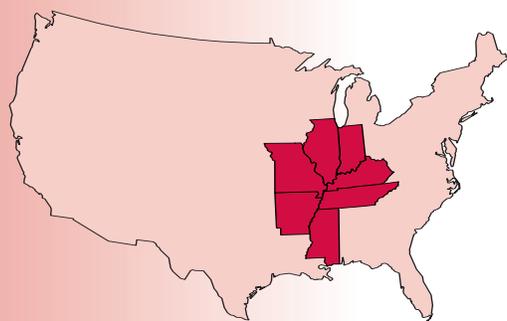
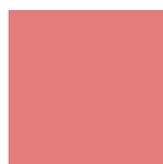
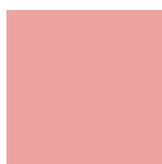
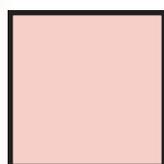


Federal Reserve Bank of St. Louis

REGIONAL ECONOMIC DEVELOPMENT

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Selected Papers from the Transportation and Economic Development Conference (TED 2006) and Proceedings of the Second Annual Conference of the Business & Economics Research Group (BERG)

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Selected Papers from the Transportation and Economic Development Conference (TED 2006) and Proceedings of the Second Annual Conference of the Business & Economics Research Group (BERG)

77 **Editor's Introduction**
Thomas A. Garrett

78 **Encouraging Development
through Better Integration of
U.S. and Canadian Transportation:
The Open Prairies Proposal**
Richard Beilock, Robert Dolyniuk, and
Barry Prentice

87 **Intra-NAFTA Trade and
Surface Traffic:
A Very Disaggregated View**
Mark Funk, Erick Elder, Vincent Yao, and
Ashvin Vibhakar

100 **Transportation Infrastructure,
Retail Clustering, and
Local Public Finance: Evidence
from Wal-Mart's Expansion**
Michael J. Hicks

115 **On the Economic Analysis of
Smoking Bans**
Michael R. Pakko

131 What Do We Know About Oil Prices and State Economic Performance?

David A. Penn

**140 The Long-Run Relationship Between Consumption and Housing Wealth
in the Eighth District States**

David E. Rapach and Jack K. Strauss

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Editor's Introduction

Thomas A. Garrett

This issue of *Regional Economic Development* contains six selected papers from two conferences sponsored or co-sponsored by the Federal Reserve Bank of St. Louis during 2006. The Transportation and Economic Development (TED) conference was held in Little Rock, Arkansas, in March. The TED conference was sponsored by numerous groups, including the Institute for Economic Advancement at the University of Arkansas in Little Rock, the Federal Reserve Bank of St. Louis, and the Transportation Research Board. TED conference participants addressed regional and national transportation issues as they relate to economic development, such as NAFTA, border trade, and transportation logistics.¹

The Center for Regional Economics—8th District (CRE8) at the Federal Reserve Bank of St. Louis sponsored the second annual conference of the Business and Economics Research Group (BERG) in St. Louis in June. Researchers from university based centers for business and economic research located within the Eighth Federal Reserve District presented a wide variety of papers on economic issues relevant to District states.²

The first three papers in this issue were presented at the TED conference. Richard Beilock of the University Florida, Robert Dolyniuk of the Manitoba Trucking Association, and Barry Prentice

of the Transportation Institute of the University of Manitoba explore the potential economic gains had by freer motor carrier transportation between the United States and Canada. Mark Funk, Erick Elder, Vincent Yao, and Ashvin Vibhakar at the University of Arkansas at Little Rock look at the different effects of NAFTA across industries in five southern states. Michael Hicks of the Air Force Institute of Technology and Marshall University examines the role of public infrastructure and local government fiscal policies on local agglomeration in the retail sector of Indiana counties.

Three papers from the second annual BERG conference focus on economic issues in Eighth District states. Mike Pakko of the Federal Reserve Bank of St. Louis discusses the potential economic effects of smoking bans and offers an empirical evaluation of the revenue effects on bars and restaurants resulting from a recent smoking ban in one Missouri town. David Penn of Middle Tennessee State University examines how economic output of states located in the Eighth Federal Reserve District is affected by changing oil prices. Finally, David Rapach and Jack Strauss of Saint Louis University look at the long-run relationship between consumption and housing wealth for Eighth District states.

I would like to acknowledge the help of the authors, referees, and conference participants who contributed to this conference volume.

¹ Information on TED can be found at www.ted2006-littlerock.org.

² The second annual BERG conference agenda, along with BERG members and past events, can be found at <http://research.stlouisfed.org/berg/>.

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Encouraging Development through Better Integration of U.S. and Canadian Transportation: The Open Prairies Proposal

Richard Beilock, Robert Dolyniuk, and Barry Prentice

The two largest economic blocks in the world, the European Union (EU) and the area encompassed by the North American Free Trade Agreement, were formed to exploit efficiencies inherent in having larger markets that permit the freest possible flows of capital, labor, and goods and services. In this spirit, throughout much of the EU, member state motor carriers enjoy cabotage, the right to perform domestic movements within another member state. For all practical purposes, motor carrier cabotage does not exist in North America. In this article, the feasibility and likely benefits and costs of North American cabotage are explored in a limited experiment called “Open Prairies.” Open Prairies would allow cabotage for U.S. and Canadian carriers throughout the Canadian Prairie provinces and several Upper Great Plains U.S. states. (JEL F13, F14, F16)

Federal Reserve Bank of St. Louis *Regional Economic Development*, 2006, 2(2), pp. 78-86.

The world’s two largest economic blocks, the European Union (EU) and the area encompassed by the North American Free Trade Agreement (NAFTA), both were formed to exploit efficiencies inherent in having larger markets that permit the freest possible flows of capital, labor, and goods and services. In this regard, the EU is ahead of NAFTA, particularly with respect to transportation. For example, a French motor carrier can deliver a load from Metz to Düsseldorf and thereafter make hauls within Germany, on the same footing as its German counterparts, before returning to France.¹ In other words, throughout much of the EU, a member state’s motor carriers may engage freely in point-to-point movements entirely within the national

boundaries of another member state. That freedom to perform domestic movements within another jurisdiction is known as cabotage. The reality is much different in North America. On both sides of the border, regulations preclude cabotage in virtually all cases.²

In terms of economic efficiency, the EU system seems superior to the more restricted approach among the North American nations. That cabotage is attractive for shippers and carriers is suggested by the fact that the volume of such activity has increased dramatically. In terms of tonne-kilometers, it nearly doubled in the EU between 1999 and 2004.

¹ Technically, a French carrier in Germany has the right to perform domestic movements “on a temporary basis,” according to Council Regulation 3118/93. However, the meaning of this phrase is not clear in the regulation, has never been ruled on by any court in the EU, and is not enforced (ECORYS Nederland, 2004).

² In theory, cabotage is allowed for movements “incidental” to an international movement. However, only in rare instances would a movement qualify as “incidental.” Moreover, there are cross-agency conflicts in limitations placed on foreign carriers. Such problems were summed up in a Canadian industry journal: “U.S. Customs regulations allow for Canadian-based vehicles to transport domestic shipments (point-to-point in the U.S.) when the shipment is incidental to...an international movement...Because the INS regulation prohibits this type of move, in effect, the U.S. Customs regulation is moot at the present time” (*Highway Star Magazine*, 2005).

Richard Beilock is a professor of food and resource economics at the University of Florida; Robert Dolyniuk is the president of the Manitoba Trucking Association; and Barry Prentice is a professor of economics at the Transportation Institute of the University of Manitoba.

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That cabotage need not undermine the viability of domestic carriers is suggested by the fact that across EU nations cabotage accounts for between 0.4 and 2.56 percent of total domestic truck movements (ECORYS Nederland, 2004).

To be sure, there are potential problems from cabotage, including security considerations and cross-national differences in labor laws, weight and size limits, and tax regimes. Moreover, as was the case for economic deregulation of trucking within the borders of the United States and Canada, political realities may slow or entirely preclude liberalization long after the preponderance of scientific opinions and evidence from other areas of the world commend it as beneficial. Without a clearly relevant example of success, it is very difficult to change the status quo. Consider, for instance, the likely pace of economic deregulation in the United States and Canada if Florida³ had deregulated its intrastate trucking in 1960, instead of 1980. In that event, there would have been an actual example from which to judge the effects, rather than just the speculations of academics, those in the industry, and bureaucrats.

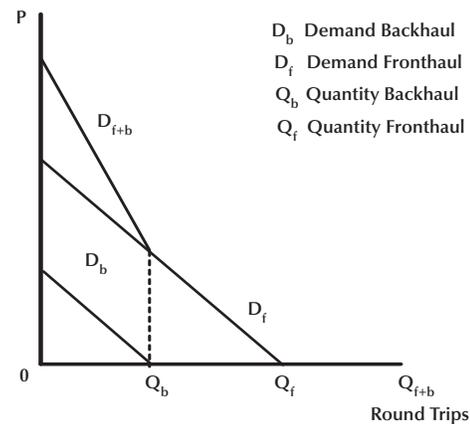
In this paper, we offer an approach to effect a limited experiment in motor carrier cabotage in North America, which we call "Open Prairies." Open Prairies would allow cabotage for U.S. and Canadian motor carriers throughout the Canadian Prairie provinces and several Upper Great Plains U.S. states. The plan would include a sunset provision to require both nations to reaffirm the arrangement after a specified period. We also discuss variants of the plan, each with different rules regarding conditions under which cabotage would be permitted. The likely sources of costs and benefits of the scheme are briefly discussed as well.

The limited scope of this paper should be clearly understood. It presents a possible approach for an experiment in motor carrier cabotage in North America, but does not provide quantitative estimates of the potential impacts based on extrapolations from similar experiences in other parts of the globe or from assuming specific changes in the performance of North American carriers. Indeed,

³ Florida was the first state to deregulate intrastate motor carrier transportation.

Figure 1

Joint Production of Fronthaul and Backhaul



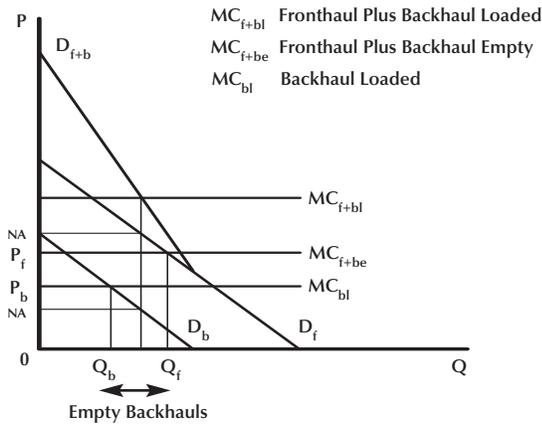
central to our proposal is the observation that arguments based on such estimates have proven insufficiently compelling to make cabotage for motor carriage in North America a serious political issue. To do that, we assert, requires a demonstration of cabotage within North America.

THEORETICAL FRAMEWORK

The absence of cabotage means that Canadian trucks carrying goods across the border to the United States are not allowed to carry goods from one point to another *within* the United States. In Canada, customs and immigration laws create reciprocal restrictions on American truckers so that they are prohibited from carrying loads with both origin and destination in Canada. As a result, trucks experience more empty miles and on average charge higher freight rates to cover their costs.

The outbound and inbound movements of a truck's round trip are joint products: The outbound trip cannot be created without the existence of a return trip. By convention, the direction that has the strongest demand (D_f) for trucking services is referred to as the fronthaul; the other direction, with the weaker demand (D_b), is referred to as the backhaul. With joint products, demands are summed vertically, as illustrated in Figure 1, to obtain the

Figure 2
Calculation of Fronthaul and Backhaul Freight Rates in an Unbalanced Market



total demand. Based on this total demand (D_{f+b}), the freight rates in each direction are determined.

Figure 2 illustrates the calculation of freight rates in an unbalanced market⁴ in which a large number of trucks are forced to return empty ($Q_f - Q_b$). Freight moving in the direction of the fronthaul bears the bulk of the costs. The freight rate for the fronthaul (P_f) covers the cost of the fronthaul movement plus the cost of returning empty. This follows because freight rates must be sufficient to attract the marginal trucker. With freight imbalances, that trucker would not have a backhaul and would, therefore, need to be compensated for empty return costs. Higher fronthaul freight rates that include the marginal costs of the fronthaul and the empty return (MC_{f+be}) discourage trade.

In sharp contrast, the backhaul freight rate (P_b) covers only the marginal cost of returning full, rather than empty. These costs include the following: search for the backhaul load, repositioning, loading and unloading of the load, and additional fuel and wear and tear associated with running full, rather than empty. It could be argued that the low-cost transport in the backhaul direction results in more freight movements in that direction (and thus

results in more closely balanced transport markets) than if backhaul rates were higher. This is certainly true. But there are limits to which lower freight rates can stimulate movements in the “light” direction. Unbalanced freight lanes can have many causes, including differences in resource endowments, income distributions, and populations. Just because freight rates are low does not mean that freight volumes in opposite directions will balance quickly or, indeed, ever.

IN PRAISE OF TRIANGULATION

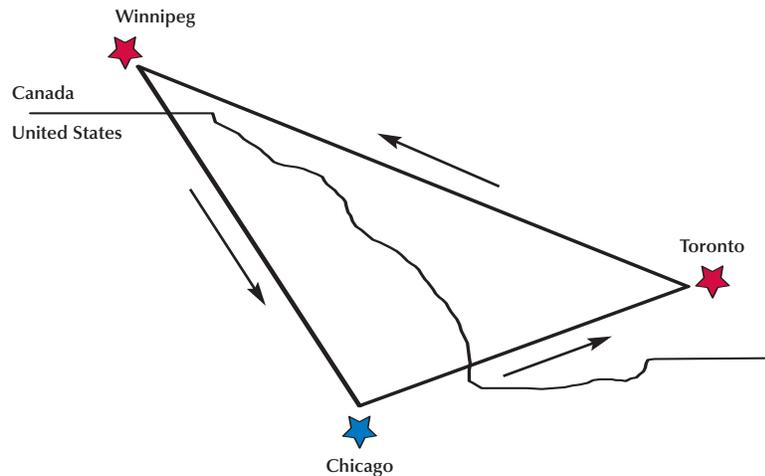
Trucking companies try to avoid taking low-paying backhaul rates by employing routes that provide better-paying loads over three or more legs. The trucking industry refers to this as triangulation.⁵ Figure 3 represents a common triangulation route that Canadian truckers may use to avoid the Winnipeg-to-Toronto backhaul traffic lane. Carriers can earn fronthaul rates on loads to Chicago, pick up another fronthaul load to Toronto, and finally return to Winnipeg with a fronthaul load. Parenthetically, for the Toronto-to-Winnipeg leg, the deregulation of the Canadian trucking industry helped truckers find loadings.

Similar patterns are available to U.S. carriers, except the base of the triangle has to be within their own national jurisdiction.

The economic and environmental benefits of triangulation should be stressed. Low backhaul rates along a route reflect low economic value for marginal (i.e., additional) truck services. For trucks running empty along a route, because of insufficient load availability, there are no positive benefits from the movement, and monetary, environmental, and safety costs are still incurred. With triangulation, a fronthaul and a backhaul situation is transformed into three or more fronthauls. The lose-lose situations of empty movements are minimized. As a result, downward pressure is exerted on freight rates on the triangulation routes. Recall that in typical fronthaul-backhaul situations, the freight rate for the fronthaul incorporates the costs of the fronthaul, as well as the empty backhaul movement

⁴ That is, a market with more freight movements in one direction than in the opposite direction.

⁵ In this discussion “triangle” or “triangular route” will refer to roundtrip routings with more than two legs. So, a “triangle” may geometrically be a literal triangle (three legs), rectangle or trapezoid (four legs), etc.

Figure 3**Triangulation of Truck Routes to Avoid Backhaul Freight Rates**

along the routing. If there are no backhauls, that element disappears. That is, P_f declines from MC_{f+be} to MC_f .⁶ In simple terms, with triangulation, fewer trucks handle the same amount of freight for higher per-unit-distance returns to themselves, but lower total costs to shippers and lower total costs to the environment and lower safety risks.

Suppose that loads are available along and in the directions indicated in Figure 3. If a Canadian trucker secures the loading from Winnipeg to Chicago, that trucker potentially could take advantage of the triangular movement back to Winnipeg via Toronto. On the other hand, however, suppose a U.S. carrier secures either the loading from Winnipeg to Chicago or that from Chicago to Toronto. That carrier would be precluded from avoiding the backhaul movement because the Toronto-to-Winnipeg portion of the triangle would involve cabotage. The same would be true for Canadian drivers with respect to triangulation possibilities with point-to-point movements on the U.S. side of the border, for example, a Winnipeg-Chicago-Kansas City triangle. Triangulation helps truckers make the best use of their assets for their

own sakes, society's, and the environment's. Restrictions against cabotage limit triangulation.

ELUSIVE BENEFITS AND NON-ELUSIVE COSTS OF RESTRICTING CABOTAGE

Suppose the only opportunity for a triangular routing between Canada and the United States were the one depicted in Figure 3. Under current laws, with cabotage precluded, only Canadian truckers would have the opportunity to use it. If underlying operational costs of trucking firms on both sides of the border were the same, Canadian trucking firms would dominate. This follows because Canadian trucking firms would be able to offer haulage along each leg of the triangle, including those crossing the border, for MC_f . U.S. trucking firms would be precluded entirely from the intra-Canada movement (i.e., Toronto to Winnipeg) and at best could offer service on the cross-border routings for MC_{f+be} . Under these conditions, Canadian trucking firms would have incentives to lobby for continuance of cabotage restrictions as it artificially gives them a cost advantage relative to their U.S. counterparts for the cross-border

⁶ MC_f is not shown in Figure 2. It would be somewhere between MC_{f+be} and MC_{bl} .

movements and a monopoly for the intra-Canada movement.

Monopolies and cost advantages resulting from the laws of man, rather than those of nature or technology, may bestow relative advantages on some, but virtually always at the cost of greater disadvantages to society. If restricting use of the Figure 3 triangle to carriers with owners owing some allegiance to Queen Elizabeth makes sense for society, perhaps even greater gains could be realized from the further stipulation that those owners be left-handed. Of course, that is ludicrous. Given the fiction of only one possible triangular routing, “protected” Canadian carriers might benefit, but with overall net losses to society over a free-transport market solution. In the real world, the potential for losses would be magnified and the “protected” carriers may realize little or even negative benefits from their protection.

In reality, at any moment there would be tens or hundreds of thousands of possibilities for triangular routings between the United States and Canada and these would be changing across time. For example, a Canadian carrier’s vehicle might arrive in Chicago with a load from Winnipeg and, only then, the carrier become aware of possible, but legally precluded, lucrative loads from Chicago to Fargo and others from Fargo to Winnipeg. Because trade sanctions almost always are reciprocal, opposing cabotage freedoms to protect triangles over which your and your countrymen’s firms have exclusive use denies you access to triangles having more than one terminal point in the other country. Every opportunity denied reduces the net gain from the protected triangles. There still might be a net gain for some if it were a zero-sum game. But the self-imposed limits for the sake of protection would also tend to limit opportunities for exploiting economies of size and scope.

Even without economies of size or scope, if there actually were economic profits to be had from denying cabotage to others, this would mean higher than technically necessary freight rates. Over the medium and long term, such rates would erode the competitiveness of the shipper/receiver firms and ultimately lower freight levels. Over the long run, protected trade inevitably means less economic

activity. Protections might grant you a larger portion of the economic pie, but that pie will be smaller, as might be your slice of it. This is particularly true as the other giant trading block, the EU, allows cabotage. An apt analogy is the Canadian experience during the 1980s. Beginning in the late 1970s, economic regulations in the U.S. trucking system were being greatly reduced,⁷ while the Canadian system remained largely unchanged. After a sharp recession in the first few years of the 1980s, the nearly deregulated U.S. trucking industry entered a period of significant growth and productivity gains (Jones, Fullerton, and Beilock, 1992). In part because of perceived and realized corrosive effects on the Canadian economy from a more competitive U.S. transportation system and, hence, cheaper U.S. goods, as well as diversions of Canadian freight through the United States, Canada soon began deregulating its trucking.

DESIGNING A NORTH AMERICAN EXPERIMENT IN CABOTAGE FOR TRUCKING

For the reasons just presented, it behooves North American policymakers to consider moving toward the more liberal EU system. Just as Florida’s and Arizona’s complete deregulation (the first and second states, respectively, to deregulate intrastate trucking) provided valuable input to other state and U.S. federal authorities in determining proper directions for their reforms, a limited North American experiment in free cabotage could be of significant value.⁸ The following might be considered as minimum requirements for a Canadian/U.S. experiment in cabotage for trucking:

⁷ The first and most major legislation leading to deregulation was the Motor Carrier Act of 1980. However, beginning around 1977, the ICC began liberalizing its administration of existing regulations.

⁸ Reflecting the importance of the Florida and Arizona examples, the U.S. federal government funded studies of their pre- and post-deregulation experiences; e.g., see Beilock and Freeman (1985) and Freeman and Beilock (1984). Other examples include a study funded by the Utah state legislature during drafting and consideration of a trucking reform bill, which drew heavily from the Florida and Arizona experiences; see Beilock and Freeman (1984) and a study funded by the California Public Utilities Commission of Florida and Arizona household goods carriers for its deliberations to deregulate intrastate household goods carrier freight rates (see Beilock, 1990).

- The experiment should be reversible. Indeed, to prevent the experiment from passively morphing into the status quo, the mechanism for its termination should be in place from the onset.
- The areas in both countries should be large enough to facilitate real and detectable effects from allowing cabotage.
- The experiment is intended to be limited, and the directly affected regions should account for relatively small shares of both economies and populations.

The first requirement can be dealt with, simply, through a sunset provision indicating a date after which the experiment is ended unless it is reaffirmed through new legislation.⁹ Given that this would require timely action by two federal governments and, possibly, various states and provinces, it seems likely that it would be continued only if the results had been markedly and broadly favorable.

THE PRAIRIE PROVINCES AND THE UPPER GREAT PLAINS STATES

We propose all or part of the Canadian Prairie provinces and the Upper Great Plains U.S. states as the best candidate region for a cabotage experiment. Hereafter, we will refer to a cabotage experiment in this region as Open Prairies.

As suggested from our earlier discussion, for there to be significant potential benefits from allowing cabotage, there needs to be significant improvements in equipment utilization rates (i.e., in the percent of full kilometers—or tonne-kilometers—to total kilometers—or total tonne-kilometers). The three Canadian Prairie provinces (Alberta, Manitoba, and Saskatchewan) and the five U.S. states to their south (Idaho, Montana, Wyoming, and North and South Dakota) are geographically vast, accounting for, respectively, a fifth and an eighth of the areas of their countries. Given these large areas, if cabotage does result in altered equip-

ment utilization rates, the effect should be discernable to researchers.

While the region is geographically large, it accounts for modest shares of each nation's population and economy.¹⁰ Just over one-sixth of all Canadians live in the Prairie provinces and they earn roughly one fifth of the nation's income. The five U.S. states are even smaller, accounting for about 1 percent of U.S. population and production. If there were a desire to have the two regions be more equal, relative to their nations, Alberta might be omitted. This would reduce the Canadian region to around 7 percent of the total population and 6 percent of production. Of course, U.S. states, such as Nebraska and Minnesota, might also be added.

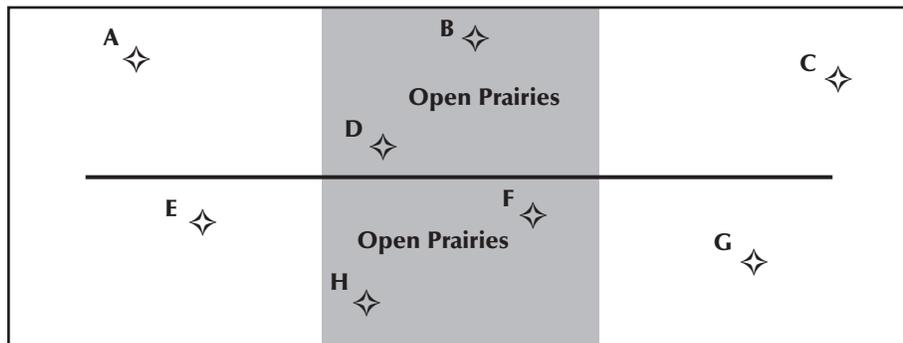
The exact boundaries of the region for Open Prairies would depend on political considerations. The attraction of the Prairie province–Upper Great Plains states area is that it provides large land areas over which to test whether there are benefits from allowing cabotage, while directly involving relatively small portions of the populations and economies of the two nations. Another attraction of the region is that it is relatively poor. With the exception of Alberta, the states and provinces in the proposed region have lower per capita incomes and have slower population growth rates than the averages for their countries. Open Prairies would involve temporary cessation of some restrictions. In other words, carriers and shipper/receivers within the region would have some prerogatives not enjoyed by others. As such, it is highly likely that the overall economic impacts to the affected regions would be positive. For equity reasons, it would be appealing if that favored area was economically less favored and Open Prairies could double as regional development.

The assertion that the overall effects would be positive was not meant to imply that there would not be losers, nor that there might not be other, unexpected, problems. Indeed, identifying the nature and extent of such problems is the underlying rationale for a limited experiment.

⁹ There is an interesting precedent for this. Florida's total economic deregulation of intrastate motor carriage was due to the last-minute failure of its legislature to renew regulations before their sunset provisions came into force.

