

## **The Effects of Wal-Mart on Local Labor Markets**

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Abstract: We estimate the effects of Wal-Mart stores on county-level retail employment and earnings, accounting for endogeneity of the location and timing of Wal-Mart openings that most likely biases the evidence against finding adverse effects of Wal-Mart stores. We address the endogeneity problem using a natural instrumental variable that arises from the geographic and time pattern of the opening of Wal-Mart stores, which slowly spread out from the first stores in Arkansas. The employment results indicate that a Wal-Mart store opening reduces county-level retail employment by about 180 workers, implying that each Wal-Mart worker replaces approximately 1.5 retail workers. This represents a 3.2 percent reduction in average retail employment. The payroll results indicate that Wal-Mart store openings lead to declines in county-level retail earnings of about \$2.3 to \$2.8 million, or 2.8 percent.

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## I. Introduction

Wal-Mart is more than just another large company. It is the largest corporation in the world, with total revenues of \$285 billion in 2005. It employs over 1.2 million workers in the United States, at about 3,600 stores.<sup>1</sup> To put this in perspective, the Wal-Mart workforce represents just under one percent of total employment in the United States, and just under ten percent of retail employment. It exceeds the number of high school teachers or middle school teachers, and is just under the size of the elementary school teacher workforce. Wal-Mart is reported to be the nation's largest grocer, with a 19 percent market share, and its third-largest pharmacy, with a 16 percent market share (Bianco and Zellner, 2003).

During the past two decades, as Wal-Mart sharply expanded its number of stores in the United States, it increasingly encountered resistance from local communities. Opponents of Wal-Mart have tried to block its entry on many grounds, including the prevention of urban sprawl, preservation of historical culture, protection of the environment and "main-street" merchants, and avoidance of road congestion.<sup>2</sup> Yet two of the most commonly-heard criticisms are that Wal-Mart eliminates more retail jobs than it creates for a community, and that it results in lower wages, especially in retail.<sup>3</sup> Wal-Mart executives dispute these claims, especially with regard to employment. For example: its Vice President Bob McAdam has argued that there are many locations where Wal-Mart creates jobs in other businesses in addition to what Wal-Mart itself offers (PBS, 2004); the Wal-Mart web-site Walmartfacts.com trumpets the positive effects of Wal-Mart stores on retail jobs in the communities where stores open;<sup>4</sup> and an advertisement run in the *USA Today*, *The Wall Street Journal*, and *The New York Times* on January 14, 2005, displayed an open letter from Lee Scott, Wal-Mart President and CEO, stating "This year, we plan to create more than 100,000 new jobs in the United States."<sup>5</sup> Of course Wal-Mart offers other potential benefits in the form of lower prices for consumers (Basker, 2005a; Hausman and Leibtag, 2004).

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<sup>1</sup> See <http://www.walmartfacts.com/newsdesk/wal-mart-fact-sheets.aspx#a125> (as of September 8, 2005).

<sup>2</sup> See, for example, Bowermaster (1989), Rimer (1993), Nieves (1995), Kaufman (1999), Ingold (2004), and Jacobs (2004).

<sup>3</sup> See, for example, Quinn (2000), Norman (2004), and Wal-Mart Watch (2005).

<sup>4</sup> See <http://www.walmartfacts.com/newsdesk/wal-mart-fact-sheets.aspx> (as of December 15, 2005).

<sup>5</sup> Of course if this refers to gross rather than net job creation, it could be consistent with Wal-Mart destroying more jobs than it creates.

In this paper, we seek to provide a definitive answer regarding whether Wal-Mart creates or eliminates jobs in the retail sector. Also, because of concern over the effects of Wal-Mart on wages, and because policymakers may be interested in the impact of Wal-Mart on taxable payrolls, we also estimate the effects of Wal-Mart on earnings in the retail sector, reflecting the combination of influences on employment, wages, and hours. We believe that our evidence improves substantially on existing studies of these and related questions, most importantly by implementing an identification strategy that accounts for the endogeneity of store location and timing and how these may be correlated with future changes in earnings or employment. Indeed, it has been suggested that Wal-Mart's explicit strategy was to locate in small towns where the population growth was increasing (Slater, 2003, pp. 92), and it is reasonable to expect that Wal-Mart entered markets where projected retail growth was strong. If Wal-Mart tends to enter fast-growing areas in booming periods, then we might expect to observe employment and earnings rising in apparent response to Wal-Mart's entry, even if the stores actually have negative effects on both outcomes.

Our identification strategy is driven by a systematic pattern in the openings of Wal-Mart stores. Sam Walton, the founder of Wal-Mart, opened the first Wal-Mart store in 1962 in Rogers, Arkansas, in Benton County. Five years later, Wal-Mart had 18 stores with \$9 million of annual sales. Wal-Mart first grew into a local chain store in the northwest part of Arkansas. It then spread to adjacent states such as Oklahoma, Missouri, and Louisiana. From there, it kept expanding to the rest of the country after closer markets were largely saturated (Slater, 2003, pp. 28-29). The relationship between Wal-Mart stores' opening dates and their distance to the headquarters is primarily a result of Wal-Mart's "saturation" strategy for growth, which was based on control and distribution of stores, as well as word-of-mouth advertising. In his autobiography, Sam Walton describes the control and distribution motive as follows:

"[Our growth strategy] was to saturate a market area by spreading out, then filling in. In the early growth years of discounting, a lot of national companies with distribution systems already in place—Kmart, for example—were growing by sticking stores all over the country. Obviously, we couldn't support anything like that. ... We figured we had to build our stores so that our distribution centers, or warehouses, could take care of them, but also so those stores could be controlled. We wanted them within reach of our district managers, and of ourselves here in Bentonville, so we could get out there and look after them. Each store had to be within a day's

drive of a distribution center. So we would go as far as we could from a warehouse and put in a store. Then we would fill in the map of that territory, state by state, county seat by county seat, until we had saturated that market area. ... So for the most part, we just started repeating what worked, stamping out stores cookie-cutter style” (Walton, 1992, pp. 110-111).

One might wonder whether this need to be near a distribution center requires a steady spreading out from Arkansas. Why not, for example, open distribution centers further away, and build stores near them? The explanation seems to lie in the word-of-mouth advertising advantage perceived to result from the growth strategy Wal-Mart pursued:

“This saturation strategy had all sorts of benefits beyond control and distribution. From the very beginning, we never believed in spending much money on advertising, and saturation helped us to save a fortune in that department. When you move like we did from town to town in these mostly rural areas, word of mouth gets your message out to customers pretty quickly without much advertising. When we had seventy-five stores in Arkansas, seventy-five in Missouri, eighty in Oklahoma, whatever, people knew who we were, and everybody except the merchants who weren’t discounting looked forward to our coming to their town. By doing it this way, we usually could get by with distributing just one advertising circular a month instead of running a whole lot of newspaper advertising” (Walton, 1992, p. 111).

Wal-Mart’s practice of growing by “spreading out” geographically means that distance from Benton County, Arkansas, and time—and more specifically their interaction—is a good predictor of when and where stores opened.<sup>6</sup> Thus, the key innovation in this paper is to instrument for the opening of Wal-Mart stores with interactions between time and the distance between Wal-Mart host counties and Benton County, Arkansas, where Wal-Mart headquarters are located.

## **II. Literature Review**

There are a number of studies that address claims about Wal-Mart’s impacts on local labor markets, emphasizing the retail sector. However, we regard much of this literature as uninformative about the causal impact of Wal-Mart on retail employment and earnings. First, some of the existing work is by advocates for one side or the other in local political disputes regarding Wal-Mart’s entry into a particular market. These studies are often hastily prepared, plagued by flawed methods and arbitrary assumptions, and sponsored by interested parties such as Wal-Mart itself, its competitors, or union groups

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<sup>6</sup> For example, Wal-Mart’s expansion did not reach California until 1990. It first entered New England in 1991. In 1995 Wal-Mart opened its first store in Vermont and finally had a presence in all 48 contiguous states.

(e.g., Bianchi and Swinney, 2004; Freeman, 2004; and Rodino Associates, 2003), and can hardly be expected to provide impartial evidence on Wal-Mart's effects. Hence, they are not summarized here.

There is also an academic literature on the impact of Wal-Mart stores, focusing on the effects of Wal-Mart openings on local employment, retail prices and sales, poverty rates, and the concentration of the retailing industry, as well as the impact on existing businesses. This research is limited by three main factors: the restriction of much of it to small regions (often a single small state); its lack of focus on employment and earnings effects; and its failure to account for the endogeneity of Wal-Mart locations, either at all or (in our view) adequately.

Many of these studies, especially the early ones, focus on the effects of Wal-Mart at the regional level, spurred by the expansion of Wal-Mart into a particular region. The largest number of studies focus on the effects of Wal-Mart on retail businesses and sales, rather than on employment and earnings. The earliest study, which is typical of much of the research that has followed, is by Stone (1988). He defines the "pull factor" for a specific merchandise category as the ratio of per capita sales in a town to the per capita sales at the state level, and examines the changes in the pull factor for different merchandise categories in host and surrounding towns in Iowa after the opening of Wal-Mart stores. Stone finds that in host towns, pull factors for total sales and general merchandise (to which all Wal-Mart sales belong) rise after the arrival of Wal-Mart. Pull factors for eating and drinking and home furnishing also go up because Wal-Mart brings in more customers. However, pull factors for grocery, building materials, apparel, and specialty stores decline, presumably due to direct competition from Wal-Mart. He also finds that small towns surrounding Wal-Mart towns suffer a larger loss in total sales compared to towns that are further away.<sup>7</sup> Related results for other regions—which generally, although not always, point to similar conclusions—are reported in Keon, et al. (1989), Barnes, et al. (1996), Davidson and Rummel (2000), and

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<sup>7</sup> Stone's study was updated regularly (see, for example, Stone, 1995, 1997), but its central message remained the same: Wal-Mart pulls more customers to the host town, hurts its local competitors, but benefits some other local businesses that do not directly compete with it. Using the same methods, Stone, et al. (2002) show similar results regarding the effects of Wal-Mart Supercenters on existing businesses in Mississippi.

Artz and McConnon (2001). All of these studies use administrative data, and employ research designs based on before-and-after comparisons in locations in which Wal-Mart stores did and did not open.<sup>8</sup>

The studies reviewed thus far do not address the potential endogeneity of the location and timing of Wal-Mart's entry into a particular market. In addition, these studies do not focus on the key questions with which this paper is concerned—the effects of Wal-Mart on retail employment and earnings. A few studies come closer to the mark. Ketchum and Hughes (1997), studying counties in Maine, recognize the problem of the endogenous location of Wal-Mart stores in faster-growing regions. They attempt to estimate the effects of Wal-Mart on employment and earnings using a difference-in-difference-in-differences (DDD) estimator that compares changes in retail employment and earnings over time in counties in which Wal-Mart stores did and did not locate, compared to changes for manufacturing and services. However, virtually none of their estimated changes are statistically significant, so one cannot learn much from these data (and the data appear very noisy). More important, their approach does not address the key endogeneity questions of whether Wal-Mart location decisions were based on anticipated changes after stores opened, or instead only prior trends that were already different (despite the authors posing these questions).<sup>9</sup> Hicks and Wilburn (2001), studying the impact of Wal-Mart openings in West Virginia, estimate positive impacts of Wal-Mart stores on retail employment and the number of retail firms. They do not explicitly account for endogeneity, although they do address the issue. In particular, they report evidence suggesting that Wal-Mart location decisions are independent of long-term economic growth rates of individual counties in their sample, and that current and lagged growth have no significant effect on Wal-Mart's decision to enter. However, these results do not explicitly address endogeneity with

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<sup>8</sup> A couple of studies rely on surveys of local businesses rather than administrative data. McGee (1996) reports results from a small-scale survey of small retailers in five Nebraska communities conducted soon after Wal-Mart stores entered. He finds that 53 percent of the responding retailers reported negative effects of Wal-Mart's arrival on their revenues while 19 percent indicated positive effects. In a survey of Nebraska and Kansas retailers, Peterson and McGee (2000) find that less than a third of the businesses with at least \$1 million in annual sales reported a negative effect after Wal-Mart's arrival, while close to one half of the businesses with less than \$1 million in annual sales indicated a negative effect, with negative effects most commonly reported by small retailers in central business districts. The research design in these surveys fails to include a control group capturing changes that might have occurred independently of Wal-Mart openings. In addition, reported assessments by retailers may not reflect actual effects of these openings.

<sup>9</sup> The second question can only be addressed via an instrumental variables approach, and the first requires looking at changes in growth rates, not changes in levels; their study only does the latter.

respect to future growth. The latter, in particular, could generate apparent positive impacts of Wal-Mart stores.

