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WALMART SUPERCENTERS AND MONOPSONY POWER: HOW A LARGE, LOW-WAGE EMPLOYER IMPACTS LOCAL LABOR MARKETS

Justin C. Wiltshire
Department of Economics, University of Victoria
Victoria, B.C., Canada V8W 2Y2

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Keywords: Walmart, monopsony, wage income, job loss, local labor markets

JEL Classifications: J42, R23, J2, J31

Author Contact:
Justin C. Wiltshire, Dept. of Economics, University of Victoria, P.O. Box 1700, STN CSC, Victoria, B.C., Canada V8W 2Y2; E-mail: wiltshire@uvic.ca; Tel: (250) 858-2100

Walmart Supercenters and Monopsony Power: How a Large, Low-Wage Employer Impacts Local Labor Markets

Justin C. Wiltshire[†]

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Abstract

This paper considers the extent and impact of monopsony power exercised by Walmart Supercenters. I address the issue of potential bias from endogenous store entry, as well as other identification concerns, by adopting a stacked synthetic control approach to estimate average county-level labor market effects of the Walmart Supercenter roll-out across the U.S. Crucially, I construct the pools of synthetic control donor counties from novel observations of counties where Walmart tried to open a Supercenter but was blocked by local efforts. I first show Supercenter entry sharply increased labor market concentration. Supercenters were able to hire large numbers of retail workers with zero increase in average earnings, indicating Walmart had wage-setting power. I then show Supercenter entry caused large declines in overall local employment and earnings, particularly among local goods-producers, indicating Walmart displaced manufacturing demand away from local producers and to its own national and international suppliers. In counties with a Supercenter, subsequent exogenous minimum wage increases led to significant growth in aggregate and retail employment. These results run counter to predictions for competitive labor markets, and indicate Walmart Supercenters gradually accumulated and exercised monopsony power, with negative consequences for workers.

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[†]University of Victoria Department of Economics, 3800 Finnerty Road, Victoria, British Columbia, V8P 5C2, Canada. Email: wiltshire@uvic.ca. I received financial support for this project from the Washington Center for Equitable Growth. I am grateful to Giovanni Peri, Marianne Bitler, Marianne Page, and Brendan Price for invaluable feedback. The paper also benefited from my conversations with Sarah Abraham, Kate Bahn, Donald Davis, Ellora Derenoncourt, Guido Imbens, Alan Manning, Kevin Rinz, Michael Reich, and Anna Stansbury, and participants in the UC Davis applied micro brown bag series and econometrics reading group, at the University of Victoria, and at the Visiting Scholar seminar at the Institute for Research on Labor and Employment, as well as conference panelists and participants. All errors remain my own.

1 Introduction

Walmart is the largest private-sector employer in the United States and, indeed, the world, and has long been the archetype of a low-wage employer with harmful labor market power. Yet prior research has disagreed about how to satisfactorily identify the firm's impact on local labor markets, leading to conflicting estimates of its effects (Basker, 2005a, 2007b; Neumark et al., 2008). Given that large retail chains—and Walmart, in particular—have come to employ a substantial proportion of the lower-paid U.S. workforce in recent decades, and given that U.S. labor-income inequality grew sharply over the same period through 2019 (Hoffmann et al., 2020; Autor et al., 2023), it is important to gain clarity about Walmart's impact on local labor markets.

This paper is the first to demonstrate the extent and impact of monopsony power exercised by Walmart Supercenters. I do this by exploiting the rollout of Walmart Supercenters across the U.S., showing that Supercenter entry into counties affected local labor markets in ways that cannot be reconciled with 'competitive' models. I find that entry sharply increased labor market concentration, and Supercenters were able to hire large numbers of retail workers with zero increase in average earnings, indicating Walmart had wage-setting power for its own workers. I then show that Supercenter entry more broadly caused large and significant declines in overall local employment and earnings. I show that these negative effects were particularly large among local goods-producers, indicating a Supercenter's arrival redirected manufacturing demand away from (often less-productive) local producers and to Walmart's own national and international suppliers. Finally, I show that an exogenous minimum wage increase yielded large employment gains in counties where Supercenters were present—a result again inconsistent with competitive labor markets. These results together admit Supercenter presence had negative spillover effects on local labor markets that extended well beyond their own walls. They also suggest that minimum wage increases could actually yield employment gains if targeted at labor markets characterized by low-wage monopsony power, in line with the conclusions of Azar et al. (2023).

Throughout, I broadly retain the term 'monopsony' to refer to all potential sources of labor market power, such as monopsonistic competition, bargaining power, and labor market frictions—especially "imperfect" monopsony. For example, if highly-productive firms crowd out labor demand from local competitors, they may depress the arrival rate of differentiated job offers to workers. Alternatively, if a large firm is the primary customer for local suppliers whom it squeezes to cut costs, it may indirectly dictate local wages and employment. And if that large firm is also a retailer that disproportionately sources its goods non-locally, relative to local competitors, then its entry into a market could depress local demand for manufacturing labor and establish its own lower wages as the outside option for those workers. Such 'monopsonistic' employers can substantially impact local labor markets beyond their own walls, with negative impacts on local workers.

My estimating strategy exploits the rollout of over 1,900 Supercenters across more than 1,200 counties between 1990 and 2005. I estimate county-specific treatment effects with a bias-corrected synthetic control estimator, then stack and average these in event time. Synthetic control estimators (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2021; Powell, 2021) have traditionally been used with a single treated unit to estimate a counterfactual outcome path, and treatment effects. They can also be adapted to cases with many treated units and staggered treatment timing (Cavallo et al., 2013; Dube and Zipperer, 2015; Acemoglu et al., 2016b; Abadie and L'Hour, 2021; Ben-Michael et al., 2022). I argue this strategy addresses the recent literature which shows that, when treatment adoption is staggered, two-way fixed effects (TWFE) estimators for both difference-in-differences (static DiD) and event-study (dynamic DiD) research designs may yield significantly biased estimates due to heterogeneous treatment effects across cohorts or event years (Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021; Baker et al., 2022). Unlike the regression-based alternatives proposed by those authors, synthetic control estimators yield transparent and interpretable estimates for each treated county, are not subject to extrapolation bias, and do not require parallel mean outcome trends (Abadie, 2021). I demonstrate that my estimating strategy affects the magnitude, significance, and even the sign of the estimates, compared to a TWFE event-study approach. I also demonstrate that my results are robust to using the Callaway and Sant'Anna (2020) DiD estimator—though my preferred stacked synthetic control estimator offers greater precision and flexibility, at the cost of being less computationally efficient.

Crucially, I offer a novel solution to address the lingering concern that there was some unobservable reason, correlated with outcomes of interest, which motivated Walmart to open stores in the counties it entered but not in the counties it didn't (a subset of which often serve as controls). Inadequately addressed, this would introduce endogeneity bias into any estimated treatment effects. My solution is to construct the synthetic control donor pools (potential control counties) from only those counties where Walmart attempted to place a first Supercenter during the period, but where they were blocked from doing so by local efforts. This permits the adoption of an identification strategy similar in spirit to the quasi-experimental 'Million Dollar Plants' approach in Greenstone et al. (2010), as the donor pool counties and treated counties are plausibly similar in those unobservable variables that Walmart may use to identify locations it wishes to enter.

Walmart is a natural choice to focus on when looking for evidence of monopsony power that exacerbates income inequality, both because it has long been the largest private-sector employer in the United States, and because its employees have long earned particularly low wages. Between 1990 and 2005 Walmart grew its ranks by over one million employees (Figure 1), constituting 50% of all U.S. retail employment growth and 4% of total U.S. employment growth. The majority of these new jobs went to hourly workers in the firm's "Supercenter" stores—which were first introduced in 1988, and which competed in many more retail subsectors than the firm's smaller "Discount" stores, especially competing with grocers. By the end of 2005 Walmart had opened over 1,900 Supercenters, each typically employing over 350 workers—equivalent to 20% of average pre-entry county retail employment and 2.5% of total employment. Annual firm-wide turnover of nearly 70% during this period meant most of these employees had to be replaced each year, leading the firm to continuously play a large, active role in local less-skilled labor markets. Despite this and the fact wages have historically increased as firm and establishment size grow, Walmart successfully kept wages and benefits well below those paid by similar large retail chains and, indeed, below the retail average as a whole: as of 2022, self-imposed minimum wages at Amazon, Costco, and Target were $15/hour or more, while that at Walmart stores was just $12/hour. The firm's ability to so effectively control its labor costs while still meeting its mammoth staffing requirements may well have been a major factor in the widespread decline in firm-size wage premia seen in recent decades, especially in the retail sector (Stansbury and Summers, 2017; Bloom et al., 2018).

The Walmart literature has understandably tended to focus on its impact on the retail sector. For example, Hausman and Leibtag (2007) found Supercenters offered consumers substantially lower grocery prices worth 3% of average annual household income, which surely contributed to their popularity, and also caused revenues at nearby incumbent competitors to drop 16% (Arcidiacono et al., 2020),[1] leading some smaller lo-

[1] See, also, Basker (2005b) and Basker and Noel (2009).

cal retailers to exit (Basker, 2005a; Jia, 2008). Despite low prices, Supercenters were also highly profitable, helping boost Walmart's annual profit more than 500% from 1990–2005, greatly benefiting shareholders.

