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

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Abstract

This paper estimates the effects of real exchange rate fluctuations on retail trade industries located on the U.S.-Canada border. Consumers living near this border can shift their expenditures between the two countries, so real exchange rate fluctuations can act as demand shocks to border areas' retail trade industries. Using county level data, we estimate the effects of real exchange rates on the number of establishments and their average payroll in five retail industries. To isolate the expenditure-shifting effect of exchange rates, our estimation uses observations from counties off the border to control for the economy-wide effects of the unobserved structural shocks. In three of the industries, the number of operating establishments responds either contemporaneously or with a lag of one year to real exchange rate movements.

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This paper estimates the effects of real exchange rate fluctuations on the number of establishments and their average payroll in retail trade industries located on the U.S.-Canada border. It is widely known that there are large and persistent deviations from purchasing power parity (PPP) (see Rogoff (1996) for a survey). Engel (1999) and Engel and Rogers (1996) have documented that deviations from the law of one price for traded goods can account for most of the observed deviations from PPP between the U.S. and Canada, and that these price differences even characterize otherwise similar goods sold on different sides of these countries common border. Because consumers who live near the border can shift their expenditures towards the cheaper country, movements in the international relative price of goods may act as a demand shock for retail producers located near the border. Using county level data for several retail industries, our empirical analysis estimates the impact of industry-specific real exchange rate fluctuations on net entry and average store size in border counties. In three of the five industries we consider, a real exchange rate appreciation causes the number of establishments to decline contemporaneously or with a lag of one year. Therefore, the "long-run" changes in the number of establishments that theory predicts occur in fact very quickly.

Di Matteo and Di Matteo (1993,1996) provide compelling evidence that movements in the real exchange rate between the U.S. and Canada does induce the type of expenditure shifting behavior on the part of consumers that we have described above. In addition, Ford (1992) provides survey data which indicates that the primary reason that Canadians shop in the U.S. is lower prices. Figure 1 summarizes this evidence. Its top panel depicts the nominal exchange rate (Canadian dollars per U.S. dollar) and the aggregate real exchange rate constructed using aggregate CPIs from 1972-1998, and its bottom two panels plot a measure of cross-border shoppers, same-day automobile trips by U.S. vehicles into Canada and by Canadian vehicles into the U.S. The figure demonstrates that the appreciation of the Canadian dollar from 1986-1992 was accompanied by a large increase in trips by Canadians and a slight decrease in trips by Americans. The subsequent depreciation of the Canadian dollar from 1992-1998 was accompanied by a large decrease in trips by Canadians and a large increase in trips by Americans. The spike in U.S. trips in 1980-1981 came at a time when the Canadian National Energy Policy dramatically reduced the price of gasoline in Canada relative to the U.S.

The real exchange rate is an endogenous variable, determined by structural shocks that themselves can have direct effects on consumers' purchasing decisions and retailers' costs. To address this issue, we first present a model economy of cross-border shopping which predicts a relationship between the real exchange rate for an industry and the average size of firms and the number of firms operating in the industry. The results of this model motivate our empirical specification. To identify the expenditure shifting effects of real exchange rates on border counties' retail trade industries, our estimation uses observations from retail trade industries in counties that do not share a border with Canada to control for the direct effects of unobserved structural shocks. Our estimation procedure, based on Blundell and Bond's (1998) GMM estimator of a univariate panel-data autoregression, also accounts for unobserved county-specific means of the observable variables, autocorrelation in county-specific disturbances to demand and cost, and differences between prices in a given area and their national aggregate counterparts.

Our empirical analysis shows that real exchange rate fluctuations have a significant impact on retail trade activity in border counties. In Food Stores and Eating Places, a real exchange rate appreciation causes the number of operating establishments to fall contemporaneously, while in Gasoline Service Stations the number of establishments falls after one year. In these industries, we also estimate that our measure an economically significant decline in our measure average establishment size, average payroll. However, this decline is not statistically significant. In Drinking Places, average payroll drops precipitously following a real appreciation, but the number of establishments does not change. In only one of our five industries, Apparel and Accessory Stores, we measure no statistically significant impact of exchange rates on industry activity. However, the point-estimates for this industry are large, suggesting that our estimation procedure has little power when applied to this industry's data. We believe that these results are interesting because the response of border counties' retail trade industries to this particular demand shock yields insight into how retail industries throughout the U.S. respond to other demand shocks that are more difficult to measure.

The rest of this paper is organized as follows. In the next section, we present a model of retail industry dynamics with cross-border shopping, and we use it to characterize the expenditure-shifting effects of exchange rates. Section II derives the estimating equations we use from the model and describes our data estimation procedure in more detail. Section III presents the results of our estimation, and Section IV discusses the relationship of our work with the relevant literature from international macroeconomics and industrial organization. Section V contains concluding remarks regarding our results' implications for future research.

I A Cross-Border Shopping Model

In our empirical analysis, we examine the effects of movements in industry-level relative prices between the U.S. and Canada. These relative prices are endogenous variables so we require a model economy to clarify how our estimation identifies the expenditure-shifting effects of movements in these prices on border-counties' retail trade industries. Furthermore, we expect heterogeneity across counties on the border with respect to their exposure to cross-border shopping. The model provides a measure of exposure which we incorporate into our empirical analysis.

In the model, there are two countries, the United States (U) and Canada (C), each of which is composed of distinct counties. A county can be located on the border between the two countries, or it can be an interior county. Each county has a retail-trade sector that produces differentiated goods in a monopolistically-competitive market with free entry. Each country has a domestic currency and prices of all goods sold in a country are denominated in the local currency. Deviations from PPP arise from differences in the two countries' retailers' marginal costs. One potential cause of such cost differences is wholesale price stickiness.Another source of cost differences, suggested by the experience of the National Energy Policy, is regulatory differences between the countries that cannot be legally exploited by wholesale arbitrageurs. We analyze the response of a border county's retail-trade industry to relative cost shocks, focusing on the behavior of variables we observe in our data, the number of establishments and their average payroll. Firms enter and set prices after observing costs. Diverging costs lead to price differences across international producers and this induces some of the consumers living in border counties to shift their expenditures towards the country with lower prices.

In what follows, we consider consumers and producers in county U located in country U and producers in county C located directly across the international border in country C.

A Consumers

There are S_j consumers in county j and a fraction, λ, of those consumers purchase from both domestic and foreign retailers. We refer to these consumers as travellers. The remainder of consumers in a county only purchase from domestic retailers and are referred to as non-travellers. Consumers have the following preferences:

$$U = d^{1-\gamma} \left(\int_0^{N_U} x_{jU}^{\nu} dj + \int_1^{N_C} x_{iC}^{\nu} di \right)^{\frac{\gamma}{\nu}},$$

where d is consumption of an outside homogeneous good, x_{jU} is consumption of differentiated good j sold in county U, and x_{iC} is consumption of differentiated good i sold in county C. For non-travellers, consumption of goods sold by foreign retailers equals zero. The number of distinct goods offered for sale in county j is N_j and is determined in equilibrium. All consumers are endowed with ω units of the outside good which they use to finance consumption expenditures.

The price of the manufactured good in country j, q_j, is exogenous to consumers and producers. The law of one price holds for this good so the nominal exchange rate is determined as follows:

$$e = \frac{q_U}{q_C}$$

We let p_{ij} denote the price of retail good i offered in county j denominated in the local currency. The budget constraint of a consumer in county U denominated in that country's currency is

$$\int_0^{N_U} p_{iU} x_{iU} di + \int_0^{N_C} (e p_{iC}) x_{iC} di + q_U d = q_U \omega$$

The consumer in county C faces an analogous budget constraint.

Consumers in each county choose consumption of each relevant good to maximize their period utility subject to their budget constraints. The solutions to these optimization problems give the following Marshallian demands. The demands of a traveller in county U for retail goods are given by

$$\begin{aligned} x_{iU}^T &= \frac{(\gamma q_U \omega)\, p_{iU}^{\frac{1}{\nu-1}}}{P^T} & i \in [0, N_U] \\ x_{iC}^T &= \frac{(\gamma q_U \omega)\, (e p_{iC})^{1/(\nu-1)}}{P^T} & i \in [0, N_C] \end{aligned}$$

where $P^T = \int_0^{N_U} p_{lU}^{\nu/(\nu-1)} dl + \int_0^{N_C} (e p_{lC})^{\nu/(\nu-1)}\, dl$. The demands of a non-traveller in county U are given by

$$\begin{aligned} x_{iU}^S &= \frac{(\gamma q_U \omega)\, p_{iU}^{\frac{1}{\nu-1}}}{P^S} & i \in [0, N_U] \\ x_{iC}^S &= 0 & i \in [0, N_C] \end{aligned}$$

where $P^S = \int_0^{N_U} p_{lU}^{\nu/(\nu-1)} dl$. The Marshallian demands for travellers and non-travellers of county 1 have expressions symmetric to these.