In more recent work, Basker (2005b) studies the effects of Wal-Mart on retail employment using nationwide data. Basker attempts to account explicitly for endogeneity by instrumenting for the actual number of stores opening in a county in a given year with the planned number. The latter is based on numbers that Wal-Mart assigns to stores when they are planned; according to Basker, these store numbers indicate the order in which the openings were planned to occur. She then combines these numbers with information from Wal-Mart *Annual Reports* to measure planned openings in each county and year. Her results indicate that county-level retail employment grows by about 100 in the year of Wal-Mart entry, but declines to a gain of about 50 jobs in five years as other retail establishments contract or close. In the meantime, possibly because Wal-Mart streamlines its supply chain, wholesale employment declines by 20 jobs in the longer term.<sup>10</sup>

The principal problem with this identification strategy, however, is that the instrument is unconvincing. For the instrument to be valid, two conditions must hold. The first is that planned store openings should be correlated with (predictive of) actual openings; this condition is not problematic. The second condition is that the variation in planned openings generates exogenous variation in actual openings that is uncorrelated with the unobserved determinants of employment that endogenously affect location decisions. This second condition holds if we assume, to quote Basker, that “the number of *planned* Wal-Mart stores ... for county  $j$  and year  $t$  is independent of the error term ... and *planned* Wal-Mart stores affect retail employment per capital only insofar as they are correlated with the *actual* construction of Wal-Mart stores” (2005b, p. 178). The second part of the assumption is potentially problematic; actual stores should, of course, be the driving influence, although planned stores—even if

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<sup>10</sup> Using the same instrumental variables strategy, Basker (2005a) estimates the effects of Wal-Mart entry on prices of consumer goods at the city level, finding long-run declines of 8-13 percent in prices of several products including aspirin, detergent, Kleenex, and toothpaste, although it is less clear to us why location decisions would be endogenous with respect to price. Ordinary least squares (OLS) estimation also finds long-run negative effects of Wal-Mart on prices for nine out of ten products, although only three are significant and all are smaller than the IV results. Hausman and Leibtag (2004) study the effects of Wal-Mart on food prices.

they do not materialize or do so only with delay—may still affect decisions of other businesses.<sup>11</sup> The first part of the assumption is a more serious concern, though, as it seems most likely that planned openings will reflect the same unobserved determinants that drive endogenous location as are reflected in actual openings, and we cannot think of an argument to the contrary (nor does Basker offer one). In this case if the OLS estimate of the effect of Wal-Mart stores on retail employment is biased upward because of endogeneity, the instrumental variable (IV) estimate will also be biased upward, most likely by more than the OLS estimate.<sup>12</sup>

Another potential problem is that there is substantial measurement error regarding store openings in Basker's data.<sup>13</sup> This measurement error arises because Basker (and Goetz and Swaminathan, 2004,

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<sup>11</sup> The response of supermarkets in southern California is a telling example. In 2003, after Wal-Mart announced plans to build 40 Supercenters in California to enter the state's grocery market, supermarket chains in southern California including Albertsons, Ralphps, and Vons immediately sought to lower costs and avoid being undercut by Wal-Mart. Their unionized workers would not accept lower compensation and began a strike that lasted more than four months. Eventually, the union leaders conceded and essentially accepted the supermarkets' position that cutting labor costs was necessary for competing with Wal-Mart. All of this occurred before Wal-Mart opened its first Supercenter in California (in March 2004). See, for example, Goldman and Cleeland (2003) and Hiltzik (2004).

<sup>12</sup> To take a simple example, suppose that the simultaneous model for the change in employment ( $\Delta E$ ), planned openings ( $P$ ), and actual openings ( $A$ ) is

$$\begin{aligned}\Delta E &= \alpha A + \varepsilon \\ P &= \beta \Delta E + v \\ A &= \gamma \Delta E + v + \eta,\end{aligned}$$

with  $\varepsilon$ ,  $v$ , and  $\eta$  uncorrelated, and  $v$  and  $\eta$  uncorrelated with  $\Delta E$ . Employment depends on actual openings (so we assume that the first assumption discussed in the text holds). Both planned and actual openings are simultaneously determined with employment, but actual openings reflect additional information captured in  $\eta$ , orthogonal to  $v$  either because it reflects additional information not in the information set when  $P$  was determined, or simply random variation due to construction delays, zoning disputes, etc., which make actual and planned openings deviate. In this case the asymptotic bias in the OLS estimate of  $\alpha$  is

$$(\gamma/(1-\alpha\gamma)) \cdot \text{Var}(\varepsilon)/\text{Var}(A)$$

which is positive assuming  $\alpha < 0$  (Wal-Mart reduces employment) and  $\gamma$  (and similarly  $\beta$ )  $> 0$  (employment encourages Wal-Mart openings). The IV estimate of  $\alpha$  using  $P$  as an instrument for  $A$  is asymptotically biased upward, with bias

$$(\beta/(1-\alpha\gamma)) \cdot \text{Var}(\varepsilon)/\text{Cov}(A,P).$$

The ratio of the IV bias to the OLS bias is

$$\{\beta \cdot \text{Var}(A)\} / \{\gamma \cdot \text{Cov}(A,P)\} = \{\beta \cdot (\gamma^2 \text{Var}(\Delta E) + \text{Var}(v) + \text{Var}(\varepsilon))\} / \{\gamma \cdot (\gamma\beta \text{Var}(\Delta E) + \text{Var}(\varepsilon))\}.$$

Given that  $A$  is based on more information than  $P$  (contained in  $\eta$ ), we would expect  $\beta > \gamma$ , in which case this ratio clearly exceeds one, so the upward IV bias exceeds the upward OLS bias.

<sup>13</sup> This is detailed in an appendix available from the authors.

discussed below), had to collect information about Wal-Mart locations and opening dates from a variety of sources including Wal-Mart editions of the *Rand McNally Road Atlas*, annual editions of the *Directory of Discount Department Stores*, and *Wal-Mart Annual Reports*, which together do not always pin down the timing of each store opening. Basker also motivates the IV as correcting for bias from measurement error in the actual opening dates of Wal-Mart stores. But the same argument against the validity of planned openings as an instrument applies.

Using various data sources, Goetz and Swaminathan (2004) study the relationship between Wal-Mart openings between 1987 and 1998 and county poverty rates in 1999, conditional on 1989 poverty rates (as measured in the 1990 and 2000 Censuses of Population). They also use an IV procedure to address the endogeneity of Wal-Mart entry, instrumenting for Wal-Mart openings during 1987-1998 in an equation for county poverty rates in 1999. Their IVs include an unspecified pull factor, access to interstate highways, earnings per worker, per capita property tax, population density, percentage of households with more than three vehicles, and number of female-headed households. The results suggest that county poverty rates increase when Wal-Mart stores open, perhaps because Wal-Mart lowers earnings (although the authors offer other explanations as well). However, why the IVs should affect Wal-Mart openings only, and not changes in poverty directly (conditional on Wal-Mart openings), is not clear, and it is not hard to construct stories in which invalid exclusion restrictions would create biases towards the finding that Wal-Mart openings increase poverty.<sup>14</sup>

Our research addresses the four principal shortcomings of the existing research on the effects of Wal-Mart on local labor markets. First, we estimate the effects of Wal-Mart openings on retail earnings as well as employment. Second, we have—we believe—a far more convincing strategy to account for the potential endogeneity of Wal-Mart openings, which seems most likely to bias upward any estimated

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<sup>14</sup> For example, consider the use of the number of female-headed households as an IV, and suppose that this variable is positively correlated with changes in poverty rates (because of rising inequality over this period), and also positively correlated with Wal-Mart openings (because they locate in lower-income areas). In this case the IV estimate of the effect of Wal-Mart openings on changes in poverty rates is biased upward because of the positive correlation between the instrument (female-headed households) and the error term in the equation for the change in the poverty rate. In addition, although the authors do not specify how they construct their pull factor, we assume it is similar to the measure described above—a ratio of county to statewide retail sales. Given that this is a dependent variable in other studies of the effects of Wal-Mart, it is hard to justify using it as an IV for Wal-Mart openings.

effects of Wal-Mart stores on employment and earnings. Third, we are able to use administrative data on Wal-Mart openings that eliminate the measurement error in recent work. And finally, we use a data set that is national in scope.

### **III. Data**

Our empirical analysis relies on data from various sources, and computations of our own, which we describe in this section.

#### *Employment and Payroll Data*

Employment and payroll data are drawn from the U.S. Census Bureau's County Business Patterns (CBP).<sup>15</sup> CBP is an annual series that provides economic data by industry and county. The series includes most economic activity, but excludes data on self-employed individuals, employees of private households, railroad employees, agricultural production workers, and most government employees. CBP data are extracted from the Business Register, which is a U.S. Census Bureau file of all known single and multi-establishment companies in the United States. The Business Register includes payroll and employment data from multiple sources including Census Bureau surveys, the Internal Revenue Service, the Social Security Administration, and the Bureau of Labor Statistics.

Payroll in the CBP includes salaries, wages, reported tips, commissions, bonuses, vacation allowances, sick-leave pay, employee contributions to qualified pension plans, and taxable fringe benefits, and is reported before deductions for Social Security, income tax, insurance, etc.<sup>16</sup> It does not include profit or other compensation earned by proprietors or business partners. Payroll is reported on an annual basis. The employment measure in the CBP data is a count of jobs, rather than not the number of people employed (in one or more jobs). Employment covers all full- and part-time employees, including officers and executives, as of the pay period including March 12 of each year. Workers on leave are included, while proprietors and partners are not. The most significant limitation of the CBP data for studying the effects of Wal-Mart is that a wage cannot be computed. The CBP data do not even provide a breakdown

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<sup>15</sup> A good description of the CBP data and its differences relative to other data sources is available at <http://www.calmis.ca.gov/FILE/ES202/CEW-About.htm> (as of September 6, 2005).

<sup>16</sup> Given that it excludes non-taxable fringe benefits, of which the most important is health insurance, we refer to the CBP measure as earnings, rather than compensation.

of employment into full-time and part-time workers, which would permit calculation of an approximate wage assuming a given number of hours for full-time and part-time workers. In addition, we cannot tell whether changes in payrolls reflect changes in pay rates for comparable workers or shifts in skill composition. As a consequence, these data cannot be used to address questions of the effects of Wal-Mart on wages. We can, though, estimate Wal-Mart's effect on total retail payrolls, which is of independent interest.

We downloaded CBP data by two-digit SIC major group (three-digit NAICS subsector since 1998) from 1977 through 2002, from the Geospatial and Statistical Data Center at the University of Virginia (through 2001) and the U.S. Census (for 2002).<sup>17</sup> We began with 1977 because CBP data are not continuously machine-readable for the years 1964-1976, and ended with 2002 because that was the last year available. As explained below, however, most our analysis goes only through 1995—a period for which our identification strategy is most compelling.

### *Samples*

We study the retail sector as a whole, as well as the General Merchandising subsector—which includes Wal-Mart and other general department-style stores. Looking at results for these different retail sectors is useful for assessing whether the results are sensible. In particular, if Wal-Mart reduces retail employment, we might expect the employment reduction to show up for the aggregate retail sector, but to be offset by an increase in general merchandising.<sup>18</sup> Some complications arise in working with the CBP data because by federal law no data can be published that would disclose the operations of an individual employer. As we look at more disaggregated subsets of industries, it is more likely that data are not disclosed and so our sample becomes smaller. Consequently, we constructed two samples with which we could consistently compare at least some retail industry sectors for the same set of observations, as follows:

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<sup>17</sup> Available at <http://fisher.lib.virginia.edu/collections/stats/cbp/> (as of April 5, 2005) and <http://www.census.gov/epcd/cbp/download/cbpdownload.html> (as of April 5, 2005).

<sup>18</sup> There is no clear prediction for how payrolls effects should vary across retail sectors, since workers are presumably at least somewhat mobile across these sectors.

- A sample: all county-year observations with complete (non-suppressed) employment and payroll data for aggregate retail, and in total.
- B sample: all observations in the A sample that also have complete data for the General Merchandising retail subsector (SIC 53 or NAICS 452) to which Wal-Mart belongs.

Because the rules for whether or not data are disclosed depends on the size of the retail sector and the size distribution of establishments within it, sample selection is endogenous. We therefore emphasize results for the A sample, which includes nearly all counties and years. We use the B sample only to compare estimates for aggregate retail and general merchandising; as long as any biases from selection into the B sample are similar across retail subsectors, the estimates for aggregate retail and general merchandising can still be meaningfully compared. However, this analysis of retail subsectors using the B sample is a minor part of our overall analysis.<sup>19</sup>

#### *Wal-Mart Store Data*

Wal-Mart provided us with administrative data on 3,066 Wal-Mart Discount Stores and Supercenters.<sup>20</sup> The data set contains every Discount Store and Supercenter still in operation in the United States at the end of fiscal year 2005 (January 31, 2005).<sup>21</sup> Variables in the data set include store number, street address, city, state, ZIP code, square footage, store type, opening date, store hours (e.g., open 24 hours), latitude, longitude, county FIPS code, and Metropolitan Statistical Area (MSA) code for each store. After dropping stores in Alaska and Hawaii, we used 2,211 stores in our main analysis through 1995, and 2,795 stores when we use the full sample period through 2002. By 2005, Wal-Mart also had 551 Sam's Club stores in the United States (the first opened in 1983), on which we also obtained data, although the data were less complete (for example, lacking information on square footage). We do

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<sup>19</sup> Basker (2005b) also focuses on the retail subsector excluding Eating and Drinking Places and Automotive Dealers and Gasoline Service Centers—subsectors that are least likely to compete directly with Wal-Mart. However, constructing data for this subsector results in nearly two-thirds of county-year pairs being discarded because of non-disclosure, in which case endogenous sample selection can be severe.

