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Creative Destruction? Impact of E-Commerce on the Retail Sector

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Abstract. Using an administrative payroll data set for 2.6 million retail workers, we find that the staggered rollout of a major e-commerce firm's fulfillment centers reduces traditional retail workers' income in geographically proximate counties by 2.4%. Wages of hourly workers, especially part-time hourly workers, decrease significantly 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, some workers experience an increase in credit card delinquency. Using data for 3.2 million stores, we find that sales (employment) at proximate stores decrease by 4% (2.1%). Exits, especially of young and small stores, increase, and entry decreases. Our results highlight how creative destruction led by e-commerce impacts local labor markets.

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1. Introduction

Technological advances can lead to Schumpeterian creative destruction, in which old capital (both physical and human) is replaced by new capital and incumbent firms are replaced by new entrants. Technological innovation led by e-commerce has changed the landscape of the retail sector in the United States dramatically with the share of e-commerce sales in total retail sales increasing from 0.63% in 1999 to 13.3% in 2021 (Census Bureau). The retail sector is an important sector of the U.S. economy; it employed approximately 16 million workers at the end of 2019, and each additional job in the retail sector helped create 1.22 jobs in other sectors (Bureau of Labor Statistics, BLS). In this paper, we study how increased competition from e-commerce affects the traditional retail sector and its employees.

It is unclear how brick-and-mortar retail firms would respond to competition from e-commerce firms and what the resulting impact on their workers would be. On the one hand, traditional retailers may focus on increased customer services (e.g., in-store assistance of sales specialists, technical support, pickup, and returns) and offer more incentives to employees for providing such services. As a result, brick-and-mortar firms may hire more workers. On the other hand, affected firms may cut operating costs by reducing wages, adjusting their level and composition of employment, or closing stores. Similarly, it is not clear if and how brick-and-mortar retail firms would adjust different margins based on the geography of their retail stores and their worker composition.

Identifying the causal impact of e-commerce on traditional brick-and-mortar retailers is challenging because we cannot observe the counterfactual. We address this identification challenge by using the staggered rollout of the fulfillment centers (FCs) of a major e-commerce retailer across the United States. At the beginning of 2000, the e-commerce retailer we study 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. We use this rollout as a proxy for the presence of local e-commerce.

To examine the impact on employees, we analyze the impact of the establishment of FCs on the wage income of retail workers using a matched employer-employee payroll data set from a major credit bureau. This data set contains major retail firms that employ approximately 2.6 million retail workers; these workers comprise 18%

of total U.S. retail employment. The rich payroll information allows us to group workers into hourly workers and nonhourly workers. These data include total compensation, bonuses, commissions, and wage/salary rate. Using difference-in-differences regressions, we find that the labor income of retail workers in counties with FCs decreases by 2.4%, on average, after the establishment of FCs. This negative effect is also significant for workers within 100 miles of FCs but disappears beyond 100 miles. In turn, these results highlight the geographically circumscribed impact of e-commerce on jobs.

This aggregate effect on the workers, however, hides important worker-level heterogeneity. Our main results are focused on hourly workers, who experience a 2.5% decrease in labor income, equivalent to an $825 decrease in annual income. Most of the effect can be attributed to a reduction in the number of hours worked. Among hourly workers, we find a particularly strong negative impact on part-time hourly workers, and both young and old workers experience a sharper drop in labor income. The effect is more negative for workers who have less tenure, and the effect is attenuated by experience. Further, these negative income shocks also have differential consequences to workers; those who have a higher prior credit card utilization and workers who are otherwise more financially vulnerable experience higher credit card delinquencies and subsequent declines in their credit scores.

We deepen our analysis by examining the impact at the retail store-level. Using the National Establishments Time Series (NETS) data set, we find that, after the establishment of the FCs, the average brick-and-mortar retail store experiences a 4% decrease in sales. We document that, in response to these lower sales, affected stores adjust employment through a 2.1% reduction in head count. In addition to lower sales and reduced head count, affected stores experience an increase in the probability of closing by three percentage points relative to the average annualized exit rate of 13.6% in our sample: a 22% increase. But heterogeneity exists; small and young stores are more likely to exit than large and older stores. Further, the entry rate for small store openings is reduced by 8.1%.

Finally, we move to county-level data to examine the impact of FC establishment on county-level employment and total wages. Using the Quarterly Census of Employment & Wages (QCEW) provided by the BLS, we find that the establishment of FCs has a negative effect on the employment of the retail sector in FC counties, whereas it creates jobs in the transportation and warehousing sector. Further, the negative impact on the retail sector remains significant for counties within 100 miles of FCs.

The introduction of an FC to a county may impact local brick-and-mortar stores in two ways. First, it may impact local stores through increased labor market competition wherein workers leave to work at the FC. We have no direct evidence of this, but our results are largely inconsistent with this interpretation. Second, the establishment of an FC increases the attractiveness of shopping at the e-commerce company at the expense of the local retailer. The objective of the e-commerce retailer's FC network deployment is to optimize delivery to consumers. In turn, these proximate locations help to speed up deliveries in the areas near the FCs. It is likely that some consumers are more sensitive to shipping time and, thus, are more willing to purchase from the e-commerce retailer with reduced shipping time. Our results are consistent with this interpretation.

The main concern with our empirical approach is that the decision to establish an FC in a county may be correlated with unobservables, such as local economic conditions, and thus may bias our estimates. The largest concern is that the establishment of an FC is correlated with consumer demand, in particular the establishment of an FC into areas where retail is already declining. However, we find no evidence of negative selection; growth in the unemployment rate, median household income, and working age population do not correlate with the choice of FC locations. If anything, we observe positive selection wherein an increase in population density predicts FC establishment. To further reduce the concern of consumer demand driving FC establishment, we include establishment-level fixed effects, control for time-varying measures of education and wealth, include region × year–quarter fixed effects to absorb unobservable local economic conditions, and run a triple difference specification in which we add more granular county × year–quarter fixed effects using workers of nonretail firms as a control group. Our results are robust when we construct a Bartik-style instrumental variable (IV) using the interaction of the distance between the FCs and the closest U.S. Postal Service (USPS) network facility and state-level generosity of corporate subsidies. Finally, we include firm × year–quarter fixed effects in our regressions to control for any strategic firm-level response. For diagnostic purposes and transparency, we present many of our results visually to test for pretrends prior to the establishment of the FC and find none, increasing our confidence that the results we present are less likely to be biased.

2. Literature Review

To the best of our knowledge, our paper is the first to study the heterogeneous impact of e-commerce on retail sector employment, the income of retail workers and their credit scores, and the entry–exit dynamics of retail stores. Prior literature shows that e-commerce provides consumers lower prices and increases consumer surplus. Brynjolfsson and Smith (2000) show that the e-commerce prices of homogeneous products—books and CDs—are lower than those of conventional retail stores. Later, Brynjolfsson et al. (2003) show that increased product variety of online bookstores enhanced consumer welfare by $731 million to $1.03 billion in the year 2000. Further, the internet and information systems facilitated the creation of a used product market. Ghose et al. (2006) show that online used book sales only cannibalize 16% of the new book purchases. Furthermore, they show that an increase in book readership from the online used book marketplace increases consumer surplus. Relatedly, Ghose and Yao (2011) show that internet-based electronic markets exhibit low price dispersion and, in turn, increase consumer surplus by almost $100 million a year. We add to this literature by analyzing the impact of electronic markets on workers in brick-and-mortar stores.

In their study, Forman et al. (2009) highlight the importance of geography. They show that consumer surplus from online buying depends on physical location. In the early days of e-commerce, there were many reasons why consumers did not buy online, including delays in shipping, difficulty in assessing product quality, and challenges in returns. Furthermore, they show that, when a local store opens, people move away from online purchasing. Using recent data, we show that, when a large e-commerce retailer opens a storage facility (FC), brick-and-mortar sales go down. Later studies also highlight the importance of geography for the interaction between online and off-line retailers (Brynjolfsson et al. 2009, Overby and Forman 2015, Kitchens et al. 2018, Kumar et al. 2019, Nault and Rahman 2019, Chen and Qian 2020, Chan et al. 2021). Whereas the aforementioned literature deepens our understanding at the firm level, worker-level outcomes are largely overlooked. We contribute to this literature by analyzing the labor market consequences of a major e-commerce retailer expanding its FC network.

Finally, we also contribute to the literature on information technology (IT) and labor markets. Forman et al. (2012) show that the benefits of internet technology accrue only to counties that are already highly wealthy, educated, heavily populated, and have IT-intensive industries. Later studies document the impact of information systems and the internet on the labor market, job-hopping behavior, hiring biases, worker participation in the gig economy, and wages in high tech (Tambe and Hitt 2014, Chan and Wang 2018, Huang et al. 2020, Tambe et al. 2020). We add to this literature and document the impact of e-commerce—which relies heavily on information technology—on the wages of brick-and-mortar workers.

