and Metcalf (1994).) While the full-shifting hypothesis is reasonable in the absence of further evidence, the conclusions reached on the basis of it have the potential to be seriously misleading if it turns out to be incorrect. For example, imagine trying to determine who bears the burden of a set of commodity taxes as in a value-added tax. If there is differential shifting across commodities, then the answer will be quite different than the answer found under the standard assumption.

There have, in fact, been a few empirical studies designed to examine the full-shifting hypothesis.[1] Harris (1987) carefully examines cigarette prices before and after an increase in the federal excise tax on cigarettes in 1983. He finds that the 8-cent per pack tax led to a price increase of 16 cents per pack. In contrast to Harris's case-study approach, several studies apply structural econometric methods to the problem. Examples are Sumner's (1981) and Sullivan's (1985) studies of the effects

[1] These studies all employ microlevel data. Poterba, Rotemberg, and Summers (1986) use macrodata to investigate a question related to but distinct from ours—do direct and indirect taxes have different effects on the price level? They interpret the results as a test of rigidities in nominal prices and do not focus on tax incidence issues. Dornbusch (1987) uses aggregate data to examine the relationship between the prices of imported goods and exchange rates.

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of cigarette taxation and Karp and Perloff's (1989) examination of the effect of taxes on television set prices in Japan. They make assumptions about the functional form of costs and demand in the industry to estimate the underlying parameters that go into the industry's "mark-up" equation. Having done so, they can explicitly calculate the implied relationship between taxes and prices.

We cannot implement this kind of approach here since we have only price data. Instead, we estimate reduced form models. Evidently, this precludes us from drawing precise inferences about market conduct. In this, we follow the important contribution of Poterba (1996), who surveys earlier empirical work going back to the 1930s. He examines quarterly data on tax rates and prices in eight SMSAs over the period 1947–77 for three commodity groups: women and girls' clothing, men and boys' clothing, and personal care items.[2] Poterba estimates reduced form equations and never rejects the view that prices react one-for-one to tax changes.

Our approach is similar in spirit to that of Poterba (1996), although we employ more disaggregated data. He uses Bureau of Labor Statistics (BLS) city-specific consumer price indices. Such data, however, exist for only 28 cities. Moreover, for most commodity groups, one cannot obtain city-specific price indices over periods of more than a decade; hence the need to analyze only three commodity groups, as noted above. Finally, although the BLS characterizes these prices as being "disaggregated," they are really quite broad. "Women and girls' clothing," for example, encompasses a plethora of products. Lumping together all of these commodities makes it difficult to interpret the results, if for no other reason than the weights on the components of such a composite are likely to vary from area to area and across time.[3] In contrast, we study very specific items—a dozen large Grade A eggs or a three-pack of boy's underwear, for example. In addition, our data set has information on 155 cities; with more data, we expect to obtain more precise results.

FRAMEWORK

The U.S. federal system of government provides a natural setting for examining tax shifting, since different states and cities levy different tax rates and use very different tax bases. On the null hypothesis that all industries are competitive and in long-run equilibrium with horizontal supply curves, we should expect to observe that all post-tax prices adjust to reflect only differences in taxes, other things being the same. According to this view, pretax prices in different jurisdictions should reflect only differences in costs of delivering the commodity and taxes. Conversely, to the extent that the after-tax prices in various jurisdictions differ by amounts that are greater or less than the associated taxes, it suggests that this paradigm is inappropriate, and our views on the incidence of taxes must be modified accordingly.

There is a large number of potential models available for thinking about the link between taxes and prices with different market structure, some of which were referred to above. Our analysis is not structural, in that we do not appeal to any particular model in interpreting the results. However, it is useful to lay out a simple model in order to motivate the econometric specification that we adopt.

Consider a firm operating in market i in city j at time t. We assume that the firm

[2] A number of other papers have focused on price variation within cities in order to determine how a neighborhood's prices depend on the incomes of its members. (See MacDonald and Nelson (1991) and Alcaly and Klevorick (1971).)

[3] Further, as noted by Carlton (1986), the use of BLS data has been criticized because these data are not accurate measures of transaction prices.

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in which we are interested chooses a variable x_{ijt}[4] to maximize profits, which is the difference between costs $c^{ij}(x_{ijt}; z_{ijt}, \tau_{ijt})$, and revenues $R^{ij}(x_{ijt}; z_{ijt})$, where z_{ijt} is a vector representing the behavior of other firms in the market and τ_{ijt} is the *ad valorem* tax rate. We assume that the firms choose variables to form a Nash equilibrium and denote the equilibrium values as $(z^*_{ijt}(\tau_{ijt}), x^*_{ijt}(\tau_{ijt}))$. A natural way to write the solution to the maximization problem is:

$$[1] \quad q_{ijt} = \phi_{ijt}[m_{ijt}(1 + \tau_{ijt})]$$

where

q_{ijt} = tax-inclusive price of good i in city j at time t,
ϕ_{ijt} = mark-up on good i in city j at time t, and
m_{ijt} = marginal production cost of good i in city j at time t.

This is the standard formula, which says that price is equal to a mark-up over marginal cost. Equation 1 is not particularly useful for empirical purposes. First, the left-hand side is a tax-inclusive price, which often times we do not observe (in our data, we do not). It will be useful to have an explicit expression for the tax-exclusive price, p_{ijt}:

$$[2] \quad p_{ijt} = \phi_{ijt} m_{ijt}.$$

Second, the mark-up parameter ϕ_{ijt} (and possibly m_{ijt}) is typically a function of the tax, so that equation 2 can be written as:

$$[3] \quad p_{ijt} = f^{ijt}(\tau_{ijt}, \theta_{ijt})$$

where θ_{ijt} are factors that affect the underlying cost of producing the commodity and will typically vary across location and across time.

Our method is to study reduced form relationships of the kind illustrated in equation 3. Let C_{ijt} be those observable variables that may reflect intertemporal and spatial differentials in costs. In addition, we assume that there are unchanging characteristics of the communities themselves that affect costs (e.g., location and climate), as well as changes in the macroeconomic environment that affect the costs of all cities the same way each period. Under these assumptions, we augment the equation with fixed effects for city and for time. Assuming a semilogarithmic specification for equation 3, we obtain:

$$[4] \quad \ln p_{ijt} = \beta_{li}\,\tau_{ijt} + \beta_{2i}C_{ijt} + CITY_{ij} + TIME_{it} + \varepsilon_{ijt}$$

where $CITY_{ij}$ represents the city effects, $TIME_{it}$ represents the time effects (i.e., quarterly dummy variables), and ε_{ijt} is a white noise error.