The evidence on Walmart's labor market impact is decidedly more ambiguous. Ketchum and Hughes (1997), and Hicks and Wilburn (2001) found generally null or small positive effects, but these studies were confined to single states and did not adequately address endogeneity concerns. Basker (2005a), Dube et al. (2007) and Neumark et al. (2008) made much more serious efforts to address endogeneity bias, proposing clever instrumental variable strategies as solutions. Basker (2005a) found local retail subsector employment slightly increased with Walmart entry, with the increase halving over the next five years.[2] Conversely, Neumark et al. (2008) found Walmart entry caused local retail employment to fall, and found negative effects on local retail payroll, consistent with Dube et al. (2007). Yet these authors object that each other's instruments violate the exclusion restriction and are not valid: Neumark et al. (2008) argues that the same important variables which influence Walmart entry similarly influence Basker (2005a)'s instrument (planned entry). Basker (2007b) argues the instrument used by Dube et al. (2007) and Neumark et al. (2008) (Walmart's expansion pattern across the U.S.) violates the exclusion restriction as county characteristics are spatially correlated. These substantive criticisms means the authors' conflicting results may all be subject to unresolved bias.

Despite an absent consensus on Walmart's labor market effects, the firm is often passingly cited as a *potential* monopsonist (Naidu et al. (2018); Dube et al. (2020); Azar et al. (2023); Kahn and Tracy (2023)). Yet to my knowledge no study has yet provided direct evidence that Walmart's wage actions affect local labor markets.[3] Indeed, labor markets are often treated as efficient, absent market interventions such as a minimum wage. Yet a large body of research has explored how firms may be able to exercise outsized power in labor markets, allowing them to pay lower wages than would otherwise prevail (Katz and Krueger, 1992; Card and Krueger, 1994, 1995; Burdett and Mortensen, 1998; Bhaskar and To, 1999; Manning, 2003a; Stansbury and Summers, 2020; Azar et al., 2023, 2020; Kahn and Tracy, 2023; Dube et al., 2020; Rinz, 2020; Autor et al., 2023).

In such cases, interventions that might otherwise be distortionary may actually return the local labor

[2] Basker (2005a) also found small negative effects on wholesale employment due to Walmart's vertical integration.

[3] A working paper from Derenoncourt et al. (2021) did find positive cross-employer wage elasticities when Walmart and others raised their self-imposed minimum wages, but the authors subsequently announced they were withdrawing the results.

market to efficiency: while a number of studies have found negative employment effects from minimum wage increases (Neumark and Wascher, 2000; Neumark et al., 2014; Neumark and Shirley, 2021), a large and growing literature has found minimum wages have small or non-negative employment effects (Card and Krueger, 1994, 1995, 2000; Dube et al., 2010; Allegretto et al., 2011; Giuliano, 2013; Dube and Zipperer, 2015; Allegretto et al., 2017; Azar et al., 2023; Cengiz et al., 2019; Dube and Lindner, 2021; Wiltshire et al., 2023). Monopsony power has often been invoked to explain these latter results (Card and Krueger, 1995; Naidu et al., 2018; Azar et al., 2023; Wiltshire et al., 2023). The idea is that monopsonist employers can pay wages below the marginal revenue product of labor (MRPL) and earn rents off of each worker they employ. Moderate minimum wage increases can be absorbed by such employers without making it unprofitable to retain their employees, leading to null or positive local employment effects. For example, Cengiz et al. (2019) and Dube and Lindner (2021) find that minimum wages largely shift low-wage workers from below to just-above the new minimum wage, without disemployment effects or much labor-labor substitution. These results would be expected where monopsony power is exercised. In fact, as Azar et al. (2023) suggest, the sign and magnitude of minimum wage employment effects may largely boil down to whether local employers exercise labor market power.

This paper contributes to the economic literature in three ways. First and foremost, I provide specific evidence that Walmart—the largest private-sector employer in the U.S. and the world—exercises monopsony power. Second, I bring a new methodological approach to the literature on Walmart's labor market effects, to address concerns about endogeneity of entry and other sources of bias. This brings clarity to the conflicting results in that literature, demonstrating that Walmart Supercenters negatively impacted local employment and earnings and increased local employment concentration among retail firms. In this effort I go beyond the scope of prior research on Walmart, and consider the impact on the entirety of an affected labor market rather than restricting the analysis to retail (or occasionally other sectors). Third, I add to the minimum wage literature by demonstrating that an exogenous minimum wage increase led to employment gains in labor markets where this low-wage monopsony employer operated. This affirms a key takeaway from Azar et al. (2023), that different employment effects in the minimum wage literature might be reconciled by accounting for local monopsony power, with direct policy implications: minimum wage increases can boost employment specifically in labor markets where employers wield outsized power. In a companion paper

(Wiltshire, 2024) I present evidence that Walmart's labor practices led to higher federal expenditures on social safety net programs, including the Earned Income Tax Credit and Medicaid.

This paper proceeds as follows. Section 2 outlines how large retailers might impact local labor markets. Section 3 discusses Walmart's relevant history and institutional details. Section 4 discusses methodological considerations and describes my empirical strategy, while Section 5 describes the data. Section 6 presents and discusses the results. Section 7 concludes.

2 How Large Retailers Could Impact Local Labor Markets

Classical competitive labor market theory assumes employment levels and earnings are efficient in the absence of binding wage constraints (e.g. a minimum wage) and given non-wage labor costs, with individual firms perceiving perfectly elastic labor supply and accepting the competitive market equilibrium compensation level as given. However, both theoretical (Robinson, 1933; Burdett and Mortensen, 1998; Bhaskar and To, 1999; Manning, 2003a; Berger et al., 2022; Loertscher and Muir, 2022) and applied research (Katz and Krueger, 1992; Card and Krueger, 1994, 1995; Card et al., 2018; Azar et al., 2023, 2020; Kahn and Tracy, 2023; Dube et al., 2020) suggests various reasons why or how some employers may in fact operate with a degree of power in labor markets, which affords them an opportunity to pay wages below the marginal revenue product of labor (MRPL) if they are willing to constrain the amount of labor they employ. This sort of market power has alternatively been referred to as oligopsony, monopsonistic competition, and bargaining power, depending on the context, but typically does not refer to a 'true monopsony' with a solitary employer of labor. Nonetheless, the term 'monopsony' broadly captures the concept if we take Manning (2003b)'s advice and don't take the 'mono-' part too seriously.[4] Beyond the classic example of a company town, monopsony power has been identified as a potential consequence of, variously, collusive behavior among employers; costs for workers to search for or change jobs; and heterogeneity among both job characteristics and worker preferences over those characteristics (see Manning (2020) and Card (2022) for thorough recent reviews of the literature)[5]—all of which Manning (2003b) argues essentially boil down to labor market "thin"-ness (or employer 'concentration') in one or more dimensions.

[4]Stansbury and Summers (2020) differentiate monopsony power from a decline in worker bargaining power based on the predictions for employment. My focus is more strictly on the 'monopsony' side of their definitions.

[5]Immigration may also give employers a degree of monopsony power.

Notwithstanding the difficulty of determining how a market should be delineated, one common mesaure of market concentration—basd on firm shares of a market—is the Herfindahl-Hirschman Index (HHI), where s_i is firm i's share of some market, and $HHI = \sum_{i=1}^{N} s_i^2$. Several recent papers use a form of HHI to measure labor market concentration as a determinant of monopsony power (Azar et al., 2020, 2022), including in the low-wage retail sector (Azar et al., 2023).

Labor markets may not be perfectly delimited in space or along other dimensions, but a fairly uncontroversial statement is that markets for less-skilled workers are geographically localized. This is because wages for these workers are typically too low to compensate for significant travel, and because competition with other workers increases markedly as distance from a potential employer increases (Manning and Petrongolo, 2017).[6] Thus the spillover effects of significant idiosyncratic shocks, like local entry or exit of large employers, are likely to be broadly restricted to the geographic vicinity of such shocks—for example, with anchor tenants in shopping malls (Pashigian and Gould, 1998), very large manufacturing plants (Greenstone et al., 2010), and big box retailers in general (Haltiwanger et al., 2010; Shoag and Veuger, 2018).

Spillovers from the entry of large employers are often positive (Greenstone et al., 2010; Moretti, 2010), particularly if the employer produces tradeable goods or services and employs both highly-skilled and less-skilled workers. However, if the firm operates in the nontradeable sector, competes with incumbent local firms, is a large player in the local market for less-skilled labor, if it can squeeze local suppliers (e.g. if it accounts for a large proportion of its suppliers' sales and thus wields some power over its supply chain),[7] and if its supply chain structure redirects production *away from* local producers that supply incumbent competitors and *to* its own non-local suppliers, then these labor market spillovers from entry may be negative.[8]

These negative effects may result because the entry of such a firm (i) may take market share from—and thus dampen demand for less-skilled labor among—local incumbent competitors, depressing the future arrival rate of job offers to local less-skilled workers and granting the entrant direct power to set low local wages; (ii) may redirect production for its own substantial market share to local producers who agree to lower prices, reducing local employment and granting the entrant some indirect power to set low local

[6]This is often less true for many highly-skilled workers—particularly those for whom 'working from home' is an option, as became especially evident during the SARS-CoV-2/COVID-19 pandemic (Dingel and Neiman, 2020; Bartik et al., 2020; Barrero et al., 2021).

[7]For example, regional food and beverage manufacturers such as Coca Cola and Pepsi bottlers, industrial bakeries, dairies, and prepared-produce manufacturers.

[8]Though product quality may rise and/or local prices may fall, benefiting local consumers (Bennett and Yin, 2019).

wages; and (iii) may redirect production for its own substantial market share to its own lower-price *non*-local producers, which would reduce labor demand in local goods-production and services-provision firms, reducing local employment and wages, and giving greater weight to the entrant's wage offers as the local outside option. In all cases, negative effects are likely to be exacerbated by local multiplier effects (Moretti, 2010). (i) is an example of direct monopony power in the local labor market. (ii) and (iii) are interesting because monopsony (price-setting) power in other input markets can grant the entrant monoponsy (i.e. wage setting) power in the local labor market. (iii) is especially interesting because it may only be possible for firms that have the capital to build or access supply chains and distribution networks that are only viable at scale.