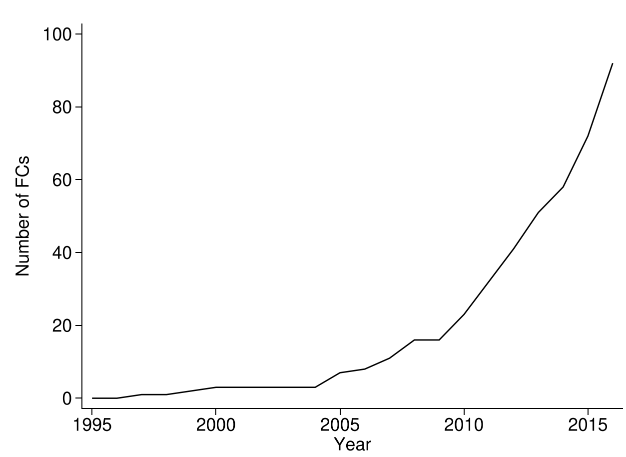
Our paper relates to research on the causes and consequences of disruption in the retail sector. Matsa (2011) shows that supermarket stores have fewer inventory shortfalls after the entry of Walmart. Khanna and Tice (2000) study how discount department stores respond to Walmart's entry. Holmes (2011) 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 et al. (2008) estimate the employment and earnings effect as a result of Walmart store openings. We contribute to this literature by investigating the disruption attributed to e-commerce.

3. Empirical Design

In this study, we seek to examine the impact of e-commerce–driven creative destruction on the retail sector. We proxy the presence of e-commerce in the local area by the establishment of FCs of a major e-commerce retailer. The e-commerce retailer built its first FC in 1997, and the number of FCs increased to more than 90 by the end of 2016 (Figures 1 and 2). To isolate the effects of the establishment of fulfillment centers from other regional, sectoral, and macrolevel shocks, we exploit the staggered rollout of FCs to capture the increase in the presence of local e-commerce. Specifically, our empirical strategy estimates the impact of the establishment of an FC in a county on the retail sector in that same county and neighboring counties (see Internet Section IA1 on location choice of FCs).[1]

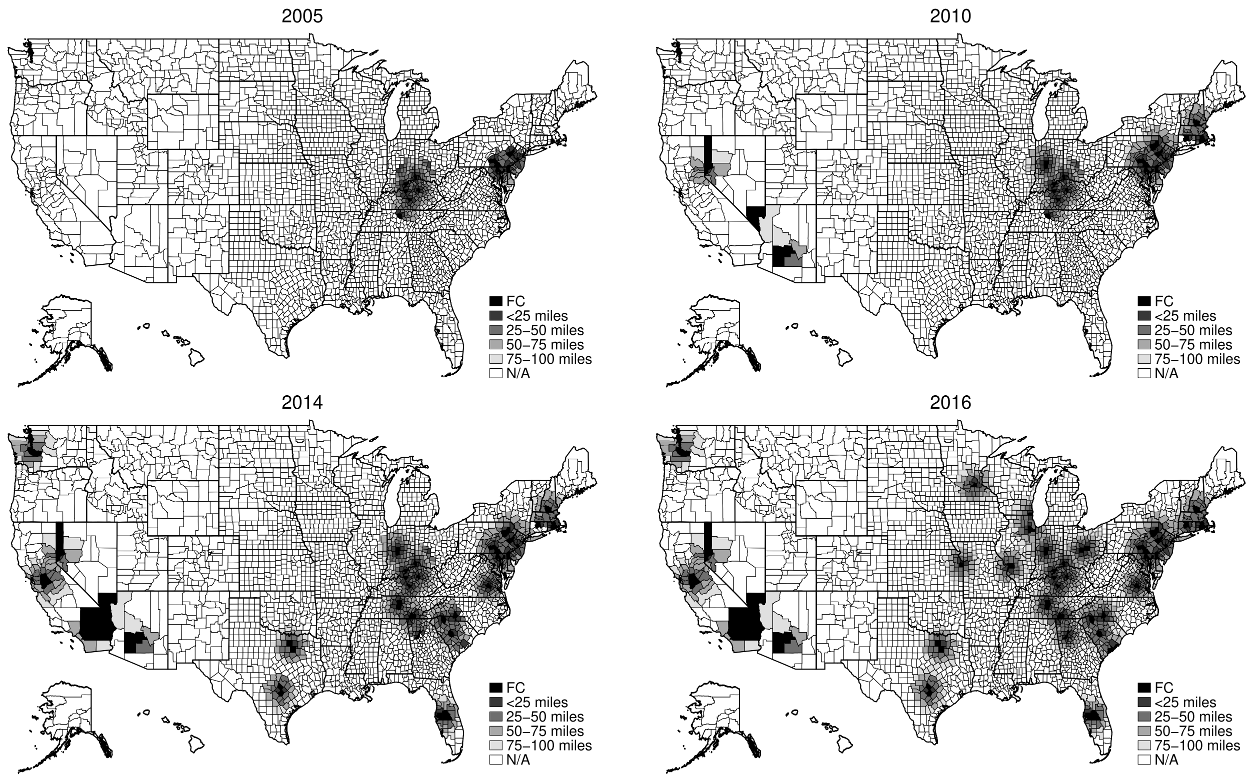
Our empirical objective is to evaluate the local effect of the establishment of a new FC. We do so by focusing on two definitions of local: (1) the focal county (i.e., the county where the FC opened) and (2) all counties within 100 miles of the FC (excluding the focal county where the FC is located). We remove FCs established in a county that contains an existing FC or established within 20 miles of an existing FC. This reduces our sample to 50 FCs. Our worker-level data begins in 2010, whereas our establishment- and county-level data are available for earlier years. To be consistent with the worker-level

Figure 1. Number of the Major E-Commerce Retailer's Fulfillment Centers



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

Figure 2. Major E-Commerce Retailer's Fulfillment Centers Network



Notes. The maps highlight the locations of the major e-commerce retailer's FCs in years 2005, 2010, 2014, and 2016. The dark regions highlight the counties with fulfillment centers, whereas the light regions highlight the neighboring counties.

data, we utilize data beginning in 2010. Therefore, our study focuses on the 39 FCs established after 2009.[2]

We treat each county as treated in the first quarter in which an FC opens in one of the two definitions of a local county. For example, for the analysis centered on the focal county level, the indicator for Fulton County, Georgia, activates in the first quarter of 2015 because an FC was opened in Union City, Georgia (which is in Fulton County), in February 2015. In our 100-mile level analysis, Cobb County, Georgia, is treated in the third quarter of 2011 because of the opening of an FC in Hamilton County, Tennessee, in September 2011.

A standard approach for evaluating the impact of the opening of an FC is comparing differences in the performance of a brick-and-mortar establishment before and after the opening of an FC in treated versus untreated counties. For this difference-in-differences specification to yield unbiased estimates, parallel trends between the treated and control counties must be present. However, focal and surrounding counties where FCs open are very different from the rest of the United States in terms of demographics and local economic variables, such as population, population density, retail sales, retail sales per capita, household income, and unemployment rates (see Internet Table IA1). Therefore, these untreated counties may not serve as appropriate counterfactuals for our analysis. As a result, to ensure that both treated and control counties are on the common empirical support, we only include counties that were classified as treated at any time by the opening of an FC in our analysis. 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. As a robustness check, we conduct coarsened exact matching (CEM) to produce a matched sample (Iacus et al. 2012); our results hold using the matched sample (see Internet Section IA7).

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 and sales/employment of retail stores by estimating the following:

$$Log(Y_{i,c,t}) = \alpha + \beta PostFC_{c,t} + \eta_i + \theta_t + \epsilon_{i,c,t}, \qquad (1)$$

and for worker-level data, we estimate regressions at a quarterly frequency, in which each quarterly observation is the income of worker i who works in county c at time t. The indicator variable *PostFC* equals one in the quarter when an FC is established in county c or within 100 miles of county c, and *PostFC* remains equal to one for all subsequent quarters. We control for time-invariant, worker-specific characteristics and year–quarter shocks by including worker (η_i) and year–quarter (θ_t) fixed effects, respectively. Standard errors are clustered at the FC level. The variable β estimates the percentage change in income attributed to the establishment of an FC. For establishment-level data, we use the annualized version of Equation (1) and include establishment and year fixed effects.

The reader may be concerned that the establishment of an FC in a county changes the composition of firms. In this case, better performing firms or firms that have more options for managing their geographically varied portfolio of stores choose to exit those counties. Further, better performing retailers decide not to enter the treated counties. Whereas this would lead to an apparent drop in retail performance, the effect 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. We include only stores/workers that are present both before and after the establishment of an FC. In addition, we include establishment-/worker-level fixed effects to control for establishment-/worker-level quality and other time-invariant covariates of the establishments/workers.

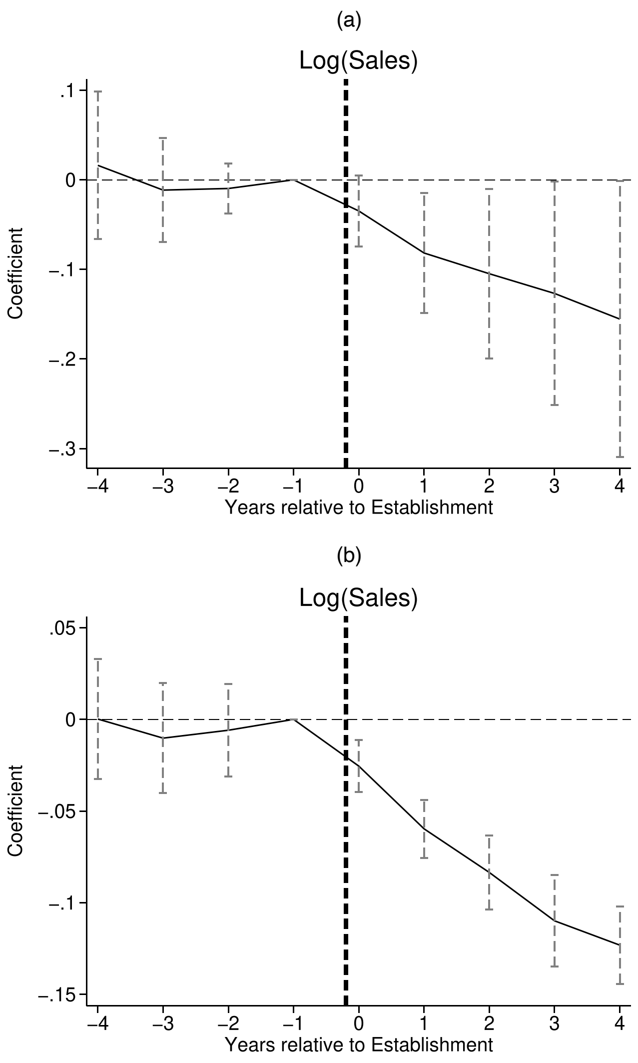
3.1. Identification Challenges

For β from Equation (1) to represent an unbiased estimate of the impact of the establishment of an FC on the income of retail workers and sales/employment at retail stores, we must assume that *PostFC* is orthogonal to any unobservables contained within ϵ. 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 is naturally a function of local economic conditions. Because 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 that feature high retail sales and high population density. To test this, on a cross-section consisting of all counties in the United States, we regress the likelihood of establishing an FC in county c on county-level levels (year 2010) and 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 (see Internet Table IA2).