¹⁰ Information presented in this paragraph is based on Statistics Canada and U.S. Census and Bureau of Economic Analysis data obtained through various websites.

Figure 4
Schematic of Canada and the U.S. with Open Prairies Area



ALTERNATIVE RULES FOR ALLOWING CABOTAGE

There are several possible alternative rules for determining what movements would qualify as permissible cabotage. These will be explained with the aid of Figure 4, which presents a schematic of the two countries. The horizontal line in the middle of the figure represents the United States–Canada border, the gray area is the Open Prairies region, and the crosses denoted by letters are origin and destination points.

Some potential alternatives for permissible cabotage include the following (where “O” and “D” denote origin and destination, respectively, in the Open Prairie region):

- **OD International, OD Cabotage:** Allowing cabotage only if both the prior international movement and the cabotage are entirely within the Open Prairies region. For example, a Canadian carrier would be eligible for a cabotage movement within the United States only if the movement to the United States was from B or D and to either F or H. Further, that cabotage could only be between F and H.
- **D International, OD Cabotage:** This variant is identical to the previous one, with the exception that any origin for the preceding international movement would suffice. So, the aforementioned Canadian carrier could

have brought a load from A or C, in addition to B or D, prior to permissible cabotage between F and H.

- **OD International, O Cabotage:** Like the first variant, the international movement would have to be within the Open Prairies region, such as a movement by a U.S. carrier from H to B. The carrier would then be allowed to perform a movement within Canada as long as the origin were within the Open Prairies region. So, that carrier could make a haul from B to D, A, or C. Or “deadhead” (i.e., travel without a load) to D and make a haul to B, A, or C.
- **D International, O Cabotage:** This variant is identical to the previous one, except that there would be no limitation with respect to the origin for the international movement.
- **O International, D Cabotage:** The origin for the international movement would have to be within the Open Prairies region, but the destination could be anywhere in the other nation, such as G for a Canadian carrier. To qualify as permissible cabotage, that carrier would then have to make a haul to a destination within that country’s part of the Open Prairies region. So, for example, the Canadian carrier with the international haul to G could then make a haul to F or H, but not E.

- **O or D Cabotage:** The origin or destination of the prior international movement would not matter, but at least the origin or destination for the cabotage movement would have to be in the Open Prairies region.

Obviously there are more potential variants, including combinations of some of those already shown. Selection of which variant would depend on two elements. The first is the degree of freedom deemed desirable for the experiment. The second is the regulating authorities' willingness to mandate controls dependent on previous, as well as current, movements. For example, for the last variant listed above, knowledge about the routing taken for a non-native carrier to enter the country would not be necessary to determine whether the current domestic (i.e., cabotage) movement were permissible. Such knowledge would be necessary, however, for most of the previous variants.

NUMBER OF CABOTAGE MOVEMENTS PERMISSIBLE

A related consideration would be whether and the extent to which a foreign carrier could engage in successive cabotage movements before returning to its own country. If successive movements are either limited or precluded entirely, authorities will need the capacity to check on previous hauls.¹¹

POTENTIAL PROBLEMS

There are, to be sure, potential problems or complications with cabotage. For example, cabotage would allow Canadian (U.S.) firms and laborers to operate within the United States (Canada). This occurs frequently in various industries to the extent that they are foreign owned and staffed: for example, a McDonald's in Ottawa and a Tim Hortons in Portland. The complication with trucking is that the location and duration of work would be difficult to predict. To deal with this, systems would have to be developed to address relevant tax issues.

¹¹ The EU approach is to allow cabotage on a "temporary basis" (ECORYS Nederland, 2004). Defining and enforcing this has proven difficult.

While it is important to recognize and prepare systems to deal with issues such as taxation, it is also important not to characterize as problems factors that may be related to international movements but would be materially unaffected by allowing cabotage. Into this category fall three important considerations: national security; vehicle standards, including weight and length requirements; and traffic safety. National security issues related to foreign carriers, basically, concern the danger of allowing undesirable individuals or materials to cross your nation's borders. Open Prairies would not affect security procedures at borders. Likewise, vehicle standards, including weight and length limits, are checked at border crossings, as well as at check points within each country. These would be unaffected. Finally, whether for an international movement or permissible cabotage, all carriers are subject to that country's safety regulations, including hours of service.

SUMMING UP

The near-total exclusion of transportation from the Free Trade Agreement and, subsequently, from the NAFTA almost surely has negative effects regarding overall efficiency and production in North America. The particular focus of this paper has been the effective banning of cabotage for trucking. The EU, which allows cabotage for trucking, could serve as an example to judge the merits of freer international trade of trucking services; but it is, for most observers, too far removed.

An approach was proposed for instituting a limited, reversible experiment in cabotage for trucking in Canada and the United States. It would be centered on the Canadian Prairie provinces and U.S. Upper Great Plains states. This is a geographically large area that accounts for a relatively small portion of each country's population and economic activity. It seems likely that the experiment would have positive overall effects for this generally depressed region. Moreover, as the region accounts for small portions of the two economies, costs from any negative distributional effects would likely be minor.

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Intra-NAFTA Trade and Surface Traffic: A Very Disaggregated View

Mark Funk, Erick Elder, Vincent Yao, and Ashvin Vibhakar

This paper studies surface traffic from intra-North American Free Trade Agreement (NAFTA) trade in five Mid-South states using trade data disaggregated by state, industry, and transportation mode. The data reveal that intra-NAFTA trade traffic differs widely across states, industries, and transportation modes. Unfortunately, the aggregated data used in most previous studies of NAFTA sacrifice valuable information about these differences. Accounting for these variations is crucial if analysts seek accurate estimates of the economic relationships within the NAFTA region or seek reliable forecasts of transportation needs. The data demonstrate that, within the NAFTA region, (i) the pattern of surface-transported trade within each state differs across industries; (ii) the pattern of surface-transported trade within each industry differs across states; and (iii) the mode of transport for intra-NAFTA trade depends on the importer, the exporter, and the industry. (JEL F14, R40)

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The signing of the North American Free Trade Agreement (NAFTA) in December 1992 and its implementation starting in January 1994 sparked an enormous effort to measure NAFTA's effects on the NAFTA economies. Most such studies use aggregated data and thus sacrifice valuable information on the differences among states, regions, and industries (e.g., Gould, 1998, and Krueger, 1999). The aggregated approach also prevents economists from drawing clear policy conclusions on the impacts of NAFTA, especially because policymakers often focus narrowly on specific states or specific industries. For example, with the NAFTA-region surface-transported trade rising by 85 percent between 1995 and 2004, the Mid-South transportation infrastructure, public sector agencies, and transportation companies require detailed information on transportation infrastructure needs. While aggre-

gated trade volumes may indicate total transportation needs, trade traffic disaggregated by state, industry, and transport mode could assist planners in determining the future road and rail needs in their specific region.

Using a rich dataset on post-NAFTA trade traffic from the Bureau of Transportation Statistics (BTS), we describe intra-NAFTA trade traffic by transportation mode (truck or rail) since NAFTA's implementation for each 2-digit standard industrial classification (SIC) industry in five Mid-South states (Arkansas, Louisiana, Mississippi, Tennessee, and Texas). Examining the data at this level of disaggregation yields insights into how NAFTA has affected trade and transportation patterns. We uncover many striking differences in the NAFTA-region trade traffic among states, industries, and transportation modes. Analysis at the detailed level can account for these differences and thus should

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Table 1**SIC Industries**

01-09 Agriculture
10-14 Mining
20 Food and Kindred Products
21 Tobacco Products
22 Textile Mill Products
23 Apparel and Other Textile Products
24 Lumber and Wood Products
25 Furniture and Fixtures
26 Paper and Allied Products
28 Chemicals and Allied Products
30 Rubber and Miscellaneous Plastics Products
31 Leather and Leather Products
32 Stone, Clay, Glass and Concrete
33 Primary Metal Industries
34 Fabricated Metal Products
35 Industrial Machinery and Equipment
36 Electrical and Electronic Equipment
37 Transportation Equipment
38 Instruments and Related Products
39 Miscellaneous Manufacturing

improve the accuracy of the estimates of the NAFTA-area economic relationships.

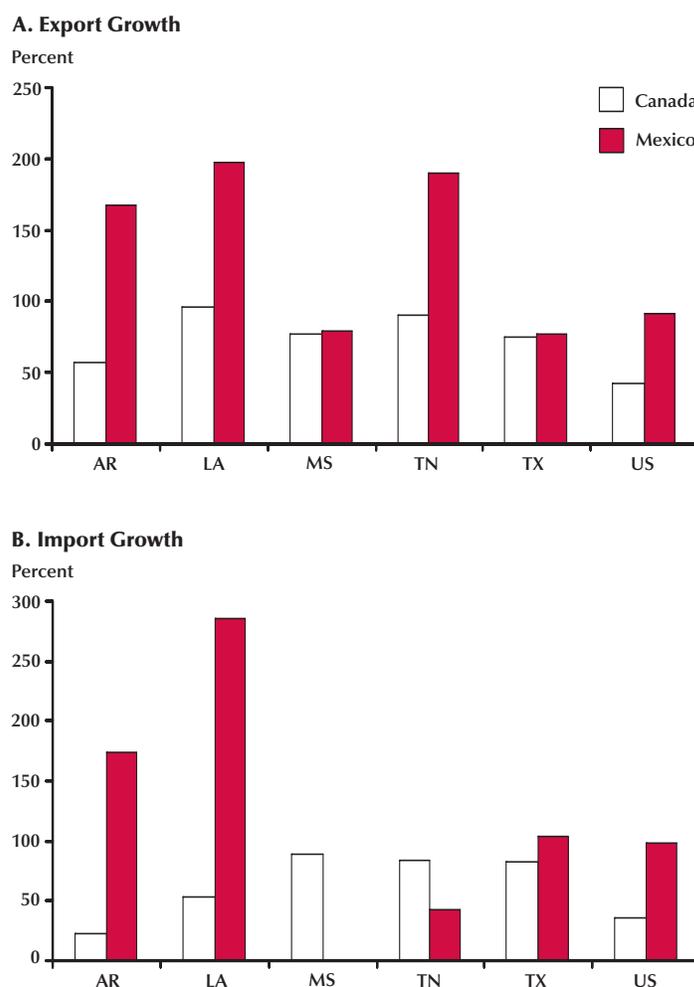
The sections of this article describe the following: the BTS data, the trade traffic by state and industry, the data disaggregated by transportation mode, and the completely disaggregated data. Throughout, we emphasize the disparities in growth since the implementation of NAFTA, details that are unavailable in the aggregated data. The disaggregated data demonstrate that the intra-NAFTA trade traffic differs across states, industries, and transportation modes.

NAFTA DATA

The BTS provides surface transportation data disaggregated to the mode of transport (truck, rail, mail, and pipe) for U.S. imports and exports to Canada and Mexico in the Transborder Freight Database. The BTS reports monthly data at the

industry level using the 2-digit schedule B industry definition for exports and the 2-digit TSUSA industry definition for imports, covering 100 industries. The BTS data are a subset of the U.S. Census trade data and are the best publicly available data on transborder transportation flows. However, the data have some limitations. For example, the recorded mode of transport is the mode in use when the shipment crossed the border. We aggregated foreign destination to national levels from the BTS-provided Canadian province and Mexican state levels, thus reducing concerns over the accuracy of the foreign origin and destination. The trade shipped by mail and pipe were dropped because of the low volume and low frequency of observed trade; water and air shipments are not provided. Thus, the data should not be seen as measuring trade relationships, but rather as measuring the surface traffic from intra-NAFTA trade. We aggregated the data to annual frequency and into 20 SIC 2-digit agricultural, mining, and manufacturing industries (see Table 1 for a list of industries). The data begin in April 1994, thus limiting our sample of full-year observations to 1995-2004. The BTS data did not account for trans-shipments until 1997. Using the BTS statistics on trans-shipments, we adjust the data for the period 1997-2004 to account for trans-shipments. We deflate the data using the CPI (2002 base year).

A few previous studies used regional-level trade data (e.g., Wall, 2003) or state-level trade data (e.g., Coughlin and Wall, 2003) aggregated over industries and mode. Wall (2003) found that the South Central United States enjoyed some of the fastest growth in NAFTA trade, whereas Coughlin and Wall (2003) found that three of the states sampled in this article enjoyed export growth above the national average (Arkansas, Tennessee, and Texas). We aggregate our traffic data to the state level and show cumulative export and import growth of truck- and rail-transported trade during the post-NAFTA period in Figure 1. Of the five Mid-South states, Louisiana experienced the largest growth in surface-transported exports to both Canada and Mexico (96 percent and 198 percent, respectively). Texas experienced the slowest surface-transported export growth to Mexico (77 percent), while Arkansas's surface-transported exports to Canada

Figure 1**Surface-Transported Export and Import Growth, 1995-2004**

grew by only 57 percent. On the import side, Louisiana and Arkansas experienced strong growth in surface-transported imports from Mexico (285 percent and 173 percent, respectively), while Mississippi's surface-transported imports from Mexico showed no growth.

NAFTA TRADE DATA DISAGGREGATED BY INDUSTRY

Aggregating the trade traffic (see Figure 1) across all industries for each U.S. state–foreign

country combination conceals the substantial variation that exists at the industry level. Romalis (2005) finds that the impact of NAFTA varied substantially at the commodity level, particularly in highly protected sectors. We explore this variation at the industry level in a couple of different ways. First, for each U.S. state in our sample, we aggregate the truck and rail exports and imports to and from Mexico and Canada for each industry and calculate the average growth rate for each industry using the geometric mean. Using Arkansas as an example, this aggregation involves adding the Arkansas truck

Table 2**Surface-Transported Export and Import Growth by State, 1995-2004**

State	Median	Standard deviation	Min SIC	Min	Max SIC	Max
A. Export growth						
AR	4.0	8.9	39	-17.8	22	14.2
LA	3.7	10.2	24	-8.8	37	29.6
MS	5.3	9.9	39	-4.6	10-14	33.6
TN	6.4	6.3	24	-4.5	38	23.8
TX	4.2	7.0	21	-17.5	35	11.7
Maximum	6.4					33.6
Minimum	3.7			-17.8		
B. Import growth						
AR	5.9	5.9	33	-1.1	01-09	21.1
LA	7.2	8.6	31	-3.5	34	34.8
MS	7.5	9.1	38	-13.4	34	22.0
TN	9.2	24.8	36	-4.9	21	115.7
TX	7.0	5.3	01-09	-2.5	25	19.2
Maximum	9.2					115.7
Minimum	5.9			-13.4		

NOTE: The fourth column of Table 2A (Table 2B) reports the SIC code of the industry with the lowest average surface-transported export (import) growth rate for a particular state, and the fifth column reports that industry's average growth rate. The sixth column reports the SIC code of the industry with the highest average surface-transported export (import) growth rate for a particular state, and the last column reports that industry's average growth rate.

exports of industry 1 to Mexico, Arkansas rail exports for industry 1 to Mexico, Arkansas truck exports of industry 1 to Canada, and Arkansas rail exports for industry 1 to Canada. The aggregated series is Arkansas surface-transported exports of industry 1, and the average growth rate of this series is calculated. The median export (import) growth rate across a state's 20 SIC industries is reported in the second column of Table 2A (Table 2B). The state with the highest median surface-transported export growth rate was Tennessee, at 6.4 percent; and the state with the lowest median growth rate for surface-transported exports was Louisiana, at 3.7 percent. For the five Mid-South states we examine, Coughlin and Wall (2003) also found that NAFTA's impact on state exports was largest for Tennessee and smallest for Louisiana. For imports transported by surface modes, Tennessee had the highest median growth rate, at 9.2 percent; Arkansas

had the lowest median growth rate, at only 5.9 percent.

Reporting only the median average growth rates masks a substantial amount of variation across states and across industries. The fourth column of Table 2A (Table 2B) reports the SIC code of the industry with the lowest average surface-transported export (import) growth rate for a particular state, and the fifth column reports that industry's average growth rate. The sixth column reports the SIC code of the industry with the highest average surface-transported export (import) growth rate for a particular state, and the last column reports that industry's average growth rate. Finally, the third column reports the standard deviation of the average growth rates across a particular state's 20 industries. For all five states, the difference between the minimum and the maximum growth rates was 28 percent or more. For example, for Arkansas exports,

Table 3**Surface-Transported Export and Import Growth by U.S. State–Foreign Country Combination, 1995-2004**

State	Country	Median	Standard deviation	Min SIC	Min	Max SIC	Max
A. Export growth							
AR	Mexico	9.3	15.5	39	-23.9	30	35.4
AR	Canada	4.9	7.1	39	-13.5	22	14.6
LA	Mexico	13.3	12.5	24	-21.0	10-14	31.9
LA	Canada	2.9	10.8	39	-7.4	37	30.9
MS	Mexico	14.7	25.2	36	-22.1	20	77.0
MS	Canada	6.7	9.0	39	-4.6	10-14	33.8
TN	Mexico	13.3	12.3	24	-19.1	34	34.1
TN	Canada	5.6	6.8	01-09	-5.2	38	23.2
TX	Mexico	4.2	7.6	21	-17.5	35	14.2
TX	Canada	5.7	4.0	39	-7.2	25	14.1
Maximum		14.7					77.0
Minimum		2.9			-23.9		
B. Import growth							
AR	Mexico	8.5	14.9	01-09	-6.8	37	42.2
AR	Canada	4.1	8.5	22	-5.2	01-09	29.2
LA	Mexico	17.8	43.0	24	-18.2	37	175.9
LA	Canada	5.5	9.4	32	-4.2	34	33.1
MS	Mexico	-0.1	23.8	38	-47.5	24	63.8
MS	Canada	8.1	7.7	32	-4.8	34	22.3
TN	Mexico	10.9	19.9	24	-12.1	20	64.1
TN	Canada	6.8	25.3	32	-5.3	21	115.7
TX	Mexico	5.5	6.6	24	-4.0	25	22.3
TX	Canada	7.9	5.6	26	1.9	22	20.4
Maximum		17.8					175.9
Minimum		-0.1			-47.5		

NOTE: The fourth column of Table 3A (Table 3B) reports the SIC code of the industry with the lowest average surface-transported export (import) growth rate for a particular U.S. state–foreign country combination, and the fifth column reports that industry's average growth rate. The sixth column reports the SIC code of the industry with the highest average surface-transported export (import) growth rate for a particular U.S. state–foreign country combination, and the last column reports that industry's average growth rate.

SIC 39 (Miscellaneous Manufacturing) had the lowest average growth rate, at -17.8 percent; SIC 22 (Textile Mill Products) had the highest average growth rate, at 14.2 percent.

The results from a lesser degree of aggregation are reported in Table 3. In that table, unlike Table 2, the Mexico and Canada trade traffic are not aggre-

gated together. Not surprisingly, the greater disaggregation leads to greater variation. The highest median industry growth rate for any state's surface-transported exports was for Mississippi exports to Mexico, at 14.7 percent—followed by Tennessee exports to Mexico and Louisiana exports to Mexico, both at 13.3 percent. The lowest median industry

Table 4**Surface-Transported Trade in 2004 by Mode and U.S. State (2002\$)**

State	Truck exports (\$)	Truck imports (\$)	Rail exports (\$)	Rail imports (\$)
A. Canada trade				
AR	684,267,201	535,451,400	202,652,108	180,653,725
LA	540,396,139	442,323,001	665,276,768	160,882,116
MS	490,497,001	532,416,001	173,136,084	117,522,505
TN	4,083,726,001	2,264,711,000	664,154,653	1,118,257,593
TX	7,490,419,001	4,455,484,900	1,665,231,032	1,638,128,908
B. Mexico trade				
AR	178,779,101	246,335,002	123,725,697	22,910,516
LA	211,021,110	157,577,302	243,201,610	14,016,310
MS	208,129,871	265,056,032	152,102,929	1,971,084
TN	1,276,195,001	2,526,309,361	314,716,088	493,519,204
TX	33,660,047,000	30,234,739,420	5,090,747,901	1,424,301,861

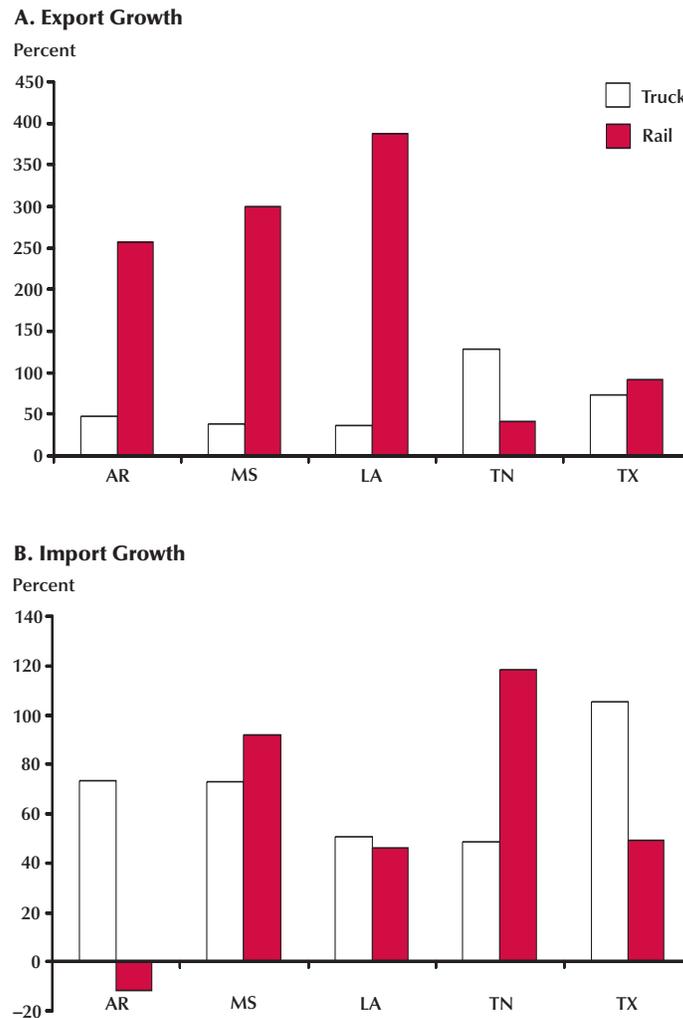
growth rate was for Louisiana exports to Canada, at 2.9 percent. The highest industry average growth rate was for Mississippi exports of SIC 20 (Food and Kindred Products) to Mexico, at 77.0 percent; the lowest industry average growth rate was for Arkansas exports of SIC 39 (Miscellaneous Manufacturing) to Mexico, at -23.9 percent. Texas surface-transported exports to Canada showed the least variation across industries, with a standard deviation of average growth rates of only 4.0 percent; but, even so, there was still a relatively sizeable difference, with an average growth rate of -7.2 percent for SIC 39 (Miscellaneous Manufacturing) and an average growth rate of 14.1 percent for SIC 25 (Furniture and Fixtures). The U.S. state-foreign country combination with the greatest amount of variation across industries (as measured by the standard deviation of the average growth rates) was Mississippi surface-transported exports to Mexico, with a standard deviation of 25.2 percent.

Surface-transported imports exhibit similar variation. The U.S. state-foreign country combination with the highest median industry growth rate was Louisiana imports from Mexico, at 17.8 percent; the U.S. state-foreign country combination with the lowest median industry growth rate was Mississippi imports from Mexico, at -0.1 percent.

The industry with the highest growth rate was Louisiana imports of SIC 37 (Transportation Equipment) from Mexico, at 175.9 percent; the industry with the lowest average growth rate was Mississippi imports of SIC 38 (Instruments and Related Products) from Mexico, at -47.5 percent. The U.S. state-foreign country combination with the greatest variation across industries was Louisiana imports from Mexico, with a standard deviation of 43 percent; Texas imports from Canada exhibited the least amount of volatility, with a standard deviation of average growth rates across industries of only 5.6 percent. Finally, notice that the fastest-growing and slowest-growing industries varied from state to state and by NAFTA partner. For example, Arkansas surface-transported agricultural imports from Canada grew by 29.2 percent, while Arkansas surface-transported agricultural imports from Mexico grew by -6.8 percent. Aggregation over industries or over states masks these details, which are central for state and regional policymakers. Disaggregated data allow state policymakers to assess each industry's trade traffic and its impact on state employment, wages, and taxation—and hence assists policymakers in transportation planning.

Figure 2

Surface-Transported Export and Import Growth by Mode, 1995-2004



NAFTA TRADE DATA DISAGGREGATED BY TRANSPORTATION MODE

Table 4 shows the truck- and rail-transported trade between the Mid-South states and the NAFTA partners.

Texas surface-transported trade was (unsurprisingly) dominated by Mexico. The other Mid-South states had more trade traffic with Canada. The Texas trade traffic dwarfed those of other states, which

suggests that analysis using data aggregated over states may miss vital details for smaller states. Trade by truck was substantially larger than trade by rail for all states except Louisiana; for the other four states, truck exports were at least twice as large as rail exports. However, the real value of trade shipped by rail grew much faster than the trade shipped by truck, especially for Louisiana and Mississippi. Figure 2 shows the cumulative export and import growth by mode for each state during the post-NAFTA period. Table 5 shows the changing

Table 5**Truck Share of Surface-Transported Exports and Imports by U.S. State**

State	Truck share of exports, 1995 (percent)	Truck share of exports, 2004 (percent)	Truck share of imports, 1995 (percent)	Truck share of imports, 2004 (percent)
A. To Canada				
AR	90.0	77.2	61.0	74.8
LA	71.0	44.8	77.1	73.3
MS	92.9	73.9	80.7	81.9
TN	77.1	86.0	77.9	66.9
TX	83.8	81.8	68.1	73.1
B. To Mexico				
AR	69.4	59.1	96.8	91.5
LA	68.4	46.5	97.8	91.8
MS	80.1	57.8	94.3	99.3
TN	78.5	80.2	84.3	83.7
TX	87.8	86.9	93.7	95.5

share of trade with Canada and Mexico transported by truck during the post-NAFTA period. For most states, the share of exports by truck declined during the post-NAFTA period. The pattern is not as clear with surface-transported imports. The data also show that a larger share of Mexican traded goods was transported by truck, especially for imports from Mexico.