In all cases, the entry of such a firm could potentially yield negative effects on the employment and wages of local less-skilled labor despite the direct positive shock to retail labor demand that its entry would represent. The thinner the local less-skilled labor market after entry, the more likely is the entrant to exercise some degree of monopsony power, and the greater the likelihood of seeing negative employment and wage effects. This is graphically demonstrated in Panel A of Figure 2.

For the same reason it might lead to lower employment and earnings, monopsony power has been invoked as a possible explanation for findings of non-negative employment effects resulting from minimum wage increases. In a classic study, Card and Krueger (1994, 2000) find non-negative employment effects in New Jersey fast food establishments after that state's 1992 minimum wage increase, compared with similar establishments in eastern Pennsylvania where the minimum wage was unchanged. While the results have famously been challenged (e.g. Neumark and Wascher (2000)), they have nonetheless been pointed to as potential evidence of monopsony power.[9] Numerous recent studies have also found non-negative or only small negative employment effects of minimum wage increases (Dube et al., 2010; Allegretto et al., 2011; Giuliano, 2013; Dube and Zipperer, 2015; Allegretto et al., 2017; Reich et al., 2017; Azar et al., 2023; Cengiz et al., 2019; Dube and Lindner, 2021; Wiltshire et al., 2023)—though again some of these research designs and results are contested (Neumark et al., 2014; Jardim et al., 2022; Neumark and Shirley, 2021), and most of this literature does not consider the retail sector.[10] Most notably, (Cengiz et al., 2019) find U.S. minimum

[9]Many economists were also skeptical that monopsony power could be found among fast food restauranteurs, but recent evidence of anti-poaching agreements between fast food franchisees (Naidu et al., 2018; Krueger and Ashenfelter, 2022) suggests a re-think may be in order.

[10]Wursten and Reich (2023) notably find no disemployment effects in grocery and general merchandise stores.

wage increases had null or even slightly positive employment effects in the wage distribution right around the minimum wage, which suggests low-wage employers were paying wages below MRPL and were thus willing and able to absorb minimum wage increases without laying off workers—counter to predictions for competitive labor markets but consistent with monopsony power,[11,12] while Azar et al. (2023) find evidence of monopsony power by looking at employment effects of minimum wage increases in more concentrated occupational labor markets. Provided a new minimum wage is not too high and labor supply is not perfectly elastic, a simple monopsony model predicts that a binding minimum wage increase might even lead to local employment gains (Panel B of Figure 2).

The basic idea is that an employer with a degree of monopsony power faces a less-than-perfectly elastic labor supply curve, such that they must trade-off between the amount of labor employed and the compensation offered. This allows these firms to offer wages *below* MRPL if they constrain the amount of labor they employ, yielding greater profit than would be possible with a competitive labor market. If monopsonists are unable to wage-discriminate among their workers, then any higher wage offered to attract additional labor must also be paid to incumbent employees—that is, firms with monopsony power face a marginal cost of labor that is *higher* than the wage they pay. The implication is that labor markets in which monopsony power is exercised will see lower employment and wages than otherwise identical competitive markets. In such cases, a *binding* minimum wage increase may force the common wage paid above the monopsony wage, relaxing the incentives to constrain employment and increasing the number of workers hired by moving up the labor supply curve—to a point. Provided the new minimum wage is not above the initial MRPL, a minimum wage increase will induce firms with some monopsony power to *increase* the amount of labor they employ, potentially leading to an increase in local employment levels.

[11] Cengiz et al. (2019) also implement a stacked event-by-event estimating strategy which is similar in spirit to my own approach.

[12] Cengiz et al. (2019) also show that negative estimated aggregate employment effects that arise from classic two-way fixed effects regressions are generally driven by employment losses in the upper tail of the wage distribution, where minimum wage increases likely have no effect, and by the inclusion of the 1980s in samples, when few minimum wage changes occurred. Dube and Lindner (2021) found that large positive estimated employment effects in the upper tail of the wage distribution largely disappeared once they controlled for differences between cities that did and did not enact minimum wages.

3 Walmart

In 1988 Walmart opened the doors of its *Supercenter* store in Washington, Missouri—the first of over 1,900 such stores the company would open in the U.S. through 2005. Supercenters offered a variety of services beyond those offered by its "discount" stores and well outside of the retail archetype of the day, including photo processing, tire & lube, and financial services, while offering an expansive selection of general merchandise and full service grocery options. Many remained open all night. The sheer scope of these stores necessitated the firm hire substantial numbers of workers: each Supercenter employed "350 or more Associates on average" (Wal-Mart Stores, Inc., 2006a).[13]

This need for workers meant Supercenter entry was a significant shock to the local labor markets where they located. Assuming the 350 employees per store figure stated by Walmart to be true for every store (a conservative estimate), among those counties receiving their first Supercenter between 1990 and 2005 the workforce of a single Supercenter constituted, on average, 20% of pre-entry retail employment and 2.5% of pre-entry county total employment. These labor demands also meant Supercenter openings were the primary driver of the company's addition of over one million employees to its U.S. payrolls between 1990 and 2005 (Wal-Mart Stores, Inc., 2006c). To put this in perspective, this expansion of Walmart's workforce constituted fully half of all retail job growth and 4% of total U.S. non-farm employment growth over the period. It also more-than-tripled Walmart's share of both U.S. retail employment—which grew from 2.5% to 8.8%—and of total U.S. employment—which grew from 0.3% to 1%—turning the firm into the largest private-sector employer in the country, the largest U.S. grocer, and the world's largest retailer.

What's more, most of these employees had to be replaced each year. Drogin (2003) summarizes administrative data which Walmart was compelled to submit to a court. The result shows 3,945,151 unique employee records between January 1996 and March 2002. Given annual U.S. employee levels (see Figure 1, with approximately 620,000 U.S. employees in January 1996 climbing to 1.08 million in January 2002), I calculate this translates to average annual turnover of 69% across the firm over this period, and almost certainly higher among its sales staff. This is consistent with anecdotal reports of very high turnover, exceeding 90% in some stores (Hines, 2020). Thus the labor demand shock from Supercenter entry was largely real-

[13] Basker (2007a) claims Supercenters employ around 425 employees each. My own (unreported) estimates, using total employees with annual store-type and distribution center counts, suggest there were as many as 475 employees per Supercenter, on average.

ized anew each year. At 350 employees per store and 69% average annual turnover, the average Supercenter had hired at least $350 \times (1+(0.69 \times 5)) = 1{,}558$ employees by the fifth year after entry—equivalent to 88% of average county retail employment and 11% of average county total employment in the year before entry.

Despite such substantial new demand for labor, Walmart was able to maintain the strict emphasis on payroll control that has long been a core element of its business model (Walton and Huey, 1993; Lichtenstein, 2009). This helped the firm maintain its low prices (Hausman and Leibtag, 2007; Arcidiacono et al., 2020) and boost annual profit over 500%, from $2.1 billion in 1990 to $13.6 billion in 2005 (Figure 1). California grocers also cited it when they attempted to cut employee wages and benefits in anticipation of the 2003 Supercenter rollout in that state—resulting in a months-long strike of 67,300 grocery workers (Foster, 2004).

While Walmart does not disclose its wage schedule, a 2002 ruling compelled the firm to turn over administrative records on personnel and compensation. The submitted analysis (Drogin, 2003) makes it possible to back out annual earnings and employee counts by employment status and sex for hourly Walmart employees from 1999-2001.[14] Table 1 summarizes these data by year and full-time/part-time status, alongside mean earnings for both U.S. retail employees and all U.S. employees.[15] These Walmart employees earned substantially less than average U.S. workers: the average 'tenured' full-time hourly Walmart employee earned $16,882 (current USD) in 2000, equivalent to around 67% and 47% of mean annual earnings for full-time retail employees and all full-time employees respectively. Part-time earnings are ordinally similar.

A large wage premium has historically been associated with firm and establishment size (Brown and Medoff, 1989), widely attributed to greater productivity at those firms (Idson and Oi, 1999). In recent decades other factors have worked to reduce this premium (Stansbury and Summers, 2017), and to completely eliminate it in the case of the retail sector (Bloom et al., 2018).[16] While Table 1 makes clear that low wages were by no means unique to Walmart within the retail sector (also see Ilg and Haugen (2000)), the firm's hourly wages lag even the industry average. Of note, Walmart's hourly wage rates also buck the trend set by its large, national retail chain competitors, whose workers generally earn wages above the industry mean. For example, as of 2022 Walmart paid its U.S. workers a voluntary minimum wage of $12 per

[14] Covers active employees with $\geq$ 45 weeks of tenure at Walmart *Discount*, *Supercenter*, or *Neighborhood Market* stores. I consider only hourly employees as they constitute the vast majority of Walmart's workforce and their average annual earnings are 30-40% those of salaried employees. The resulting sample covers around one-half of Walmart's total U.S. workforce for each year.

[15] Walmart's 2005 Equal Employment Opportunity Commission EE0-1 report also shows the firm's store employees are disproportionately comprised of minorities and women. Appendix Figure A1 presents the main table from that report.

[16] Though Cardiff-Hicks et al. (2015) find wages are inverse-U-shaped in firm size.

hour, while the firm's largest competitors offered much more: Amazon and Target guaranteed their workers a minimum of $15/hour, while Costco's minimum wage was $16/hour.[17] This suggests Walmart may be almost entirely responsible for the elimination of the large firm wage premium in the retail sector despite being responsible for almost half of general merchandiser productivity growth, and despite having value-added per worker over 40% higher than that seen among other general merchandise retailers in the 1980s and 1990s (Basker, 2007a).

An obvious question is how Walmart is able to attract so many new workers to staff its Supercenters while so effectively controlling wages. If labor markets remain perfectly competitive (and if labor supply is not perfectly elastic), then the positive demand shock from Supercenter entry should yield an increase in both local employment and local worker compensation.[18] However, if Supercenter entry introduces a degree of monopsony power into previously competitive labor markets then, as discussed in Section 2, it is possible that local employment and worker compensation could remain constant or even fall despite the large, positive shock to local labor demand which Supercenter entry represents.