We find that counties with higher retail sales, population density, and household income are likely to have FCs. The coefficient estimates for unemployment rate and age are insignificant. Further, using differences, we observe that FCs are more likely to be located in counties with faster growing population densities. But, importantly, FCs are not more likely to be located in counties that have experienced more growth in retail sales. Whereas our county-level fixed effects absorb all the time-invariant level effects of county-specific characteristics, our estimates are biased downward insofar as counties that experience large increases in population density are also more likely to engage in e-commerce transactions that substitute for local retail purchases. However, if this were true, we would also observe downward trends in retail sales in the period before the FC's establishment, but we do not observe this (see Section 5.1, Figure 3).

Figure 3. Effect of FCs on Retail Store Sales



Notes. These figures present the dynamic effect of FCs on the sales of retail establishments/stores. We use NETS establishment-level sales data to estimate Equation (2) and plot the estimated coefficients from the *PreFC* ($j = -4$ to $j = -2$) and *PostFC* ($j = 0$ to $j = 4$) dummies, which are defined at an annual frequency. *PreFC*(-1) is dropped from the estimation so that all coefficient estimates can be treated as changes relative to the sales one year before the establishment of FCs. The regressions include establishment/store fixed effects to estimate within store coefficients. The broken lines around the coefficients represent 95% confidence intervals. Panel (a) includes data for establishments located in counties with FCs. Panel (b) includes data for establishments located in counties within 100 miles of FCs but not in counties with FCs. (a) Counties with FCs. (b) Counties within 100 miles of FCs.

Another possible concern with our identification strategy includes the "endogenous" choice of the e-commerce retailer to expand its network by entering first in areas in which online sales were expected to grow faster, that is, change in consumer preferences. We test for this prediction using local web search activity for eBay.com, the second largest e-commerce website that does not have FCs (see Internet Section IA3). We find no effect of FCs on web search activity for eBay. These results suggest that our results are not completely driven by expected growth in online sales in the local area. Nonetheless, we control for any region-level time-varying unobservables using region (census division)–year–quarter fixed effects. Further, the anecdotal evidence from news articles suggests that these FCs can reduce shipping time and boost online consumption in the local areas (see Internet Section IA3).

It is plausible that governments may want the FC to be built in an area with weak economic conditions to boost the local economy. As a result, FC county selection may be negatively correlated with local economic conditions. However, we find that 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 (see Internet Table IA2, panel B, columns (4) and (5)). We conduct additional tests to mitigate concerns that firm-specific or local economic unobservables may explain the results (see Section 5.2.2).

4. 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 data set and its construction in this section.

4.1. Worker Data and Sample Construction

Our novel comprehensive consumer data are provided by a major credit bureau. These data, which employers provide directly to the credit bureau, contain detailed employment information, including company name; three-digit North American Industry Classification System (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 through which payments are made to the employee, total compensation, wage/salary, overtime, bonuses, commissions, and wage/salary rate. We group workers into hourly and nonhourly workers (referred to as salaried workers) based on their payment structure.

We obtain the 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.[3] These data are matched to credit files through tokenized social security numbers, 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 the impact of the arrival of an FC. We keep workers with a single employer any time during our sample period to estimate firm × year–quarter fixed effects. All dollar values are converted to December 2016 dollars using the seasonally adjusted consumer price index (CPI) for all urban consumers from the Bureau of Labor Statistics.

Our sample contains 2.6 million workers from 57 retail firms who provide such data to the credit bureau. These workers make up 18% of the 14.42 million total retail employees in the United States in the first quarter of 2010. The median firm has more than 14,000 workers in the sample, which suggests that our sample includes mostly large firms. Table 1, panel A, presents summary statistics for worker-level payroll data. The mean quarterly income of hourly workers is $7,314. Annualized income is $29,256, which is slightly higher than the mean income of 8.79 million retail sales workers ($25,250) and 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, and commissions (referred to as a bonus). In our sample, salaried workers earn $85,964 annually, on average (see Internet Section IA2).

The granularity of our worker-level data helps us answer questions that cannot be addressed using only 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 tenure, gender, and/or age insulate or exacerbate these effects? The detailed composition of the workers' compensation also allows us to understand the channels through which workers are affected. Do firms reduce workers' wages or bonuses? Do firms reduce wage rates or reduce the number of hours worked? Finally, 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.

4.2. Establishment Data

In addition to worker-level data, we make use of establishment-level data for the retail sector from the NETS database (Walls & Associates).[4] This database provides an annual record for a large part of the U.S. economy, including establishment job creation and

Table 1. Summary Statistics

	Full sample			FC counties		
	N	Mean	Standard deviation	*N*	Mean	Standard deviation
Panel A: Worker-level data						
Hourly workers						
Total income, $ per quarter	35,000,000	7,314	4,190	1,880,155	8,249	4,361
Wage income, $ per quarter	34,915,922	6,350	3,548	1,874,147	7,162	3,691
Bonus, $ per quarter	32,732,475	1,029	1,101	1,763,223	1,151	1,194
Hours worked per week	34,715,542	30.9	10.1	1,865,655	32.7	9.65
Wage rate, $ per hour	34,715,542	14.9	4.54	1,865,655	16	4.69
Salaried workers						
Total income, $ per quarter	5,459,955	21,491	18,250	293,902	22,708	18,329
Panel B: Establishment-level data						
Sales, $ 000 s 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						
Employment, average all sectors	1,837,920	1,692.6	8,724.1	26,000	12,072.1	23,555.3
Retail trade, NAICS 44 and 45	91,896	4,664.297	14,836.41	1,400	32,373.0	37,766.3
Transportation and warehousing, NAICS 48 and 49	91,896	1,212.8	5,471.0	1,400	12,155.6	14,117.1
Restaurants and accommodation, NAICS 72	91,896	3,695.5	13,805.2	1,400	28,589.6	43,897.2

Notes. This table presents summary statistics for the full sample and for counties with FCs. 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 (two-digit NAICS) employment data between 2010 and 2016 for all industries. All dollar values are converted to December 2016 dollars using CPI from BLS.

destruction, sales growth performance, survivability of business start-ups, mobility patterns, changes in primary markets, corporate affiliations that highlight mergers and acquisitions, and historic Dun & Bradstreet credit and payment ratings. At the beginning of our sample year, 2010, the database covers 3,287,183 active establishments that employ 27,404,989 workers with total sales of $2.9 trillion. These data are available up to 2014.

Similarly to how we define retail firms with our worker-level data set, we select establishments in six-digit NAICS industries that are more likely to be affected based on the e-commerce retailer's product catalog (see Internet Table IA3 for a complete list of the selected industries). To reduce noise from very small retail stores, we keep retail stores that have more than two employees before the establishment of the FC. The summary statistics reported in panel B of Table 1 indicate that the average retail store in our sample has annual sales of approximately $1.5 million and 12 employees.

4.3. County Employment Data

For our county-level analysis, we use QCEW data provided by the BLS. QCEW contains county-level data on employment and total wages in each two-digit NAICS industry for each quarter. We retain all industries and counties to understand the aggregate effect on retail, warehouses/transportation, and restaurants. We use quarterly data beginning in the first quarter of 2010 and ending in the fourth quarter of 2016. We report summary statistics in Table 1, panel C. Note that in the counties with FCs, about 32,373 workers have employment in the retail sector, whereas transportation and warehousing account for 12,155 workers per county.

5. Results

In this section, we first describe why FCs matter for the local area. Next, we 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 heterogeneous impact of FC entry on the employment levels and exit rates 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.

5.1. Why Do FCs Matter?

First, the optimization and expansion of the FC network are vital to the e-commerce retailer being able to meet customer demand. As discussed before, the e-commerce retailer built its first FC in 1997, and the number of FCs increased to more than 90 by the end of 2016 (Figures 1 and 2). Second, the establishment of a new FC is intended to avoid long-zone shipping and reduce the order-to-delivery time.[5] This is reflected in FCs being located primarily on the east and west coasts, where population density is highest (consistent with our findings in Section 3.1).[6] Third, customers may value the convenience of faster delivery, so the establishment of an FC would induce customers nearby to be more willing to shop through the major e-commerce retailer rather than shop at a local brick-and-mortar store.[7]

We conduct tests to understand if FCs impact sales of the e-commerce retailer. Unfortunately, we do not observe the geography-specific sales data for the e-commerce retailer. Instead, we use Google's search volume index data to test whether the establishment of an FC by the e-commerce retailer in a local area causes an increase in web search activity for Prime service within the shopping category. We expect that web searches for Prime service within the shopping category would be positively correlated with e-commerce retailer sales in the local area. We find that, within a year of establishment of FCs, the local Google search activity for the e-commerce retailer (its Prime service) increased by 7% (20%) compared with four years before the establishment of FCs in the metropolitan statistical area (MSA) (see Internet Section IA3).[8] The results suggest that the establishment of an FC encourages customers nearby to shop through the major e-commerce retailer.