However, this level of aggregation obscures substantial industry-level variation. Table 6 shows the export and import growth by U.S. state–mode combination (compared with export and import growth by U.S. state–foreign country combination, which is shown in Table 3). As in Table 3, there is substantial variation across states and industries. The U.S. state–mode combination with the highest median industry growth rate for exports was Mississippi rail exports, at 21.7 percent; the lowest was for Tennessee rail exports, at –1.3 percent. There is also a substantial amount of variation within a given state. While Mississippi rail exports clearly has the widest variation in industry growth rates (using the range or the standard deviation of the estimated industry growth rates as a measure of dispersion), eight of the ten U.S. state–mode export combinations have a range in excess of 27

percent or more *per year* between the fastest-growing industries and the slowest-growing industries.

On the import side, the U.S. state–mode combination with the highest median industry growth rate was Tennessee truck imports, at 11.9 percent; the lowest was Arkansas rail imports, at only 0.1 percent. The U.S. state–mode combination with the least variation was Arkansas truck imports, with a standard deviation of average growth rates of 6.5 percent; the greatest variation was Tennessee truck imports, at 24.9 percent. Texas rail imports of SIC 34 (Fabricated Metal Products) had the lowest average growth rate, at –39.1 percent; Tennessee truck imports of SIC 21 (Tobacco) had the highest average growth rate at 115.7 percent. In general, exports and imports were much more volatile for rail shipments than for truck shipments. Again, notice that the fastest- and the slowest-growing industries varied from state to state and by NAFTA partner. Aggregation over industries or over states complicates transport planning by masking the volatility of the trade traffic.

Disaggregating to the industry level provides insight into these growth patterns as shown in Figure 3, panels A and B. SIC 10-14, SIC 28, and SIC 30 (Food, Chemicals, and Rubber, respec-

Table 6**Surface-Transported Export and Import Growth by U.S. State–Mode Combination, 1995-2004**

Mode	State	Median	Standard deviation	Min SIC	Min	Max SIC	Max
A. Export growth							
Truck	AR	4.9	8.6	39	-17.8	22	14.7
Truck	LA	3.7	7.7	37	-7.3	34	20.5
Truck	MS	1.9	8.3	37	-9.2	10-14	24.2
Truck	TN	7.9	6.5	24	-3.8	38	23.8
Truck	TX	4.2	7.3	21	-17.5	35	12.2
Rail	AR	11.2	10.9	10-14	-4.8	36	34.3
Rail	LA	4.7	31.4	36	-27.5	37	103.5
Rail	MS	21.7	43.7	26	-13.0	37	164.4
Rail	TN	-1.3	8.5	39	-26.4	37	9.3
Rail	TX	5.5	12.0	23	-25.2	26	18.8
Maximum		21.7					164.4
Minimum		-1.3			-27.5		
B. Import growth							
Truck	AR	5.7	6.5	10-14	-0.7	01-09	24.2
Truck	LA	4.2	9.3	31	-4.8	34	34.8
Truck	MS	7.7	8.8	38	-13.4	34	21.9
Truck	TN	11.9	24.9	36	-5.0	21	115.7
Truck	TX	7.5	7.4	01-09	-2.2	21	27.2
Rail	AR	0.1	13.0	37	-17.7	10-14	30.6
Rail	LA	6.6	20.8	37	-34.0	32	49.1
Rail	MS	5.7	18.1	35	-28.6	28	31.5
Rail	TN	9.7	20.3	32	-24.0	25	49.2
Rail	TX	1.8	15.1	34	-39.1	32	24.8
Maximum		11.9					115.7
Minimum		0.1			-39.1		

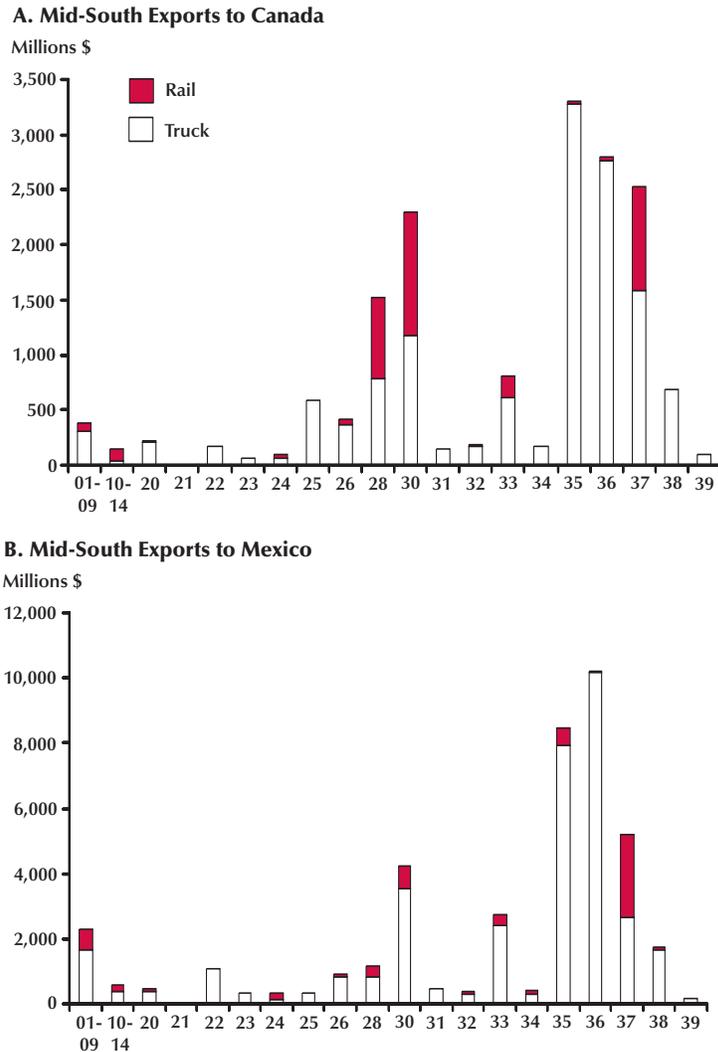
NOTE: The fourth column of Table 3A (Table 3B) reports the SIC code of the industry with the lowest average surface-transported export (import) growth rate for a particular U.S. state–mode combination, and the fifth column reports that industry's average growth rate. The sixth column reports the SIC code of the industry with the highest average surface-transported export (import) growth rate for a particular U.S. state–mode combination, and the last column reports that industry's average growth rate.

tively) were much more likely to be exported by rail if the destination was Canada than if the destination was Mexico. SIC 28 and SIC 30 (Chemicals and Rubber, respectively) have a larger share of surface-transported Canadian trade than of surface-transported Mexican trade. Note that while Mexico dominated the Mid-South surface-transported trade for most industries, Texas was the only Mid-

South state to trade by truck and rail more with Mexico than with Canada.

Similarly, the disaggregated data shown in Figure 4, panels A and B, provide insight into why Mid-South imports from Mexico were more likely to be shipped by truck than were imports from Canada. Just over 50 percent of all Mid-South imports from Mexico—but less than 20 percent of

Figure 3
Mid-South Surface-Transported Exports, 2004



all Mid-South imports from Canada—were from industries SIC 35 and SIC 36 (Industrial Machinery and Electrical Equipment, respectively). These two industries tended to ship by truck, for both Canada and Mexico, whether a Mid-South export or import. Similarly, a relatively large proportion of Mid-South imports from Canada come from industries SIC 28, SIC 30, and SIC 33 (Chemicals, Rubber, and Primary Metals, respectively); a relatively large proportion these industries’ shipments travel by rail, regardless of the NAFTA source and destination.

NAFTA TRADE DATA COMPLETELY DISAGGREGATED

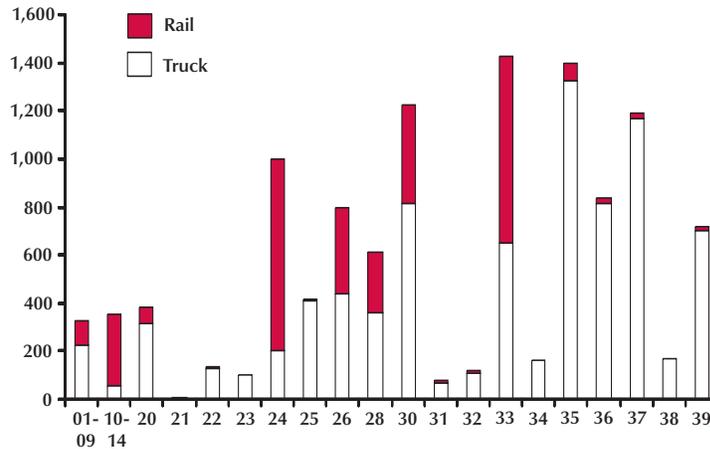
Table 7, panels A and B, report data disaggregated by state, country, and mode of transportation. The U.S. state–mode–foreign country combination with the highest median growth rate for truck exports was Tennessee exports to Mexico (16.0 percent), while the combination with the highest median growth rate for rail exports was Mississippi rail exports to Canada (17.7 percent). The combina-

Figure 4

Mid-South Surface-Transported Imports, 2004

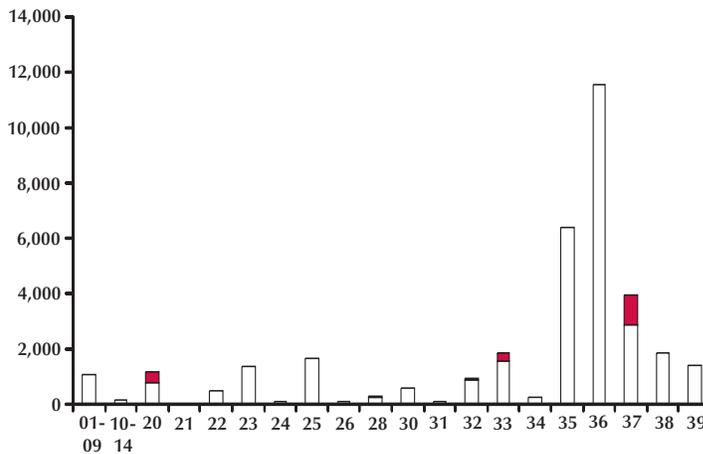
A. Mid-South Imports from Canada

Millions \$



B. Mid-South Imports from Mexico

Millions \$



tion with the lowest median growth rate for truck exports was Mississippi exports to Mexico (0.8 percent), and the lowest median growth rate for rail exports was for Tennessee exports to Mexico (-3.0 percent). Once again, the difference, for a given U.S. state-mode-foreign country combination, between the fastest- and slowest-growing industries is striking. For exports, 16 of the 20 U.S. state-mode-foreign country combinations had a 30 percent per year difference between the minimum and the maximum industry-level growth rates.

This enormous difference shows the importance of using disaggregated trade data. Table 7, panel B, contains similar results using import data.

CONCLUSION

Most studies of NAFTA use aggregated data and thus sacrifice valuable information on crucial differences among states, regions, and industries. The aggregated approach prevents economists from

Table 7**Surface-Transported Exports and Imports Growth by U.S. State–Mode–Foreign Country Combination, 1995-2004**

Mode	State	Country	Median	STD	Min SIC	Min	Max SIC	Max
A. Exports growth								
Truck	AR	Mexico	8.1	14.7	39	-23.9	30	35.3
Truck	AR	Canada	3.0	6.9	39	-13.5	22	15.1
Truck	LA	Mexico	8.7	11.0	36	-17.6	20	26.4
Truck	LA	Canada	1.9	8.8	37	-8.8	23	22.5
Truck	MS	Mexico	0.8	17.7	36	-22.1	31	35.3
Truck	MS	Canada	6.5	8.0	37	-9.6	10-14	24.6
Truck	TN	Mexico	16.0	13.2	24	-19.7	34	34.2
Truck	TN	Canada	6.4	6.5	39	-2.4	38	23.2
Truck	TX	Mexico	4.0	8.0	21	-17.5	35	15.1
Truck	TX	Canada	5.5	4.2	39	-7.1	25	14.1
Rail	AR	Mexico	14.2	31.3	28	-5.1	33	70.4
Rail	AR	Canada	11.1	9.5	10-14	-4.8	28	27.6
Rail	LA	Mexico	20.0	23.6	24	-27.0	10-14	52.5
Rail	LA	Canada	2.3	15.9	39	-23.0	10-14	32.5
Rail	MS	Mexico	3.0	2.9	01-09	-2.2	28	4.3
Rail	MS	Canada	17.7	23.3	26	-13.7	36	64.2
Rail	TN	Mexico	-3.0	30.1	24	-18.3	37	87.1
Rail	TN	Canada	-1.7	10.2	32	-26.3	24	5.7
Rail	TX	Mexico	6.3	13.1	23	-25.2	30	23.6
Rail	TX	Canada	3.8	11.4	31	-17.6	36	22.7
Maximum			20.0					87.1
Minimum			-3.0			-27.0		
B. Imports growth								
Truck	AR	Mexico	10.0	13.6	31	-5.6	37	42.1
Truck	AR	Canada	3.9	9.2	22	-5.4	01-09	32.5
Truck	LA	Mexico	13.0	44.0	24	-19.2	37	175.9
Truck	LA	Canada	5.7	9.6	31	-4.9	34	33.1
Truck	MS	Mexico	2.1	26.1	38	-47.5	24	79.2
Truck	MS	Canada	7.9	7.3	32	-4.8	34	22.3
Truck	TN	Mexico	13.0	19.6	24	-11.8	20	64.1
Truck	TN	Canada	5.9	25.3	10-14	-2.3	21	115.7
Truck	TX	Mexico	7.5	7.6	24	-3.7	10-14	24.2
Truck	TX	Canada	7.9	5.6	26	2.0	22	20.4
Rail	AR	Mexico	-13.1	0.0	28	-13.1	28	-13.1
Rail	AR	Canada	0.1	14.8	37	-24.7	10-14	30.6
Rail	LA	Mexico	0.0	0.0	0	0.0	0	0.0
Rail	LA	Canada	6.6	18.7	37	-35.6	01-09	17.7
Rail	MS	Mexico	0.0	0.0	0	0.0	0	0.0
Rail	MS	Canada	3.7	18.3	35	-28.8	28	32.7
Rail	TN	Mexico	16.0	8.6	37	9.9	33	22.1
Rail	TN	Canada	9.7	21.9	32	-24.1	25	49.2
Rail	TX	Mexico	-5.6	20.9	25	-34.1	32	28.9
Rail	TX	Canada	2.8	11.7	34	-34.2	22	19.0
Maximum			16.0					175.9
Minimum			-13.0			-47.5		

NOTE: The fourth column of Table 3A (Table 3B) reports the SIC code of the industry with the lowest average surface-transported export (import) growth rate for a particular U.S. state–mode–foreign country combination, and the fifth column reports that industry's average growth rate. The sixth column reports the SIC code of the industry with the highest average surface-transported export (import) growth rate for a particular U.S. state–mode–foreign country combination, and the last column reports that industry's average growth rate. The "minimum" median is reported as -2.6 in the table even though there is a -24.1 for Arkansas rail imports from Mexico; but there is only one industry for that category.

drawing clear conclusions on the narrow policy issues, such as transportation planning, of most interest to state and regional policymakers. Using a rich dataset on post-NAFTA trade traffic from the BTS, we uncover many striking differences in intra-NAFTA trade traffic between states and industries and by transportation mode.

Within each state, the pattern of surface-transported intra-NAFTA trade differed substantially across industries. For example, Mississippi exports to Mexico showed industry-level growth rates ranging from -22.1 percent to 77.0 percent, with a standard deviation of 25.2 percent. Within each industry, the trade traffic varied across states. As shown in Table 7B, for SIC 24 (Lumber and Wood Products) Louisiana truck imports from Mexico fell by -19.2 percent annually, while Mississippi truck imports from Mexico rose 79.2 percent annually. Results like these support Coughlin and Wall's (2003) conclusions on the importance of firm mobility in determining the effects of NAFTA on state-level trade. Further, the transportation mode of intra-NAFTA trade depended on the state and the industry. Of the 20 possible U.S. state-mode-foreign country combinations we examined for the Mid-South, 16 showed export growth rates differing at the industry level by 30 percent or more per year. In general, exports and imports were much more volatile for rail shipments than for truck shipments. All of these differences highlight the need to disaggregate the data to draw policy-relevant conclusions.

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Transportation Infrastructure, Retail Clustering, and Local Public Finance: Evidence from Wal-Mart's Expansion

Michael J. Hicks

The author examines the role highway infrastructure and local property tax rate variability play in retail agglomeration in Indiana from 1988 through 2003. To account for data errors and the potential endogeneity of taxes and infrastructure on retail agglomeration, he introduces a unique identification strategy that exploits the entrance timing and location of Wal-Mart stores in Indiana. Using a time-series cross-sectional model of Indiana's 92 counties from 1988 through 2003, he estimates the impact highway infrastructure, property taxes, and big-box competition have in creating regional agglomerations. Among two separate specifications and a full and rural-only set of the data, the author finds considerable agreement in the results. In the full sample, he finds no relationship between property tax rates or highway infrastructure and retail agglomeration. Within the non-metropolitan statistical area (MSA) counties, this relationship is very modest, though it possesses considerable statistical certainty. Highway impacts within the non-MSA counties are significant and positively related to retail agglomeration, with the presence of highways explaining about 10 percent of total agglomeration variability. (JEL R11, R53)

Federal Reserve Bank of St. Louis *Regional Economic Development*, 2006, 2(2), pp. 100-14.

The role of public infrastructure and local public finance in a firm's location decision is an appropriately enduring theme of regional economic analysis. It is also of continuing interest to policymakers at all levels of government. Of broad interest also is the role taxes and public infrastructure play in generating industry agglomeration and growth. In keeping with this level of interest, this paper reports evidence of the impact of tax rates and public infrastructure on local agglomeration in the retail sector. In so doing, I highlight the application of a statistical technique for avoiding one of the most common estimation problems encountered in this type of research—the potential simul-

taneous determination of agglomeration, infrastructure, and taxes.

One concern inherent in any of these studies is endogeneity of public infrastructure investment and public finance decisions—primarily tax rates and industry agglomeration. Endogeneity (also referred to as simultaneity) occurs when the variable being estimated (such as local agglomerations) also simultaneously effects one of the variables used to estimate its presence (such as tax rates). Such bi-directional causation violates the assumptions of the most common statistical methods employed to estimate impacts. Indeed, together with data errors, this problem has been characterized by one leading reviewer as the

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dominant challenge to this type of research (Wasylenko, 1997).¹

This paper addresses the role transportation infrastructure and property tax rates play in retail agglomeration in Indiana. I also provide a description of the general changes to the retail sector in Indiana. To correct for the endogeneity concern with regard to retail agglomeration with public infrastructure and taxation, I employ a unique identification strategy that captures active firm entrance decisions by the nation's leading retail firm, Wal-Mart.

To examine this issue, I review recent studies of the role transportation and public finance play in local agglomeration. I then provide a theoretical description and an empirical model of agglomeration economies and outline my instrument selection. This is followed by a discussion of the data, econometric considerations, and estimation results. I conclude by providing an explanation of the results and routes for further analysis. Before proceeding, it is important to clearly frame the problem I try to solve and outline my strategy and assumptions.

THE RESEARCH STRATEGY

The retail sector is enjoying a resurgence of interest from policymakers. Because retail, like the service sector, is subject to less capital mobility, it factors into an increasing number of economic development investment efforts. Also, a well-developed retail sector is often viewed as an important local amenity that helps attract workers and commerce (Gibson, Albrecht, and Evans, 2003). Regional economists are showing increased interest in the retail sector both because of newfound policy interest and the dramatic changes that have occurred in the sector over the past two decades. These changes are heavily associated with Wal-Mart's expansion.²

The strategy I pursue in this paper is to evaluate how local tax rates and public infrastructure may influence agglomeration. To do this I focus analysis

narrowly on a single infrastructure measurement and limited tax instruments. This process of narrowly examining tax and infrastructure impacts can bias estimates (by omitting important contributing variation), which I seek to avoid by limiting my analysis to a single state—Indiana. The choice of Indiana is motivated by the statewide homogeneity of relevant public finance structure, with the exception of property tax rates, on which I focus my analysis. Unfortunately, this approach suffers the problem of simultaneous determination of agglomeration and tax rates. To address this I exploit the variability in entrance location and timing of the region's leading retailer, Wal-Mart.

The second concern I address is in my infrastructure measurement. Because I acknowledge the possibility that public infrastructure, broadly defined, plays a role in retail agglomeration, I would prefer to employ measures of infrastructure that fully capture these impacts. Unfortunately, the flow of benefits from public infrastructure is poorly measured.³ To circumvent this, I also use the timing and location of Wal-Mart to correct for this problem. Relegating the econometric discussion to later sections, I follow with a discussion of the problems of endogeneity in public infrastructure and taxation.

The third method I employ is to both structure my model to account for location fixed effects and to estimate separately the full sample of 92 Indiana counties; in a separate regression, I limit my estimation to non-MSA counties. The former consideration was made in response to several leading critics of this type of model, who point out the need for cross-sectional fixed effects (Holtz-Eakin, 1994; and Evans and Karras, 1996). The latter approach is mimicked by Chandra and Thompson (2000), who also examine highway impacts on economic growth at the county level.

ENDOGENEITY IN PUBLIC INFRASTRUCTURE, TAXATION, AND DATA QUALITY

Estimates of the role public infrastructure and taxation play on local economic conditions such

¹ Some researchers, such as Chandra and Thompson (2000), test for and reject endogeneity but purposefully restrict their sample to non-metropolitan statistical area (MSA) counties where highway infrastructure would be simply part of a network, not a node.

² For extensive discussions of Wal-Mart, see Stone (1995 and 1997); Hicks and Wilburn (2001); Basker (2005); Neumark, Zhang, and Ciccarella (2005); and Hicks (2006).

³ See arguments by Fox and Porca (2001).

Hicks

as retail agglomeration are plagued by the potential for endogeneity and data quality concerns. For example, are highways constructed to exploit existing retail patterns, or do they spawn agglomeration? Are property taxes intentionally kept low to foster capital investment, or are they low due to the influence of a politically active business sector? Also, are public infrastructure data, such as the presence or extent of highways, sensitive to local quality differences?

These and similar questions persistently darken much regional economic analysis, and studies of public infrastructure and taxation often treat the problem. Early criticism of studies that did not account for endogeneity include Holtz-Eakin (1994), Evans and Karras (1996), and later Chandra and Thompson (2000). Each of these researchers attempted to avoid endogeneity through specification techniques in panel models (regional fixed effects) or through exclusion of the most problematic data points (for example, MSA counties). I will incorporate both techniques and extend the method to an instrumental variable method that enjoys growing popularity.

The instrumental variable panel method employs multiple equations that evaluate cross sections (such as counties or states) over time. These models may directly outline a structural relationship, use lagged dependent variables, or combine these techniques to account for the endogeneity of the variable under consideration. This is accomplished by first estimating the dependent variable (the first stage) and then estimating the impact of the explanatory variables on the adjusted dependent variable (the second stage). The process is sometimes repeated (a third stage).⁴ Unfortunately, though this process is very widely employed, there are several limitations. First, there are no clear mathematical methods that generate an unambiguous choice of the structure of the first equation. The reason is that an appropriate instrument is correlated with the dependent variable, but not the error term (which is not known). This means that the structure of the equation must be supported

⁴ The two-state method is known as two-stage least squares (2SLS); with the additional step, it is three-stage least squares (3SLS). Both are also estimated using techniques other than least squares (most usually the maximum likelihood method).

heuristically or purely theoretically (most often the former). Second, the most contentious debates in empirical economics in the past year involve the instrument choice.⁵

Despite these limitations (and indeed in the face of counterevidence of endogeneity) many researchers prefer the incorporation of direct corrections of endogeneity in the estimation. This may perhaps be recommended because, in a panel setting, two more palatable improvements on the estimation process are available. The first is a simple first-stage estimate, which includes the lagged independent variables. This process is viewed almost as a default approach in panel models because the direct causal link is indeed broken (it may be argued that no variable in time t determines another variable in time $t-1$).⁶ Second, the use of panel models in general, and instrumental variable methods in particular, are widely viewed as more robust to errors in data than other econometric techniques. Despite these drawbacks of these techniques, their use in this setting is especially appropriate. It is this method I employ to estimate agglomeration of retail trade.

AGGLOMERATION, GROWTH, AND PUBLIC INFRASTRUCTURE

A number of research efforts to identify public infrastructure's role in agglomeration and growth have appeared in recent years. Transportation infrastructure is often part of broader studies for both policy and technical reasons. Wasylenko (1997) provides a key review of findings, as does Fox and Porca (2001), with the latter focusing on rural growth and the former reviewing the broad literature. Empirical studies include Eberts (1991) and Fox and Murray (1990).

