The Dark Side of Technological Progress? Impact of E-Commerce on Employees at Brick-and-Mortar Retailers

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First Draft: October 11, 2017
Current Draft: June 2, 2018

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Abstract

Using an employer-employee payroll dataset for approximately 2.6 million retail workers, we analyze the impact of the staggered rollout of a major e-commerce retailer's fulfillment centers on the income and employment of workers at geographically proximate brick-and-mortar retail stores. We find that the establishment of an e-commerce fulfillment center in a county has a negative effect on the income of retail workers in that county and in neighboring counties within 100 miles. Wages of hourly workers, especially part-time hourly workers, decrease significantly. This decrease is driven by a drop in the number of hours worked. We observe a U-shaped pattern in which both young and old workers experience a sharper decrease in wage income. Consequently, in these counties, there is a decrease in credit scores and an increase in delinquency for retail workers that have higher prior credit utilization. Using sales and employment data for 3.2 million stores, we find that retail stores in counties around fulfillment centers experience a reduction in sales and in their number of employees. Further, there is a decrease in entry and an increase in exits for stores in the retail sector, with small and young retail stores exiting at a higher rate. Our robustness tests show that our results are unlikely to be driven by prevailing local economic conditions. Overall, our results highlight the extent to which a dramatic increase in e-commerce retail sales can have some adverse consequences for workers at traditional brick-and-mortar stores.

I. Introduction

Technological advances can create enormous economic benefits for society. But, technological progress and automation can also reshape and transform some labor markets. They can change the way some tasks are conducted, and these changes can augment the productivity of some workers but replace other workers entirely.[1] In this paper, we study the impact of e-commerce, which is one manifestation of technological advances in the retail sector. In particular, we study the effect of e-commerce on the employees of traditional brick-and-mortar retail stores.

The retail sector is a major employer in the U.S., employing approximately 16 million workers, or 13% of private sector employment, at the end of 2016 (Bureau of Labor Statistics). The retail industry landscape has changed dramatically in the last few decades. In the earlier decades, the disruption was mainly driven by the expansion of major retail chains such as Walmart and by the rise of discount retailers (Jia (2008), Holmes (2011), Basker (2005), Neumark et al. (2008)). But, the recent disruption in the retail sector is attributed to the rise of technology led by e-commerce. It is projected to result in the shuttering of more than 8,000 retail stores by the end of 2017.[2] So, the retail sector is an economically important setting for studying the impact of technological progress, as represented by e-commerce, on the labor force in the traditional retail sector.

Identifying the causal impact of e-commerce on the employees of traditional brick-and-mortar retailers is challenging, since we cannot observe the counterfactual, i.e., what would the income and employment of brick-and-mortar retailer employees be in the absence of e-commerce? We address this identification concern by using the staggered roll-out of the fulfillment centers (*FCs*) of a major e-commerce retailer. We use this rollout

[1] See National Academy of Sciences Report on Automation (2017); Acemoglu (2002); Autor, Levy, and Murnane (2003); Brynjolfsson and McAfee (2011 and 2014); Autor (2015); Autor, Dorn, and Hanson (2015).

[2] See `https://on.wsj.com/2poCwtG`. Further, some online retailers have highly automated warehouses that use robots to bring items for a retail order from their storage shelves. The world's largest e-commerce retailer employed 45,000 robots in its fulfillment centers, a 50% increase from previous years holiday season. `https://bit.ly/2EldAGf`

as a proxy for the presence of local e-commerce and a large administrative employer-employee payroll dataset with detailed data on income and employment from a major credit bureau. Although the e-commerce retailer has FCs across the country, the timing of the establishment of FCs was staggered across the U.S. from 1997 to 2016. Our empirical strategy estimates the causal impact of the establishment of a new FC by a major e-commerce retailer on the income and employment of retailer workers in that county and in neighboring counties.

We use a large administrative employer-employee payroll dataset from a major credit bureau. This dataset contains detailed data on income and employment. We restrict the sample to 57 major retail firms that employ a total of approximately 2.6 million retail workers. This comprises 18% of total U.S. retail employment in the first quarter of 2010. Our rich payroll information allows us to group workers into hourly workers and non-hourly workers (referred to as *salaried workers*). These data include total compensation, wage/salary, overtime, bonuses, commissions, and wage/salary rate. We analyze this matched employer-employee dataset, and we use a difference-in-differences setting to exploit the staggered introduction of the FCs. We find that the labor income of retail workers in counties with FCs, on average, decreases by 2.4% after the establishment of FCs. This negative effect is also significant for workers within 50 or 100 miles of FCs. These results are confined to hourly workers, who experience a decrease in labor income by 2.5%, which is equivalent to an $825 decrease in annual income. Most of the effect derives from a reduction in the number of hours worked. Among hourly workers, we find a particularly strong negative impact on part-time hourly workers. Further, we observe a U-shaped pattern in which both young and old workers experience a sharper drop in labor income.

One potential concern about our identification strategy and our results may be: Why do the e-commerce retailer's FCs matter for traditional brick-and-mortar store sales? At the beginning of 2000, the e-commerce retailer had only three FCs, but the staggered

introduction of FCs across different counties resulted in the retailer having more than 90 FCs by the end of 2016 (Figure 1 and Figure 2). Optimizing and expanding the FC network is an important strategy used by the e-commerce retailer to meet customer demand and save costs. The establishment of FCs allows the e-commerce retailer to optimize and distribute their inventory placement (even for third party sellers). This in turn allows the e-commerce retailer to reduce its shipping costs and shipping times. The e-commerce retailer can offer same-day or 2-day shipping for a longer time during the shopping day from the nearby FCs, making a purchase from the online retailer attractive relative to the traditional brick-and-mortar stores in the vicinity.

Moreover, the e-commerce retailer does not collect local sales taxes and has only recently begun to collect state sales tax on sales from its inventory (Baugh, Ben-David, and Park (2018)). However, it does not yet collect state sales tax from most of its third party sellers (that account for more than 60% of the retailer's sales). This could lead to a price advantage over the traditional retailers. Also, the establishment of an FC is meant to avoid long-zone shipping. Therefore, the establishment of an FC is likely to have a more significant effect on geographically nearby areas, a fact that we exploit in our identification strategy.

Consistent with aforementioned arguments, we find that the establishment of the e-commerce retailer's FCs impact the sales of geographically proximate traditional brick-and-mortar retail stores. Using sales data from the National Establishment Time Series (NETS), we find that after the establishment of the e-commerce retailer's FC, the annual sales of stores decrease by \$63,639 per store, i.e., 2.8% of the total annual sales of the average store in our sample. We find that the annual sales of stores in the top tercile, based on sales one year before the FC, decrease by \$200,389.

A second potential concern regarding our identification strategy and our results is that the decision of establishing an FC in a county is probably not random. The decision may in fact be correlated with local economic conditions. As the goal is to better serve

customers in surrounding areas, FCs are more likely to be built close to, or in, areas with high retail sales and population density. Our fixed effect methodology can control for these level differences. However, it is possible that the e-commerce retailer may choose to locate its FCs in areas with decreasing competition from brick-and-mortar retail stores, i.e., areas where the retail sales of brick-and-mortar stores are declining.

We formally test this hypothesis using differences in the demographics and retail sales data computed from 2000 and 2010 county-level census data. We find that the change in population density is the only change that is significantly and positively related to an FC establishment, while growth in the unemployment rate, median household income, and age distribution do not correlate with the location choice of FCs. Further, the positive coefficient on retail sales growth gives us confidence that our estimation may not be driven by a downward trend in the traditional retail sector. We also find a lack of pre-trends before the establishment of the FCs, lending credence to our identification strategy.

The inclusion of state-year-quarter fixed effects mitigates potential concerns about unobservable local economic conditions being the driver of our results. We further add more granular county-year-quarter fixed effects to control for local economic conditions by using the data available for workers of non-retail firms as a control group. Moreover, the opening of FCs has no impact on the sales of full-service restaurants in the county, supporting the view that the impact is due to the establishment of the FCs and not due to negative local economic conditions.

Another potential concern is that there may be an omitted firm-specific shock to the traditional retailers that is contemporaneous with the establishment of the e-commerce retailer's FC in that county. For example, some firms may have concentrated operations in certain areas, and they may face firm-specific negative shocks (e.g., the failure of a major lender, the bankruptcy of a supplying firm) at the same time the e-commerce retailer builds an FC in that county. To address this concern, we include firm$\times$year-quarter fixed effects in our regressions. Our results are robust to the inclusion of these

fixed effects.

As we argued before, the establishment of FCs can cut shipping costs and shipping time. It is possible that this may induce some consumers to purchase from the online retailer instead of brick-and-mortar stores. It is likely that some of these benefits disappear as consumers become more and more distant from the FCs, thue reducing the impact on traditional retail stores and consequently on the workers at these stores. On the other hand, for counties with FCs or counties are geographically close to the FCs, any positive labor demand due to hiring by the FCs may reduce the direct negative impact on the traditional stores. In line with these arguments, we find that the income effect is -1.9% for counties with FCs, and it is strongest (-3.9%) in counties within 50 miles of FCs. This effect monotonically decreases over the distance after 50 miles. It becomes insignificant for counties within 500 miles of FCs.

As a reaction to the increased competition from the e-commerce retailer, the affected stores may focus on customer service and rely more on employees. On the other hand, facing lower sales, affected stores may not only cut wages but also adjust their overall employment level. We find that for all stores, employment decreases by 2.4%, which is equivalent to 41 fewer workers per 100 stores for a store with an average of 22 employees. We find that, for large stores, employment decreases by 1 worker per store for a store with an average of 40 employees.

Finally, using NETS data, we analyze store closures and new store openings. We find that the exit rate increases by almost 3%. The average exit rate in our sample is 13.6%. Small stores are more likely to exit than large stores. More interestingly, young stores are more likely to close. We also find that after the establishment of FCs in a county, the entry rate for small stores reduces significantly by 11.8%. This low entry rate effect is not just limited to counties with FCs; it also exists for counties in 50 or 100 miles of FCs.

Our paper directly relates to the literature that documents the effect of technological

changes on labor market (Krueger (1993); Autor, Katz, and Krueger (1998); Acemoglu (2002); Autor, Levy, and Murnane (2003); Autor (2015); Autor, Dorn, and Hanson (2015)). Further, we add to the literature on the income and employment effect of competition. This issue has been studied in the context of the expansion of large chain stores (Basker (2005); Neumark, Zhang, and Ciccarella (2008)) and import competition (Autor, Dorn, and Hanson (2013); Autor, Dorn, Hanson, and Song (2014)). The rise of e-commerce is one of the most important technological changes in the retail sector, and it may create competition for brick-and-mortar retail stores. We use the establishment of a major e-commerce retailer's FCs as a proxy for the increase in local competition, and we test the effect of this competition on the income and employment of retail workers.

Our paper also relates to research on the causes and consequences of disruption in the retail sector. Existing research focuses on the disruption that results from the rollout of large chains. Holmes (2011) estimates the benefits and costs for the rollout of Walmart store openings. Jia (2008) quantifies the effect of the expansion of retail chain stores on other retailers. Basker (2005) and Neumark, Zhang, and Ciccarella (2008) estimate the employment and earnings effect as a result of Walmart store openings. We investigate the disruption attributed to e-commerce.

Our research contributes to the literature on the impact of e-commerce. Compared to brick-and-mortar retailers, e-commerce provides consumers a lower price (Brynjolfsson and Smith (2000)), and increased product variety (Brynjolfsson, Hu, and Smith (2003); Ghose, Smith, and Teland (2006)). The introduction of e-commerce by a firm increases its market value and revenue (Subramani and Walden (2001); Pozzi (2013)). While these above papers examine the impact of e-commerce on consumers and firms that adopt e-commerce, we analyze the labor market consequence of a major e-commerce retailer's expansion of its FC network.