B Technology

A retail producer employs a fixed amount of labor and capital each period to maintain the store. This costs ϕ units of the manufactured good with a fraction, δ, of that fixed cost paid to labor and the remainder to capital. She also uses labor and materials to produce final output, x, using a constant returns to scale technology. The fraction of fraction of variable costs which is paid to labor equals α. With this production specification, nominal payments to labor (payroll) of firm i located in county j in units of the local currency is equal to

$$\alpha(c_j)(x_{ij}) + q_j\delta\phi$$

where c_j is unit variable costs denominated in the local currency. Hence, payroll in units of the manufactured good of firm i located in county j is given by

$$W_j \equiv \alpha(c_j/q_j)(x_{ij}) + \delta\phi. \quad (1)$$

C Free-Entry Equilibrium

The retail production technology is available to two large pools of potential entrants, one pool in each country. The nominal prices of the outside homogeneous good, q_U and q_C, and nominal costs, c_U and c_C, are random variables that are exogenous from the perspective of a single county's retail sector. After observing these variables, potential entrants simultaneously decide whether or not to irreversibly incur the fixed cost ϕ and produce. Those that enter choose their nominal retail prices to maximize profits. In equilibrium, further entry is not profitable in either location. For simplicity, we suppose producers must incur the fixed cost ϕ every period, so a border county's retail-trade sector is characterized by the infinite repetition of this free-entry game[1].

The isoelasticity of consumers' Marshallian demand curves implies that producers' profit maximizing prices will follow the familiar constant percentage mark-up over marginal cost

[1] We make the assumption that each establishment incurs the fixed-cost ϕ each period only for convenience. With minor modifications, our analysis carries through if we instead assume that ϕ is a sunk cost of entry and that incumbent establishments each face a probability δ of having to pay this cost again or exit the industry.

rule:

$$p_{ij} = \frac{c_j}{\nu} \tag{2}$$

Because producers earn zero profits, it must be the case that

$$\left(p_{ij} - c_j\right) x_{ij} - q_j \phi = 0. \tag{3}$$

Substituting (2) into (3) and rearranging yields the equilibrium output of a representative retail producer in county j:

$$x_{ij} = \frac{q_j \phi \nu}{c_j \left(1 - \nu\right)}. \tag{4}$$

Substituting this expression for x_{ij} into (1) gives equilibrium payroll per producer in units of the manufactured good:

$$W_j = \frac{\alpha \phi \nu}{1 - \nu} + \delta \phi. \tag{5}$$

Note that this equation implies that payroll per firm is constant and does not depend on cost or nominal exchange rate shocks. Instead, the industry responds to these disturbances by changing the number of producers on both sides of the border.

To determine the number of retail producers on both sides of the border, we equate the output per producer that is consistent with free-entry from (4) with the output demanded by consumers at the prices given by (2). Let $r \equiv \left(ep_C / p_U\right)$ denote the relative price of retail trade goods in the two countries, which we henceforth refer to as the industry real exchange rate. The equilibrium market clearing conditions for retail trade producers in the two counties are

$$\frac{\left(1 - \lambda\right) S_U}{N_U} + \frac{\lambda \left(S_U + S_C\right)}{N_U + N_C r^{\nu/(\nu-1)}} = \frac{\phi}{\gamma \omega \left(1 - \nu\right)} \tag{6}$$

$$\frac{\left(1 - \lambda\right) S_C}{N_C} + \frac{\lambda \left(S_U + S_C\right)}{N_U r^{\nu/(1-\nu)} + N_C} = \frac{\phi}{\gamma \omega \left(1 - \nu\right)} \tag{7}$$

The first term on the left-hand side of (6) is the quantity demanded from a retail producer in county U by that county's non-travellers, while the second term reflects that producer's demand from both county's travellers.

If there were no cross-border shopping, then $\lambda = 0$ and finding the values of N_U and N_C that satisfy (6) and (7) is straightforward. In the case of interest where $\lambda > 0$, these conditions form a system of two quadratic equations in the variables N_U and N_C. If $r = 1$ then it is straightforward to show that these equations have the same solution as they do when $\lambda = 0$.

$$\bar{N}_U = S_U \frac{\gamma\omega\,(1-\nu)}{\phi}$$
$$\bar{N}_C = S_C \frac{\gamma\omega\,(1-\nu)}{\phi}$$

That is, in the absence of fluctuations in r, the retail trade sector in a border county should be no different from its counterpart in an interior county.

The objective of our analysis is to determine the responses of N_U and N_C to changes in r, which are induced by changes in ec_C/c_U. To do so, we suppose that r is close to one and take a log-linear approximation of (6) and (7) around the point $r = 1$, $\bar{N}_U$, $\bar{N}_C$. Doing so and solving the resulting system of log-linear equations yields

$$\ln\left(N_U/\bar{N}_U\right) = \left[\frac{\nu\lambda}{(1-\nu)(1-\lambda)}\right]\left[\frac{S_C}{S_U+S_C}\right] ln(r) \tag{8}$$

$$\ln\left(N_C/\bar{N}_C\right) = \left[\frac{-\nu\lambda}{(1-\nu)(1-\lambda)}\right]\left[\frac{S_U}{S_U+S_C}\right] ln(r) \tag{9}$$

These equations imply that when a change in ec_C/c_U causes r to fall, the number of producers operating in county U, which is relatively more expensive, falls. Simultaneously, the number of producers in county C, which is relatively cheaper, rises. Recall that (5) gives each producer's payroll, which does not depend on r. Therefore, this model has the stark prediction that all of a retail-sector's response to real exchange rate movements occurs through changes in the number of producers and average producer size is unaffected. If we relaxed the assumption that producers' entry decisions are made after observing r, and replaced it with the assumption that entry decisions must be made prior to realizing r, then the solutions for N_U and N_C are very similar to (8) and (9) with $\mathbf{E}[\ln r]$ taken with respect to the information at the time of entry replacing $\ln r$. In this case, establishments' average payroll is no longer constant but instead varies with $(\ln r - \mathbf{E}[\ln r]) \times [S_C/(S_U + S_C)]$ in county U and with $(\ln r - \mathbf{E}[\ln r]) \times [S_U/(S_U + S_C)]$ in county C. The technical appendix describes this extension of our model in more detail.

Because different border counties have different home and foreign populations, (S_U and S_C), the second term in brackets in equations (8) and (9) will vary across border counties. We refer to this term as the county's *exposure measure.* The exposure measure has the intuitive property that it is increasing in the population of foreign consumers and decreasing in the population of domestic consumers. Hence a heavily populated U.S. county located next to a sparsely populated Canadian area will have a relatively low exposure measure because the presence of foreign consumers has only a small effect on the domestic industry. The converse is true for a lightly populated U.S. county located near a large concentration of Canadians. The measurement of each border county's exposure to cross-border shopping is an important component of our estimation.

II Data and Estimation

The model of retail industry dynamics with cross-border shopping is useful for clarifying the sense in which real exchange rates can be thought of as a demand shock for border counties and for suggesting how to estimate a county's exposure to cross-border shopping. For estimation, we extend the model to account for county-specific and economy-wide disturbances to technology and demand as well as unobserved demand and cost heterogeneity across counties. The resulting empirical model does *not* impose the restriction that only the number of establishments responds to a demand shock. Instead, it allows for many patterns of adjustment. This section extends the model in this way, describes the data we use in its estimation, and summarizes our GMM estimation procedure. We begin with the model's extension.

A The Empirical Model

The variables we observe for each county are the number of establishments operating in an industry and the U.S. dollar value of industry payroll. Gather these into the vector

$$y_t = \begin{bmatrix} \ln N_t & \ln A_t \end{bmatrix}'.$$

Our data set provides annual observations of y_t from 1977 through 1996 for six retail trade industries in every county in the ten continental states that border Canada. In our data, N_t is defined as the number of stores operating at any time during the year, and A_t is the dollar value of total industry payroll for the year.

Define y_{it} to be the value of y_t for county i for a particular industry. Our model implies that the logarithms of establishments and payroll satisfy

$$y_{it} = \bar{y}_i + \psi \left(s_i \times \ln r_{it} \right), \tag{10}$$

where $\overline{y}_i$ is the steady-state value of y_{it}, r_{it} is the relative retail price between county i and its Canadian counterpart,

$$\psi = \begin{bmatrix} \frac{\nu\lambda}{(\nu-1)(1-\lambda)} & 0 \end{bmatrix}', \tag{11}$$

and

$$s_i = \frac{S_{iC}}{S_{iU} + S_{iC}}. \tag{12}$$

Here, the population of U.S. county i is S_{iU}, and the population of its Canadian counterpart is S_{iC}. If county i does not share a border with Canada, then we set $S_{iC} = 0$. Below, we discuss our measures of S_{iC} for counties that do border Canada. We allow the intercept term in (10), $\bar{y}_i$, to vary across counties because it is a function of parameters describing demand and cost that potentially vary across counties, such as ω and ϕ.

The location-specific price data needed to construct r_{it} are not available to us, so we replace r_{it} with its observable national-level counterpart, r_t, which is the industry-level relative price constructed using national price-indices for the U.S. and Canada and the nominal exchange rate. We further discuss our measure of r_t in Section C below. To replace r_{it} with r_t, we suppose that the linear projection of r_{it} on r_t has a slope coefficient of one for each border county. That is

$$\ln r_{it} = a_i + \ln r_t + \zeta_{it}, \tag{13}$$

where

$$\mathbf{E}\left[\zeta_{it} \ln r_t\right] = \mathbf{E}\left[\zeta_{it}\right] = 0,$$

$\{\zeta_{it}\}$ is a covariance-stationary stochastic process that is independent across counties.[2] Using (13) to replace $\ln r_{it}$ in (10) yields

$$y_{it} = \bar{y}_i + \psi s_i a_i + \psi \left(s_i \times \ln r_t\right) + \psi s_i \zeta_{it}.$$

To incorporate movements in y_{it} that are not the consequence exchange-rate fluctuations, we suppose that the demand curves and cost functions in each county are subject to both county-specific and economy-wide disturbances. Towards that end, let ϕ_{it} denote the fixed cost incurred by producers that choose to enter county i at time t and let ω_{it} denote per-capita income in county i at time t. Denote the logarithmic deviations of ϕ_{it} and ω_{it} from their steady state values with f_{it} and z_{it}, and group these together in the vector

$$x_{it} = \begin{bmatrix} f_{it} & z_{it} \end{bmatrix}'.$$

The modification of (10) that allows for fluctuations in x_{it} is

$$y_{it} = \bar{y}_i + \Gamma x_{it} + \psi s_i a_i + \psi \left(s_i \times \ln r_t\right) + \psi s_i \zeta_{it}, \tag{14}$$

where (5) and (6) and (7) imply that

$$\Gamma = \begin{bmatrix} -1 & 1 \\ 1 & 0 \end{bmatrix}.$$

[2] If the assumption that the slope coefficient in (13) equals $b \neq 1$, then our definition of the parameter matrix β in (22) should be multiplied by b. All other aspects of our analysis remain unchanged.