Such impacts on local workers might even be exacerbated (and perhaps further facilitated) by Walmart squeezing its local suppliers to cut costs (Bloom and Perry, 2001; Wilmers, 2018) (a consequence of monopsony power over suppliers), and perhaps especially through consequent diversion of local market share away from local suppliers of incumbent competitors and to its own national and international suppliers—made possible by its industry-revolutionizing distribution network (Grean and Shaw, 2002; Appelbaum and Lichtenstein, 2006; Basker, 2007a)—which may have reduced employment and wages among local goods-producers—even before Chinese imports decimated U.S. manufacturing employment beginning in 2001 (Acemoglu et al., 2016a). While reports suggest Walmart currently sources more than 70% of its merchandise globally (AAM, 2020), in the 1990s the firm sourced many products from firms which—at least at the beginning of their relationships with Walmart—still had substantial U.S. manufacturing operations. These included such firms as Coca Cola bottlers, Vlasic Pickles, Nabisco, Dean Foods, Hoover, Maytag, Huffy Bikes, Dial Soap, Levi Strauss, Master Lock, and Eastman Kodak, to name a few—firms which Walmart squeezed hard for efficiencies over my period of interest (Fishman, 2003). The arrival of a Supercenter

[17] Suggestive evidence in Cascio (2006) and Cardiff-Hicks et al. (2015) is consistent with hourly wages at Walmart trailing those at Costco for years by about the same proportion, though it is not clear where the estimates in Cascio (2006) come from.

[18] With perfectly inelastic labor supply, employment would remain unchanged but compensation would increase even more.

likely shifted manufacturing demand away from many local suppliers and to Walmart's suppliers, who were often consolidating their manufacturing operations into a few large plants elsewhere in the U.S. even before offshoring began in earnest.

The pace and scale of Walmart's Supercenter expansion, and the share of county-level retail and total employment which a typical Supercenter employs, along with the particularly low wages earned by hourly workers in these establishments, the non-tradeable (or local) nature of brick-and-mortar retail services, and the novelty of Walmart's supply chain and distribution network (especially during the period on which I focus), together all suggest Walmart could potentially exercise power in labor markets. Indeed, much of the recent literature on monopsony (Naidu et al., 2018; Azar et al., 2023; Kahn and Tracy, 2023; Dube et al., 2020) explicitly cites Walmart as a potential monopsonist, yet I am aware of no previous efforts to explicitly measure whether the firm does, in fact, exercise monopsony power in the labor markets where it is active.[19]

4 Methodology

I examine whether Walmart Supercenters exercise monopsony power by exploiting two unique treatments in a natural experiment. Each treatment offers an opportunity to test whether affected local labor markets respond in ways that are consistent with competitive employer behavior. I begin by considering the roll-out of Walmart Supercenters across the United States in the 1990s and early 2000s, where treatment is the entry of the first Supercenter into a county. I consider the dynamic percentage change impact on labor market concentration, then on employment and earnings, in aggregate and variously disaggregated including for retail trade specifically.

I then consider the impact of the 1996/97 federal minimum wage increases on counties *which already had a Supercenter* before 1996—in states where the state minimum wage tracked the federal minimum wage from 1990–2003, to ensure the federal increases were binding and plausibly exogenous to local conditions. This contrasts with state or city minimum wage changes, the timing and amount of which are more likely correlated with local conditions (Allegretto et al., 2017; Dube and Lindner, 2021). In this analysis, *all* 'treated' and 'control' counties experienced the binding federal minimum wage increases, and treatment is

[19] Dube et al. (2020) uses survey data from Walmart employees, among other sources, and finds firms have a degree of monopsony power. However, this measures the *potential* to exercise monopsony power rather than whether such power is *actually* exercised.

the *interaction* of the minimum wage increase and pre-1996 Supercenter presence. I consider the dynamic percentage change impact on employment and earnings, in aggregate and for retail.

4.1 Threats to Identification

For both treatments, any comparison of treated counties to a set of counties without a Walmart Supercenter faces a classic endogeneity bias concern: there may be unobservable variables correlated with the outcomes of interest which may also have influenced Walmart's decision to open in the treated counties. A priori, such bias could go in either direction (e.g. Walmart may systematically choose to locate Supercenters in counties where they expect robust economic growth; or they may systematically choose to locate in economically depressed counties where their low-price, low-wage business model might be more attractive). Any plausible research design must address this issue—for example, by trying to find a valid instrument as in Basker (2005a); Dube et al. (2007) and Neumark et al. (2008),[20] or by dropping all untreated counties which Walmart did not clearly attempt to enter. I do the latter, having identified those counties where Walmart clearly expressed an interest in building a Supercenter during the period, but where local efforts prevented them from doing so.[21] This is a "natural experiment" setting. As the donor pool was selected by Walmart according to the same criteria and in the same period as the treated sample, the same unobservables that influenced Walmart's decision to enter the treated counties should obtain in the donor pool counties.

A recent literature (Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021; Baker et al., 2022) has made clear that additional caution should be exercised with the 'Supercenter entry' treatment, as entry occurred at different times for different counties and given treatment effects may be heterogeneous across cohorts and time. 'Difference-in-differences' (DiD) research designs (often called an 'event study' if treatment effects are modelled as dynamic) are common when using panel data with such staggered treatment adoption, and many use OLS or IV regression with two way fixed effects (TWFE) estimator of the form:

$$y_{it} = \alpha + \lambda_i + \gamma_t + \sum_{e=l, e\neq -1}^{m} \beta_e \mathbb{1}\{E_{it} = e\} + u_{it} \tag{1}$$

where λ_i and γ_t are unit and time fixed effects, and β_e are event-time treatment effects with β_{-1} excluded.

[20] As noted earlier, these authors raise serious concerns that each other's instruments violate the exclusion restriction

[21] I discuss these untreated 'donor pool' counties in detail in Section 5.

Untreated "control" units may or may not be included, and the sample may or may not be balanced in event time.

If the endogeneity concerns were addressed and there were no further identification issues, (1) might yield unbiased estimates of the treatment effects of Supercenter entry.[22] Yet these research designs do not consider the potential confounding effects of counties being treated at different times. For example, Goodman-Bacon (2021) shows that the TWFE estimator with staggered adoption is in fact a weighted average of all 2×2 DiD estimators, meaning that already-treated cohorts can serve as controls for other treated cohorts. When treatment effects are not constant, changes in the treatment effects from those 2×2 comparisons which use already-treated cohorts as controls are subtracted from the estimate of the causal parameter of interest, causing the magnitude and even the sign of the estimate to be wrong, with the underlying weights which combine the individual treatment effects into the causal parameter of interest also generally being uninterpretable and often negative. The issues persists even with dynamic specifications. Sun and Abraham (2020); Callaway and Sant'Anna (2020); De Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021) and Borusyak et al. (2021) propose a variety of alternative estimators designed to address these concerns, each of which uses some untreated or not-yet-treated cohort as a control group and which depends on some form of a parallel pre-trends assumption.[23] As a robustness check on my preferred estimating strategy, I also present results using the Callaway and Sant'Anna (2020) estimator.

4.2 Stacked-in-Event-Time (bias-corrected) Synthetic Control Estimator

To address the potential biases that may arise from the staggered roll-out of Walmart Supercenters across the U.S., I adapt a "stacked" synthetic control estimator to accommodate many treated units (Cavallo et al., 2013; Acemoglu et al., 2016b; Abadie and L'Hour, 2021; Peri et al., ming) and staggered treatment timing (Dube and Zipperer, 2015; Ben-Michael et al., 2022; Wiltshire, 2022). Intuitively, synthetic control methods (SCMs) construct a weighted average of untreated counties to resemble the pre-treatment characteristics of each treated unit (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015; Abadie, 2021). SCMs are similar to a dynamic DiD research design in that the differences in outcome values between a treated unit and its SCM-estimated counterfactual yield a dynamic path of estimated treatment effects on the treated

[22] I present naive estimates from various specifications of Equation 1 in Appendix Table A11.

[23] Various other standard identifying assumptions are also necessary.

unit. Crucially unlike DiD, however, SCMs under a linear factor model do not depend on a parallel trends assumption (Abadie, 2021; Arkhangelsky et al., 2021). Moreover, while estimates from linear regression estimators (often used with a DiD research design) can suffer from severe extrapolation bias (e.g. Abadie et al. (2015); Abadie (2021)), synthetic control estimates of treatment effects are free of extrapolation bias when the weights are restricted to be non-negative.[24] Given a 'donor pool' of untreated (control) counties that are plausibly not subject to endogeneity concerns, a well-matched synthetic control estimates the evolution of an outcome of interest in a treated county *in the absence of treatment*, facilitating an estimate of the dynamic effects of treatment as the differences between that county's actual outcome path and its synthetic control.[25] The stacked-in-event-time synthetic control estimator is an event-time-specific weighted average of the treatment effects over all treated counties.

As I detail in Section 5, I select the donor pool counties as those where Walmart wanted to place a first Supercenter in the period of interest but where local efforts prevented them from doing so. This makes these control counties much more comparable to the treated counties than if they had simply been selected according to observables, increasing the likelihood of a good pre-treatment fit and marginalizing the likelihood of endogeneity bias in the estimated treatment effects. I then apply the synthetic control bias-correction for inexact matching on pre-treatment covariates, proposed by Abadie and L'Hour (2021); Abadie (2021) and described below. Using the `allsynth` Stata package (Wiltshire, 2022), I estimate these bias-corrected treatment effects for each treated county, then stack and average them by event year.

