Federal Reserve Bank of Chicago

Real Exchange Rate Fluctuations and the Dynamics of Retail Trade Industries on the U.S.-Canada Border

Jeffrey R. Campbell and Beverly Lapham

WP 2002-17

Real Exchange Rate Fluctuations and the Dynamics of Retail Trade Industries on the U.S.-Canada Border*

Jeffrey R. Campbell† Beverly Lapham‡

November 2002

Abstract

Consumers living near the U.S.-Canada border can shift their expenditures between the two countries, so real exchange rate fluctuations can act as demand shocks to border areas' retailers. Using annual county-level data, we estimate the effects of real exchange rates on the number of establishments and their average employment in border counties for four retail industries. In three of the four industries we consider, the number of operating establishments responds either contemporaneously or with a lag of one year, so "long-run" changes in net entry in fact occur quickly enough to matter for short-run fluctuations.

*The opinions expressed herein are those of the authors and are not necessarily those of the Federal Reserve Bank of Chicago or the Federal Reserve System. We thank Lisa Barrow, Martin Boileau, Lars Hansen, Allen Head, John Rogers, Chad Syverson, and Oved Yosha for helpful comments on previous versions of this paper. The National Science Foundation supported Campbell's research through grant SBR-9730442, "Business Cycles and Industry Dynamics." The Social Science and Humanities Research Council of Canada supported Lapham's research. The latest versions of this paper and its technical appendix are available on the World Wide Web at `http://www.nber.org/~jrc`. Please direct correspondence to Campbell at Economic Research, Federal Reserve Bank of Chicago, 230 South LaSalle Street, Chicago, Illinois 60604.

†Economic Research, Federal Reserve Bank of Chicago and NBER

‡Department of Economics, Queen's University

This paper estimates the effects of real exchange rate fluctuations on the number of establishments and their average employment in U.S. retail trade industries located near the U.S.-Canada border. It is widely known that there are large and persistent deviations from purchasing power parity between these countries, and Engel (1999) and Engel and Rogers (1996) have documented that these deviations largely arise from violations of the law of one price for traded goods. For retailers near these countries' common border, real exchange rate movements represent changes in the price of a substitute good. Thus, they have effects similar to an ordinary demand shock. For each of four retail industries, we estimate a panel-data vector autoregression using annual county-level data on the number of retail establishments and their average employment from the ten contiguous states that border Canada. The model's explanatory variables include current and lagged real exchange rates interacted with a measure of the local importance of the Canadian market.

The analysis increases our understanding of the relative importance of intensive and extensive margins of adjustment in retail industries in response to demand shocks. We find that in three of the four industries we examine – Food Stores, Gasoline Service Stations, and Eating Places – fluctuations in the real exchange rate induce a change in the number of stores either contemporaneously or with a lag of one year. That is, "long-run" changes in the number of establishments occur quickly enough to contribute to these industries' short-run fluctuations. In the one industry we examine in which adjustment of net entry plays no significant role in short-run fluctuations, Drinking Places, producers enjoy well-known licensing restrictions on entry. Border counties account for a trivial fraction of economic activity in the U.S., and the amount of trade facilitated by cross-border shopping is small relative to either the U.S. or Canadian economy. Our results nevertheless offer insight into the nature of aggregate fluctuations, because they exemplify these important retail industries' responses to other demand shocks that are more widespread.

The real exchange rate is a market-determined price, and its movements reflect unknown structural disturbances that could directly affect retailers' costs. We do not expect these cost effects to differ between border and non-border retailers, because all retailers face substantial barriers to the international flow of wholesale goods and labor. Therefore, we can control for changes in retailers' costs that are correlated with the real exchange rate using observations from counties off of the U.S.-Canada border. The estimator we use can be viewed as an extension of the familiar difference-in-differences procedure that accommodates autocorrelation in the model's disturbance terms. Our application of it uses variation in counties' proximity to the Canadian market to identify the demand-shifting effects of real exchange rates. In all of the industries we study, we find that these effects are significant.

In the next section, we briefly describe the trading arrangements between the U.S. and Canada that make it relatively easy for consumers to import small quantities of retail goods for personal use. Section II describes the data, our empirical model, and the GMM estimation procedure we use. Section III presents the estimation results, and Section IV contains concluding remarks regarding their implications for future research.

I Barriers to Trade and Cross-Border Shopping

Our empirical analysis will rely on two assumptions. First, movements in the real exchange rate do not present arbitrage opportunities that would cause U.S. border retailers' costs to systematically differ from those of their counterparts in the country's interior. Second, these same movements induce border area consumers to shift their expenditures between U.S. and Canadian border retailers. In this section, we provide evidence supporting these assumptions.

U.S. and Canadian consumers can legally cross their common border for recreational purposes subject only to a (usually) brief customs inspection. Furthermore, these consumers can import small quantities of goods purchased abroad for personal use. Figure 1 illustrates that consumers in both countries take advantage of international price differences to shift their expenditures towards the low-price country. Its top panel depicts the Canada-U.S.

real exchange rate between 1977 and 1996, normalized to equal 1 in 1977. Over the same time period, its bottom two panels each plot the number of one-day trips by Canadians and Americans to the other country. This variable is the official measure of cross-border shoppers used by Canadian government agencies.

During the appreciation of the Canadian dollar between 1986 and 1992, the number of Canadian one-day trips increased dramatically. It reached a peak of approximately 59 million trips during 1991. The estimates of Canadian expenditures in the U.S. during that year reported in Ford (1992) range from 4 to 11 billion Canadian dollars. These values are small relative to either the Canadian or U.S. economies, but the studies on cross-border shopping commissioned by local and provincial Canadian governments during that time, such as New Brunswick (1992) and Ontario (1991), indicate that their perceived impact on border communities was substantial. The flow of cross-border shoppers reversed direction when the Canadian dollar subsequently depreciated: American one-day trips to Canada climbed from 19 million in 1992 to nearly 24 million in 1996. The spike in American trips in 1980-1981 came at a time when the Canadian National Energy Policy subsidized petroleum's import and taxed its export. These policies greatly reduced the price of gasoline in Canada relative to its price in the U.S., and American consumers took advantage of the opportunity to export low-cost gasoline in their automobile tanks tax-free. Because the export tax was much more easily enforced against large tanker trucks, the same price difference presented no arbitrage opportunity to wholesalers.

The extent and timing of cross-border shopping indicates that wholesale arbitrage does not eliminate international price differences near the U.S.-Canada border. The resulting inference that it is considerably easier for consumers to shift their expenditures on retail goods and services than it is for retailers to shift their expenditures on inputs is consistent with these countries' bilateral trading institutions. The U.S. and Canada have enjoyed a free trade agreement since 1989. The Canada-U.S. Free Trade Agreement (FTA) and its successor, NAFTA, guarantee mutual tariff-free market access for most *domestically produced* goods. Both agreements guarantee free trade in gasoline between the two countries, so we expect wholesale arbitrage to have tempered retail gasoline price differences (net of excise and sales taxes) near the border since 1989. However, the FTA and NAFTA have not joined the United States and Canada into a customs union with free mobility of all goods and factors of production. Instead, they contain numerous exceptions to the general rule of free trade. For example, Canada continues to maintain severely limiting import quotas on dairy products, poultry, and eggs; and the United States' sugar quotas limit imports of food items composed of more than ten percent sugar. These and other remaining restrictions on trade in agricultural products make it infeasible for a grocery retailer or restaurant to rely on a foreign grocery wholesaler or food-service distributor. Alcohol distribution is also subject to considerable regulation in both countries that serves as a non-tariff barrier to wholesale trade.[1]

The existing barriers to trade in wholesale goods suggest that proximity to the border does not endow a retailer with easier access to less-expensive foreign goods. Both countries' existing immigration restrictions for low-skilled workers lead to the same conclusion regarding labor. The FTA and NAFTA have both made it easier for professional workers to move between the U.S. and Canada, but they have not enabled workers in the retail sector to do so. Taken together, these institutional features of U.S-Canada trade imply that movements in the real exchange rate do not present arbitrage opportunities that would cause U.S. border retailers' costs to systematically differ from those of their counterparts in the country's interior.

The extent to which the demand-shifting effects of exchange rates differentially affect retailers near the border depends on the distance that consumers are willing to travel to obtain a discount. Even if all cross-border shoppers make their purchases in border areas,

[1] See Taylor (1988) for a description of the restrictions on the export of alcohol, dairy products, poultry, and eggs to Canada before and after the FTA, and see Wilson (1993) for a brief synopsis of the ongoing trade dispute between the two countries over trade in beer.

exchange rate movements affect the demand of interior retailers when they induce interior residents to shop abroad. Ford's (1992) survey of Canadian consumers' shopping decisions at the peak of Canadian cross-border shopping in the early 1990's indicates the extent to which this occurs. She surveyed consumers in three cities: Niagara-St. Catherines (very close to the border), Hamilton (about 30 minutes drive from the border) and Toronto (about 1 hour from the border). Her respondents from Toronto tended to shop in the U.S. twice a year for electronics and appliances and those from Hamilton shopped quarterly for those items as well as apparel and linens. About 70% of her respondents from Niagara-St. Catherines shopped in the U.S. These shoppers averaged two trips per month, primarily to buy food and gasoline. Furthermore, 96% of her survey respondents who purchased food and gasoline in western New York did so in the two border counties of Erie and Niagara. If we extrapolate these results to the entire U.S.-Canada border, U.S. gasoline and grocery retailers located in border communities faced increases in demand for their goods during the appreciation of the Canadian dollar and decreases in demand during the subsequent depreciation. If U.S. consumers' travel habits mimic those of Canadians, then these demand changes were not shared by food and gasoline retailers located in interior counties.

As our discussion indicates, Food Stores (SIC 54) and Gasoline Service Stations (SIC 554) share three characteristics that are salient for our analysis. First, wholesalers' ability to arbitrage the industry's inputs between the two countries is limited, so any cost disturbances associated with real exchange rate movements will be common to all counties in the U.S. Second, consumers can and do shift their purchases between the two countries relatively easily. Third, the value of a typical purchase is low relative to the cost of travel, so the demand effects of cross-border shopping are confined to border counties. Together, these imply that we can use observations from these industries in interior counties to control for unobserved common cost shocks and thereby identify the demand-shifting effects of real exchange rates. Our own experience as cross-border shoppers suggests that Eating Places (SIC 5812) and Drinking Places (SIC 5813) share these characteristics. We restrict our empirical analysis to these four retail trade industries.

II Data and Estimation

This section describes the data we use, presents our panel-data VAR model, and discusses the GMM procedure we use for its estimation. We begin with the data.

A Observations of Retail Trade Industries

Our observations of retail trade industries come from the United States Census' annual publication, *County Business Patterns* (*CBP*). We construct our data set from twenty years of this publication, from 1977 through 1996. We focus on counties in the ten contiguous states that border Canada so that the sample's interior counties are as otherwise as similar as possible to the border counties. For each retail trade industry the *CBP* reports each county's mid-March employment and the total number of establishments with employment during the year, among other variables. From these observations, we construct for each industry

$$y_{it} = \begin{bmatrix} \ln N_{it} & \ln A_{it} \end{bmatrix}', \tag{1}$$

where N_{it} is the number of stores operating at any time during the year divided by the 1990 population of county i (establishments), and A_{it} is average number of employees at those stores (average employment).