We suppose that x_{it} is the sum of an economy-wide disturbance that is common to all counties and a county-specific disturbance, which follows a stationary first-order autoregression.[3]

$$x_{it} = \bar{x}_t + u_{it} \tag{15}$$

$$\Gamma u_{it} = \Lambda \Gamma u_{it-1} + v_{it} \tag{16}$$

The roots of $|I - \Lambda L|$ lie outside of the unit circle, and

$$E\left[v_{it}\right] = 0$$

$$E\left[v_{it} v_{i\tau}\right] = 0 \text{ if } t \neq \tau.$$

Furthermore, we assume that $\{v_{it}\}_{t=-\infty}^{\infty}$ is independent of $\{v_{jt}\}_{t=1}^{T}$ if $i \neq j$. Our estimation procedure requires no further assumptions on the disturbance process aside from the standard technical regularity conditions for GMM. In particular, the second moments of v_{it} may vary over time and across counties.

If we replace x_{it} in (14) using (15) and multiply both sides of the resulting equation by $(I - \Lambda L)$, we get

$$y_{it} = \alpha_i + \mu_t + \Lambda y_{it-1} + \psi\left(s_i \times r_t\right) - \Lambda \psi\left(s_i \times \ln r_{t-1}\right) + \varepsilon_{it}, \tag{17}$$

where

$$\alpha_i = (I - \Lambda)\left(\bar{y}_i + \psi s_i a_i\right), \tag{18}$$

$$\mu_t = (I - \Lambda L)\,\Gamma \bar{x}_t \tag{19}$$

and

$$\varepsilon_{it} = v_{it} + s_i\,(I - \Lambda L)\,\psi \zeta_{it} \tag{20}$$

This is a panel-data vector autoregression in y_{it} with additional explanatory variables.

[3]Extending the analysis to the case of higher-order autoregressions is straightforward.

To get our final estimating equation, we relax the common factor structure in (17) that imposes non-linear constraints on the model's parameters. Gather r_t and its first lag into the vector

$$e_t = \begin{bmatrix} r_t & r_{t-1} \end{bmatrix}', \tag{21}$$

and define the coefficient matrix

$$\beta' = \begin{bmatrix} \psi & -\Lambda\psi \end{bmatrix}. \tag{22}$$

With this notation, (17) can be written as

$$y_{it} = \alpha_i + \mu_t + \Lambda y_{it-1} + \beta' \left(s_i \times e_t\right) + \varepsilon_{it} \tag{23}$$

This is our final estimating equation. When estimating the parameters of (23), we do *not* impose the non-linear constraints on β and Λ that the model implies. This allows the empirical model to encompass a wide variety of responses of y_{it} to r_t. We now turn to the discussion of the observations of y_{it}, e_t, and s_i we use in our estimation.

B Observations of Retail Trade Industries

Our source of retail trade industry observations is 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. For each retail trade industry, the *CBP* reports each county's total employment, the number of establishments with employees, first quarter payroll, and annual payroll. In addition, the *CBP* reports the total number of establishments falling into several predetermined employment size classes. The establishment counts give the number of establishments that had paid employment at any time during the year, while the employment counts measure employment during a mid-March pay period. Because this data is based on administrative payroll tax records, its quality is very high. Our empirical work uses the establishment count observation for N_{it} and uses annual payroll divided by the establishment count for A_{it}.[4]

To estimate our model, we must determine which counties in our sample are exposed to cross-border shopping and which ones are not. Our discussion above has presumed that only those counties that share a border with Canada are exposed to cross-border shopping. For some retail trade industries, this is clearly not the case. For example, Ford (1992) surveyed Canadian consumers in Toronto, Hamilton, and the Niagara-St. Catherines region regarding their shopping destinations in the U.S. Many consumers reported shopping outside of the New York border counties of Erie and Niagara, particularly if the shopping trips were for durable goods such as furniture and electronics. Conversely, U.S. consumers from counties without a Canadian border can shop in Canada if they are willing to travel. However, Ford's (1992) survey data indicates that purchasers of food and gasoline, the two most frequently purchased items by cross-border shoppers, tended to shop very near the border. For this reason, we restrict our analysis to retail trade industries that Ford's (1992) data and our own experience as cross-border shoppers indicate consumers are unwilling to travel far to purchase. The industries we consider are Food Stores (SIC 54), Gasoline Service Stations (SIC 554), Apparel and Accessory Stores (SIC 56), Eating Places (SIC 5812), and Drinking Places (SIC 5813). Of these industries, Apparel and Accessory Stores is the one most likely to violate our assumption that only stores in border counties are exposed to cross-border shopping. We include it in our analysis simply because Ford (1992) finds that clothing is one of the most frequently purchased items by Canadians in the U.S.

The Census bureau's disclosure policy creates a potential problem with our data. The Census Bureau withholds the employment and payroll information for any county-industry observation where that data may disclose information about any individual producer. The Census Bureau never withholds the establishment counts by size category. There is no

[4] We use annual payroll instead of employment to construct our measure of average establishment size because both it and the establishment count observations reflects the industry's activity over an entire calendar year.

precise rule that the Census uses to determine which observations must be withheld, but these disclosure cases tend to occur in counties with small populations and few establishments. For some counties, the census withholds data in almost every year of our sample period, while for others this only occurs occasionally. To produce a balanced panel of employment and payroll observations across counties, we estimate payroll per establishment for establishments in each size category for all establishments in counties with withheld data using state level data. We replace the withheld payroll observations with their forecast values using our estimates and the published establishment counts. This paper's technical appendix describes this data replacement procedure in greater detail.

The sample of all counties in states that border Canada range from very small counties, such as Divide County, North Dakota to very large, urban counties, such as Erie County, New York. Our model economy describes competition between a large number of producers, so it is unrealistic to expect our model to describe the retail industry dynamics in very small counties. For this reason, we confine our analysis to counties with relatively large numbers of establishments using two selection criteria. First, we consider only counties with populations greater than 20,000 people, as measured in the 1990 decennial census. There are 256 such counties in the ten continental states that border Canada, 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, less than 4% of our county-industry-year observations have imputed payroll data. However, these imputed observations are heavily concentrated in a single industry, Apparel and Accessory Stores. As noted above, disclosure withholding primarily affects counties with few producers, so our resulting sample is of relatively unconcentrated industries in relatively populated counties.

Our county selection criteria produce different samples for each industry we consider. Table 1 provides summary statistics for each industry's sample of counties. Its first column reports the number of counties included in each 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. None of the 256 counties with populations greater than 20,000 had ten or more of their establishment and payroll observations withheld for ten or more years in Food Stores or Gasoline Service Stations. Our disclosure criterion eliminates between 10 and 30 counties from the remaining industries' samples. For Apparel and Accessory Stores one of these eliminated counties is a border county, and in Eating Places and Drinking Places five eliminated counties are border counties. The sample quartiles of average establishment counts indicate the extent to which our selection procedures leave relatively unconcentrated industries. For all industries but Drinking Places, the median county has more than 25 establishments in an average year. For Drinking Places, the median county has approximately 18 establishments. Again 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 12. It appears that our county selection procedure produced a sample of relatively unconcentrated industries.

Our data describe two margins along which an individual industry can vary its activity: the number of establishments and their average payroll. To assess how these margins individually contribute to local retail trade industries' idiosyncratic 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, *across counties*, of these standard deviations for each retail trade industry. In practice, these medians are close to their corresponding means. Relative to many aggregate time series, these median standard deviations are quite high for all of the industries. The lowest are in Eating Places, 0.09 for establishments and 0.10 for average payroll. The remaining industries display somewhat more variance, with Drinking Places displaying the highest standard deviations, 0.17 and 0.22 respectively. In all industries, establishments' median standard deviation is not much lower than that of average payroll, indicating that these industries' structures are far from rigid.

C International Relative Prices

Our measures of international relative prices are based on national price indices from the United States and Canada for specific goods and the exchange rate between the two countries' currencies. For each retail trade industry, we found matching consumer price indices for the goods for sale by that industry from the two countries. Table 3 lists the U.S. and Canadian CPI series used to construct the relative price series for each of the five 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 all between 0.06 and 0.09. The standard deviation of the relative price of Gasoline is much higher than this, 0.21. Most of this variance reflects fluctuations in the years of the Canadian National Energy Policy (NEP). In response to international oil price shocks in the 1970's, the Canadian federal government implemented the NEP which, among other things, imposed import subsidies and export taxes on petroleum products. Thus while gasoline prices rose considerably in the U.S. in response to these shocks, Canadian gasoline prices did not and the relative price of gasoline between the two countries exhibited considerable fluctuation.