4.2.1 Formal Setting

Formally, I observe data for a total of $I+J$ counties over calendar years t, with treated counties $j = 1, \ldots, I$, and a carefully selected set of untreated "donor pool" counties $j = I+1, \ldots, I+J$, some combination of which are comparable to each treated county, and all of which are plausibly *not* selected into non-treatment

[24] Unlike k-nearest neighbor matching methods, Abadie and L'Hour (2021) note this does not impose a fixed number of matches, and it matches the treated unit to an unequally-weighted set of untreated units (the 'synthetic control'). Like matching methods, however, standard synthetic control weights are sparse, non-negative and sum to one, which ensure against extrapolation bias and allow the synthetic control to be interpretable in a straightforward way. This contrasts with regression-based estimators which, as Abadie et al. (2015) show, implicitly weight control units with weights that also sum to one, but unlike standard synthetic control estimators allow those weights to be negative.

[25] An additional benefit over regression-based estimators is that, conditional on a good pre-treatment fit and enough pre-treatment periods, under a linear factor model the synthetic control estimator can accommodate unit-level, time-variant unobserved confounds and still yield estimates relatively free of bias (Abadie et al., 2010), while a demeaned synthetic control estimator can do the same with even fewer conditions (Ferman and Pinto, 2021). TWFE estimators cannot accommodate time-variant unobserved confounds.

based on unobservables correlated with outcomes of interest. I observe all counties through calendar year T. Each treated county $i = j \leq I$ is observed a strictly positive number of years prior to treatment. Treatment occurs in calendar year $T_{0i} + 1 \leq T$ (let $T_{0j} = T \ \forall \ j > I$), which can vary over the treated counties.[26] For each j and t, I observe an outcome of interest, Y_{jt}, and a set of k predictors of the outcome such that the vector $\mathbf{X}_j = (X_{1,j}, ..., X_{k,j})'$ contains the values of these predictors for j. These k predictors can include r covariates and M linear combinations of the outcome variable, all observed prior to treatment.[27] The $k \times J$ matrix $\mathbf{X}_0 = [\mathbf{X}_{I+1}, ..., \mathbf{X}_{I+J}]$ contains the values of the predictors for the donor pool counties. For each $\{j,t\}$, define Y^N_{jt} as the potential outcome if j does $\{N\}$ot received an intervention, and for $t > T_{0j}$ define Y^{Int}_{jt} as the potential outcome if j receives an $\{Int\}$ervention. In each t the marginal treatment effect of interest for j is:

$$\tau_{jt} = Y^{Int}_{jt} - Y^N_{jt} \tag{2}$$

For each treated unit, in the treated period $t > T_{0i}$ I observe $Y^{Int}_{it} = Y_{it}$, so to obtain an estimate of τ_{it} I need only estimate Y^N_{it}—the counterfactual dynamic path of Y_i (in the absence of treatment)—then calculate:

$$\hat{\tau}_{it} = Y_{it} - \hat{Y}^N_{it} \ \ \forall \ \ \{i,t\} \tag{3}$$

For the 'Supercenter entry' treatment, I want to estimate the *event-year-specific* average treatment effects on the treated (ATT), $\overline{\tau} = (\overline{\tau}_0, ..., \overline{\tau}_E)$, where treatment occurs in event year $e(T_{0i} + 1) = 0 \ \forall \ i$ observed at least E years after treatment. I normalize the outcome in each i and its donor pool to 100 in T_{0i}, so as to make τ_{ie} comparable across all i. The estimator for the ATT of interest in e then:

$$\hat{\overline{\tau}}_e = \sum_{i=1}^{I} \gamma_i \hat{\tau}_{ie} = \sum_{i=1}^{I} \gamma_i (Y_{ie} - \hat{Y}^N_{ie}) \tag{4}$$

with weights $\gamma_i \geq 0 \ \forall \ i$ and $\sum_i^I \gamma_i = 1$ on the treated counties, which estimates the average percentage change in the outcome from the final pre-treatment event year, $e = -1$.[28]

The classic synthetic control estimator for Y^N_{it} is a weighted average of the outcome values of the *donor pool* units, selected to make the synthetic control resemble the treated unit in the pre-treatment period:

[26] I focus on an "absorbing treatment" (Sun and Abraham, 2020) such that any treated county remains treated.

[27] I discuss the predictors for the various specifications in Appendix Section A.2.

[28] For the results presented I set $\gamma_i = \frac{pop_{1990,i}}{\sum_i^{n_1} pop_{1990,i}}$, where $pop_{1990,i}$ is the 1990 population in treated county i. This ensures each $\hat{\overline{\tau}}_e$ is not overly-affected by large percentage effects in small counties. The pattern of results also broadly holds if counties are equally weighted, with slightly different magnitudes.

$$\hat{Y}_{it}^{N} = \sum_{j=I+1}^{I+J} w_j^i Y_{jt} \;\; \forall \;\; t \tag{5}$$

These weights, $\mathbf{W}^i = (w_{I+1}^i, ..., w_{I+J}^i)'$, minimize the distance between i and its donor pool counties, given a set of weights on the k predictors, $v_1^i, ..., v_k^i$, which determine the relative importance of the predictors.[29] Given $v_1^i, ..., v_k^i$, I follow Abadie et al. (2010) and select the the synthetic control for i, $\hat{\mathbf{W}}^i = (\hat{w}_{I+1}^i ..., \hat{w}_{I+J}^i)'$, to minimize:

$$\left(\sum_{h=1}^{k} v_h^i (X_{h,i} - w_{I+1}^i X_{h,I+1} - ... - w_{I+J}^i X_{h,I+J})^2 \right)^{1/2} \quad \text{s.t.} \quad \sum_{j=I+1}^{I+J} w_j^i = 1, \; w_j^i \; \geq \; 0 \;\; \forall \;\; j \in \{I+1, ..., I+J\} \tag{6}$$

where the second constraint prevents against extrapolation bias. In practice, I also allow the donor pool for each i to vary by excluding any donor counties in the same commuting zone as i to make SUTVA plausible,[30] and ensure all treated and donor pool units are observed at least five years on either side of every $T_{0i}+1$ (balancing the sample over $e \in [-5, 5]$).

To the extent that the synthetic control outcome path for each i, $\hat{\mathbf{Y}}_i^N$, is a valid counterfactual for the actual outcome path, $\mathbf{Y}_i$, any bias resulting from dynamic, systematic changes in $\mathbf{Y}_i$ is 'cleaned' from each $\hat{\tau}_{it}$, yielding unbiased estimates of the causal effect of treatment on the outcome in each $\{i, t\}$. Yet bias may still be present as a result of differences in the predictor variables between each treated unit and its synthetic control donors. To address this, I apply the synthetic control bias-correction proposed by Abadie and L'Hour (2021)—analogous to that explored in Abadie and Imbens (2011) for bias-correction in matching estimators. Specifically, for each i I first estimate $\hat{\mathbf{W}}^i$ from synthetic control estimation on the uncorrected outcome values. I then estimate $\hat{\mu}_{0t}^i(x)$, a predictor of Y_{it} given $X_i = x$, by first regressing Y_t on the complete set of predictor variables using only i's donor pool units. This admits $\tilde{Y}_{it} = Y_{it} - \hat{\mu}_{0t}^i(X_i)$ and $\tilde{\hat{Y}}_{it}^N = \sum_{j=I+1}^{I+J} \hat{w}_j^i \left(Y_{jt} - \hat{\mu}_{0t}^i(X_j) \right)$, which I convert to event time.[31] The estimator of the bias-corrected average treatment effect on the treated, of Supercenter entry, for each e, is then:

$$\hat{\bar{\tau}}_{BC_e} = \sum_{i=1}^{I} \gamma_i \hat{\tau}_{BC_{ie}} = \sum_{i=1}^{I} \gamma_i (\tilde{Y}_{ie} - \tilde{\hat{Y}}_{ie}^N) \tag{7}$$

Each $\hat{\bar{\tau}}_{BC_e}$ is an estimate of the percentage average treatment effect on the treated in event year e. I

[29] I use the regression-based method (Kaul et al., 2022) to select the v_h^i weights.

[30] This removes at most one donor county from each donor pool.

[31] Figure 1 of Wiltshire (2022) shows the effects of the bias-correction procedure on canonical synthetic control estimates from Abadie et al. (2010).

provide a Stata package, `allsynth`, to automate implementation of this procedure (Wiltshire, 2022). Note this setup nests the case of the "classic" synthetic control estimator for τ_{1t} with $I = 1$.

4.3 Inference

Though the literature has rapidly been evolving, test statistics based on permutation tests remain the standard for synthetic control inference. The idea of using the ratio of the mean square prediction error (RMSPE) to test for significance of estimated treatment effects from synthetic control estimates stems from Abadie et al. (2010) and Abadie et al. (2015), and has been further developed in a series of recent papers (Cavallo et al., 2013; Abadie et al., 2015; Dube and Zipperer, 2015; Ferman and Pinto, 2017; Firpo and Possebom, 2018; Abadie and L'Hour, 2021). Several more-recent papers (Doudchenko and Imbens, 2016; Hahn and Shi, 2017; Arkhangelsky et al., 2021; Chernozhukov et al., 2021; Ben-Michael et al., 2021, 2022) have considered alternatives that are variously appropriate under different assumptions and settings.

I primarily follow Abadie and L'Hour (2021) and use the RMSPE-based approach to calculate *p*-values from the ordinal ranking of the *RMSPE* of each actual dynamic average treatment effect relative to a (sample) empirical distribution of 'placebo' dynamic average treatment effects, as described by Cavallo et al. (2013); Abadie and L'Hour (2021); Abadie (2021), and assuming treatment assignment probability is uniformly distributed. In this approach, I reassign treatment from each treated county i to each of i's untreated donor pool counties. Placebo estimated marginal treatment effects $\hat{\tau}_{jie}$ can then be estimated for each $j > I$ and e using a donor pool comprised of i and the remainder of the untreated donor pool counties for i. The number of $\hat{\tau}_{jie}$ clearly grows quickly in I and J, as does the number of possible placebo average treatment effects. Consequently, with uniform probability I sample 1,000 placebo averages to construct each sample empirical permutation distribution of average treatment effects.