Our data set is incomplete because the Census withholds the employment information for any county-industry observation where that datum may disclose information about an individual producer. The Census does not reveal how it determines which observations must be withheld, but these disclosure cases tend to occur in counties with small populations and few establishments. To produce a balanced panel of employment observations across counties, we use data in the *CBP* on each state's employment and the number of establishments by employment size class to forecast and replace the withheld observations. This paper's technical appendix describes this data replacement procedure in greater detail.

The counties on the U.S.-Canada border range greatly in size. It is unrealistic to expect any parametric model to describe the evolution of both oligopolies in rural counties and monopolistically competitive industries in urban areas, so we confine our analysis to counties with relatively large numbers of establishments using two selection criteria. First, we consider counties with populations greater than $20,000$ people, as measured in the 1990 decennial census. There are 256 such counties in the ten contiguous border states, and nineteen of these counties share a border with Canada. Second, we drop all observations from any county-industry pair with ten or more observations withheld by the Census Bureau. This criterion lessens the dependence of our results on our data replacement procedure. For the resulting sample of counties, 1.2% of our county-industry-year observations have imputed employment data. As noted above, disclosure withholding primarily affects counties with few producers, so our resulting sample is of unconcentrated industries.[2]

Our county selection criteria produce different samples for each industry we consider. Table 1 provides summary statistics for each industry's sample of counties.[3] Its first column

[2] Our data set also omits a handful of county-industry records because of apparent clerical errors in the CBP data. The data file and printed CBP publication both contain observations of employment for counties in some years that are extremely high. In these observations, the CBP reports the existence of a extraordinarily large establishment in the county, and the reported employment far exceeds its values in the data set's other years. For observations from Food Stores, we have replaced these employment observations with their values in the Bureau of Labor Statistics' Covered Employment and Wages (ES-202) data based on unemployment insurance records. In the other three industries, all observations from a county-industry pair with one of these erroneously high employment observations are deleted from our final data set. This results in the loss of three interior counties in Eating Places, two interior counties in Drinking Places, and five counties in Gasoline Service Stations. Two of that industry's five lost counties are border counties. See this paper's technical appendix for more information regarding this data replacement.

[3] The statistics in Table 1 use the raw establishment counts from the *CBP*. These have not been scaled by the county's 1990 population.

reports the number of counties included in the sample; and its remaining three columns report the first quartile, median, and third quartile, across counties, of the average number of establishments, across years, serving that industry. All 256 counties are in our sample for Food Stores. The sample for Gasoline Service Stations excludes 5 counties, two of which border Canada. The samples for Eating Places and Drinking Places exclude 14 and 23 counties. For each of these industries, five of the excluded counties are border counties. The first sample quartiles of average establishment counts indicate the extent to which our selection procedures leave relatively unconcentrated industries. With the exception of Drinking Places, the first quartiles of the average establishment counts are all above 15. For Drinking Places, the first quartile is 11.5. It appears that our county selection procedure produced a sample of unconcentrated industries.

To assess how variations in the number of establishments and their average employment each contribute to retail trade industries' county-specific fluctuations, we regressed each of these variables' logarithms against a set of time dummies. We then tabulated the sample standard deviations of that regression's residuals *for each county.* Table 2 reports the medians of these standard deviations for each retail trade industry separately for border and interior counties. In practice, these medians are close to their corresponding means. Relative to many aggregate time series, these median standard deviations are quite high. For interior counties, the lowest are in Eating Places, 0.09 for establishments and 0.10 for average payroll. Drinking Places has the highest median standard deviations, 0.17 and 0.22 respectively. Overall, establishments' median standard deviations are not much lower than those of average employment, indicating that these industries' structures are far from rigid. The median standard deviations for border counties do not differ substantially from those of interior counties.

Figure 2 provides a visual impression of our data for one industry, Gasoline Service Stations. Its left panel plots the logarithm of the number of establishments in all of our sample's border and interior counties, and its right panel plots the logarithm of these two groups' average employment. Both panels also contain the logarithm of the relative price of gasoline between the U.S. and Canada, and all of these series have been normalized so that their values in 1977 equal zero.

Between 1977 and 1981, the relative price of gasoline fell 44%. Establishment counts fell much more rapidly in border counties over this period. There were 30% fewer establishments in border counties in 1981 than there were in 1977, and the corresponding number for interior counties is 20%. In contrast, average employment in border and interior counties were nearly identical for these five years. Thus, it appears that retail gasoline industries in border counties shrank and recovered through this period by adjusting net entry, leaving establishments' average size constant. Between 1985 and 1991, when gasoline became relatively cheap in the U.S., the number of establishments in border counties grew by approximately 10%, while the number in interior counties shrank 3%. Average employment grew in both border and interior counties through this period, but it grew by much more (40% versus 20%) in border counties. Overall, Figure 2 shows that changes in both average establishment size and in the number of establishments were used in border counties' retail gasoline industries to accommodate the demand shifts due to cross-border shopping. Movements in the number of establishments were particularly important during the period of inexpensive Canadian gasoline.

B International Relative Prices

The international relative price of gasoline used in Figure 2 equals the ratio of the two countries' consumer price indices for gasoline multiplied by their nominal exchange rate. That is

$$r_t = \frac{p_t^C}{p_t^U} n_t, \tag{2}$$

where p_t^C and p_t^U are the two countries' national-level price indices and n_t is the price of a Canadian dollar in U.S. dollars. Hence, an increase in the real exchange rate reflects an depreciation from the U.S. perspective. We constructed our measures of international relative prices for the other three industries in a similar fashion using industry-specific consumer price indices.[4] Table 3 lists the U.S. and Canadian CPI series used to construct the relative price series for each of the four industries we consider.

The first two columns of Table 4 report the sample standard deviation and first autocorrelation for the industries' relative price series, expressed in logarithms. For all of the industries but Service Stations, the standard deviations of the relative price series are between 0.07 and 0.09. The standard deviation of the relative price of Gasoline is much higher than this, 0.21. Unsurprisingly, the relative price series are all highly persistent, with first order autocorrelations between 0.75 and 0.88. Table 4's final column reports the contemporaneous correlation between each industry's relative price series and that constructed with the aggregate CPI's for all goods less energy. The relative prices for Eating Places and Drinking Places are both highly correlated with this aggregate real exchange rate. The relative prices of food purchased at stores and gasoline have somewhat lower correlations.

C Canadian Market Size

Exposure to Canadian shoppers and competitors differs across border counties. For example, the number of one-day trips by Canadians returning from Whatcom County, Washington and returning from Niagara County, New York during 1990 were 13,414,110 and 7,274,940 respectively. The populations of those counties in that year were 127,780 and 220,756. This suggests that the size of the Canadian market relative to the local market was much larger for Whatcom county than for Niagara county. Accordingly, we expect retail activity to respond more to real exchange rate changes in Whatcom county.

Our model accounts for these observable differences across border counties by specifying the effect of r_t on y_{it} to be a function of the share of customers for county i's retailers that are Canadian in a typical year. If we let S_{iU} be the population of county i and let S_{iC} be

[4] For Drinking Places, relative price data is not available until the third year of our sample.

the Canadian population "close" to county i, then this share is

$$(3) \qquad s_i = \frac{S_{iC}}{S_{iU} + S_{iC}}.$$

For interior counties, we define S_{iC} and s_i to equal zero. This measure accords well with the intuition that being located next to Canadian land is irrelevant for a border county's retail industry if there are no nearby Canadians to act as either customers or competitors.[5]

Measuring S_{iC} for border counties is not straightforward, because there is no natural or political geographic partition of Canada that indicates which Canadians are potential cross-border shoppers for county i. It is possible to measure the number of potential cross-border shoppers as the number of Canadians living within a particular distance of county i, but this measure is unsatisfactory because it does not account for potential geographic obstacles to travel. For instance, travel bottlenecks such as bridges may make even a short distance costly to travel, while an adequate highway leading to the border may make shopping trips very convenient.

Our preferred measure of S_{iC} uses observations of the number of Canadians who cross the international border into county i to estimate the number of Canadians who are potential cross-border shoppers for that county. Using interview data from border crossing points, Statistics Canada tabulates the number of U.S. and Canadian travelers that travel through each official border crossing point while either embarking upon or returning from a trip lasting one-day or less to the other country. Statistics Canada does not keep track of travelers' identities, so an individual making multiple trips to or from Canada in a year will contribute to the count of travelers on each trip. This data is available from 1990 through 1999. We average the data across these years to measure the average annual number of U.S. and Canadian travelers for county i, which we denote with T_{iU} and T_{iC}.

To construct a measure of S_{iC} based on T_{iC}, we assume that the average number of

[5] In Campbell and Lapham (2001), we present a simple two-country, two-county model of cross-border shopping with free entry in which the elasticities of average store size and the number of stores in the U.S. county with respect to the real exchange rate were proportional to this proposed ideal measure of s_i.

trips taken per consumer, ψ, is constant across locations. Given a value for ψ, we can then measure S_{iC} with T_{iC}/ψ. The resulting measure of s_i is

$$s_i = \frac{T_{iC}}{\psi S_{iU} + T_{iC}}. \tag{4}$$

As (4) makes clear, the problem of choosing ψ is one of expressing county i's population in units of travelers. For our baseline measure of s_i, we assume that all U.S. travelers entering Canada for one-day trips from county i are residents of that county, and use the average of T_{iU}/S_{iU} across border counties to measure ψ. The resulting value of ψ is 7.49. Across the nineteen border counties, the mean and standard deviation of this baseline measure of s_i are 0.60 and 0.27. In Section III we examine the implications of measuring s_i differently.

D The Empirical Model

Our empirical model provides a framework for using our observations to estimate the demand-shifting effects of real exchange rate movements. The specific autoregressive equation we estimate is

$$y_{it} = \alpha_i + \mu_t + \Lambda y_{it-1} + \beta' (s_i \times e_t) + \varepsilon_{it}. \tag{5}$$

In (5), α_i is a random county-specific intercept, μ_t is a time-specific effect common to all counties,

$$e_t = \begin{bmatrix} \ln r_t & \ln r_{t-1} \end{bmatrix}' \tag{6}$$

is a vector containing the current and lagged exchange rate, ε_{it} is a disturbance term with

$$\mathbf{E}[\varepsilon_{it}] = 0, \tag{7}$$

and Λ and β are (2×2) matrices of unknown coefficients.[6]

[6] The fixed effects in (5) allow us to use unscaled observations on the number of establishments rather than the per capita measure we use. We chose the latter specification, however, because the GMM estimation procedure described below treats α_i as a component of the model's error rather than as a parameter to be estimated. Scaling establishments by population reduces the overall error variance.

The time-specific intercepts, μ_t, capture the effects of economy-wide demand and cost shocks that affect all counties' equally. A structural shock that influences the real exchange rate will affect an interior county's industry entirely through changing μ_t. The term $\beta' (s_i \times e_t)$ in (5) captures the additional demand-shifting effects of this shock on a border county.

D.1 GMM Estimation

The estimation of panel-data vector autoregressions similar to (5) without the explanatory variables $s_i \times e_t$ is a well-studied problem. To estimate (5) we use a GMM estimator based on Blundell and Bond (1998) which uses moment conditions derived from the lack of serial-correlation in ε_{it} and an assumption that y_{it} is mean-stationary. To apply these moment conditions, we assume that the roots of $|I - \Lambda L|$ lie outside of the unit circle and that if $s_i = 0$ and $t \neq \tau$, then

$$\mathbf{E}\left[\varepsilon_{it}\varepsilon_{i\tau}\right] = 0. \tag{8}$$

That is, for an interior county ε_{it} is the fundamental error (in the sense of the Wold decomposition theorem) for $y_{it} - (I - \Lambda L)^{-1} \mu_t$. We furthermore assume that for interior counties

$$\mathbf{E}\left[\alpha_i\right] = 0. \tag{9}$$

Given the presence of μ_t in (5), this is only a normalization.