Studies specifically examining the agglomeration/growth nexus include Chandra and Thompson (2000) and Hicks (2002 and 2005b). The former

⁵ A recent front page *Wall Street Journal* article concerns Caroline Hoxby's research on school choice (using streams to adjust for the endogeneity of school districts) and an emerging debate on Wal-Mart's impact. See Dube, Edilin, and Lester (2005), Hicks (2006), and the economics section of *The Economist* for this debate.

⁶ This relationship is described as a predetermined, not strictly exogenous relationship.

authors evaluate county-level impacts of interstate highways in a quasi-experimental panel setting. They find that the construction of highways leads to aggregate economic growth in counties with the interstate and that selected sectoral earnings increase. (Notably, for our purposes, these include retail trade.) They also find that counties adjacent to interstate highways experience a decline in many of the same sectors, suggesting an inter-regional reallocation. This study is consistent with findings by Holtz-Eakin (1994), whose state-level study found no net increase in economic activity associated with highway construction.⁷ Hicks (2002 and 2005b) examines firm-level productivity along an Appalachian development corridor. Employing three different models, he finds three distinct but related effects. In the first model (a panel vector autoregression), he finds that there is considerable evidence of leakage associated with the construction of a highway. In the second model, in which he tests convergence in a fixed-effects panel model, he finds that even with the leakages, regions tend to experience sectoral-share convergence, suggesting that the net impact of the infrastructure is greater than zero. In the final model (a CES [constant elasticity of substitution] production function), he finds a modest aggregate productivity increase associated with proximity to the highway, amounting to roughly 1 percent per mile. Notably, he finds considerable cross-industry variation. The proximity of the findings in Chandra and Thompson (2000) and Hicks (2002 and 2005b) speak to a familiar story of some potential growth associated with highway construction, but matched by considerable inter-regional reallocation of trade.

More generally, the results of growth on agglomeration are mixed. An example of the difference in similarly focused studies is Harmatuck (1996), who found output elasticities of public investment to average 0.03. These findings were largely supported by Holtz-Eakin and Schwartz (1994), who found little evidence of meaningful linkages between marginal increases in public investment and output changes in private sector economic activity.

Other research does find sector-specific linkages, most often in manufacturing. Morrison and Schwartz (1991) find modest increases in manufacturing output associated with aggregate public infrastructure. The retail literature focuses on the transactions costs associated with shopping. The resulting cost savings to consumers are often explained as demand externalities (see Eppli and Benjamin, 1994). More recent studies are turning to supply linkages (Cho, Sohn, and Hewings, 2000), a seemingly important area of inquiry given the dominant role supply chains play in big-box retail locations.⁸

Variations in the type of agglomeration may also be a factor in the type of public infrastructure impacts. Localization economies (the regional concentration of an industrial sector) may lead to regional scope economies, which share inputs or exploit spillovers to reduce costs that result in one type of agglomeration (see Fujita and Thisse, 1996, for a description of agglomerations). Malmberg and Maskell (2001) note this phenomenon, which the retail literature refers to as demand externalities.

Agglomeration of population due to concentration in urban areas potentially reduces cost through scale economies, which are obviously a growing characteristic of the retail sector in recent years. For example, Boyd (1997) reports the average retail firm size (in terms of sales) grew 40 percent from 1997 to 1992, while the number of firms declined from over 1.8 million to almost 1.4 million. This trend has continued.

Both types of agglomeration should yield similar results in aggregate industry estimates, so I loosely refer to them together for the remainder of this paper. Whichever definition of agglomeration is employed, a far less theoretical concern is the quality of data used in a model. What constitutes a road and, more importantly, what generates a flow of services are difficult to capture in the types of data sets that are publicly available. One clear example is that two census tracts (or indeed two counties) may enjoy the presence of an interstate highway, but only the tract with an exit will experience any local benefit in retail trade. Thus, even

⁷ Munnell (1990) and Rephann and Isserman (1994) identified leakages along public infrastructure, which is consistent with both Chandra and Thompson (2000) and Holtz-Eakin (1994).

⁸ See also Gulyani (2001).

Hicks

fairly precise data on infrastructure may poorly measure benefits.

Despite these limitations, some researchers have examined public infrastructure with some success. Carlino and Mills (1987), using a two-stage least-squares model, found that gross measures of highway infrastructure positively affected aggregate growth rates. Rainey and Murova (2004) found roads provide a direct link to growth in a regional Cobb-Dougllass production function. One result of this is the development of local agglomeration. It is not clear, however, that these studies do not suffer from simultaneity or endogeneity bias.⁹

These authors all specified their empirical model in different ways, asserting either production relationships or supply relationships leading to regional variation in a number of measures of interest. The tendency of the literature to focus on manufacturing likely motivates these choices. For the retail sector, agglomeration resulting from travel costs is a clearer presentation. To illustrate the point, it is useful to deal with a description of travel costs. Adapting from Madden and Savage (2000), I posit two inverse demand functions:

$$(1) \quad \begin{aligned} P_s &= \alpha_1^s + \alpha_2^s Q_s \\ P_d &= \alpha_1^d + \alpha_2^d Q_d \end{aligned}$$

with travel costs represented as the difference between the competitive prices for each equation such that $T = P_d - P_s$. The equilibrium conditions are, hence,

$$(2) \quad Q^* = (\alpha_1^d - \alpha_1^s - T) / (\alpha_2^d + \alpha_2^s);$$

the first derivative of equilibrium output with respect to travel costs is then

$$(3) \quad \frac{\partial Q^*}{\partial T} = -1 / (\alpha_2^d + \alpha_2^s),$$

which is obviously non-positive. Extending this analysis regionally, one can see informally that if $T > \alpha_1^d - \alpha_1^s$ in equilibrium, there will be no local retail. Hence, agglomeration will occur in locations

with lower transactions costs. (Notably this model could trivially extend this example to taxes.)

In summary, though there are mixed findings about infrastructure across the literature (much of which I have not reviewed), there is at least tentative (and theoretical) evidence of a role of infrastructure in local agglomeration, even if the aggregate general equilibrium effects are not clear. A familiar story might be that infrastructure improves productivity (hence growth), but also reallocates economic activity, which potentially net out. The research also relies on the rather crude estimates of infrastructure to populate the model. However, the role of this paper is not to provide yet another estimate of this relationship broadly, but instead to exploit a unique method of estimating regional variations in public infrastructure not represented clearly by the data. To do this it is also useful to understand the relationship between agglomeration and taxation.

Agglomeration and Taxation

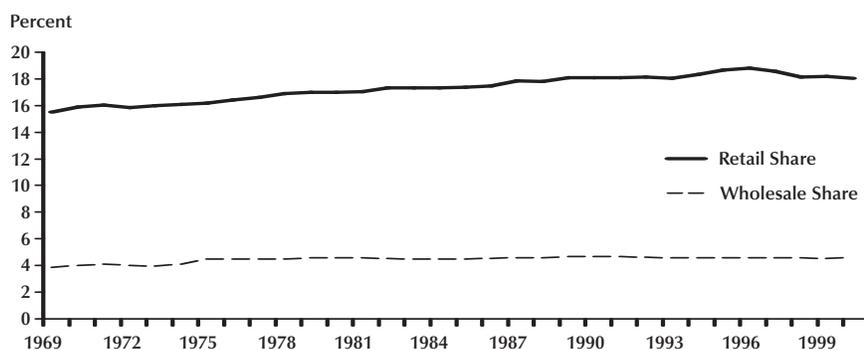
As with public infrastructure's role in generating agglomeration, the effects of tax structure on economic development is widely researched. Most studies of local taxation and commercial economic activity focus on footloose industries, such as manufacturing and research and development, that should be more sensitive to local tax issues. Bartik's (1991) review of existing studies finds that tax elasticity of output ranges from -0.1 to -0.6 , with the mean at about -0.3 . Other studies include Carlton (1979), Bartik (1985), and Helms (1985), all of whom found property taxes to significantly influence firm location decisions.¹⁰ Few studies have examined property tax rates and retail. Thus, for a population-linked industry such as retail, an ideal choice of tax instrument is local varying property taxes of the type found in Indiana. Tax rates (or calculated effective tax rates) are the dominant measure of taxation in these studies.

As with the issue of public infrastructure, this paper seeks to extend the literature through the application of an instrumentation choice, which serves two purposes: control data errors and eliminate endogeneity.

⁹ Additional empirical studies that find a link between infrastructure and growth include Deno (1988), Duffy-Deno and Eberts (1989), Garcia-Mila and McGuire (1992), and Carlino and Voith (1992). Additional empirical studies that reject these linkages include Holtz-Eakin and Schwartz (1994) and Holtz-Eakin and Lovely (1996).

¹⁰ See Ondrich and Wasylenko (1993) and Hines (1996) for examples of studies of corporate tax and industry location.

Figure 1
Retail Employment



SOURCE: Bureau of Economic Analysis, Regional Economic Information System.

Table 1
Percent Difference in Retail Trade Subsector Share, 2000 (Indiana – U.S.)

	Establishments	Sales (\$1,000s)	Payroll (\$1,000s)	Employees
Motor vehicles and parts	1.9	0.5	-0.1	-0.4
Furniture and home furnishings	-0.1	-0.6	-0.4	-0.4
Electronics and appliance stores	0.0	-0.6	-0.5	-0.3
Building and garden equipment	1.7	1.2	1.8	1.2
Food and beverage	-2.0	-2.7	-2.8	-2.9
Health and personal care stores	-0.6	0.0	0.0	-0.1
Gasoline stations	0.6	1.2	1.0	1.2
Clothing and clothing accessories	-2.3	-1.7	-2.0	-2.1
Sporting goods	-0.1	-0.7	-0.6	-0.8
General merchandise	0.5	1.9	2.8	3.9
Miscellaneous retailers	0.4	-0.3	0.0	0.0
Non-store retailers	0.1	1.7	0.9	0.7

SOURCE: Bureau of the Census, County Business Patterns, and author's calculations.

The Retail Sector in Indiana

Indiana's retail sector and the upstream wholesale sector have garnered an increasing share of the state's employment over the past three decades. This is consistent with national trends (see Figure 1), and the mix of retail activity is not dissimilar from the national mix (see Table 1).

Regionally, retail has become very modestly

less agglomerated in the recent two decades, with the maximum of the Gini coefficient declining, but with almost no change in the median of the Gini. Another inequality measure, Theil's T, provides similar results, with very little change in the mean inequality, but with some reduction in extremes. (See Figure 2.)

As with much of the nation, a significant change

Figure 2
Retail Gini Index

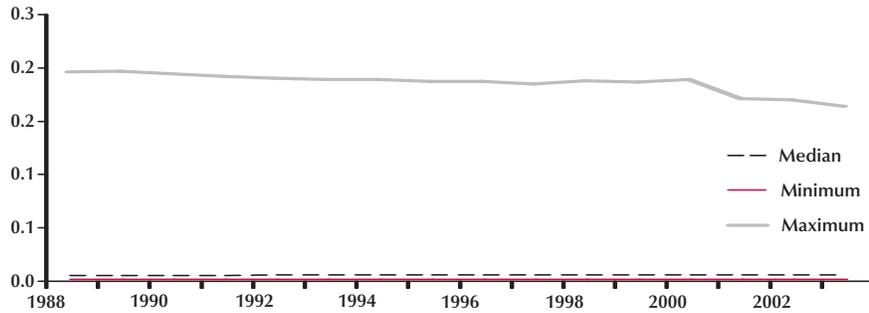
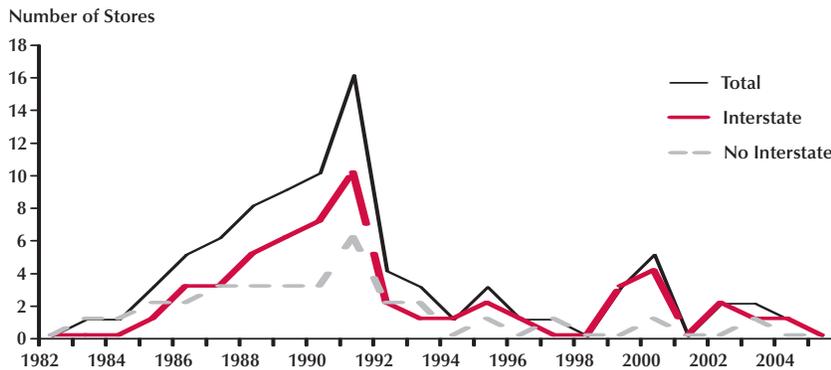


Figure 3
Wal-Mart's Entrance in Indiana Counties



to the retail industry has been the growth of Wal-Mart stores across Indiana. Wal-Mart's expansion since 1962 has been a much heralded wave emanating from Bentonville, Arkansas, toward the coasts. Although the structure of the entrance decisions have been hotly debated, it is clear that the retailer enters a state proximally to regional distribution centers and fills the void between stores quickly, in perhaps 3 to 5 years. Wal-Mart's entrance at the state level is marked by a surge of stores, as can be seen in Figure 3. Note the difference in magnitude between entrance in counties with and without interstates (just under half of Indiana counties have interstate highways). Notably, following the initial

burst of entrance, only four Wal-Mart stores are located in counties without interstate highway access.

Figure 4 provides a geographic snapshot of the entrance of Wal-Mart stores since 2000. The cumulative impact of Wal-Mart's presence since 1983 then illustrates the result of this burst of entrance, followed by the lower persistence of Wal-Mart stores entering in predominantly interstate-accessible counties, a pattern that differed from the early focus predominance of entrance into non-interstate counties. (See Figure 5.)

The patterns evidenced by higher retail trade shares and entrance by Wal-Mart accompany a

decrease in spatial distribution differences in retail trade. This is probably best exemplified through an examination of the Moran's I for retail employment in the state. Moran's I is a measure of local spatial autocorrelation and is represented as

$$(4) \quad M_i(\theta) = \frac{n \sum_{i=1}^n \theta_i \sum_{j=1}^n \widehat{W} \theta_j}{\widehat{W} \sum_{j=1}^n \theta_j^2},$$

where θ is the Gini index of retail employment. Moran's I is a straightforward estimate of the degree of local spatial autocorrelation in retail employment inequality in Indiana's counties. This Moran's I was calculated annually for each year in the 1988-2004 period. As is clear from Figure 6, Indiana has experienced a large reduction in spatial autocorrelation in retail unemployment.

One conclusion to be drawn from the evidence of spatial agglomerations, Wal-Mart entrance, and the spatial autocorrelation of retail employment inequality is that the increase in retail's share of employment results in more spatially even distribution in retail accessibility. This is consistent with, among other things, a general reduction in transportation-related transactions costs in retail shopping (at the intercounty level). Of course, a significant proportion of any retail shopping travel occurs within counties and is not addressed in this analysis.

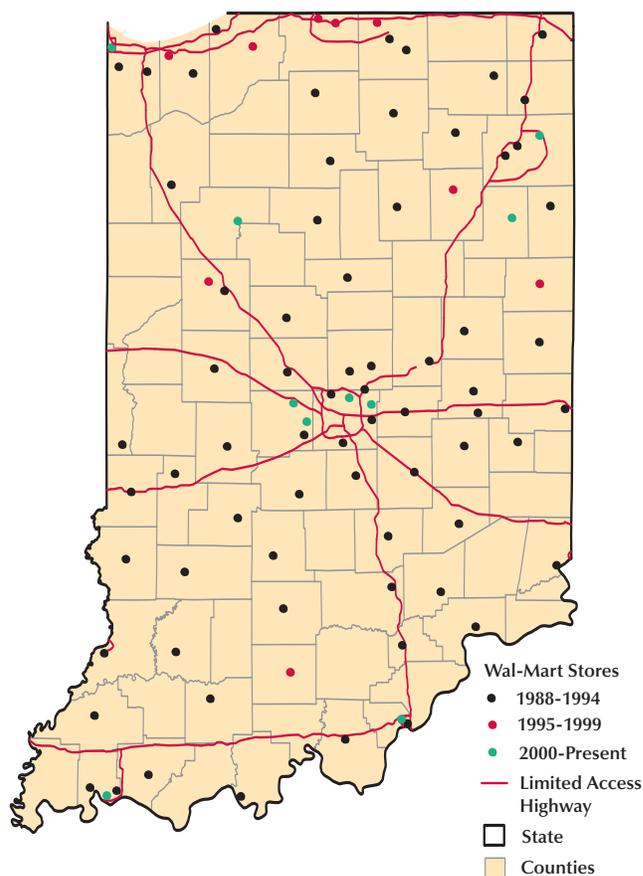
Another facet of this phenomenon is that the growth in the employment share of retail trade accompanies a decline in spatial inequality in employment in general. This should be an especially welcomed finding for rural areas. For the question at hand, these data provide an insight into the average change in retail markets. Of perhaps greater policy import is the marginal effect of fiscal structure and public infrastructure on agglomeration. For answers to this question, I turn to an empirical model of retail agglomeration.

MODELING RETAIL AGGLOMERATION

The lesson of the existing literature is that the potential public infrastructure and public finance

Figure 4

Indiana Wal-Mart Stores, 2000 to Present



impacts—in this case property tax rates—on agglomeration warrant empirical analysis. Following a consideration of the theoretical model above (where travel costs of tax rate differentials generate agglomeration), I propose the following empirical model of agglomeration:

$$(5) \quad A_{i,t} = \beta_1 + \beta_i + \beta_3 \Pi_{i,t} + \beta_4 \Gamma_{i,t} + \delta \tilde{W} A_{j,t} + \phi A_{t-n} + \varepsilon_{i,t},$$

where local agglomeration in county i in year t is a function of a common intercept and county fixed effects; county property tax rates, Π ; the number of interstate highways, Γ ; and the spatial autocorrelation component, $\tilde{W} A_{j,t}$, which includes the first-order contiguity matrix W of A , in surrounding

Figure 5
Cumulative Wal-Mart Entrance in Indiana

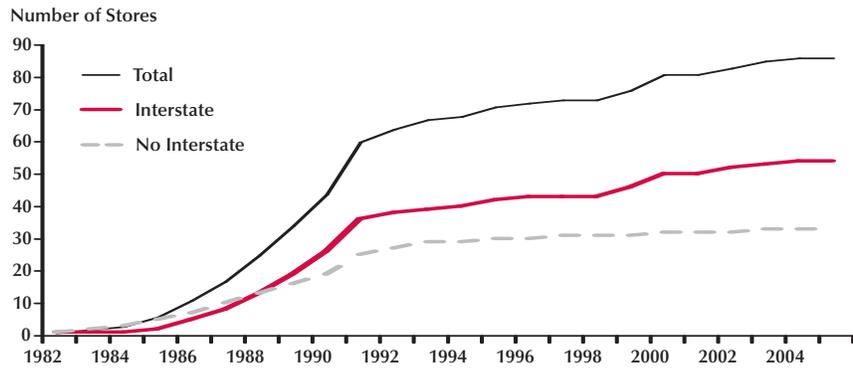
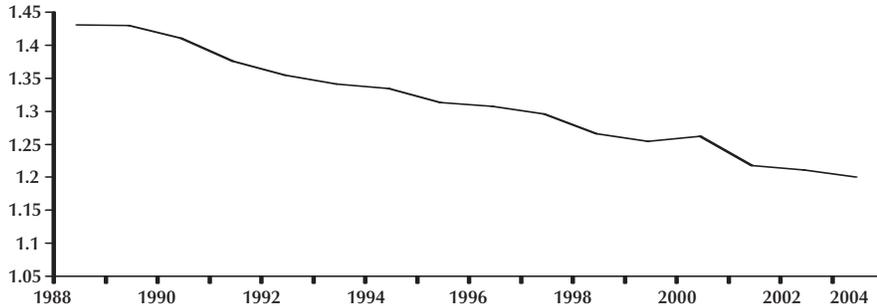


Figure 6
Moran's I of Retail Employment Inequality



contiguous counties j in time t . This first-order contiguity matrix is composed of a value 1 for each county j contiguous to county i and 0 otherwise. The matrix is row standardized to, among other things, account for the differing number of contiguous counties to the 92 counties in the state. This specification includes the time autoregressive components ϕ , for A in $t - n$ lags. The ε denotes the error term, assumed to be white noise.¹¹ To identify this equation, I developed an identification strat-

egy around interpretation of Wal-Mart's entrance decision.

Wal-Mart's entrance decision is hotly debated in the literature examining big-box impacts on employment and earnings and fiscal impact. This work has yielded insight into the retailer's choice. Several econometric studies of Wal-Mart were unable to reject exogeneity of local growth in Wal-Mart's entrance decision (e.g., Hicks and Wilburn, 2001; Franklin, 2001; and Global Insight, 2005). Basker (2005) offered an entrance-timing dummy to identify the wage and industry structure equations. Neumark, Zhang, and Ciccarella (2005) offer an appealing observation that Wal-Mart built its

¹¹ Another common representation of the fixed effects is as a representation of an error component where $e = m + v$, with m being the fixed effect and v the observation varying component of the error term.

retail store network roughly concentrically from Bentonville, Arkansas, extending new firms within a day's drive of existing regional headquarters. Hicks (2006) provided a market-size instrument based on a radio interview with a Wal-Mart official who claimed market size was a leading factor in site location.¹² Hicks (2006) compares exogeneity tests and identification strategies and finds no significant variation across instruments and only modest evidence of endogeneity across a wide variation in choice variables.

The evidence in the Wal-Mart research is useful in identifying a model of agglomeration for two reasons. Concern regarding endogeneity of local tax structures is an important fixture in the public finance literature. Brueckner (2003) offers a thorough review of strategic tax models. Thus, identifying agglomeration based on the dominant firm's entrance decision should precede the endogeneity concern because its entrance should be correlated with the agglomeration measure, but not the error term in the ordinary least squares specification. Second, Wal-Mart's well-known supply-chain channels are closely linked to public infrastructure (primarily interstates and their intersections), thus evidence of supply-chain network decisions by the leading retailer should aid in identifying the equation.

One weakness is that the data (and indeed Wal-Mart's birth) are all subsequent to the interstate highway system, so earlier path dependencies on retail agglomeration are not visible in this modeling effort. Nonetheless, the short-run agglomeration effects are of interest.

Thus, the identifying equation for the estimation takes the form

$$(6) \quad \hat{A}_{i,t} = \beta_1 + \beta_2 \chi_{i,t} + \beta_3 [\theta_{i,t}]t + \beta_4 N_{i,t} + \varepsilon_{i,t},$$

where agglomeration, $\hat{A}_{i,t}$, is estimated as a function of an intercept; a Wal-Mart entrance dummy, $\chi_{i,t}$; a weighted Wal-Mart exposure variable, $[\theta_{i,t}]t$, which is a presence dummy multiplied by a time trend, county population N in county i , and time t ; and the standard white noise error term, $\varepsilon_{i,t}$. Lagged

explanatory variables from equation (4) are also included in this specification. This is the identifying equation, to which will be added lagged predetermined variables, as is the common approach for panel models in order to account for the bias caused by ordinary least-squares estimates of spatial lag models. This approach has been referred to as a spatial 2SLS and is shown to be an unbiased, near equivalent of the more computationally demanding maximum likelihood method (Franzese and Hays, 2004).

DATA AND ECONOMETRIC CONSIDERATIONS

The data are from several common sources. The Wal-Mart data are from two data releases by Wal-Mart and are described in some detail in Hicks (2005a). These releases have been employed by a number of studies.¹³ The data clearly describe the entrance data of Wal-Mart, the county, and whether or not the store is still operating. The big-box data are the sum of all retail establishments with more than 100 employees and are from the U.S. Census Bureau's County Business Patterns, 1988 to the present, as are retail employment data.¹⁴ The infrastructure data are from the U.S. Department of Transportation, Office of Freight Management, Freight Analysis Framework, and were compared with date information confirmed by the Indiana Department of Transportation. Chandra and Thompson (2000) employed the PR-511 master file, which identifies the opening and closing of each of the highways in the interstate highway system. My data collection problem was considerably less difficult, because most links were completed prior to the beginning of the data period. I code the data as count variables for the presence of each open interstate highway in the county.¹⁵ The tax data are from the Indiana Department of

¹² Neumark, Zhang, and Ciccarella derived this instrument from a reading of Sam Walton's autobiography, whereas Hicks relied on a radio broadcast describing market size as an entrance.

¹³ See Hicks (2005a,b and 2006), Neumark, Zhang, and Ciccarella (2005), and Sobel and Dean (2006).

¹⁴ Clearly, the definition of big-box is more than employment and includes store style, but this is used to reflect the presence of other large retailers.

¹⁵ I chose to employ this count measure of highways as an improvement of the more commonly employed presence dummy. Other alternative

Table 2**Selected Summary Statistics**

	Mean	Median	Maximum	Minimum	Standard deviation
Retail employment	2,920	2,227	20,026	165	2,919
Property tax rate (mils)	7.310	7.683	21.444	1.335	2.849
Interstate	0.489	0.000	1.000	0.000	0.500
Retail pull factor	1.000	0.994	1.182	0.989	0.022
Per capita big-box retail	0.00006	0.00005	0.00024	0.00000	0.00005

Revenue and are county-specific property tax rates for commercial property. One caution is that Indiana communities do have some flexibility in assessment of local property taxes. For example, Wal-Mart received tax incentives of mixed types in the location of four facilities in the state (three distribution centers and one store). A detailed treatment of these is offered by Mattera and Purinton (2004). The tax data were available only from 1988 to the present, which is the limit of the analysis. The dependent variable is a modified Theil's inequality index of retail employment, which was modified to center on 1 for ease of interpretation.¹⁶

Each of the variables appears stationary (both visually and through an augmented Dickey-Fuller test), though the relatively brief time series available obviously weakens these tests. Autocorrelation was addressed through the addition of the first-order autocorrelation component. Further, a Hausman test confirms that fixed, rather than random, effects are appropriate in this model. Without testing, I transform each model's standard errors, using White's (1980) method to generate homoskedastically distributed errors. Summary statistics of some relevant variables appear in Table 2.

specifications were possible (e.g., number of miles of interstate), but I elected not to test that model because I was convinced that the number of miles failed to capture the importance of multiple interstates in measuring the flow of benefits of the highways.