Finally, we also test if the establishment of an FC reduces sales at local brick-and-mortar stores. We use Dun & Bradstreet's (NETS) store-level sales data and estimate the following Equation (2) at the store level:

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

Figure 3 plots the estimated coefficients and 95% confidence intervals. The variable $PreFC_{i,t}(-j)$ ($PostFC_{i,t}(j)$) is a dummy that takes a value of one if it is j years before (after) the establishment of FCs. The variable $PreFC(-4)$ ($PostFC(+4)$) equals one if it is four or more years before (after) the establishment of an FC. The variable $PreFC(-1)$ is dropped from the estimation so all coefficient estimates can be treated as store changes relative to the sales one year before the establishment of FCs. We also include store/establishment fixed effects (η_i). Coefficients on *PreFC(-4)*, *PreFC(-3), and PreFC(-2)* are all statistically insignificant from the sales in *PreFC(-1)* (the omitted category). This suggests that no pretrend exists in the sales data, thus supporting the validity of our parallel trends assumption. Further, we find that, within a year of the establishment of FCs, local brick-and-mortar stores observe a decline in sales by at least 4% (see Internet Table IA13 for specifications with more granular fixed effects).

Further, if the FC effect is not local, all retailers should see a drop in performance regardless of where they operate. On the other hand, if the opening of an FC matters more for the local area, retailers that operate in FC areas (more exposed to FCs) should see a decline in their performance. Our results suggest that retailers that are more exposed to FCs have worse stock performance relative to retailers that are less exposed to FCs, highlighting the local effect of FCs (see Internet Section IA5).

We also conduct two placebo tests on sales of retail stores. First, we show no effect on the sales of full-service restaurants, another nontradeable sector, after the establishment of FCs in the local area (see Internet Table IA14). Next, for each treated FC county, we assign its FC establishment date to the corresponding matched control county. We estimate the main specification using matched control counties for sales. The placebo FCs have no impact on sales of retail stores in these placebo FC counties (see Internet Table IA15). These placebo tests strengthen the interpretation that FCs are driving the impact on retail stores.

Overall, these results indicate that, with the establishment of an FC in the local area, customers appear to shift some of their purchases from local brick-and-mortar stores to the major e-commerce retailer.

5.2. How Do FCs Affect Local Brick-and-Mortar Store Workers?

In this section, we first discuss our baseline results using worker-level data, and then, we test how results vary based on the distance from FCs and the robustness of our results to local economic unobservables. Next, we test heterogeneity along worker dimensions, such as age, tenure, gender, and worker status (i.e., part versus full time). Further, we decompose workers' income into wage rate and hours worked. Finally, we report credit outcomes and additional robustness.

5.2.1. Baseline Results. In Table 2, we report the impact of the establishment of FCs on the income of retail workers in counties with FCs or in neighboring counties using the difference-in-differences specification shown in Equation (1). We include worker and year–quarter fixed effects in all regressions in order to absorb, as much as possible, the variation that arises from worker-specific, time-invariant characteristics and temporal trends.

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. Because the arrival of an FC may affect hourly and salaried workers

Table 2. Effect of FCs on Income of Retail Workers

	Log(total income)			
			Hourly workers	
	All workers	Salaried workers		CEM matched
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	−0.024***	−0.010	−0.025***	
	(0.005)	(0.008)	(0.005)	
PostFC × Treated				−0.024***
				(0.003)
Observations	2,174,057	293,902	1,880,155	2,237,053
Adjusted R^2	0.846	0.849	0.809	0.823
Panel B: Counties within 100 miles of FCs				
PostFC	−0.023***	−0.007	−0.024***	
	(0.004)	(0.006)	(0.004)	
PostFC × Treated				−0.016***
				(0.002)
Observations	11,134,980	1,590,558	9,544,422	8,623,011
Adjusted R^2	0.858	0.863	0.822	0.829
Worker fixed effects	✓	✓	✓	✓
Year–quarter fixed effects	✓	✓	✓	✓

Notes. This table presents results of worker-level panel regressions that assess the effect of FCs on income using Equation (1). Panel A includes retail workers in counties with FCs. Panel B includes retail workers in counties within 100 miles of FCs but not in counties with FCs. Columns (1) and (2) include all and salaried workers, respectively. Columns (3) and (4) include hourly workers. In column (4), we present results of matching estimates in which we utilize CEM to identify matched counties. We match on (1) population size, (2) population density, (3) per capita income, (4) unemployment rate, (5) the percentage of the population age below 18 and above 65, (6) percentage of the population with high school and college degrees, (7) retail spending per capita and (8) percentage of the population with broadband access. See Internet Section IA7 for details on matching. All regressions include worker and year–quarter fixed effects. Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

differently, we run separate regressions for those two types of workers. As shown in column (2), salaried workers mostly have muted responses to the establishment of FCs. Results in column (3) show that the income of hourly workers decreases by 2.5%, equivalent to an \$825 reduction in annual income. We focus on hourly workers throughout the rest of our analysis as they account for more than 90% of our sample, and they experience the largest negative effects. Next, we build upon this analysis of hourly workers by using CEM to identify appropriate control counties. We match on a number of county-level characteristics including total population, population density, per capita income, unemployment rate, age composition, education composition, retail spending per capita, and broadband access per capita (see Internet Section IA7 for details on matching). For hourly retail workers in FC counties, income decreases by 2.4% after the establishment of FCs relative to retail workers in control counties. The magnitude is comparable to our baseline estimate using only treated FC counties, −2.5%. Moving on to workers in counties within 100 miles of the focal county where an FC was established, we continue to observe a strong negative effect on total income (see panel B).

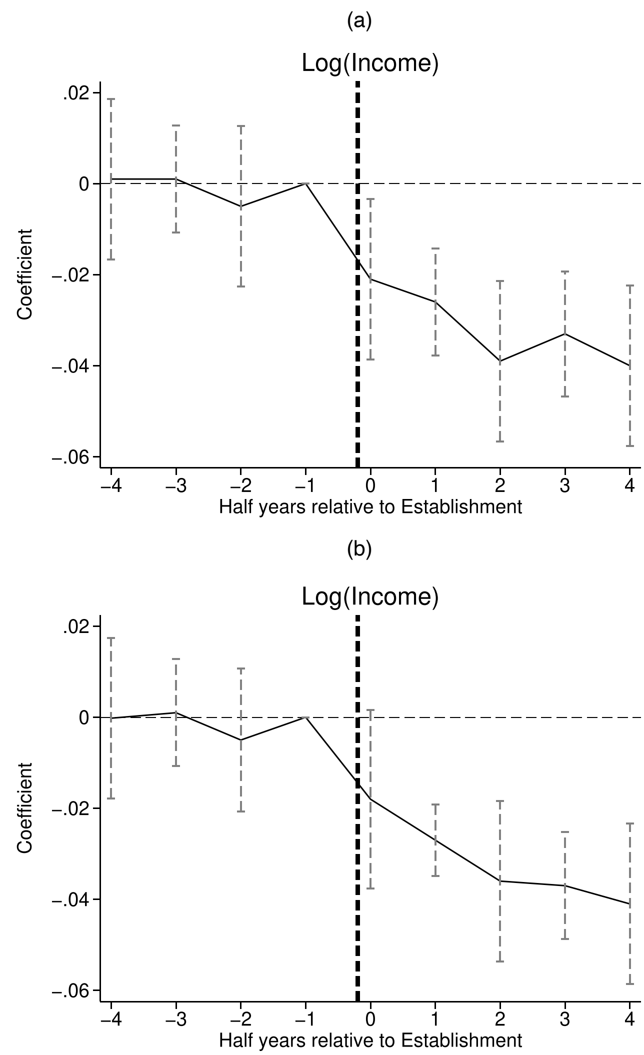
Our identification strategy relies on the staggered temporal roll out (shocks) of FCs across different counties. Our strategy also assumes that workers in counties that have yet to be treated by the establishment of an FC serve as an appropriate control group. This assumption would be violated if FCs 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 the treatment and control groups follow parallel trends before the shock. To test this, we examine the dynamic temporal effects by including leading and lagging indicators of FC establishment by estimating

$$Log(Total\ Income_{i,c,t}) = \alpha + \sum_{j=2}^{4} \beta_j PreFC_{c,t}(-j) + \sum_{j=0}^{4} \gamma_j PostFC_{c,t}(j) + \eta_i + \theta_t + \epsilon_{i,c,t}. \tag{3}$$

To increase the power of our estimates, the *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 of one if it is j half-years before (after) the establishment of FCs. The variable *PreFC(-4)* equals one if it is two or more years before the establishment of an FC, and *PostFC (+4)* equals one if it is two or more years after the establishment of an FC. The variable *PreFC(-1)* is dropped from the estimation so all coefficient estimates can be treated as percentage changes relative to the income that workers received in the six-month period before the establishment of the FC.