We restrict the assumption of serially uncorrelated disturbance terms in (8) to interior counties, because county-specific shocks might cause the real exchange rate between a particular U.S. border county and its Canadian counterpart to persistently deviate from the real exchange rate measured using national level price indices. Such deviations contribute to the error term in (5) for that border county, causing (8) to fail. Similarly, the time-series averages of establishments per capita and average employment in border counties may differ systematically from their counterparts in interior counties, violating (9).

The appropriate moment conditions used by our GMM estimator are

$$\mathbf{E}\left[I\left\{s_i=0\right\}\Delta\varepsilon_{it}y_{it-\tau}\right]=0,\ t=3,\ldots,T,\ t>\tau\geq 2, \tag{10}$$

$$\mathbf{E}\left[I\left\{s_i=0\right\}\left(\alpha_i+\varepsilon_{it}\right)\Delta y_{it-1}\right]=0,\ t=3,\ldots,T, \tag{11}$$

$$\mathbf{E}\left[I\left\{s_i=0\right\}\left(\alpha_i+\varepsilon_{it}\right)\right]=0,\ t=2,\ldots,T. \tag{12}$$

Taken together, the moment conditions in (10), (11), and (12) are more than sufficient for identifying and estimating the 4 autoregressive parameters and the $2(T-1)$ year-specific intercepts for $T=20$.[7] However, these conditions clearly leave β unidentified. Because (7) applies to border counties, it must be the case that

$$\mathbf{E}\left[\Delta\varepsilon_{it}s_i\right]=0,\ t=3,\ldots,T. \tag{13}$$

These $2(T-2)$ moment conditions identify β. The GMM estimator we use is based on the moment conditions in (10), (11), (12), and (13). We use a one-step GMM estimator, in which the weighing matrix is a version of that used by Blundell and Bond (1998) appropriately modified to account for the additional moment conditions in (12), and (13).

D.2 Comparison with Difference-in-Differences Estimation

Although we estimate Λ, β, and the values of μ_t jointly, it is possible to estimate Λ and μ_t using data from only interior counties and the moment conditions (10), (11), and (12). These estimates can then be substituted into a second stage GMM estimator that uses only data from border counties and (13) to estimate β. Considering such a sequential estimator clarifies the relationship between the GMM procedure we use and the more familiar difference-in-differences scheme.

If we use (5) to replace $\Delta\varepsilon_{it}$ in (13), the resulting moment conditions are

$$\mathbf{E}\left[\left(\Delta y_{it}-\Lambda\Delta y_{it-1}-\Delta\mu_t-\beta s_i\Delta e_t\right)s_i\right]=0,\ t=3,\ldots,T.$$

[7] The derivation of (11) requires an additional assumption: The initial value, $y_{i1}-(I-\Lambda)^{-1}\alpha_i$ must have zero covariance with α_i. This paper's technical appendix discusses this assumption and other aspects of the estimation procedure in greater detail. See also Blundell and Bond (1998).

The second-step GMM estimator of β replaces Λ and $\Delta\mu_t$ with their estimated values from the first step, $\hat{\Lambda}$ and $\Delta\hat{\mu}_t$,and makes the sample analogues of the resulting approximate moment conditions as close to zero as possible.

The relationship between our estimator and difference-in-difference estimators can be most easily seen by considering the case where $T = 3$ and lagged real exchange rates are excluded from (5), so that e_t equals $\ln r_t$. In this case, the GMM estimator is simply that obtained from regressing $\Delta y_{i3} - \hat{\Lambda}\Delta y_{i2} - \Delta\hat{\mu}_3$ on $s_i \Delta \ln r_3$.

$$\hat{\beta} = \frac{\Delta \ln r_3 \sum_{i=1}^{N} s_i \left(\Delta y_{i3} - \hat{\Lambda}\Delta y_{i2} - \Delta\hat{\mu}_3 \right)}{\left(\Delta \ln r_3\right)^2 \sum_{i=1}^{N} s_i^2} \tag{14}$$

If we dismissed the possibility that y_{it} displays autocorrelation, then we could constrain $\hat{\Lambda}$ to equal zero and choose $\widehat{\mu}_t$ to satisfy the sample analogue of (12). The resulting value of $\widehat{\mu}_t$ equals the sample average of y_{it} across interior counties. If we furthermore redefined s_i to be a simple indicator variable that equaled one for all border counties, then the expression for $\hat{\beta}$ in (14) would reduce to the difference in the growth rates of Δy_{i3} between border and interior counties scaled by $\Delta \ln r_3$, $\left(\left(\sum_{i=1}^{N} s_i \Delta y_{i3} \right) / \sum_{i=1}^{N} s_i - \Delta\widehat{\mu}_3 \right) / \Delta \ln r_3$. This corresponds exactly to the difference-in-differences estimator used by Card and Krueger (1994) in which the border counties play the role of the treatment group and the interior counties serve as the control group. In this sense, our GMM estimator can be viewed as an extension of the difference-in-differences estimator that accounts for serial correlation in the dependent variable and observable heterogeneity of individuals' treatment effects.

III Estimation Results

Our baseline empirical analysis for the four industries we consider produces estimates of eight autoregressive equations' parameters. To conserve space, we report complete results for one industry, Food Stores, as an example. For the remaining industries, we report the estimates of the coefficients on current and lagged relative prices, β, and summarize our estimates of Λ and μ_t.

A Food Stores

Table 5 presents the GMM estimates of Λ and β in (5) for Food Stores. Below each estimate is its heteroskedasticity-consistent standard error. The Table's final row reports the value of a Wald test of the null-hypothesis that the international relative prices can be excluded from that equation. These tests are asymptotically distributed as χ^2 random variables with two degrees of freedom. Figure 3 plots the estimates of μ_t along with the logarithm of the real exchange rate for that industry.

The estimated elements of Λ indicate that both the number of establishments in a county and their average employment are persistent time series. Its diagonal elements are both large and positive, while its off-diagonal elements are much smaller and statistically insignificant. The other three industries' estimated autoregressive coefficients are very similar to Food Stores', with the notable exception that the coefficient on lagged establishments in the average employment equation is positive and statistically significant in the other three industries.

Turning to estimates of the time-specific effects depicted in Figure 3, we note that the estimates for establishments and the estimates for average employment have a strong negative comovement with each other. The figure also indicates that the estimates of μ_t covary with the real exchange rate, at least until the implementation of the FTA in 1989. This suggests that movements in these time-specific effects may be correlated with disturbances associated with real exchange rate movements. To assess this covariance more formally, we regressed the estimated coefficients against the current and lagged real exchange rate's logarithm. We could not reject the null hypothesis that the real exchange rate had no power to explain the estimates of μ_t. This was also the case when we repeated this procedure for Eating Places and Drinking Places, but the real exchange rate for Gasoline Service Stations does help predict the estimates of μ_t for that industry.

Before estimation, we divided s_i by its mean value, so that the coefficients on current and lagged relative prices can be interpreted as elasticities at a county with the mean value of s_i, 0.60. The estimates in Table 5 indicate that in the Food Stores industry, the number of establishments responds to movements in the relative price of food purchased from stores after one year. In the establishments equation, the coefficient multiplying $s_i \times \ln r_t$ equals $-.087$ and is not statistically significant while that on $s_i \times \ln r_{t-1}$ equals 0.165. This latter estimate has the expected sign and is statistically significant at the 5% level. The Wald exclusion test statistic for the establishments equation equals 5.93, which has a probability value of 0.052. The individual coefficient estimates for the average employment equation are not statistically significant at conventional levels. However, the Wald exclusion test statistic for this equation equals 10, indicating that the real exchange rate affects Food Stores' average employment at a very high level of confidence.

To better gauge the economic significance of our estimates for Food Stores, we have plotted the responses of $\ln N_{it}$ and $\ln A_{it}$ to a persistent innovation in the relative price. Figure 4 displays these impulse response functions over a ten-year horizon. Its top panel plots the response of $\ln N_{it}$, whereas its bottom panel plots that of $\ln A_{it}$. For both panels, we assumed that $\ln r_t$ follows an AR(1) process

$$\ln r_t = \kappa + 0.87 \ln r_{t-1} + \upsilon_t$$

where υ_t is an *i.i.d.* disturbance term with mean zero and standard deviation 0.037. With these parameter values, the unconditional standard deviation and first autocorrelation of $\ln r_t$ equal their sample values. Each panel's solid line plots the response to a one standard deviation positive impulse to υ_t. The dashed lines plot the upper and lower limits of point-wise 95% confidence intervals for the impulse response function. These confidence intervals reflect sampling uncertainty regarding the model parameters Λ and β, but they do not reflect uncertainty about the true process for $\ln r_t$. When calculating these impulse response functions, we do not allow υ_t to influence μ_t at any horizon. Thus, they reflect only the demand-shifting effects of a real exchange rate shock arising from cross-border shopping.

Both impulse response functions in Figure 4 display little contemporaneous effect of exchange rates on either variable, but eventually real depreciations (from the U.S. perspective) affect both of them positively. As the point estimates in Table 5 indicate, the initial response of establishments to a real exchange rate shock is small and negative. At a horizon of one year, the impulse response function crosses zero. Although neither of these estimated effects is individually statistically significant, the Wald test of the hypothesis that they both equal zero has a probability value of 0.057.[8] The response of establishments reaches its peak value of 0.74% six years after the initial shock. Average employment's impulse response function builds more rapidly. It equals 0.72% after one year and reaches its peak of 0.90% after two years. It appears that the Food Stores industry responds to a shock to its relative price by changing both its average establishment size and the number of establishments.

B Other Industries

Table 6 reports the estimates of β and the exclusion tests for all four of the industries we consider, and Figures 5, 6, and 7 graph the other industries' impulse response functions. Just as with Food Stores, the responses in these figures are to a persistent real exchange rate shock from an autoregression parameterized to match the reported statistics in Table 4.

The results for Gasoline Service Stations are similar to those for Food Stores. In particular, the lagged gasoline-based real exchange rate has a positive and statistically significant coefficient while the coefficient on the current relative price is insignificant. Thus, in this industry the number of establishments responds to real exchange rate shocks after one year. The Wald test rejects the exclusion restriction for establishments at the 1% level. Exclusion of the real exchange rates from the average employment equation cannot be rejected at any conventional significance level. However, the point estimates and Wald test statistic for that equation are not very small; so it may be premature to conclude that Gasoline Service Stations' average employment does not respond to real exchange rate shocks. The impulse response functions for Gasoline Service Stations reflect the delayed response of net entry to the exchange rate shock. In the year of the shock, average employment rises more than 1%,

[8] We have conducted these tests for all of the impulse-response functions reported in this paper. These test statistics' values are nearly identical to the corresponding statistics in Table 6, discussed below.

while the number of establishments falls very slightly. Thereafter, the increase in average employment persists as the number of establishments rises. After five years, the number of establishments has increased by approximately 2%.