One important caveat with our result is that, given the scope of our paper and empirical strategy, we are limited to documenting one facet of the impact of the technological

innovation in the retail sector. There are many positive benefits of e-commerce for consumers, including potentially lower prices, more choices, convenience in shopping, gains from competition, and lower effort (e.g., no driving). Moreover, the establishment of the FC may have positive spillovers in the local community and could increase employment. However, the scope of our paper is limited; it focuses only on the impact on the workers in the geographically proximate traditional retail stores.

If there are no frictions in the labor market (i.e., workers can easily switch jobs and their skills are completely transferable) then the short-term displacement of some traditional retail store workers that we document may not matter for the workers or the local economy. However, in the presence of frictions in the labor market, the short-term impact on the workers and local economy can be negative. Moreover, to the extent that the scope of work differs between traditional retail stores and warehouses, at least some workers can be worse off. For example, there is no need for cashiers at an FC, and skills may not be completely transferable between traditional retail stores and e-commerce FCs.

In the long run, some of the affected workers may find alternate employment in their same field, or they may acquire new skills to find employment in another field. This would result in workers being better off after the establishment of an FC. Again, the scope of our paper is limited, and we focus only the short-term impact of the establishment of the FCs of the e-commerce retailer. Our results highlight one negative consequence of technological progress in the retail sector: the short-term negative impact on the wages and employment of some traditional brick-and-mortar retail store employees.

The rest of the paper proceeds as follows. We discuss our empirical methodology and identification threats in Section II. In Section III, we describe the various sources for the data used in the analysis and provide summary statistics. Our main empirical results are presented in Section IV. We conclude in Section V.

II. Empirical Design and Identification Challenges

A. Empirical Design

In this study, we seek to examine the impact of technological progress, in the form of the expansion of e-commerce, on traditional brick-and-mortal retail establishments and their workers. In order to isolate the effects of the establishment of FCs from other regional, sectoral, and macro-level shocks, we exploit the staggered rollout of FCs to capture the increase in the presence of local e-commerce. While the location that the major e-commerce retailer chooses for its FCs is certainly not random, we present evidence demonstrating that the timing of an FC establishment is plausibly exogenous to unobserved factors that may impact local retail establishment performance. Specifically, our empirical strategy estimates the impact of the establishment of an FC in a county on retail workers in the same county and in neighboring counties.[3]

Why do FCs matter? First, the optimization and expansion of the FC network is important to the e-commerce retailer to meet customer demand and reduce costs. The e-commerce retailer built its first FC in 1997, and the number of FCs increased to over 90 by the end of 2016 (Figure 1 and Figure 2). Second, the establishment of a new FC is meant to avoid long-zone shipping. The FC is established to reduce the costs associated with shipping as well as the time required for customers to receive their packages.

Furthermore, it is evident that FCs are primarily located on the east and west coasts, where population density is highest.[4] Third, as discussed in Houde, Newberry, and Seim (2017); customers may value the convenience effect due to faster delivery, so that the establishment of an FC would induce customers nearby to be more willing to shop

[3] A complete list of fulfillment centers of the e-commerce retailer is available at `http://www.mwpvl.com/`.

[4] The 2016 annual report of the e-commerce retailer states "If we do not adequately predict customer demand or otherwise optimize and operate our fulfillment network and data centers successfully, it could result in excess or insufficient fulfillment or data center capacity, or result in increased costs, impairment charges, or both, or harm our business in other ways."..."In addition, a failure to optimize inventory in our fulfillment network will increase our net shipping cost by requiring long-zone or partial shipments.". New FCs would be close to large cities, allowing for the possibility of next-day or same-day delivery and the wider rollout of its grocery business (Stone(2013)).

through the major e-commerce retailer rather than shop at a local brick-and-mortar store. As mentioned before, there may be lower prices for consumers, as the e-commerce retailer does not collect state taxes for sales by third party vendors on its platform, which represents approximately 60% of its sales.

Furthermore, the e-commerce retailer does not collect local sales taxes, if any tax at all, for most of its customers. E-commerce retailing in principal allows for the ability to serve all potential customers on a national scale. However, in practice, the establishment of an FC will have a larger effect on areas surrounding the FC. As such, this increase in the value to local consumers of shopping from the major e-commerce retailer may negatively affect the income and employment of workers in brick-and-mortar stores.

Our empirical objective is to evaluate the local effect of the establishment of a new FC.[5] We do so by focusing on three definitions of *local*: 1) the focal county (i.e., the county where the FC opened) 2) all counties within 50 miles of the FC (excluding the focal county where the FC is located) and 3) all counties within 100 miles of the FC (again excluding the focal county where the FC is located).

We treat each county as *treated* in the first quartert in which an FC opens in one of the three definitions of a *local* county. For example, for the analysis that focuses on the focal county level, the indicator for Fulton County, GA, turns on in the first quarter of 2015 since an FC was opened in Union City, GA, (Fulton County) in February of 2015. In our 50-mile analysis, Cobb County, GA, (a county that abuts Fulton County, GA) is treated in the first quarter of 2015 from the opening of the same FC in Union City, GA. Yet, in our 100-mile level analysis, Cobb County, GA, is treated in the third quarter of 2011 due to the opening of an FC in Hamilton County, TN, in September of 2011. As our data start in 2010, our study focuses on the 39 FCs established after 2010.[6]

A standard approach for evaluating the impact of the opening of an FC would be

[5]We follow Houde, Newberry, and Seim (2017) and remove FCs that are established in a county with an existing FC or within about 20 miles of an existing FC, which reduces our sample to 50 FCs.

[6]Our results remain robust to the inclusion of all 50 FCs (which includes FCs established before 2010) and is reported in Table IX.

to compare differences in brick-and-mortar establishment performance before and after the FC opening in treated and in untreated counties. However, for this difference-in-differences specification, we assume parallel trends between the treated and control counties. However, Table A1 indicates that focal and surrounding counties where FCs open are very different not only in levels but also in trends from the rest of the U.S. in terms of demographics and local economic variables, such as population, population density, retail sales, retail sales per capita, household income, and unemployment rate. Therefore, these untreated counties may bias our results. Consequently, we only include counties that were classified as *treated* at any time by the opening of an FC in our analysis, and we exploit the variation in the timing of the establishment of FCs, using FCs that will be treated but are not yet as de facto controls.

In our baseline analysis, we apply a difference-in-differences estimation to quantify the impact of the establishment of an FC on the income of workers in brick-and-mortar stores, and we estimate the following:

$$\ln(Total\ Income_{i,c,t}) = \alpha + \beta PostFC_{c,t} + \eta_i + \theta_t + \epsilon_{i,c,t}, \tag{1}$$

where each quarterly observation is the income of worker i working in county c at time t. *PostFC* is an indicator that equals 1 in the quarter an FC is established in county c or within 50 or 100 miles of county c, and it remains 1 for all subsequent quarters. Time-invariant worker-specific characteristics and year-quarter shocks are controlled for with the inclusion of worker (η_i) and year-quarter (θ_t) fixed effects, respectively. Standard errors are clustered at the county level. The variable β estimates the percentage change in income attributed to the establishment of an FC.

B. Identification Challenges

In order for β from Equation 1 to represent an unbiased estimate of the impact of the establishment on an FC on the income of local brick-and-mortar retail workers, we must

assume that *PostFC* is orthogonal to any unobservables. Yet, because the location of FCs is not randomly decided by the major e-commerce retailer, dealing with this endogenous selection represents our main econometric challenge.

A primary concern in our analysis is that the decision to establish an FC in a specific county will naturally be a function of local economic conditions. Since one of the primary objectives of establishing FCs is to improve the ability to serve local customers, FCs may be more likely to be built in areas with high retail sales and high population density. To test this, on a cross-section consisting of all counties in the U.S., we regress the likelihood of establishing an FC in county c on county-level long differences (between 2000 and 2010) of retail sales, population density, unemployment rate, household income, and the percentage of the population between the ages of 18 and 65 (Table II).

We observe that FCs are more likely to be located in counties with faster growing population densities; but, importantly, not those that have experienced more growth in retail sales. While our county-level fixed effects will absorb all time-invariant level effects of county-specific characteristics, our estimated will be biased downward insofar as counties that experience high population density increases are also more likely to engage in e-commerce transactions that substitute for local retail purchases. However, if this scenario were true, we could also observe downward trends in retail sales in the periods before the FC's establishment, which we do not (Figure 3).

A second concern surrounds the ability for the major e-commerce retailer to negotiate with the state and local government about the location of the FC in exchange for tax benefits or other incentives. It is plausible that governments may want the FC to be built in an area with weak economic conditions to boost the local employment and economy. As a result, FC county selection may be negatively correlated with local economic conditions; and, in particular, negatively correlated with the economic fortunes of brick-and-mortar retail stores, thus potentially biasing our estimates downward.

However, as shown in Table II, Columns (3) and (4), the establishment of an FC does

not relate to the ex ante change in the local unemployment rate and median household income, which lessens this concern. Furthermore, we deepen our analysis by conducting a placebo test on sales in another non-tradeable sector: full-service restaurants. As reported in Section IV.B.5, we do not find any effect on their sales with the establishment of FCs in the local area.

The e-commerce retailer may also consider the tax implications of an FC. The establishment of an FC means establishing a physical presence. Though it is not necessary, the major e-commerce retailer must collect sales tax in some states. Even if this is true, it is biased against finding any negative impact on nearby traditional retail stores. Moreover, the tax consideration is important in early years. After building FCs in low-tax states, and given the fact that they have collected sales tax in many states, being closer to a large market dominates the tax consideration (Stone (2013); Houde, Newberry, and Seim (2017)). We control for any state-level time-varying unobservables, such as tax incentives, using state-year-quarter fixed effects.

Last, we may be concerned that the arrival of an FC in a county changes the composition of firms. In this case, higher performing firms or firms with greater options in managing their geographically varied portfolio of stores choose to exit those counties. Or conversely, the best performing retailers choose not to enter the treated counties. This would lead to the appearance of a drop in retail performance, but this would be entirely attributable to a compositional effect whereby the firm-quality distribution experiences a leftward shift. To deal with this, we focus solely on the intensive margin of competition by including only firms that are present before and after the arrival of an FC. In addition, we include establishment-level fixed effects to control for establishment quality.

In summary, it is difficult to establish a convincing causal relation; however, our confidence in interpreting our *PostFC* coefficient as causal is increased by the inclusion of fine-grained establishment, worker, industry-year, state-year, and year-quarter fixed effects. Our confidence is also increased by the ability to rule out reverse causality or

selection on trends, as seen in the lack of any discernible pre-trends in our regression and in our full-service placebo test.

III. Data

Our empirical analysis makes use of data at three levels of analysis: 1) individual worker-level data that we obtain from a major credit bureau, 2) establishment-level data that we obtain from the National Establishments Time Series Database, and 3) county-level data that we obtain from the Bureau of Labor Statistics. We describe each dataset and its construction in this section.

A. Worker Data

Our novel comprehensive consumer data are provided by a major credit bureau. The data contain detailed employment information, including company name, 3-digit NAICS, the date an employee was most recently hired for the current position, an indicator of whether an employee is presently active, and rich payroll information that includes the payment structure by which payments are made to the employee, total compensation, wage/salary, overtime, bonuses, commissions, and wage/salary rate. We group workers into hourly workers and non-hourly workers (referred to as *salaried workers*) based on their payment structure.