In Figure 4(a), we show the dynamic effect of FCs on income for counties with FCs by plotting the coefficients from the specification in Equation (3). The broken lines 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). This suggests that no pretrend exists in the data, thus supporting the validity of our parallel trends assumption. Within six months of the establishment of an FC, the income of hourly workers decreases by 2.3% relative to the income two years before the FC's establishment. This negative effect further increases to −4.2% two years after the FC's establishment. We find a similar pattern in Figure 4(b), in which we focus our analysis on counties within 100 miles of the county in which an FC opened.

Figure 4. Dynamic Effect of FCs on Income



Notes. These figures present the dynamic effect of FCs on the income of hourly retail workers. Panel (a) includes workers in counties with FCs. Panel (b) includes workers in counties within 100 miles of FCs but not in counties with FCs. We estimate Equation (3) and plot the estimated coefficients from *PreFC* ($j = -4$ to $j = -2$) and *PostFC* ($j = 0$ to $j = 4$) dummies, which are defined at a semiannual frequency. *PreFC*(-1) is dropped from the estimation so that all coefficient estimates can be treated as percentage changes relative to the income in the six-month period before the establishment of FCs. The broken lines around the coefficients represent 95% confidence intervals. (a) Counties with FCs. (b) Counties within 100 miles of FCs.

5.2.2. Can Firm-Specific or Local Economic Unobservables Drive the Results? Our results so far suggest a robust and negative relationship between the arrival of an FC and workers' income. However, absent truly exogenous variation in both the geographic location and the 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, similar to the major e-commerce retailer, brick-and-mortar retailers strategically respond to changes in consumer preferences by shifting operations online. 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 variation 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) of Table 3 that, when we include these firm–year–quarter fixed effects, the establishment of FCs in the county results in lower total income for hourly workers in the brick-and-mortar stores. We find that the magnitude diminishes to 2.1% from the baseline magnitude of 2.5% for counties with FCs.

As discussed in Section 3.1, local economic conditions may also play an important role in the establishment of FCs by the e-commerce retailer. Regions with and without FCs may have different economic environments that could correlate with the establishment of an FC. For example, regions with weaker 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 region level, we include region–year–quarter fixed effects and report the results in Table 3, column (2). The estimated effect for counties with FCs is about −1.7%. Our results remain robust when we combine firm–year–quarter and region–year–quarter fixed effects in column (3).[9]

Table 3. Firm-Specific Unobservables and Local Economic Conditions

	Log(total income)			
	Hourly workers			
	(1)	(2)	(3)	(4)
Panel A: Counties with FCs				
PostFC	−0.021***	−0.017***	−0.014***	
	(0.004)	(0.004)	(0.003)	
PostFC × *Retail*				−0.044***
				(0.006)
Observations	1,880,155	1,880,155	1,880,155	5,576,789
Adjusted R^2	0.841	0.810	0.842	0.850
Panel B: Counties within 100 miles of FCs				
PostFC	−0.021***	−0.015***	−0.014***	
	(0.003)	(0.003)	(0.003)	
PostFC × *Retail*				−0.025***
				(0.006)
Observations	9,544,422	9,544,422	9,544,422	26,759,697
Adjusted R^2	0.853	0.823	0.854	0.856
Worker fixed effects	✓	✓	✓	✓
Firm–year–quarter fixed effects	✓		✓	
Region–year–quarter fixed effects		✓	✓	
County–year–quarter fixed effects				✓

Notes. This table presents results of worker-level panel regressions that assess 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, region–year–quarter, and firm–year–quarter and region–year–quarter fixed effects in columns (1)–(3), respectively. In column (4), we include all hourly workers in other industries in addition to retail workers. We interact *PostFC* with *Retail*, and *Retail* identifies retail workers, and we control for county–year–quarter fixed effects. Panel A includes workers in counties with FCs. Panel B includes workers in counties within 100 miles of FCs but not in counties with FCs. Standard errors clustered by FC are reported in parentheses.
*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Whereas the region–year–quarter fixed effects may control for region-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 possibly even because of such a shock). To control for county-specific, time-varying shocks, we expand our sample threefold to include data on hourly workers at nonretail firms. In doing so, we can employ a triple difference (difference-in-difference-in-differences) methodology in which we exploit within county–year–quarter variation across retail versus nonretail industries. We can then control for county time specific shocks and identify our parameter of interest by comparing retail workers to nonretail 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 nonretail workers. In column (4), we interact the *PostFC* dummy with a *Retail* dummy set to one if the focal worker works in a retail industry and zero otherwise. We find that the income of retail hourly workers in counties with FCs is reduced by 4.4% compared with all other hourly workers within the same county after controlling for county-level, time-varying unobservables. So it seems unlikely that a local negative shock that solely affects a county's retail firms but not its nonretail firms is driving our results. Results remain similar when we extend our analysis to focus on counties within 100 miles of the county in which the FC was established.

We also conduct several other robustness tests for our regression estimates. First, we control for local demand for online shopping because of time-varying social economic factors by including proxies for local house prices, per capita income, unemployment rate, age composition, education composition, and time spent on work- and grocery-related travel. We find consistent results (see Internet Section IA6). We further construct a Bartik-style instrument (Bartik 1991) by interacting the distance between the FCs and the closest USPS network facility and the generosity of subsidies offered to corporations by local and state governments in a given year. Our IV estimates increase the magnitude of our effect sizes relative to those estimated by ordinary least squares (OLS), indicating that OLS regression may underestimate the negative effect of e-commerce on the income of retail workers because of positive selection of FCs on economic conditions (see Internet Section IA8). Finally, our results are robust to data selection, fixed effects, and construction of dependent variable (see Internet Section IA9).

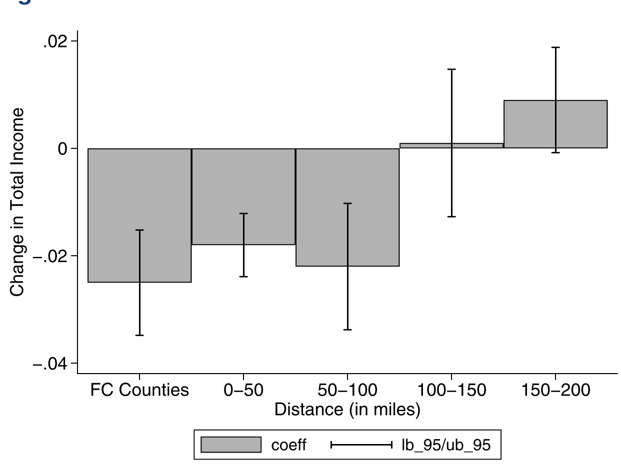
Overall, the results presented in this section ameliorate our concerns that our results are being driven by firm-specific unobservable variables or some other omitted local economic conditions that coincide with the staggered establishment of FCs.

5.2.3. How Does the Impact of FCs Vary with Distance from the Focal County? As discussed in Section 5.1, the e-commerce retailer likely optimizes its FC network to reduce shipping 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 could benefit from the FC's establishment, and they could 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 the FC increases. However, as shown in the FC network map (Figure 2), some FC clusters exist in the United States, particularly in the Midwest and Northeast. As such, many counties may be affected by multiple FCs, limiting our ability to evaluate the relationship between large distances and FC establishment. U.S. counties without FCs are placed into one of four categories based on their distance from the closest FC (i.e., 0–50, 50–100, 100–150, and 150–200 miles). To ensure that the county is affected by its closest FC, we omit counties served by any other FC within 500 miles before the establishment of the closest FC.

Our results in Figure 5 show that the income effect is −1.8% for counties within 50 miles of FCs and −2.2% for counties within 50–100 miles of FCs. For counties within 100–150 and 150–200 miles of FCs, the effect is almost zero and statistically insignificant. These results highlight the geography of workers/jobs affected by e-commerce.

Figure 5. Distance to FC and Effect of FCs on Income



Notes. This figure presents the heterogeneous effect of FCs on the income of hourly retail workers based on the distance between a given county and its closest FC. U.S. counties without FCs are classified into four groups based on the distance to the closest FC (i.e., 0–50, 50–100, 100–150, and 150–200 miles). We drop counties that are served by any other FC within 500 miles before the establishment of the closest FC. We estimate Equation (1) using subsamples. Standard errors are clustered by FC. We plot coefficients and 95% confidence intervals.

5.2.4. Are All Retail Workers Affected Equally by the Establishment of FCs? Our rich worker-level data allow us to analyze which workers are most impacted by the negative wage shock because of the establishment of the FCs. This rich demographic information, obtained in Q12010, allows us to consider heterogeneity along worker dimensions such as age, tenure, gender, and worker status (i.e., part versus full time). All regressions include worker, firm–year–quarter, and region–year–quarter fixed effects. We continue to use this tighter specification for all our worker-level regression estimates.[10]

5.2.4.1. Age. We start by exploring worker heterogeneity by age. We divide the retail workers in our sample into six age groups. Figure 6(a) reports results from running Equation (1) over these six age groups. We find evidence for a stronger negative impact on the total income of young and old workers. We observe that, for workers under the age of 25, total income decreases by 2.1%. This negative effect is lower for the 25–34 age group (−1%). We find that this negative effect increases with age. For the 35–44, 45–54, and 55–64 age groups, the effect is −1.2%, −1.3%, and −1.6%, respectively. For the oldest group (i.e., workers older than 64 years of age), the negative income effect is −3.6%. These results suggest that a worker's age could have a moderating impact on the impact of new technologies. One explanation for this result may be that a worker's age proxies for productivity and accumulated firm-specific human capital.