For Eating Places, fluctuations in the number of establishments play a central role in its responses to real exchange rate disturbances. Although neither of the estimated coefficients in the establishments equation are individually significant, the Wald test's probability value is relatively low, 0.026. The corresponding Wald test statistic for the average employment equation has a very high probability value. Establishments' impulse-response function hits its peak value, 0.86%, two years after the shock, and this response is statistically significant at the 5% level. Average employment's response is small and statistically insignificant at all horizons. Apparently, long-run industry analysis, in which the free-entry/zero-profit condition determines producers' sizes and changes in the number of producers accommodate shifts in demand, characterizes Eating Places' *short-run* fluctuations. The period of time over which the number of producers in Eating Places is fixed is at *most* one year.

Our final industry is Drinking Places. These estimates are quite different from those of the other industries. The standard errors of the estimated coefficients for the establishments equation are much larger than in the other industries we consider. Furthermore, the coefficient on the contemporaneous real exchange rate in the average employment equation is 0.597. This is by far the largest absolute value of any of our estimated coefficients, and it is statistically significant at the 10% level. The Wald exclusion test also indicates that real exchange rate fluctuations have a strong impact on the average employment of Drinking Places in border counties. The response of establishments to the shock is not statistically significant at any horizon. In contrast, Drinking Places' average employment rises nearly 3% in the period of the shock, and it slowly falls back to its pre-shock level.

C Alternative Canadian Market Size Measures

The construction of s_i required a choice of ψ in (4), so we wish to examine the implications of taking alternative approaches to its measurement. We have also calibrated ψ based on Ford's (1992) survey of Canadian consumers' cross-border shopping habits. The calibrated value of ψ is much larger than our baseline choice, 17.75. The empirical results we obtain using it are nearly identical to our baseline results. We do not report them here.

Table 7 reports the estimates obtained from two other measures of s_i. The table's upper panel reports the result of using the fraction of total one-day trips across the border at county i that are made by Canadians:

$$s_i = \frac{T_{iC}}{T_{iU} + T_{iC}}.$$

This trips-based measure requires no choice of ψ, but it replaces S_{iU} with a noisy proxy, T_{iU}. The table's lower panel reports the results of setting s_i equal to the fraction of local residents who are Canadian:

$$s_i = \frac{S_{iC}}{S_{iU} + S_{iC}},$$

where S_{iC} is measured using Canada's 1991 census as the number of Canadians living within fifty miles of county i's central point, as defined by the U.S. Census. As we noted above in Section II, this population-based measure takes no account of the potential difficulties in traveling between these Canadians' homes and county i.

In spite of these potential shortcomings, the results from these two alternative measures of s_i are similar to each other and to our baseline results. The point estimates from using the trips-based measure are comparable to our baseline estimates, and the pattern of inference is identical to that based on the test statistics in Table 6. Using the population-based measure generally lowers the measured impact of real exchange rates. With two exceptions, in the establishments equation for Food Stores and in the average employment equation for Gasoline Service stations, the point estimates and the Wald test statistics tend to be closer to zero. However, these estimates still indicate that establishments in Food Stores, Gasoline Service

Stations, and Eating Places, respond within one year to real exchange rate disturbances.

D OLS Estimation

To gain a sense of how our results depend on our GMM estimation procedure, Table 8 reports results from estimating a version of our model using ordinary least squares. These estimates are only consistent as the number of time periods in the sample becomes large for a fixed number of counties, an assumption that poorly characterizes our sample of 256 counties over 20 years. Nevertheless, the estimated coefficients on the real exchange rate are not far from those reported in Table 6. The standard errors are comparable to those from the GMM estimation, and the inferences based on these exclusion tests are similar to those that use the GMM based tests. However, the exclusion tests' probability values rise to 0.220 and 0.092 for the coefficients in Food Stores' and Eating Places' establishments equations, while the test's probability value falls to 0.026 for Gasoline Service Stations' average employment equation. It appears that our results manifest themselves even in a simple OLS regression.

E Canada-U.S. FTA Subsample Estimation

The FTA substantially integrated the market for wholesale gasoline, so we may expect that our model's parameters for Gasoline Service Stations changed in 1989. Although the FTA did not completely integrate the wholesale goods markets that supply the other three industries, the possibility that it could have affected them in a way that changes our model's parameters is clear. Table 9 addresses this possibility. Its top panel reports estimates of β obtained using only data from the pre-FTA period of 1977 through 1988, and its bottom panel reports the analogous estimates obtained from the last 8 years of our sample.

As expected, the standard errors of the subsample estimates are considerably larger than those from the full sample. In Gasoline Service Stations, coefficient estimates for the establishment question are indeed sensitive to the subsample chosen. In the earlier sample, the coefficient multiplying $s_i \times \ln r_t$ is close to zero, that for $s_i \times \ln r_{t-1}$ is positive and not statistically significant, and the Wald test statistic has a probability value of 0.068. Using the later sample, these coefficients are -0.385 and 0.413. Both of these coefficients are statistically significant at the 5% level, but the Wald test statistic is almost exactly equal to its value in the earlier sample. The negative and statistically significant estimate from the later sample is difficult to interpret because it does not have the expected sign. We speculate that because wholesale gasoline is more easily arbitraged between border counties and Canada following the FTA's implementation, real exchange rate fluctuations after 1989 may be associated with systematic retail cost differences between border and interior counties.

For the other three industries, there are two notable differences between the subsample and full sample estimates. First, there is little evidence in either subsample that the number of Eating Places responds to the real exchange rate. We attribute this to a lack of statistical power to reject the null hypothesis in a smaller sample. Although the subsample point estimates from that equation are similar to those from the full sample, their associated standard errors are much larger. Second, the estimated timing of the response of the number of Food Stores to a real exchange rate shock depends greatly on the time period used for estimation. Using either the full sample or the post FTA period, the contemporaneous impact of a shock to the real exchange rate on the number of Food Stores is relatively small and negative, while its effect after one year is larger and positive. In the earlier subsample, the contemporaneous impact equals 0.415 and is statistically significant at the 1% level. The coefficient multiplying the lagged real exchange rate is large, negative, and statistically significant at the 1% level. Its magnitude is such that the estimated response of the number of establishments after one year to a completely transitory real exchange rate movement is approximately zero.

F Results Summary

Drinking Places conforms to the short-run/long-run dichotomy from basic microeconomic theory. Immediately following a demand shock, activity increases at each producer, while the number of producers does not change. Food Stores and Gasoline Service Stations do not fit this familiar description of industry evolution as well, because both the number of establishments and their average size increase following a demand shock. In Eating Places, there is no evidence that average producer size changes after a demand shock. Instead, that industry's short-run dynamics better match a *long-run* industry analysis in which demand only influences the number of producers.

While we find aspects of the most simple long-run analysis compelling for Eating Places' short-run fluctuations, it is incomplete because it abstracts from size differences between producers. Entering and exiting establishments tend to be smaller than the average continuing incumbent, so if all else is held equal an increase in entry or a decrease in exit will tend to decrease average producer size. This suggests that our estimates of the response of average employment to the real exchange rate may reflect both an increase in the average size of incumbents and an increase in the number of small establishments.

IV Conclusion

Much of the theory of industrial organization assumes that entry responds to persistent shocks only in the long run, so that incumbent producers can temporarily earn economic profits following a favorable aggregate demand or cost shock. Baumol, Panzar, and Willig (1982) show that the opposite assumption of very rapid entry with no sunk costs implies that incumbents never earn positive profits and that price always equals average cost. In spite of this theoretical importance, little is known about the speed with which entry can take place following a demand shock. Only in Drinking Places, where alcohol licensing restrictions might present a barrier to entry, does the real exchange rate affect industry activity without changing the number of establishments. In the other three industries we consider, either potential entrants, potential exiters, or both respond relatively rapidly to demand shocks. In models of perfect or monopolistic competition with instantaneous entry subject to a sunk cost, demand shocks change the equilibrium decisions of potential entrants, not those of exiting incumbents. This is a very robust theoretical result that only depends on the cost of entry being invariant to the number of entrants and their identities.[9] Thus, our results strongly suggest that potential entrants can affect their decisions shortly after demand shocks.

This paper's examination of retail industries' responses to demand shocks also sheds light on previous empirical work that documents their responses to cost disturbances. Card and Krueger (1994) estimate that the imposition of New Jersey's minimum wage in 1992 actually increased employment in their sample of fast food outlets relative to a control sample from Eastern Pennsylvania over an eight-month period. They find these employment gains following a minimum wage increase to be at odds with the basic theoretical prediction of downward sloping labor demand. Fast food outlets belong to one of the industries we have considered, Eating Places. Our finding that the number of establishments serving that industry responds to demand shocks very quickly suggests that a long-run analysis may reconcile Card and Krueger's observations with competitive economic theory. Because the *location* of the average cost curve's minimum can either decrease, remain unchanged, or increase following a factor price change; the effect of a minimum wage increase on an individual producer's long-run size is ambiguous. To the extent that greater output is associated with greater employment, Card and Krueger's finding that employment increased in their panel of incumbent producers can be interpreted as an increase in the location of the average cost curve's minimum. In this interpretation, industry-wide output and employment fell following the minimum wage increase because a drop in entry reduced the *number* of producers. Determining how well this theory characterizes Eating Places' responses to minimum wage changes is on our agenda for future research.[10]

[9] See, for example, Campbell and Fisher (1996). They present a perfectly competitive industry dynamics model with idiosyncratic producer risk, sunk costs of entry, and endogenous exit. In that model, demand shocks only contemporaneously impact the number of entrants.

[10] Card and Krueger (1994) discuss this possibility and examine it by estimating the relationship between

Our results also present a challenge for the branch of international macroeconomics which focuses on the puzzle of persistent deviations from purchasing power parity (PPP), such as Betts and Devereux's (2000) and Chari, Kehoe, and McGrattan's (2002) general equilibrium analyses. The failure of consumer prices to respond to nominal exchange rate movements and the infrequency with which individual retailers change their prices suggest that sticky nominal retail prices play a central role in generating and maintaining deviations from PPP. Our observation that real exchange rate fluctuations affect the number of establishments serving border counties' retail trade industries presents two difficulties for the view that deviations from PPP arise from retail-level price stickiness. First, it is difficult to imagine that some retailers' prices are fixed for a longer period than it takes to open or close an entire establishment in their industry.[11] Second, sticky prices are by definition those of incumbent producers. Because an incumbent's artificially low price reduces the payoff to entry, periods in which U.S. retailers' prices are low relative to those of Canadian retailers should not be periods of increased entry in the U.S. Our future research will assess the severity of these difficulties for models of real exchange rates and monetary non-neutrality that rely on sticky prices.

minimum wage changes and the openings of new McDonald's restaurants. They find no significant effect of minimum wage changes on McDonald's entry, but McDonald's may not be representative of potential entrants.

[11] Bils and Klenow (2002) measure the average duration of retailers' prices for a variety of items. Those sold in Eating Places and Drinking Places have average durations of between 7 and 10 months, while the durations of those sold by Food Stores and Gasoline Service Stations are between 1 and 2 months.

References

Baumol, William J., John C. Panzar, and Robert D. Willig. (1982) *Contestable Markets and the Theory of Industry Structure.* New York, Harcourt Brace Jovanovich.

Betts, Caroline and Michael Devereux. (2000) Exchange Rate Dynamics in a Model of Pricing-to-Market. *Journal of International Economics* **50**, 215-244.

Bils, Mark and Peter J. Klenow. (2002) Some Evidence on the Importance of Sticky Prices. NBER Working Paper 9069.