We obtain income and employment data of active employees at the end of each quarter from the retail firms, which consistently supply data from 2010 to 2016.[7] The data are matched to credit files through tokenized Social Security Numbers (SSNs), which provide demographic information such as the individual's ZIP code of residence, age, and gender. We use the workers' county of residence to determine their location when examining

[7]We identify firms in 3-digit NAICS industries that are most likely to compete with the major e-commerce retailer's product catalog. The 3-digit NAICS codes that we classify as *retail* includes 442 (furniture and home furnishing stores), 443 (electronic and appliance stores), 444 (building material and garden equipment and supplies dealers), 448 (clothing and clothing accessories stores), 451 (sporting goods, hobby, book, and music stores), 452 (general merchandise stores), and 453 (miscellaneous store retailers). In robustness tests, we use workers from all non-retail firms as a control group.

the impact of the arrival of an FC.[8] Our sample consists of all workers employed in the first quarter of 2010, and the sample follows them until they exit. Our sample is thus unbalanced and does not allow for worker entry. In addition, we use the workers' residency in 2010Q1 to determine their location (county) for all empirical analysis. We drop workers who have multiple employers at any time during our sample period. If a worker switches employers during the sample period, we keep our observations for the first job. All dollar values are converted to December 2016 dollars using the seasonally adjusted consumer price index for all urban consumers from the Bureau of Labor Statistics.

Our sample contains 2.6 million workers from 57 retail firms, which accounts for 18% of the 14.42 million total U.S. retail employment in the first quarter of 2010. The median firm has more than 14,000 workers in the sample, which suggests that we cover mostly large firms. Table I, Panel A presents summary statistics for worker-level payroll data. The mean quarterly income of hourly workers is $7,326. Annualized income is $29,304, which is slightly higher than the mean income of 8.79 million retail sales workers ($25,250) and slightly higher than the mean income of 4.53 million retail salespersons ($27,180), as estimated by the BLS in May 2016. The mean number of hours worked is 30.9 per week, with an average wage rate of $14.9 per hour. For retail workers in our sample, wage income contributes to about 87% of their total income. The remaining income derives from overtime/bonuses/commissions (referred to as a bonus). In our sample, salaried workers earn $86,016 annually, on average.

The granularity of our worker-level data helps us answer questions that cannot be addressed solely from aggregate data. Given the fine-grained nature of these data, we can examine deeper worker-level heterogeneity and analyze which workers are more vulnerable to the establishment of an e-commerce FC. For example, are full-time workers more affected than part-time workers? Further, does a worker's gender and age insulate or ex-

[8]We do not observe the county of the workers' workplace. However, more than 90% of workers in our sample are hourly workers who are less likely to spend time and money on commuting to work. While we believe that a workers' residence does an adequate job of proxying for their workplace, we recognize that this will be measured with some error.

acerbate these effects? The detailed composition of the worker's compensation also allows us to understand the channels through which workers are affected. Do firms reduce workers wages or bonuses? Do firms cut wage rates or the number of hours worked? Lastly, these granular data allow us to improve the identification of our regression parameters through the inclusion of fine-grained fixed effects within a panel regression environment.

B. Establishment Data

In addition to worker-level data, we use establishment-level data for the retail sector from the National Establishment Time Series (NETS) Database (Walls & Associates, 2014).[9] This database provides an annual record for a large part of the U.S. economy that includes establishment job creation and destruction, sales growth performance, survivability of business startups, mobility patterns, changes in primary markets, corporate affiliations that highlight M&A, and historical D&B credit and payment ratings. At the beginning of our sample year, 2010, the database covers 3,287,183 active establishments employing 27,404,989 workers with total sales of $2.9 trillion. These data are available until 2014.

Similar to how we defined retail firms with our worker-level dataset, we select establishments in 6-digit NAICS industries that are more likely to be affected based on the e-commerce retailer's product catalog. Table A2 provides a complete list of industries selected. To reduce noise from very small retail stores, we keep retail stores with more than two employees before the establishment of the fulfillment center. Table I, Panel B reports summary statistics for our sample. It shows that the average retail store in our sample has annual sales of approximately $1.5 million and 12 employees.

[9]Walls & Associates converts Dun & Bradstreet (D&B) archival establishment data into a time-series database of establishment information.

C. *County Employment Data*

For our county-level analysis, we use publicly available Quarterly Census of Employment & Wages (QCEW) data provided by the Bureau of Labor Statistics (BLS). This dataset provides county-level data on employment, wages, and the number of establishments in each 6-digit NAICS industry by quarter. We again select industries that are likely to be affected, based on the major e-commerce retailer's product catalog. We use quarterly data beginning in the first quarter of 2010 and ending in the fourth quarter of 2016. Summary statistics are reported in Table I, Panel C.

IV. Results

In this section, we describe our main empirical results. We first describe our baseline results using worker-level data. We then describe the robustness tests that we conduct to rule out competing interpretations of our results and to strengthen the identification of our parameters. Next, we describe results using NETS establishment data that allow us to analyze the impact of FC entry on the entry and exit of establishments in the local retail sector. Finally, we present the impact of FC establishments on the aggregate county-industry level employment using QCEW data from BLS.

A. *How Do FCs Affect Local Brick-and-Mortar Stores? Evidence from Worker-level Data*

A.1. Baseline Results

In Table III, we report the impact of the establishment of FCs on the income of retail workers in counties with FCs or neighboring counties using the difference-in-differences specification shown in Equation 1. We include worker fixed effects and year-quarter fixed effects in all regressions in order to absorb as much variation as possible arising from worker-specific time-invariant characteristics and temporal trends. Since we define the arrival of an FC at the county level, we cluster all standard errors at the county level.

As shown in Panel A, Column (1), the total income of retail workers in counties with FCs decreases by 2.4%, on average, after the establishment of an FC. Moving to workers in counties within 50 or 100 miles of the focal county where an FC was established, we continue to observe a strong negative effect on total income (Panel B). Since the arrival of an FC may differentially impact hourly and salaried workers, we run separate regressions for those two types of workers.

Results in Column (2) show that the income of hourly workers decreases by 2.5%, equivalent to an $825 cut in annual income. As shown in Column (3), salaried workers mostly have muted responses to the establishment of FCs. These muted responses may be attributed partly to the infrequent adjustment of salaries or the inflexibility of firms in adjusting the incomes of salaried employees in the short term. We focus on hourly workers thoughout the rest of our analysis, because hourly workers account for more than 90% of our sample and are the ones who experience the largest negative effects.

Our identification strategy, which relies on the staggered temporal rollout (shocks) of FCs across different counties, assumes that workers in counties that have yet to be treated by the establishment of an FC serve as an appropriate control group. This assumption would be violated if FCs are established in counties or regions that are experiencing upward trends in online shopping and downward trends in sales at traditional brick-and-mortar retailers. In this case, the negative income effect may be driving the FC establishment and not vice versa. As such, our difference-in-differences assumption is only valid if treatment and control groups follow parallel trends before the shock. To test this, we directly examine the dynamic temporal effects by including leading and lagging indicators of FC establishment by estimating the following:

$$Log(Total\ Income_{i,c,t}) = \alpha + \sum_{j=2}^{4} \beta_j PreFC_{c,t}(-j) + \sum_{j=0}^{4} \gamma_j PostFC_{c,t}(j) + \eta_i + \theta_t + \epsilon_{i,c,t}. \quad (2)$$

To increase the power of our estimates, *PreFC* and *PostFC* dummies are defined at half-

year intervals. The variable $PreFC_{c,t}(-j)$ ($PostFC_{c,t}(j)$) is a dummy that takes a value 1 if it is j half-years before (after) the establishment of FCs. Also, *PreFC(-4)* equals 1 if it is two or more years before the establishment of FCs, and *PostFC (+4)* equals 1 if it is two or more years after the establishment of FCs. The variable *PreFC(-1)* is dropped from the estimation so that all coefficient estimates can be treated as percentage changes relative to the income workers received six months before the establishment of FCs.

In Figure 3, Panel A, we show the dynamic effect of FCs on income for counties with FCs by plotting the coefficients from the specification in Equation 2. The shaded area around the coefficients represent 95% confidence intervals. Coefficients on *PreFC(-4)*, *PreFC(-3)*, *and PreFC(-2)* are all statistically insignificant from the income of workers in *PreFC(-1)* (the omitted category). That suggests that there is no pre-trend in the data, and our parallel trends assumption appears to be valid. Within six months of the establishment of an FC, the income of hourly workers decreases by 2.1% relative to the half-year shortly before the FC's establishment. The negative effect further increases to -4.1% two years after the FC's establishment. We find a similar pattern in Panels B and C of Figure 3, where we focus our analysis on counties within 50 miles of the county in which an FC opened and within 100 miles, respectively.

A.2. Can Unobservable Firm-Specific Variables or Local Economic Conditions Be Driving the Results?

Our results so far suggest a robust and negative relationship between the arrival of an FC and a worker's income. However, absent truly exogenous variation in both the geographic location and temporal timing of FC establishment, we may still be concerned that the arrival of an FC is correlated with unobservables present in the error term of Equation 1. These unobservables may include firm-specific characteristics or local economic conditions that jointly affect both the likelihood of an FC arriving in the county and the income of workers in local brick-and-mortar establishments.

For example, if a major lender or supplier to a brick-and-mortar retailer files for bankruptcy and this attracts the e-commerce retailer to establish an FC in the county as a result, then this negative correlation between our error term and the FC establishment amplifies our negative effect sizes. To address this concern, we include firm-year-quarter fixed effects to absorb all time-specific characteristics of our sample firms and identify our parameter of interest by exploiting varition within-firm-time across counties. As such, we can only estimate our *postFC* variable from firms that operate in more than one county.

We see in Column (1) that when we include these firm-year-quarter fixed effects, the establishment of FCs in the county results in lower total income for retail hourly workers in the brick-and-mortar stores. We find that the magnitude diminishes from the baseline magnitude (2.5%) to 2.1% for counties with FCs. When we extend our analysis to focus on counties within 50 and 100 miles of the county in which the FC was established, we continue to see precisely estimated negative effects.

As discussed in Section II.B, local economic conditions may also play an important role in the establishment of FCs by the e-commerce retailer. States with and without FCs may have different economic and regulatory environments, which could correlate with the establishment of an FC. For example, regions with suppressed economic activity (which would negatively impact retail sales) may be more inclined to offer sizable incentives for e-commerce retailers to establish an FC in their region. To control for time-varying unobservables at the state level, we include state-year-quarter fixed effects and report the results in Table IV, Column (2). The estimated effect for counties with FCs drops from -2.5% to -0.8%, but the result remains statistically significant from 0. Further, the results are strong both economically and statistically for workers within 50 or 100 miles of FCs. Our results remain robust when we combine firm-year-quarter and state-year-quarter fixed effects in Column (3).

While the state-year-quarter fixed effects may control for state-level heterogeneity, they may be insufficient to fully absorb any time-varying heterogeneity that arises at the

county level. For example, it may be that the e-commerce retailer decides to build an FC in a county at the same time that an unexpected negative economic shock occurs in that county, or because of such a shock. Thus, it is possible that our baseline estimates may be driven by unexpected local economic shocks rather than by competition from e-commerce. To control for county-specific time-varying shocks, we expand our sample threefold to include data on hourly workers at non-retail firms. In doing so, we can employ a triple difference (difference-in-difference-in-differences) methodology whereby we exploit within county-year-quarter variation across industry type (retail versus non-retail). In doing so, we can carefully control for county-time specific shocks and identify our parameter of interest by comparing retail workers to non-retail workers in FC-treated counties.

If FCs are being established in regions that experience economic hardship, then we should observe no difference in incomes between retail and non-retail workers. In Column (4), we interact the *PostFC* dummy with a *Retail* dummy set to 1 if the focal worker works in a retail industry and 0 otherwise. We find that the income of retail hourly workers in counties with FCs is reduced by 4.4% compared to all other hourly workers within the same county, after controlling for county-level time-varying unobservables. As a result, it seems unlikely that a local negative shock that solely affects a county's retail firms but not its non-retail firms is driving our results.