From the viewpoint of tax incidence, the key parameter is β_{li}. In interpreting its value, it is useful to relate β_{li} back to the question of whether taxes are under- or overshifted into prices, which is a statement about tax-inclusive prices q_{ijt}. Imagine an increase of dx in the tax revenue raised from a particular commodity (i.e., the specific tax equivalent of a given *ad valorem* tax increase). By how much does the tax-inclusive price rise? One can show that:

$$[5] \quad \frac{\partial q_{ijt}}{\partial x} = 1 + \frac{\beta_{li}}{1 + \beta_{li}\tau_{ijt}}.$$

The conventional assumption is that $\beta_{li} = 0$ so that the tax-inclusive price perfectly reflects any taxes levied on it in dollar terms. Assuming that the tax rate, τ_{ijt}, is small relative to one, then we can think of β_{li} as a coefficient of under- or overshifting. With competitive markets

[4] This could be a vector. In the scalar case, it can be interpreted as price or quantity.

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and constant costs, $\beta_{li} = 0$ for all i. In this context, it is important to note our implicit assumption that β_{li} is independent of i—it is the same in every city. This is clearly a restrictive assumption, and below we discuss some possible ways of relaxing it.

A further conceptual issue concerns market dynamics. Our discussion so far has, in effect, focused only on the ultimate or long-run impact of a tax. There are, however, a host of reasons why tax incidence may differ in the long and short runs in competitive and other market structures. These include entry and exit of firms and changes in capacity choices by existing firms. It seems reasonable to allow for the possibility that such effects take place only slowly. More generally, numerous theories suggest that firms' prices will not respond instantaneously to changes in their economic environments. (See, e.g., Ball and Mankiw (1994).) Mindful of these concerns, we also estimate several models that include some dynamic component.

DATA

Price Data

Our price data are from publications issued by the American Chamber of Commerce Researchers Association (ACCRA). ACCRA's objective is to construct quarterly price indices for each of a large group of U.S. cities. The ACCRA data gathering teams are instructed to select establishments and neighborhoods used by a "mid-management executive household." Schoeni (1996) compared an ACCRA-based price index to an index based on BLS data for 23 cities. The two indices agree fairly closely, with a correlation of 0.715.[5] (As Schoeni notes, one would not expect the two indices to correspond exactly because they cover somewhat different commodities and the geographic boundaries of the communities are not quite the same.)

We use the raw data upon which the price indices are based, reported in volumes that are published each quarter. The series is available from 1975 second quarter to the present. However, the set of cities surveyed and the array of commodities whose prices are sampled grow through time. In the end, we chose 12 commodities and 155 cities. The commodities were chosen because the data for them existed over a reasonably long period of time.[6] We converted the prices into real terms by deflating with the consumer price index. They are listed in Table 1 along with the years for which the data exist and summary statistics. The proxies for cross-city variation in costs, detailed below, are available only after the second quarter of 1982, so that our econometric work is based on observations from 1982 second quarter through 1990 third quarter (about 4,200 observations per commodity).

Three aspects of the table are noteworthy. (1) The commodities are narrowly defined. In some cases, we even have specific brand names. (2) The characteristics of some of the commodities change during our time period. The time effects in equation 4 adequately capture such changes.[7] (3) There is substantial spatial

[5] Consistent with this finding, when Card and Krueger (1995) used the ACCRA data in some of their work on the minimum wage, they found that using ACCRA data and the (limited) BLS data on city price indices produces very similar results.

[6] In addition to the commodities in the table, we also collected data on cigarettes, alcohol, gasoline, and tobacco products. However, due to the complex tax regulations governing these items, we do not have results for them at the present time.

[7] Consider, for example, the change in the spin balance from two wheels to one. In effect, this is a one time change in the price of the "commodity" spin balance, which increases the intercept of the regression line in every subsequent quarter. Since each quarter has its own dichotomous variable, this effect is automatically captured.

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TABLE 1
SUMMARY OF PRICE DATA (1982 DOLLARS)

Item	Mean (in 1982 Dollars)	Standard Deviation	Number of Observations	Quarters (Year/Quarter)
Bananas: 1 pound	0.36	0.07	4,626	1982/261990/3
Bread: 24 ounce loaf	0.57	0.12	4,626	1982/261990/3
Big Mac: Quarter Pounder with cheese (McDonalds)	1.26	0.09	4,444	1982/261990/3
Crisco: 3 pound can	2.24	0.28	4,626	1982/261990/3
Eggs: 1 dozen large Grade A	0.78	0.16	4,626	1982/261990/3
Kleenex (facial tissues)				
200 count	0.86	0.07	849	1982/261983/3
175 count	0.85	0.07	3,777	1983/461990/3
Milk: 1/2 gallon carton	1.07	0.13	4,626	1982/261990/3
Monopoly (board game) Parker Brothers, No. 9 edition	8.25	1.11	4,582	1982/261990/3
Shampoo: 11 ounce bottle	2.50	0.32	4,581	1982/261990/3
Soda: 1 liter Coke	1.20	0.25	4,626	1982/261990/3
Spin balance				
2 wheels	10.41	1.54	849	1982/261983/3
1 wheel	4.83	0.71	3,638	1983/461990/3
Underwear (boys): 3 briefs, cotton (lowest price)	3.78	0.68	4,312	1982/261990/3

and temporal variation in prices, as indicated by relatively large standard deviations.

Similarly, we selected the cities mainly on the criterion that they be in the data set for a sufficiently long period.[8] The set of cities that reports to ACCRA varies over time because some local Chambers of Commerce choose not to collect the data in some quarters. We have no reason to believe that this biases the sample. However, the fact that the panel is unbalanced suggests that heteroskedasticity may be an issue in the estimation of equation 4. In all of our estimates, therefore, we report t-statistics calculated from robust (Huber) standard errors.

The ACCRA publications report the average, net-of-tax price for each commodity in each city. The samples used to construct a "representative" price for each commodity in each city are relatively small—the price for each city is generally based on the average of a sample of between 3 and 12 stores. This reduces the signal-to-noise ratio in this variable as a representation of the true mean price in each city. Since the price variable appears on the left-hand side, measurement error of this kind should not bias the results. In particular, even if the samples of stores were "unrepresentative," we can think of no plausible reason that this should induce a correlation between the regression error and any of the right-hand-side variables in equation 4.

Tax Data

Our tax rate variable, τ_{ijt}, includes all sales taxes levied on the commodity (state,

[8] We chose cities that appeared in the data for at least one quarter every year between 1982 and 1990.

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county, and local). We also need information about the tax status of each of the goods in our data set. For example, many jurisdictions subject food to a lower rate of tax or exempt it altogether. Some of the tax information was available in the series *Significant Features of Fiscal Federalism*, published by the Advisory Council on Intergovernmental Relations. However, many of the required county and city tax rates were not included in this series. We obtained the remainder of the tax data from Vertex, Inc., a firm near Philadelphia that provides advice to companies relating to compliance with state and local taxes. In this context, it is important to note that the total tax rate on a commodity is not necessarily the simple sum of the state, county, and local rates. Rather, in a few states, such as New Mexico, the state in effect reduces its component of the total tax rate on a commodity if a given locality decides to levy its own tax on that commodity.

The mean tax rates and standard deviations for different commodity groupings are given in Table 2. The figures suggest that taxes are lowest on repairs and food. The coefficient of variation of taxes is lowest for clothing and greatest for the general tax rate. The ranges of the tax rates are fairly similar for all the cases.