<sup>20</sup> Unfortunately, we are often missing information on when Discount Stores converted to Supercenters, which is frequent in the latter part of the sample. All we know is the current store type.

<sup>21</sup> We also received data identifying a small number of stores (54, as of 2005) that closed. We return to this issue in some of our robustness analyses. This data set also indicated some store relocations within counties, which are treated as continuing stores, apparently because Wal-Mart replaced smaller, older stores with larger ones in nearby locations.

most of our analysis considering the Wal-Mart stores other than Sam's Clubs, but also some analysis incorporating information on the latter.<sup>22</sup>

#### *County-Year File*

We constructed a county-year file by first collecting county names and FIPS codes for the 3,141 U.S. counties from the U.S. Census Bureau.<sup>23</sup> We then created time-consistent geographical areas which accounted for merges or splits in counties during the sample period. For counties that split during the sample period we maintained the definition of the original county, and for counties that merged during the sample period we created a single corresponding county throughout.<sup>24</sup> This leads to a file of 3,094 counties over 19 years (26 years when we use the full sample), to which we merge the CBP and Wal-Mart data.

#### *County Population Data*

County population data for each year were collected from the U.S. Census Population Estimates Archives.<sup>25</sup> These were assigned to the counties.

#### *Distance Construction*

We compiled latitude and longitude data for each county centroid from the U.S. Census Bureau's Census 2000 Gazetteer Files.<sup>26</sup> Using the Haversine distance formula, we constructed distance measures from each county to Wal-Mart headquarters in Benton County, Arkansas, for reasons explained below.<sup>27</sup>

### **IV. Empirical Approach and Identification**

We estimate models for changes in retail employment and payrolls. We generally capture increased exposure to Wal-Mart stores via a measure of store openings in a county-year cell—i.e., the change in the number of stores. We define both types of changes on a per person basis to eliminate the

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<sup>22</sup> Sam's Clubs are different because one has to become a member, like at Costco. The small number of Wal-Mart Neighborhood Markets are not included in any data we have, but the first one did not open until 1998, beyond the sample period used for most of our analysis.

<sup>23</sup> Downloaded from <http://www.census.gov/datamap/fipslist/AllSt.txt> (as of April 5, 2005).

<sup>24</sup> The code for creating consistent counties over time through 2000 was provided by Emek Basker, and supplemented by us.

<sup>25</sup> Downloaded from <http://www.census.gov/popest/archives/> (as of April 5, 2005).

<sup>26</sup> Downloaded from <http://www.census.gov/tiger/tms/gazetteer/county2k.txt> (as of April 5, 2005).

<sup>27</sup> The Haversine distance formula is used for computing distances on a sphere. See Sinnott (1984).

undue influence of a small number of large employment changes in extraordinarily large counties. As long as we divide both changes by the number of persons in the county, the estimated coefficient on the Wal-Mart variable still measures the effect of a Wal-Mart store opening on the change in the level of retail employment or earnings.<sup>28</sup> To control for overall income growth that may affect the level of demand for retail, we include changes in total payroll per person as a control variable. In addition, all models include fixed year effects to account for aggregate influences on changes in retail employment or earnings that might be correlated with Wal-Mart openings, which occur with greater frequency later in the sample.

We denote the county-level measures of retail employment and payrolls (per person) as  $Y$ , the number of Wal-Mart stores (per person) as  $WM$ , total payrolls per person as  $TP$ , and year fixed effects (in year  $s$ ) as  $YR^s$ . Indexing by county  $j$  ( $j = 1, \dots, J$ ) and year  $t$  ( $t = 1, \dots, T$ ), and defining  $\alpha$ ,  $\beta$ ,  $\gamma$ , and  $\delta_s$  as scalar parameters, our baseline model for the change in the dependent variable for each observation  $jt$  is:

$$(1) \quad \Delta Y_{jt} = \alpha + \beta \Delta WM_{jt} + \gamma \Delta TP_{jt} + \sum_s \delta_s YR^s_{jt} + \varepsilon_{jt}.$$

We begin by reporting OLS estimates of this equation. Throughout, we report standard errors that cluster on state. Thus, these standard errors are robust to heteroscedasticity of the error across states, and autocorrelations within and across counties. With these standard errors, then, we allow for both temporal and spatial autocorrelations.<sup>29</sup>

In the corresponding model for levels of employment we would certainly also want to include county fixed effects to account for fixed differences in retail employment or payrolls across counties. The latter, however, drop out of a model for first differences of retail employment or payrolls. However, it is possible that there is systematic variation in these first differences across different regions, corresponding

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<sup>28</sup> Let  $\beta$  be the coefficient on the change in the number of Wal-Mart stores per person. Since a one-unit change in the per capita Wal-Mart measure is an increase of one in stores per person, the effect of a store opening on retail employment per person is  $\beta/\text{population}$ . Thus, the effect on retail employment is  $\beta$ . This parallels Basker's specification, so the units are comparable.

<sup>29</sup> We also calculated standard errors clustering on state-year cells, which allows contemporaneous correlations across counties, but not autocorrelation within or across counties. In this case the standard errors were about 20 percent smaller. Standard errors clustering on county only, allowing no cross-county correlation but autocorrelation within counties, were about 50 percent smaller than in the baseline estimates.

to faster or slower growth. The most flexible approach would be to include county fixed effects in the first-difference regressions. However, as explained in the next subsection, we will be particularly concerned with whether there are systematic differences in the rates of change of our dependent variables in geographic “rings” centered on Benton County, Arkansas. We therefore instead estimate models in which we augment equation (1) to include dummy variables for counties with centroids in rings within a radius of 100 miles from Benton County, Arkansas, 101-200 miles, 201-300 miles, etc., out to the maximum radius of 1800 miles (denoted  $DIST^i$ ) as in

$$(1') \quad \Delta Y_{jt} = \alpha + \beta \Delta W M_{jt} + \gamma \Delta T P_{jt} + \sum_i \varphi_i DIST^i_{jt} + \sum_s \delta_s YR^s_{jt} + \varepsilon_{jt},$$

where  $\varphi_i$  are also scalar parameters. This model allows for “distance-specific” linear trends in retail employment and earnings.<sup>30</sup>

#### *Endogeneity of Wal-Mart Location Decisions and Identification*

Consistent estimation of equations (1) or (1') requires that  $\varepsilon_{jt}$  is uncorrelated with the right-hand-side variables. If Wal-Mart location decisions are based in part on changes in employment or payrolls, then this condition is violated. This endogeneity is natural, since it would be surprising if a company as successful as Wal-Mart did not make location decisions (including the location and timing of store openings) in a systematic fashion related to current conditions and future prospects that might be related to both employment and payroll. As but one example, Wal-Mart may open stores where real estate development and zoning have recently become favorable to retail growth.

Our identification strategy in light of this potential endogeneity is based on the geographic pattern of Wal-Mart store openings over time. Figure 1 illustrates quite clearly how Wal-Mart stores spread out geographically throughout the United States, beginning in Arkansas as of 1965, expanding to Oklahoma, Missouri, and Louisiana by 1970, Tennessee, Kansas, Texas, and Mississippi by 1975, much of the South and the lower Midwest by 1985, more of the Southeastern seaboard, the plains, and the upper Midwest by 1990, and then, in turn, the Northeast, West Coast, and Pacific Northwest by 1995. After 1995, when the

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<sup>30</sup> We verified that results were very similar using county fixed effects instead of these distance-specific fixed effects.

far corners of the country had been entered, there was only filling in of stores in areas that already had them. This pattern is what we would expect based on Wal-Mart’s growth strategy of expanding outward by leapfrogging distribution centers, and by moving successively to nearby geographic areas, as described earlier in the quotes from Sam Walton.

This pattern of growth is significant because it generates an exogenous source of variation in the location and timing of Wal-Mart store openings. In particular, Figure 2—which depicts openings only, rather than all existing store locations—clearly indicates that time and distance from Arkansas (in particular, Benton County) predict where and when Wal-Mart stores will open. However, this does not necessarily imply that time and distance can serve as instrumental variables for exposure to Wal-Mart stores. To see this, suppose we appeal to Figure 2 to posit an equation for Wal-Mart openings of the form

$$(2) \quad \Delta WM_{jt} = \kappa + \pi \Delta TP_{jt} + \sum_i \lambda_i DIST^i_{jt} + \sum_s \mu_s YR^s_{jt} + \eta_{jt}.$$

Then time gives us no identifying information, because time is already captured in the year fixed effects included in either equation (1) or (1’), and the same is true of *DIST* in equation (1’).

However, the model in equation (2) has a specific implication that is belied by the data. In particular, the additivity of the distance and year effects implies that differences across years in the probability of Wal-Mart openings are independent of distance from Benton County. Looking at Figure 2—especially through 1995, the period during which Wal-Mart stores spread to the borders of the continental United States—it is clear that in the area near Arkansas openings are concentrated in the earliest years. In contrast, in the 1981-1985 period openings are more concentrated further away from Arkansas. This pattern becomes more obvious in the 1986-1990 and 1991-1995 maps, where openings thin considerably in the area of Wal-Mart’s original growth, and are more common first in Florida, then in the Southeast and the lower Midwest, and finally in California, the upper Midwest, and the Northeast.<sup>31</sup>

The fact that the rate of openings slows considerably in the Southeast, for example, in the later years, and increases in areas further away—in a rough sense spreading out from Benton County like a

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<sup>31</sup> As suggested by Figure 1, after 1995 Wal-Mart’s growth consists more of filling in within areas to which it had already expanded.

wave (albeit irregular)—contradicts the implication of the additivity of distance and time effects in equation (2) that the time pattern of Wal-Mart store openings is independent of distance from Benton County. Instead, it implies that the model for Wal-Mart openings should have a distance-time interaction, with the probability of openings higher early in the sample period in locations near Benton County, but higher later in the sample period further away from Benton County. Because this relationship holds through 1995, when Wal-Mart had begun to saturate border areas, we restrict most of our analysis to this period, although we also report results using the full sample through 2002. The most flexible form of this interaction, expanding on equation (2), includes interactions between dummy variables for year and for the different distance ranges, as in

$$(3) \quad \Delta WM_{jt} = \kappa + \pi \Delta TP_{jt} + \sum_i \lambda_i DIST^i_{jt} + \sum_s \mu_s YR^s_{jt} + \sum_s \sum_i \theta_{si} (DIST^i_{jt} \times YR^s_{jt}) + \eta_{jt}.$$

Given this specification, the endogenous effect of Wal-Mart openings is identified in equation (1') by using the distance-time interactions as instruments for exposure to Wal-Mart stores.<sup>32</sup>

Four comments are in order. First, we also report estimates of a simpler specification that substantially restricts the instrument set, using linear distance interacted with the year dummy variables.

Second, although there are, technically speaking, more instruments than are needed to identify the model, overidentifying tests are in our context uninformative about the validity of the instruments.

Conceptually, there is one instrument—the interaction of distance and time—and all we do here is to use a flexible form of this variable. Were there a good reason—based, for example, on location strategies gleaned from Sam Walton's writings—to think that there were particularly exogenous location decisions in a particular time period or at a specific distance from Benton County, then arguably one might want to use this information to identify the model, and test for the validity of distance-time interactions from other periods or further distances using overidentification tests. Walton's writings, however, bolstered by the maps in Figure 2, suggest that distance and time would have acted in a similar fashion throughout the entire period during which Wal-Mart stores spread to the borders of the United States. Thus, we do not

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<sup>32</sup> The same goes for equation (1), omitting the distance dummy variables from the first- and second-stage equations.

believe that an overidentification test is informative about the validity of any of our instruments, as the entire set of instruments is either jointly valid, based on a priori arguments, or is not.<sup>33,34</sup>

Third, one might argue that equation (3) simply exploits a non-linearity to identify the effect of Wal-Mart stores. But this non-linearity arises naturally from Wal-Mart's growth strategy, rather than arbitrarily as an artifact of the data.

Fourth, in addition to these interactions predicting store openings, the other key condition for our identification strategy to be valid is that we can exclude distance-time interactions from the models for changes in retail employment and payrolls. Given that points at a given distance from Benton County are located on a circle with Benton County at its center, it is not immediately clear why this condition should not be satisfied. A particular area—say, 500 miles straight east from Benton County—may have economic conditions or structure that differ from those in Benton County and hence also have systematically different trends in retail employment or earnings. But there is no obvious reason why all points on the circle with radius of 500 miles should exhibit systematically different changes in particular periods relative to Benton County, aside from the effects of Wal-Mart that—roughly speaking—influence points at a common distance from Benton County at the same time. On the other hand, we cannot

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<sup>33</sup> An analogous example would be a setting in which the amount of schooling an individual has is argued to be a valid instrument, on a priori grounds. One could not, then, split schooling up into single-year categories and do an overidentification test to test the validity of schooling as an instrument. More formally, in this case (or in ours), the standard overidentification test asks whether any function of the instrument is uncorrelated with the error, assuming some simpler function of the instrument is enough to identify the model. However, this is a stronger condition than is required for consistency of the IV estimate.