Unsurprisingly, the relative price series are all highly persistent, with first order autocorrelations between 0.74 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 Apparel and Accessory Stores, Eating Places, and Drinking Places are all highly correlated with this aggregate real exchange rate. The relative prices of food purchased at stores and gasoline have somewhat lower correlations.

D Measures of Exposure to Cross-Border Shopping

Our model predicts that the elasticity of retail trade activity on the U.S. side of the border with respect to the real exchange rate depends on the share of the border area's consumers that are Canadian, $s_i = S_{iC}/\left(S_{iU} + S_{iC}\right)$. Consumers' strong preferences for product diversity directly produce this result, but it 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 measure S_{iU}, we use data on each county's population in the 1990 decennial census. Measuring S_{iC} is less straightforward, because there is no natural or political geographic partition of Canada that indicates which Canadians are potential cross-border shoppers for which counties. It is possible to measure S_{iC} as the number of Canadians living within a particular distance of county i, however this measure of S_{iC} is unsatisfactory because it does not account for potential geographic obstacles and obstacles to travelling between these Canadians' homes and county i. 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 such 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. Using interview data from border crossing points, Statistics Canada tabulates the number of U.S. and Canadian travellers 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 travellers' identities, so an individual making multiple trips to or from Canada in a year will contribute to the count of travellers on each trip. This data is available from 1990 through 1999. We average the data across these years to measure the average number of U.S. and Canadian travellers for county i, which we denote with T_{iU} and T_{iC}.

Our model implies that the number of Canadians crossing the border on one-day trips is

λS_{iC}, but our model implies nothing about the frequency of cross-border shopping during one year for a travelling consumer. To construct a measure of S_{iC} based on T_{iC}, we assume that the average number of trips taken by a travelling consumer, θ, is constant across locations. Given prespecified values of λ and θ, we can then measure S_{iC} with $T_{iC}/(\lambda\theta)$. The resulting measure of county i's exposure to cross-border shopping is

$$s_i = \frac{T_{iC}}{\lambda\theta S_{iU} + T_{iC}}. \tag{24}$$

As this expression for s_i makes clear, the problem of choosing $\lambda\theta$ is one of expressing county i's population in units of travellers. For our baseline measure of s_i, we assume that all U.S. travellers entering Canada for one-day trips from county i are residents in that county, and use the average of T_{iU}/S_{iU} across border counties to measure $\lambda\theta$. The resulting value of $\lambda\theta$ is 7.49.[5]

In general, our empirical results are not very sensitive to our choice of $\lambda\theta$, but we wish to examine the implications of using other exposure measures. One alternative measure of s_i dispenses with population data altogether by using $T_{iU} = \lambda\theta S_{iU}$ to rewrite s_i as $T_{iC}/(T_{iU} + T_{iC})$. We refer to this as our trips-based exposure measure. The other measure of s_i we consider is a naive one based only on population data. For this, we measure S_{iC} with the number of Canadians living within a radius of fifty miles of county i's central point, as defined by the U.S. Bureau of the Census. The Canadian population data comes from that country's 1991 census. We refer to this measure of s_i as our population-based exposure measure.

The first two columns of Table 5 report the sample means and standard deviations of the three measures of s_i that we use in estimation across the 19 border counties in our sample. Its

[5]We have also considered calibrated values of λ and θ based on Ford's (1992) survey data of Canadian consumers' cross-border shopping habits. The calibrated values of λ and θ are 0.71 and 25, so that $\lambda\theta = 17.75$. This is far different from our baseline measure of $\lambda\theta$, but the resulting exposure measure is almost perfectly correlated with our baseline measure.The cross-sectional correlation coefficient between the two measures equals 0.98. The empirical results we obtain with this alternative measure of s_i are nearly identical to our baseline results, so we do not report them.

final column reports the sample correlation of the alternative measures of s_i with our baseline measure. By construction, all of our exposure measures are between zero and one. Our baseline measure's mean is 0.60, which is between the other two measures' means, 0.57 and 0.70. These means are close to their (unreported) corresponding median values. The baseline measure's standard deviation is 0.27, which is very close to the standard deviation of the population-based measure. The standard deviation of the trips-based measure is somewhat lower than this, 0.14. Both alternative exposure measures have positive correlations with the baseline measure, 0.59 and 0.57.

E GMM Estimation

The estimation of panel-data vector autoregressions similar to (23) without the explanatory variables $s_i \times e_t$ is a well-studied problem. To estimate (23), 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. A novel complication that arises in our analysis is the presence of the measurement error $\psi s_i \zeta_{it}$ due to the replacement of $\ln r_{it}$ with $\ln r_t$. We have placed no restrictions on the serial correlation properties of ζ_{it}, so this measurement error's presence invalidates Blundell and Bond's (1998) moment conditions when applied to observations from border counties. However, s_i equals zero for the majority of our sample counties, so their moment conditions remain valid if we impose them only on observations from counties without a Canadian border. The appropriately modified moment conditions which we use in our GMM estimator are

$$\mathbf{E}\left[I\left\{s_i = 0\right\} \Delta\varepsilon_{it} y_{it-s}\right] = 0,\ t = 3, \ldots, T,\ t > s \geq 2, \tag{25}$$

$$\mathbf{E}\left[I\left\{s_i = 0\right\} \left(\alpha_i + \varepsilon_{it}\right) \Delta y_{it-1}\right] = 0,\ t = 2, \ldots, T. \tag{26}$$

We can impose additional restrictions by assuming that $\bar{y}_i$ has a zero mean, which is merely a normalization given the presence of μ_t in (23). This constrains the error term's level to have a zero mean, conditional upon not being a border county.

$$\mathbf{E}\left[I\left\{s_i = 0\right\}\left(\alpha_i + \varepsilon_{it}\right)\right] = 0,\ t = 2, \ldots, T \tag{27}$$

Taken together, the moment conditions in (25), (26), and (27) are more than sufficient for identifying and estimating the 4 autoregressive parameters and the $2\,(T-1)$ year-specific intercepts for $T = 20$. However, these conditions clearly leave β unidentified. Because we have assumed that both v_{it} and ζ_{it} have zero unconditional means, it must be the case that

$$\mathbf{E}\left[\Delta\varepsilon_{it} s_i\right] = 0,\ t = 3, \ldots, T. \tag{28}$$

These $2\,(T-2)$ moment conditions allow us to estimate β along with the model's other parameters.[6]

The GMM estimator we use is based on the moment conditions in (25), (26), (27), and (28).[7] 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 (27), and (28). We use a one-step estimator rather than an asymptotically efficient two-step estimator because the number of moment conditions we use exceeds the number of cross-sectional observations in our sample. This implies that the usual consistent estimator of the optimal weighting matrix's inverse is singular, and so non-invertible. Existing Monte Carlo results for panel data VAR estimation typically consider estimation of a single-equation with a small value of T, so that this issue does not arise. For this reason, we investigated the estimator's properties in a sample of the size we use in a Monte Carlo experiment. We found that the estimator displayed very little bias and that the asymptotic distributions of t-statistics and χ^2 tests provided reliable guides for inference. The technical

[6]Note that because we have allowed a_i to potentially have a non-zero mean in (13), we cannot claim that $\mathbf{E}\left[\alpha_i s_i\right] = 0$, which would be necessary for adding the moment condition $\mathbf{E}\left[\left(\alpha_i + \varepsilon_{i2}\right) s_i\right] = 0$.

[7]For Drinking Places, relative price data is not available until the third year of our sample. This slightly changes the set of available moment conditions. The technical appendix describes these changes in more detail.

appendix describes the estimation procedure and this Monte Carlo experiment in much more detail.

III Estimation Results

Our baseline empirical analysis of retail trade industry dynamics for the five industries we consider produces estimates of ten autoregressive equations' parameters for each industry. 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 and summarize our estimates of the autoregressive coefficients. Table 6 presents the GMM estimates of the coefficients in (23) for Food Stores. 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. 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. The relevant 5% critical value for this test is 5.99.

The estimates in Table 6 indicate that fluctuations in the U.S.-Canada relative price of food purchased in stores significantly impacts the Food Stores industries in counties that border Canada. In the establishments equation, $\widehat{\beta}_0$ equals -0.101. This has the sign predicted by our model and is statistically significant at the 5% level. The corresponding estimate $\widehat{\beta}_1$ is very close to zero and is not significant. The Wald exclusion test statistic for the establishments equation equals 10.70, which far exceeds the 5% critical value. The point estimates of β_0 and β_1 for the average payroll equation are similar to those from the establishment equation, but they are not statistically significant at conventional levels. The Wald exclusion test statistic for this equation is 4.37, and its probability value is 0.11.

Recall that the instantaneous-entry model presented in Section I predicts that the expenditure shifting effects of an exchange-rate appreciation decrease the number of stores in a border county but should leave their average sizes unchanged. These results are statistically consistent with this view. However, the estimated magnitude of average payroll's response to the current relative price and the relatively high probability value for that equation's exclusion test suggest the more conservative conclusion that the Food Stores industry responds to relative price movements by changing both the number of stores and their average size. At our point estimates, about 2/5 of the reduction in total payroll comes from reducing the number of stores, and the remaining 3/5 comes from reducing stores' average payroll.

Finally, note that the autoregressive parameter estimates indicate that both the number of establishments in a county and their average payrolls are persistent time series, with establishments displaying somewhat more persistence than payroll. The diagonal elements of Λ are both positive and significant, while its off-diagonal elements are much smaller. These parameters are tightly estimated. The other four industries' estimated autoregressive coefficients are very similar to Food Stores. Finally, note that the estimates of Λ and β together do not satisfy the common-factor restriction of the instantaneous entry model. For this reason, we investigate richer parameterizations of our model below.