One nice feature of the ranked-RMSPE *p*-value is that it normalizes post-treatment deviations from the synthetic control by a measure of pre-treatment fit, discounting units with noisy pre-trend outcome paths. However, Hahn and Shi (2017) argue that permutation tests of this sort may suffer from size distortions, while Zhang (2019) observes that with many treated units the ranked-RMSPE may be significantly under-powered.[32] Both identify tests in the spirit of Andrews (2003) as preferable alternatives, assuming

[32] The null distribution is constructed from averages of randomly selected placebo treatment effects, and the likelihood that the placebo synthetic controls include positively weighted *actually-treated* units increases in the number of treated units.

observations are stationary and ergodic under the null—though tests of this sort may be under-powered with at most 10 pre-treatment years. Alternatively, Chernozhukov et al. (2021) and Ben-Michael et al. (2021) propose a conformal inference method, Ben-Michael et al. (2022) propose a wild boostrap approach, and Arkhangelsky et al. (2021) propose several resampling-based approaches. Given the uncertainty around which approach is best, I present p-values from (i) Andrews tests, (ii) the ranked-RMSPE approach, and (iii) placebo-average-variance estimation under the assumption of homoskedasticity across units (Arkhangelsky et al., 2021), along with plots of the sample placebo-average distributions and the placebo-average-variance estimated 95% confidence intervals. Details are in Appendix A.3. I construct the p-values to be conservative, including ensuring the RMSPE-ranked p-value can never got to zero (e.g. even when the ATT estimate has the largest RMSPE relative to the empirical distribution of N placebo ATTs, the associated p-value will be $rank(RMSPE_{ATT})/(N+1) = \frac{1}{N+1} > 0$), calculating the $RMSPE$ through event-year e using the estimates from all post-treatment periods through e (tempering the impact of larger estimates in later post-treatment periods), and deriving each p-value using two-sided tests.

5 Data

Data cover the years 1990 to 2005. My earnings and employment data come from the Quarterly Census of Employment and Wages (QCEW). These data, from the U.S. Bureau of Labor Statistics (BLS), are fed by state Unemployment Insurance (UI) accounting systems, and cover more than 95% of all employed individuals from UI-reporting establishments, in county-by-industry-by-time cells. While some data are available at the 6-digit (NAICS 2002) industrial classification level, it is only at the 2-digit "Supersector" level of broad industrial categorizations that there are relatively few cells suppressed due to privacy concerns (i.e. when cells represent particularly few workers). Even at the 2-digit level, suppressions are only minimal for Retail Trade, though they are also minimally suppressed in aggregate and for the 'goods-producing' and the 'service-providing' industry agglomerations. I calculate annual averages from quarterly observations of employment and per-worker earnings for each cell. All dollar figures are deflated to 2017 USD using the Personal Consumption Expenditures: Chain-type Price Index (PCEPI). I also use the BLS Local Area Unemployment Statistics reports of county-by-year labor force numbers, and I use these data to calculate the labor force participation rate (LFP) as the local labor force divided by the local population aged 15 and older (from county-by-year intercensal population estimates from the U.S. Census Bureau).

I take the locations and dates of Walmart Supercenter openings from Holmes (2011). Additionally and importantly, I assemble a unique data set of 39 "donor pool" counties in which Walmart attempted to place a first Supercenter during the period of interest, but where the company was ultimately prevented from doing so by local efforts. I gathered anecdotal observations of frustrated efforts to open Supercenters from Sprawl-Busters (2018) Newsflash Blog, then confirmed each event using local news reports and council minutes, and cross-referenced these with the Holmes (2011) data. This is likely the universe of counties in which Walmart tried but failed to place a first Supercenter during this period. As two examples of the ways in which Supercenters were delayed or blocked from entering these counties, consider Bedford Township, MI (Monroe County)—a suburb of Toledo, OH—where in 2003 the township voted against rezoning land to permit a Supercenter first proposed in 2001, ultimately resulting a years-long lawsuit. Also consider Potsdam, NY (St Lawrence County), where residents spent years fighting the town over its approval of a Supercenter development initially proposed in 1998—though the company ultimately opened the store in 2008 after the residents lost a state Supreme Court ruling. The complete list of donor pool counties is in Appendix Table A1. Walmart's interest in locating a Supercenter in these donor pool counties means they are much more likely to share (with *treated* counties) those unobserved features and trends which the firm used to select counties that it wished to enter—in a 'revealed preference' way, similar in spirit to the 'Million Dollar Plants' approach in Greenstone et al. (2010), which compared locations which received a very large plant to those which had been shortlisted for the same plant but which ultimately fell short. This makes the donor pool counties potentially much more comparable on *unobservables* to counties which received a Supercenter than a control group of counties selected only according to observables.[33]

In Figure 3 I map the full set of counties in the contiguous U.S. which received their first Supercenter between 1995 and 2005, the subset which constitutes my full treated sample, and the donor pool set of untreated counties (listed in Table A1). To determine my treated sample, I (1) exclude treated counties which received their first Supercenter before 1995—to ensure sufficiently many counties are treated in each calendar year, with at least five years of pre-treatment observations. I then (2) exclude treated counties without at least five years of post-treatment observations, and (3) require all treated and donor pool counties to have

[33]See Appendix A.1 for further discussion of the donor pool counties.

had a 1990 population between 10,000 and 1 million people. (1) ensures the estimated event year treatment effects are identified from a balanced sample of treated counties; (2) and (3) remove outlier counties from the sample to provide a common support for matching. This yields 582 treated counties and 39 donor pool counties. For each outcome I further require all treated counties to have had 1990 values within the minimum and maximum observed in the donor pool, such that each 1990 treated county outcome can be reconstructed as a covex combination of a subset of donor pool counties. This yields treated county samples of between 505–578 counties, depending on the outcome. The treatment cohorts are broadly similar in size.

For numerous variables of interest I plot the densities by sample in Figure 4. I also report summary measures of select variables for 1990, by sample, in Table 2. From the figures and the table it is clear that the treated sample is much more similar to the donor pool sample than to the excluded untreated counties. In the treated and donor pool samples, the median county population was approximately half that of the sample means, and is approximately 50% larger in the donor pool than in the treated sample, but the two groups are broadly similar in overall and retail employment levels, population, average earnings, establishment size, retail share of total employment, and retail HHI. By contrast, the median county in the excluded untreated sample has barely one fifth the population of the mean county, and is one fourth the size of the median treated county. Moreover, retail share of employment and average establishment size are both lower in the excluded sample than the treated sample, while retail HHI is much higher. Median earnings in the treated sample are also closer to those in the donor pool than in the excluded sample.

Selecting the donor pool using counties where Walmart wanted to build a Supercenter but couldn't, rather than using the entire set of untreated counties, thus ensures that unobservable similarities between the treated sample and the donor pool partially manifest as more-similar observables, while it is by no means clear that any set of observable variables would sufficiently identify a set of control counties with comparable unobservables to the treated sample in the way the donor pool selection process does. The synthetic control estimator will then further identify the weighted combination of donors which best reproduces the observed values of the covariates in the pre-treatment period for each treated unit individually, while the bias-correction adjusts for any remaining observable differences between each treated unit and its donors.

I also employ the historical Dun's Market Indicators data, licensed from Dun & Bradstreet (DNB), which contain establishment-by-year observations of employment, sales, and location. These data are (mostly) collected by the firm in the course of its business as a credit reporting agency, and are discussed at length in Howland et al. (1982) and Carlson (1995).[34]

The primary advantage of the DNB data is the level of granularity they provide: establishment level observations of employment permit calculation of labor market employment level-concentration. An ideal measure for my purposes would be an HHI based on shares of local job openings or new hires, as this would allow measurement of the impact on local employment flow-concentration rather than level-concentration. Flow-concentration is likely much more relevant for measuring labor market monopsony power, as it captures concentration of active labor market involvement. [35] This could be particularly true with Supercenters. As employee turnover at Walmart stores often exceeded 70%, the number of local job openings created by a Supercenter was nearly as large in each year after opening as in the year of opening itself: by the fifth year after entry, the average Supercenter had hired workers equivalent to 11% of average pre-entry county aggregate employment and 88% of pre-entry retail employment. If a Supercenter's presence also reduced the number of new job openings posted by incumbent local establishments (e.g. because of incumbent cost-cutting due to output market competition from the Supercenter), then a Supercenter could be responsible for a large share of new low-wage local job postings each year, even if employment level shares stayed relatively constant in the post-entry years. Unfortunately, data on local job postings or new hires by establishment or firm are not available for my years of interest. Still, employment-level HHI may partially capture changes in employment concentration over time if employment shares can be observed with relative precision.

I use the DNB data to calculate local retail employment-level HHI. I aggregate establishment-level employment by firm by county by year to measure employment-level in each labor market. Due to occasional errors which substantially overstate the number of employees in a given establishment, I restrict the data to establishments reporting $\leq 1{,}000$ employees, and focus on establishments which list retail (SIC 2-digit codes 52–59) as their primary industry.[36] Appendix Figure A2 compares national DNB and QCEW employee counts by year.

I take minimum wage data from the Tax Policy Center. The federal minimum wage was $4.25 from April 1991 to the end of September 1996. On October 1, 1996, it was raised to $4.75 an hour, and on

[34] See, also, Carlton (1983). They have been demonstrated to be quite comprehensive (Carlson, 1995), but also have a number of disadvantages. Like the QCEW, they do *not* observe the number of *new* employees or new job postings.

[35] Azar et al. (2020) estimate local HHI for retail occupations using online job posting data from 2010 onward.

[36] These codes broadly encompass the retail sector, though SIC 58 observes "Eating and Drinking Places".