¹⁶ The scaling process also reduces concerns over the normality of the error term. One concern here is in the interpretation of a coefficient, which is essentially a logarithmic transformation of an index value. One interpretive technique championed by Kennedy is in the transformation of the estimated coefficient such that the marginal effect is described as $\exp[1/2 \log(\beta)] - 1$. The Theil's T is the logarithm of the ratio of county retail per capita to state retail per capita.

ESTIMATION RESULTS AND ANALYSIS

Table 3 illustrates the estimates of equation (5) above, including two modifications: model 1, without time or space autocorrelation components, and model 3, the additional specification of per capita big-box retail stores. The models are tested on the full sample and rural (non-MSA) counties. The sample period was from the 1990-2003 period, which included 34 suppressed observations in the full sample. The suppressed observations were due to Census protection of firm identities. All of the suppression occurred in the 1990s.

Model 1 in both instances is biased through autocorrelation, which appears both spatially and temporally. The results from models 2 and 3 across all Indiana counties and the non-MSA counties provide insight regarding the urban/rural differences on tax and infrastructure's impact on agglomeration.

In the state as a whole, property tax rates do not play a role in retail agglomeration; whereas, in the non-MSA counties alone, the effect is statistically important, but near the minimally significant threshold for economic effects. A 1-percentage-point decrease in property tax rates (which is about one-quarter of the standard deviation) leads to an increase in the Theil's T of roughly 1 percent of the state's standard deviation. At the margin, this is a small effect, which should be noted only because the spread of the property tax rates is more than 10 mils, or four times the standard deviation.

As with property tax rates, highway infrastructure possesses a statistically certain effect on retail inequality only in the non-MSA regions of Indiana.

Table 3
Estimation Results, Dependent Variable Is Retail Inequality

Variable	Full sample (N = 1,258, with 92 counties)			Rural (N = 742, with 55 counties)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Common intercept	-0.21 (-7.17)	-0.06 (3.64)	0.43 (0.60)	-0.23 (-18.55)	-0.08 (-3.54)	-0.09 (-2.83)
Property tax rate	-0.006 (-3.16)	-0.0009 (-1.13)	0.003 (1.12)	-0.008 (-6.39)	-0.0026 (-1.93)	-0.002 (-1.95)
Interstate count	0.03 (0.85)	-0.13 (-0.73)	-0.007 (-0.26)	0.04 (2.98)	0.01 (1.54)	0.01 (1.57)
Per capita big-box	—	—	-2,160 (-1.57)	—	—	136.07 (0.39)
Spatial lag	—	0.39 (8.35)	0.33 (4.65)	—	0.06 (0.71)	0.05 (0.63)
AR(1)	—	0.61 (13.83)	0.68 (9.82)	—	0.63 (6.79)	0.63 (6.80)
Adjusted R ²	0.93	0.96	0.93	0.94	0.97	0.91
D-W	0.47	1.45	1.31	0.96	1.65	1.66

NOTE: The t-statistics are in parentheses.

And, the effect of highway infrastructure is economically meaningful, with the presence of a highway leading to about a 10 percent increase in the relative share of retail employment in a county.

The per capita big-box variable had no effect on retail agglomeration.¹⁷ The spatial and time autocorrelation variables behave as expected, while the model diagnostics are satisfying.

SUMMARY AND CONCLUSIONS

This paper presents an extension to the analysis of tax and infrastructure's role in generating industry agglomeration. The first major contribution is in evaluating the retail sector—an often ignored component of regional economic activity. Secondly, my strategy for identifying firm entrance offers a novel approach to solving a ubiquitous concern with agglomeration studies.

Using this approach, I find first that neither property taxes nor highway infrastructure contribute to retail agglomeration in a sample that includes both MSA and non-MSA counties in Indiana. This finding mimics those of Holtz-Eakin (1994) and Evans and Karras (1996). However, in

non-MSA counties, I find that a modest increase in local retail agglomeration is associated with lower property tax rates. This is the only relevant regionally varying tax instrument in Indiana. Second, I find that highway infrastructure explains about 10 percent of the variation in retail agglomerations at the county level in Indiana.

These questions in general are not new; however, the results suggest that the leakage impact of highways on rural retail is far lower than that found by Chandra and Thompson (2000) and Hicks (2002 and 2005b). What is especially novel in this analysis is the use of firm-level entrance decisions by the leading firm in this industry to identify the model. Further, analysis of retail agglomeration by transportation researchers is notably lacking. For these reasons, this study provides insight into matters of retail agglomeration, public infrastructure, and taxation.

Additional analysis is warranted. Extension of this modeling approach regionally would be insightful. One caveat is that the selection of Indiana was made to isolate variations in tax structure, so any extension of this modeling effort must take into account other location-determining tax instruments. Second, evaluation of the competitive environment for retail subsequent to the reduction in spatial inequality is also important. Although spatial inequality may be a welcomed

¹⁷ The statistical significance of this variable in the full model approached common levels of significance, but the magnitude of the coefficient was far below any meaningful threshold of economic importance.

Hicks

economic outcome, if it occurs at the expense of competition, its welfare effects may be uncertain. Also, upstream linkages, especially in wholesale, are also important to evaluate within the context of agglomeration and transportation. This would be a natural extension of this study.

Finally, these results imply policy considerations. First, local policymakers should carefully assess the role of local tax rates with respect to public infrastructure. And, while this is hardly a novel prescription, the findings that regional retail agglomeration are sensitive to local property tax rates should provide a cautionary note to public policymakers. Perhaps most important is the finding that public infrastructure plays a role in agglomeration, even in a period of robust declines in spatial autocorrelation and spatial inequality. Although this falls short of a prescription for highway construction (I have neither assessed benefits nor costs), it should herald the worth of specific local analysis.

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On the Economic Analysis of Smoking Bans

Michael R. Pakko

This paper evaluates the literature on the economic effects of smoking bans. Many studies focus exclusively on aggregate impact and thus may overlook the importance of distributional effects, which reveal inefficiencies often undetectable in analyses of aggregated data. These effects also account for the political economy of smoking bans, igniting controversy and public debate. The political resolution often involves exemptions for certain types of establishments, which limits the applicability of many existing studies to the more comprehensive smoking-ban proposals. The paper also analyzes data from Maryville, Missouri—the first city in Missouri to ban smoking in restaurants—to illustrate some of these points.

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In Missouri and across the nation, communities are debating the efficacy of banning smoking in all public places, including privately owned establishments. The policy issues involved are multidimensional, but the public debate is often summarized in terms of public health versus economic impact.

The focus of policymakers is often directed toward considering the aggregate, or overall, economic effects of smoking bans on business in a community. Data on communitywide economic activity are often readily available, and it might seem that the overall effect of a public policy on economic activity is the appropriate measure to consider.

But it is also important to account for the distributional impact and economic inefficiencies that are often imposed by government intervention in the market, particularly in cases where the proposed policy imposes blanket restrictions. These differential effects reveal inefficiencies that are often undetectable in analyses of aggregated data.

Distributional effects also contribute to the political economy of smoking bans, as economic

interests clash. The resolution of these conflicts often results in legislation that exempts certain types of businesses from these bans. Such compromises represent a political outcome that reduces the potential inefficiency and welfare losses that might otherwise be imposed by more comprehensive smoking prohibitions. However, the prevalence of these exemptions, in turn, limits the applicability of many studies to the more comprehensive legislation that has been proposed in many communities.

AGGREGATE ECONOMIC IMPACT

The consensus of the literature on the economic effects of existing smoking regulations is that no statistically significant impact on overall business in a community can be ascertained.¹ Some communities appear to experience a decline in sales or

¹ Some of the more prominent studies include Huang et al. (1995), Glantz and Smith (1994 and 1997), Bartosch and Pope (1999 and 2002), Hyland, Cummings, and Nauenberg (1999), and Huang, De, and McClusker (2004). A recent comprehensive survey is provided by Scollo, Hyland, and Glantz (2003).

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employment at restaurants and bars, while others appear to experience an increase, at least over time.² Some studies find no evidence of consumer-flight to other locations, while others show some effect on bordering communities.³ However, the statistical significance of these findings is often weak or lacking.

There are a number of reasons that this conclusion is not very surprising. First, these studies are necessarily conducted with limited data. Sample periods are short, and detailed local data are often scarce. Accordingly, it can be difficult to control for the many possible idiosyncratic factors that may affect economic outcomes without sacrificing some ability to adequately test hypotheses (a statistical problem known as “limited degrees of freedom”). Moreover, the possibility that important variables may have been omitted from the analysis implies that the statistical significance of their conclusions is often fragile (“omitted-variable bias”).

In addition, studies of the impact of smoking bans necessarily focus on communities that are among the first to implement such ordinances and are therefore more likely to have a proportionately smaller smoking population and/or fewer businesses that would be adversely affected by a smoking ban.⁴ This introduces a source of “sample-selection bias” that limits the general applicability of results, particularly in cases where demographic features differ

and policy proposals are more comprehensive or restrictive than those examined in the literature.⁵

More importantly, basic consumer theory suggests reasons that aggregate economic effects might be limited: When an option is denied to consumers, they tend to substitute other similar products and services. A disruption in the availability or price of a good can temporarily skew spending as consumers reallocate their expenditures, but with the ultimate effect of leaving their spending on broad categories such as “entertainment” unchanged.

However, the lack of a measurable overall effect can mask some important distributional and social-welfare effects.

DISTRIBUTIONAL EFFECTS AND ECONOMIC EFFICIENCY

When consumers are forced to reallocate their spending, the notion of “revealed preference” tells us that they are likely to be made worse-off in terms of economic efficiency. In making choices about their spending patterns, consumers reveal their preferred consumption bundle. By eliminating options available to consumers, a ban on an activity forces them to choose a spending allocation that could have been chosen before the ban, but was not.

This notion of economic welfare differs considerably from the analysis implicit in many economic studies of smoking bans, which present the elimination of a risk as an unambiguous benefit and the absence of a significant aggregate economic effect as evidence that a smoking-ban policy would be costless.⁶ Neither of these characterizations of costs and benefits is complete, however.

Economists observe that individuals make choices each day based on their preferences and the options provided by the market. Those choices frequently involve uncertainty and risk. People make choices because the benefits they expect to

² In an early study of smoking bans, Glantz and Smith (1994) found that, among 15 municipalities, there were two significant positive effects and one significant negative effect on bar and restaurant sales. Evans (1997) cited several methodological criticisms of that study and found that nine cities in the sample were subject to significant negative effects. A subsequent study by Glantz and Smith (1997) showed two statistically significant positive effects and two significant negative effects.

³ In a study of 239 cities in Massachusetts, Bartosch and Pope (2002) found a statistically significant positive effect for cities bordered by nonsmoking municipalities.

⁴ Glantz and Smith (1994) focus their analysis on the first 15 U.S. cities to enact smoke-free ordinances affecting restaurants. The DHSS study of Maryville, Missouri, considered in this paper (Cowan et al., 2004) represents an analysis of “the first such ordinance in Missouri to completely prohibit smoking in all restaurants.” Of the first nine states to implement statewide bans, eight were below the U.S. median with regard to percentage of smokers. In fact, the first two states to adopt smoking bans, California and Utah, have the two lowest rates of smoking prevalence in the nation, according to statistics from the Centers for Disease Control and Prevention (see Adams and Cotti, 2006).

⁵ More general methodological critiques of the literature include Dunham and Marlow (2000) and Evans (1996 and 1997).

⁶ For example, Glantz and Smith (1997) conclude that “legislators and government officials can enact health and safety regulations to protect patrons and employees in restaurants and bars from the toxins in secondhand tobacco smoke without fear of adverse economic consequences” (p. 1690).

gain are greater than the costs and risks involved.⁷ This is true whether the decision is about skydiving, smoking cigarettes, or even working in or frequenting establishments where they may be exposed to secondhand smoke. Indeed, the act of driving a car to pursue these activities presents grave risks. To prohibit an activity simply because it involves risk cannot be justified in economic terms. In fact, government intervention can introduce inefficient distortions into those market mechanisms that efficiently deal with risk.

In our free market economy, the “invisible hand” guides businesses to provide the goods and services that consumers demand. For business owners and their employees, the impact of a ban can vary significantly, depending on their specific clientele and their marketing strategies. It is sometimes argued that secondhand smoke imposes external costs, requiring government intervention. But in the case of private businesses—especially those in the entertainment and hospitality sectors—the profit motive provides a mechanism for business owners to internalize those costs. Individuals assess their own risks and benefits, but it is in the business owner’s best interest to accommodate customers and employees, smokers and nonsmokers alike. Failure to do so is reflected in the bottom line.

As public attitudes have evolved, an increasing number of restaurants and other entertainment venues offer smoke-free environments.⁸ For example, the St. Louis Coalition for Tobacco-Free Missouri lists over 400 smoke-free restaurants (plus multiple chain outlets) in the St. Louis area.⁹ Meanwhile, some businesses continue to accommodate smokers and nonsmokers with distinct and separate settings under strictly regulated standards, while others offer venues for a clientele that expects a smoke-filled atmosphere. Each proprietor is making a careful business decision about how to best fill a niche in the market and make a profit in the process.

The increasing number of establishments choos-

ing to go smoke-free reveals that the market is responsive to people’s changing attitudes. As consumers demand smoke-free options, businesses find it advantageous to provide them. A government regulation that attempts to force the market toward a new equilibrium, however, is likely to impose transitional costs and/or long-term hardships on many individual businesses.

A number of economic studies have examined these distributional effects. Because detailed data are often limited, much of the research on differential impacts comes from the results of surveys that assess attitudes and expectations.¹⁰ The pattern of these effects is not surprising. Proprietors and customers of businesses such as bars, bingo halls, bowling alleys, billiard parlors, and casinos tend to express greater concerns about revenue losses from smoking bans. Family-oriented restaurants, chain outlets, fast-food restaurants, and take-out establishments are generally considered less likely to be adversely affected by smoking bans.

Survey results reveal that bar owners perceive a particularly significant threat to their business. In one nationwide survey of restaurant and bar owners, 39 percent of restaurant owners expected revenue losses after a smoking ban, while 83 percent of bar owners expected losses.¹¹

Among bar and restaurant customers, smokers (who tend to spend more than nonsmokers) are more likely to decrease their patronage after a smoking ban, whereas nonsmokers (who are more numerous) are more likely to increase their patronage. The overall effect of these tendencies on overall restaurant and bar sales is a subject of debate.¹² Differential impacts on bars and restaurants are evident, however. For example, a survey in Massachusetts found that 44 percent of smokers

⁷ A seminal article on the topic of risky choices is Friedman and Savage (1948). Viscusi (1992) applies risk analysis to the specific issue of smoking. See also Lemieux (2000) and Petkantchin (2005).

⁸ Brooks and Mucci (2001) present evidence of changing attitudes toward smoking in restaurants among adult survey respondents in Massachusetts.

⁹ See www.breatheasymmo.org/directory.asp?coal=15.

¹⁰ Survey data are often treated with skepticism by economists, but they can provide relevant information about preferences and therefore, by implication, about economic welfare. Prominent studies of this type include Beiner and Siegel (1997), Dunham and Marlow (2000), Brooks and Mucci (2000), and Tang et al. (2003).

¹¹ Dunham and Marlow (2000).

¹² For example, Corsun, Young, and Enz (1996) found that smokers in New York City were eating out less after a restaurant smoking ban, but that nonsmokers were eating out more often, resulting in a positive impact on restaurant industry revenues. In a subsequent rejoinder, Evans (1996) raised methodological criticisms and recalculated the net effect to be negative.

predicted decreased patronage at bars, while 24.5 percent of nonsmokers predicted increased patronage. The proportions for restaurant patronage were significantly different, with only 32 percent of smokers reporting decreased patronage and 37.7 percent of nonsmokers reporting increased patronage.¹³ This finding is consistent with greater concerns about revenue losses expressed by bar owners than by restaurant owners.¹⁴

Among studies that have examined detailed sales data after smoking bans, one found that the revenues of bars in Corvallis, Oregon, that offer video poker suffered significant revenue losses.¹⁵ A recent study of gaming in Delaware after a smoking ban found a revenue decline of approximately 15 percent at racetrack casinos in that state.¹⁶ One prominent study of bar sales in several municipalities that had imposed smoking bans showed mixed results, but found that the only statistically significant case showed a negative effect on bar sales relative to a comparison city.¹⁷ A recent comprehensive study of bars and pubs in Ontario found significant declines in sales—over 23 percent in Ottawa, where a comprehensive smoking ban was implemented in September 2001.¹⁸ Several sources document declines in alcohol sales following smoking bans.¹⁹

The overall change in overall employment at bars and restaurants is another measure of economic activity that is often considered. Just as is the case for aggregate sales figures, overall employment data often show no significant effects from smoking

bans.²⁰ One recent study of hospitality-industry employment in New York City found a negative effect on restaurant employment and a positive effect for hotels. Neither effect was significant, however.²¹ Local data and anecdotes that are more specific to subsets of businesses in a community tend to suggest employment losses. For example, a coalition of pub and bar owners in Ottawa, Ontario, estimated a loss of 230 jobs among their members in the first two months of a smoking ban in that city.²²

As smoking bans proliferate across the nation, county-level employment data have provided useful information about the economic impact of smoking bans. By using pooled data covering the entire United States, Adams and Cotti (2006) and Phelps (2006) have been able to increase the statistical power of tests for economic impact. Both studies find little effect on employment at restaurants after a smoking ban is implemented, although Adams and Cotti find that restaurants in warm-weather climates tend to fare better than those in colder regions of the country. With respect to bar employment, both studies find statistically significant losses that range from 5 percent to 17 percent.²³

Here again, however, the notion of revealed preference is informative. In the disruption imposed by a smoking ban, some workers will find themselves dislocated. Most will find new employment quickly, one hopes. But by their revealed preference, we can deduce that these employees considered the costs and benefits of their employment—including the potential health risks that their job entails—and chose not to find an alternative. A government ban will force some of these individuals to do so.

The increasing number of smoke-free venues provides options for employees as well as customers. The motivation to retain good workers provides an incentive for proprietors to offer accommodating work environments. In the process, relative risks and returns of employment options can be efficiently allocated by the market.

¹³ Biener and Siegel (1997).

¹⁴ Proprietors of billiard parlors and pool halls have expressed concerns that are similar to those of bar owners. See Fabrizio et al. (1995).

¹⁵ Dresser et al. (1999).

¹⁶ Pakko (2006 and forthcoming). These findings contradict earlier estimates by Mandel, Alamar, and Glantz (2005).

¹⁷ Glantz and Smith (1997).

¹⁸ Evans (2005); smoking bans in London, Kingston, and Kitchener, Ontario, that have been implemented more recently were also found to be associated with significant declines in bar and pub sales.

¹⁹ For example, Clower and Weinstein (2004) report a sharp decline in alcoholic beverage sales in Dallas following the implementation of a comprehensive smoking ban, in contrast to increasing sales around the state. Thalheimer (2005) found similar effects for Lexington, Kentucky. An association of pub and bar owners in Ottawa, Ontario, reported statistics from the Brewers of Ontario that beer sales declined 10.5 percent in Ottawa during the first eight months of the smoking ban in that city (PUBCO, 2002). The decline in Ottawa beer sales is also reported in Bourns and Malcomson (2002).

²⁰ See, for example, Hyland, Cummings, and Lubin (2000).

²¹ Hyland et al. (2003).

²² PUBCO (2001).

²³ The employment data used in these studies report only the number of employees. There may be additional effects on the number of hours worked that would not be revealed in these analyses.

POLITICAL ECONOMY

Among businesses, comprehensive smoking bans tilt the economic playing field in ways that are fundamental to the political economy of the issue: Establishments that cater to a largely smoking clientele are likely to be opposed to a ban, and those who explicitly cater to a nonsmoking customer base might be driven to oppose it—to protect their own market niche. Businesses in communities that have a relatively high proportion of smokers relative to nonsmokers will be opposed to regional smoking bans, as will businesses and municipalities bordering communities that have not adopted a smoking ban. Many establishments that would be largely unaffected might be inclined to stay on the sidelines of the debate.

Tavern and bar owners have been among the most vociferous opponents of a complete ban on smoking. Existing empirical evidence supports the casual observation that bars stand to suffer a greater threat of revenue losses from smoking bans than do restaurants in general. This differentiation is evident in the political dynamics of public debate on smoking bans. It also explains the tendency of many community smoking bans to include exemptions for stand-alone bars or other establishments that receive a high proportion of their revenues in alcohol sales relative to food sales. In many local ordinances, exemptions also exist for bowling alleys, bingo halls, fraternal organizations, and the like.

These political compromises arise in response to the economic pressures that drive particular businesses to actively oppose smoking-ban ordinances. Those who are most threatened by any public policy proposal tend to be more adamant in their opposition and are more likely to have their interests accommodated in final legislation.²⁴ Exemptions represent something of a second-best outcome (achieved through the political process rather than through market mechanisms) for mitigating the most economically disruptive effects of a proposed public policy.

²⁴ An alternative explanation of this feature of the political economy of smoking bans is that the hospitality industry has been duped into supporting the interests of a powerful tobacco company lobby. See, for example, Dearlove, Bialous, and Glantz (2002).

The prevalence of such exemptions in existing smoking ordinances reflects underlying economic pressures and provides indirect evidence of the potential adverse effects of more comprehensive smoking-ban proposals. In fact, the resources that businesses expend on their opposition to smoking bans, and their lobbying efforts to obtain exemptions, represent direct costs of smoking-ban proposals—whether or not they are ultimately implemented.

The fact that many local ordinances have exempted bars and other establishments is also an important consideration for interpreting previous studies of the effects of smoking bans on bar and restaurant sales. These studies have often considered communities with ordinances that contain numerous exemptions. The applicability of many of these case studies to contemporary policy debates over more restrictive proposals is therefore questionable.²⁵

CASE STUDY: MARYVILLE, MISSOURI

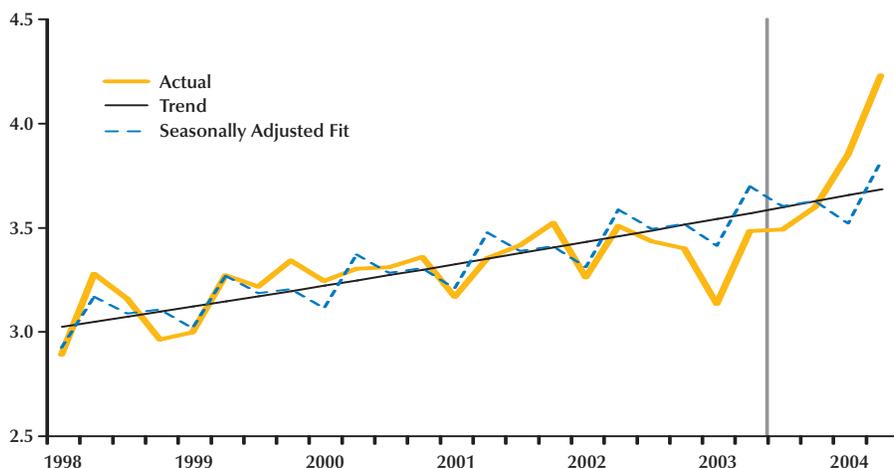
On June 9, 2003, Maryville, Missouri, adopted an ordinance that prohibited smoking in restaurants.²⁶ An examination of the first year of the smoking ban, recently released by the Missouri Department of Health and Senior Services (DHSS), presents data on taxable sales receipts for Maryville that span a period of over five years before and one year after the implementation of the ordinance.²⁷ The study is being widely distributed and presented as evidence in support of similar (and more restrictive) bans in other communities.

The authors of the DHSS study state at the outset that their findings are “consistent with those from studies of smoke-free ordinances in other U.S. cities”—namely, that no “detrimental changes” in

²⁵ Indeed, one study (Goldstein and Sobel, 1998) is widely cited as showing that “even in the number one tobacco-producing state in the US, ETS regulations present no adverse economic impact” (p. 286). However, it considered only the effects of requiring separate smoking and nonsmoking sections in restaurants. A recent citation is in Scollo, Hyland, and Glantz (2003).

²⁶ Maryville City Council (2003).

²⁷ Cowan et al. (2004).

Figure 1**Actual and Fitted Values for the Trend Equation (Fitted Time-Trend and Seasonal Effects Only)**

total bar and restaurant revenue were observed after the ordinance was implemented. However, after comparing the growth rates for sales of eating and drinking establishments (standard industrial classification [SIC] code 581) with total retail sales in Maryville—and with corresponding data for the state of Missouri—and noting that eating and drinking establishment sales in Maryville rose sharply in the final two quarters of the study, the authors conclude that “the ordinance may have been beneficial for this area of business.”