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 current relative price. Figure 2 displays these impulse-response functions over a ten-year horizon for Food Stores. Its top panel plots the response of $\ln N_{it}$, whereas its bottom panel plots the response 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 of $\ln r_t$ equals its sample value of 0.075. 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 pointwise 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 $\ln r_t$.

Both variables' impulse response functions display considerable persistence. This reflects both the persistence in $\ln r_t$ and the large deviation of the estimated model from one with a common-factor representation. As the parameter estimates imply, the number of establishments falls during the period of the shock and then continues to fall. The instantaneous impact is about four tenths of one percent, and the impact at a horizon of five years is slightly less than two percent. As the test statistics suggest, the pointwise confidence intervals for these responses never include zero. For establishments' average payroll, the instantaneous impact of the shock is about six tenths of one percent, and its impact after five years is around one percent. The impact of the shock on average payroll after one year is negative and statistically significant. The relevant pointwise confidence intervals for the shock's impact at other horizons all include zero. Thus, over short horizons of a year or less, it appears that both the number of establishments and their average sizes change following a shock to the relative price. Over the long-run, however, the industry accommodates the shock primarily by through entry and exit.

For the sake of brevity, we report only the estimates of β and the values of the exclusion tests for the remaining industries. Table 7 reports the estimates of β and the exclusion tests for all five of the industries we consider, and Figures 3–6 graph the impulse response functions for the remaining industries. In those exercises, we assumed that the relevant industry-specific real exchange rate followed a first-order autoregression parameterized to match the reported statistics in Table 4 and simulated the industry's response to a one-standard deviation positive shock.

In Gasoline Service Stations the lagged gasoline-based real exchange rate has a negative 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 only after one year. Just as with Food Stores, the Wald test rejects the exclusion restriction for establishments, but not for average payroll. However, the Wald test statistic for the average payroll equation is not very small, and the point estimates in that equation are larger than those for the establishments equation, so it seems premature to conclude that Gasoline Service Stations' average payroll does not respond to a real exchange rate shock. 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 payroll falls more than 1%, while the number of establishments rises very slightly. The reduction in average payroll persists, while the number of establishments falls. After five years, the number of establishments has declined approximately 2%. These results resemble the textbook description of the transition from the short-run to long-run response to a demand shock. Initially the number of establishments is held fixed and the industry responds to a negative demand shock by decreasing their average size. If the demand shock persists as it does in our simulation, then the number of establishments decreases, either by reducing entry or increasing exit.

In Apparel and Accessory Stores, the estimated coefficients on the contemporaneous real exchange rate are large and negative in both equations, but they are not statistically significant. The Wald tests also offer no evidence that these coefficients are statistically significant. These estimates' standard errors are somewhat larger than those for Food Stores and Gasoline Service Stations, so it is difficult to determine whether these estimates indicate the absence of any effect or simply an absence of statistical power to reject the null hypothesis. As noted previously, this industry may be one in which a significant portion of cross-border shopping occurs in interior counties, particularly at discount outlet centers. This may provide a partial explanation for our findings of little effect of relative price movements in this industry.

The results for Eating Places are very similar to those for Food Stores, but the statistical strength of the results is somewhat weaker. In the establishments equation, neither of the coefficients are individually significant, and the Wald test's probability value is 0.072. The corresponding Wald test statistic for the average payroll equation is close to its median value under the null hypothesis. The impulse response functions for Eating Places show that fluctuations in the number of establishments play a central role in the responses to real exchange rate disturbances. The contemporaneous responses of establishments and average payroll to the exchange rate shock are nearly equal. After impact, average payroll quickly returns to its pre-shock value, while the number of establishments remains persistently low. Of the five industries we consider, Eating Places most resembles the stark behavior of our model with immediate entry.

Our final industry is Drinking Places. These estimates are quite different from those of the other industries. The estimated coefficients for the establishments equation are close to the corresponding estimates for Eating Places, but their standard errors are much larger. In contrast, the coefficient on the contemporaneous real exchange rate in the average payroll equation is -0.775. This is by far the largest absolute value of any of our estimated coefficients, and it is statistically significant at the 5% level. The Wald exclusion test also indicates that real exchange rate fluctuations have a strong impact on the average payrolls of Drinking Places in border counties. The impulse response functions reflect the importance of establishments' average payroll in responding to exchange-rate shocks. In the year of the shock, the number of establishments falls slightly and remains low. However, the response of establishments to the shock is not statistically significant at any horizon. In contrast, Drinking Places' average payroll falls nearly 4% in the period of the shock, and it only slowly rises back to its pre shock level.

To determine the robustness of our conclusions to our choice of exposure measure, we have re-estimated (23) using both of the alternative exposure measures discussed above. Table 8 reports estimated coefficients and exclusion tests for our estimates that use the trips-based exposure measure, and Table 9 reports the analogous results for the population-based exposure measure. The results using the trips-based exposure measure are very similar to those from our baseline exposure measure, although the standard errors tend to be larger and the statistical significance of the results tends to be weaker. The estimates which use the population-based exposure measure are quite different from those that use either of the other two exposure measures. The only Wald tests which reject the null hypothesis of exclusion are those for establishments in Food Stores and for average payroll in Gasoline Service Stations. With the notable exception of this latter equation, the point estimates for the remaining equations are all very different from those of our baseline estimation. We believe that these differences reflect the inaccuracy of measuring the number of Canadians that are potential customers for a U.S. county's retail trade industry with the number of Canadians living within fifty miles of the county's center. Because the decision to enter a county reveals that one is willing to cross-border shop, we believe that our baseline and trips-based exposure measures more accurately measure the size of each U.S. market's relevant Canadian customer base. This accuracy is reflected in our baseline estimates' greater magnitude and statistical significance.

IV Related Literature

Much of the theory of industrial organization assumes that entry responds to persistent shocks only in the long run. The implications of sluggish entry are well-known: When entry takes time, 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. Using a cross-section of rural retail and service industries, Bresnahan and Reiss (1991) examined a long-run free-entry model's prediction that the number of producers is proportional to market size. That analysis reveals no evidence of barriers that permanently exclude potential entrants, but it says nothing about the speed with which entry can respond to demand shocks.

Our analysis suggests that entry into retail trade industries responds very quickly to demand shocks. Only in Drinking Places, where alcohol licensing restrictions may indeed present a barrier to entry, does the real exchange rate affect industry activity without changing the number of establishments. The number of establishments responds contemporaneously to a real exchange rate shock in Food Stores and in Eating Places, and it responds after one year in Gasoline Service Stations. In these industries, either potential entrants, potential exiters, or both must respond very quickly to demand shocks. In our model economy and its extension discussed in Footnote 1 that incorporates sunk costs of entry and long-lived producers, all fluctuations in the number of producers reflect the decisions of potential entrants. 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.[8] Thus, our results strongly suggest that potential entrants can affect their decisions very quickly following demand shocks.

Although real exchange rate fluctuations are substantial, their nonfinancial effects have been difficult to detect. Baxter and Stockman (1989) examine data from several OECD countries and find that the characteristics of a country's business cycle fluctuations do not depend on whether or not it has adopted a fixed exchange rate. Conversely, Frankel and Rose (1995) survey a large literature that concludes that macroeconomic quantities do not contribute to better forecasts of real exchange rates. This paper belongs to a literature which attempts to identify the real effects of exchange rate fluctuations. For example, Gourinchas (1998) regresses job creation and destruction rates from export-intensive and import-intensive manufacturing industries on real exchange rates. That estimation shows little effect of exchange rates on the level an industry's activity, but it does indicate that real appreciations increase the reallocation of employment across an industry's plants. Several differences between this estimation and ours can account for the differences in our results. First and foremost, our pa-

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

per and his examine different industries with different production technologies. Second, the econometric strategies for accounting for the direct effects of unobserved structural shocks on preferences and technology are substantially different. Gourinchas includes a vector of macroeconomic aggregates in his regression equations to proxy for unobserved aggregate shocks, while we estimate a set of time-dummies using data from counties that do not border Canada. Gourinchas' approach requires that some exchange rate variation is exogenous, in the sense that it does not reflect changes to either tastes or technology. Our strategy requires no such assumption. Finally, Gourinchas' estimation pools data from several manufacturing industries. His estimation uses cross-industry variation in the industry-specific real exchange rate fluctuations to identify the parameters of interest. Our estimation pools data from the same retail trade industry in different counties and uses cross-sectional variation in the counties' locations relative to the U.S.-Canada border to identify the expenditure-shifting effects of exchange rates.

The deviations from PPP that induce cross-border shopping form the central puzzle in international macroeconomics. Another implication of our empirical results is that they may provide insight into the source of these international relative price differences. One candidate explanation for persistent deviations from PPP is nominal stickiness of producers' prices denominated in the currency of the consumer, as in Devereux and Engel (2000). Our model assumed that all retailers prices are perfectly flexible and that deviations from PPP reflect differences in U.S. and Canadian retailers' marginal costs. When they are considered more generally, our empirical results reinforce the assumption of perfectly flexible retail prices. If we assume that the time required to plan and build a new store is no less than the time horizon over which retailers' prices are fixed, then our estimates of net entry's response place a bound on the duration of retailers' nominal price stickiness.[9] In Food Stores and Eating Places, the results suggest that the impact of real exchange rate appreciations on net entry is immediate,

[9] Although we find this assumption to be a reasonable description of retail trade technology, it does rule out the contestability of retail markets, as that term is defined by Baumol, Panzar, and Willig (1982).

indicating that nominal price stickiness and menu costs play little role in generating real price differences between U.S. and Canadian grocery stores and restaurants. In Gasoline Service Stations, where the presence of nominal retail price stickiness is particularly implausible, there is a significant response of net entry after one year. Our results suggest that for these industries costs of changing *retail* prices play little role in generating deviations from PPP. In the absence of regulation, retailers would exploit differences in the two countries' wholesale prices, eliminating the need of consumers in border areas to do so themselves. Therefore, the observed deviations from PPP must arise from either time-varying retail markups,.differences in U.S. and Canadian retailers' costs of producing retail services, as in Burstein, Neves, and Rebelo (2000), or from wholesale price differences, like those for gasoline the early 1980's, due to regulation and taxation.