5.2.4.2. Tenure. Next, we directly test the differential effect based on accumulated firm-specific human capital. We divide the retail workers in our sample into five quintiles based on tenure at the beginning of the sample period. We observe that, for the workers with the shortest tenure, total income decreases by 3.3%. This negative effect diminishes over the worker's tenure. For the longest tenured group, the negative income effect is −1.1%.

5.2.4.3. Work Status. Further, we test how the negative income effect varies across workers' working status (i.e., part versus full time). Similar to age, a worker's working status may reflect the underlying level of firm-specific human capital accumulation. Firms may invest more in the human capital development of full- than part-time workers. We define a worker as a part-time worker if the worker works 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 work status at the beginning of our sample period. In line with the work hour reduction results previously reported, we find that the negative effect is stronger for part-time workers, that is, the impact on part-time workers is about −2.5%, −1.5 percentage points more negative than that of full-time workers.

5.2.4.4. Gender. It is possible that the negative impact of FCs varies by gender given the significant fraction of female retail employees. We test for any differential effect of FCs on male versus female workers. We do not find a significant difference in the effect based on worker gender. All the heterogeneity results appear similar for counties within 100 miles of FCs. These results are reported in Figure 6(b).

5.2.4.5. Retail Subsectors. We also test heterogeneity based on the subsectors of the retail industry. We find that retail workers working in general merchandise stores (−2.2%) and those working in the home improvement sector (−3.5%) observe a greater decline in income (see Internet Figure IA7). The results are consistent with the hypothesis that largest e-commerce retailer has a greater advantage in sectors in which in-store experience is less relevant (Chen and Qian 2020). Further, we find that employment of nonfranchise stores decreases by 2.8%, whereas the employment of franchise stores decreases by 1.3% (see Internet Section IA12, Table IA18).

5.2.4.6. Skill Composition Changes. The affected retail firms may employ more technical support and cut down working hours of the salespersons. However, we do not have details on the occupation of the retail workers. Therefore, we utilize occupational employment and wage statistics to understand the skill composition changes within the retail industry. In 2010, retail salespersons accounted for around one fourth of the employees in the retail sector, and cashiers accounted for 18% of the employees. Using annual MSA–occupation level employment data, we find a decline in employment of retail salespersons and general operations managers. However, we find an increase in demand for the number of jobs related to e-commerce: stock clerks and order fillers; packers and hand packagers; bookkeeping, accounting, and auditing clerks; light truck drivers; delivery service truck drivers; and shipping, receiving, and traffic clerks (see Internet Section IA11 for details).

5.2.5. Decomposing the Impact of FC Establishment on Wages. Our detailed payroll data on workers allows us to decompose total income into wage 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 4, column (1), reports results using wage income as the dependent variable. We find a significant negative impact on wage income across all three panels. The economic magnitude is −1.1% (~$315). In column (2), we find that bonuses decline by 1.1% (~$50). To further investigate the source of this wage reduction, we can decompose wage income into hours worked and wage rate. In column (3), we report results for hours worked, and we 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)). These results appear similar for counties within 100 miles of FCs. The results documented in Table 4 suggest that the negative impact of FC establishment on local retail workers is driven mainly by the reduction in hours worked.

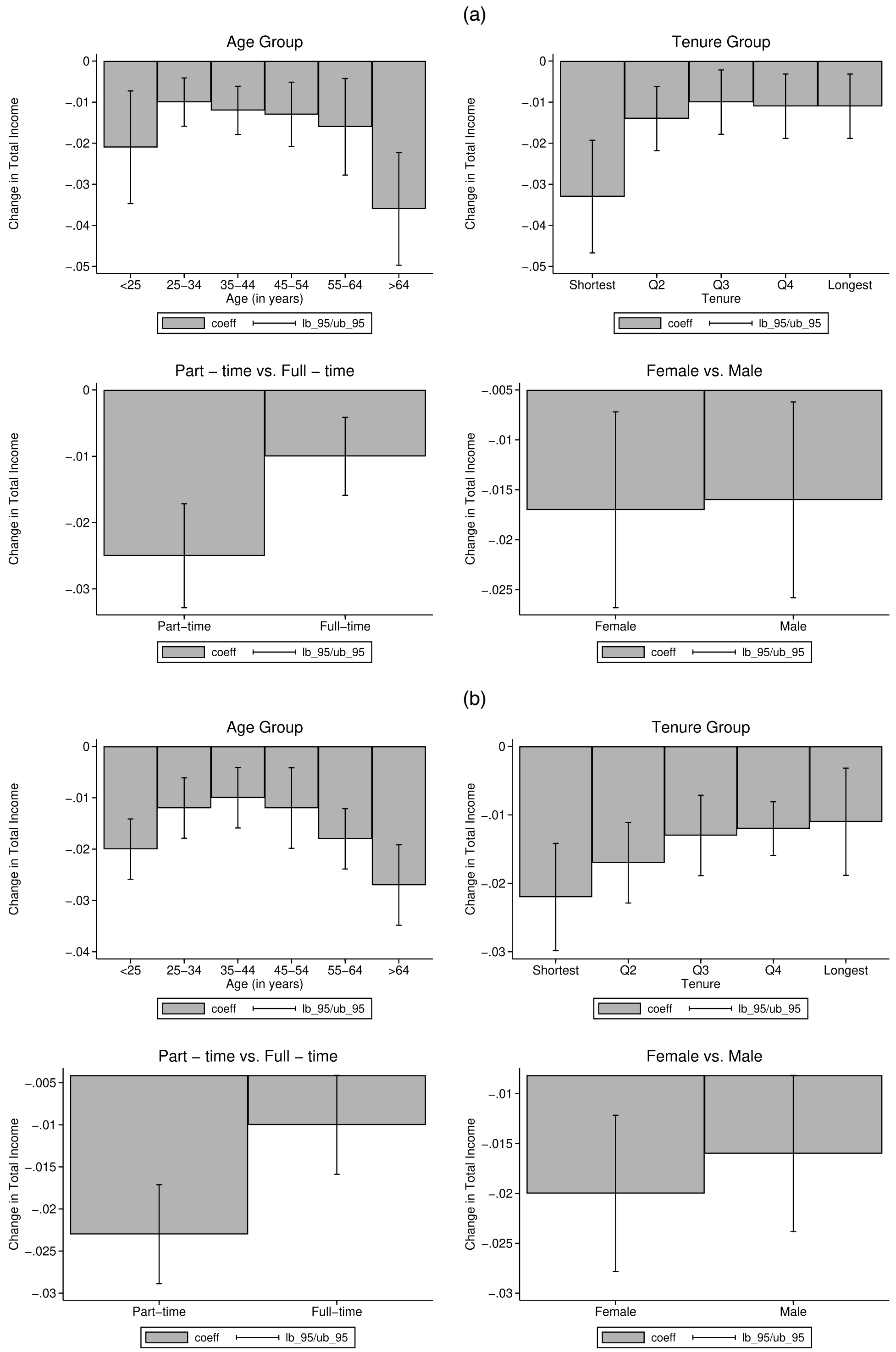
5.2.6. Impact on Credit Scores of Affected Brick-and-Mortar Retail Workers. To the extent that labor markets are frictionless (i.e., workers can easily change jobs and skills are completely transferable) the short-term displacement of some traditional retail store workers that we document may not matter for the workers or the local economy. However, in the presence of labor market frictions, the short-term impact on the workers and the 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 cannot directly verify whether the affected workers offset their reduced hours at 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 workers who are already living at the margin (i.e., workers with high credit 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 credit card utilization is higher than the median utilization ratio, and we assign a worker to the low utilization group if credit card utilization is lower than the median.

We report our results in Table 5. We find that the credit scores of workers with high utilization of credit cards declines significantly. In counties with FCs, the decreases in credit scores of workers in the high utilization group are three points more than that of workers in the low utilization group. It seems that the decline in credit scores is driven by increases in credit card delinquencies among the affected workers. These results suggest that some of the affected retail workers experience some frictions in the labor market that preclude them

Figure 6. Heterogeneous Effect of FCs on Income



Notes. These figures present the heterogeneous effect of FCs on the income of hourly retail workers based on workers' characteristics. Panel (a) includes workers in counties with FCs. Panel (b) includes workers in counties within 100 miles of FCs but not in counties with FCs. We estimate Equation (1) using subsamples. The subsamples are defined by workers' age, tenure, work status (part versus full time), or gender. All regressions include worker, firm–year–quarter and region–year–quarter fixed effects. Standard errors are clustered by FC. We plot coefficients and 95% confidence intervals. (a) Counties with FCs. (b) Counties within 100 miles of FCs.

Table 4. Decomposition of Income Effect for Hourly Workers

	Log(wage income) (1)	Log(bonus) (2)	Log(hours worked) (3)	Log(wage rate) (4)
	Panel A: Counties with FCs			
PostFC	−0.011***	−0.011*	−0.012***	0.000
	(0.003)	(0.007)	(0.002)	(0.001)
Observations	1,874,147	1,763,223	1,865,655	1,865,655
Adjusted R^2	0.842	0.739	0.731	0.970
	Panel B: Counties within 100 miles of FCs			
PostFC	−0.011***	−0.008	−0.012***	−0.001
	(0.003)	(0.009)	(0.002)	(0.001)
Observations	9,516,525	8,916,039	9,471,353	9,471,353
Adjusted R^2	0.848	0.745	0.744	0.969
Worker fixed effects	✓	✓	✓	✓
Firm–year–quarter fixed effects	✓	✓	✓	✓
Region–year–quarter fixed effects	✓	✓	✓	✓

Notes. This table presents results of worker-level panel regressions that assess 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 includes workers in counties within 100 miles of FCs but not in counties with FCs. All regressions include worker, firm–year–quarter, and region–year–quarter fixed effects. Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

from mitigating the extent to which the establishment of e-commerce FCs in their county depresses their wage income and, subsequently, their credit scores.