September 1, 1997 was raised again to \$5.15/hour, where it remained through 2005. These two increases cumulatively represented a 21.2% increase in 11 months. For the 'minimum wage increase' treatment I restrict the sample as described in Section 4 to only those counties where the minimum wage tracked the federal minimum wage both before and after this increase, meaning the increase was binding in all counties in this subsample. I further restrict the treated counties to be those which had a Supercenter prior to the increase, and donor pool counties to have had no Supercenter throughout the entire period. This yields 25 donor pool counties and 182–188 treated counties for this treatment, depending on the outcome.

6 Results and Discussion

6.1 Impact of Supercenter Entry

6.1.1 Labor market concentration

I first look at the effect of Supercenter entry on local labor market concentration, and begin by estimating the effect on retail employment-level concentration, calculated using the Dun & Bradstreet data. Panel A of Table 3 presents the estimates from four synthetic control specifications: results using a limited set of predictor variables are presented in column (1) without the bias-correction applied; and in column (2) *with* the bias-correction applied. Results using the full set of predictor variables are presented in column (3) without the bias-correction applied; and in column (4) *with* the bias-correction applied. The estimated effects in event year 5 range from 14.4% to 20.3%, with a tighter spread between the uncorrected and corrected estimates when using the complete set of predictors (columns (3) and (4)). All three *p*-values for each specification are below 0.04.

Column (4), which uses the complete set of predictors and has the bias-correction applied, presents the estimates from the preferred specification. Retail employment-level HHI increased 20.2% by the fifth year after Supercenter entry, with an RMSPE-ranked *p*-value of 0.005, and Andrews and placebo-variance *p*-values both below 0.0001. Panel A, county retail employment-level HHI can be seen to increase over 15% by the end of the first year after Supercenter entry, growing to 20.2% in the fifth year after entry. Panel A of Figure 5 displays the results of the preferred specification. The thick, blue line shows the estimated average treatment effect (across all treated counties, ATT) of Supercenter entry on retail employment-level

HHI, which has a nice pre-treatment fit then jumped sharply by the end of the first year after Supercenter entry before continuing to grow more slowly through event year five. The dark grey lines show the sample distribution of placebo ATTs, which are all much more tightly clustered around zero than the ATT estimate. The light grey band around the ATT estimate shows the 95% confidence intervals based on the variance of the placebo ATTs (the same approach used to estimate the placebo-variance p-value).

Overall, it's clear that Supercenter entry made local retail employment significantly more concentrated among firms. This matters because the counties which experienced the largest increase in concentration due to Supercenter entry were also the most concentrated by the fifth year after entry—an important determinant of labor market power (Azar et al., 2023). Evidence to this effect can be seen in Panel B of Figure 5, which plots the event-year 5 estimated *effect of Supercenter entry* on retail employment-level HHI (in level terms) in each county against the event-year 5 *level* of the same variable. There is a clear, sharply positive correlation between the two (0.83, with a standard error of 0.036), indicating that the most concentrated counties in event year 5 were broadly those that experienced the largest increases due to Supercenter entry.

I next look at the effect of Supercenter entry on the retail sector's share of overall local employment (a measure of overall local industrial concentration), calculated using the QCEW data. Panel B of Table 3 presents the estimates from the four synthetic control specifications. The estimated effects in event year 5 range from 1.6% to 2%, with a tighter spread again seen between the uncorrected and bias-corrected estimates when using the complete set of predictors (columns (3) and (4)). Using the preferred specification (column (4)), the estimated effect of Supercenter entry was a 2% increase in retail's share of local employment by the fifth year after entry. The RMSPE p-value is 0.088, and the other two p-values are ≤ 0.0001. Panel C of Figure 5 displays the results of the preferred specification, again showing an excellent pre-treatment fit with a sharp and significant increase in concentration in the year of Supercenter entry that persisted through event year 5.

Panel D of Figure 5 shows a significant correlation between the magnitude of the local effect of Supercenter entry on retail employment-level HHI and the magnitude of the local effect on retail's share of local employment (0.013, standard error of 0.005), indicating that those counties which saw the largest increases in retail employment-level concentration due to Supercenter entry were, on average, those counties that disproportionately saw local employment become more concentrated in the retail sector.

These pieces of evidence together indicate that Supercenter entry sowed the seeds necessary for Walmart to exercise local labor market power, by becoming a dominant player in now-concentrated retail and aggregate local labor markets—that is, by gaining monopsony power. In the next section, I explore whether and how the firm exercised that power, and how it impacted workers.

6.1.2 Labor market outcomes

In Section 6.1.1, above, I present evidence that Supercenter entry caused local aggregate employment to become significantly more concentrated within the hands of a few employers in the local retail sector—which plausibly means Walmart. That is, Supercenter entry allowed Walmart to gain local monopsony power. I here consider whether and how the firm exercised that power, and how it impacted local workers.

I begin by taking a broad look at the impact of Supercenter entry on aggregate employment and earnings. I then explore more disaggregated industrial groupings, looking at the retail sector and the aggregate non-retail sector, and then specifically at goods-producing industries and non-retail services-providing industries. Finally, I consider the impact of Supercenter entry on labor force activity. For parsimony, I focus on the estimates from my preferred specification—that is, using the complete set of predictor variables and applying the bias-correction for predictor-variable pairwise matching discrepancies.

A. Aggregate effects

Panel A of Figure 6 shows the estimated average effect of Supercenter entry on aggregate local employment. There is an excellent pre-treatment fit, and no effect in the year of Supercenter entry, but a downward trend begins in the year following Supercenter entry, quickly becoming more negative than any of the placebo ATTs. Looking at Panel A, column (4) of Table 4 we can see that, five years after Supercenter entry, the estimated effect on aggregate local employment is -3.1%. This is a very large and statistically significant estimated effect (RMSPE p-value = 0.019, Andrews and placebo-variance p-values both < 0.0001). This pattern holds even in the uncorrected estimates in column (3), though the magnitude of the estimate is somewhat smaller at -1.8%.

Panel C of Figure 6 shows the estimated effect on aggregate total earnings. The pre-treatment fit is slightly above most of the placebo ATTs, though still close to zero. Consistent with such a large decrease in aggregate employment, we see a sharp decline in earnings: five years after Supercenter entry, the estimated effect on aggregate total earnings is -4.3% (Panel B, column (4) of Table 4). The Andrews and placebo-variance p-values are both < 0.0001, but the RMSPE p-value is 0.27, which creates some uncertainty about the significance of this estimate. This uncertainty is underscored by the uncorrected estimates in column (3), with aa smaller estimated effect of -1.6%, and two of three p-values *geq* 0.2. As I show below, this uncertainty arises due to heterogeneity across industries, and is resolved by disaggregating the data. Still, this point estimate of the overall impact on earnings is larger than the largest estimated average benefit to consumers (as a share of income) resulting from Walmart's lower prices (Hausman and Leibtag, 2007).

With a large negative effect on aggregate employment but an even larger negative effect on aggregate earnings, we could expect a smaller negative impact on average earnings. This is exactly what we see in Panel E of Figure 6. As with total earnings, the pre-treatment fit is slightly above most of the placebo ATTs, though even closer to zero. We see a sharp decline in earnings by the fifth year after Supercenter entry: the estimated effect on aggregate average earnings (earnings per worker) is -3.2% (Panel C, column (4) of Table 4). Nearly identically with the estimates for aggregate earnings, the Andrews and placebo-variance p-values are both < 0.0001 but the RMSPE p-value is 0.27. Again, as I show below the resulting uncertainty surrounding the significance of this estimated effect is the result of heterogeneity across industries.

B. Retail sector effects

I next explore where these large, negative aggregate effects are being realized. I begin by looking at the effects on the retail sector. Panel B of Figure 6 shows the average impact on retail employment. The pre-treatment fit is very good. As expected, given the number of employees rquired to staff a new Supercenter, retail employment immediately jumps nearly 2% in the year of Supercenter entry. This jump in event year 0 is significant according to all three p-values. For the preferred specification, the effect then increases to 2.4% in event year 2 before declining to 1.9% in the fifth year after entry, with the RMSPE p-value growing to 0.12 in event year 3, and to 0.16 by event year 5. The decline from peak is even larger in the uncorrected estimates in column (3), with an event-year 5 estimated effect of 1.1% and two of three p-values ≥ 0.19. This leaves some uncertainty about whether the estimated increase in retail employment following Supercenter entry remains statistically significant, but it is clear that the negative effect on aggregate employment is not being driven by patterns in the retail sector. These results suggest that Supercenter entry constitutes a large increase in retail labor demand, as the Supercenters staff up, which begins to abate after a few years—

perhaps as incumbent retailers shed staff.

Consistent with this pattern of retail employment effects, Panel D of Figure 6 and Panel E of Table 4 show that, faollowing Supercenter entry, retail total earnings increased, peaking in event year 2. By event year 5 the effect size was still 4.4%, with all p-values below 0.07. The uncorrected estimates also peaked in event year 2 before declining to 0.7% in event year 5, with an RMSPE-ranked p-value of 0.08 and a placebo variance p-value of 0.51, suggesting the large positive estimated effect on retail total earnings is being driven by the bias correction. However, the patterns for retail per-worker earnings (Panel F of Figure 6 and Panel F of Table 4) suggest average retail earnings did not increase. The pre-treatment fit is okay but more positive than all placebo ATTs. Average earnings then appear to decrease in the year of Supercenter entry, and slowly continue downward to -1.4% by event year 5. This negative estimate is likely a noisy zero, as while the Andrews and placebo-variance p-values are both ≤ 0.0002, the RMSPE p-value is 0.83. The uncorrected estimates in column (3) also suggest there was likely no effect on average retail earnings. Still, I can confidently reject any positive effect of Supercenter entry on average retail earnings despite the large increase in retail labor demand, and it is certainly possible that the minimum wage was all that prevented average earnings from a signficant decline following Supercenter entry. Regardless, these retail-sector results show that the average Supercenter was able, by the fifth year after entry (and given 70% annual turnover), to hire workers equivalent to 11% of average pre-entry county aggregate employment and 88% of pre-entry retail employment without any increase at all in average retail earnings. Unless labor supply to the retail sector was perfectly elastic, this means Walmart was able to set wages below the competitive level and still attract enough workers—that is, Supercenters exercised monopsony power in the retail labor market.