Blundell, Richard and Stephen Bond. (1998) Initial Conditions and Moment Restrictions in Dynamic Panel Data Models. *Journal of Econometrics,* **87**(1), 115–143.

Campbell, Jeffrey R. and Jonas D.M. Fisher. (1996) Aggregate Employment Fluctuations with Microeconomic Asymmetries. NBER Working Paper 5767.

Campbell, Jeffrey R. and Beverly Lapham. (2001) Real Exchange Rate Fluctuations and the Dynamics of Retail Trade Industries on the U.S.-Canada Border. NBER Working Paper 8558.

Card, David and Alan Krueger. (1994) Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania. *American Economic Review,* **84**(4), 772-793.

Chari, V.V., Patrick J. Kehoe, and Ellen R. McGrattan. (2002) Can Sticky Price Models Generate Volatile and Persistent Real Exchange Rates? *Review of Economic Studies,* **69**(3), 533–563.

Engel, Charles. (1999) Accounting for U.S. Real Exchange Rate Changes. *Journal of Political Economy,* **107**, 507-538.

Engel, Charles and John Rogers. (1996) How Wide is the Border? *American Economic Review,* **86**(5), 1112-1125.

Ford, Theresa. (1992) International Outshopping Along the Canada-United States Border: The Case of Western New York. Canada-United States Trade Center Occasional Paper No. 12, State University of New York at Buffalo

New Brunswick, Government of. (1992) A Discussion Paper on Cross Border Shopping. Fredericton: Department of Economic Development and Tourism.

Ontario, Government of. (1991) Report on Cross-Border Shopping. Toronto: Standing Committee on Finance and Economic Affairs.

Taylor, Teresa. (1988) The U.S.-Canada Free Trade Agreement: Winners and Losers in the Northeast-Midwest Region. Northeast Midwest Institute Center for Regional Policy.

Wilson, Arlene. (1993) The Canada-U.S. Free Trade Agreement: Lessons for the NAFTA. Congressional Research Service Report for Congress 93-153 E.

Table 1: Quartiles from Sample Counties of Average Establishment Counts

Industry	Counties(i)	First Quartile(ii)	Median(ii)	Third Quartile(ii)
Food Stores	256	27.0	45.5	94.8
Gasoline Service Stations	251	18.5	28.0	51.5
Eating Places	242	39.0	73.8	152.0
Drinking Places	233	11.5	18.0	38.0

Notes: (i) Refers to the number of counties included in the estimation sample for each industry. (ii) For each included county, the average number of establishments serving each industry between 1977 and 1996 was calculated. 'First Quartile', 'Median', and 'Third Quartile' refer to the quartiles of that statistic across all sample counties for that industry. See the text for further details.

Table 2: Median Within-County Standard Deviations[(i)]

Industry	Establishments		Average Employment	
	Interior Counties	Border Counties	Interior Counties	Border Counties
Food Stores	0.11	0.09	0.13	0.11
Gasoline Service Stations	0.13	0.16	0.15	0.20
Eating Places	0.09	0.08	0.10	0.11
Drinking Places	0.17	0.15	0.22	0.20

Note: (i) For each industry, each of the variables was first logged and regressed against a set of time dummies. The sample standard deviations of the residuals from those regressions were tabulated for each county. The values reported in the table are the medians, across interior counties and across border counties, of these statistics. See the text for further details.

Table 3: Consumer Price Index Sources for Relative Price Series

Industry	U.S. CPI[i]	Canadian CPI[i]
Food Stores	Food at Home	Food Purchased from Stores
Gasoline Service Stations	Gasoline	Gasoline
Eating Places	Food Away from Home	Food Purchased from Restaurants
Drinking Places	Alcoholic Beverages Away from Home	Served Alcoholic Beverages

Note: (i) For each industry, the column headed U.S. CPI reports the name of the U.S. consumer price index series used in constructing the relative price, and the column headed Canadian CPI reports the name of the analogous Canadian series. See the text for further details.

Table 4: Summary Statistics for Relative Price Series[i]

Industry	Standard Deviation	First Autocorrelation	Correlation with Aggregate Real Exchange Rate
Food Stores	0.075	0.87	0.63
Gasoline Service Stations	0.214	0.88	0.47
Eating Places	0.070	0.75	0.93
Drinking Places[ii]	0.085	0.82	0.92

Notes: (i) The first two columns report the standard deviation and first autocorrelation of the relative price series used for the corresponding industry over the sample period 1977-1996. The final column gives the contemporaneous correlation between the relative price series and the relative price of "all goods less energy". (ii) Sample period for the relative price series for Drinking Places begins in 1979. See the text for further details.

Table 5: Estimates for SIC 54, Food Stores[(i),(ii)]

	Dependent Variable	
	Establishments	Average Employment
Lagged Establishments, $\ln N_{it-1}$	0.788^{***} (0.026)	-0.078 (0.047)
Lagged Average Employment, $\ln A_{it-1}$	0.015 (0.020)	0.496^{***} (0.045)
Current Real Exchange Rate, $s_i \times \ln r_t$	-0.087 (0.059)	0.036 (0.129)
Lagged Real Exchange Rate, $s_i \times \ln r_{t-1}$	0.165^{**} (0.072)	0.140 (0.149)
Exclusion Test for Real Exchange Rate[(iii)]	5.93 (0.052)	10.00 (0.007)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically different from zero at the 10%, 5%, and 1% levels. (iii) The Wald exclusion tests are asymptotically distributed as χ^2 random variables with 2 degrees of freedom. Probability values from this distribution appear below each test statistic. See the text for further details.

Table 6: Baseline Estimation Results[i][ii]

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[iii]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[iii]
Food Stores	−0.087	0.165**	5.93	0.036	0.140	10.00
	(0.059)	(0.072)	(0.052)	(0.129)	(0.149)	(0.007)
Gasoline Service Stations	−0.041	0.127**	9.30	0.128	−0.024	3.38
	(0.042)	(0.052)	(0.010)	(0.089)	(0.062)	(0.184)
Eating Places	0.111	0.041	7.32	0.035	−0.018	0.14
	(0.092)	(0.071)	(0.026)	(0.097)	(0.092)	(0.931)
Drinking Places	0.145	−0.056	1.41	0.597*	−0.098	6.63
	(0.147)	(0.144)	(0.493)	(0.325)	(0.292)	(0.036)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically different from zero at the 10%, 5%, and 1% levels. (iii) Asymptotically, this test statistic has a χ^2 distribution with 2 degrees of freedom. Probability values from this distribution appear in parentheses below each test statistic. See the text for further details.

Table 7: Estimation Results using Alternative Sensitivity Measures[i][ii]

s_i =Canadian Trips/Total Trips

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[iii]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[iii]
Food Stores	−0.101	0.174**	4.88	0.075	0.121	8.32
	(0.068)	(0.081)	(0.087)	(0.156)	(0.169)	(0.016)
Gasoline Service	−0.042	0.132**	7.55	0.130	−0.021	3.13
Stations	(0.044)	(0.056)	(0.023)	(0.094)	(0.066)	(0.209)
Eating Places	0.110	0.050	6.85	0.051	−0.027	0.23
	(0.131)	(0.093)	(0.033)	(0.113)	(0.108)	(0.891)
Drinking Places	0.184	−0.100	1.24	0.809**	−0.161	8.59
	(0.194)	(0.190)	(0.537)	(0.338)	(0.306)	(0.014)

s_i =Canadian Population/Total Population

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[iii]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[iii]
Food Stores	−0.133*	0.223**	7.42	−0.040	0.017	0.26
	(0.070)	(0.087)	(0.024)	(0.108)	(0.115)	(0.879)
Gasoline Service	0.015	0.031	4.11	0.127**	−0.009	10.02
Stations	(0.034)	(0.048)	(0.128)	(0.055)	(0.054)	(0.007)
Eating Places	0.053	0.034	6.66	−0.043	0.088	1.47
	(0.081)	(0.066)	(0.036)	(0.070)	(0.076)	(0.480)
Drinking Places	−0.030	0.112	2.93	0.206	−0.135	1.00
	(0.104)	(0.100)	(0.231)	(0.208)	(0.189)	(0.607)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically different from zero at the 10%, 5%, and 1% levels. (iii) Asymptotically, this test statistic has a χ^2 distribution with 2 degrees of freedom. Probability values from this distribution appear in parentheses below each test statistic. See the text for further details.

Table 8: OLS Estimation Results[i][ii]

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[iii]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[iii]
Food Stores	-0.127	0.190	3.02	0.075	0.088	4.88
	(0.092)	(0.110)	(0.220)	(0.110)	(0.136)	(0.087)
Gasoline Service Stations	-0.051	0.146^{**}	14.46	0.127	-0.009	7.28
	(0.062)	(0.062)	(0.000)	(0.095)	(0.093)	(0.026)
Eating Places	0.133	0.006	4.78	0.022	0.034	0.38
	(0.082)	(0.093)	(0.092)	(0.149)	(0.132)	(0.831)
Drinking Places	0.151	-0.054	0.64	0.622^{**}	-0.074	12.52
	(0.174)	(0.178)	(0.53)	(0.295)	(0.328)	(0.002)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically different from zero at the 10%, 5%, and 1% levels. (iii) This test statistic has an asymptotic χ^2 distribution with 2 degrees of freedom as $T \to \infty$. Probability values from this distribution appear in parentheses below each test statistic. See the text for further details.

Table 9: Subsample Estimation Results[(i)(ii)]

Before the FTA, 1977–1988

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[(iii)]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[(iii)]
Food Stores	0.415***	−0.346***	9.64	0.129	0.403*	13.01
	(0.140)	(0.155)	(0.008)	(0.185)	(0.231)	(0.001)
Gasoline Service Stations	−0.005	0.080	5.37	0.139	0.017	4.73
	(0.056)	(0.082)	(0.068)	(0.103)	(0.052)	(0.094)
Eating Places	0.076	−0.020	0.21	0.107	−0.088	0.41
	(0.185)	(0.121)	(0.899)	(0.172)	(0.195)	(0.816)
Drinking Places	0.305	−0.255	1.23	0.774	0.227	4.58
	(0.277)	(0.395)	(0.541)	(0.519)	(0.473)	(0.101)

After the FTA, 1989–1996

Industry	Establishments $s_i \times \ln r_t$	Establishments $s_i \times \ln r_{t-1}$	Establishments χ^2 Test[(iii)]	Average Employment $s_i \times \ln r_t$	Average Employment $s_i \times \ln r_{t-1}$	Average Employment χ^2 Test[(iii)]
Food Stores	−0.097	0.243	7.14	0.164	−0.088	2.76
	(0.177)	(0.172)	(0.028)	(0.225)	(0.259)	(0.252)
Gasoline Service Stations	−0.385**	0.413**	5.44	0.102	0.154	3.61
	(0.174)	(0.177)	(0.066)	(0.246)	(0.229)	(0.165)
Eating Places	0.105	0.153	2.79	−0.213	0.083	2.28
	(0.116)	(0.200)	(0.248)	(0.146)	(0.171)	(0.320)
Drinking Places	−0.125	0.285	1.12	0.599	−0.126	4.70
	(0.222)	(0.334)	(0.571)	(0.443)	(0.492)	(0.095)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically different from zero at the 10%, 5%, and 1% levels. (iii) Asymptotically, this test statistic has a χ^2 distribution with 2 degrees of freedom. Probability values from this distribution appear in parentheses below each test statistic. See the text for further details.