5.3. How Do FCs Affect Local Brick-and-Mortar Retail Stores?

So far, we have used detailed worker-level data and the staggered establishment of FCs by the e-commerce retailer to understand the impact on the wages of workers at traditional brick-and-mortar retail stores in the focal county and in geographically proximate counties. We attempt to further understand whether the affected stores also adjust overall employment levels or even exit in addition to reducing the number of hours of part-time and hourly workers? Do stores respond differently based on their size and age?

5.3.1. Effect on Retail Store Employment. Table 6 reports results for the effect of FCs on local establishment-level employment using NETS data. Column (1) reports difference-in-differences estimates for all stores in counties that have FCs. In all the specifications, we include establishment fixed effects, six-digit NAICS–year fixed effects, and region–year fixed effects. We find that, for all stores, employment decreases by 2.1%, which is equivalent to a reduction in force of 36 workers per 100 stores based on an average of 17 employees per store. For small stores, employment decreases by 2.3%, which is equivalent to reducing eight workers per 100 stores for a store with an average of four employees. For large stores, employment decreases by 128 workers per 100 stores based on an average of 40 employees per store. The effect is diminished in counties that are within 100 miles of an FC.

Based on the results presented, it appears that, after the establishment of the e-commerce retailer's FCs, traditional brick-and-mortar retail stores in the focal county react to competition by both reducing the number of hours of work assigned to hourly workers and also reducing employment levels. Using similar specifications, we also find that, after the staggered establishment of the e-commerce firm's FCs, sales decline significantly in the focal county of the FC (see Internet Table IA13).

Similar to our worker-level results, the negative effects on sales and employment of retail stores are confined to proximate counties, that is, distance less than 100 miles (see Internet Figure IA8). We find similar results when we control for various county-level characteristics (see Internet Table IA16). We also reestimate column (1) of Table 6, panel A, in which we include matched retail stores in control counties (using CEM) to our analysis. We find that the employment of retail stores in FC counties decreases by 1.5%–1.8% after the establishment of FCs relative to retail stores in control counties, whereas the impact of FCs on sales is −2.4% to −2.7% (See Internet Table IA7, panels B and C). Finally, we reestimate our employment regression using a weighted least squares model and an inverse probability tilting as treatment model as suggested by Callaway and Sant'Anna (2021). We find that our two-way fixed effect estimates are conservative estimates of the treatment effect and, thus, constitute lower bounds. For example, we find a decline in total employment at retail stores by 4.5% using the Callaway and Sant'Anna (2021) methodology compared with our baseline two-way fixed effects estimates a decline of 2.4%. See Internet Section IA10 for details.

5.3.2. Closures of Retail Stores. Next, we analyze whether the increase in competition after the establishment of the e-commerce retailer's FCs can lead, in extreme cases, to an increase in retail store closures.

Table 5. Heterogeneous Effect on Credit Scores: Credit Card Utilization

	Credit scores (1)	Credit card 90+ day delinquency (2)
Panel A: Counties with FCs		
PostFC × *Low* (1)	1.270**	−0.001*
	(0.522)	(0.001)
PostFC × *High* (2)	−1.520***	0.006***
	(0.455)	(0.001)
Difference ((2) − (1))	−2.999***	0.007***
	(0.535)	(0.001)
Observations	1,209,810	1,080,344
Adjusted R^2	0.812	0.108
Panel B: Counties within 100 miles of FCs		
PostFC × *Low* (3)	0.495*	0.001*
	(0.283)	(0.000)
PostFC × *High* (4)	0.068	0.000
	(0.491)	(0.001)
Difference ((4) − (3))	−0.427	−0.000
	(0.445)	(0.001)
Observations	6,436,987	5,774,659
Adjusted R^2	0.822	0.112
Worker fixed effects	✓	✓
Low–firm–year–quarter fixed effects	✓	✓
Low–region–year–quarter fixed effects	✓	✓

Notes. This table presents results of worker-level panel regressions that assess the heterogeneous effect of FCs on the credit scores and the 90+ day credit card delinquencies of hourly workers based on credit card utilization using Equation (1). All regressions include worker, group-specific firm–year–quarter, and group-specific region–year–quarter fixed effects. Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

In Table 7, we attempt to understand whether the establishment of FCs leads to store closures and how this effect varies with store size and age. Our dependent variable *exit* is a time series dummy variable. For a given store, the dummy is set to zero when the store is operating normally and is set to one for the last year of its operation. Table 7, column (1) reports results for all stores. We find that the exit rate increases by 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;

Table 6. Effect of FCs on Employment by Retail Stores

	Log(1+employment)			
	All (1)	Small (2)	Medium (3)	Large (4)
Panel A: Counties with FCs				
PostFC	−0.021***	−0.023***	−0.016***	−0.024***
	(0.005)	(0.005)	(0.004)	(0.007)
Observations	142,820	51,726	44,950	46,144
Adjusted R^2	0.97	0.84	0.88	0.96
Panel B: Counties within 100 miles of FCs				
PostFC	−0.009**	−0.013***	−0.008**	−0.006
	(0.004)	(0.004)	(0.003)	(0.005)
Observations	852,547	304,558	271,277	276,706
Adjusted R^2	0.98	0.84	0.89	0.97
Establishment fixed effects	✓	✓	✓	✓
Industry–year fixed effects	✓	✓	✓	✓
Region–year fixed effects	✓	✓	✓	✓

Notes. This table presents results of establishment-level panel regressions that assess the heterogeneous effect of FCs on the employment by retail establishments/stores based on the size of stores. Panel A includes establishments in counties with FCs. Panel B includes establishments in counties within 100 miles of FCs but not in counties with FCs. Column (1) reports results for all stores, whereas columns (2)–(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 region–year fixed effects. Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Effect of FCs on Retail Store Closures

	Exit							
	Size groups				Age groups			
	All (1)	Small (2)	Medium (3)	Large (4)	All (5)	Young (6)	Medium (7)	Old (8)
Panel A: Counties with FCs								
PostFC	0.028***	0.034***	0.030***	0.022***	0.031***	0.038***	0.034***	0.017***
	(0.006)	(0.008)	(0.007)	(0.005)	(0.008)	(0.009)	(0.008)	(0.006)
Observations	121,102	43,480	39,006	38,616	76,452	31,637	21,728	23,081
Adjusted R^2	0.19	0.20	0.19	0.19	0.19	0.20	0.19	0.18
Panel B: Counties within 100 miles of FCs								
PostFC	0.019***	0.025***	0.017***	0.013***	0.020***	0.028***	0.016***	0.012**
	(0.006)	(0.008)	(0.006)	(0.005)	(0.007)	(0.009)	(0.005)	(0.005)
Observations	753,963	270,920	242,707	240,330	510,339	223,560	146,980	139,793
Adjusted R^2	0.20	0.20	0.19	0.19	0.20	0.20	0.19	0.19
Establishment fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Industry–year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Region–year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes. This table presents results of establishment-level panel regressions that assess the heterogeneous effect of FCs on the exit rates of retail establishments/stores based on the size and age of stores. Here, we define the exit dummy, our dependent variable, as one if the establishment ceases to exist one year before the end of the sample period and zero otherwise. Panel A includes establishments in counties with FCs. Panel B includes establishments in counties within 100 miles of FCs but not in counties with FCs. Column (1) reports results for all stores, whereas columns (2)–(4) report results for terciles based on sales one year before the establishment of FCs in the county or neighboring county. Column (5) reports results for all stores for which we observe the store's age, whereas columns (6)–(8) report results for terciles based on a store's age one year before the establishment of FCs in the county or neighboring county. All regressions include establishment, industry–year, and region–year fixed effects. Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

that is, small stores are more likely to exit than large stores. We test the role of a store's age on exits by partitioning the stores into young, medium, and old based on terciles of ex ante age. Results in column (4) of Table 7 show that young stores are more likely to close. Overall, this exit rate impact is more pronounced for young and small retail stores.

It is possible that some retail stores may respond to FC entry by relocating outside of their current county or state. We do not find evidence for this possibility (see Internet Section IA12, Table IA17). Finally, we analyze the impact of the establishment of the FC on entry into the local retail market. We find that, after the establishment of an FC in the affected county, the entry rate for small stores is significantly reduced by 8.1% in counties within 100 miles from an FC (see Internet Section IA12, Table IA19).

In summary, using detailed establishment-level data from NETS, we find that, after the staggered establishment of the FCs of the e-commerce retailer, geographically proximate traditional brick-and-mortar retail stores experience a decline in employment, an increase in closures among incumbent firms, and a decline in the entry rate. The impact on store closures is more pronounced for young stores and small stores.