Overall, the results presented in this subsection reduce our concerns that our results are being driven by firm-specific unobservable variables or some other omitted local economic conditions that coincide with the staggered establishment of FCs.

A.3. How Does the Impact of FCs Vary with Distance From the Focal County?

As discussed in Section II.A, the e-commerce retailer likely optimizes its FC network to reduce shipping costs and time by reducing the need for long-zone shipping. As a result, consumers in the focal county as well as consumers in geographically proximate

counties should benefit from the FCs establishment, and they should alter their purchasing behavior at the expense of local brick-and-mortar retail stores.

We analyze the role of geographic proximity by examining the diminishing effects of the establishment of an FC as distance from it increases. However, as shown in the FC network map (Figure 2), some FC clusters exist in the U.S., particularly in the Midwest and Northeast. As such, many counties are always within a few hundred miles of an FC, limiting our ability to evaluate the relationship between large distances and FC establishment. However, the western U.S. has fewer FC clusters (due to lower population density) where many counties counties are proximate to no more than one FC. Therefore, we focus on the 24 states west of the Mississippi river and extend our analysis to counties that are within 150, 200, 250, and 500 miles of FCs. Our results in Table V show that the income effect is -1.9% for focal counties with FCs. The magnitude of the effect increases to 3.9% in counties within 50 miles of FCs, after which it monotonically decreases over distance to a statistically insignificant -0.2% for counties within 500 miles of FCs.

A.4. Decomposing the Impact of FC Establishment on Wages

Our detailed payroll data on workers allows us to decompose their total income into wage income and bonus income. We can further decompose wage income into hours worked and wage rate. We run Equation 1 using different components of total income as our dependent variables. Table VI, Column (1) reports results for wage income as the dependent variable. All regressions include worker, firm-year-quarter, and state-year-quarter fixed effects. We continue to use this tighter specification for all our worker-level regression estimates.[10] We find a significant negative impact on wage income across all three panels. The economic magnitude ranges from -0.7% to -1.4%. In Column (2), we find that bonuses decline by 0.3% to 2.6%. To further investigate the source of this wage reduction, we decompose wage income into hours worked and wage rate. In Column (3),

[10]Note that our results are robust and in fact are stronger for our baseline model with only worker and year-quarter fixed effects.

we report results for hours worked and find that the estimated coefficients are almost the same as those in Column (1). We do not find economically significant changes in the wage rate (Column (4)).

The results documented in Table VI suggest that the negative impact of FC establishment on local retail workers is mainly driven by the reduction in hours they work. As previously documented, the negative impact is concentrated in hourly workers. At the same time, the wages of many of the hourly retail workers are bound by the applicable minimum wage. So, our results suggest that in the presence of this wage floor, firms cut down on the number of hours demanded from their part-time, hourly workers.

A.5. Are All Retail Workers Affected Equally by the Establishment of FCs?

Our rich worker-level data allow us to analyze *which* workers are more impacted by the negative wage shock due to the establishment of the FCs. This rich demographic information allows us to consider heterogeneity along worker dimensions such as age, gender, and worker status (part-time versus full-time).

We start by exploring worker heterogeneity by age. We split the retail workers in our sample into six age groups. Results from running Equation 1 over these six age groups are reported in Table VII. We find evidence for a stronger negative impact on the total income of young and old workers. We observe that for young workers under the age of 25 within 50 miles of FCs, total income decreases by 2.7%. The negative effect is lower for age group of 25-34 years (-1.8%). We find that this negative effect increases with age. For age groups 35-44 years, 45-54 years, and 54-64 years, the effect is -1.2%, -1.6%, and -2.0%, respectively. For the oldest group, i.e., workers older than 64 years of age, the negative income effect is as high as -3.5%. These results appear similar for focal counties and those within 100 miles of FCs.

These results suggest that a worker's age has a large moderating impact on the arrival of new technologies. One explanation for this result may be that a worker's age

proxies for their productivity and accumulated firm-specific human capital. On the one hand, young workers may be more productive, but firms may not have invested much in enhancing their firm-specific human capital. On the other hand, old workers may have accumulated firm-specific human capital but may be less productive compared to younger workers. Explaining how age plays a prominent role is outside the scope of our paper; however, our results at the least suggest that both younger and older workers shoulder a disproportionate share of the negative impact of FC establishment as opposed to middle-age workers.

Next, we test how the negative income effect varies across worker's working status, i.e., part-time workers versus full-time workers. Similar to age, a worker's working status may reflect his or her underlying level of firm-specific human capital accumulation. Firms may invest more in the human capital development of full-time workers than part-time workers. We define a worker as a part-time worker if the hours worked is less than 32 hours per week, otherwise the worker is considered a full-time worker. We define the worker's employment status in a time-invariant fashion by categorizing each worker by their work status at the beginning of our sample period, i.e., in 2010Q1. Table VIII, Panel A reports the differential effect on part-time and full-time workers. In line with the hour reduction results previously reported, we find that the negative effect is stronger for part-time workers, i.e., the impact on part-time workers is about -1% more.

It is possible that the negative impact of FCs varies by gender, given the significant fraction of female retail employees. We test whether there is any differential effect of FCs on male versus female workers. Table VIII, Panel B suggests that there is no difference in the effect based on worker gender.

In summary, the heterogeneity in the negative impact of establishment of FCs may be relevant for designing remedial responses to the negative impact of establishment of FCs on local retail employees. We find that young and old workers (as opposed to middle-aged workers) and part-time workers (as opposed to full-time workers) experience

disproportionately more negative effects from the establishment of FCs in their focal and proximate counties.

A.6. Additional Robustness Tests

We further conduct additional tests to ensure the robustness of the results reported so far. In our main analysis (i.e., results from estimating Equation 1), we focus on post-2010 FCs, since our sample starts with 2010. We include all FCs in Table IX, Panel A as a robustness test. The income effect is about -1% and is still significant in Column (2) and Column (3). The lower magnitudes can be attributed to the non-availability of pre-treatment worker data for FCs established before 2010.

In our baseline tests, we assigned FC treatment to workers based on the the ZIP code of their residence in 2010Q1. It is possible that some workers move to avoid the negative income shocks caused by FCs. Therefore, as a further robustness test, we remove migrants whose last observed ZIP code in the data is different from the first observed ZIP code. Results documented in Panel B of Table IX indicate that our main results remain unaffected.

In our analysis so far, we have used quarterly income computed from raw payroll data under the assumption that it is a timely reflection of the impact of FCs. But, it is possible that quarterly income is subject to seasonal variation. Since income is the key outcome measure in our analysis, we show robustness to our dependent variable by using an alternative income measure that is based on the projected annual income of a worker every month as computed by the credit bureau. We rerun our analysis with this projected annual income instead of the quarterly income we have used so far. Results documented in Panel C of Table IX show a significant negative effect, suggesting that our analysis is robust to this measure of income.

A.7. Credit Outcomes

Our results so far indicate that the establishment of FCs by a major online retailer has a negative impact on the wages of workers at traditional brick-and-mortar retail stores in the focal county and geographically proximate counties. The effects are predominantly borne by young and old workers as opposed to middle-aged workers, part-time workers as opposed to full-time workers, and hourly workers as opposed to salaried employees.

However, to the extent that labor markets are frictionless (i.e., workers can easily switch jobs, and skills are completely transferable) the short-term displacement of some traditional retail store workers that we document may not matter for the workers or the local economy. However, in the presence of labor market frictions, the short-term impact on the workers and local economy can be negative. Moreover, to the extent that the scope of work differs between traditional retail stores and warehouses, at least some workers can be worse off.

Our data prevent us from identifying any other source of income for the affected workers, specifically part-time and hourly workers, except income from their primary employer in the credit bureau payroll database. So, we are unable to directly verify whether the affected workers offset the reduced hours with brick-and-mortar retail stores by picking up additional working hours with another employer (who may not be part of the payroll database that we use).

We test for this possibility indirectly by considering the credit outcomes of the workers. If workers can easily substitute their sources of income, then it should have no effect on their credit outcomes. Otherwise, the declines in income may lead to worse credit outcomes, especially for the workers who are already living at the margin (i.e., workers with high bank card utilization). We use credit score as a measure of the credit outcomes for the affected workers. We assign a worker to the high utilization group if her bank card utilization is higher than the median utilization ratio, and we assign workers to the low utilization group if their bank card utilization is lower than the median.

We report our results in Table X. We find that the credit score for workers with high utilization of bank credit cards declines significantly. In counties with FCs, the decreases in credit scores of workers in the high utilization group are 3 points more than that of workers in the low utilization group. It seems that the decline in credit scores is driven by a higher bank credit card delinquency among the affected workers.

Overall, the evidence suggests that technological change, as manifested by an e-commerce retailer establishing an FC, leads to a decline in wages for workers in traditional brick-and-mortar stores. Among the affected workers, those who have a prior higher credit card utilization and those who are otherwise more financially vulnerable experience higher credit card delinquencies and a subsequent decline in their credit score. These results suggest that some of the affected retail workers experience some frictions in the labor market that preclude them from mitigating the extent to which the establishment of e-commerce FCs in their county depresses their wage income and subsequently their credit scores.

B. How Do FCs Affect Local Brick-and-Mortar Retail Stores? Evidence from NETS Data

So far, we have used detailed worker level data and the staggered establishment of FCs of the e-commerce retailer to understand the impact on the wages of the workers at traditional brick-and-mortar retail stores in the focal county and geographically proximate counties. We next use NETS establishment-level data in order to understand the impact of the establishment of FCs on traditional brick-and-mortar retail stores themselves. This analysis could highlight the aggregate implications of the negative income effects that workers suffer when they work fewer hours.

B.1. Effect on Retail Store Sales

In Table XI, we use NETS data to understand the effect of FCs on the sales of local brick-and-mortar stores. Column (1) reports difference-in-differences estimates for

all stores in the counties with FCs. In all the specifications, we include establishment fixed effects, 6-digit NAICS-year fixed effects, and state-year fixed effects. We find that the annual sales of local brick-and-mortar retail stores decrease by 2.8% (equivalent to $63,639 per store) after the establishment of an FC of an e-commerce retailer.

In the next three columns, we partition the incumbent establishment/store sample into terciles based on sales one year before the establishment of FCs in the county. We find that for the bottom tercile (*Small*), the annual sales decrease by 2.9%, which is equivalent to $7,565 per store. For the medium group (*Medium*), we find that sales decrease by 2%, which is equivalent to $12,348 per store. For the top group (*Large*), sales decrease by 3.4%, which is equivalent to $200,389 per store. These results suggest that the establishment of FCs negatively affects the sales of local brick-and-mortar stores, especially large retail stores. The effect is diminished in counties that are 50 or 100 miles from FCs.

These results also suggest that after the staggered establishment of the e-commerce firm's FCs, sales decline significantly in the focal county of the FC. This decline in sales may result in financial stress on the store, or it may motivate the parent company to focus on improving the operational performance of the store. One consequence of the decline in sales may be a reduction in the number of hours of work assigned to part-time and hourly workers.

B.2. Effect on Retail Store Employment

So far, we find that the establishment of FCs negatively affects the income of retail workers and the sales of local brick-and-mortar stores. Thus, it is instructive to understand how stores respond to lower sales after the increase in competition due to the establishment of the e-commerce retailer's FCs. Do they also adjust overall employment levels in addition to reducing the number of hours of part-time and hourly workers?

Table XII reports results for the effect of FCs on local establishment-level employment.