V Conclusion

The persistent deviations from PPP that our empirical work uses imply that residents of border counties face different prices than those who do not live on the border. Our empirical work used these deviations and the resulting expenditure-shifting by border residents to identify the effects of real exchange rates on border counties' retail trade industries. Although this paper has focused on producers' behavior, our results suggest that it is possible to also learn about consumers' behavior by comparing those who live on the U.S.-Canada border with those who do not. For example, with sticky nominal wages, the real wages of those living on an international border will differ from those who do not, potentially allowing us to estimate the wage elasticity of labor supply. There is clearly more work to be done examining these price differences and their implications.

Our results document how five retail industries respond to a particular demand shock that arises from real exchange rate fluctuations. In three of the five industries we consider, the response of net entry to this demand shock is statistically and economically significant.

Our interest in these results arises from an assumption that these retail trade industries respond similarly to other transitory demand shocks, such as large local income expansions and contractions. Verifying this assumption directly is our agenda for future research. The County Business Patterns data we used is available for all U.S. counties between 1977 and 1996. We believe that the modelling and estimation techniques developed for this paper can be extended and fruitfully applied to this data more generally to measure, for example, retail industry responses to local government spending shocks and the comovement of city and suburban retail trade industries. This paper's results give us reason to believe that the net entry of establishments also plays an important role in these fluctuations.

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Table 1: Quartiles from Sample Counties of Average Establishment Counts

Industry	Counties$^{(i)}$	First Quartile$^{(ii)}$	Median$^{(ii)}$	Third Quartile$^{(ii)}$
Food Stores	256	26.7	45.0	93.3
Gasoline Service Stations	256	18.9	28.8	51.2
Apparel and Accessory Stores	228	16.2	28.2	68.5
Eating Places	245	39.1	74.2	145.7
Drinking Places	235	11.8	18.6	38.5

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[1]

Industry	Establishments	Average Payroll
Food Stores	0.11	0.14
Gasoline Service Stations	0.13	0.16
Apparel and Accessory Stores	0.16	0.18
Eating Places	0.09	0.10
Drinking Places	0.17	0.22

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 that regression was tabulated for each county. The values reported in the table are the medians, across counties, of these statistics. See the text for further details.

Table 3: Consumer Price Index Sources for Relative Price Series

Industry	U.S. CPI	Canadian CPI
Food Stores	Food at Home	Food Purchased from Stores
Gasoline Service Stations	Gasoline	Gasoline
Apparel and Accessory Stores	Apparel and Upkeep	Clothing and Footwear
Eating Places	Food Away from Home	Food Purchased from Restaurants
Drinking Places	Alcoholic Beverages Away from Home	Served Alcoholic Beverages

Notes: For each industry, the column headed U.S. CPI reports the name of the 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

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
Apparel and Accessory Stores	0.064	0.74	0.96
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: Summary Statistics for Cross-Border Shopping Exposure Measures

Exposure Measure[i]	Mean[ii]	Standard Deviation[ii]	Correlation with Baseline Measure[ii]
Baseline, $s_i = T_{i1}/\left(7.49 \times S_{i0} + T_{i1}\right)$	0.60	0.27	1.00
Trips-Based Measure, $s_i = T_{i1}/\left(T_{i0} + T_{i1}\right)$	0.70	0.14	0.59
Population-Based Measure, $s_i = S_{i1}/\left(S_{i0} + S_{i1}\right)$	0.57	0.27	0.57

Note: (i) In the expressions under each entry, S_{i0} is county i's population, from the 1990 census, S_{i1} is the Canadian population within 50 miles of county i's central point, from the 1991 Canadian census, T_{i0} and T_{i1} are the average annual numbers of U.S. and Canadian travellers crossing into Canada from county i while taking cross-border trips lasting a day or less. (ii) All sample statistics are calculated across the 19 border counties in our sample. See the text for further details.

Table 6: Estimates for SIC 54, Food Stores[(i),(ii)]

	Dependent Variable	
	Establishments	Average Payroll
Lagged Establishments,	0.957^{***}	-0.017
$\ln N_{it-1}$	(0.011)	(0.018)
Lagged Average Payroll,	0.096^{***}	0.681^{***}
$\ln W_{it-1}$	(0.021)	(0.050)
Current Real Exchange Rate,	-0.101^{**}	-0.155
$s_i \times \ln r_t$	(0.049)	(0.111)
Lagged Real Exchange Rate,	-0.001	-0.043
$s_i \times \ln r_{t-1}$	(0.070)	(0.139)
Exclusion Test for	10.70	4.37
Real Exchange Rate[(iii)]	(0.005)	(0.112)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$, $**$, and $***$ indicate that the estimate is statistically significantly 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 7: Estimation Results using Baseline Exposure Measure(i)(ii)

	Establishments			Average Payroll		
Industry	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test(iii)	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test(iii)
Food Stores	-0.101^{**}	-0.001	10.70	-0.155	-0.043	4.37
	(0.049)	(0.070)	(0.005)	(0.111)	(0.139)	(0.112)
Gasoline Service	0.033	-0.091^{*}	7.73	-0.121	0.029	2.89
Stations	(0.044)	(0.047)	(0.021)	(0.089)	(0.058)	(0.235)
Apparel and	-0.147	0.065	2.23	-0.215	0.050	0.93
Accessory Stores	(0.099)	(0.106)	(0.328)	(0.237)	(0.260)	(0.629)
Eating Places	-0.135	0.067	5.27	-0.117	0.098	1.28
	(0.086)	(0.079)	(0.072)	(0.110)	(0.098)	(0.529)
Drinking Places	-0.152	0.098	1.32	-0.775^{**}	0.322	6.68
	(0.143)	(0.138)	(0.517)	(0.340)	(0.275)	(0.035)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$ and $**$ indicate that the estimate is statistically significantly different from zero at the 10% and 5% 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: Estimation Results using Trips-Based Exposure Measure[(i)(ii)]

	Establishments			Average Payroll		
Industry	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test[(iii)]	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test[(iii)]
Food Stores	-0.097	-0.001	5.34	-0.201	-0.016	5.97
	(0.068)	(0.082)	(0.069)	(0.128)	(0.155)	(0.051)
Gasoline Service	0.042	-0.095^{*}	5.57	-0.121	0.044	1.81
Stations	(0.047)	(0.049)	(0.062)	(0.094)	(0.059)	(0.404)
Apparel and	-0.090	0.006	0.96	-0.235	0.008	1.02
Accessory Stores	(0.121)	(0.138)	(0.619)	(0.245)	(0.273)	(0.599)
Eating Places	-0.133	0.061	3.69	-0.106	0.113	1.12
	(0.119)	(0.108)	(0.158)	(0.128)	(0.108)	(0.571)
Drinking Places	-0.192	0.144	1.11	-0.981^{***}	0.426	9.09
	(0.197)	(0.193)	(0.575)	(0.356)	(0.289)	(0.011)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$ and $***$ indicate that the estimate is statistically significantly different from zero at the 10% 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 9: Estimation Results using Population-Based Exposure Measure[(i)(ii)]

	Establishments			Average Payroll		
Industry	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test[(iii)]	$s_i \times \ln r_t$	$s_i \times \ln r_{t-1}$	χ^2 Test[(iii)]
Food Stores	−0.003	−0.112	8.80	−0.123	0.110	1.45
	(0.059)	(0.084)	(0.012)	(0.104)	(0.129)	(0.485)
Gasoline Service	−0.026	−0.010	3.56	−0.134**	0.014	9.21
Stations	(0.034)	(0.042)	(0.169)	(0.062)	(0.054)	(0.010)
Apparel and	−0.133	0.000	2.85	−0.186	0.061	1.12
Accessory Stores	(0.110)	(0.119)	(0.240)	(0.178)	(0.174)	(0.571)
Eating Places	−0.050	0.006	3.67	0.062	−0.080	0.73
	(0.075)	(0.073)	(0.160)	(0.080)	(0.095)	(0.693)
Drinking Places	0.002	−0.092	3.43	−0.340	0.213	2.35
	(0.097)	(0.103)	(0.180)	(0.229)	(0.204)	(0.308)

Notes: (i) Heteroskedasticity-consistent standard errors appear in parentheses below each coefficient estimate. (ii) The superscripts $*$ and $**$ indicate that the estimate is statistically significantly different from zero at the 10% and 5% 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