The purpose of the present study is to subject the data from the DHSS study to a more rigorous statistical analysis. Using the data reported in the DHSS study, I have applied basic linear regression techniques to test the hypothesis that the smoking ban had no significant effect on Maryville bar and restaurant sales. Of particular interest as well is the alternative hypothesis that the ordinance had “beneficial” effects.

An investigation of developments in the Maryville economy turned up an important additional factor that is included in the analysis: the opening of a new, popular restaurant chain outlet during the sample period. That factor appears to be more relevant for explaining total restaurant and bar sales in Maryville than the smoking ban.

Analysis of the Maryville Data

Figure 1 presents the data for eating and drinking establishments in Maryville, as reported in the DHSS study. The sample period runs from the first quarter of 1998 through the second quarter of 2004. As noted by the authors of the DHSS study, a trend and seasonal variation are important features of the data series. A sharp increase in sales at the end of the sample period is also evident.

The first line of Table 1 reports a summary of the regression results that were used to generate the trend line and seasonally adjusted estimates illustrated in Figure 1. The regression includes only a constant, a linear time trend, and quarterly dummy variables for quarters 2, 3, and 4.²⁸ It shows that sales at eating and drinking establishments grew at an average quarterly rate of 0.77 percent over the sample period and that seasonal variation generated more than 7¹/₂ percent quarterly variation over a typical year.²⁹ As a measure of fit, the adjusted R² statistic suggests that the regression

²⁸ The first quarter serves as the baseline for seasonality of the regression.

²⁹ Although the coefficient on quarter 2 is the only individually significant seasonal variable, F-tests of the joint significance of the seasonal dummies showed them to be significant in all of the specifications reported in this paper (with exceptions noted).

Table 1**Trend Analysis****[Dependent Variable = $\ln(E\&D_Maryville)$]**

	Constant	Trend	Q2	Q3	Q4	SmokeBan	Applebee's	Adjusted R ²	Q
1	14.8858** (0.0225)	0.0077** (0.0012)	0.0726** (0.0245)	0.0387 (0.0255)	0.0376 (0.0255)			0.6595	6.4060 [†]
2	14.9024** (0.0207)	0.0055** (0.0014)	0.0749** (0.0216)	0.0369 (0.0224)	0.0381 (0.0224)	0.0752* (0.0281)		0.7368	3.2281
3	14.8993** (0.0149)	0.0056** (0.0009)	0.0623** (0.0161)	0.0512** (0.0168)	0.0523** (0.0169)		0.1755** (0.0325)	0.8548	1.0313
4	14.9020** (0.0156)	0.0052** (0.0010)	0.0637** (0.0165)	0.0497** (0.0172)	0.0512** (0.0172)	0.0172 (0.0256)	0.1605** (0.0398)	0.8507	0.9895

NOTE: ** Indicates significant at the 95/99 percent levels. [†]Q-statistic indicates the presence of autocorrelated residuals.

explains nearly two-thirds of the variation in Maryville bar and restaurant sales.^{30,31}

Line 2 shows the results when a dummy variable for the smoking ban is included in the regression. The dummy variable takes on a value of 1 in the final four quarters of the sample period and is zero before. The point estimate of the coefficient on this variable indicates that sales at eating and drinking establishments in Maryville were more than 7¹/₂ percent higher during the smoking ban than trend growth and seasonal variation would predict.³² This estimate is significant at the standard 95 percent confidence level. According to this initial evaluation of the effect of the smoking ban in Maryville, the DHSS conclusions appear to be supported. The inclusion of a dummy variable covering the period of the smoking ban improves

the overall fit of the equation, and its coefficient estimate is positive and significant.

As an illustration of this finding, Figure 2 shows a plot of the actual and predicted values from the regressions with the smoking-ban dummy variable included. After controlling for the smoking ban, the unexplained increase in the final two quarters of the sample is still present, but its prominence is diminished. However, sales in the first two quarters of the smoking ban now appear to be considerably lower than the values predicted by the estimation equation. In fact, the last four residuals from this equation are outliers, the first two negative and the last two positive.

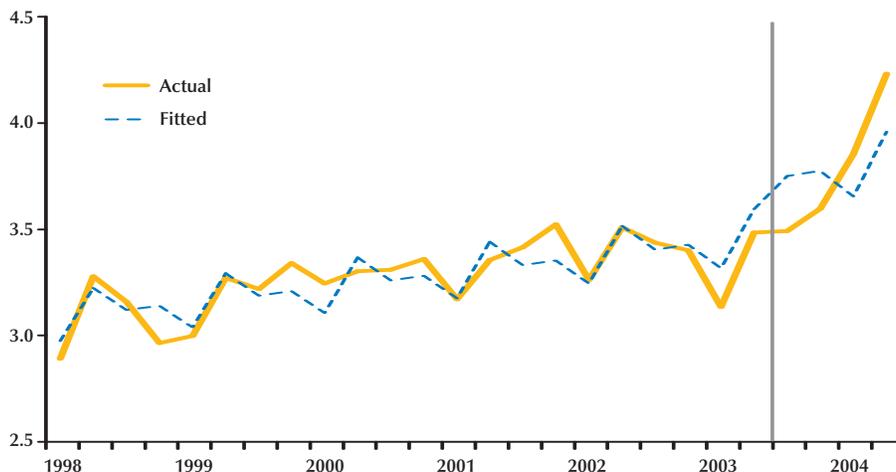
Results such as this are often fragile. First, the significance of the dummy variable indicates that a correspondence exists in the data, but it does not establish causality. More importantly, findings are often subject to omitted-variable bias. If an important independent influence has been excluded from the analysis, the results can be misleading.

In the following sections, I consider the inclusion of additional data series to control for changes in overall economic conditions in Maryville and Missouri. First, investigation into the local economic environment in Maryville yielded information about one important idiosyncratic event that is relevant to the analysis: the opening of a new Applebee's in town.

³⁰ The equation actually explains a greater proportion of the variation: The unadjusted R² is 0.714. The adjusted R² penalizes the inclusion of superfluous explanatory variables and is a particularly relevant measure of fit for small samples in which degrees of freedom are limited.

³¹ Tests of the residuals from this baseline trend/seasonal specification suggested the presence of serially correlated residuals. Subsequent analysis showed that this was an artifact of the outlying observations at the end of the sample period. Serial correlation was not detected in specifications that included dummy variables for end-of-period effects.

³² The coefficient on a dummy variable in a semilogarithmic equation such as this provides only an approximation to the percentage effect. For a coefficient value β , the true percentage effect is $\exp(\beta)-1$. In this case, the calculated value is 7.81 percent. See Halvorsen and Palmquist (1980).

Figure 2**Actual and Fitted Values for the Trend Equation (Including the Smoking-Ban Effect)**

The Applebee's Effect

In mid-February 2004 (halfway through the third quarter of the smoking ban), Applebee's opened a new franchise in Maryville. According to local news reports, it has been a phenomenal success. In a report on the restaurant's one-year anniversary, the *Maryville Daily Forum* quotes the company's vice president of operations for Applebee's parent company as saying that "Maryville has been one of the busiest stores in the country since its opening. We call it our crown jewel."³³

Maryville is a fairly small town, with a resident population of 11,000. It has only 37 restaurants and bars. It is quite conceivable that the opening of a new, popular restaurant chain outlet would have a significant independent effect on the town's total bar and restaurant sales.

To test for the influence of the "Applebee's effect," I constructed a variable that takes on a value of 1 in the second quarter of 2004 and 2 in the first quarter (since Applebee's opened midway through the quarter). The results of including this variable in the basic trend regression equation are reported in line 3 of Table 1. The Applebee's variable is highly

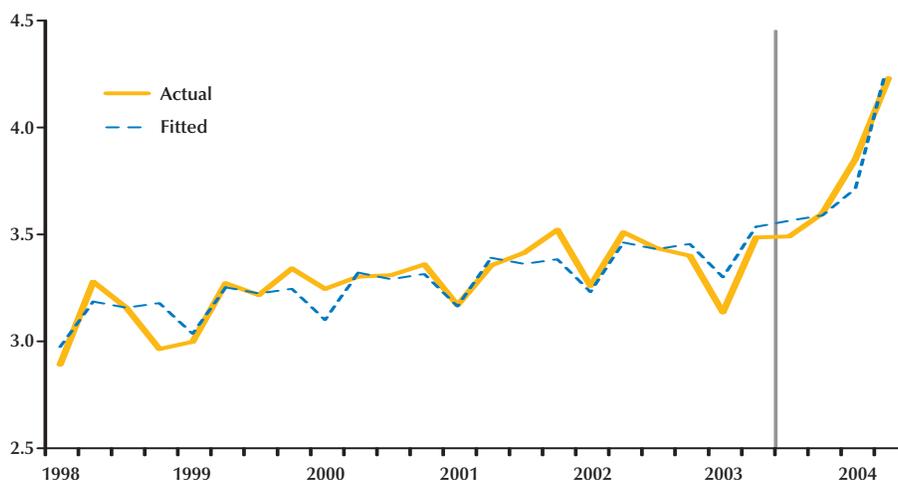
significant, with a point estimate that suggests it accounts for a 19.2 percent increase in Maryville bar and restaurant sales in the second quarter of 2004 (along with a 9.6 increase in the first quarter).³⁴

With both the smoking ban and Applebee's dummy variables included in the regression (line 4), the Applebee's effect accounts for an increase of more than 17 percent above trend at the end of the sample period—an effect that remains highly significant. The coefficient on the smoking-ban dummy is small and is not statistically significant. In fact, the fit of the regression deteriorates when adding the smoking-ban dummy variable to the equation that already includes the Applebee's variable (lines 3 and 4).

Figure 3 illustrates this result, showing the actual and fitted values from the regression that includes the Applebee's variable (line 3). Compared with Figures 1 and 2, Figure 3 clearly shows that the inclusion of the Applebee's variable effectively accounts for the surge in restaurant and bar sales in the first two quarters of 2004, leaving little additional variation for which the smoking-ban dummy variable can account.

³³ Goff (2005).

³⁴ These figures are related to the actual coefficient estimates using the method described in footnote 32.

Figure 3**Actual and Fitted Values for the Trend Equation (Including the Applebee's Effect)**

The regressions reported in Table 1 include no controls for overall economic conditions. This is another potentially important source of omitted-variable bias in the results, particularly because the sample period includes a national economic recession. A number of variables were applied to the analysis to better control for economic conditions. The results, reported in the appendix, were all broadly consistent with the trend analysis presented in Table 1.

DISCUSSION AND CONCLUSIONS

The findings of this analysis of the Maryville data suggest no significant effect of the smoking ban on bar and restaurant sales. The evident increase in sales near the end of the sample period more closely corresponds to the opening of the new Applebee's in town than it does to the implementation of the smoking ban. Although these findings do not establish causality, a consideration of the particular demographics and the limited scope of the ordinance in this case suggest that any claims about the smoking ban having beneficial effects on bar and restaurant sales in Maryville cannot reasonably be substantiated.

These results illustrate many of the points raised in the first section of this paper. First, the sample period is short. With only 26 observations, limited degrees of freedom make it difficult to test hypotheses with a high degree of confidence. The sharp increase in Maryville bar and restaurant sales in the first two quarters of 2004 is an unusually prominent outlier in the data, so it is more readily associated with statistically significant effects. The key issue is resolving the source of those effects.

More generally, it is not surprising that a smoking ban like the one in Maryville would have no measurable impact on the city's total bar and restaurant sales. Consumers tend to substitute similar expenditures when one set of consumption options is restricted. Spending patterns can change without having a significant impact on broad spending categories such as "entertainment" or on specific categories such as "sales revenues of eating and drinking establishments."

But the lack of aggregate effects does not preclude the existence of significant distributional effects. It is generally acknowledged that some businesses are likely to be affected more than others by a smoking ban. The owners of businesses who are likely to be most severely affected tend to raise the loudest objections and are therefore more likely

Pakko

to be granted exemptions. It is no accident that bars are often exempted from smoking bans.

In Maryville, the ordinance exempts stand-alone bars. It exempts seven establishments by name and also excludes any other businesses that receive more than 60 percent of their revenues from alcohol sales.³⁵ By excluding bars, the Maryville City Council mitigated some adverse economic impacts that might have occurred under a comprehensive ban. The specific exemptions included in the ordinance suggest that it represented a political compromise that accommodated concerns raised by local business owners.

Indeed, the Maryville ordinance affected very few businesses at all. According to the Missouri Tobacco Use Prevention Program (2002), 70 percent of the restaurants in Maryville were smoke-free well before the ban. Assuming that figure excludes bars that were exempted, the ordinance affected no more than nine restaurants. It would be very surprising to find that the smoking ban had any significant effect on total bar and restaurant sales in Maryville.

This observation points to a more general reason for exercising caution in extrapolating the findings from this type of study to an evaluation of policy proposals in other municipalities. Studies of the impact of smoking bans necessarily focus on communities that are among the first to implement such ordinances, and which are therefore more likely to have a lower proportional smoking population and/or a smaller number of businesses that would be adversely affected by the proposed ban. This type of sample-selection bias limits the general applicability of results, particularly in cases where demographic features differ and public policy proposals are more comprehensive and restrictive than the Maryville ordinance.

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³⁵ Maryville City Council (2003).

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APPENDIX

Including Economic Controls in the Maryville Regressions

Table A1 presents results that include two economic control variables used in the DHSS study. The first panel includes a variable that is constructed by subtracting eating and drinking sales from total retail sales in Maryville.³⁶ Although the coefficient on this variable is significant in only one of the equations reported, it is jointly significant with the linear time trend in all four specifications. Comparisons with Table 1 show that including this economic control variable provides for a slightly improved fit. However, the conclusions to be drawn from this specification are the same as before: The smoking-ban dummy variable is positive and significant if included alone, but the Applebee's variable provides for a much better fit, and the smoking ban has no significant influence after controlling for the Applebee's effect.

The second panel of Table A1 reports the results of including a variable for sales at eating and drinking establishments for the rest of Missouri (Missouri minus Maryville). Again, although the coefficient on this variable is not individually significant, it is jointly significant with the time trend. However, the seasonal effects were found to be individually and jointly insignificant in all four specifications. Evidently, the seasonal pattern in total Missouri bar and restaurant sales is able to adequately capture the seasonal variation in Maryville's sales in this regression. In light of this finding, the third panel of Table A1 presents the results of excluding the seasonal dummy variables.

Again, the slightly better fit of these equations relative to the trend specifications in Table 1 shows that bar and restaurant sales in the rest of Missouri help to explain the Maryville sales pattern. When it is included in this specification, the Applebee's variable continues to be highly significant. However, the smoking-ban dummy variable is no longer significant, even when the Applebee's effect is not considered.³⁷

As an additional robustness check, I obtained data on employment and unemployment for Nodaway County for use as alternative control variables for local economic conditions.³⁸ Table A2 reports the results of including these data in the regressions. The first panel shows the results of including (the natural log of) Nodaway County employment. The second panel considers the Nodaway County unemployment rate as a control variable. The regressions including the unemployment rate proved the best overall for fit of all the specifications considered. The findings reinforce those reported in Table A1: The Applebee's effect unambiguously dominates the smoking-ban effect. When the Nodaway county unemployment rate is used as an explanatory variable, the smoking-ban dummy is not significant, even when included alone.³⁹

All of the regressions considered above use the log of Maryville bar and restaurant sales as the dependent variable. Two alternative ratios were also considered: The first is the ratio of Maryville eating and drinking establishment sales to total retail sales. The second is the ratio of eating and drinking establishment sales in Maryville relative to the eating and drinking establishment sales for Missouri.⁴⁰

³⁶ This difference was included in the regression instead of total Maryville retail sales because the total includes the dependent variable. Inclusion of the total would therefore introduce a problematic correlation of the regressor with the residuals.

³⁷ In the specification that excludes seasonal factors, the smoking-ban dummy variable is very near the significance threshold.

³⁸ The data are quarterly averages of monthly figures, obtained from the Bureau of Labor Statistics.

³⁹ In the specification that includes the smoking-ban dummy variable alone, evidence of serially correlated errors remains. The inclusion of an autoregressive error specification did not alter the overall results, however. In fact, the AR(1) error specification had the effect of reducing the size of the coefficient on the smoking-ban dummy variable.

⁴⁰ The use of these ratios as dependent variables can be thought of as imposing ex ante restrictions on the relationships considered in Table 2. Following Glantz and Smith (1994 and 1997) analysis of these types of ratios have been widely used in the literature on the economic effects of smoking bans. Evans (1997) points out the use of ratios can be misleading when the numerator is relatively large. However, Maryville bar and restaurant sales comprise only about 10 percent of total Maryville retail sales and only 0.23 percent of Missouri bar and restaurant sales.

Table A1**Trend Analysis Using Sales Data to Control for Economic Factors****[Dependent Variable = ln(E&D_Maryville)]**

	Constant	Trend	X	Q2	Q3	Q4	SmokeBan	Applebee's	Adjusted R ²	Q
A. X = ln(Total_Maryville – E&D_Maryville)										
1	4.4048 (5.8283)	0.0019 (0.0034)	0.6152 (0.3421)	0.0386 (0.0300)	-0.0209 (0.0410)	-0.0640 (0.0615)			0.6923	4.5727 [†]
2	2.9130 (4.8276)	-0.0014 (0.0030)	0.7039* (0.2834)	0.0362 (0.0247)	-0.0314 (0.0340)	-0.0781 (0.0509)	0.0813** (0.0252)		0.7908	0.0907
3	9.7845* (4.0943)	0.0029 (0.0023)	0.3002 (0.2403)	0.0463* (0.0204)	0.0214 (0.0291)	0.0018 (0.0437)		0.1646** (0.0332)	0.8588	0.3858
4	8.2930 (4.2689)	0.0015 (0.0026)	0.3880 (0.2506)	0.0441* (0.0203)	0.0101 (0.0305)	-0.0149 (0.0458)	0.0296 (0.0260)	0.1355** (0.0417)	0.8610	0.0398
B1. X = ln(E&D_Missouri – E&D_Maryville)										
1	-10.3031 (13.1294)	-0.0014 (0.0049)	1.2051 (0.6281)	-0.0829 (0.0843)	-0.1397 (0.0960)	-0.0364 (0.0454)			0.6981	3.5275
2	5.1811 (15.1076)	0.0023 (0.0051)	0.4649 (0.7226)	0.0145 (0.0964)	-0.0317 (0.1089)	0.0095 (0.0500)	0.0626 (0.0346)		0.7288	3.2178
3	5.7685 (9.7439)	0.0024 (0.0035)	0.4368 (0.4661)	0.0066 (0.0616)	-0.0143 (0.0719)	0.0245 (0.0341)		0.1642** (0.0347)	0.8540	1.0627
4	7.1627 (11.3809)	0.0027 (0.0038)	0.3701 (0.5443)	0.0157 (0.0725)	-0.0049 (0.0822)	0.0283 (0.0379)	0.0076 (0.0296)	0.1593** (0.0404)	0.8464	1.0788
B2. X = ln(E&D_Missouri – E&D_Maryville) - without seasonals										
1	6.3462* (3.0451)	0.0048** (0.0016)	0.4086** (0.1451)						0.6720	3.3851
2	7.1113* (2.8715)	0.0032 (0.0017)	0.3728* (0.1368)				0.0612 (0.0295)		0.7132	1.0364
3	7.2297** (2.1155)	0.0030* (0.0012)	0.3673** (0.1008)					0.1645** (0.0323)	0.8428	0.0003
4	7.2746** (2.1743)	0.0029* (0.0013)	0.3652** (0.1036)				0.0053 (0.0261)	0.1604** (0.0385)	0.8356	0.0018

NOTE: **/ indicates significance at the 95/99 percent level. †Q-statistic indicates the presence of autocorrelated residuals.

Table A2**Trend Analysis Using Employment Data to Control for Economic Factors**[Dependent Variable = $\ln(\text{E\&D_Maryville})$]

	Constant	Trend	X	Q2	Q3	Q4	SmokeBan	Applebee's	Adjusted R ²	Q
A. X = $\ln(\text{Nodaway Employment})$										
1	13.1760** (1.2127)	0.0066** (0.0014)	0.6729 (0.4772)	0.0665* (0.0243)	0.0622 (0.0300)	0.0251 (0.0265)			0.6748	5.3472 ⁺
2	13.9639** (1.1526)	0.0051** (0.0015)	0.3687 (0.4528)	0.0713** (0.0222)	0.0499 (0.0277)	0.0312 (0.0242)	0.0682* (0.0296)		0.7322	3.1574
3	14.2822** (0.8496)	0.0052** (0.0010)	0.2427 (0.3341)	0.0604** (0.0165)	0.0592** (0.0203)	0.0473* (0.0184)		0.1691** (0.0340)	0.8513	0.8511
4	14.3714** (0.8817)	0.0050** (0.0011)	0.2085 (0.3464)	0.0619** (0.0170)	0.0569* (0.0211)	0.0470* (0.0188)	0.0143 (0.0265)	0.1576** (0.0408)	0.8456	0.8619
B. X = Nodaway Unemployment Rate										
1	15.0337** (0.0539)	0.0107** (0.0014)	-0.0907** (0.0309)	0.0369 (0.0243)	0.0127 (0.0235)	-0.0336 (0.0326)			0.7502	5.9226 ⁺
2	15.0110** (0.0517)	0.0084** (0.0018)	-0.0695* (0.0308)	0.0468 (0.0233)	0.0175 (0.0222)	-0.0166 (0.0317)	0.0537 (0.0273)		0.7814	4.2901 ⁺
3	14.9697** (0.0416)	0.0073** (0.0013)	-0.0444 (0.0247)	0.0463* (0.0177)	0.0366 (0.0179)	0.0153 (0.0261)		0.1494** (0.0340)	0.8694	1.5735
(4.)	14.9687** (0.0426)	0.0070** (0.0015)	-0.0427 (0.0255)	0.0478* (0.0184)	0.0362 (0.0183)	0.0159 (0.0267)	0.0111 (0.0247)	0.1406** (0.0398)	0.8637	1.6207

NOTE: ** Indicates significance at the 95/99 percent level. ⁺Q-statistic indicates the presence of autocorrelated residuals.

Table A3 presents the results of regressions using these ratio-dependent variables. A downward trend is found for the ratio of bar and restaurant sales relative to total retail sales in Maryville, but no trend is evident in the ratio of sales in Maryville relative to Missouri. Seasonal effects are significant in both sets of regressions, indicating that seasonal patterns in Maryville bar and restaurant sales differ from total retail sales in Maryville and from bar and restaurant sales for the state of Missouri.⁴¹

The first panel, using the ratio of bar and restaurant sales relative to total retail sales in Maryville as the dependent variable, shows that the effect of the smoking-ban dummy is positive and highly significant when included without the Applebee's variable in the regression. However, the Applebee's effect again provides for a better fit, and it remains significant (whereas the smoking-ban dummy does not) when both are included in the regression.⁴²

The second panel of Table A3 reports the results of using the ratio of bar and restaurant sales for Maryville to bar and restaurant sales for Missouri as the dependent variable. In this set of regressions, the

⁴¹ Given the insignificance of seasonal dummy variables reported in Panel B1 of Table 2, this finding indicates that seasonal sales patterns in Maryville are different from, but have a predictable relationship to, total Missouri bar and restaurant sales. This is likely attributable to the nature of Maryville as a college town, home to Northwest Missouri State University.

⁴² The ratio of bar and restaurant sales to total sales for the state of Missouri was considered as an additional explanatory variable, but its inclusion did not improve the overall fit of the regression, nor did it alter any of the results of hypothesis tests.

⁴³ Two ratios were considered as additional explanatory variables for this specification: Total retail sales for Maryville relative to the state of Missouri and Nodaway employment relative to Missouri employment. Neither variable improved the fit of the equation or altered the results.

Table A3**Analysis of Ratios**

	Constant	Trend	Q2	Q3	Q4	SmokeBan	Applebee's	Adjusted R ²	Q
A. Dependent Variable = (E&D_Maryville/Total_Maryville) × 100									
1	10.4377** (0.1944)	-0.0152 (0.0104)	0.1605 (0.2115)	-0.5295* (0.2199)	-1.1162** (0.2202)			0.6112	3.2560
2	10.6060** (0.1660)	-0.0383** (0.0108)	0.1837 (0.1726)	-0.5477** (0.1794)	-1.1113** (0.1795)	0.7658** (0.2251)		0.7414	0.0787
3	10.5365** (0.1529)	-0.0311** (0.0090)	0.0846 (0.1652)	-0.4377* (0.1722)	-1.0085** (0.1730)		1.2854** (0.3327)	0.7662	0.0861
4	10.6037** (0.1498)	-0.0396** (0.0098)	0.1206 (0.1580)	-0.4756** (0.1647)	-1.0379** (0.1649)	0.4401 (0.2455)	0.9008* (0.3816)	0.7895	0.4627
B. Dependent Variable = (E&D_Maryville/E&D_Missouri) × 100									
1	0.2431** (0.0049)	0.0001 (0.0003)	-0.0133* (0.0053)	-0.0253** (0.0055)	-0.0057 (0.0056)			0.4321	4.0358 [†]
2	0.2457** (0.0049)	-0.0003 (0.0003)	-0.0129* (0.0051)	-0.0256** (0.0053)	-0.0056 (0.0053)	0.0118 (0.0066)		0.4854	2.7680
3	0.2459** (0.0035)	-0.0004 (0.0002)	-0.0154** (0.0038)	-0.0228** (0.0040)	-0.0027 (0.0040)		0.0357** (0.0077)	0.7130	0.9435
4	0.2456** (0.0037)	-0.0003 (0.0002)	-0.0155** (0.0039)	-0.0226** (0.0041)	-0.0026 (0.0041)	-0.0016 (0.0061)	0.0371** (0.0095)	0.6989	0.8795

NOTE: **/ indicates significance at the 95/99 percent level. [†]Q-statistic indicates the presence of autocorrelated residuals.

smoking-ban dummy variable is not significant even when included in the absence of the Applebee's effect. The Applebee's dummy variable is highly significant with or without controlling for the effect of the smoking ban. When both variables are included in the regression, the coefficient on the smoking-ban dummy is negative, but insignificant.