The results appear similar to sales. Column (1) reports difference-in-differences estimates for all stores in counties with FCs. We find that for all stores, employment decreases by 2.4%, which is equivalent to a reduction in employees of 41 workers per 100 stores for a store with an average of 22 employees. For *small* stores, employment decreases by 2.7%, which is equivalent to reducing 11 workers per 100 stores for a store with an average of 4 employees. For large stores, employment decreases by 1 worker per store for a store with an average of 40 employees. Similar to sales results, the effect is diminished in counties that are 50 or 100 miles of an FC.

Based on the results presented in the previous two subsections, it appears that after the establishment of the e-commerce retailer's FCs, traditional brick-and-mortar retail stores in the focal county adjust to the decline in the sales both by reducing the number of hours of work assigned to part-time and hourly workers and also by reducing employment levels.

B.3. Closures of Retail Stores

Next, we analyze whether the increase in competition and the consequent decline in store sales after the establishment of the e-commerce retailer's FCs can lead, in extreme cases, to an increase in retail store closures.

In Tables XIII and XIV, we attempt to understand whether the establishment of FCs leads to store closures and how this effect varies with store size and age. Here we define *exit*, our dependent variable, as a dummy equal to 1 if the establishment ceases to exist one year before the end of the sample period, and 0 otherwise. Table XIII, Panel A, Column (1) reports results for all stores.We find that the exit rate increases by almost 3%. The average exit rate in our sample is almost 13.6%. The effect is negatively correlated with the ex ante size of the store, i.e., small stores are more likely to exit than large stores. This effect is consistent for counties 50 miles or 100 miles from a FC.

We further test the role of a store's age on exits, and we report the results in Ta-

ble XIV. Here, we partition the *All* stores further into terciles, i.e., *young*, *medium*, and *old* based on ex ante age. The average age in the bottom tercile is about eight years. We find that young stores are more likely to close.

So, based on the analysis in Tables XIII and XIV, it appears that there is an increase in the exit rate of local brick-and-mortar retail stores after the establishment of the FCs of the e-commerce retailer. This exit rate impact is more pronounced for young and small retail stores, as they are likely to be more financially stressed and may not be able to survive the decline in sales after the FCs are established in the focal county.

B.4. Entry of Retail Stores

In all of our previous analysis, we focus on the effect of the establishment of FCs on incumbent brick-and-mortar retail stores. But it is possible that entry into the local retail sector is discouraged by the establishment of the FC, the consequent increase in competition, and the decline in sales and closure of some of the incumbent brick-and-mortar retail stores. We analyze the impact of the establishment of the FC on entry into the local retail market in Table XV. Column (1) of Panel A reports county-level results on the number of entrants. We find that after the establishment of an FC in the affected county, the entry rate for small stores is significantly reduced by 11.8%. This low entry rate effect is not just limited to counties with FCs; it also persists in counties 50 or 100 miles from an FC.

B.5. Effect on Sales of Full-Service Restaurants

We have attempted to rule out the key competing alternate explanation for the pattern we observe in our data: the possibility that our results are driven by some omitted local economic conditions and not by the establishment of the e-commerce retailer's FC in the county. As a further test to reduce the concerns about this interpretation, we test whether FCs have any effect on the sales of local full-service restaurants. If some omitted local economic shock is positively correlated with the establishment of an

FC in the affected counties, we would expect that the sales of full-service restaurants (another important non-tradable sector) also respond to this negative economic shock and experience a decrease in the focal counties.

Table XVI reports the results for the effect of FCs on the sales of full-service restaurants. Panel A, column (1) reports difference-in-differences estimates for all full-service restaurants. We do not find any negative effect on sales of full-service restaurants with the establishment of FCs in the counties. Similar to the previous subsection, we partition the data into terciles based on ex ante sales. We find that sales of *small* restaurants increase in the focal counties, but we do not find any effect on sales for medium- or large-sized restaurants nor any effect within 50 or 100 miles of FCs. These results indicate that it is unlikely that an omitted local economic shock is responsible for the negative impact on the sales of traditional brick-and-mortar retailers.

In summary, using detailed establishment level data from NETS, we find that after the staggered establishment of the FCs of the e-commerce retailer, the geographically proximate traditional brick-and-mortar retail stores experience a decline in sales, a decline in employment, a decline in entry in the local retail sector, and an increase in closures among the incumbent firms. The impact on store closures is more pronounced for young stores and small stores, whereas the decline in sales is more pronounced for larger stores. In addition, as a placebo, we find that the staggered establishment of the FCs of the e-commerce retailer does not correlate with the sales of full-service restaurants. Overall, our results suggest that the establishment of the e-commerce retailer's FCs has a negative impact on the financial health of the local traditional brick-and-mortar retail stores.

C. How Do FCs Affect Local Wages and Employment? Evidence from BLS County-Industry Data

Finally, to understand the aggregate effect at the county level, we use county-level QCEW data on total wages, employment level, average wage, and number of establish-

ments for each county. We report the results of the analysis in Table XVII. Using county-level QCEW data, we find that the establishment of an FC has a negative effect on local wages and employment. This is consistent with evidence using payroll data and NETS establishment-level data. After absorbing 6-digit NAICS-year quarter fixed effects and state-year quarter fixed effects, we find a muted effect for the counties with FCs, but we find a very strong negative effect on total wages, employment level, average wage, and number of establishments for counties within 50 or 100 miles of FCs. We further test for pre-trends for all the counties within 50 miles (including the focal county), and we find no pre-trends. Overall, the results using county-level QCEW data are largely supportive of our findings using administrative employment data and NETS data.

V. Conclusion

The recent disruption in the retail sector can be attributed to the rise of e-commerce. At the beginning of 2017, e-commerce sales accounted for 8.3% of total retail sales in the U.S., compared to 3.8% in 2010. We use the staggered rollout of a major e-commerce retailer's FCs as a proxy for local e-commerce presence. Using a payroll dataset for 2.6 million retail workers, we find that the labor income of retail workers in counties with FCs, on average, decreases by 2.4% after the establishment of FCs. Wages of hourly workers decrease significantly by 2.5%, equivalent to $825. Most of the effect comes from a reduction in the number of hours worked.

Further, using sales and employment data for 3.2 million stores, we find that retail stores in counties with FCs experience a reduction in sales and employees. We find that for stores in the top tercile based on sales one year before the FC, after the establishment of FCs in their county, their sales decrease by almost 3.4%, which is equivalent to $200,389 per store. For these stores, after the establishment of FCs in their county, their employment decreases by almost 2.5%, which is equivalent to one worker per store for a store with an average of 40 employees. Also, there is a decrease in entry and an increase

in exits for stores in the retail sector, with small and young retail stores exiting at a higher rate. We find that the opening of FCs has no impact on the sales of a full-service restaurant, which supports the proposition that negative local economic shocks may not drive our results.

Overall, our results highlight how the dramatic increase in e-commerce retail sales can have adverse consequences for workers at traditional brick-and-mortar stores. At the same time, our results should be interpreted carefully in light of the many benefits of e-commerce. In this paper, we do not consider the impact of e-commerce on consumers, the increase in employment by the e-commerce firm, or the e-commerce firm's ecosystem and the ancilliary benefits to the county. Further, we do not consider the long-term dynamics of the labor market in the counties affected by the FCs nor do we consider the long-term effects on the traditional brick-and-mortar retail workers who are affected by the establishment of e-commerce FCs in the focal county. Given the limited scope of this paper, we do not aim to quantify the aggregate effect of e-commerce on the retail sector. Our results can only show that the growth of e-commerce has some adverse consequences for some traditional brick-and-mortar retail workers, and they can provide one piece of evidence to help fully quantify the impact of e-commerce.

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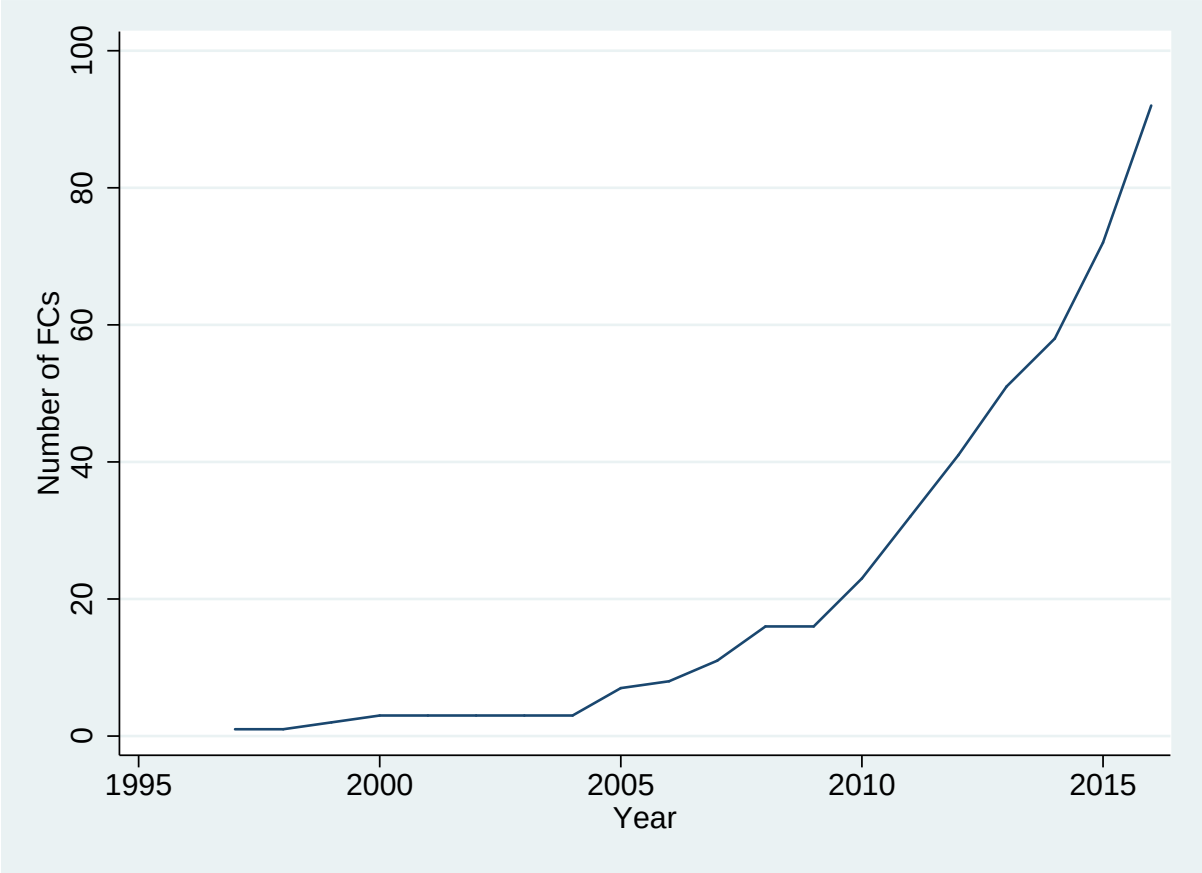
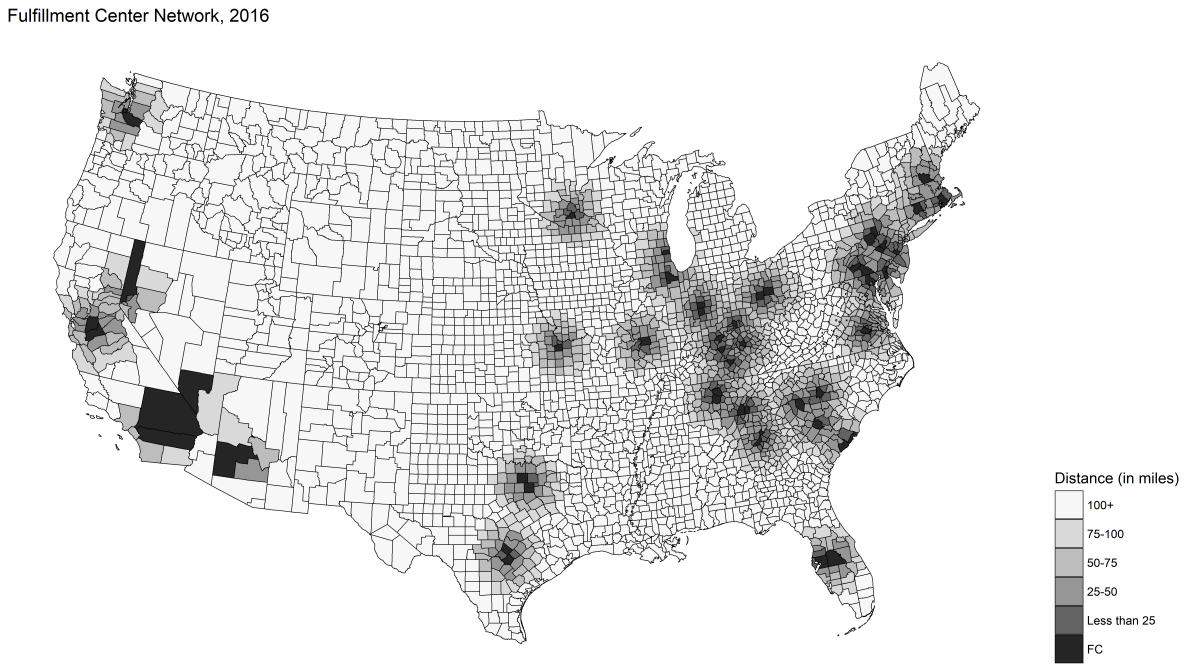
Figure 1: Number of E-Commerce Retailer's Fulfillment Centers



Note: The figure plots the number of the major e-commerce retailer's fulfillment centers over time.