With respect to temporal variation, it will not surprise most readers to know that mean tax rates have been increasing through time. The average general tax rate is close to 4.5 percent at the beginning of the sample period and trends upward to almost 6 percent by the end. However, food and repairs have been largely exempt from this tendency (and average tax rates on food even fell in the early 1980s). Tax rates appear to vary more in the cross section than they do through time. The intertemporal coefficient of variation in mean general tax rates is just 0.11, while the cross-sectional variation in 1988 quarter 1 (a typical quarter) is 0.21. This will be important in our discussion below on some of the issues associated with dynamics. To get a feel for the cross-sectional dispersion in tax rates and its intertemporal variation, consider the histograms in Figure 1, which record the variation in tax rates on two commodities (food and clothing) at two points in time, five years apart. This pictorially reinforces the point that there is variation both in the cross section and in the time series. We will be getting our identification of tax rate effects from intertemporal variation/deviations from city-specific means.

Cost Data

Our model also requires that we account for measurable differences in costs across jurisdiction and time, C_{ijt}. There are no really satisfactory quarterly data on input costs at the community level. However, the ACCRA data contain several variables that may serve at least as rough proxies in this context. Specifically, we attempt to proxy for differences in rental, wage, and energy costs as follows: For rental costs, we use the rental value of a typical two-bedroom apartment. In principle, it would have been desirable to have

TABLE 2
SUMMARY OF TAX RATES

Item	Mean	Standard Deviation	Minimum	Maximum
Food tax rate	0.019	0.028	0	0.0825
Soda tax rate	0.051	0.019	0	0.085
Clothes tax rate	0.052	0.017	0	0.085
Repairs tax rate	0.022	0.028	0	0.0825
General tax rate	0.055	0.012	0	0.085

Notes: The rates in this table and the commodities in our sample correspond as follows: food tax rate applies to milk, eggs, bananas, bread, and Crisco; soda tax rate to soda; clothes tax rate to underwear; repairs tax rate to spin balance; and general tax rate to Big Mac, Monopoly, Kleenex, and shampoo.

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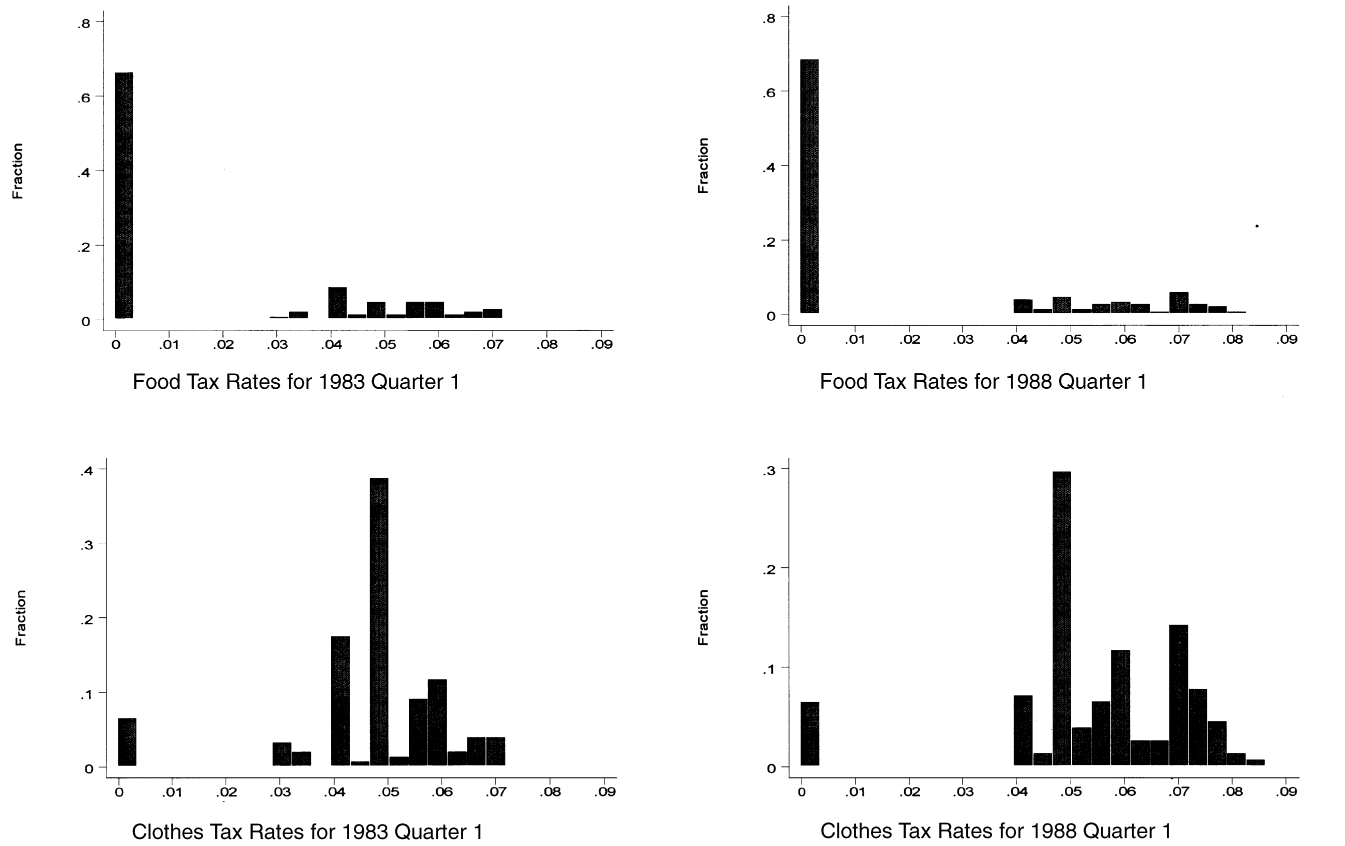
Figure 1. Tax Rates on Food and Clothing



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a measure of commercial rather than residential rents. However, we were unable to obtain a series on this for our cities. For wage costs, we use the minimum labor charge for a home-service call to repair a clothes washing machine. Clearly, this need not be representative of the general wage level, but it may be correlated with it. For energy costs, we used the price of one gallon of unleaded gasoline. Again, there are many other dimensions to energy costs, but we expect this to be a significant component of total costs.[9]

We believe that the inclusion of these three variables together with time effects and city fixed effects should pick up a very substantial fraction of intercity cost variation. While the cost variables are far from ideal, our confidence in them is somewhat bolstered by the fact that, for every commodity we study, whenever the coefficient on one of these cost variables is statistically significant, it is also positive. This is just what one would expect if they are indeed proxying for costs. In any case, if we omit these variables from the analysis, our results are basically unchanged. (For specifics, see the Appendix.) As a further check, we re-estimated the model with some possibly better cost data that are available for a subsample of our cities on an annual basis. This exercise, which is described in greater detail below, also does not affect our substantive findings.

RESULTS

We begin by discussing the estimates of equation 4 and then analyze some alternative specifications to assess the robustness of the results.