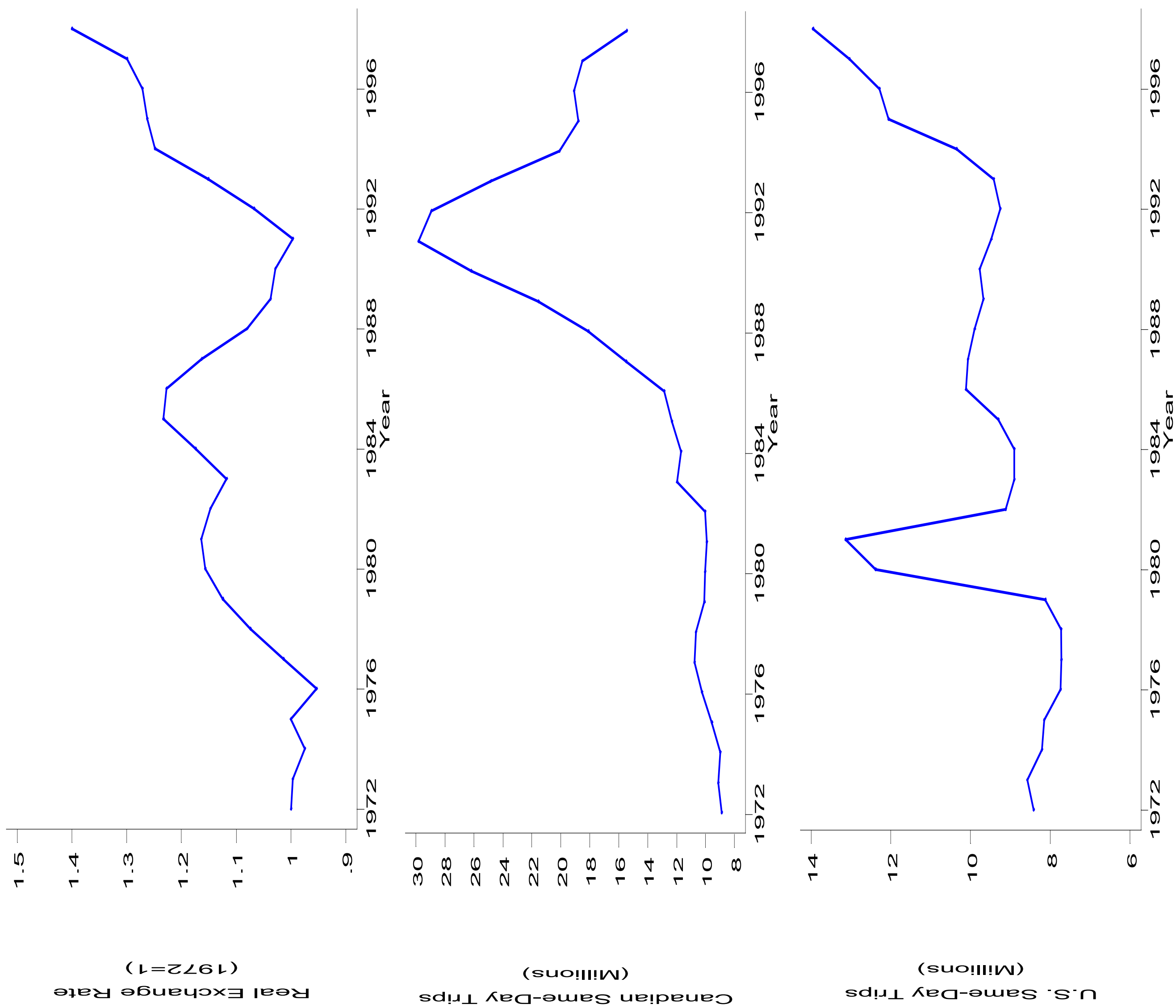

Figure 2: Impulse Response Functions for SIC 54, Food Stores

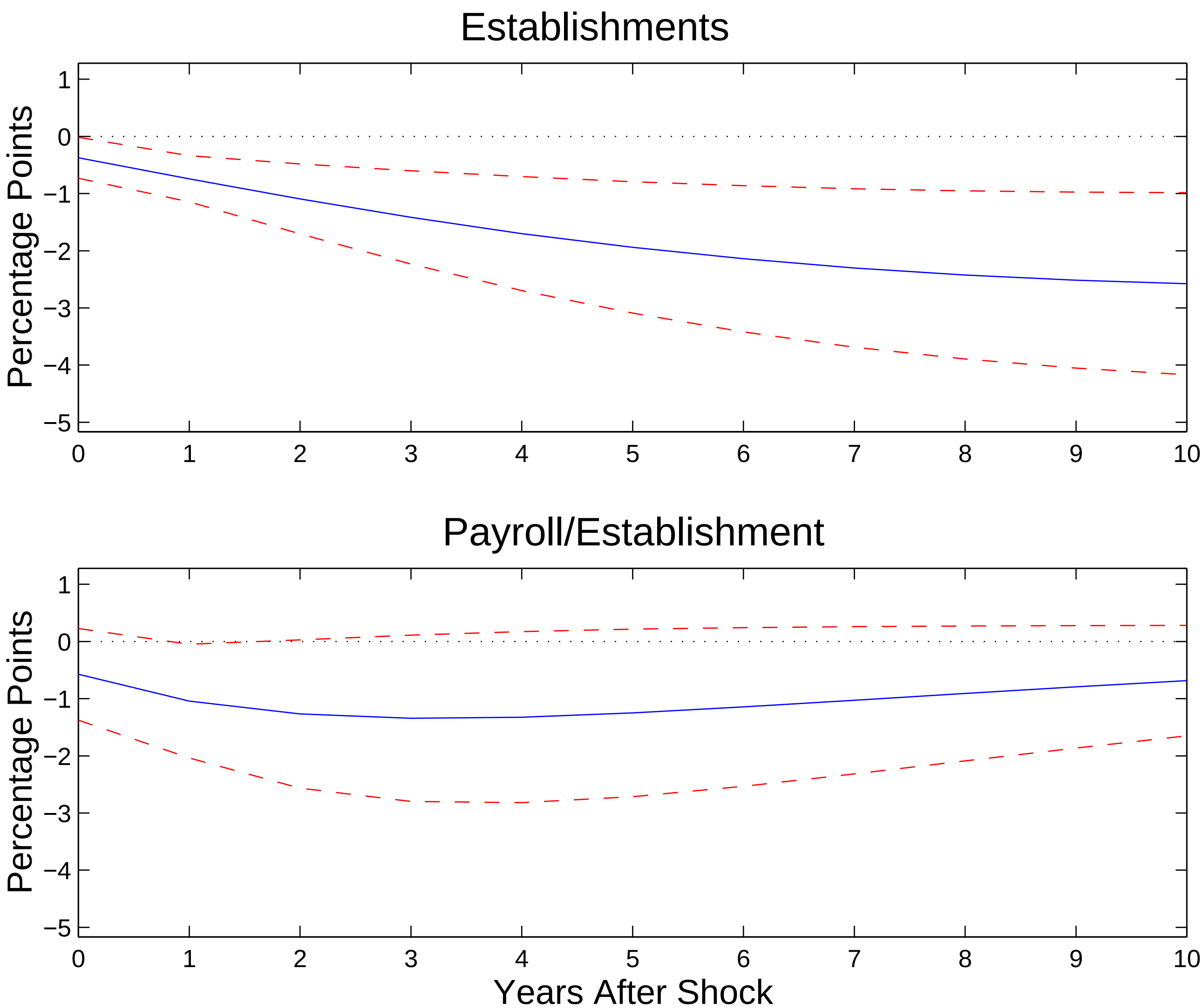

Figure 3: Impulse-Response Functions for SIC 5540, Gasoline Service Stations

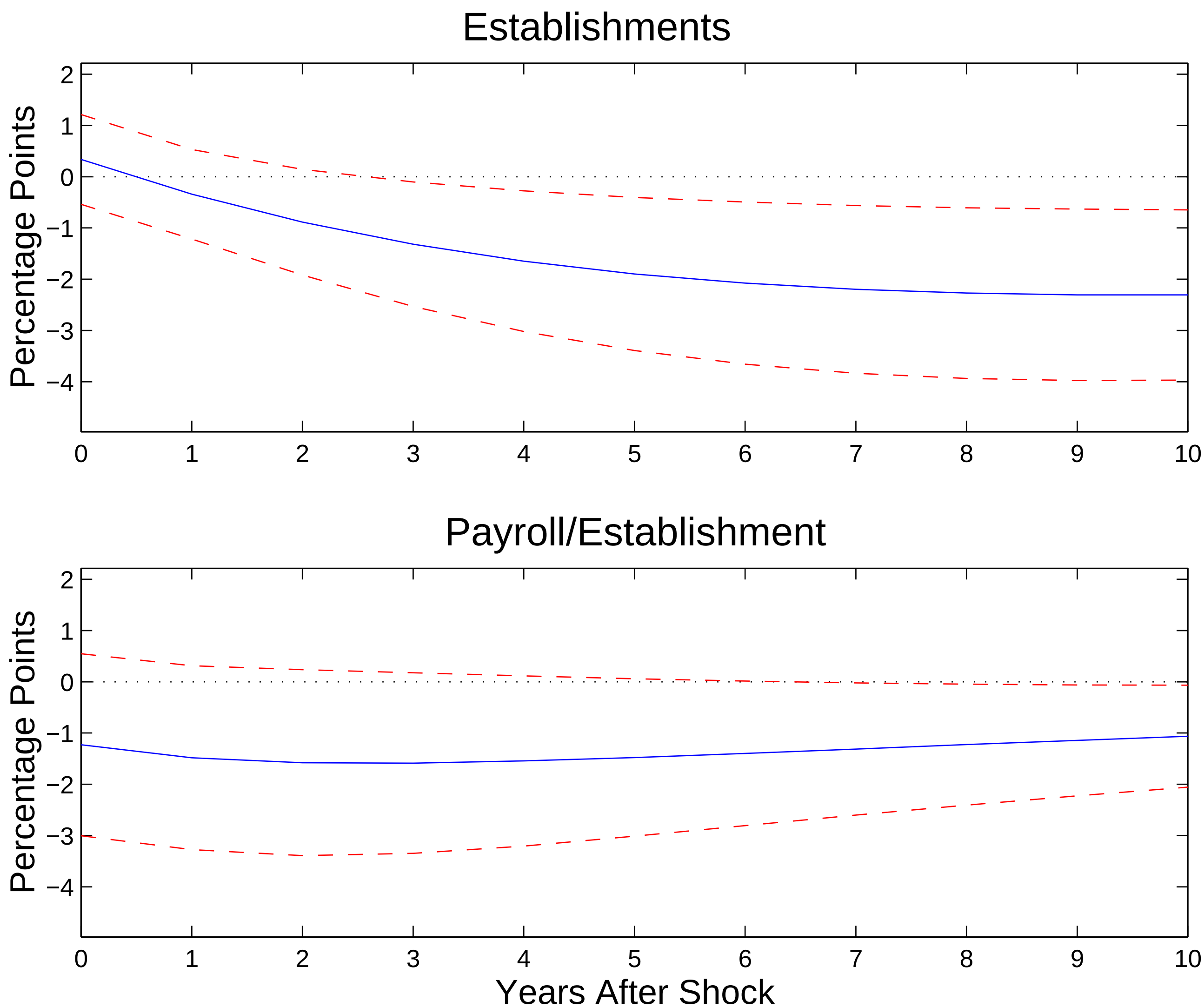