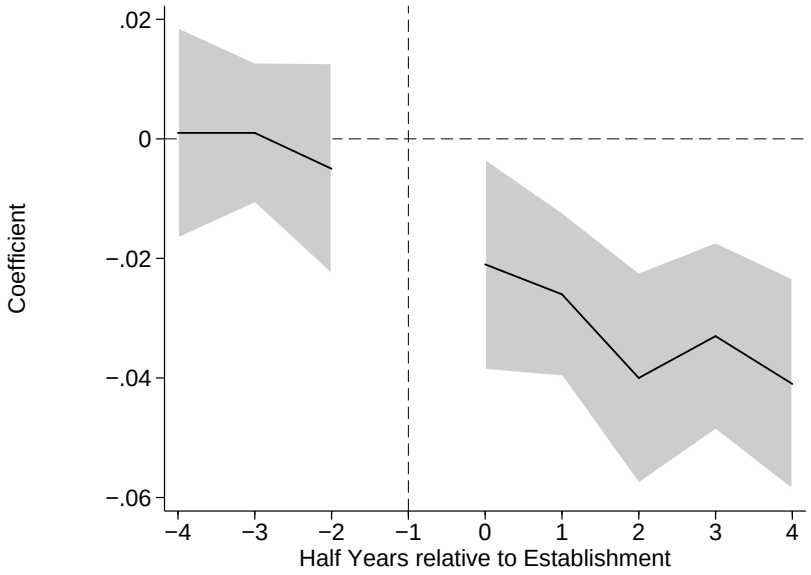
Figure 2: Major E-Commerce Retailer's FC Network



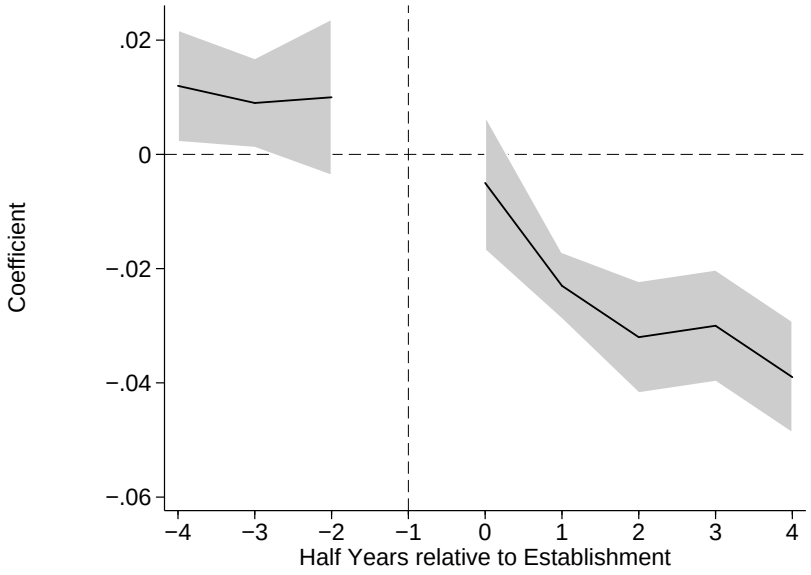
Note: The map highlights the locations of the major e-commerce retailer's fulfillment centers. The dark regions highlight the counties with fulfillment centers, while the light regions highlight the neighboring counties.

Figure 3: Dynamic Effect of FCs on Income

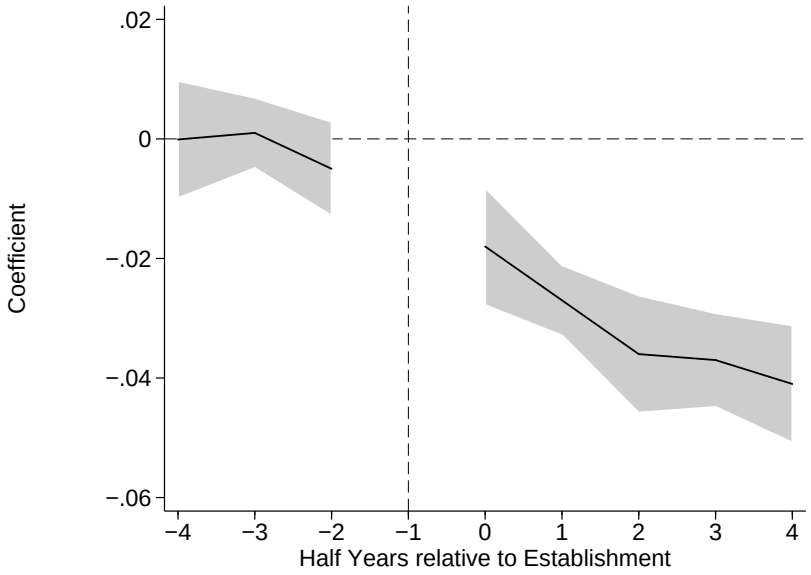
Panel A: Counties with FCs



Panel B: Counties within 50 Miles of FCs



Panel C: Counties within 100 Miles of FCs



Note: These figures present the dynamic effect of FCs on the income of hourly retail workers. We estimate Equation 2 and plot the estimated coefficients from $PreFC$ (j=-4 to j=-2) and $PostFC$ (j=0 to j=4) dummies which are defined at the semiannual frequency. $PreFC(-1)$ is dropped from the estimation so that all coefficient estimates can be treated as percentage changes relative to the income within two quarters before the establishment of FCs. The shaded area around the coefficients represents 5% confidence intervals. Panel A includes workers in counties with FCs. Panel B (C) includes workers in counties within 50 (100) miles of FCs but not in counties with FCs.

Table I: Summary Statistics

	Full Sample			FC Counties		
	N	Mean	Std Dev	N	Mean	Std Dev
Panel A: Worker-Level Data						
Hourly Workers						
Total Income ($ per quarter)	34,806,676	7,326	4,197	1,881,184	8,248	4,363
Wage Income ($ per quarter)	34,689,666	6,362	3,553	1,875,176	7,162	3,693
Bonus ($ per quarter)	32,510,284	1,030	1,105	1,764,173	1,151	1,195
Hours Worked (per week)	34,489,187	30.9	10.1	1,866,683	32.7	9.65
Wage Rate ($ per hour)	34,489,187	14.9	4.54	1,866,683	16	4.69
Salaried Workers						
Total Income ($ per quarter)	5,438,083	21,504	18,271	293,960	22,710	18,335
Panel B: Establishment-Level Data						
Sales ($ 000s per year)						
All Stores	13,902,516	1,494.2	8,708.3	186,400	2,272.8	10,747.7
Small Stores	4,855,755	205.1	65.6	64,208	260.9	331.3
Medium Stores	4,413,654	461.8	102.9	59,372	617.4	677.8
Large Stores	4,633,087	3,828.8	14,800.0	62,820	5,893.8	17,954.7
Employment (workers per year)						
All Stores	13,902,516	12.3	63.0	186,400	17.0	68.8
Small Stores	4,855,755	3.7	3.8	64,208	4.0	7.5
Medium Stores	4,413,654	5.5	3.2	59,372	6.7	4.3
Large Stores	4,633,087	27.9	107.4	62,820	40.1	114.8
Panel C: County-Industry Level Data						
Total Wage ($ 000s per quarter)	3,212,502	539.1	3,047.2	39,015	3,206.3	7,152.6
Employment	3,212,502	88.2	430.4	39,015	501.2	980.4
Establishment	3,212,502	8.1	23.0	39,015	32.2	44

Note: This table presents the summary statistics for the full sample and for counties with fulfillment centers. Panel A presents statistics for the quarterly worker-level data between 2010 and 2016 for retail workers. Panel B provides statistics for the annual sales and employment data at the establishment level for retail stores between 2010 and 2014. Panel C shows statistics for the quarterly county-industry level (6-digit NAICS) earnings and employment data between 2010 and 2016 for retail sector listed in Table A1. All dollar values are converted to December 2016 dollars using CPI from BLS.

Table II: Determinants of FC Locations: OLS

	FC				
	(1)	(2)	(3)	(4)	(5)
Δ Log(Retail Sales)	0.012***	0.005	0.005	0.005	0.005
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Δ Log(Population Density (per sq mile))		0.114***	0.115***	0.115***	0.116***
		(0.025)	(0.025)	(0.025)	(0.025)
Δ Unemployment rate			-0.000	-0.000	-0.000
			(0.001)	(0.001)	(0.001)
Δ Log Median household income				-0.008	-0.008
				(0.019)	(0.019)
Δ Perc. age b/w 18 and 65					0.000
					(0.001)
Constant	0.013***	0.009***	0.009***	0.011**	0.010**
	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Observations	3128	3109	3109	3109	3109
R^2	0.04	0.05	0.05	0.05	0.05

Note: This table presents the results for determinants of fulfillment centers' location. The dependent variable is whether the county has an FC in the post-2010 period. The independent variables are long differences computed from 2000 and 2010 county-level census data.