Results from the Canonical Specification

To begin, we estimated equation 4 for each of our commodities.[10] This specification includes city effects, time effects, and the three cost variable described above.

The first column in Table 3 shows the number of observations used to estimate the regressions for the associated commodity. (The sample sizes vary somewhat across commodities because some data are missing during certain time periods.) The second column shows the estimate β_{li} for the corresponding commodity and the associated t-statistic. Recall from the third section that in the commonly assumed case of full shifting, β_{li} is zero. We cannot reject the standard model of full shifting for several of our commodities—Big Macs, eggs, Kleenex, Monopoly games, and spin balances. However, more than half of our commodities exhibit overshifting. The coefficients in the bananas, bread, Crisco, milk, shampoo, soda, and boys' underwear equations are all positive and exceed their standard errors by more than a factor of two.

From a quantitative standpoint, how important is overshifting for the commodities that exhibit it? One can obtain an answer by recalling from equation 5 that if the tax rate is relatively small, then β_{li} measures the extent of overshifting. For example, the estimate of β_{li} for bananas is 0.83, suggesting that an increase in the tax rate that is sufficient to raise 10 cents of revenue per pound increases the tax-inclusive price by about 18 cents. Some of the overshifting parameters are less than one, but those for bread, Crisco, shampoo, soda, and boys' underwear exceed one—raising a dime of revenue per unit sold increases the price per unit by more than 20 cents.

[9] The means and standard deviations of the three cost variables are rent: mean = 322, standard deviation = 64; wage: mean = 22.07, standard deviation = 3.58; gas: mean = 0.97, and standard deviation = 0.20.

[10] We also augmented equation 4 with a quadratic term in the tax rate. In a few cases, the quadratic term was statistically significant, but did not affect the substantive conclusions.

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TABLE 3
ESTIMATED SHIFTING PARAMETERS FROM THE CANONICAL MODEL[a]

	(1) Observations	(2) Shifting Parameter
Bananas	4,057	0.828 (2.37)
Bread	4,057	2.42 (4.90)
Big Mac	4,020	–0.0963 (–0.514)
Crisco	4,057	1.03 (6.26)
Eggs	4,057	0.0443 (0.233)
Kleenex	4,158	0.0818 (0.319)
Milk	4,057	0.525 (3.61)
Monopoly	4,158	0.698 (1.66)
Shampoo	4,157	1.042 (3.17)
Soda	4,158	1.29 (4.38)
Spin balance	4,158	–0.0416 (–0.077)
Underwear	3,888	1.51 (3.07)

[a]Column (1) shows the number of observations; column (2) shows β_{li}, the coefficient on the tax rate from equation 4. (All figures in parentheses are *t*-statistics calculated from heteroskedasticity consistent standard errors.) All regressions include city effects; time effects; and measures of real rental, wage, and energy costs.

Making Sense of the Results

For some commodities, our results are consistent with the competitive paradigm. For the others, they are not, and it is natural to wonder if the results are plausible. There are two main ways in which one can make sense of overshifting.

Imperfect Competition

As we stressed in the third section, recent developments in incidence theory in imperfectly competitive markets indicate that overshifting is by no means a pathological phenomenon.[11] There are, of course, many models of imperfect competition. Not all of them are plausible representations of the retail sector. A model with free entry and decreasing average cost seems a sensible starting point for this section. Can overshifting occur in such a model? As shown by Delipalla and Keen (1992), the answer is yes. Indeed, in a conjectural variations model with fixed costs, constant marginal costs of production, entry, and locally constant price elasticity of demand, overshifting *must* occur, at sufficiently low tax rates. While we do not know if these assumptions on parameters are correct in the markets of the commodities we study, they are certainly the kinds of assumptions that economists are comfortable building into their models.

A closely related question is whether it is plausible for the after-tax prices of different commodities in the same store to react very differently to the same change in their tax rates. To form an intuitive basis for understanding this result, think about the standard competitive model. In that model, provided that the supply curve is upward sloping, the effect of a tax upon price depends on the elasticities of the supply and demand curves, and there is no reason to believe that these are the same across commodities. Indeed, in the familiar monopoly model, even if marginal costs are constant, the price response depends on the elasticity of the demand curve—there is no presumption that this elasticity is constant across commodities, and hence there is no presumption that the price responsiveness to tax rates is the same. In the same spirit,

[11] Even in the absence of oligopoly, overshifting is possible in a decreasing cost industry because of scale economies external to the firm. However, most plausible models of scale economies would introduce some kind of imperfect competition.

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Delipalla and Keen (1992) show that the extent of overshifting depends on the elasticity of the slope of the demand curve and the change in marginal cost as output increases. Even for three commodities as "similar" as milk, eggs, and bread (they are often near each other in the grocery store), there is no reason to believe that demand and cost conditions (including elasticities of slopes) are the same.[12] In short, from a theoretical standpoint, neither overshifting nor differential responses to taxes are unexpected phenomena. Rather, they can occur in a variety of models, including ones in which long-run profits are bid to zero.

In the Delipalla–Keen and related models, the producer of the commodity in effect sells it directly to the consumer. Given that many of our commodities are retailed in common outlets, it might be worthwhile to distinguish between market structures at the retail and upstream levels. For commodities whose prices are set in national or global markets, the relevant effect is that coming from the retail market. For commodities that are priced and produced locally, the results we observe could be due to noncompetitive behavior at both levels. This is another reason why differences among commodities might occur. It is tempting to try to rationalize the findings for the various commodities in Table 3 on the basis of guesses about the nature of the various market structures. But without details on the individual markets, this would be a perilous exercise.[13]

Given the potential importance of noncompetitive retail markets in the interpretation of some of our results, it behooves us to ask whether it is reasonable to characterize retail trade as being a noncompetitive industry. It turns out that a number of papers in the industrial organization literature have made just this claim. These papers, which are reviewed in Anderson (1990), examine the relationship between grocery store prices or grocery store profits and market concentration. They find a positive and statistically significant effect and conclude that many local markets are indeed imperfectly competitive. This finding has been questioned by other work, also surveyed in Anderson (1990). The critics argue that the positive correlation between grocery prices and market concentration may be due to higher costs in more concentrated markets, not market power. Our goal is not to assert that one side or the other is correct in this debate. Rather, we want to point out that the claim that local retail markets are imperfectly competitive is taken seriously by industrial organization economists.

Finally, we note that Hall (1988), using an entirely different data set and methodology, also finds that retail trade is not competitive. Using aggregate data on output and labor input changes, he estimates for a variety of industries a parameter that is equal to the ratio of price to marginal cost. Under competition, of course, this ratio is unity; for retail trade, Hall reports a value of 2.355. This is not only consistent with our qualitative finding of noncompetitive behavior, but the quantitative results are remarkably consistent with several of our estimated shifting parameters.

Common Effects on Taxes and Prices

The interpretation of our results has implicitly assumed that the year effects and fixed effects adequately capture changes

[12] Indeed, the literature suggests that the elasticities of demand of these commodities are rather different from each other, let alone the elasticities of the slopes of the demand curves. An estimate of the elasticity of demand for milk is –1.63 (Boehm, 1975); for eggs –0.15 (Tomek and Robinson, 1981); and for bread –0.372 (Mariak and Logan, 1971). Hence, even in the basic textbook model of taxation in short-run competitive markets, we would expect these commodities to be associated with different amounts of tax shifting.