<sup>34</sup> In an earlier version of this paper, we estimated similar models in levels—i.e., the level of retail employment on the number of stores (both on a per person basis), instrumenting for the number of stores with distance-time interactions. However, as Figures 1 and 2 make clear, it is openings that are predicted by the distance-time interaction, not the number of stores; the number of stores remains high near Benton County late in the sample period. Given that, the earlier models were misspecified. If the distance-time interactions appear in the equation for openings (or the first difference for the number of stores), the equation in levels would have to have these interactions multiplied by a linear time trend, which would capture the fact that the number of stores *grew faster*, for example, at distances far from Benton County late in the sample period.

Soon after our research was completed, we discovered very recent research done concurrently by Dube, et al. (2005), which also exploits the geographic pattern of Wal-Mart openings to identify their effect on retail earnings growth. Aside from focusing only on earnings, this latter paper differs in a couple of ways, including using Basker's data on Wal-Mart stores rather than administrative data, and restricting the analysis to the 1992-2000 period. The discussion of the maps in Figures 1 and 2, which suggest that the distance-time quasi-experiment is unlikely to be powerful after 1995 or so, raises concerns about Dube, et al.. using data only from 1992-2000. They rationalize this as speaking more to the contemporaneous effects of Wal-Mart stores. This may better address current policy debates, but the more recent data may not provide reliable identification of the effects of Wal-Mart stores. In addition, the instrument they use has the same specification error noted above, with the first-stage for the number of Wal-Mart stores including distance-time interactions.

completely rule this out, which is why we estimate specifications allowing for different trends for the “rings” of different radius around Benton County. Of course the most flexible model is the fully saturated one that includes county-specific fixed effects as well as their interactions with the year fixed effects in the equations we estimate, allowing for arbitrary differences in the changes in the dependent variables by county and year. In this case, of course, the distance-time interactions would provide no identifying information. But even in the absence of attempts to account for endogeneity, a model with unrestricted time effects for each county would not permit the identification of the effects of Wal-Mart stores. Thus, some more restricted version of how distance-time interactions enter the employment and earnings equations would have to be imposed.

#### *Theoretical Framework Underlying the Specifications*

The empirical specifications can be given more rigorous justification in a simple theoretical model. This model oversimplifies, ignoring a host of issue such as strategic interactions, mobility, general equilibrium effects, etc.<sup>35</sup> But it does demonstrate that the specifications we estimate are consistent with a simple model of Wal-Mart’s entry decisions and effects on the local labor market in the retail sector.

Consider the retail sector in each county. We assume that workers cannot move from one county to another and consumers will only shop in their own county. Labor demand in the retail sector is a derived demand and thus affected by various factors in the market for retail services. For example, factors shifting the demand curve for retail service (income growth, introduction of new consumer goods, etc.) will have an impact on labor demand in the retail sector. Similarly, shocks shifting the supply curve for retail services (efficiency gains from technological progress, better supply-chain management, etc.) will influence labor demand. In addition, factors that change the labor intensiveness of the supply technology in the retail sector, even if not shifting the supply curve, will also impact labor demand. Wal-Mart is assumed to affect labor demand in the retail sector through two channels: (1) its greater efficiency shifts the supply curve for retail services; and (2) its adoption of technologies changes labor intensiveness

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<sup>35</sup> For more complex models of Wal-Mart entry and its effects on other competitors, see Holmes (2005) and Jia (2005).

in the retail sector. Thus, the labor demand function in the retail sector in county  $j$  at time  $t$  can be written as:

$$(4) \quad L^d_{jt} = f[w_{jt}, WM_{jt}(o_{jt-1}, o_{jt-2}, \dots), X_{jt}],$$

where  $w_{jt}$  is the wage rate.  $WM_{jt}$  measures the number of Wal-Mart stores, which is a function of the number of Wal-Mart stores opened at county  $j$  in all previous years up to year  $t$ , where  $o_{jt-k}$  measures the number of stores that opened in county  $j$  in year  $t-k$ .  $X_{jt}$  is a vector of exogenous variables that shift supply and demand in the retail service market and thus affect the equilibrium level of retail services.  $X_{jt}$  includes both observables such as income growth and unobservables such as introduction of new consumer goods (e.g., iPods) and technology shocks in the retail sector (e.g., on-line shopping).

Labor supply to the retail sector in county  $j$  at time  $t$  is written as:

$$(5) \quad L^s_{jt} = g(w_{jt}).^{36}$$

Initially, Wal-Mart chooses an expansion plan  $WM^P_j = (o_{j1}, o_{j2}, \dots)$  in county  $j$  to maximize future expected profit from operation in that county:

$$(6) \quad \underset{WM^P_j = (o_{j1}, o_{j2}, \dots)}{\text{Max}} \sum_t \rho^t [r(WM_{jt}, X_{jt}^e) - c(\sum_i DIST^i_j \times t, w_{jt}^e)],$$

where  $\rho^t$  is a discounting factor,  $X_{jt}^e$  is the expected values of all exogenous factors that shift retail supply and demand of retail services, and  $w_{jt}^e$  the expected wage rate in county  $j$  at time  $t$ ;  $r(\cdot)$  and  $c(\cdot)$  are the revenue and cost functions. Costs include not only future wages but also costs related to management, distribution, and advertising, which are assumed to be a function of the interaction between time and distance to Wal-Mart headquarters ( $DIST$ ). For example, management and control costs may have initially been high for areas far from Wal-Mart's but fallen as Wal-Mart's expansion reached these areas.

Solving the maximization problem in equation (6) yields Wal-Mart's optimal number of new openings in county  $j$  at time  $t$  (for all  $t$ ):

$$(7) \quad o_{jt}^* = o_{jt}^*(\sum_i [DIST^i_j \times t], X_j^e, w_j^e),$$

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<sup>36</sup> This can also depend on other variables that may overlap with  $X$ , which we suppress to focus on essentials.

where  $X_j^e = (X_{j1}^e, X_{j2}^e, \dots)$  and  $w_j^e = (w_{j1}^e, w_{j2}^e, \dots)$ .

Substituting (7) into (4), we obtain:

$$(8) \quad L_{jt}^d = f(w_{jt}, WM_{jt} \{o_{jt-1}^* [\sum_i [DIST^i_j \times (t-1)], X_j^e, w_j^e], \\ o_{jt-2}^* [\sum_i [DIST^i_j \times (t-2)], X_j^e, w_j^e], \dots\}, X_{jt}).$$

Assuming the labor market clears in every time period, the equilibrium condition  $L_{jt}^d = L_{jt}^s$  gives wage and employment equations as follows:

$$(9) \quad \begin{aligned} w_{jt}^* &= w_{jt}^* [WM_{jt} (o_{jt-1}^*, o_{jt-2}^*, \dots), X_{jt}] \\ L_{jt}^* &= L_{jt}^* [WM_{jt} (o_{jt-1}^*, o_{jt-2}^*, \dots), X_{jt}] \end{aligned}$$

where we have assumed that, in equilibrium, Wal-Mart's expectations of exogenous factors and wage rates equal their realized values:

$$(10) \quad X_{jt}^e = X_{jt}, \quad w_{jt}^e = w_{jt}^*, \quad \text{for all } j \text{ and all } t.^{37}$$

Equations (7), (9), and (10) have the following three implications that are consistent with the empirical framework we use. First, local wages and employment are affected by Wal-Mart openings. Second, Wal-Mart openings are related to expectations (and realizations) of any factors that affect the time path of retail services, some of which can be observed and others not.<sup>38</sup> And third, Wal-Mart openings are affected by distance interacted with time, which is therefore a candidate IV. While a measure of total payrolls is included in our regressions, we assume that year and distance ("ring") fixed effects (in equation (1')) are appropriate controls for other factors in  $X_{jt}$ . Thus, in this simple framework, the equations we estimate can be interpreted as reduced forms for the labor demand and supply equations, in which Wal-Mart's presence is endogenous.

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<sup>37</sup> For the simplicity of exposition, here we impose a rather strong equilibrium condition, which is not absolutely necessary. For example, we may assume that Wal-Mart's expectations are good enough that their decisions remain optimal *ex post*. That is, if Wal-Mart opened a store last year based on its prediction of its impact on the local wage rate, some impact must be realized later so that Wal-Mart's decision in the previous year is still a good one in all years thereafter. Another simple alternative is simply to assume that Wal-Mart can perfectly predict the future, and write this down as an assumption instead of an equilibrium condition.

<sup>38</sup> Notice that all these factors in  $X_{jt}$ , together with Wal-Mart's entry, also determine the equilibrium prices of retail services/goods. By predicting all the factors affecting the equilibrium level of retail services, Wal-Mart also predicts the equilibrium prices of retail services/goods although the expected prices do not show up in these equations.

## V. Empirical Results

### *Descriptive Statistics*

Descriptive statistics for population, employment, and payroll are reported in Table 1. Most of the statistics are for the A sample, with the exception of those for general merchandising. The other (unreported) statistics for the B sample were similar, although counties in the B sample are larger as we would expect since data are less likely to be suppressed for larger counties and counties with larger retail sectors. On average, counties have approximately 78,000 residents. The second row indicates that the data cover 3,032 counties, and that 98 percent of all counties are represented in the A sample.

The average level of aggregate retail employment is 5,659, which is about 5.5 percent of total employment.<sup>39</sup> The level of employment in general merchandising is about one-sixth of the retail total, or 966 workers on average. Average aggregate retail payrolls are around \$92 million, and general merchandising payrolls are about \$14 billion. Although not reported in the table, average retail payrolls per worker across counties is around \$13,700, and the figure for general merchandising is around \$13,100. Finally, Table 1 also reports figures for manufacturing employment, which is a bit higher than retail employment, as well as manufacturing payrolls, which are much higher, reflecting higher payrolls per worker in the manufacturing sector (about \$27,500).

Table 2 provides some descriptive statistics on Wal-Mart stores, as of 1995. As indicated in the first row, for counties observations with one or more stores open the average number of stores is 1.43. The second row indicates that just over half of counties have at least one Wal-Mart store. The remaining rows of the table report the distribution of number of stores per county (for counties with one or more stores). Around 78 percent of the counties with Wal-Mart stores have only one store, about 13 percent have two stores,<sup>39</sup> and around four percent have three stores. There is then a smattering of observations with more stores (with a maximum of 17 stores at the end of the sample period in Harris County, Texas, which includes Houston).

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<sup>39</sup> On average across counties, the employment rate is approximately 0.26 (or 260 per 1,000). From other data sources, if we exclude categories not covered by CBP, the national employment rate is around 0.3. But if employment rates vary by county and counties differ in population, we would not expect the average employment rate across counties to match this exactly.

### *Preliminary Evidence on Endogeneity Bias*

Prior to turning to the OLS and IV estimates, it is useful to ask whether we can detect evidence of endogeneity, and infer something about the direction of endogeneity bias, without relying explicitly on the identifying assumptions underlying the IV estimation. Ultimately, as the earlier discussion makes clear, the key endogeneity problem comes from the possibility that Wal-Mart store openings are correlated with expected future retail employment growth (in the absence of stores opening). However, one way to get some indirect evidence on endogeneity is to look at the relationship between past growth in retail employment and decisions to open Wal-Mart stores.

A recent study commissioned by Wal-Mart (Global Insight, 2005) uses this approach. It looks at the counties that Wal-Mart entered, and calculates retail employment per capita growth rates in the five years preceding Wal-Mart's entrance. It reports that in 45 percent of these counties growth was faster than for the nation overall, while in 55 percent it was slower, and on this dismisses the endogeneity problem. However, this calculation is problematic and potentially misleading for two reasons. First, growth rates for the nation as a whole appear to have been computed over the whole sample period. This implies that growth rates for counties in the five years before Wal-Mart entered are being compared to national growth rates from other periods. It is entirely possible that counties that Wal-Mart entered were growing faster than the national average for the corresponding time period, although not relative to the national average for the whole sample period.

Second, the discussion of Wal-Mart's growth strategy indicates that it is inappropriate to compare growth rates in counties where stores opened to those in counties nationwide. The maps in Figures 1 and 2 clearly indicate that the typical choice about where to open a Wal-Mart store in any period was not between a county in Arkansas and a county in Idaho. Rather, the correct comparison for any period is between counties in the approximate geographic region in which stores were being opened in that period (oversimplifying, on the circle of appropriate radius around Benton County, Arkansas). That is, we should choose a particular period, identify the region in which many stores were opening, and ask whether—within that region—stores were opening in fast-growing counties. Another way to see this is

that what we are fundamentally concerned about is endogeneity bias in equation (1'), for example, which conditions on year and distance.

We pursued this strategy for different selected periods and regions to give a sense of what this calculation yields throughout the sample. We first focused on store openings in the early 1980s, in states close to Arkansas, restricting attention to openings in the 1983-1985 period. We computed openings per state, and chose the ten states with the highest number of openings. These ten states are shaded in the first map in Figure 3, which clearly shows that these are states in relatively close proximity to Arkansas (as well as Florida). The map also shows the same openings, in the 1981-1985 period, that were displayed in Figure 2. The overlaying of these openings and the shaded states makes clear that these were the states in which openings were concentrated in this period. For each county in these ten states, we computed the annualized rate of growth of retail employment over the immediately preceding five-year period 1977-1982 (paralleling the Global Insight calculation). We then estimated linear probability models for whether Wal-Mart entered a county (opening its first store, which, as Table 2 shows, would be the case for most openings), as a function of the prior growth rate of retail employment. We add controls for county population as well as state dummy variables, to account for the effects of population density as well as other unspecified features of states on store openings. We then did the same for the 1988-1990 period, using retail employment growth from 1982-1987, and the 1993-1995 period, using retail employment growth from 1987-1992. The corresponding maps are shown in the second and third panels of Figure 3.