Figure 4: Impulse-Response Functions for SIC 56, Apparel and Accessory Stores

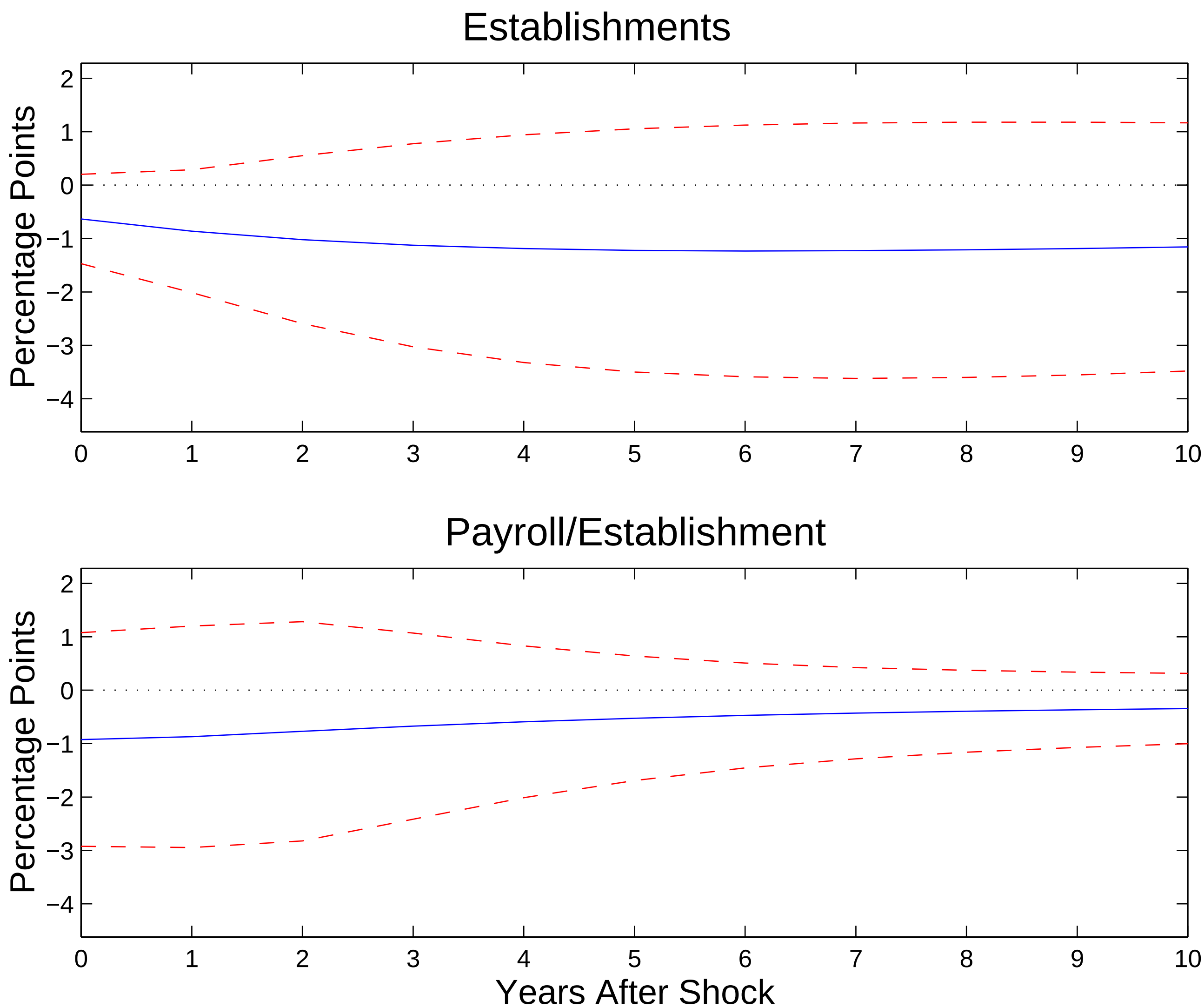

Figure 5: Impulse-Response Functions for SIC 5812, Eating Places

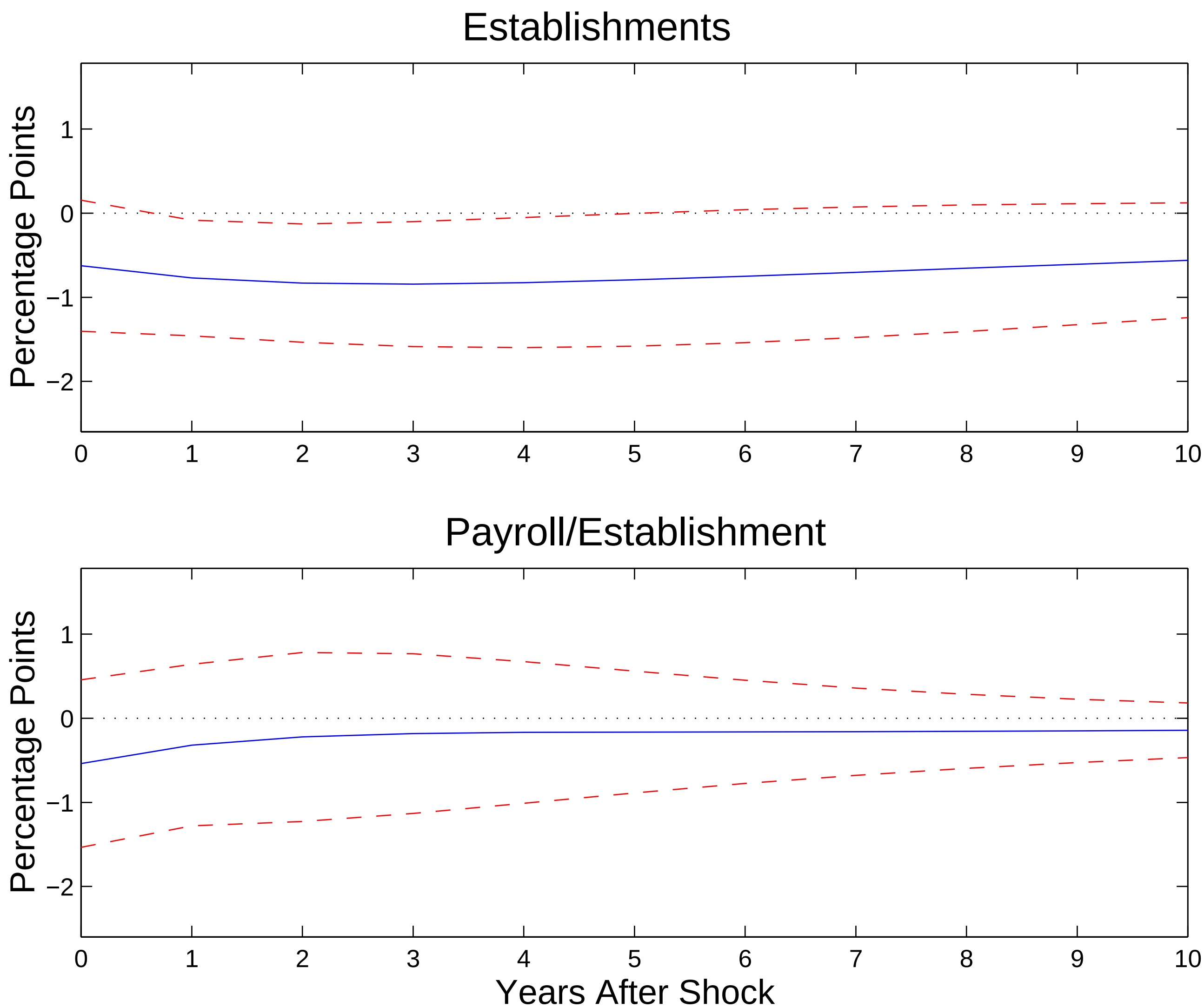

Figure 6: Impulse-Response Functions for SIC 5813, Drinking Places