What Do We Know About Oil Prices and State Economic Performance?

David A. Penn

The persistent rise of oil and gasoline prices during the past few years raises the issue of the effect of oil prices on the aggregate economy. Recent research shows that oil prices have an asymmetric effect: Rising prices have a measurable negative impact on aggregate economic activity, but falling prices do not have a commensurate positive impact. This study examines the effect of oil price changes on the states of the Eighth Federal Reserve District, using various measures of oil price increases. The study finds that some states are more sensitive to oil price changes than others. The study also finds only limited support for the asymmetry hypothesis at the state level. (JEL R11, Q43)

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Recent gasoline price increases have caused significant economic heartburn for households, energy-sensitive businesses, and transportation-sensitive government agencies. Households that loaded up on gas-guzzling sport utility vehicles when gasoline prices were low are now especially feeling the pinch in their pocketbooks. Transportation-intensive businesses such as airlines, delivery, and trucking have been hit hard by fuel price increases, with limited ability to pass cost increases on to customers. School systems that transport large numbers of students to and from school have been hit hard, as have state and local highway departments that depend heavily on petroleum-derived asphalt for road construction and maintenance. Rising gasoline prices have forced households, businesses, and governments to adjust by consuming less energy or spending less on everything else. High gasoline prices have changed vehicle buying preferences, with sales of large SUVs down about 6 percent from last year.

Clearly, higher gasoline prices have changed household (and probably business and government) spending habits. The issue for this study is this:

How has the increase in oil and gasoline prices affected the economies of states in the Eighth Federal Reserve District? The effect of higher oil prices on the national economy has received a fair degree of attention in the literature, but the impact on state economies has received much less attention.

LITERATURE

During the past two decades a number of studies have explored the effect of oil prices on the national economy, concluding that oil prices and aggregate measures such as output or employment are negatively related: that is, rising oil prices cause the economy to slow, while falling oil prices stimulate the economy.¹ More recent research on this matter, however, shows that since the mid-1980s the connection from oil prices to economic activity has changed; current thinking by economist is that rising oil prices generate a negative impact on aggregate economic activity, but falling prices have little effect (Hamilton, 2003). What is more, oil price

¹ Hamilton (2003) offers an overview of this scholarship.

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Penn

increases that simply average out recent price declines have little effect. The next section of this paper applies current thinking about the oil price–economy connection at the national level to the states in the district of the Federal Reserve Bank of St. Louis.

APPROACH AND DATA

The model for this study follows that outlined by Hamilton (2003) and Mehra and Petersen (2005). In brief, Hamilton measures the sensitivity of quarterly gross domestic product (GDP) growth to alternative measures of oil price changes; he finds that oil price increases matter while price declines do not, especially since the early 1980s. And rising oil prices matter more when the increase does not simply correct a recent decline. Drawing on Hamilton’s work, Mehra and Petersen investigate the effect of various measures of oil price change on consumer spending at the national level. Similar to Hamilton, they find that oil price increases that follow a recent peak matter for consumer spending.

In the discussion below, I adapt and apply the model in Mehra and Petersen (2005) to show how state economic output is affected by changing oil prices, focusing just on the states of the Eighth Federal Reserve District. Autoregressive distributed lag (ARDL) models are estimated for each state, using various measures of oil price change. The models are autoregressive because previous values of real earnings help explain current real earnings.

Details of the model are shown in the following equation:

$$\Delta y_t = \beta_0 + \sum_{i=1}^4 \beta_{1i} \Delta y_{t-i} + \sum_{i=1}^4 \beta_{2i} \Delta \text{oilprices}_{t-i} + \sum_{i=1}^4 \beta_{3i} \Delta \text{Fedfunds}_{t-i}.$$

The equation shows how quarterly real output growth (Δy_t) depends on growth of real output in the previous four quarters (Δy_{t-i}), the change in oil prices from the previous four quarters ($\Delta \text{oilprices}_{t-i}$), and the change in the federal funds rate from the previous four quarters ($\Delta \text{Fedfunds}_{t-i}$). The coefficient

$$\sum_{i=1}^4 \beta_i$$

is the sum of the coefficients for the four lagged values of real income, oil prices, and federal funds.

For the measure of oil prices, I use the oil and gasoline deflator published by the Bureau of Economic Analysis (BEA), deflated using the GDP deflator. By using national figures for oil and gasoline prices, I impose the restriction that oil prices in the various states fluctuate in the same pattern as they do nationally. Following Mehra and Petersen (2005), nominal short-term interest rates are also included in the state models, as measured by the federal funds rate adjusted for inflation using the GDP chain-weighted deflator. Measuring quarterly real output presents a problem, because state-level data for quarterly GDP do not exist. A proxy that mimics the growth rate of gross state product (GSP) on a quarterly basis is needed. Earned income (or just earnings) fits the bill well; the largest component of value-added, earned income includes all payroll for all hourly and salaried workers plus all income earned by the self-employed. Comparing annual earnings growth for the seven states with annual growth of GSP shows a close correspondence, with an R^2 of 0.8 or more.

As in Hamilton (2003) and Mehra and Petersen (2005), the effects of oil prices are tested using three different measures. First, the *oil price change* is simply the quarterly change of the inflation-adjusted oil and gasoline price index from the BEA. The second measure, *positive oil price change*, restricts price changes to positive changes only; otherwise, the measure is set equal to zero. Finally, the *net oil price change* measures a positive change from a recent previous maximum, thereby excluding price increases that simply correct a recent price decline. I use both four-quarter and eight-quarter horizons to determine the previous maximum; calculation details are provided in Appendix A. Depictions of the oil price change, positive oil price change, and net oil price change (four-quarter and eight-quarter horizons) are shown in Figures 1 through 4.

Using these measures of oil prices, the study will test the following propositions:

Figure 1

Quarterly Oil Price Change, 1960-2005 (first differences of logs)

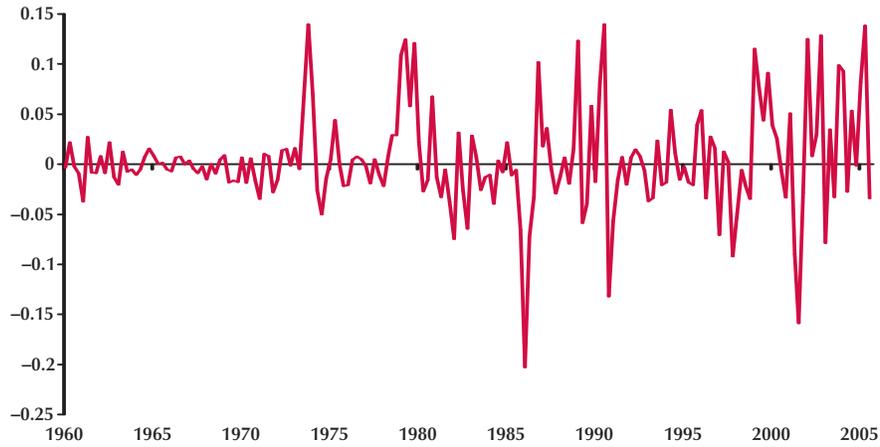


Figure 2

Positive Quarterly Oil Price Change, 1960-2005 (first differences of logs)

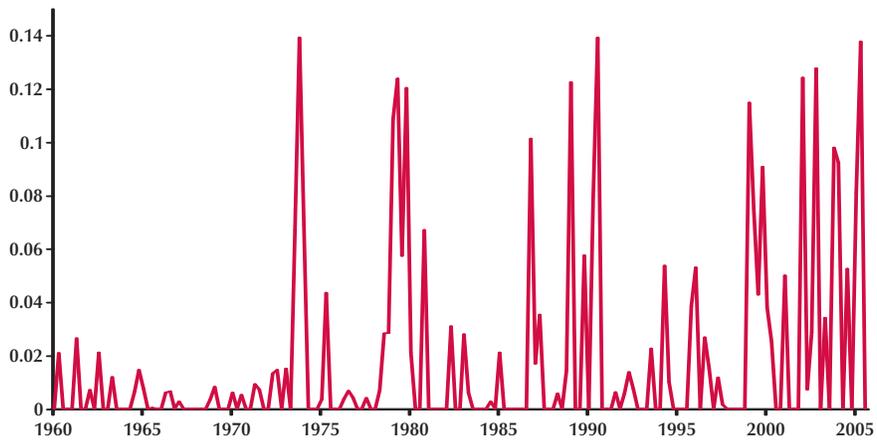


Figure 3

Net Quarterly Oil Price Change, Four-Quarter Horizon (first differences of logs)

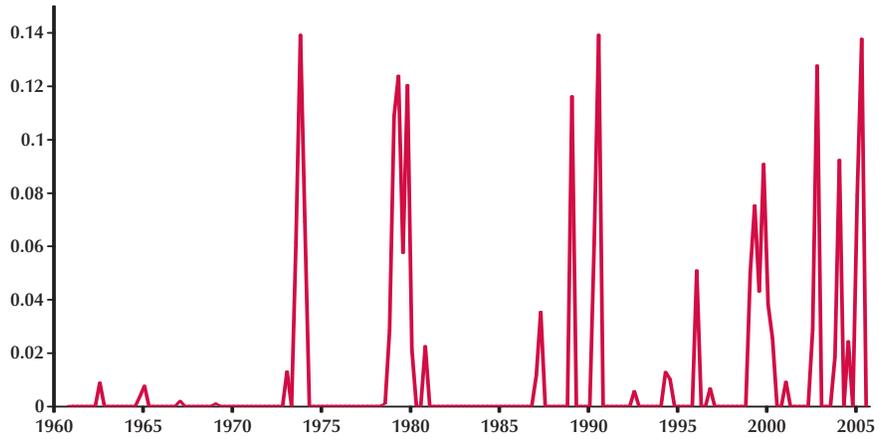


Figure 4

Net Quarterly Oil Price Change, Eight-Quarter Horizon (first differences of logs)

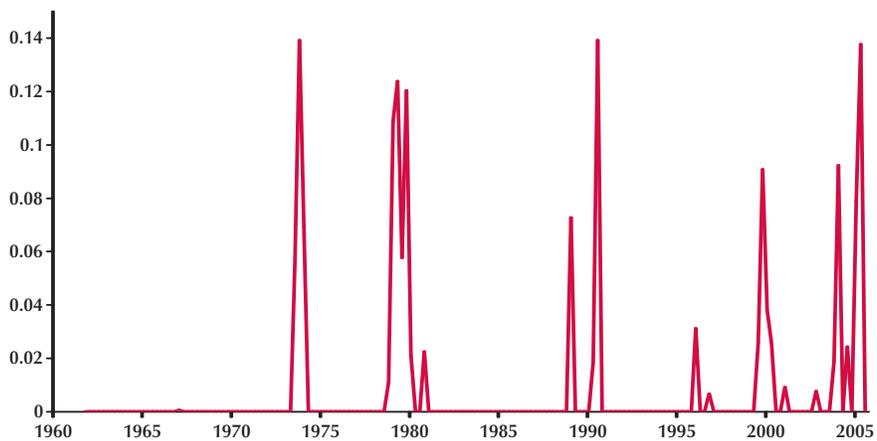


Table 1**Consumption of Gasoline, Distillates, and Jet Fuel per Dollar of GSP, 2002**

State	BTU per dollar GSP	National rank
Arkansas	4.33	9
Illinois	1.95	48
Indiana	3.41	19
Kentucky	4.09	11
Mississippi	5.03	4
Missouri	3.26	24
Tennessee	3.26	23
U.S.	2.71	

SOURCE: Compiled from the Energy Information Administration and Bureau of Economic Analysis.

Table 2**Expenditures for Gasoline, Distillates, and Jet Fuel per Hundred Dollars of GSP, 2002**

State	Spending	National rank
Arkansas	4.14	8
Illinois	1.99	48
Indiana	3.15	20
Kentucky	3.86	11
Mississippi	3.07	23
Missouri	4.55	4
Tennessee	2.96	26
U.S.	2.56	

SOURCE: Compiled from the Energy Information Administration and Bureau of Economic Analysis.

Proposition 1: Simple oil price changes (positive and negative) don't matter.

Proposition 2: Oil price increases matter.

Proposition 3: Net oil price increases matter more.

PETROLEUM CONSUMPTION IN THE EIGHTH FEDERAL RESERVE DISTRICT

Examining the pattern of petroleum consumption and expenditures may offer clues concerning the connection of oil prices and state economic activity. Using data from the Energy Information Agency and the BEA, a measure of the energy intensity of each state can be estimated by dividing the measure of energy usage by GSP, resulting in the amount of energy consumption or expenditure needed to produce one dollar of GSP. Table 1 shows consumption of gasoline, distillates (diesel), and jet fuel per dollar of GSP for 2002. Among the Eighth District states, Mississippi and Arkansas consume much more energy per dollar of GSP than most of the other states of the union. Kentucky and Indiana are clearly above the United States average, whereas Tennessee and Missouri are slightly above average. Only Illinois ranks below the national average in energy intensity—in fact, greatly below. Energy intensity varies considerably within the District,

ranging from 1.95 British thermal units (BTU) per dollar of GSP for Illinois to 5.03 BTU for Mississippi.

Another view of energy intensity can be derived by examining spending for energy instead of units of energy consumed. Of course, if energy prices vary among the states, the pattern of energy expenditures may differ from energy consumption per unit of GSP. Table 2 shows spending for gasoline, distillates, and jet fuel per hundred dollars of GSP for 2002. National rankings are the same as for Table 1 except for Missouri and Mississippi; Missouri ranks high and Mississippi about average on this measure of energy intensity. One would expect a priori that the more energy-intensive states will be more sensitive to changes in energy prices. I shall test this proposition later in the paper.

Another important measure of energy intensity is gasoline spending per capita, providing evidence of the energy intensity for the transportation sector. In this regard the Eighth District states show wide divergence. Illinois ranks 46th lowest among the 50 states and Washington, D.C., in terms of spending per capita for gasoline, with \$572 in 2002; this is substantially below the United States average of \$623 per capita. The other six states in the District rank above the United States average: Indiana ranks 29th (\$651 per capita), Tennessee 26th (\$658), Arkansas 17th (\$681), Kentucky 16th (\$691), Mississippi 12th (\$702), and Missouri 11th (\$708).

Table 3
State Motor Gasoline Taxes (cents per gallon)

State	Tax
Tennessee	21.00
Kentucky	18.50
Indiana	18.00
Illinois	19.00
Mississippi	18.40
Missouri	17.00
Arkansas	21.50
Average of 50 states	21.17

NOTE: State rates are effective January 1, 2006. Additional taxes are levied by these states: Illinois (6.25 percent sales tax), Indiana (6 percent sales tax). Local sales taxes may also be applicable.

SOURCE: Energy Information Administration.

By comparison, Wyoming ranks highest for per capita spending for gasoline, at \$875 in 2002. Judging from these spending figures, we may expect that, with the exception of Illinois, an increase in gasoline prices in the Eighth Federal Reserve District states will likely have a greater impact than in most other states.

Gasoline taxes can also have an impact on consumption. Table 3 shows current state government gasoline tax rates per gallon consumed. Only in Arkansas does the state gasoline tax rate exceed the 50-state average. For Illinois and Indiana, taxes are more complex; in addition to the state tax per gallon, additional state and local sales taxes apply.

Taking into account transportation costs and local taxes, retail gasoline prices may differ considerably, both within states and between states. However, given tax rates and transportation costs, it is reasonable to assume that *changes* in prices will be roughly equivalent across areas.

RESULTS

The model estimated in this study is dynamic; real earnings depend on oil prices and past values for real earnings. And past real earnings depend on past oil price changes. Thus, we can think of two channels for the impact of oil prices; the direct

impact on current real earnings and an indirect impact by way of past earnings. The former channel is estimated by the oil price coefficient, whereas the latter channel is estimated by the oil price multiplier. Both will be discussed below.

Given the context of petroleum expenditures in Table 2, one would expect the largest effects of oil price hikes to occur in Missouri, Arkansas, and Kentucky, with more modest impacts in Indiana, Mississippi, and Tennessee. The smallest impact is expected for Illinois.

First I must make several assumptions about the suitability of the model variables and later will speculate about the sensitivity of the model estimates if the assumptions are incorrect. As discussed earlier, I assume that earned income is a good proxy for GSP. Earnings growth may not be a good proxy for growth in other GSP components, such as profits, interest income, and indirect taxes. Second, we assume that oil and gasoline prices change by the same proportion across the seven states and that these changes are accurately measured by the BEA oil and gasoline price deflator. Finally, we assume that changes in the general level of prices are the same across the states, so that we may apply the national GDP price deflator to the oil price index and to earned income to adjust for changes in the general price level. The dependent variable is the quarterly change of real earned income; specifically, the first difference (quarter-to-quarter change) of the natural log of real earned income. Oil price changes also enter the regressions as differences of logs, whereas the interest rate is the simple quarter-to-quarter difference. Earnings and oil prices are deflated using the consumer expenditure deflator and the GDP deflator, respectively. The structure of the model is this: Real earnings growth is believed to depend on the previous four quarters of oil price growth, interest rate changes, and changes in real earned income.

Oil price coefficients are presented in Table 4 for each of the four measures of oil prices,² using private sector real earnings growth as the dependent variable. The estimates are the sum of the four lagged coefficients from the regressions; *t*-values

² Complete results for Tennessee are presented in Appendix B. Complete results for other states are available on request.

Table 4**Oil Price Coefficients by State**

State	Oil price change	Positive oil price change	Net oil price change (4)	Net oil price change (8)
Tennessee	-0.026 (0.83)	-0.115 (2.79)	-0.115 (2.36)	-0.119 (2.13)
Kentucky	-0.063 (1.74)	-0.129 (2.66)	-0.122 (2.18)	-0.141 (2.18)
Indiana	-0.011 (0.32)	-0.103 (2.12)	-0.108 (1.86)	-0.120 (1.63)
Illinois	-0.011 (0.32)	-0.063 (1.39)	-0.103 (2.20)	-0.151 (2.99)
Mississippi	0.002 (0.07)	-0.090 (2.04)	-0.071 (1.40)	-0.065 (1.12)
Missouri	-0.013 (0.40)	-0.095 (2.24)	-0.115 (2.38)	-0.135 (2.49)
Arkansas	-0.016 (0.49)	-0.101 (2.27)	-0.098 (1.96)	-0.114 (2.07)

NOTE: The *t*-values are in parentheses. Coefficients are the sum of values for four lags. Net oil price (*) is the net change of oil prices evaluated at four- and eight-quarter horizons. Values show the percent change in current real income from a 1 percent change in the price of oil sustained for four quarters.

are shown in parentheses. The coefficients show the negative impact on a state's current real earnings caused by a sustained four-quarter increase in oil prices. For example, if oil prices rose 10 percent per quarter for four quarters in Tennessee, the quarterly real earnings growth rate would be reduced by 1.15 percent.

Several things in the table are worth mentioning. First, none of the coefficients for the simple oil price change measure (first column) are significantly different from zero at the 5 percent level. This result is consistent with Hamilton (2003) and Mehra and Petersen (2005) for the national economy. Second, positive changes in oil prices matter for all but one state, Illinois. For the other six states, the positive oil price change coefficients range from -0.09 in Mississippi to -0.129 in Kentucky, with significance of 5 percent or better. Last, oil price increases do matter for Illinois, but only when measured as a net price increase. We may surmise from these results that the six states excluding Illinois show some sensitivity to simple oil price increases, regardless of whether they simply correct recent price decreases. Illinois appears to show more resilience to oil price increases.

In this regard, the results differ from the findings of Hamilton (2003) and Mehra and Petersen (2005) for the national economy in that net oil price changes do *not* matter more than positive price changes for three states: Tennessee, Mississippi,

and Arkansas. For the other four states, net changes evaluated at either the four- or eight-quarter horizons do matter more than simple positive changes, especially for Illinois.

Given a 1 percent change in oil prices this quarter, how much will real earnings decline in the future? The long-term link between oil price changes and real earnings growth is the long-term multiplier. The multiplier shows the effect of an oil price increase on real earnings growth four quarters later, taking into account the direct effect of oil prices on earnings and the indirect effect as oil price increases ripple throughout the economy. Oil price multipliers are shown for the Eighth District states in Table 5 for private sector earnings. The positive oil price change multiplier for Tennessee is -0.222, which means that a 1 percent increase in the price of oil sustained for each of four quarters will cause real private sector earnings to grow 0.22 percent less than would have occurred in the absence of higher oil prices.

The relative size of the multipliers in this table are similar to the oil price coefficients in Table 3: Positive oil price changes matter for six states, and the net oil price change matters for Illinois. Only for Illinois do net oil price changes matter more than positive oil price changes, the result Hamilton found for the national economy. The correspondence of the Illinois result with the national economy may well be due to the relative size of the

Table 5**Long-Term Oil Price Multiplier**

State	Positive oil price change	Net oil price change (4)
Tennessee	-0.222 (3.40)	-0.224 (2.98)
Kentucky	-0.189 (3.07)	-0.196 (2.59)
Indiana	-0.219 (2.26)	-0.255 (2.07)
Illinois	-0.150 (1.54)	-0.219 (2.55)
Mississippi	-0.151 (2.38)	-0.132 (1.60)
Missouri	-0.172 (2.77)	-0.200 (2.98)
Arkansas	-0.208 (2.59)	-0.209 (2.23)

NOTE: The *t*-values are in parentheses. Coefficients show the effect on private real earnings growth from a 1 percent four-quarter sustained rise in oil prices. The multiplier is calculated as the sum of the four lagged oil price coefficients divided by 1 minus the sum of the four lagged earnings coefficients.

state's economy; with a GSP of \$521.9 billion in 2004, Illinois is fifth largest among the 50 states³ and more than twice as large as the second state (Indiana) in the Eighth District. In Mississippi's case, the small size of the multiplier might be attributable to the substantial presence of oil production in the state, particularly offshore production. In 2004, Mississippi ranked 12th largest in terms of oil production, averaging 47,000 barrels per day, about 50 percent more than Illinois (30,000 barrels per day). While higher oil prices undoubtedly affect consumer spending and transportation-sensitive businesses in Mississippi, the oil and gas production sector in Mississippi benefits. The negatives are offset by the positives, and the state shows only a small net difference in real earnings due to oil price increases.⁴

³ Illinois ranks in size of GSP behind California, New York, Texas, and Florida.

⁴ Although oil production for Illinois is substantial, the size of the sector relative to the state's GSP is small and has little relative impact.

CONCLUSIONS

We may conclude that for six of the seven states in the Eighth District, positive oil price changes matter, whereas simple oil price changes (positive and negative) do not, as Hamilton (2003) and Mehra and Petersen (2005) found for the national economy. However, net oil price changes matter only for Illinois, probably because of the size and similarity with the national economy.

Also, measures of energy intensity do a poor job predicting the sensitivity of state economies to oil price changes, with the exception of Illinois. Much more work is needed to explore the importance of oil prices and state economies. What factors more fully explain the differences between states in terms of oil price sensitivity? This and other questions, such as the stability of the oil price coefficients, need more attention by economists.

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APPENDIX A

Calculating the Net Oil Price Change

The net oil price change is computed by comparing the current value of the real oil and gas price index with its maximum over the previous four quarters. More specifically, let $Oilprice_i$ indicate the value of the oil and gas price index for the current period i , and let $Maxoilprice_{i-4}$ indicate the maximum of the index over the previous four quarters. Then the net oil price change ($\Delta Netoilprice_i$) is:

$$\Delta Netoilprice_i = (Oilprice_i - Maxoilprice_{i-4}) \text{ if } Oilprice_i > Maxoilprice_{i-4}, 0 \text{ if } Oilprice_i \leq Maxoilprice_{i-4}.$$

APPENDIX B

Detailed Model Estimates for Tennessee Using Quarterly Data, 1960-2005

Each model estimates a different measure of oil price change. The dependent variable is the log difference of quarterly real earned income.

Variable	Model			
	One	Two	Three	Four
Change in earnings ₁₋₄	0.621 (5.77)	0.480 (4.26)	0.489 (4.10)	0.504 (4.09)
Change in federal funds ₁₋₄	-0.008 (4.45)	-0.007 (4.32)	-0.007 (4.01)	-0.007 (3.90)
Oil price change ₁₋₄	-0.026 (0.83)			
Positive oil price change ₁₋₄		-0.115 (3.40)		
Net oil price change(4) ₁₋₄			-0.115 (2.98)	
Net oil price change(8) ₁₋₄				-0.119 (2.13)
Oil price multiplier	-0.069 (0.91)	-0.222 (3.40)	-0.224 (2.98)	-0.240 (2.73)
Adjusted R ²	0.340	0.370	0.357	0.358
Log likelihood	585.2	589.2	577.0	562.7

NOTE: The t -values are in parentheses. Coefficients are the sum of estimates for four lags. The oil price multiplier is the oil price coefficient divided by 1 minus the earnings coefficient.