The results, which are reported in Table 3, indicate that Wal-Mart stores in fact entered counties—among an appropriate comparison group—that had previously had faster retail employment growth. For the 1993-1995 estimation, the set of ten states with the most openings includes California and Washington, which are quite far geographically from the other eight states; we therefore also show results excluding these two states. As the table shows, in every case the relationship between prior

growth and whether a store opened is positive (and sometimes significant, although that is not of foremost interest here).<sup>40</sup>

This evidence does not directly address the issue of endogeneity bias—which concerns the link between store openings and future retail employment absent store openings.<sup>41</sup> But it does show that a calculation paralleling that used in the Global Insight study, but using an appropriate comparison group, does in fact suggest that endogeneity is a concern.

### *First-Stage Estimates*

Turning to our main analysis, we first provide information on the first-stage estimates. Figure 4 shows estimates of the coefficients of the distance-time interactions in equation (3), which identify the model, although for ease of interpretability we present estimates for the change in the number of stores, rather than the number of stores per person. The omitted distance category is 900-1000 miles. As a consequence, for each distance range except this one the corresponding panel displays how the probabilities of store openings vary by year, relative to the pattern for the counties 900-1000 miles from Benton County, Arkansas. We chose to omit the year 1988, which is in the middle of the sample period, to make it easier to read off of the figures how openings differ with distance at the beginning and end of the sample period. The baseline first-stage regression includes 272 distance-time interactions (between 17 year dummy variables and 16 distance dummy variables). In Figure 4, we break these up into the 16 distance groups; the first panel, for example, shows the estimated coefficients of the interactions between the year dummy variables and the dummy variable for distance 0-100 miles from Benton County, Arkansas.

Starting with the graphs for distances near Benton County, Arkansas, we see that store openings were more likely early in the sample period, and less likely later on. Conversely, at distances far from Benton County, Arkansas, store openings were much more common later in the sample period.

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<sup>40</sup> Growth rates are measured on an annualized basis. Thus, for example, the estimate in column (1) implies that a one-percent higher annual growth rate boosts the probability of a store opening by 0.0044; this is a 2.7 percent increase in the probability of a store opening.

<sup>41</sup> For this same reason, it is not sufficient to look at whether there is a change in the growth rate of retail employment as a function of whether a store opened to control for endogeneity, because store openings may be based on predicted future growth that is not simply a deviation from past trends.

Alternatively, looking across the graphs by distance, rather than across years for a given distance, we see that store openings were much more likely at nearby distances early in the sample, and at farther distances later in the sample. Thus, these regressions confirm the impression from the maps in Figure 2 that store openings spread out over the sample period, concentrated near Benton County early in the sample period and farther from Benton County later. It also turns out that the F-statistic for the distance-time interactions in the first stage is always very large, as reported in the tables that follow.

### *Effects on Retail Sector Employment*

Table 4 turns to results on the effects of Wal-Mart stores on employment in the retail sector. In this case the dependent variable is the change in retail employment per person at the county level, and the Wal-Mart measure is the change in the number of stores per person. We report estimates both with and without distance-specific trends.

The OLS estimates in Table 4 point to increases in the aggregate retail sector and in general merchandising resulting from Wal-Mart stores opening, with the point estimates in the range of 23-32.<sup>42</sup> These estimates are insensitive to the inclusion of distance-specific trends. For the A sample, the estimate implies an increase in aggregate retail employment of about 0.6 percent at the mean. The estimate for general merchandising is close to but slightly less than the estimate for aggregate retail, for the B sample for which these estimates are comparable. The estimates suggest that, although Wal-Mart would be expected to lead to growth in general merchandising employment (where Wal-Mart is located), there is no displacement of employment from the rest of the retail sector; if there were, then even if aggregate retail employment did in fact increase, we would expect the latter increase to be smaller than the increase in general merchandising. This is perhaps the first hint that something is amiss in the OLS estimates. Finally, note that, as we would expect, the estimated coefficient of total payrolls per person is positive.

In contrast to the OLS estimates, the IV estimates—which conditional on the identification strategy being valid are interpretable as causal effects of Wal-Mart openings on retail employment—point to employment declines in the aggregate retail sector. Without distance-specific trends, the estimates for

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<sup>42</sup> These estimates are about the same order of magnitude as the long-run OLS estimates reported in a recent study commissioned by Wal-Mart (Global Insights, 2005, Table 11) as well as Basker (2005b).

the A sample indicate that a Wal-Mart store opening reduces employment at the county level by about 194 workers. With distance-specific trends the estimate falls to 178. In the B sample the estimates are smaller by about 40. Since the average number of workers in a Wal-Mart store is about 360 (Basker, 2005b), the estimated employment decline (using a figure of 180) implies that that each Wal-Mart worker takes the place of 1.5 retail workers. On a county basis, this estimate implies a 3.2 percent reduction in retail employment attributable to a Wal-Mart store opening.

This strikes us as a plausible estimate. First, casual observation suggests that staffing levels at big-box retailers are much lower than at small retailers. In addition, a negative employment effect would seem to be the natural expectation if Wal-Mart is indeed more efficient and can be profitable while charging significantly lower prices. Of course, there is potentially a positive effect from Wal-Mart's greater efficiency, if lower prices in retail boost consumer demand enough through either income or substitution effects that overall retail sector employment increases. The estimates suggest, however, that this is not the case. Also, this "retail scale" effect may be small because the types of products sold at Wal-Marts and the retail establishments with which they compete may be more likely to be inferior goods, so that much of the increase in real income stemming from lower prices is spent in other sectors of the economy (including higher-end retail).

For general merchandising, the IV estimates continue to point to increases in employment, and the IV estimates are similar to the OLS estimates (a bit larger without the distance-specific trends, and a bit smaller with them). We would expect increases in general merchandising employment since this is the sector in which Wal-Mart is classified. The evidence that the estimated increase in general merchandising employment is considerably less than the size of an average Wal-Mart store suggests that Wal-Mart reduces employment at other employers in the general merchandising sector, rather than solely in the remainder of the retail sector. Thus, the estimates are consistent with Wal-Mart reducing overall retail employment, although shifting its composition somewhat toward general merchandising.

The finding that the OLS estimates of employment effects for the aggregate retail sector are generally positive, and the IV estimates negative, is consistent with Wal-Mart endogenously locating

stores in places where retail growth is increasing, as we might expect. As shown in the table, there is always statistically significant evidence of endogeneity bias in the aggregate retail sector.

On the other hand, we find no evidence of endogeneity in the estimates for general merchandising. It is not clear that we would expect any endogeneity bias in the estimated effect of Wal-Mart on employment in this particular subsector of retail. Wal-Mart may well have chosen to locate in areas where retail growth was likely to be strong, but the composition of the retail sector is more likely to be a direct consequence of where Wal-Mart chose to locate.

#### *Effects on Retail Sector Earnings*

Table 5 turns to effects on retail sector earnings. Everything is the same as in the employment equations, except that the dependent variable is now the change in retail payrolls per person; again, because the Wal-Mart variable is the change in the number of stores per person, its estimated coefficient measures the effect of a Wal-Mart opening on total retail payrolls in the county. The first three columns report results without distance-specific trends, and the last three columns including these trends. The results are similar in both cases.

The OLS estimates indicate that Wal-Mart store openings increases retail payrolls by about \$0.5 million, whether we look at aggregate retail or just the general merchandising sector. In contrast, the IV estimates indicate that aggregate retail payrolls fall when Wal-Mart stores open, by approximately \$2.3 to \$2.8 million, for the A sample. (The range is slightly lower for the B sample.) However, the positive effect of Wal-Mart openings on payrolls in general merchandising remains, although the estimates are smaller than the OLS estimates.

Not surprisingly, the payroll results parallel those for employment. Of course, much of the public debate over Wal-Mart concerns the levels of wages they pay, and whether they drive down wages of their competitors. There is, as yet, no direct evidence on this question. As pointed out earlier, because the CBP data do not distinguish part-time from full-time workers, the data cannot be used to compute hourly wages. In addition, the data do not allow any controls for individual worker characteristics that would be required to determine whether wages were changing for comparable workers. Nonetheless, we can put

together the employment and payroll results to draw some inferences. The employment results indicated that a Wal-Mart store opening reduces retail employment by 180 workers. As noted earlier, average earnings per retail worker in the sample are \$13,700. Combining these two numbers implies a decline in retail earnings of \$2.47 million, which is right in the middle of the range of the direct payroll estimates. Thus, loosely speaking, it does not appear that Wal-Mart openings have much impact on retail earnings per worker, although again we remind the reader that the implications for retail wages are unclear. For example, if Wal-Mart induces a shift toward older workers, or toward full-time workers, constant retail earnings per worker would be consistent with a decline in wages rates for comparable workers.<sup>43,44</sup> Nonetheless, the combined estimates unambiguously imply that overall earnings in the retail sector decline after Wal-Mart stores open.

#### *Robustness and Other Analyses*

We next describe some additional analyses exploring the robustness of our results and some other issues. We focus on the aggregate retail estimates for the fullest (A) sample. The first row of Table 6 repeats the baseline results from Tables 3 and 4, for purposes of comparison, and the other analyses follow.

First, we have included a control for total payrolls per person in all of the specifications reported thus far. Because this variable includes retail payroll, it is potentially endogenous. We therefore report two additional specifications: one with retail payrolls subtracted out of total payrolls; and the other simply dropping the total payrolls control. The qualitative conclusions are very similar, although both changes—

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<sup>43</sup> The Global Insight (2005) study reports—based on a representative sample of Wal-Mart workers from October 2004—that 75 percent of the company’s workers are full-time employees, although it does not provide the definition of full-time. (It also states that the company’s full-time and part-time workers are “paid on the same scale” (p. 15), which we presume means the same hourly wage.) This number is close to the retail average share working full-time based on CPS data (<http://www.careervoyages.gov/pdf/retail-profile-504.pdf>, as of December 21, 2005).

<sup>44</sup> In principle, one could study these issues using data from the Current Population Survey (CPS) or the Decennial Census of Population. However, in the CPS most county identifiers are suppressed for reasons of confidentiality. Census data are less attractive because they are not available for each year but only once a decade. Furthermore, in downloadable Census estimates by county from all long-form respondents, neither hours, education, nor income are available by industry. This leaves the option of using public use samples from the Census. But these offer relatively few observations per county, which coupled with having data only for one year per decade implies that the resulting estimates would likely be uninformative.

and more so dropping the total payroll control—yields slightly larger negative effects on retail employment and payroll.

Second, we sharpen the identification by dropping counties that never had a Wal-Mart store during the sample period. In this case, identification of the effects of Wal-Mart stores comes only from the time-series variation in store openings for the set of counties that got a store. This provides a potentially “cleaner” control group that omits observations on counties in which stores never opened. The results are qualitatively similar, although the estimated negative employment effects are smaller by around 20-35 percent, and the estimate payroll effects are smaller by around 10-25 percent.

Third, we consider some other differences in the sample definition. We first report estimates dropping the small number of counties with stores that closed; these estimates are very close to the baseline estimates.<sup>45</sup> Following that, we report estimates extending through the entire period covered by the data—ending in 2002 rather than 1995—even though, as discussed earlier, the identifying relationship between time, location, and store openings is strongest through 1995. For the longer period the evidence suggests sharper declines in retail employment and payroll; the estimates are larger by about 35 percent for the specifications excluding the distance-specific time trends, and by a bit more when the time-trends are included.

Fourth, we have thus far ignored information on Sam’s Clubs. We do not necessarily want to treat these as equivalent to other Wal-Mart stores, so we study them in two ways. First, we omit all counties that had a Sam’s Club at some point during the sample period; 97 percent of these counties also had a Wal-Mart. Second, we use the full sample, but recalculate the exposure measure treating each Sam’s Club store like other Wal-Mart stores. As the table shows, the results dropping the counties with Sam’s Clubs are qualitatively similar, although the estimated effects are smaller, especially for retail payrolls. The results are quite insensitive to simply treating Sam’s Clubs like other Wal-Mart stores.

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<sup>45</sup> An alternative is to incorporate information on these counties resetting exposure to zero when a store closes. However, we are skeptical that store closings and openings have symmetric (opposite-sign) effects, and therefore chose instead to report this sensitivity analysis.

Fifth, to this point we have simply used a count of Wal-Mart stores (on a per capita basis) to measure exposure to Wal-Mart. But store size varies—for example, counties with smaller populations may get smaller stores<sup>46</sup>—and the exposure measure should perhaps take this into account. Thus, we computed an exposure measure that weights by store size relative to the average store size. These estimates are reported in the next row, and reveal somewhat larger effects than those estimated using the baseline specification.