Figure 1: The Real Exchange Rate and Cross-Border Shopping

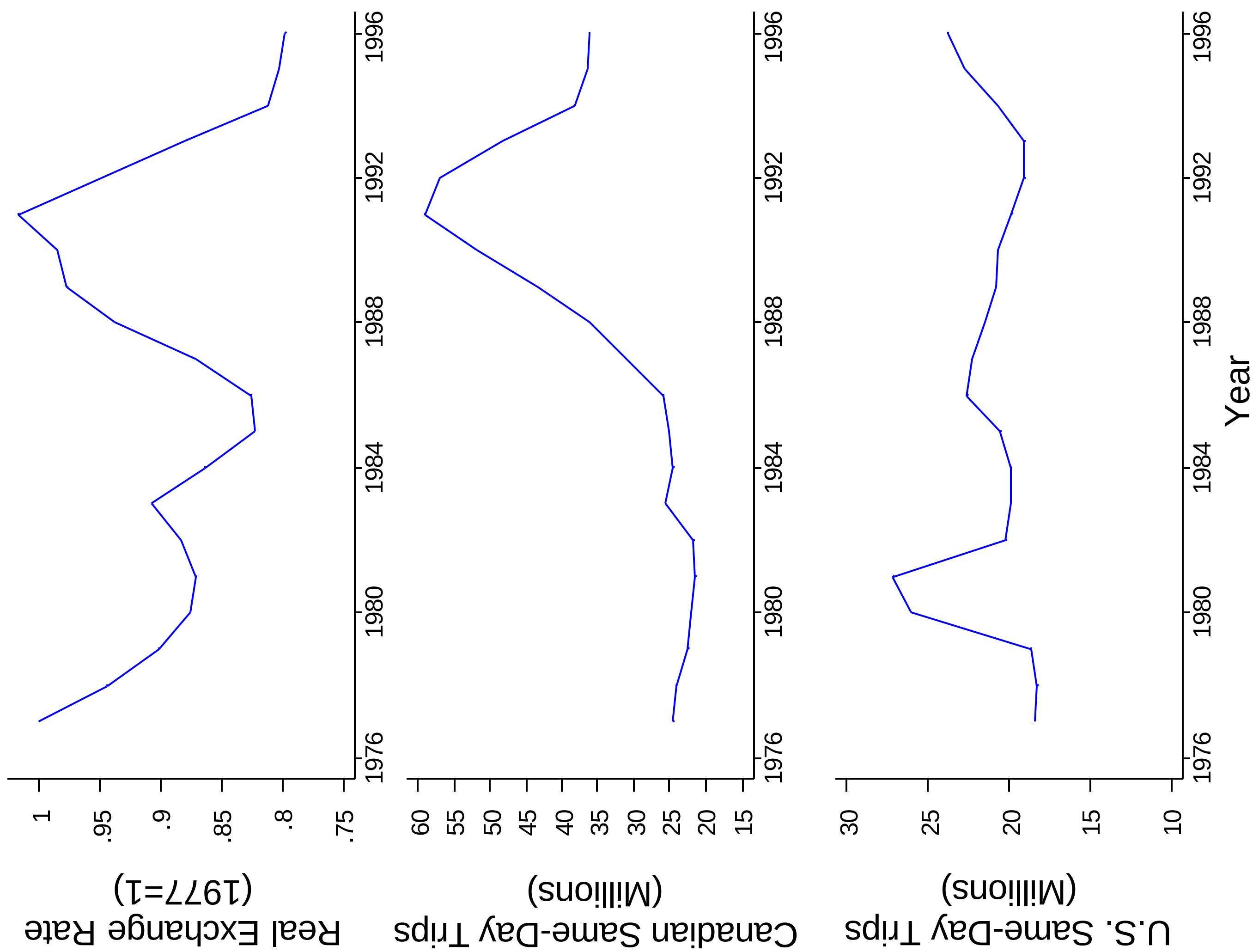


Figure 2: Gasoline Service Stations in Border and Interior Counties

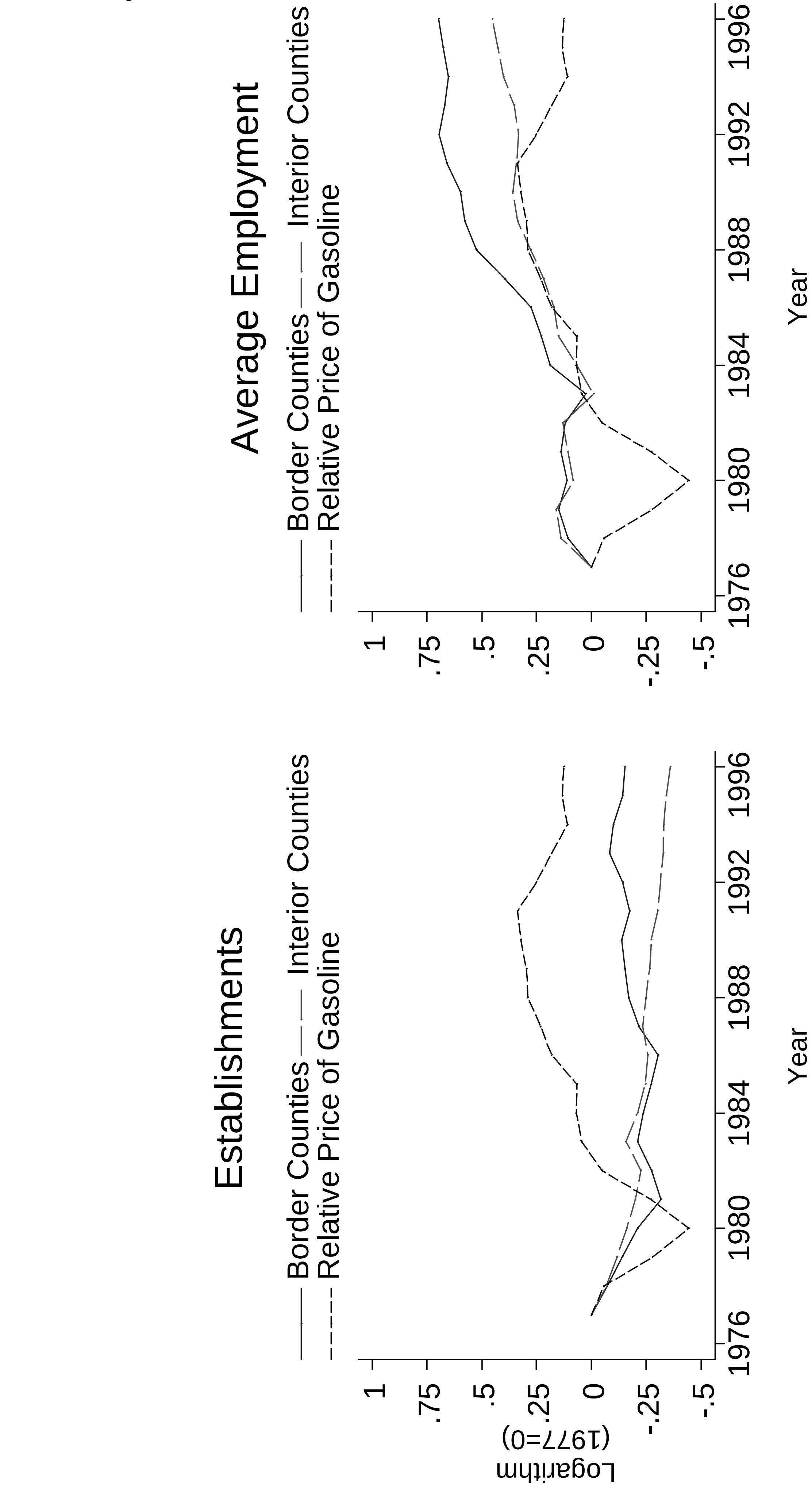


Figure 3: Estimated Coefficients on Time Dummies for SIC 54: Food Stores

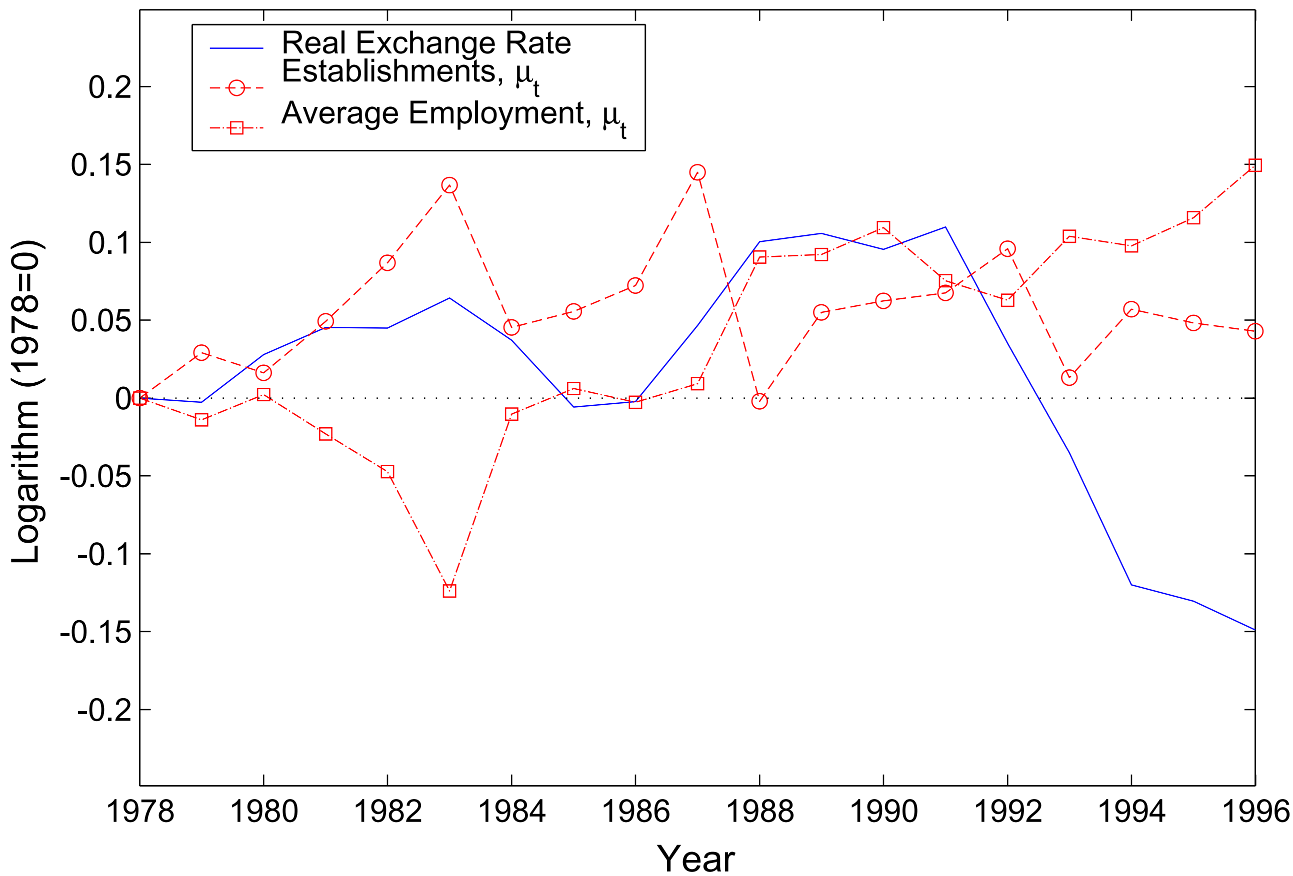


Figure 3: Impulse Response Functions for SIC 54, Food Stores

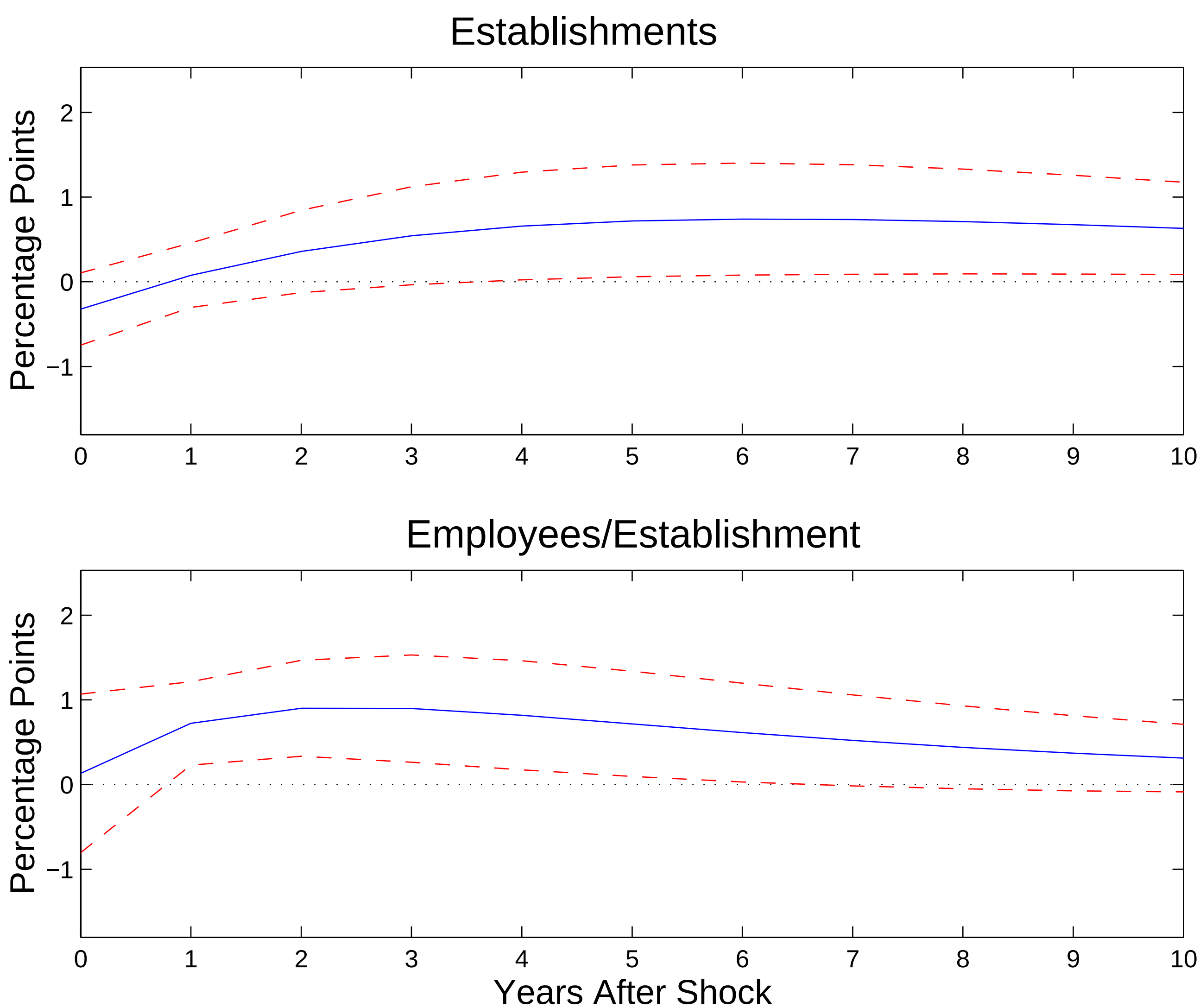


Figure 4: Impulse Response Functions for SIC 554, Gasoline Service Stations

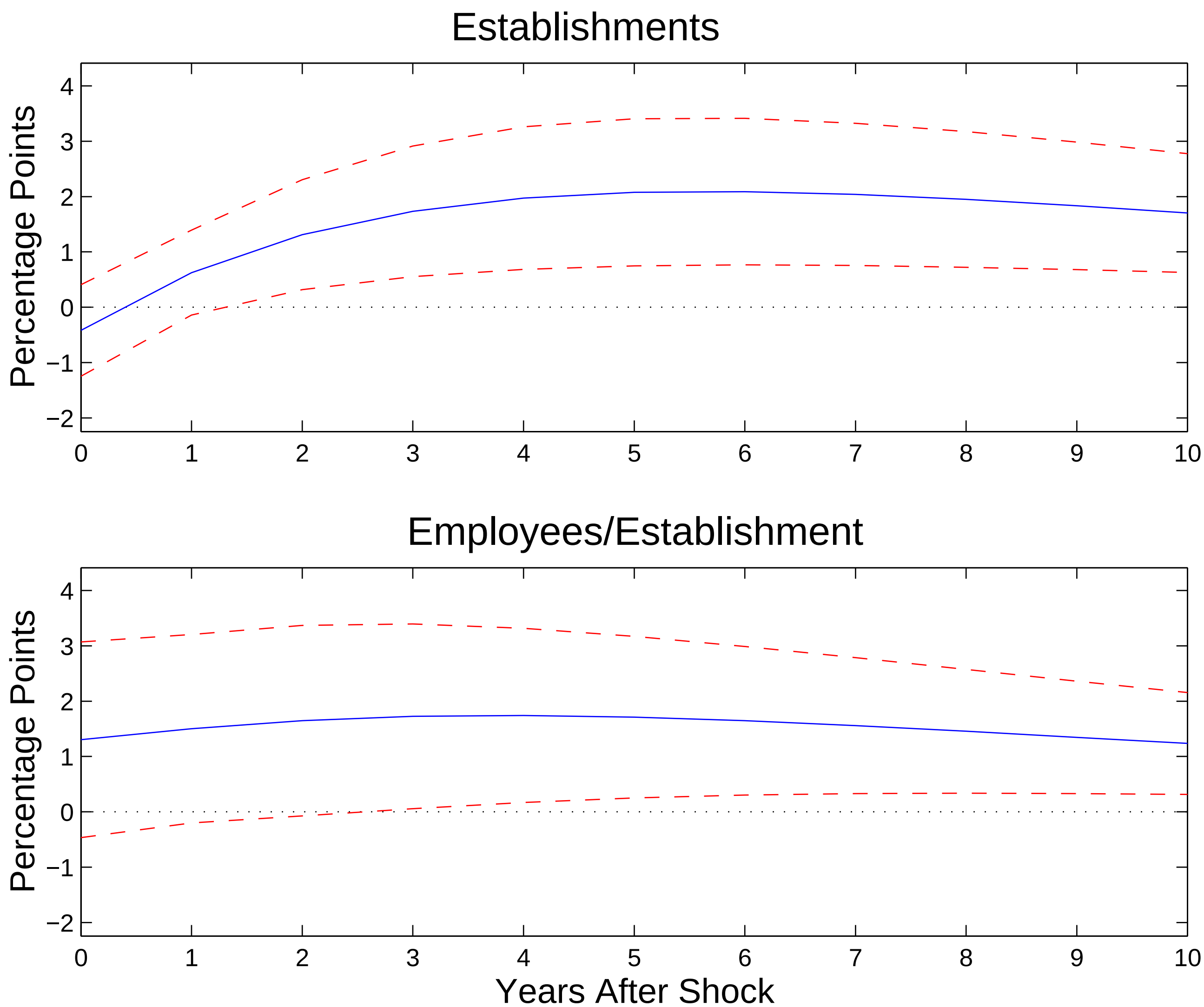


Figure 5: Impulse Response Functions for SIC 5812, Eating Places

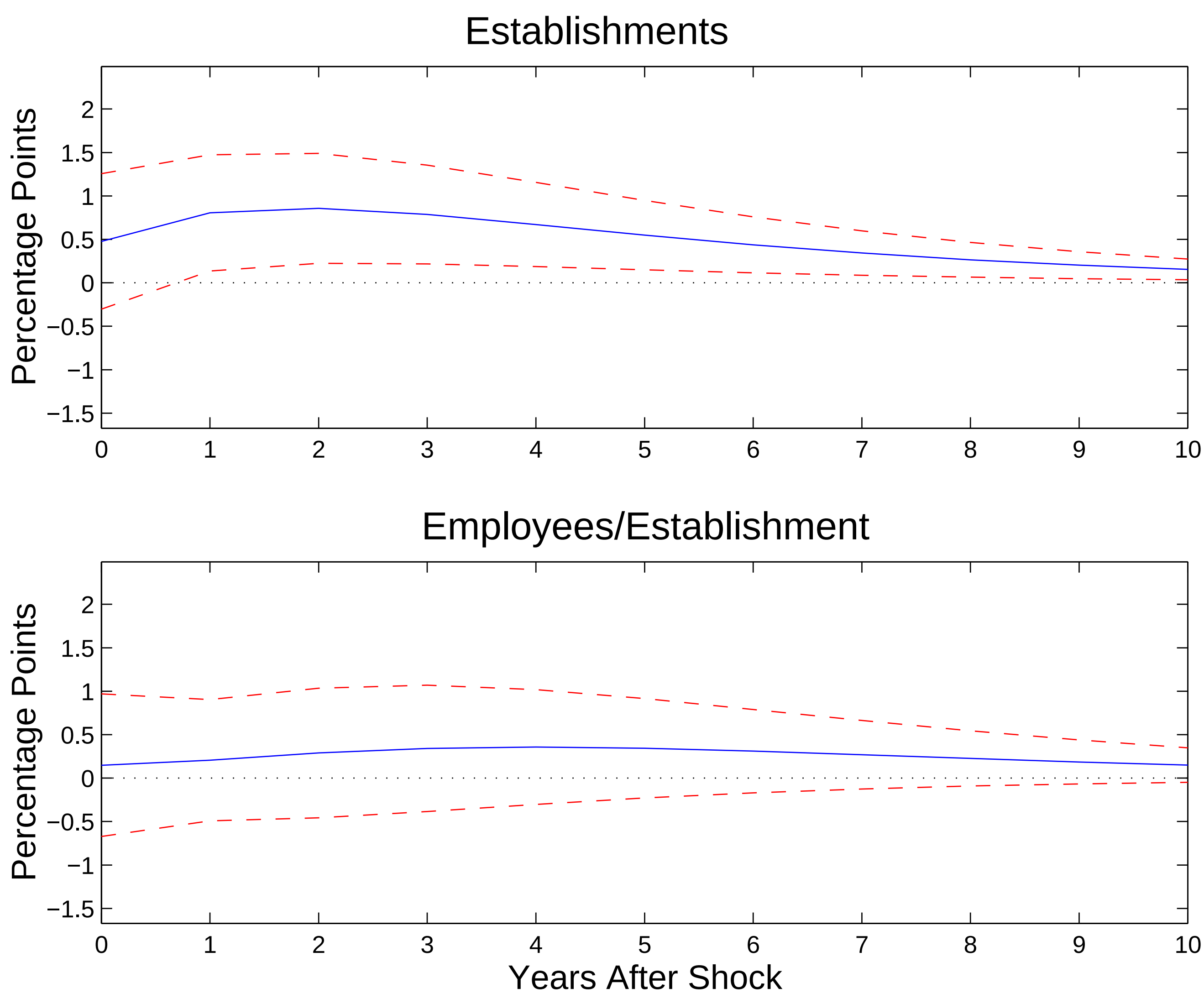
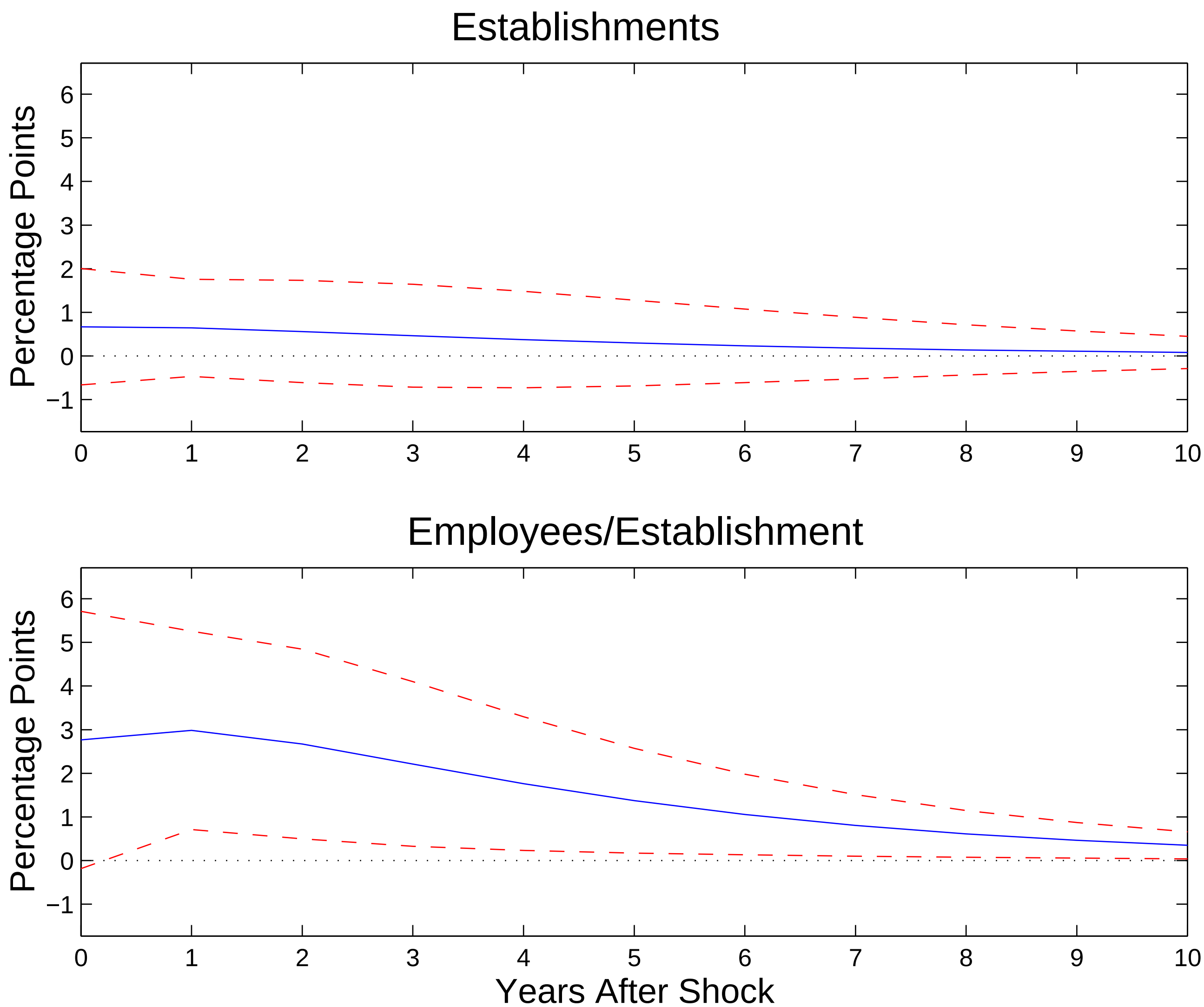


Figure 6: Impulse Response Functions for SIC 5813, Drinking Places



Working Paper Series

A series of research studies on regional economic issues relating to the Seventh Federal Reserve District, and on financial and economic topics.

Working Paper Series *(continued)*

Working Paper Series *(continued)*

Working Paper Series *(continued)*

The Double Play: Simultaneous Speculative Attacks on Currency and Equity Markets *Sujit Chakravorti and Subir Lall*	**WP-00-17**
Capital Requirements and Competition in the Banking Industry *Peter J.G. Vlaar*	**WP-00-18**
Financial-Intermediation Regime and Efficiency in a Boyd-Prescott Economy *Yeong-Yuh Chiang and Edward J. Green*	**WP-00-19**
How Do Retail Prices React to Minimum Wage Increases? *James M. MacDonald and Daniel Aaronson*	**WP-00-20**
Financial Signal Processing: A Self Calibrating Model *Robert J. Elliott, William C. Hunter and Barbara M. Jamieson*	**WP-00-21**
An Empirical Examination of the Price-Dividend Relation with Dividend Management *Lucy F. Ackert and William C. Hunter*	**WP-00-22**
Savings of Young Parents *Annamaria Lusardi, Ricardo Cossa, and Erin L. Krupka*	**WP-00-23**
The Pitfalls in Inferring Risk from Financial Market Data *Robert R. Bliss*	**WP-00-24**
What Can Account for Fluctuations in the Terms of Trade? *Marianne Baxter and Michael A. Kouparitsas*	**WP-00-25**
Data Revisions and the Identification of Monetary Policy Shocks *Dean Croushore and Charles L. Evans*	**WP-00-26**
Recent Evidence on the Relationship Between Unemployment and Wage Growth *Daniel Aaronson and Daniel Sullivan*	**WP-00-27**
Supplier Relationships and Small Business Use of Trade Credit *Daniel Aaronson, Raphael Bostic, Paul Huck and Robert Townsend*	**WP-00-28**