Table III: Effect of FCs on Income of Retail Workers

	Log(Total Income)		
	All Workers	Hourly Workers	Salaried Workers
	(1)	(2)	(3)
Panel A: Counties with FCs			
PostFC	−0.024***	−0.025***	−0.010
	(0.005)	(0.005)	(0.008)
Observations	2,175,144	1,881,184	293,960
Adjusted R^2	0.845	0.809	0.849
Panel B: Counties within 50 Miles of FCs			
PostFC	−0.020***	−0.022***	−0.001
	(0.004)	(0.004)	(0.008)
Observations	5,643,934	4,744,111	899,823
Adjusted R^2	0.865	0.828	0.856
Panel C: Counties within 100 Miles of FCs			
PostFC	−0.023***	−0.024***	−0.007*
	(0.002)	(0.002)	(0.004)
Observations	11,141,092	9,549,880	1,591,212
Adjusted R^2	0.858	0.821	0.863
Worker FE	✓	✓	✓
YearQtr FE	✓	✓	✓

Note: This table presents results of worker-level panel regressions assessing the effect of FCs on income using Equation 1. Panel A includes retail workers in counties with FCs. Panel B (C) includes retail workers in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1), (2), (3) includes all workers, hourly workers, and salaried workers, respectively. All regressions include worker and year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table IV: Firm-Specific Unobservables and Local Economic Conditions

	Log(Total Income)			
	Hourly Workers			
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	−0.021***	−0.008***	−0.011***	
	(0.004)	(0.002)	(0.002)	
PostFC*Retail				-0.044***
				(0.006)
Observations	1,881,184	1,881,184	1,881,184	5,596,632
Adjusted R^2	0.841	0.811	0.842	0.850
Panel B: Counties within 50 Miles of FCs				
PostFC	−0.017***	−0.020***	−0.018***	
	(0.003)	(0.003)	(0.002)	
PostFC*Retail				-0.029***
				(0.007)
Observations	4,744,111	4,744,111	4,744,111	14,247,756
Adjusted R^2	0.862	0.830	0.863	0.852
Panel C: Counties within 100 Miles of FCs				
PostFC	−0.021***	−0.015***	−0.013***	
	(0.002)	(0.002)	(0.001)	
PostFC*Retail				-0.026***
				(0.004)
Observations	9,549,880	9,549,880	9,549,880	26,914,902
Adjusted R^2	0.853	0.824	0.854	0.856
Worker FE	✓	✓	✓	✓
Firm-YearQtr FE	✓		✓	
State-YearQtr FE		✓	✓	
County-YearQtr FE				✓

Note: This table presents results of worker-level panel regressions assessing the effect of FCs on the income of *hourly workers* after controlling for firm-specific unobservables and local economic conditions. All columns include worker fixed effects. We replace year-quarter fixed effects in Equation 1 with firm-year-quarter, state-year-quarter, and firm-year-quarter and state-year-quarter fixed effects in Column (1), (2), (3), respectively. In Column (4), we include all hourly workers in other industries in addition to retail workers. We interact *PostFC* with *Retail*, where *Retail* identifies retail workers, and we control for county-year-quarter fixed effects. Panel A includes workers in counties with FCs. Panel B (C) includes workers in counties within 50 (100) miles of FCs but not in counties with FCs. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table V: Effect of FCs on Income: Worker Evidence (Western States)

	Log(Income)						
	0 mile	50 miles	100 miles	150 miles	200 miles	250 miles	500miles
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PostFC	-0.019^{**}	-0.039^{***}	-0.023^{***}	-0.020^{***}	-0.018^{***}	-0.014^{***}	-0.002
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	1,075,790	1,503,517	3,675,332	4,902,589	6,241,358	7,524,622	11,185,880
Adjusted R^2	0.846	0.856	0.839	0.839	0.842	0.849	0.864
Worker FE	YES	YES	YES	YES	YES	YES	YES
YearQtr FE	YES	YES	YES	YES	YES	YES	YES

Note: This table presents results of worker-level panel regressions assessing the heterogeneous effect of FCs on the income of hourly retail workers based on the distance from FCs using Equation 1. We include counties in the 24 western states of the U.S. and drop counties neighboring fulfillment centers established before 2010. For focal counties, we examine FCs opened after 2010 and, as we add more counties to treatment sample, we include all the FCs opened before 2010. All regressions include worker and year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table VI: Decomposition of Income Effect for Hourly Workers

	Log(Wage Income)	Log(Bonus)	Log(Hours Worked)	Log(Wage Rate)
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	-0.007***	-0.012	-0.007***	0.001***
	(0.002)	(0.008)	(0.002)	(0.000)
Observations	1,875,176	1,764,173	1,866,683	1,866,683
Adjusted R^2	0.842	0.740	0.731	0.970
Panel B: Counties within 50 Miles of FCs				
PostFC	-0.014***	-0.026***	-0.014***	-0.000
	(0.002)	(0.009)	(0.002)	(0.001)
Observations	4,730,532	4,414,584	4,711,647	4,711,647
Adjusted R^2	0.856	0.744	0.761	0.968
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.012***	-0.003	-0.011***	-0.001*
	(0.001)	(0.008)	(0.001)	(0.000)
Observations	9,521,947	8,921,138	9,476,710	9,476,710
Adjusted R^2	0.849	0.746	0.744	0.969
Worker FE	✓	✓	✓	✓
Firm-YearQtr FE	✓	✓	✓	✓
State-YearQtr FE	✓	✓	✓	✓

Note: This table presents results of worker-level panel regressions assessing the effect of FCs on the wage income, bonus, hours worked, and wage rate of hourly retail workers using Equation 1. Panel A includes workers in counties with FCs. Panel B (C) includes workers in counties within 50 (100) miles of FCs but not in counties with FCs. All regressions include worker, firm-year-quarter, and state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table VII: Heterogeneous Income Effect: Worker Age

	Log(Total Income)					
Age (in years)	<25	25-34	35-44	45-54	55-64	> 64
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Counties with FCs						
PostFC	-0.010***	-0.008***	-0.009***	-0.009***	-0.018***	-0.044***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.006)
Observations	168,518	382,786	420,745	493,286	299,402	95,063
Adjusted R^2	0.782	0.815	0.858	0.851	0.846	0.860
Panel B: Counties within 50 Miles of FCs						
PostFC	-0.027***	-0.018***	-0.012***	-0.016***	-0.020***	-0.035***
	(0.008)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)
Observations	381,232	900,078	998,542	1,289,569	858,504	262,581
Adjusted R^2	0.786	0.834	0.879	0.885	0.865	0.864
Panel C: Counties within 100 Miles of FCs						
PostFC	-0.021***	-0.012***	-0.009***	-0.012***	-0.018***	-0.026***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Observations	834,541	1,883,249	2,035,922	2,532,303	1,644,437	508,696
Adjusted R^2	0.783	0.828	0.871	0.874	0.853	0.853
Worker FE	✓	✓	✓	✓	✓	✓
Firm-YearQtr FE	✓	✓	✓	✓	✓	✓
State-YearQtr FE	✓	✓	✓	✓	✓	✓

Note: This table presents results of worker-level panel regressions assessing the heterogeneous effect of FCs on the income of hourly retail workers based on the age of workers using Equation 1. Panel A includes workers in counties with FCs. Panel B (C) includes workers in counties within 50 (100) miles of FCs but not in counties with FCs. All regressions include worker, firm-year-quarter, and state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table VIII: Heterogeneous Income Effect: Hours Worked and Gender

	Log(Total Income)		
	Counties with FCs	Counties within 50 Miles of FCs	Counties within 100 Miles of FCs
	(1)	(2)	(3)
Panel A: Part-time vs. Full-time			
PostFC*Part-time (1)	-0.016***	-0.028***	-0.021***
	(0.003)	(0.004)	(0.002)
PostFC*Full-time (2)	-0.010***	-0.014***	-0.010***
	(0.002)	(0.002)	(0.001)
Difference ((1)-(2))	-0.005	-0.014***	-0.011***
	(0.004)	(0.003)	(0.002)
Observations	1,872,894	4,725,658	9,505,116
Adjusted R^2	0.843	0.864	0.854
Worker FE	✓	✓	✓
PartTime-Firm-YearQtr FE	✓	✓	✓
PartTime-State-YearQtr FE	✓	✓	✓
Panel B: Female vs. Male			
PostFC*Female (3)	-0.011***	-0.016***	-0.014***
	(0.004)	(0.003)	(0.002)
PostFC*Male (4)	-0.019***	-0.020***	-0.015***
	(0.003)	(0.004)	(0.003)
Difference ((3)-(4))	0.008*	0.004	0.002
	(0.004)	(0.005)	(0.003)
Observations	644,483	1,910,447	3,367,008
Adjusted R^2	0.850	0.853	0.849
Worker FE	✓	✓	✓
Female-Firm-YearQtr FE	✓	✓	✓
Female-State-YearQtr FE	✓	✓	✓

Note: This table presents results of worker-level panel regressions assessing the heterogeneous effect of FCs on the income of hourly workers based on hours worked and gender of workers using Equation 1. Panel A compares part-time workers (less than 32 hours per week) and full-time workers (equal to or more than 32 hours per week). Panel B compares female workers and male workers. All regressions include worker, group-specific firm-year-quarter, and group-specific state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table IX: Robustness Check

	Log(Total Income)		
	Counties with FCs	Counties within 50 Miles of FCs	Counties within 100 Miles of FCs
	(1)	(2)	(3)
Panel A: All FCs			
PostFC	-0.006*	-0.011***	-0.010***
	(0.003)	(0.002)	(0.001)
Observations	2,789,307	6,703,938	13,858,442
Adjusted R^2	0.852	0.870	0.863
Panel B: Excluding Migrants			
PostFC	-0.013***	-0.019***	-0.014***
	(0.002)	(0.002)	(0.001)
Observations	1,632,007	4,041,405	8,208,699
Adjusted R^2	0.848	0.869	0.860
Panel C: Alternative Measure of Income			
PostFC	-0.009***	-0.013***	-0.010***
	(0.002)	(0.002)	(0.001)
Observations	1,885,938	4,753,073	9,570,559
Adjusted R^2	0.911	0.918	0.914
Worker FE	✓	✓	✓
Firm-YearQtr FE	✓	✓	✓
State-YearQtr FE	✓	✓	✓

Note: This table presents robustness checks for the effect of FCs on the income of hourly retail workers using Equation 1. Panel A uses all FCs including those established before 2010. Panel B excludes migrants whose first and last ZIP codes in the data are different. Panel C uses the modeled annualized income as an alternative measure of income. All regressions include worker, firm-year-quarter, and state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table X: Heterogeneous Effect on Credit Scores: Bank Card Utilization

	Counties with FCs (1)	Counties within 50 Miles of FCs (2)	Counties within 100 Miles of FCs (3)
	Panel A: Credit Scores		
PostFC*Low (1)	0.593	0.004	0.110
	(0.786)	(0.433)	(0.325)
PostFC*High (2)	-2.500***	-0.835*	-0.495*
	(0.220)	(0.506)	(0.282)
Difference ((2)-(1))	-3.089***	-0.849	-0.605*
	(0.735)	(0.609)	(0.367)
Observations	1,210,611	3,191,674	6,441,603
Adjusted R^2	0.812	0.826	0.822
Worker FE	✓	✓	✓
Low-Firm-YearQtr FE	✓	✓	✓
Low-State-YearQtr FE	✓	✓	✓
	Panel B: Bank Card 90+ Day Delinquency		
PostFC*Low (3)	-0.001*	-0.000	0.001
	(0.001)	(0.001)	(0.000)
PostFC*High (4)	0.006***	0.002**	0.000
	(0.001)	(0.001)	(0.001)
Difference ((4)-(3))	0.007***	0.003***	-0.000
	(0.001)	(0.001)	(0.001)
Observations	1,081,133	2,879,747	5,778,895
Adjusted R^2	0.108	0.111	0.112
Worker FE	✓	✓	✓
Low-Firm-YearQtr FE	✓	✓	✓
Low-State-YearQtr FE	✓	✓	✓

Note: This table presents results of worker-level panel regressions assessing the heterogeneous effect of FCs on credit scores and bank card 90+ day delinquency of hourly workers based on bank card utilization using Equation 1. All regressions include worker, group-specific firm-year-quarter, and group-specific state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XI: Effect of FCs on Sales of Retail Stores