The Long-Run Relationship Between Consumption and Housing Wealth in the Eighth District States

David E. Rapach and Jack K. Strauss

The authors examine the long-run relationship between consumption and housing wealth for the seven individual states in the Federal Reserve System's Eighth District. Given that state-level consumption data are not available, the authors develop a novel proxy for state-level consumption based on state-level data for personal income and savings income. They use this consumption proxy to estimate a cointegrating relationship between consumption spending and housing wealth, stock market wealth, and income in each of the Eighth District states. Their results indicate that increases in housing wealth produce sizable increases in consumption for most of the states in the Eighth District. Interestingly, the authors also find that consumption typically responds much more strongly to changes in housing wealth than to changes in stock market wealth. Their results imply that the strong increases in housing prices and home construction over the past decade have helped to buoy consumption and decrease saving in most Eighth District states. (JEL C32, C33, E21)

Federal Reserve Bank of St. Louis *Regional Economic Development*, 2006, 2(2), pp. 140-47.

High levels of consumption spending—which have driven the personal saving rate below zero during the past year—together with continued increases in housing prices are two U.S. economic facts that currently receive considerable attention in both the popular and financial press. It is natural to speculate that these two facts are linked, and analysts have posited that the strong increases in housing wealth experienced over the past decade in the United States have played an important role in stimulating household spending. There is also concern that a slowing of the housing market in the near future will depress household spending and help precipitate a general economic slowdown. For example, Ben Bernanke, current Chairman of the Board of Governors of the Federal Reserve System, remarked in early 2006 that “given the

substantial gain in house prices and the high levels of home construction activity over the past several years, prices and construction could decelerate more rapidly than currently seems likely. Slower growth in home equity, in turn, might lead households to boost their saving and trim their spending relative to current income by more than is now anticipated” (Bernanke, 2006).

The general interest in the link between consumption spending and housing wealth, along with its potential interest to policymakers, motivates the present paper, where we undertake a formal econometric analysis of the long-run relationship between consumption spending and housing wealth. We concentrate on the relationship between consumption spending and housing wealth in the seven states of the Federal Reserve System's Eighth District (Arkansas, Illinois, Indiana, Kentucky, Missouri, Mississippi, and Tennessee).

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There is already a large body of research, falling under the rubric of the “wealth effect,” that examines the relationship between consumption spending and household wealth.¹ This literature either focuses on the response of consumption to changes in financial wealth alone—especially stock market wealth—or assumes that all forms of wealth are viewed equivalently by households. As stressed by Case, Quigley, and Shiller (2005), this is a potentially important drawback to this literature: Households may view different forms of wealth differently, so that consumption can respond differently to changes in financial compared with housing wealth. For example, financial market frictions, due to certain types of liquidity constraints created by information asymmetries, may make it easier for households to increase their consumption by borrowing against increases in housing values, as evidenced by the sharp rise in home equity loans that have accompanied the strong increases in housing values over the past decade.² In addition, households may separate their wealth into different “mental accounts,” so that changes in different categories have different effects on household consumption (the psychology of “framing”).

In contrast to the substantial literature on the wealth effect, there is a relatively small literature that specifically examines the response of consumption spending to changes in housing wealth.³ Nevertheless, some recent studies suggest that there are important differences in how consumption responds to changes in financial and housing wealth. Using aggregate U.S. data, Benjamin, Chinloy, and Jud (2004) estimate that the marginal propensity to consume from real estate wealth is approximately four times larger than the marginal propensity to consume from financial wealth. Using a panel of U.S. state-level data, Case, Quigley, and Shiller (2005) find that household wealth has a significant and sizable effect on household consumption, an effect that is significantly larger than that of stock

market wealth. The present paper contributes to this recent literature by analyzing the long-run relationship between consumption spending and housing wealth in the Eighth District states.

Our econometric methodology involves estimating a cointegrating relationship between real consumption spending, housing wealth, stock market wealth, and income (a “long-run consumption function”) in each of the Eighth District states. A challenge in estimating a long-run consumption function for individual states is that state-level consumption data are not readily available. We develop a novel proxy for consumption spending at the state level that allows us to estimate a cointegrating relationship that is informative about the long-run relationship between actual (but unobserved) consumption and housing wealth in each Eighth District state. In analyzing cointegrating relationships, we pay careful attention to the integration properties of all the variables appearing in our model. Unit root tests, including unit root tests for heterogeneous panels, indicate that all of the variables in our model (more precisely, their log-levels) are integrated processes, so that it is appropriate to consider potential cointegrating relationships.

We estimate cointegrating relationships using a number of well-known procedures, and we find that consumption is significantly and positively related to housing wealth in most of the Eighth District states. We also find that housing wealth typically has a much stronger effect on consumption than stock market wealth. Panel cointegration tests support the existence of cointegrating relationships in a significant portion of the Eighth District states. Our finding of a significant and sizable housing wealth effect on consumption is in line with the recent studies of Benjamin, Chinloy, and Jud (2004) and Case, Quigley, and Shiller (2005), and our results support the conjecture that increases in housing wealth over the past decade have contributed significantly to strong consumption growth.

There is an important way in which our results differ from Benjamin, Chinloy, and Jud (2004) and Case, Quigley, and Shiller (2005): The homogeneity assumptions implicit in both of these studies are likely to be inappropriate and can mask important differences in the responses of consumption to

¹ See, for example, the surveys in Ludvigson and Steindel (1999), Poterba (2000), and Davis and Palumbo (2001).

² See Greenspan and Kennedy (2005).

³ Case, Quigley, and Shiller (2005) provide a survey of this literature, and they note that most of the studies in this area are micro studies of consumer behavior. They conclude that there is “much ambiguity in the interpretation of statistical results.”

changes in housing wealth across regions of the United States. For example, we find that the consumption response to a change in housing wealth is much stronger in Illinois than it is in Arkansas. In summary, the housing wealth effect is not uniform across the Eighth District states.

The next section describes our estimation strategy, and the following section reports our estimation results.

ESTIMATION STRATEGY

An important problem in analyzing the relationship between consumption and housing wealth in individual states is that consumption data are not readily available at the state level. In this section, we outline our strategy of using a proxy for state-level consumption that enables us to analyze the long-run relationship between consumption and housing wealth in the individual states of the Eighth District.

A state's household consumption is clearly equal to the difference between a state's personal disposable income and personal saving. While state-level income data are available from the Bureau of Economic Analysis (BEA), state-level personal saving data are not available. However, the BEA does report personal savings income, which consists of dividend, interest, and rental income from prior accumulated savings. It is likely that permanent changes in household saving will lead to permanent changes in the flow of income derived from accumulated savings; we exploit this likely link between saving and savings income to construct a proxy for consumption at the state level that enables us to analyze the long-run relationship between consumption and housing wealth at the state level. More specifically, we use available data to construct a proxy (personal disposable income minus personal savings income) for actual—but unavailable—consumption (personal disposable income minus personal saving). If there is a stable long-run relationship between actual consumption and our proxy, then we can use our proxy to analyze the long-run relationship between actual consumption and housing wealth. We emphasize that we view our consumption proxy only as a useful long-run proxy,

such that it will not necessarily be informative with respect to short-run dynamics.⁴

Let $c_{i,t}^S$ equal the log-level of the difference between personal disposable income and personal saving in state i , and let $c_{i,t}^{DIR}$ equal the log-level of the difference between personal disposable income and savings income in state i (after both differences have been converted to real terms). Assuming $c_{i,t}^S, c_{i,t}^{DIR} \sim I(1)$ (as is likely to be the case), a stable long-run relationship exists between $c_{i,t}^S$ and $c_{i,t}^{DIR}$ when these two variables are cointegrated [$c_{i,t}^S, c_{i,t}^{DIR} \sim CI(1,1)$], and the cointegrating relationship can be expressed as

$$(1) \quad c_{i,t}^S = \alpha_i + \beta_i c_{i,t}^{DIR} + e_{i,t},$$

where $e_{i,t}$ is an $I(0)$ disturbance term with an unconditional mean of zero. If a cointegrating relationship of the form in equation (1) exists for each individual state in the Eighth District, we can exploit this to analyze the long-run relationship between consumption spending and housing wealth for each state.

Consider the possibility of the existence of a stable long-run relationship between consumption and housing wealth, as well as stock market wealth and income, in state i :

$$(2) \quad c_{i,t}^S = \gamma_i + \delta_{i,hw} hw_{i,t} + \delta_{i,sw} sw_{i,t} + \delta_{i,y} y_{i,t} + u_{i,t},$$

where $hw_{i,t}$ is the log-level of real housing wealth in state i , $sw_{i,t}$ is the log-level of real stock market wealth in state i , $y_{i,t}$ is the log-level of real personal disposable income in state i , and $u_{i,t}$ is a stationary, zero-mean disturbance term.⁵ Equation (2) is a standard type of specification for a long-run con-

⁴ Given the household budget constraint that labor income plus savings income equals consumption plus saving, using personal income minus personal savings income as a proxy for consumption essentially assumes that labor income serves as a proxy for consumption. We also note that changes in rates of return potentially affect savings income in ways that have an impact on saving behavior, but it is likely that these effects are small relative to the long-run effect that we isolate. Overall, whether personal disposable income minus personal savings income is a reasonable proxy for consumption over the long run is an empirical matter, and we present evidence below that there is a stable long-run relationship between our proxy for consumption and actual consumption for aggregate U.S. data.

⁵ We could also allow a linear time trend in equation (2), but this does not affect our results in important ways, as the estimates for equation (3) reported in Table 2 change little if we include a linear time trend in equations (2) or (3). Complete results with a linear time trend included are available from the authors upon request.

sumption function; see, for example, Davis and Palumbo (2001). While we ideally would analyze equation (2) directly, as discussed above, we cannot estimate equation (2) directly because state-level data for $c_{i,t}^S$ are not available. However, we can use equation (1) to substitute for $c_{i,t}^S$ in equation (2):

$$(3) \quad c_{i,t}^{DIR} = \zeta_i + \theta_{i,hw} hw_{i,t} + \theta_{i,sw} sw_{i,t} + \theta_{i,y} y_{i,t} + \varepsilon_{i,t},$$

where $\zeta_i = (\gamma_i - \alpha_i) / \beta_i$;

$\theta_{i,j} = \delta_{i,j} / \beta_i$ for $j = hw, sw, y$; and $\varepsilon_{i,t} = (u_{i,t} - e_{i,t}) / \beta_i$

Note that $\varepsilon_{i,t}$ is a stationary, zero-mean process, as both $u_{i,t}$ and $e_{i,t}$ are stationary, zero-mean processes.

Equation (3) provides considerable information about the parameters of interest in equation (2). Note the following:

- (i) $\delta_{i,k} = 0$ implies $\theta_{i,k} = 0$ (assuming $\beta_i < \infty$);
- (ii) $\beta_i > 0$ implies $sign(\theta_{i,k}) = sign(\delta_{i,k})$;
- (iii) $\delta_{i,j} > \delta_{i,k}$ implies $\theta_{i,j} > \theta_{i,k}$;
- (iv) $\beta_i \approx 1$ implies $\delta_{i,k} = \theta_{i,k}$.

According to (i), we can use equation (3) to analyze the statistical significance of the slope parameters in equation (2). According to (ii), if $\beta_i > 0$, as it almost surely is, the signs of the slope coefficients in equation (3) are the same as those in equation (2). According to (iii), we can also compare the relative sizes of the slope parameters in equation (2) using equation (3). Finally, according to (iv), insofar as β_i approaches unity, $\delta_{i,k}$ approaches $\theta_{i,k}$.

In the next section, we estimate the cointegrating relationship in equation (3) using standard procedures: ordinary least squares (OLS), fully modified OLS (FMOLS; Phillips and Hansen, 1990), and dynamic OLS (DOLS; Saikkonen, 1991, and Stock and Watson, 1993). While OLS is super-consistent, it is subject to an endogeneity bias that renders conventional inferential procedures invalid.⁶ The FMOLS and DOLS procedures address the endogeneity bias and permit valid inference. Of course, to treat equation (3) as a cointegrating relationship,

⁶ Loosely speaking, the endogeneity bias exists when there is feedback from the left-hand-side variable to the right-hand-side variables in equation (3), as there almost surely is in our applications.

$c_{i,t}^{DIR}$, $hw_{i,t}$, $sw_{i,t}$, and $y_{i,t}$ all need to be integrated processes. We test for a unit root in these variables using the familiar augmented Dickey and Fuller ([ADF] 1979) test, as well as a more-powerful panel version of the test from Im, Pesaran, and Shin (2003). For equation (3) to be a valid cointegrating relationship, it obviously must be the case that $c_{i,t}^{DIR}$, $hw_{i,t}$, $sw_{i,t}$, and $y_{i,t}$ are cointegrated. We test for cointegration using the well-known augmented Engle and Granger ([AEG] 1987) two-step test and a more-powerful panel version of the test from Pedroni (1999, 2004).

The key to our estimation strategy is the existence of a stable long-run relationship between $c_{i,t}^S$ and $c_{i,t}^{DIR}$. Although we obviously cannot test for the existence of such a relationship for each state, we can test whether the variables are cointegrated in aggregate U.S. data. Evidence of cointegration between these two variables at the national level is highly suggestive that similar cointegrating relationships exist at the state level. Using BEA data for 1975:Q1–2004:Q4, we construct observations for $c_{US,t}^S$ and $c_{US,t}^{DIR}$, the aggregate counterparts to $c_{i,t}^S$ and $c_{i,t}^{DIR}$.⁷ The ADF statistics for $c_{US,t}^S$ and $c_{US,t}^{DIR}$ are -2.66 and -1.71 , respectively, and in neither case can the null hypothesis of a unit root be rejected at conventional significance levels (the 5 percent critical value equals -3.45), indicating that $c_{US,t}^S, c_{US,t}^{DIR} \sim I(1)$.⁸ The AEG statistic (which includes a constant in the potential cointegrating relationship) equals -3.64 , so that the null hypothesis of no cointegration between $c_{US,t}^S$ and $c_{US,t}^{DIR}$ can be rejected at the 5 percent significance level (the 5 percent critical value equals -3.34), indicating that $c_{US,t}^S, c_{US,t}^{DIR} \sim CI(1,1)$. We expect β_{US} in equation (1) to be positive and relatively close to unity, and the OLS, FMOLS, and DOLS estimates of β_{US} in equation (1) all equal 1.13. The finding of a cointegrating relationship between $c_{US,t}^S$ and $c_{US,t}^{DIR}$ at the national level increases our confidence that similar cointegrating relationships exist at the state level, and the estimates of

⁷ The observations are converted to real terms using the personal consumption expenditure deflator.

⁸ Given the obvious upward drift in $c_{US,t}^S$ and $c_{US,t}^{DIR}$, a constant and linear trend are included the ADF tests. The number of lags included in the ADF and AEG tests is determined using a top-down procedure based on a maximum lag of four quarters. We obtain similar results using the “state of the art” unit root tests in Ng and Perron (2001).

Table 1
ADF Test Results, Eighth District States, 1975:Q1–2004:Q4

State	$c_{i,t}^{DIR}$	$hw_{i,t}$	$sw_{i,t}$	$y_{i,t}$
AR	-3.08	-0.46	-2.80	-3.59*
IL	-2.00	-1.95	-2.88	-2.14
IN	-1.54	-0.91	-2.77	-1.82
KY	-2.51	-0.99	-2.83	-3.07
MO	-1.71	-1.96	-3.22	-1.98
MS	-1.87	-0.38	-2.94	-2.26
TN	-3.19	-1.24	-3.13	-3.60*
Panel test				
W_{tbar}	-0.80	0.68	0.87	-0.89

NOTE: The table reports the ADF statistic, which corresponds to the null hypothesis that the variable has a unit root against the one-sided (lower-tail) alternative hypothesis that the variable is stationary; the 5 percent critical value equals -3.45. The W_{tbar} statistic corresponds to the null hypothesis that each of the variables in the panel has a unit root against the one-sided (lower-tail) alternative hypothesis that at least a portion of the variables in the panel is stationary; the 5 percent critical value equals -1.645. *Significant at the 5 percent level.

β_{US} at the national level further indicate that estimation of equation (3) at the state level will be informative about the parameters in equation (2) at the state level.⁹

ESTIMATION RESULTS

Quarterly data for 1975:Q1–2004:Q4 for personal income, savings income (dividends, interest, and rental income), and the personal consumption expenditure (PCE) deflator from the BEA are used to construct state-level observations for $c_{i,t}^{DIR}$. The Office of Federal Housing Enterprise Oversight (OFHEO) provides a housing price index for each state. The Census Bureau provides annual housing quantity data for each state, and we interpolate the annual data to obtain quarterly observations for the quantity of housing in each state. The housing quantity data end in 2004:Q4, which is the endpoint for all of our samples. We obtain real housing wealth by multiplying housing quantity by housing price and dividing by the PCE deflator. Real stock

wealth is obtained from quarterly aggregate S&P 500 stock market capitalization data available from Global Financial Data. We compute real stock market wealth for state i by first multiplying the proportion of aggregate U.S. dividends paid to state i by aggregate S&P 500 stock market capitalization and then dividing by the PCE deflator.

Table 1 reports unit root test results for $c_{i,t}^{DIR}$, $hw_{i,t}$, $sw_{i,t}$, and $y_{i,t}$ for Arkansas, Illinois, Indiana, Kentucky, Missouri, Mississippi, and Tennessee. The ADF statistics indicate that we almost always fail to reject the null hypothesis that the variables are unit root processes.¹⁰ A potential drawback to using the ADF statistic is that it may have limited power against persistent, but stationary, alternatives. In light of this, we also employ the more powerful Im, Pesaran, and Shin (2003) panel unit root test based on the W_{tbar} statistic, which is essentially an average of the individual ADF statistics. From Table 1, we can see that the null hypothesis that each variable contains a unit root cannot be rejected at the 5 percent significance level using the panel test, so we have substantial evidence that $c_{i,t}^{DIR}$, $hw_{i,t}$, $sw_{i,t}$, $y_{i,t} \sim I(1)$ for each of the Eighth District

⁹ Case, Quigley, and Shiller (2005) derive retail sales observations from county-level sales tax data to construct a proxy for consumption. It is unclear how reliable this consumption proxy is, and Case, Quigley, and Shiller (2005) do not examine the relationship between retail sales and consumption on the national level to get a feel for reliability.

¹⁰ We obtain similar results using the Ng and Perron (2001) unit root tests.

Table 2**Coefficient Estimates for Equation (3), Eighth District States, 1975:Q1–2004:Q4**

State	$\hat{\theta}_{i,hw}^{OLS}$	$\hat{\theta}_{i,sw}^{OLS}$	$\hat{\theta}_{i,y}^{OLS}$	$\hat{\theta}_{i,hw}^{FMOLS}$	$\hat{\theta}_{i,sw}^{FMOLS}$	$\hat{\theta}_{i,y}^{FMOLS}$	$\hat{\theta}_{i,hw}^{DOLS}$	$\hat{\theta}_{i,sw}^{DOLS}$	$\hat{\theta}_{i,y}^{DOLS}$
AR	0.030* (0.007)	0.004* (0.002)	0.967* (0.008)	0.024* (0.012)	0.004 (0.004)	0.969* (0.016)	0.016 (0.021)	-0.003 (0.008)	0.992* (0.030)
IL	0.075* (0.007)	0.009* (0.002)	0.864* (0.013)	0.083* (0.010)	0.011* (0.003)	0.844* (0.020)	0.090* (0.012)	0.006 (0.004)	0.852* (0.023)
IN	0.067* (0.005)	-0.004* (0.002)	0.956* (0.007)	0.070* (0.009)	-0.006* (0.003)	0.959* (0.012)	0.072* (0.014)	-0.009 (0.005)	0.966* (0.019)
KY	0.067* (0.005)	-0.001 (0.001)	0.928* (0.005)	0.069* (0.009)	-0.004 (0.003)	0.933* (0.010)	0.069* (0.012)	-0.006 (0.004)	0.939* (0.015)
MO	0.060* (0.005)	0.008* (0.002)	0.920* (0.006)	0.063* (0.009)	0.007 (0.004)	0.919* (0.011)	0.065* (0.027)	0.007 (0.010)	0.918* (0.034)
MS	0.040* (0.005)	0.001 (0.002)	0.971* (0.006)	0.043* (0.009)	-0.002 (0.004)	0.977* (0.012)	0.043 (0.025)	-0.001 (0.010)	0.976* (0.035)
TN	0.059* (0.006)	-0.003 (0.002)	0.958* (0.005)	0.063* (0.012)	-0.005 (0.005)	0.960* (0.011)	0.062* (0.029)	-0.009 (0.012)	0.968* (0.028)

NOTE: Standard errors are given in parentheses; 0.00 indicates less than 0.005; * denotes significance at the 5 percent level.

states.¹¹ Given these results, we proceed to estimate equation (3) and to test for cointegration among the variables in equation (3) for the individual states in the Eighth District.

Estimation results for equation (3) are reported in Table 2. The table reports OLS, FMOLS, and DOLS point estimates and corresponding standard errors for $\theta_{i,hw}$, $\theta_{i,sw}$, and $\theta_{i,y}$. As noted above, the OLS standard errors are generally not valid for inference, and we include them only for the sake of completeness. The first thing to notice in Table 2 is that all of the estimates of $\theta_{i,hw}$ are positive, indicating a positive long-run relationship between consumption spending and housing wealth in the states of the Eighth District. When using FMOLS (DOLS), seven (five) of the estimates are significant, the exceptions being the DOLS estimates for Arkansas and Mississippi. Overall, there is strong evidence in Table 2 for a positive and significant relationship between consumption and housing wealth for most of the states in the Eighth District.

¹¹ In order to account for a degree of cross-sectional dependence, a common time component is subtracted from each variable before computing the W_{ibar} statistics. For more discussion on issues relating to panel unit root tests, see the recent survey in Breitung and Pesaran (2005).

The estimates of $\theta_{i,sw}$ in Table 2 are all smaller than the corresponding estimates of $\theta_{i,hw}$, and fewer of the estimates are significant.¹² This indicates that the housing wealth effect on consumption is generally much stronger than the stock market wealth effect in the Eighth District. The $\theta_{i,y}$ estimates are all positive and reasonably close to unity, so that the values seem economically plausible.

The finding of a stronger housing wealth effect in comparison with a stock market wealth effect is in line with the results in Benjamin, Chinloy, and Jud (2004) and Case, Quigley, and Shiller (2005). However, the results in Table 2 also point to a potential problem with the approaches of both these studies. Both are based on the implicit homogeneity assumption that the cointegrating coefficients are the same across all states ($\theta_{i,k} = \theta_k$ for all i), whereas Table 2 shows that the cointegrating coefficients can differ substantially across states. For example, the $\theta_{i,hw}$ estimates for Illinois, Indiana, Kentucky, Missouri, and Tennessee are typically around two to three times larger than the $\theta_{i,hw}$ estimates for Arkansas and Mississippi. Imposing homogeneity

¹² In fact, a number of the $\theta_{i,sw}$ estimates are negative, the opposite sign predicted by theory.

across states can thus mask important differences in the long-run relationship between consumption and housing wealth across regions, differences that can arise from differences in demographics, institutions, and other factors across regions.¹³

Finally, it is important to test for the existence of cointegrating relationships in the Eighth District states. The coefficient estimates reported in Table 2 assume the existence of a cointegrating relationship, and we have a spurious regression if the variables are not cointegrated. Applying the AEG test to the residuals in equation (3) for each state, we cannot reject the null hypothesis of no cointegration for any of the seven states, as the AEG statistics range from -1.51 to -3.06 , while the 5 percent critical value equals -4.10 . However, we can employ the more powerful group t panel cointegration test of Pedroni (1999, 2004), which is essentially an average of the individual AEG statistics. The null hypothesis for this test is no cointegration for each of the panel members, and the one-sided (lower-tail) alternative is that a cointegrating relationship holds for a significant portion of the panel members. The (normalized) group t -statistic equals -2.66 ; given a 5 percent critical value of -1.645 , we can reject the null hypothesis of no cointegration. We thus have evidence that a cointegrating relationship holds for at least a significant number of the states in the Eighth District.¹⁴

CONCLUSION

This paper examines the long-run relationship between consumption spending and housing wealth in the states of the Federal Reserve's Eighth District. The consumption-housing wealth relationship has received limited attention at the state level, in part

because of the lack of consumption data at this level. We develop a novel proxy for consumption at the state level that can be constructed on a quarterly basis since 1975, and this proxy can be used in a cointegration framework to analyze the long-run relationship between consumption spending and housing wealth. Our estimation results show that housing wealth exerts a significant and sizable influence on consumption spending for most of the states in the Eighth District, and this influence is typically stronger than that of stock market wealth. Our results imply that the strong increases in housing prices and home construction over the past decade have helped to buoy consumption in most of the states of the Eighth District; they also imply that sharp decreases in housing prices and home construction in the future will have a depressing effect on consumer spending.

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¹³ Allowing for heterogeneity also gives a more accurate picture of the true underlying relationships because it avoids the bias that can result from pooling (Pesaran and Smith, 1995).

¹⁴ To account for a degree of cross-sectional dependence, a common time component is subtracted from each variable before computing the group t -statistic. There are a number of econometric issues involved with panel cointegration tests, including how cross-sectional dependence is handled and the potential existence of cross-sectional cointegration; see the survey in Breitung and Pesaran (2005). We deal with these issues more extensively in ongoing research in which we consider the long-run relationship between consumption spending and housing wealth in each of the 50 individual U.S. states.

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