The final estimates for retail use a more restricted set of instrumental variables. Specifically, we use linear distance interacted with year dummy variables, reducing the number of instruments to 17. The estimates are of the same sign, but larger. However, all of the estimates are much less precise, which is not surprising given the loss of information, and the F-statistics for the first stage are much lower.<sup>47</sup> Nonetheless, the qualitative conclusions are similar with these restricted sets of instruments.

Finally, the last two rows of the table report results for manufacturing, rather than retail. There is no clear prediction for the effects of Wal-Mart on employment in other industries. For the most part, we would expect that the displacement of employment from the retail sector would have little impact on overall employment, as markets adjust, and we would not expect strong effects on industries not closely related to retail. Of course, Wal-Mart may have impacted the manufacturing industry through cost pressures, but these effects would not necessarily be manifested in the local labor markets in which Wal-Mart stores are located. Thus, we would not expect estimates for manufacturing to give strong evidence of employment or payroll effects. Conversely, we would regard consistent evidence of declines in manufacturing employment and payrolls paralleling the results for retail as suggesting that the retail

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<sup>46</sup> This may in part reflect whether or not the Wal-Mart store is a Supercenter.

<sup>47</sup> An alternative restriction on the instruments is to use distance dummy variables interacted with a linear time trend. But Figure 4 indicates quite clearly that variation in Wal-Mart store openings at different distances from Benton County (the variation depicted in each panel) is not well approximated by a linear function of time. As another approach to restricting the set of instruments, we used dummy variables for two-year instead of one-year intervals, and for 200-mile distance ranges in defining the distance dummy variables. The corresponding model is estimated for two-year first differences. (This is clear if one first starts with a model in levels with dummy variables for two-year intervals, because these same dummy variables then appear in the model for two-year first differences.) The estimates were very similar to the baseline estimates, although for the A sample indicating sharper payroll reductions.

results are spurious, reflecting a relationship between the instrumental variables and overall economic conditions, rather than an effect of Wal-Mart.

The OLS estimates for manufacturing indicate no significant effects on manufacturing employment or payroll, although the point estimates are negative. The IV estimates do not paint a consistent picture. With the distance-specific trends excluded there is no significant employment effect, and the estimated payroll effect is very large (\$7.6 million) and statistically significant. In contrast, with the distance-specific trends included, the estimated employment effect is negative and significant, but the payroll effect is positive (and insignificant). Overall, then, the IV estimates for manufacturing do not give a clear indication of any effect of Wal-Mart. The results are either insignificant or inconsistent (such as no effect on employment and a positive effect on payrolls, or a negative effect on employment but no effect on payrolls).<sup>48</sup> Moreover, in three of the four cases—and both for employment—we do not reject exogeneity of Wal-Mart openings, in which case we should rely on the OLS estimates that uniformly indicate no effect of Wal-Mart on manufacturing employment or payrolls. Of course, there is less of an expectation that Wal-Mart openings would be endogenous with respect to changes in the manufacturing sector than changes in the retail sector.

## **VI. Conclusions and Discussion**

Motivated in large part by local policy debates over Wal-Mart store openings, and the large size of Wal-Mart relative to the retail sector, we estimate the effects of Wal-Mart stores on retail employment and earnings. Critics have charged that Wal-Mart's entry into local labor markets reduces wages and employment, and the company (and others) have countered that these claims are false, and touted Wal-Mart's job creation.

Our analysis emphasizes the importance of accounting for the endogeneity of the location and timing of Wal-Mart openings that—in our view, and as borne out by the data—is most likely to bias the evidence against finding adverse effects of Wal-Mart stores. Our strategy for addressing the endogeneity

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<sup>48</sup> The sensitivity of the estimates to the exclusion or inclusion of the distance-specific trends—in contrast to the results for retail—suggest that the problem may stem from correlations between the instruments and other sources of changes in manufacturing employment and earnings.

problem is based on a natural instrumental variable that arises because of the geographic and time pattern of the opening of Wal-Mart stores, which slowly spread out in a wave-like fashion from the first stores in Arkansas.

The findings in this paper rather strongly belie claims, as well as research findings, suggesting that Wal-Mart store openings lead to increased retail employment. On average, Wal-Mart store openings—over the length of time observed in our sample—reduce retail employment by about 3.2 percent, implying that each Wal-Mart employee replaces about 1.5 employees in the rest of the retail sector. Driven by the employment declines, retail earnings at the county level also decline as a result of Wal-Mart entry, by about 2.8 percent. It is harder to draw any firm conclusions regarding the effects of Wal-Mart on wages, although the data do not provide much indication that retail earnings per worker are affected by Wal-Mart openings.

On balance, then, our evidence on the labor market effects of Wal-Mart is more consistent with the claims of Wal-Mart's critics. However, the retail employment declines associated with Wal-Mart do not necessarily imply that Wal-Mart stores worsen the economic fortunes of residents of the markets that these stores enter. Our results apply only to the retail sector, and we suspect that there are not aggregate employment effects, at least in the longer run, as labor shifts to other uses. Wage effects are more plausible, although these may operate more on the manufacturing side through the buying power that Wal-Mart exerts, than on the retail side which is a low-wage sector regardless of Wal-Mart—although there are exceptions such as relatively highly-paid grocery workers who may be harmed in competition with Supercenters. If the wage effects arise through cost pressures on Wal-Mart's suppliers, however, they would not necessarily be concentrated in the counties in which stores open, so that our methods would not be appropriate for trying to estimate these effects.

Moreover, Wal-Mart entry may also result in lower prices that increase purchasing power, and if prices are lowered not just at Wal-Mart, but elsewhere as well, the gains to consumers may be widespread. Furthermore, the gains may be larger for lower-income families (Hausman and Leibtag, 2005), although it is also possible that labor market consequences for these families are also more adverse.

Another line of criticism of Wal-Mart is that through lowering wages it increases the burden on taxpayers. A report by the Democratic Staff of the Committee on Education and the Workforce of the U.S. Congress (Miller, 2004) claims that because of Wal-Mart's low wages, an average Wal-Mart employee costs federal taxpayers an extra \$2,103 in the form of tax credits or deductions, or public assistance such as healthcare, housing, and energy assistance (see also Dube and Jacobs, 2004). There are many heroic assumptions needed to construct such estimates, and this is not the place to dissect them. However, a key implicit assumption is that in the absence of Wal-Mart, employees of the company would have higher-paying jobs, rather than, for example, no jobs. Thus, the validity of this criticism hinges on whether Wal-Mart's entry into a labor market affects overall employment and wages. We have shown that Wal-Mart reduces retail employment and earnings (but not necessarily wages), but this does not imply that employment or earnings decline overall. An open question, which our results do not address, is the effect of Wal-Mart on wages at suppliers—effects that would generally not be concentrated in the local labor markets where Wal-Mart stores open. It is also worth pointing out that if Wal-Mart causes both earnings and price declines, then taxpayer burden could increase even if the price declines more than offset the earnings declines for low-income families.

Thus, aside from the question of employment effects, there are numerous remaining questions of considerable interest regarding the effects of Wal-Mart on labor markets and goods markets, on consumption, and on social program participation and expense. The identification strategy developed in this paper may prove helpful in estimating the effects of Wal-Mart stores on some of these other outcomes as well.

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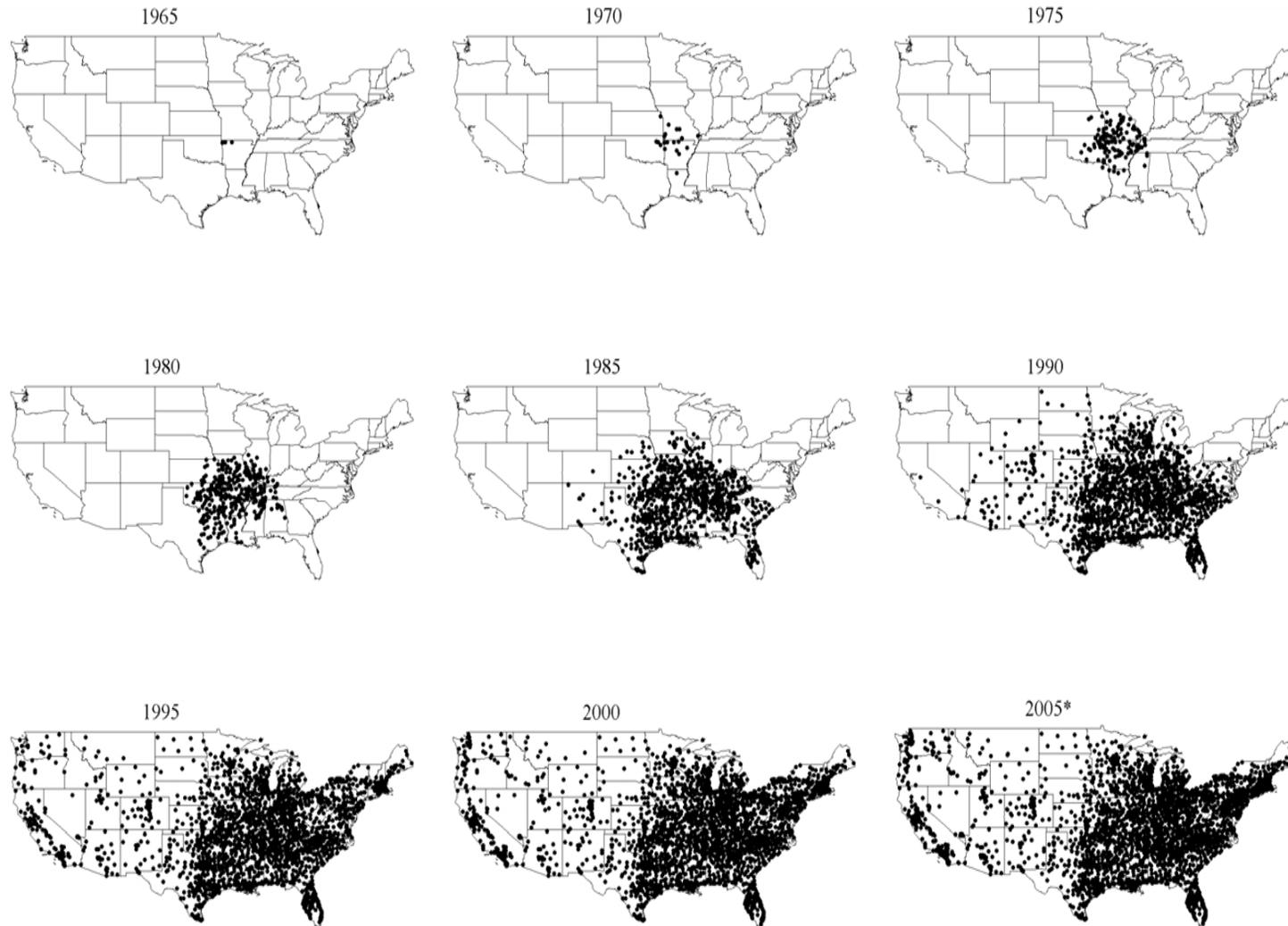
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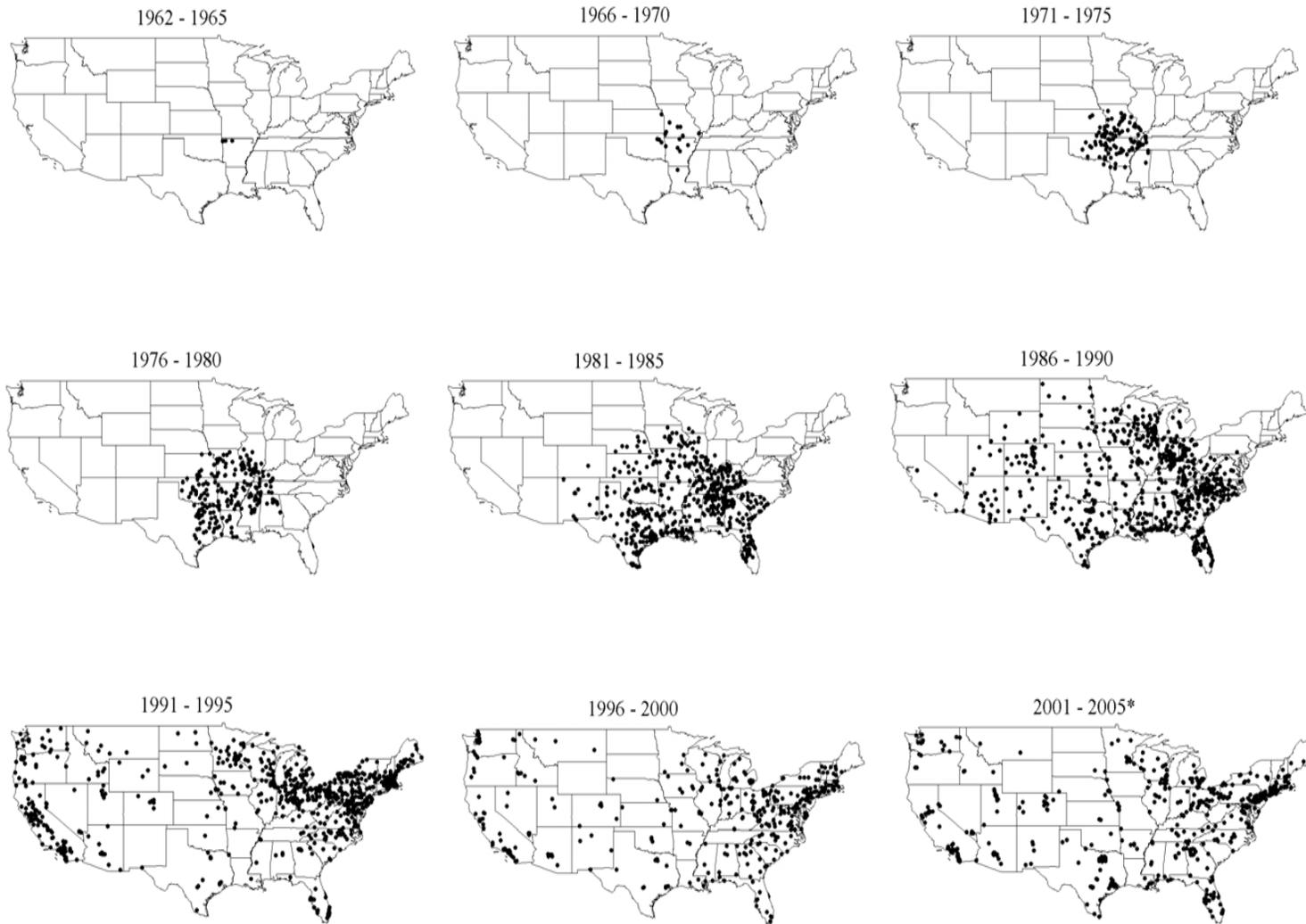
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**Figure 1: Location of Wal-Mart Stores, 1965-2005**