[13] That said, it may be useful to point out that our results are *not* the kind that would be obtained in a competitive model with an upward sloping supply curve.

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in demand across space and time. Suppose, however, that as cities grow, two phenomena occur simultaneously. First, the demand for public spending rises and tax rates increase, and second, the demand for certain commodities increases. To the extent that the commodities are characterized by increasing marginal costs, tax rates and prices rise together. We observe prices rising with tax rates, but this is due to the demand shift, and not the change in the tax rate per se. Hence, it tells us little or nothing about whether overshifting has occurred. Related to this, it is possible that governments increase taxes only during periods of high demand since political resistance will tend to be lower at those times. Again, this could suggest a positive link between taxes and prices, which has nothing to do with incidence.

While it is certainly plausible that sales tax *revenues* will increase with population, it is much less plausible that sales tax *rates* will do so. This notion was confirmed when we analyzed the relationship between tax rates and population density in another data set and found that a doubling of density does not show up even in the fourth decimal place of the tax rate.[14]

Nevertheless, the possibility remains that *some* variable is simultaneously driving prices and tax rates.[15] To investigate this notion, we begin by observing that, if it is correct, then the *ratio* of the price of commodity *i* to the price of some untaxed good should not depend on the tax rate for commodity *i*, *ceteris paribus*. Now it happens that about 65 percent of our observations had no tax on spin balances.[16] We re-estimated each of our basic equations using this subsample with the log of the ratio of the price of the particular good to the price of a spin balance on the left-hand side. In effect, spin balances served as an untaxed numeraire. We found that the commodities that exhibited statistically significant coefficients on the tax rate variable in Table 3 continued to do so.[17] We believe that this constitutes fairly compelling evidence that our results are not being driven by city-specific or time-varying shocks.[18]

[14] We were unable to examine this question using our data because quarterly population data on cities are not available. The data for this exercise contained annual observations on all 48 continental states from 1950 to 1990 and are described in Besley and Case (1995). We measured the state tax rate as sales tax revenues divided by state income. We estimated a regression of this on state fixed effects, year effects, and the population density by state (1,440 observations in total).

[15] Perhaps, for example, prices tend to be high in cities with inelastic demand and sales taxes tend to be higher in cities with inelastic demand because rates are being set according to the Ramsey rule. Or, in the case of a regulated commodity like milk, a state may allow higher milk prices through regulation and in exchange impose higher taxes on it.

[16] A spin balance is a device that spins wheels with tires mounted on them and senses whether they are in dynamic (spinning) balance. It produces readouts that allow the placement of lead weights on the rims that even things out.

[17] See column (1) of Table A.1 in the Appendix for details.

[18] Another possible problem along the same lines stems from the fact that some proportion of the sales tax falls on intermediate purchases of firms. Indeed, Ring (1999) estimates that, on average, about 41 percent of sales tax revenues are from intermediate goods. If such taxes raise the cost of final goods and services, then the tax-induced increases in prices that we observe may have nothing to do with overshifting. More formally, suppose that we can write the cost of production of commodity *i* in city *j* in year *t* as $C_{ijt} = \alpha_{ijt}+\gamma_i\tau_{ijt}$, where γ_i is a parameter that measures the dependence of costs upon (intermediate goods) taxation. If so, the estimate of β_{li} generated by the regression in equation 4 will be biased upward providing that γ_i is positive. Note that by itself the fact that a substantial proportion of state sales taxes are comprised of taxes on intermediate goods tells us little about the magnitude of γ_i. For example, if input prices are set in competitive national markets, then these taxes are borne by input suppliers, and $\gamma_i = 0$. Of course, we do not know the extent to which the inputs are purchased in national as opposed to local markets; further, the national markets might not be competitive. Hence, the possibility that our overshifting results are due to intermediate goods taxation remains a real one.

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As a further test of robustness against the possibility of common shocks, we reran the canonical specification including time-varying demographic, economic, and political variables. Since quarterly, city-specific time-varying regressors are not available, we had to rely on the yearly state level data from Besley and Case (1995). This will, at least partially, control for common influences on prices and taxes at the state level, especially given that a substantial portion of tax variation is at the state level. This led to almost no changes in the basic results.[19]

Alternative Specifications

Dynamics

One possible problem with our canonical specification is that it assumes that the full effect of a change in the tax rate occurs instantaneously. As noted in the third section, however, it might take time for changes in tax rates to become fully incorporated into prices. The most straightforward way to allow for this possibility is to include lagged values of the tax rates in addition to their contemporaneous values. We augmented equation 4 with 20 lagged values of the tax rate, placing no restrictions on the pattern of the lags.

Our initial hope was that this exercise would yield useful information about the time pattern of the response of prices to tax rates. However, this hope was frustrated by the relatively small amount of intertemporal variation in tax rates (see the fourth section, above). This made it impossible to estimate with any precision the coefficients on the lagged tax rates. However, the long-run incidence, which is given by the sum of the coefficients on the lags, can be estimated with some precision. The sums of the lag coefficients are reported in column (1) of Table 4. Qualitatively, the results tend to be in line with those from Table 3—generally, commodities that were characterized by overshifting in the canonical model are also characterized by overshifting in the model with lags. These commodities include bananas, Crisco, milk, and underwear. Similarly, most of the commodities that had insignificant coefficients in Table 3 also have insignificant coefficients in column (1) of Table 4: eggs, Monopoly games, and spin balance. The main differences are in Big Macs and Kleenex (where the coefficients go from insignificant to positive significant) and shampoo and soda (from positive significant to positive insignificant). In no case is there a statistically significant change in signs. We conclude that incorporating some simple dynamics does not seriously affect the substantive finding—a substantial number of commodities exhibit overshifting.

To sharpen these results and be able to say something useful about timing issues requires that one impose some restrictions on the lag structure.[20] The simplest restriction is that the weights on the lagged variables decline geometrically. As is well known (see Maddala (1977)), one can transform an equation with a geometric lag pattern to obtain a specification that

[19] This exercise and the others in this section address the possibility that a common shock is affecting all tax rates and prices in a given jurisdiction. Another possibility is that some shock is affecting the price and the tax rate on a *given commodity* in a jurisdiction. To deal with such a possibility, one would need an instrument that, on a city-by-city basis, is correlated with tax rates and not with prices. It is hard to think of such an instrument. In this context, it is natural to think of a model in which communities use Ramsey pricing principles to set tax rates. Since Ramsey pricing is best thought of as trying to achieve a target post-tax price, this will induce a negative correlation between tax rates and tax-exclusive prices, so that our estimates of the coefficients on the various tax rates would be biased downward.

[20] Of course, to the extent that the wrong structure is imposed, the resulting coefficients will be inconsistent. Nevertheless, dozens of previous studies in a variety of contexts have found this to be a useful way to proceed.