What are the Short-Run Effects of Increasing Labor Market Flexibility? *Marcelo Veracierto*	**WP-00-29**
Equilibrium Lending Mechanism and Aggregate Activity *Cheng Wang and Ruilin Zhou*	**WP-00-30**
Impact of Independent Directors and the Regulatory Environment on Bank Merger Prices: Evidence from Takeover Activity in the 1990s *Elijah Brewer III, William E. Jackson III, and Julapa A. Jagtiani*	**WP-00-31**
Does Bank Concentration Lead to Concentration in Industrial Sectors? *Nicola Cetorelli*	**WP-01-01**
On the Fiscal Implications of Twin Crises *Craig Burnside, Martin Eichenbaum and Sergio Rebelo*	**WP-01-02**

Working Paper Series *(continued)*

Sub-Debt Yield Spreads as Bank Risk Measures *Douglas D. Evanoff and Larry D. Wall*	**WP-01-03**
Productivity Growth in the 1990s: Technology, Utilization, or Adjustment? *Susanto Basu, John G. Fernald and Matthew D. Shapiro*	**WP-01-04**
Do Regulators Search for the Quiet Life? The Relationship Between Regulators and The Regulated in Banking *Richard J. Rosen*	**WP-01-05**
Learning-by-Doing, Scale Efficiencies, and Financial Performance at Internet-Only Banks *Robert DeYoung*	**WP-01-06**
The Role of Real Wages, Productivity, and Fiscal Policy in Germany's Great Depression 1928-37 *Jonas D. M. Fisher and Andreas Hornstein*	**WP-01-07**
Nominal Rigidities and the Dynamic Effects of a Shock to Monetary Policy *Lawrence J. Christiano, Martin Eichenbaum and Charles L. Evans*	**WP-01-08**
Outsourcing Business Service and the Scope of Local Markets *Yukako Ono*	**WP-01-09**
The Effect of Market Size Structure on Competition: The Case of Small Business Lending *Allen N. Berger, Richard J. Rosen and Gregory F. Udell*	**WP-01-10**
Deregulation, the Internet, and the Competitive Viability of Large Banks and Community Banks *Robert DeYoung and William C. Hunter*	**WP-01-11**
Price Ceilings as Focal Points for Tacit Collusion: Evidence from Credit Cards *Christopher R. Knittel and Victor Stango*	**WP-01-12**
Gaps and Triangles *Bernardino Adão, Isabel Correia and Pedro Teles*	**WP-01-13**
A Real Explanation for Heterogeneous Investment Dynamics *Jonas D.M. Fisher*	**WP-01-14**
Recovering Risk Aversion from Options *Robert R. Bliss and Nikolaos Panigirtzoglou*	**WP-01-15**
Economic Determinants of the Nominal Treasury Yield Curve *Charles L. Evans and David Marshall*	**WP-01-16**