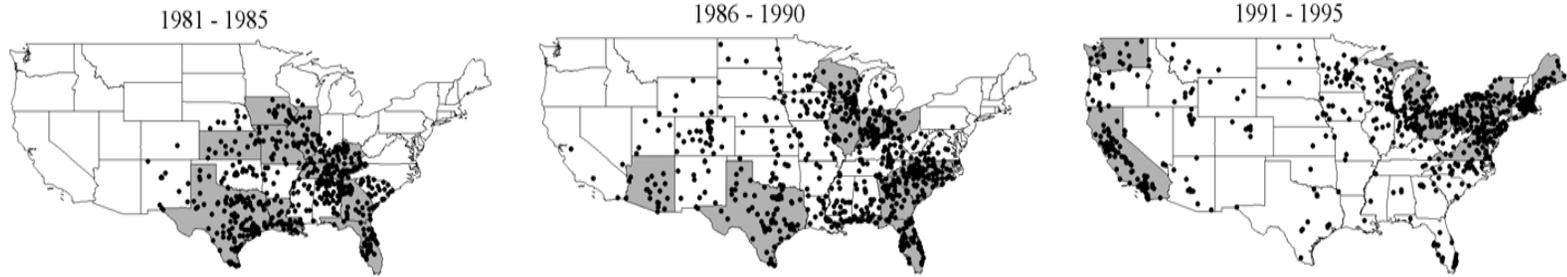
\* Includes all locations as of January 31, 2005.

**Figure 2: Location of Wal-Mart Openings, 1962-2005**

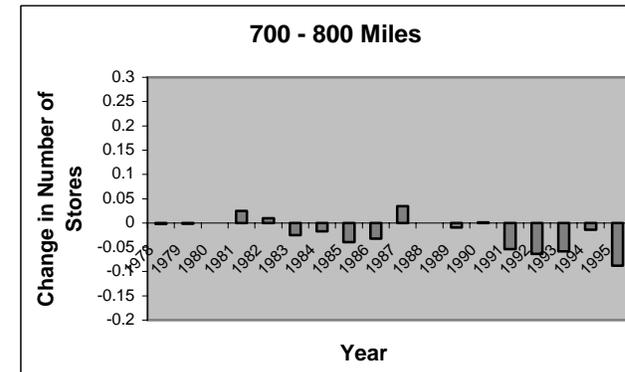
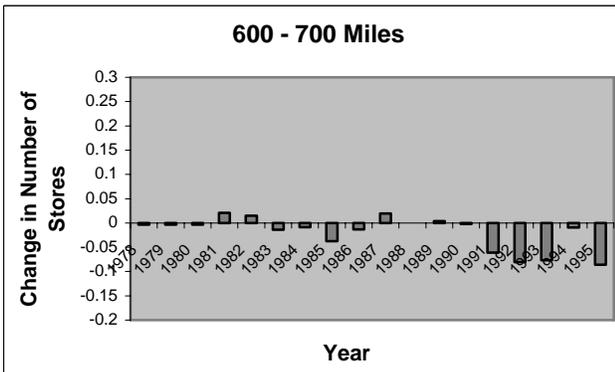
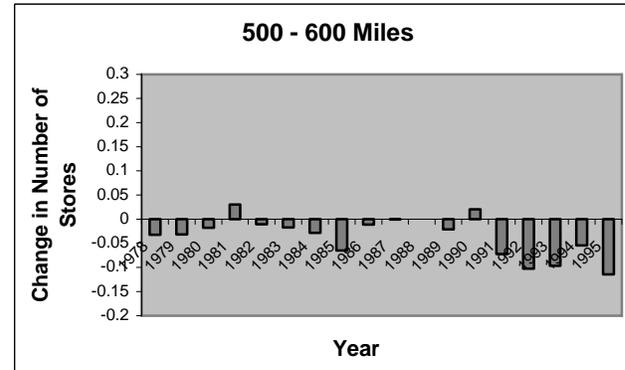
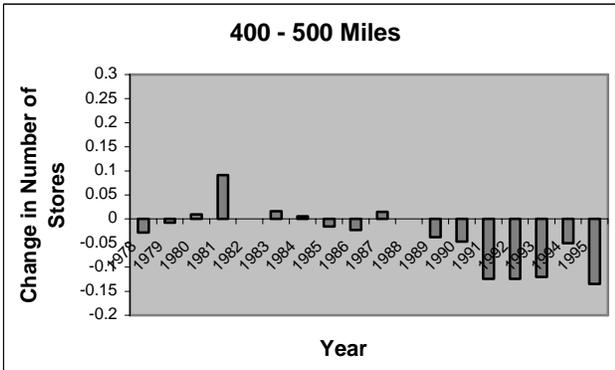
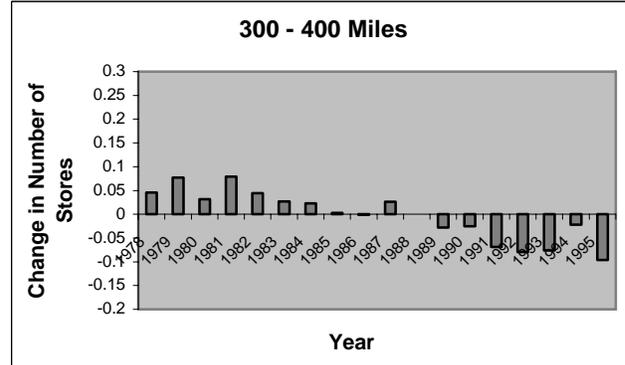
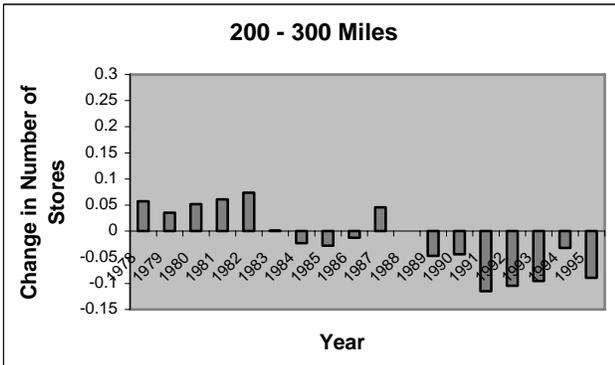
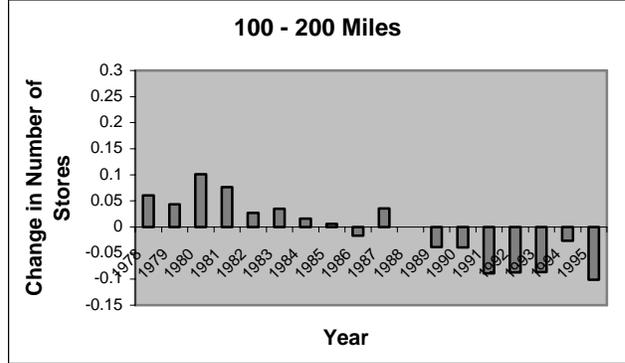
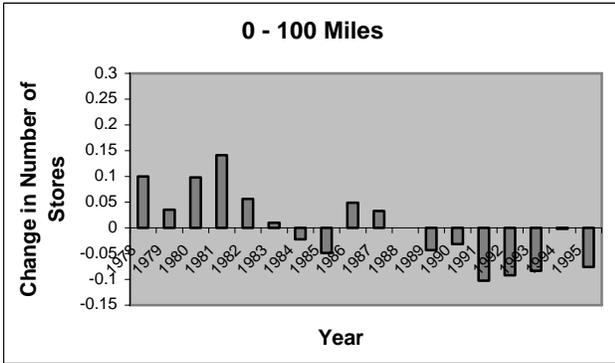


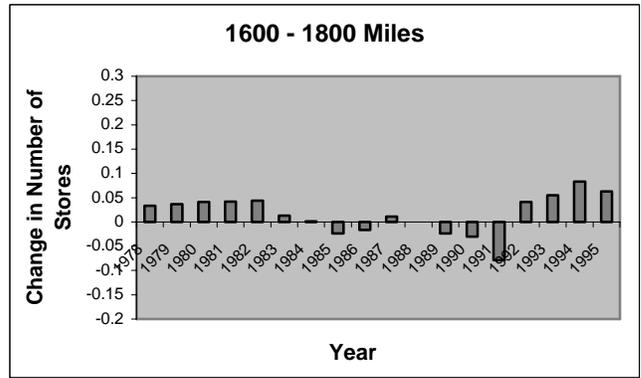
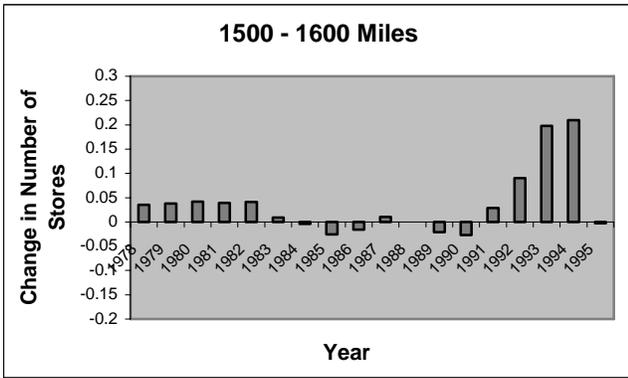
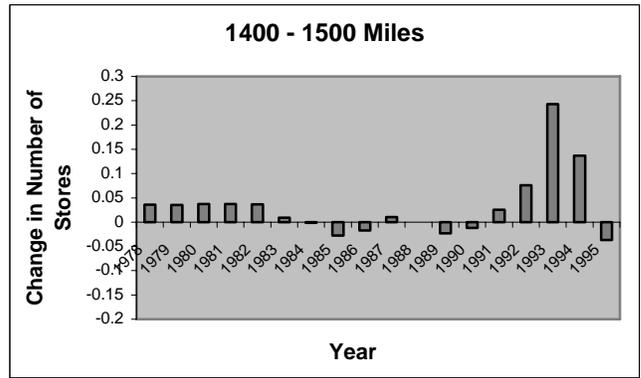
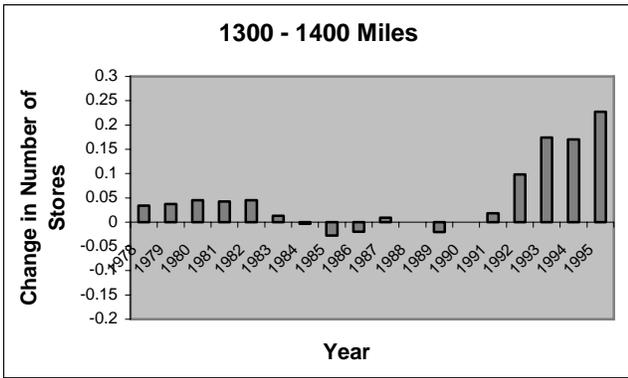
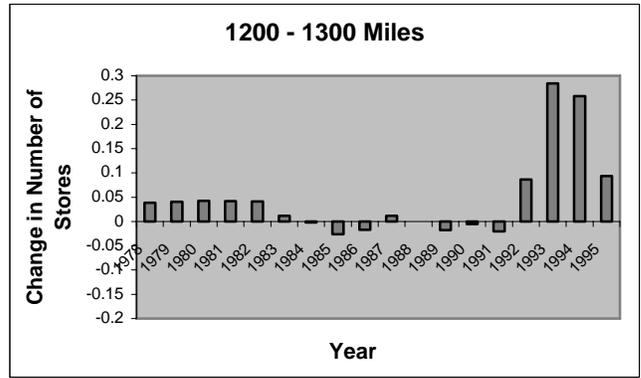
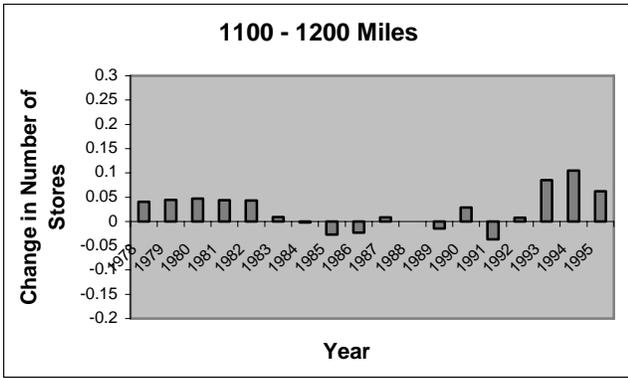
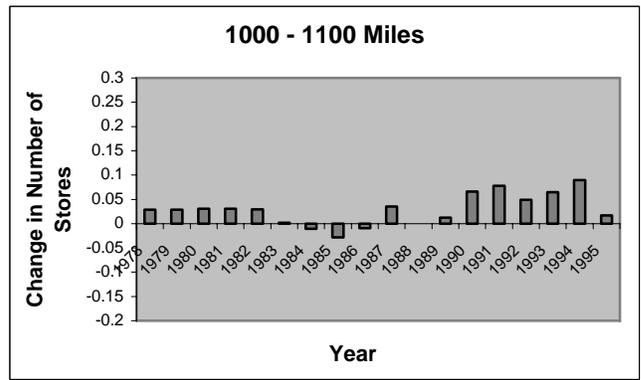
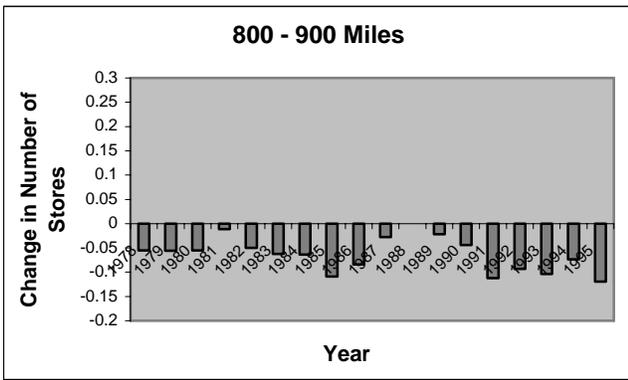
\* Includes all openings as of January 31, 2005.