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TABLE 4
ALTERNATIVE SPECIFICATIONS

	(1) Unrestricted Lag Structure	(2) Short-Run Coefficient	(3) Lag Coefficient	(4) Long-Run Coefficient	(5) Autocorrelated Errors	(6) *F*-Statistic *p*-value (on Interactions)	(7) Coefficient with Interactions
Bananas	2.10	0.620	0.224	0.799	1.02	0.054	1.28
	(2.80)	(1.70)	(12.0)		(3.41)		(3.28)
Bread	1.78	1.52	0.312	2.21	1.38	0.81	—
	(1.65)	(3.10)	(14.9)		(2.95)		
Big Mac	0.868	–0.0342	0.366	–0.0539	–0.149	0.49	—
	(3.04)	(–0.178)	(11.3)		(–.928)		
Crisco	1.21	0.612	0.519	1.27	0.870	0.059	0.813
	(2.70)	(4.47)	(25.8)		(5.04)		(3.83)
Eggs	–0.684	–0.0136	0.342	–0.0207	0.244	0.024	0.00199
	(–1.35)	(–0.070)	(16.5)		(1.20)		(0.003)
Kleenex	0.866	–0.190	0.338	–0.287	–0.281	0.41	—
	(2.09)	(–0.078)	(17.0)		(–1.14)		
Milk	0.771	0.284	0.550	0.631	0.189	0.045	0.483
	(1.98)	(2.42)	(26.5)		(1.65)		(2.50)
Monopoly	–0.683	0.461	0.397	0.765	0.643	0.23	—
	(0.838)	(1.10)	(12.7)		(1.57)		
Shampoo	0.339	0.760	0.332	1.14	1.02	0.38	—
	(0.578)	(2.34)	(15.7)		(3.38)		
Soda	1.05	0.831	0.307	1.20	0.631	0.65	—
	(1.58)	(3.00)	(16.7)		(2.15)		
Spin balance	–0.306	0.107	0.455	0.196	–1.11	0.001	0.346
	(–0.483)	(0.211)	(18.3)		(–2.20)		(0.548)
Underwear	2.23	0.883	0.455	1.62	1.28	0.41	—
	(2.68)	(1.91)	(20.8)		(2.68)		

Note: All figures in parentheses are *t*-statistics calculated from heteroskedasticity consistent standard errors. Column (1) shows the sum of the coefficients on τ_{ijt} and its lags in a specification without any lag structure imposed. Columns (2), (3), and (4) are from a specification with a geometric lag structure: (2) shows the coefficient on the contemporaneous tax rate, (3) shows the coefficient on the lagged price, and (4) shows the long-run coefficient on the tax rate. Column (5) shows the value of β_{li} when the model is estimated with a first-order autocorrelated error structure; the correlation coefficient varies across cities. Column (6) is the *p*-value on the *F*-statistic testing the joint significance of closeness to the border and land area variables interacted with the tax rate. Column (7) gives the derivative of the log-price with respect to the tax rate evaluated at the means. All specifications include city effects; time effects; and measures of real rental, wage, and energy costs.

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includes on the right-hand side all of the original variables plus the lagged dependent variable.

The coefficient on the lagged dependent variable, which we denote λ_i, measures the rate of decay in the lag distribution—the lower the value of λ_i the faster the decay, i.e., the greater the proportion of the impact that is felt immediately. In this specification, the coefficient on the contemporaneous value of the variable (here, the coefficient on τ_{ijt}) gives the short-run impact on the dependent variable. The long-run impact is the short-run impact divided by $(1 - \lambda_i)$.[21]

The results are summarized in columns (2), (3), and (4) of Table 4. Column (2) shows the coefficient on the contemporaneous tax rate. Note that for most commodities, the sign of the coefficient is the same as that on the corresponding coefficient in the canonical specification in Table 3. (The only exceptions—eggs, Kleenex, and spin balances—have imprecisely estimated coefficients in both specifications.) All the coefficients that were insignificant stay that way, and only one of the coefficients that was significant becomes insignificant (bananas). Moreover, for each commodity with a precisely estimated coefficient, the coefficient in column (2) is smaller in absolute value than its counterpart in Table 3. This is consistent with an interpretation of the coefficients in the canonical specification as being an amalgam of the long- and short-run effects, while those in column (2) are short run.

The lag coefficients (λ_i) are shown in column (3). They all fall between 0.22 and 0.55. We discuss the implications for the speed of adjustment below. Column (4) shows the long-run effects of taxes on prices, the column (2) parameter divided by one minus the column (3) parameter. There is a quite striking similarity to the results in Table 3. We conclude that allowing for dynamics in a conventional fashion does not affect our basic result, that taxes on a number of commodities are overshifted.

Let us now turn to the estimates of λ_i in column (3), which are of independent interest. As noted above, these coefficients determine speeds of adjustment. Specifically, the average lag length is given by $\lambda_i/(1 - \lambda_i)$. Hence, according to our estimates, the mean lags vary from 0.29 to 1.27 quarters. Prices react very quickly to changes in tax rates.

This finding is relevant to the debate over price stickiness in the macroeconomics literature. The New Keynesian view assumes adjustment costs to explain price rigidity, while the New Classical macroeconomists favor models with no rigidities in wages or prices. Ultimately, the question of who is right must be answered empirically. Carlton (1986) investigates this issue using data on transactions prices. He finds evidence of relatively long periods of price stickiness. However, he does not look at how shocks to costs get transmitted into prices. Cecchetti (1986) looks at evidence from magazine prices and finds that, during periods of inflation, price adjustments are more frequent, although there are considerable reductions in real prices before some adjustment. However, again, he is not looking at the effect of a price shock. Blinder (1991) uses interview data on price setting behavior of firms. He asks about hypothetical responses to cost shocks and estimates that these are passed through to prices in about three months. Remarkably, this is about the mean lag that we

[21] In a panel data context, biases may arise in estimating specifications with a lagged dependent variable. However, provided that the error term is not autocorrelated and there is a relatively large number of time-series observations (as is the case here), such biases are likely to be inconsequential. For further discussion, see Hsiao (1986). The assumption of temporally uncorrelated errors seems reasonable given that we have taken out city and time effects. Nevertheless, we also estimated the equations using lagged values as instrumental variables. This led to somewhat less precise estimates, but the substantive story was the same.

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estimate. In short, while the speed of adjustment is not central for the issue of tax shifting, our results provide some support for the view that prices are quite flexible at the microlevel.

A final issue relating to dynamics concerns the regression error in equation 4. We have been assuming that the error is uncorrelated across time periods, but, to the contrary, there might be persistence of shocks. We therefore re-estimated the canonical equation with an AR(1) error and allow the correlation coefficient to vary across cities. The results are shown in column (5) of Table 4. The general patterns exhibited in the canonical model are maintained, although the shifting parameters tend to be somewhat smaller in absolute value.[22]

Including Other Tax Rates

Our specification assumes that only a commodity's own tax rate affects its price. In principle, however, the tax rates on all commodities might affect any given commodity's price through either demand or cost interactions. This observation is particularly relevant because the tax rates on various commodities tend to change at the same time. This might call into question our interpretation of β_{li} as the independent tax effect.