Price Level Uniformity in a Random Matching Model with Perfectly Patient Traders *Edward J. Green and Ruilin Zhou*	**WP-01-17**
Earnings Mobility in the US: A New Look at Intergenerational Inequality *Bhashkar Mazumder*	**WP-01-18**
The Effects of Health Insurance and Self-Insurance on Retirement Behavior *Eric French and John Bailey Jones*	**WP-01-19**

Working Paper Series *(continued)*

The Effect of Part-Time Work on Wages: Evidence from the Social Security Rules **WP-01-20**
Daniel Aaronson and Eric French

Antidumping Policy Under Imperfect Competition **WP-01-21**
Meredith A. Crowley

Is the United States an Optimum Currency Area?
An Empirical Analysis of Regional Business Cycles **WP-01-22**
Michael A. Kouparitsas

A Note on the Estimation of Linear Regression Models with Heteroskedastic
Measurement Errors **WP-01-23**
Daniel G. Sullivan

The Mis-Measurement of Permanent Earnings: New Evidence from Social **WP-01-24**
Security Earnings Data
Bhashkar Mazumder

Pricing IPOs of Mutual Thrift Conversions: The Joint Effect of Regulation
and Market Discipline **WP-01-25**
Elijah Brewer III, Douglas D. Evanoff and Jacky So

Opportunity Cost and Prudentiality: An Analysis of Collateral Decisions in
Bilateral and Multilateral Settings **WP-01-26**
Herbert L. Baer, Virginia G. France and James T. Moser

Outsourcing Business Services and the Role of Central Administrative Offices **WP-02-01**
Yukako Ono

Strategic Responses to Regulatory Threat in the Credit Card Market* **WP-02-02**
Victor Stango

The Optimal Mix of Taxes on Money, Consumption and Income **WP-02-03**
Fiorella De Fiore and Pedro Teles

Expectation Traps and Monetary Policy **WP-02-04**
Stefania Albanesi, V. V. Chari and Lawrence J. Christiano

Monetary Policy in a Financial Crisis **WP-02-05**
Lawrence J. Christiano, Christopher Gust and Jorge Roldos

Regulatory Incentives and Consolidation: The Case of Commercial Bank Mergers
and the Community Reinvestment Act **WP-02-06**
Raphael Bostic, Hamid Mehran, Anna Paulson and Marc Saidenberg

Technological Progress and the Geographic Expansion of the Banking Industry **WP-02-07**
Allen N. Berger and Robert DeYoung

Choosing the Right Parents: Changes in the Intergenerational Transmission **WP-02-08**
of Inequality — Between 1980 and the Early 1990s
David I. Levine and Bhashkar Mazumder

Working Paper Series *(continued)*