**Figure 3: Geographic Concentration of Wal-Mart Openings by Period and Geographic Area**



**Figure 4: First-Stage Results for Change in Number of Stores**





Panels show estimated coefficients of radius-year interaction from estimates of equation (3), although estimated for the change in the number of stores rather than the number of stores per person. The omitted reference categories are 1988 and 900-1000 miles.

Table 1: County-Level Summary Statistics, Population, Employment, and Payroll, 1977-1995

	Means (1)	Medians (2)
Population	78,223 (256,595)	22,644
Number of counties/share of total	3,032 0.98	...
Aggregate retail employment (N=57,964)	5,659 (19,758)	1,105
Retail payrolls (\$1,000's) (N=57,964)	92,208 (364,022)	14,990
General merchandising employment (B sample, N=37,999)	966 (2,691)	195
General merchandising payrolls (\$1,000's) (B sample, N=37,999)	14,170 (43,099)	2,477
Manufacturing employment (N=51,579)	7,039 (26,428)	1,651
Manufacturing payrolls (\$1,000's) (N=51,579)	256,161 (1,062,647)	42,481

Figures are for full (A) sample except for general merchandising. In column (1), standard deviations are reported in parentheses. Payrolls are in thousands of 1999 constant dollars.

Table 2: County-Level Summary Statistics, Wal-Mart Stores, 1995

Average number of stores, for counties with stores	1.43 (1.16)
Share with one or more stores open, all counties	0.505
Share with given number of stores, for counties with stores	
1 store	0.783
2 stores	0.128
3 stores	0.042
4 stores	0.017
5 stores	0.013
6 stores	0.006
7 stores	0.004
8 stores	0.003
9 stores	0.002
10 stores	0.001
11 stores	0.001
12 stores	<0.001
13 stores	<0.001
14 stores	<0.001
15 stores	<0.001
16 stores	<0.001
17 stores	<0.001
Number of counties	3,064

Numbers are for full (A) sample of observations with aggregate retail employment data in 1995.

Table 3: Estimated Relationships Between Store Openings and Prior Retail Employment Growth

Openings in:	1983-1985	1988-1990	1993-1995	1993-1995, Excluding CA and WA
	(1)	(2)	(3)	(4)
Annualized Rate of Growth of Retail Employment in Previous Five-Year Period	0.439*** (0.143)	0.191 (0.177)	0.122 (0.515)	0.407 (0.613)
N	854	615	363	299

Estimates are from linear probability models, with standard errors robust to heteroscedasticity. The states included in each sample are shown in Figure 3. The previous five-year periods corresponding to the three columns are: 1977-1982, 1982-1987, and 1987-1992. County population in the ending year of these five-year periods, and state dummy variables, are included as controls. Growth rates are computed relative to the average at the beginning and end of the sample period, because there are a handful of observations with retail employment of zero.

Table 4: Estimated Effects of Wal-Mart Stores on Retail Employment

	Baseline (Equation (1))			Including Separate Time Trends Based on Distance from Benton County (Equation (1'))		
	Agg. Retail	Agg. Retail	Gen. Merch.	Agg. Retail	Agg. Retail	Gen. Merch.
	(1) <i>A Sample</i>	(2) <i>B Sample</i>	(3) <i>B Sample</i>	(4) <i>A Sample</i>	(5) <i>B Sample</i>	(6) <i>B Sample</i>
<i>OLS</i>						
Wal-Mart stores	32.04*** (5.88)	25.13*** (7.97)	23.06*** (3.54)	32.78*** (5.91)	26.18*** (7.99)	22.84*** (3.55)
Total payrolls per person	0.001*** (0.0003)	0.001*** (0.0003)	0.0001** (0.00002)	0.001*** (0.0003)	0.001*** (0.0003)	0.0001** (0.00002)
Adjusted R <sup>2</sup>	0.083	0.108	0.041	0.084	0.109	0.042
<i>IV for Wal-Mart stores</i>						
Wal-Mart stores	-193.80*** (65.23)	-155.26*** (48.81)	28.39*** (9.20)	-178.18*** (56.12)	-137.25*** (43.27)	19.65* (10.60)
Total payrolls per person	0.001*** (0.0003)	0.001*** (0.0003)	0.0001** (0.00002)	0.001*** (0.0003)	0.001*** (0.0003)	0.0001** (0.00002)
F-statistics	1,818.4	827.9	827.9	539.2	72.3	72.3
Reject null of no endogeneity bias at 5% level	Yes	Yes	No	Yes	Yes	No
N	54,554	32,668	32,668	54,554	32,668	32,668

The Wal-Mart variable is the change in the number of stores per person, and the dependent variable is the change in employment per person. The coefficients measure the effect of one Wal-Mart store opening on county-level retail employment. The instrumental variables are interactions between dummy variables for years and dummy variables for counties with centroids with a radius of 100 miles from Benton County, Arkansas, 101-200 miles, 201-300 miles, etc., out to 1800 miles. All of the radii except the last cover a 100 mile range; the last covers 1601-1800 miles, because there is only one county beyond the 1700 mile radius. All specifications include year fixed effects, and the specifications in columns (4)-(6) also include the distance dummy variables (in both the first and second stages). Standard errors (shown in parentheses below the estimates) and F-statistics are calculated clustering on state, and hence allow arbitrary correlations across counties in a state contemporaneously and over time. The sample period covers 1977-1995. See notes to Table 1 for additional details. ‘\*’, ‘\*\*\*’, and ‘\*\*\*\*’ indicate the estimate is statistically significant at the ten-, five-, or one-percent level, respectively. The results for the test of the null hypothesis of no endogeneity bias were very similar for bootstrapped distributions for the difference between the OLS and IV estimates, with the bootstrapping based on counties rather than individual observations (the level of clustering used in the standard error calculations); this is in principle preferable to the conventional Hausman test (1978) because OLS is inefficient if there is either heteroscedasticity across counties or autocorrelation within counties.

Table 5: Estimated Effects of Wal-Mart Stores on Retail Payrolls (\$1,000's)

	Baseline (Equation (1))			Including Separate Time Trends Based on Distance from Benton County (Equation (1'))		
	Agg. Retail	Agg. Retail	Gen. Merch.	Agg. Retail	Agg. Retail	Gen. Merch.
	(1)	(2)	(3)	(4)	(5)	(6)
	<i>A Sample</i>	<i>B Sample</i>	<i>B Sample</i>	<i>A Sample</i>	<i>B Sample</i>	<i>B Sample</i>
<i>OLS</i>						
Wal-Mart stores	529.5*** (98.9)	473.7*** (116.1)	471.0*** (47.9)	539.6*** (99.2)	490.0*** (116.7)	468.3*** (48.0)
Total payrolls per person	0.023*** (0.006)	0.020*** (0.007)	0.001*** (0.0003)	0.023*** (0.006)	0.020*** (0.007)	0.001*** (0.0003)
Adjusted R <sup>2</sup>	0.111	0.152	0.071	0.112	0.153	0.072
<i>IV for Wal-Mart stores</i>						
Wal-Mart stores	-2,784.2*** (1,019.5)	-2,590.6*** (914.9)	342.4*** (102.1)	-2,314.7** (944.7)	-2,182.0** (892.7)	197.2* (106.8)
Total payrolls per person	0.024*** (0.006)	0.020*** (0.007)	0.001*** (0.0003)	0.024*** (0.006)	0.020*** (0.007)	0.001*** (0.0003)
Reject null of no endogeneity bias at 5% level	Yes	Yes	No	Yes	Yes	Yes
N	54,554	32,668	32,668	54,554	32,668	32,668

The notes from Table 4 apply. The only difference is that the dependent variable is the change in retail payrolls per person, so the coefficients now measure the effect of one Wal-Mart store opening on county-level retail payrolls (measured in units of thousands of 1999 constant dollars). The F-statistics for the first stage are as reported in Table 4.

Table 6: Estimates for Alternative Specifications, Samples, and Sectors

	Baseline (Equation (1))		Including Separate Time Trends Based on Distance from Benton County (Equation (1'))	
	Employment (1)	Payrolls (\$1,000) (2)	Employment (3)	Payrolls (\$1,000) (4)
<b>A. Retail</b>				
Table 4/5 estimates	-193.80*** (65.23)	-2,784.2*** (1,019.5)	-178.18*** (56.12)	-2,314.7** (944.7)
Subtract retail payroll out of total payroll per person control	-200.90*** (68.98)	-2,936.5** (1,116.9)	-185.43*** (59.13)	-2,473.5** (1,037.2)
Omit total payroll per person control	-205.22*** (72.12)	-2,997.0** (1,183.4)	-190.46*** (61.99)	-2,545.9** (1,101.8)
Drop counties that never have a store (N=30,957)	-126.41*** (39.04)	-2,080.1*** (658.3)	-142.91*** (31.60)	-2,127.5*** (639.2)
Drop counties with closed stores (N=53,746)	-192.93*** (65.68)	-2,736.5*** (1,018.7)	-175.87*** (57.04)	-2,242.8** (959.2)
Through 2002 (N=75,482)	-271.74*** (72.89)	-3,610.4*** (1,183.5)	-299.88*** (70.08)	-3,806.7*** (1,171.6)
Drop counties with Sam's Clubs (N=49,031)	-173.06** (65.20)	-2,226.8** (979.4)	-158.30*** (57.84)	-1,386.0* (917.7)
Combine Wal-Mart stores with Sam's Clubs	-187.48*** (63.24)	-2,720.9*** (973.2)	-174.60*** (54.19)	-2,296.8** (905.6)
Weighted by store size	-208.07*** (77.80)	-3,350.1*** (1,173.5)	-222.03*** (70.68)	-3,228.2*** (1,126.2)
Instrument with linear distance × year dummy variables [first-stage F-statistic]	-366.44*** (123.75) [16.8]	-4,352.1** (1,901.1) [16.8]	-393.66*** (105.92) [7.2]	-4,253.6** (1,801.9) [7.2]
<b>B. Manufacturing</b>				
OLS	-8.05 (11.08)	-285.3 (333.9)	-11.65 (11.01)	-388.4 (332.2)
IV	39.67 (91.64)	7,611.9** (3,110.1)	-167.09** (82.62)	1,988.3 (2,598.5)
Reject null of no endogeneity bias at 5% level	No	Yes	No	No
N	47,512	47,512	47,512	47,512

Table reports IV estimates of coefficient on change in number of Wal-Mart stores per person; the only exception is OLS estimates reported for manufacturing, in Panel B. See notes to Tables 1, 4, and 5. All retail estimates are for A sample. Sample sizes are the same as in Tables 4 and 5 unless otherwise noted. Estimated coefficients of total payroll per person variable, where included, are not shown. For the estimates labeled “weighted by store size,” stores are weighted by their square footage divided by the average square footage for the sample, to maintain comparability with the baseline estimates.