To investigate this possibility, we re-estimated the canonical model including all the tax rates on the right-hand side. Unfortunately, multicollinearity among the tax rates led to absurd findings. We thereupon re-estimated the equation, without allowing the clothing rate and the general rate to appear together. We found that in only about half the cases were the tax rates that applied to the "other" commodities jointly significant. Even in these cases, the coefficients on the own tax rates were about the same size as those reported above. Patterns of significance were also preserved. In short, our interpretation of the coefficient on the own tax rate as an overshifting parameter seems legitimate.

Interactions with the Tax Variable

Another possible problem with the canonical specification is the implicit assumption that the shifting parameter is the same for each city. There are many ways in which one might relax this assumption. One natural possibility is suggested by the fact that consumers in one jurisdiction may choose to shop in another jurisdiction if the tax rates there are lower (see Trandel (1992)). The extent to which taxes could be shifted to consumers might be less in communities that are closer to jurisdictions with other tax rates, *ceteris paribus*. Another possibility is that the area of a jurisdiction might be related to the size of the retail market and hence affect the extent of shifting.

We were able to obtain the areas of 130 of our cities.[23] Using atlases, we also computed for each city the distance from the nearest state border, which we converted into a dichotomous variable taking a value of one if the distance is less than five miles and zero otherwise. We then augmented the specification in Table 3 with each of these variables interacted with the tax rate. Our first step after re-estimating the regressions was to test the joint significance of the interaction variables. The results are shown in column (6) of Table 4. These results indicate that, for most of the commodities, the interaction terms are jointly insignificant. For the cases in which they are significant (bananas, Crisco, eggs, milk, and spin balances), we report the shifting coefficient evaluated at the means

[22] The spin balance parameter is –1.11, which suggests the implausible result that the after-tax price falls after imposition of a tax. One cannot, however, reject the hypothesis that the coefficient is one.

[23] The data on area as well as the other variables discussed below were obtained from the *City and County Fact Book* for 1988.

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in column (7).[24] The commodities that exhibited overshifting in the canonical specification continue to do so when interactions are included.

We used distance from the border and area of the city largely because these variables are available and can plausibly be regarded as fixed through time. Of course, time-changing variables might also be candidates for interacting with the tax rate. However, we do not have quarterly or even annual data on any relevant city characteristics. We experimented with several variables whose values, though not fixed through time, were available at least for 1985. These are income per capita, population, population density, and retail stores per square mile. These variables might proxy for the state of retail competition and, hence, are candidates for interacting with the tax rate. Since these variables are not likely to be constant through time, including them in the analysis introduces possible measurement errors into the regression. Thus, we must be careful in placing too much weight on these results.

We find that, in almost all cases, the interaction terms are jointly significant. However, most of the results from the canonical model continue to hold. In the few cases in which they do not, this might be due to measurement error in jurisdiction characteristics. Clearly, this issue merits investigation in future work, but given the quality of the data on city characteristics, we find the results to be encouraging to the view that our initial findings are robust.[25]

Cost Variables

As noted above, our cost variables—the rental value of a typical two-bedroom apartment, the minimum labor charge for a home-service call to repair a clothes washing machine, and the price of one gallon of unleaded gasoline—are far from ideal proxies for differences in production costs across jurisdictions. It is possible to obtain somewhat better data on labor and energy prices for a subset of the communities in our sample. Specifically, from the Bureau of Labor Statistics volume *Employment and Earnings* and the Department of Energy's publication *Typical Electricity Bills*, we can obtain wage and commercial electricity rate data, respectively, on a city-by-city basis. The problem is that both the wage and electricity data are available for only 57 out of the 155 cities in our sample. Further, the electricity data are available only annually. This leaves us with many fewer observations to estimate the regression parameters. (The number of observations in a typical equation falls from about 4,200 to 390.) Nevertheless, it seemed worthwhile to reestimate the model with this subsample.

We found that four of the commodities that were characterized by overshifting in Table 3 now have insignificant coefficients on their tax-rate variables—Crisco, milk, shampoo, and underwear. On the other hand, bananas, bread, and soda continue to have the positive and statistically significant coefficients associated with overshifting. (Detailed results are reported in column (2) of the Appendix table.) What is one to make of these findings? With a dramatically smaller sample size, one expects larger standard errors—*ceteris paribus*, when the sample size decreases by a factor of n, standard errors increase by a factor of the square root of n. Hence, it is no surprise that some of our coefficients are rendered insignificant by this exercise. The fact that several commodities nevertheless continue to exhibit positive and statistically significant values of β_{li} suggests that our finding of tax overshifting in some commodities is not due to inadequate con-

[24] The shifting coefficient is $\beta_{li} + \beta_{xi}\bar{x}$, where β_{li} is the vector of means of the interaction terms and β_{xi} is the associated parameter vector.

[25] It should also be observed that the joint significance of the interaction variables is per se evidence against the long-run competitive model with constant costs, which would predict identical shifting across jurisdictions.

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trols for cost differences across cities. In this context, it is important to recall that our equations all contain city effects, and these probably pick up the important across-city differences in production costs.

Comparison with Poterba's Specification

As noted earlier, Poterba (1996) employed a similar approach to ours, but was unable to reject the hypothesis of one-for-one shifting predicted by the competitive model. In an attempt to reconcile our results with Poterba's, we focused on the major differences between his specification and our own.

First, Poterba estimates his equation in first differences rather than with city dichotomous variables. We examined whether a first-difference specification is consistent with our data. Specifically, for each commodity, we re-estimated the canonical equation deleting the city dichotomous variables and including on the right-hand side the lagged value of the left-hand side variable, as well as the lagged values of each of the right-hand side variables. If the first-difference specification is correct, then the lagged dependent variable should have a coefficient of one, and the lagged value of each right-hand side variable should be the negative of the coefficient of the associated contemporaneous variable. This joint hypothesis was strongly rejected for each commodity. We conclude that a first-difference specification is not consistent with our data.

Second, Poterba does not include time effects, although he does use the economy-wide inflation rate to control for changes in the macroeconomic environment. However, we continue to find overshifting for several commodities when we replace the time effects with the inflation rate.

Finally, Poterba's commodities are more aggregated than ours. To investigate the possible effects of aggregation, we formed a composite "grocery" commodity by taking a weighted average of the relevant individual items' prices.[26] If we estimate our canonical model using the composite, we find overshifting. The coefficient on the tax rate is 1.21 with a *t*-statistic of 5.21. Further, if we estimate the model with the composite *and* impose differencing (despite the results of the statistical test discussed above), we continue to find a positive and statistically significant coefficient—1.70 with a *t*-statistic of 9.03. This is the closest approximation to Poterba's setup allowed by our data.

Where does this leave us? For some commodities, we obtain results consistent with Poterba's—full shifting of taxes. However, unlike Poterba, we find that taxes on some commodities are overshifted, and this result holds even when we use his specification. Given that the commodities that we analyze are simply not the same as those in Poterba's sample, ultimately, it is difficult to reconcile the two sets of results.