5.4. Aggregate Effect on Employment and Wages

Finally, to understand the aggregate effect at the county level, we use county-level QCEW data on employment and total wages for each NAICS two-digit sector. We report the estimates of county-industry specific quarterly employment and wages in Table 8. In panel A, column (1), we compare the effect of FCs on the employment in retail (NAICS 44 and 45), transportation and warehousing (NAICS 48 and 49), and restaurants (NAICS 72). Here, we include all other industries and all U.S. counties to absorb the time-varying, county-specific, and industry-specific unobservables. The interaction term *Post FC* × *Retail* identifies the effect of the establishment of FCs on employment in the retail sector compared with all other sectors within the same county. In these regressions, we include county–year–quarter, industry–year–quarter, and industry–county fixed effects. As the *PostFC* dummy is defined at the county–year–quarter level, *PostFC* is completely absorbed by the fixed effects. In column (2), we remove county industries that have any masking issues during the sample period to ensure that all county industries have a full time series.[11] However, certain counties could still be missing data for some industries. In order to obtain a fully balanced panel, we only include counties that have no masking issues in column (3). Finally, we remove the three heavily masked industries in column (4).

Consistent with earlier evidence using payroll and establishment-level data, we find that the establishment of an FC has a negative effect on employment in the retail sector from columns (1)–(4). On the other hand, the establishment of FCs creates jobs in the transportation and warehousing sector. Columns (5)–(8) report the

Table 8. Aggregate Wages and Employment: County-Industry Evidence

	Log(employment)				Log(total wages)			
Dependent Variable(s)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Counties with FCs								
PostFC × Retail	−0.006	−0.007	−0.010	−0.026*	−0.019	−0.016	−0.035	−0.045**
	(0.013)	(0.014)	(0.016)	(0.014)	(0.018)	(0.018)	(0.021)	(0.019)
PostFC × Warehouse	0.178***	0.173***	0.150***	0.135***	0.117***	0.117***	0.105**	0.095**
	(0.043)	(0.045)	(0.046)	(0.045)	(0.038)	(0.040)	(0.047)	(0.043)
PostFC × Restaurant	−0.007	−0.010	−0.026	−0.042**	−0.001	−0.002	−0.012	−0.022
	(0.014)	(0.014)	(0.022)	(0.019)	(0.017)	(0.017)	(0.026)	(0.022)
Adjusted R^2	0.990	0.991	0.992	0.991	0.987	0.989	0.989	0.989
Observations	1,177,414	935,240	100,973	85,573	1,178,116	935,238	100,973	85,573
Panel B: Counties within 100 miles of FCs								
PostFC × Retail	−0.011**	−0.011*	−0.020	−0.016	−0.020***	−0.019***	−0.035**	−0.029**
	(0.005)	(0.006)	(0.013)	(0.012)	(0.005)	(0.006)	(0.015)	(0.014)
PostFC × Warehouse	−0.009	−0.006	−0.035*	−0.030*	−0.006	−0.002	−0.016	−0.010
	(0.018)	(0.016)	(0.019)	(0.017)	(0.018)	(0.015)	(0.020)	(0.018)
PostFC × Restaurant	0.012*	0.006	−0.019	−0.015	0.005	0.000	−0.021	−0.014
	(0.007)	(0.006)	(0.015)	(0.013)	(0.007)	(0.007)	(0.018)	(0.018)
Adjusted R^2	0.990	0.991	0.992	0.991	0.987	0.989	0.989	0.989
Observations	1,177,414	935,240	100,973	85,573	1,178,116	935,238	100,973	85,573
County–year–quarter fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Industry–year–quarter fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Industry–county fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Notes. This table presents the results of county–industry level panel regressions that assess the aggregate effect of FCs on employment and total wages. Retail (NAICS 44 and 45), warehouse (NAICS 48 and 49), and restaurant (NAICS 72) dummies identify the respective industries. All regressions include county–year–quarter, industry–year–quarter, and industry–county fixed effects. In columns (1) and (4), we do not condition our sample. In columns (2) and (5), we restrict our data to only those county–industry pairs for which we do not observe masking over the sample period. In columns (3) and (6), we restrict our sample to counties where there is no masking for any industry. In columns (4) and (8), we also drop three heavily masked industries, that is, NAICS2: 11 (agriculture, forestry, fishing, and hunting), 21 (mining), and 22 (utilities). Standard errors clustered by FC are reported in parentheses.

*, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

results for total wages. We find consistent results. Panel B reports the results for counties that are within 100 miles of FCs but do not contain FCs. Note that the positive effect on the transportation and warehousing sector disappears, whereas the negative effect on the retail sector in distant counties remains statistically significant.

Overall, the results using county-level QCEW data are largely supportive of our findings using administrative employment data and NETS data. We also show that our results are less likely to be driven by alternative explanations such as the e-commerce retailer choosing FC locations based on declining aggregate wages or declining retail growth (see Internet Section IA13). Further, in terms of aggregate effect, we find weak evidence for a decline in the unemployment rate or an increase in aggregate consumption in FC counties (see Internet Section IA14). Finally, we use an alternative identification strategy to assess the impact of e-commerce on retail sector workers. We hand-collect the announcement dates for the entry into different industries from the major e-commerce retailer's news announcements. We use this staggered entry into different sectors as an alternative identification strategy and find consistent results, that is, a decline in employment for treated retail stores (see Internet Section IA15).

6. Discussion and Conclusion

The recent disruption in the retail sector is attributed to the rise of e-commerce. As of 2019, e-commerce sales accounted for 11.4% of total retail sales in the United States compared with 0.63% in 1999. The roll-out of a major e-commerce retailer's FCs in a local area increases the demand for the e-commerce retailer and reduces sales of geographically proximate traditional brick-and-mortar retail stores. As a result, retail firms cut down their operating costs by adjusting their demand for retail workers through both intensive margin (number of hours) and extensive margin (employment). This negative impact disappears for workers and stores that are geographically distant from FCs. The decline in wage income is confined to hourly workers, especially part-time workers, and workers with short job tenure, whereas the impact on salaried workers is limited. Financially vulnerable hourly workers experience an increase in credit card delinquency. Stores that are more financially constrained (measured using store size and age) are more likely to adjust their employment or exit.

In the absence of labor market frictions (i.e., workers can easily switch jobs and their skills are completely transferable), the short-term displacement of some traditional retail store workers may not matter for the workers or the local economy. However, in the presence of labor market frictions, such as the extent to which the scope of work differs between traditional retail stores and FCs (for example, there is no need for cashiers at an FC), at least some workers can be worse off in the short term. Our results highlight the role of geography, human capital, and financial constraints in understanding the impact of e-commerce–driven creative destruction.

We note that our results are limited to documenting only one facet of the impact of this technological innovation on local retailers and their employees. E-commerce may offer consumers many benefits, including potentially lower prices, more choices, more convenience in shopping, gains from competition, and lower effort (e.g., no driving). Moreover, the establishment of an FC can potentially generate positive local spillovers and create additional jobs in areas such as warehousing/logistics and information technology. Given the limited scope of this paper, we do not aim to quantify the long-term effect of e-commerce, nor the aggregate impact that e-commerce has on consumer welfare.

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Endnotes

[1] A complete list of FCs of the e-commerce retailer is available at http://www.mwpvl.com/.

[2] Our identification strategy is similar to Houde et al. (2023). Internet Table IA20 reports results when we use the full sample, including all the 50 FC shocks for establishment- and county-level outcomes. The results remain robust.

[3] We identify firms in three-digit NAICS industries that are most likely to compete with the major e-commerce retailer's product catalog. The three-digit NAICS codes that we classify as retail include 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 nonretail firms as a control group.

[4] Employment in the NETS database includes all wage and salary workers, both full and part time, and excludes proprietors, partners, independent contractors, and temporary help service workers employed by outside establishments (the latter of which are included in the establishment that issues their paycheck rather than the establishment where they work). Barnatchez et al. (2017) find imputation of employment data for small establishments in the NETS database. They do not find any location-specific systematic differences in these imputations, which could be a potential concern in our case.

[5] After the opening announcement of an FC in San Bernardino in 2016, "They're also rapidly trying to establish a more localized presence around all the major metropolitan areas so they can provide faster, and in some cases, cheaper delivery," said an analyst at Robert W. Baird & Co. "Ultimately … [the company] wants to deliver anything to anybody, all within a couple of hours" (see https://tinyurl.com/stwpuhl). After the opening of an FC in Lexington and the opening announcement of an FC in Spartanburg in 2012, "We had a great first holiday season in Lexington County, and we look forward to serving our customers from both Lexington and Spartanburg Counties by the fall," said the vice president of global customer fulfillment of the large e-commerce site (see https://tinyurl.com/wnjktsv).

[6] Page 8 of 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 are close to large cities, allowing for the possibility of next- or same-day delivery and the wider rollout of its grocery business (Stone 2013, p. 8).

[7] For example, the major e-commerce retailer spent \$1.5 billion to speed up its same-day delivery in a handful of states after bolstering its fulfillment centers (see https://www.cnbc.com/2020/03/03/amazon-expands-same-day-delivery-after-building-fulfillment-centers.html). In addition, same-day delivery can significantly impact the local retail store (see https://slate.com/business/2012/07/amazon-same-day-delivery-how-the-e-commerce-giant-will-destroy-local-retail.html). In 2012, the major e-commerce filed a patent for "anticipatory shipping" to ship the package to customers before they order it (see https://www.wsj.com/articles/BL-DGB-32082).

[8] Further, we find that the establishment of FCs has a positive impact on the employment and wages in the focal county for couriers and messengers (see Internet Section IA4).

[9] Our results remain robust when we replace region–year–quarter fixed effects with state–year–quarter fixed effects. These are reported in Internet Table IA10.

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

[11] The BLS sets the employment to zero for certain county industries to protect the identity or identifiable information of the employer. Whereas the masked observations get automatically removed once the logged transformation is applied, this leads to an unbalanced panel as county industries with masking issues do not have the complete time series.

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