	Log(1+Sales)			
	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	-0.028***	-0.029**	-0.020***	-0.034***
	(0.009)	(0.012)	(0.005)	(0.012)
Observations	184,829	63,636	58,799	62,394
Adjusted R^2	0.959	0.808	0.728	0.918
Panel B: Counties within 50 Miles of FCs				
PostFC	-0.009	0.010	-0.024***	-0.017
	(0.008)	(0.008)	(0.008)	(0.018)
Observations	509,182	172,954	164,848	171,362
Adjusted R^2	0.956	0.801	0.744	0.914
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.011*	-0.018***	-0.009*	-0.005
	(0.005)	(0.003)	(0.005)	(0.011)
Observations	1,073,929	364,588	348,315	361,015
Adjusted R^2	0.958	0.800	0.765	0.921
Establishment FE	✓	✓	✓	✓
Ind-Year FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of establishment-level panel regressions assessing the heterogeneous effect of FCs on the sales of retail establishments/stores based on the size of stores. Panel A includes establishments in counties with FCs. Panel B (C) includes establishments in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all stores, while Column (2)-Column (4) report results for terciles based on sales one year before the establishment of FCs in the county or neighboring county. All regressions include establishment, industry-year and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XII: Effect of FCs on Employment of Retail Stores

	Log(1+Employment)			
	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	-0.024***	-0.027***	-0.019***	-0.025***
	(0.007)	(0.006)	(0.005)	(0.009)
Observations	184,829	63,636	58,799	62,394
Adjusted R^2	0.973	0.871	0.907	0.955
Panel B: Counties within 50 Miles of FCs				
PostFC	-0.007	-0.003	-0.015***	-0.005
	(0.004)	(0.003)	(0.005)	(0.007)
Observations	509,182	172,954	164,848	171,362
Adjusted R^2	0.973	0.875	0.912	0.956
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.008**	-0.011***	-0.007**	-0.005
	(0.003)	(0.003)	(0.003)	(0.004)
Observations	1,073,929	364,588	348,315	361,015
Adjusted R^2	0.978	0.883	0.924	0.969
Establishment FE	✓	✓	✓	✓
Ind-Year FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of establishment-level panel regressions assessing the heterogeneous effect of FCs on the employment of retail establishments/stores based on the size of stores. Panel A includes establishments in counties with FCs. Panel B (C) includes establishments in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all stores, while Column (2)-Column (4) report results for terciles based on sales one year before the establishment of FCs in the county or neighboring county. All regressions include establishment, industry-year and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XIII: Retail Store Closures: Size Effect

	Exit			
	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	0.029***	0.038**	0.027***	0.023***
	(0.010)	(0.014)	(0.009)	(0.007)
Observations	161,520	53,753	53,470	54,297
Adjusted R^2	0.214	0.222	0.213	0.207
Panel B: Counties within 50 Miles of FCs				
PostFC	0.023***	0.035***	0.022***	0.012**
	(0.007)	(0.009)	(0.007)	(0.005)
Observations	446,770	148,440	148,708	149,604
Adjusted R^2	0.202	0.207	0.201	0.200
Panel C: Counties within 100 Miles of FCs				
PostFC	0.014***	0.020***	0.012**	0.011***
	(0.004)	(0.005)	(0.005)	(0.003)
Observations	973,208	322,744	324,191	326,261
Adjusted R^2	0.204	0.207	0.199	0.204
Establishment FE	✓	✓	✓	✓
Ind-Year FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of establishment-level panel regressions assessing the heterogeneous effect of FCs on the exit rates of retail establishments/stores based on the size of stores. Here, we define *exit* dummy, our dependent variable, as 1 if the establishment ceases to exist one year before the end of the sample period, and 0 otherwise. Panel A includes establishments in counties with FCs. Panel B (C) includes establishments in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all stores, while Column (2)-Column (4) report results for terciles based on sales one year before the establishment of FCs in the county or neighboring county. All regressions include establishment, industry-year and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XIV: Retail Store Closures: Age Effect

	Exit			
	All	Young	Medium	Old
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	0.032**	0.041**	0.043***	0.007
	(0.012)	(0.016)	(0.013)	(0.010)
Observations	75,162	26,080	25,093	23,984
Adjusted R^2	0.204	0.217	0.199	0.185
Panel B: Counties within 50 Miles of FCs				
PostFC	0.032***	0.040***	0.030***	0.025***
	(0.008)	(0.011)	(0.009)	(0.007)
Observations	226,182	79,011	72,694	74,465
Adjusted R^2	0.198	0.212	0.190	0.182
Panel C: Counties within 100 Miles of FCs				
PostFC	0.016***	0.022***	0.015***	0.009***
	(0.005)	(0.008)	(0.004)	(0.003)
Observations	508,929	179,539	165,739	163,630
Adjusted R^2	0.202	0.211	0.196	0.191
Establishment FE	✓	✓	✓	✓
Ind-Year FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of establishment-level panel regressions assessing the heterogeneous effect of FCs on the exit rates of retail establishments/stores based on the age of stores. Here, we define *exit* dummy, our dependent variable, as 1 if the establishment ceases to exist one year before the end of the sample period, and 0 otherwise. Panel A includes establishments in counties with FCs. Panel B (C) includes establishments in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all stores for which we observe the store's age, while Column (2)-Column (4) report results for terciles based on store's age one year before the establishment of FCs in the county or neighboring county. All regressions include establishment, industry-year, and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XV: Opening of Retail Stores

	Log(1+Entrants)			
	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	-0.015	-0.118**	0.095	0.019
	(0.040)	(0.052)	(0.083)	(0.078)
Observations	180	180	180	180
Adjusted R^2	0.989	0.974	0.981	0.968
Panel B: Counties within 50 Miles of FCs				
PostFC	-0.088**	-0.054	-0.043	-0.133**
	(0.039)	(0.053)	(0.073)	(0.055)
Observations	2,205	2,205	2,205	2,205
Adjusted R^2	0.966	0.937	0.933	0.934
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.138***	-0.132***	-0.070*	-0.126***
	(0.030)	(0.039)	(0.038)	(0.039)
Observations	5,690	5,690	5,690	5,690
Adjusted R^2	0.954	0.919	0.916	0.913
Establishment FE	✓	✓	✓	✓
Ind-Year FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of county-level panel regressions assessing the heterogeneous effect of FCs on the entry rates of retail establishments/stores based on the size of stores. Here, we define *entry rate* as the logged value of the number of entrants in a given county. Panel A includes entrants in counties with FCs. Panel B (C) includes entrants in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all stores, while Column (2)-Column (4) report results for terciles based on the first-year sales after the entry. All regressions include establishment, industry-year, and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XVI: Placebo Tests: Sales of Full-Service Restaurants

	Log(1+Sales)			
	All	Small	Medium	Large
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	0.002	0.012***	-0.002	-0.002
	(0.002)	(0.004)	(0.002)	(0.004)
Observations	110,043	36,097	40,546	33,400
Adjusted R^2	0.979	0.895	0.856	0.944
Panel B: Counties within 50 Miles of FCs				
PostFC	0.003	0.010	-0.003	0.000
	(0.003)	(0.008)	(0.004)	(0.008)
Observations	311,132	100,077	115,726	95,329
Adjusted R^2	0.980	0.892	0.806	0.956
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.001	-0.003	-0.001	-0.001
	(0.001)	(0.003)	(0.001)	(0.005)
Observations	616,376	198,337	227,175	190,859
Adjusted R^2	0.978	0.879	0.822	0.949
Establishment FE	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓

Note: This table presents results of establishment-level panel regressions assessing the heterogeneous effect of FCs on the sales of full-service restaurants (NAICS 722511) based on the size of restaurants. Panel A includes establishments in counties with FCs. Panel B (C) includes establishments in counties within 50 (100) miles of FCs but not in counties with FCs. Column (1) reports results for all restaurants, while Column (2)-Column (4) report results for terciles based on sales one year before the establishment of FCs in the county or neighboring county. All regressions include establishment and state-year fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table XVII: Effect of FCs on Wages and Employment: County-Industry Evidence

	Log(1+Total Wage)	Log(1+Employment)	Log(1+Average Wage)	Log(1+Establishment)
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	-0.054	-0.017	-0.040	0.002
	(0.103)	(0.038)	(0.065)	(0.011)
Counties	47	47	47	47
Observations	35,146	35,146	35,146	35,146
Adjusted R^2	0.346	0.615	0.180	0.861
Panel B: Counties within 50 Miles of FCs				
PostFC	-0.185*	-0.062**	-0.132**	-0.019***
	(0.097)	(0.031)	(0.066)	(0.007)
Counties	340	340	340	340
Observations	136,336	136,336	136,336	136,336
Adjusted R^2	0.376	0.628	0.210	0.873
Panel C: Counties within 100 Miles of FCs				
PostFC	-0.093*	-0.037*	-0.057*	-0.005*
	(0.053)	(0.020)	(0.034)	(0.003)
Counties	808	808	808	808
Observations	260,341	260,341	260,341	260,341
Adjusted R^2	0.419	0.675	0.229	0.883
County FE	✓	✓	✓	✓
Ind-YearQtr FE	✓	✓	✓	✓
State-YearQtr FE	✓	✓	✓	✓

Note: This table presents results of county-industry level panel regressions assessing the aggregate effect of FCs on total wages, employment, average wage, and the number of establishments. Panel A includes counties with FCs. Panel B (C) includes counties within 50 (100) miles of FCs but not in counties with FCs. All regressions include county, industry-year-quarter, and state-year-quarter fixed effects. Standard errors clustered by county are reported in parentheses. *, **, *** indicate significance at the 10%, 5%, and 1% level.

Table A1: County Demographics

	Full Sample	Counties with FCs	Counties within 50 Miles of FCs	Counties within 100 Miles of FCs
N	3,135	50	445	1,141
Total Population	308,674,608	30,774,770	86,724,715	163,939,679
Population	98,460.80	615,495.40	194,887.00	143,680.70
Population Density	259.49	672.84	698.24	531.76
Retail Sales (in millions)	431.71	2,827.55	821.65	615.01
Retail Sales per Capita	3,552.72	4,623.48	3,692.95	3,679.81
Median Household Income	43,419.43	56,220.34	51,179.56	47,123.08
Unemployment Rate	9.36	9.64	10.17	10.27
Percent Age under 18	23.49	24.80	23.77	23.22
Percent Age over 65	15.93	12.16	14.08	15.31
Percent High School Graduate or Higher	82.51	85.84	83.04	82.31
Percent Bachelor's Degree or Higher	18.73	27.52	21.74	19.73

Note: This table presents demographics statistics of 3,135 counties based on Census 2010.

Table A2: List of Retail Sectors

NAICS	Industry Name
441310	Automotive parts and accessories stores
441320	Tire dealers
442110	Furniture stores
442210	Floor covering stores
442291	Window treatment stores
442299	All other home furnishings stores
443141	Household appliance stores
443142	Electronics stores
444110	Home centers
444120	Paint and wallpaper stores
444130	Hardware stores
444190	Other building material dealers
444210	Outdoor power equipment stores
444220	Nursery, garden, and farm supply stores
446120	Cosmetic and beauty supply stores
446191	Food, health, supplement stores
446199	All other health and personal care stores
448110	Men's clothing stores
448120	Women's clothing stores
448130	Children's and infants' clothing stores
448140	Family clothing stores
448150	Clothing accessories stores
448190	Other clothing stores
448210	Shoe stores
448310	Jewelry stores
448320	Luggage and leather goods stores
451110	Sporting goods stores
451120	Hobby, toy, and game stores
451130	Sewing, needlework, and piece goods stores
451140	Musical instrument and supplies stores
451211	Book stores
451212	News dealers and newsstands
451220	Precorded tape, cd, and record stores
452111	Department stores, except discount
452112	Discount department stores
452910	Warehouse clubs and supercenters
452990	All other general merchandise stores
453110	Florists
453210	Office supplies and stationery stores
453220	Gift, novelty, and souvenir stores
453310	Used merchandise stores
453910	Pet and pet supplies stores
453920	Art dealers
453930	Manufactured, mobile, home dealers
453991	Tobacco stores
453998	Store retailers not specified elsewhere

Note: This table presents 6-digit NAICS industries that we include in our analysis.