CONCLUSIONS

A time-honored question in public finance is how prices react to the imposition of taxes. Although there is a vast theoretical literature on this question, there has been surprisingly little empirical work. In this paper, we follow a simple and obvious strategy for addressing this issue: we examine the relationship between the prices of particular commodities and the taxes levied upon them. Specifically, we assemble a panel of quarterly data for 12 commodities and 155 cities over the period 1982–90. Importantly, such data allow us to control for fixed effects across jurisdictions that might affect prices, and for shocks in the macroeconomic environment that might affect all cities similarly.

[26] The items included are milk, eggs, bananas, bread, Kleenex, and soda. The weights are taken from the ACCRA data and are the same as they use to construct their price indices.

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At the same time, this strategy allows us to avoid problems associated with aggregation and small samples that have bedeviled the few previous efforts in this area.

We find a variety of shifting patterns. For some commodities, we cannot reject that taxes are shifted on a one-for-one basis. For others, commodity taxes are overshifted–a ten-cent increase in the revenue extracted from the sale of these commodities leads to an increase in their prices of more than a dime. The finding that some commodities exhibit overshifting is robust to a number of reasonable alternative specifications of the estimating equation. It is consistent with the predictions of certain theoretical models of imperfect competition that seem like reasonable characterizations of the retail sector. What do our results say about the markets for the commodities that exhibit overshifting? They do *not* imply that any particular model of market structure is correct, but they are inconsistent with perfect competition. Hall (1988) also found that many markets, and the retail market in particular, are not competitive. We also find that prices respond quite rapidly to the imposition of taxes—the mean lag length is only about one-quarter. This finding may have implications for the price-flexibility debate in the macroeconomics literature.

The policy implications of our results are striking. Distributional tables for proposed policy changes typically assume that commodity taxes increase consumer prices on a one-for-one basis. If, in fact, prices on some commodities go up more than on a one-for-one basis, then taxes on these items are more burdensome than the usual analyses would suggest. To the extent that our findings for food items hold more generally, taxes that fall on them are likely to be more regressive than is conventionally thought. Such considerations might be important in thinking about recent proposals to introduce a value-added tax in the United States. In the same way, these findings are relevant for evaluating European proposals to harmonize value-added tax rates across countries.

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Appendix

In Table A.1, we report the shifting parameters for several variations on our canonical model, equation 4. In column (1), the price of a spin balance is used as a numeraire. That is, the left-hand-side variable is the logarithm of the price of the relevant commodity minus the logarithm of the price of a spin balance. Column (2) uses alternative measures for the wage rate and energy costs. (These equations are estimated on an annual rather than a quarterly basis.) In column (3), the model is estimated without any cost variables.

TABLE A.1
SHIFTING PARAMETERS UNDER ALTERNATIVE SPECIFICATIONS

	(1) Spin Balance as Numeraire	(2) Alternative Cost Measures	(3) No-Cost Variables
Bananas	0.665	1.72	0.823
	(2.27)	(3.29)	(2.31)
Bread	1.42	3.90	2.46
	(3.207)	(4.03)	(4.93)
Big Mac	–0.282	0.530	–0.137
	(–1.86)	(1.26)	(–0.748)
Crisco	0.816	0.625	1.01
	(3.76)	(1.06)	(6.06)
Eggs	0.172	0.107	0.0002
	(0.827)	(0.224)	(0.001)
Kleenex	0.121	–1.08	–0.0895
	(0.599)	(–1.57)	(–0.368)
Milk	0.489	0.338	0.510
	(3.63)	(1.08)	(3.44)
Monopoly	0.357	0.403	0.961
	(0.831)	(0.344)	(2.36)
Shampoo	1.03	0.111	0.944
	(3.51)	(0.118)	(2.94)
Soda	0.930	2.27	1.32
	(2.45)	(3.29)	(4.42)
Spin balance	—	0.961	–0.259
	—	(0.567)	(–0.479)
Underwear	0.779	–1.14	1.47
	(1.93)	(–0.767)	(3.02)

Notes: Column (1) shows β_{li}, the coefficient on the tax rate, when equation 4 is estimated with the left-hand-side variable as the log of the price of the respective commodity minus the log of the price of a spin balance. (All figures in parentheses are *t*-statistics calculated from heteroskedasticity consistent standard errors.) Column (2) shows the coefficients when alternative measures of wage and energy costs are used for a subsample of the communities. Column (3) shows the coefficients when no-cost variables are included, using the same samples as in Table 3.

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LIST OF CITIES

AL	Birmingham, Dothan, Gadsden, Huntsville, Mobile, Montgomery
AR	Fayetteville, Fort Smith, Jonesboro
AZ	Phoenix
CA	Blythe, Fresno, Indio, Palm Springs, Riverside, Sacramento, San Diego, San Jose, Visalia
CO	Colorado Springs, Denver, Grand Junction, Pueblo
CT	Hartford
FL	Gainesville, Lakeland, Miami, Pensacola, West Palm Beach
GA	Albany, Americus, Atlanta, Augusta, Columbus, Macon
IA	Cedar Rapids, Fort Dodge
ID	Boise
IL	Champaign, Charleston, Decatur, Peoria, Rockford, Springfield
IN	Anderson, Bloomington, Fort Wayne, Indianapolis, Marion, South Bend, Warsaw
KS	Great Bend, Wichita
KY	Bowling Green, Lexington, Louisville, Murray, Owensboro, Somerset
LA	Baton Rouge, Lake Charles, New Orleans
MD	Baltimore
MI	Benton Harbor, Jackson, Marquette, Traverse City
MN	Minneapolis
MO	Clinton, Columbia, Jefferson City, Joplin, Kansas City, Springfield, St. Joseph, St. Louis
NC	Charlotte, Durham, Greensboro, Greenville, Hickory, Marion, Rocky Mount, Wilmington, Winston-Salem
NE	Hastings, Lincoln, Omaha
NM	Alamagordo, Albuquerque
NV	Las Vegas, Reno
NY	Binghamton, Buffalo, Elmira, Syracuse
OH	Akron, Canton, Cincinnati, Columbus, Newark, Youngstown
OK	Oklahoma City, Tulsa
OR	Portland
PA	Harrisburg, Lancaster, Waynesboro, Wilkes-Barre, York
SC	Greenville
SD	Rapid City, Vermillion
TN	Chattanooga, Dyersburg, Jackson, Knoxville, Memphis, Morristown, Nashville
TX	Abilene, Amarillo, El Paso, Harlingen, Houston, Kerrville, Killeen, Lubbock, McAllen, Odessa, San Antonio, Sherman, Temple, Texarkana, Tyler, Waco, Wichita Falls
UT	Provo, Salt Lake City
VA	Norfolk, Roanoke
WA	Richland, Tacoma, Yakima
WI	Appleton, Fond Du Lac, Green Bay, Janesville, La Crosse, Marinette, New London, Oshkosh, Wausau
WY	Casper, Charleston