C. Non-retail sector effects

Given these different patterns in aggregate and in the retail sector, I next look at the non-retail aggregate—that is, all sectors excluding retail. The results, shown in Panels A, C, and D of Figure 7 show the same patterns as for the full aggregate, but now the effects on total earnings and average earnings are substantially larger and statistically significant. Panels A, B, and C of Table 5 present the event-year 5 estimated effects: -3.2%, -6.3% and -10% for non-retail employment, total earnings, and average earnings, respectively. All three p-values for each outcome indicate these large negative effects are statistically significant, with the largest being the RMSPE-ranked p-value of 0.08 on the total earnings estimate (column (4)). Even in the uncorrected estimates, the patterns are largely the same.

Panels B, D, and F of Figure 7 show significant negative correlations between the magnitude of the local effect of Supercenter entry on retail employment-level HHI and the magnitude of the local effects on (i) non-retail employment (-0.01, standard error of 0.005), (ii) non-retail total earnings (-0.02, standard error of 0.01), and (ii) non-retail average earnings (-0.037, standard error of 0.016), indicating that those counties with the largest increases in retail employment-level concentration due to Supercenter entry were, on average, those counties that disproportionately saw non-retail employment and earnings fall. That is, the labor markets where the non-retail sector was most negatively affected by Supercenter entry were, on average, those places where Walmart had the greatest scope to exercise monopsony power. These patterns might be unexpected if we imagined that the impact of Supercenter entry is limited to the local retail sector. I therefore next further explore where these effects are being realized.

D. Goods-producing and non-retail services-providing sectors

While confidentiality concerns have resulted in the QCEW data being suppressed in many counties in certain quarters for smaller NAICS Supersectors, the data do observe groupings of goods-producing and of service-providing sectors relatively free of suppressions. Combined with the retail sector observations (which are also relatively free of suppressions), this permits me to consider the impact of Supercenter entry on goods-producing and on non-retail services-providing industrial subaggregates. I note that we should not expect the estimates in these industry subaggregates to average out to the estimates in the non-retail aggregate, for two reasons. First, due to a few suppressions and due to outcome-specific sample trimming to ensure the treated county outcome values are in the support of the donor pool county outcome values, the samples are not exactly identical. Second, and more importantly, as the treated counties are heterogeneous in the goods-producing sector share of non-retail employment, the individual-county synthetic controls will be separately matched on the values for each industrial subaggregate. This yields a more precise match for each industry subaggregate, but the synthetic controls will be different compared to each other and to the non-retail aggregate. These important details notwithstanding, the average effects in the two subaggregates are remarkably close to those in the non-retail aggregate.

Panels A, C, and E of Figure 8, and Panels D, E, and F of Table 5 present the results for the goods-producing sector. It is immediately clear that this is where the bulk of the action is coming from for the more-aggregated negative effects on employment and total earnings. Five years on, the estimated effect of Supercenter entry on employment in goods-producing industries is -5.6% (RMSPE-ranked p-value of 0.02, Andrews and placebo-variance $p < 0.0001$), and the estimated effect on total earnings is -8.8% (RMSPE-ranked p-value of 0.04, Andrews and placebo-variance $p < 0.0001$). The estimated effect on average earnings is -2.8% (RMSPE-ranked p-value of 0.13, Andrews and placebo-variance $p < 0.0001$), which I interpret as at least borderline-significant.

Panel B of Figure 8, and Panel D of Table A2 present the results for non-retail services-producing employment. Despite the apparent dip in employment in event years 2 and 3, the RMPSE-ranked p-value does drop below 0.44. Clearly, in event year 5, the estimated effect of -0.3% is not distinguishable from zero. That is, the entirety of the negative effect of Supercenter entry on aggregate (and non-retail aggregate) employment is realized in goods-producing industries.

Panel D of Figure 8, and Panel E of Table A2 present the results for non-retail services-producing total earnings. As with the employment estimates, there is a dip beginning in event year 2, reaching -3.5% in event year 3. The RMSPE-ranked p-value is 0.14 (Andrews and placebo-variance $p \leq 0.003$), suggesting this estimated effect may be borderline-significant (the uncorrected estimated effect in event year 3 is -2.8%, with an RMSPE-ranked p of 0.07). However, by event year 5 the estimate from the preferred specification is -2.3% with a placebo-variance and RMSPE p both ≥ 0.2, indicating there is no longer any significant effect. As with the employment estimates, this suggests that the entirety of the negative effect of Supercenter entry on aggregate (and non-retail aggregate) total earnings is realized in goods-producing industries. However, the average earnings estimates suggest this may not be entirely correct.

Specifically, Panel F of Figure 8, and Panel F of Table A2 present the results for non-retail services-producing average earnings. Beginning in event year 2, there is a large negative estimated effect with an RSMPE-ranked p of 0.06 (Andrews and placebo-variance p both < 0.0001). This decline continues through event year 5, reaching -13.3% with an RMSPE-ranked p of 0.06 (Andrews and placebo-variance p both < 0.0001). This suggests that the non-significant estimated decline in non-retail services-producing total earnings may have simply lacked precision that is gained when put in per-worker terms. In fact, given the small, borderline-significant negative estimated effect on goods-producing average earnings, the magnitude of the significant negative effect on non-retail aggregate average earnings makes the most sense when this large negative estimated effect on non-retail services-providing average earnings is considered (though, again, the non-retail aggregate results need not necessarily average the results of the two non-retail industry subaggregates for the reasons discussed above).

These results make clear that, while some of the decline in average earnings may have come from non-retail services-providing industries, it is workers in the goods-producing sector that bore the lion's share of the negative impact of Supercenter entry. The story that best explains this—consistent with the discussion in Section 3—is a two-fold one involving Walmart's industry-changing supply chain and its concomitant power to set terms and prices among its suppliers. First, Walmart famously squeezed its own local suppliers to cut costs and become more efficient. Over time, this plausibly led those suppliers to reduce wages and employment among their own workforces (e.g. Bloom and Perry (2001); Wilmers (2018)). Second, and perhaps more importantly, Supercenter entry constituted a large, negative demand shock for the output of local manufacturers who supplied incumbent local retailers. As Supercenters captured local retail market share, Walmart's national and international suppliers would have displaced (often less-productive) local producers as manufacturers of locally-consumed goods. This negative output demand shock depressed the labor demand of local producers, driving down employment and earnings in that sector, giving Supercenters an even larger share of local demand for less-skilled labor, and exacerbating negative local multiplier effects.

E. Labor force activity

With such substantial reductions in employment, we should also expect to see increases in unemployment, reductions in the labor force, or both. Figure 9, and Table 6 present the estimated effects of Supercenter entry on these outcomes. Panel A in each presents the estimated effect on (percent change in) the number of unemployed individuals (unemployment count), while Panel B presents the estimated effect on the unemployment rate. Both display some pre-trend noise, and the unemployment rate pre-trend is not zero. Both then show sharp increases by the second year after Supercenter entry, with the unemployment count 7.3% higher by the fifth year after entry (RMSPE $p = 0.06$, Andrews and placebo-variance p each < 0.0001), while estimated effect on the unemployment rate is a 5.8% increase. The RMSPE p for the unemployment rate estimate is over 0.5 due to the poor pre-treatment fit, but it is also confounded by changes in the denominator as seen in Panels C and D. Between the non-zero pre-trend on the unemployment rate, and potential confounding changes in the size of the labor force which could affect the denominator of that variable, the estimated effect on employment count is the cleaner of the two and shows that Supercenter entry caused a sharp and statistically significant increase in unemployment.

For both the labor force count (Panel C) and the labor force participation rate (LFP, Panel D), we see no movement through the third year after Supercenter entry before both decline around 1.3% by the fifth year after entry. The Andrews and placebo-variance p-values are < 0.0001 for both estimates, but the RMPSE-ranked p is much larger (0.69 and 0.24, respectively). However, this is due to slightly-worse-than-placebos (but still good) pre-treatment fit and the fact that the downward movement only began in event year 4. That is, the conservative construction of the RMSPE p-values inflates these p-values. If only the event years in which we see movement (4 and 5) are considered, then even with a two-sided test the resulting RMSPE p-values are much closer to indicating statistical significance at conventional levels (e.g. $p = 0.11$ for LFP, suggesting (along with the other p-values) that these negative estimated effects are at least borderline significant. At the very least, we can be highly confident that labor force participation did not increase following Supercenter entry.

Summarizing these results, Supercenter entry caused local unemployment to increase and may have also decreased labor force participation.

F. Additional robustness checks

I have thus far focused on the results from stacked synthetic control estimators. These estimates are free of the potential bias that could result from a two-way fixed effects (TWFE) estimator given staggered treatment adoption and (as is evident in the plots of the stacked synthetic control estimates) heterogeneous treatment effects across event time, which I have discussed at length. However, several non-TWFE estimators that retain a DiD research design have been developed which may also be unbiased in this environment. Here, I first consider estimated effects for a few key variables using the dynamic DiD Callaway and Sant'Anna (CS) estimator. I then consider the results from estimating three dynamic (event study) TWFE models. The CS estimates have the same sign and generally a similar magnitude to the stacked synthetic control estimates, though are less-precisely estimated. The TWFE estimates are often smaller in magnitude and less precise than the stacked synthetic control and CS estimates, and are sometimes different-signed even when the stacked synthetic control and CS estimates are statistically significant.

The Callaway and Sant'Anna (CS) estimates are in 100×log points, estimated using the same control variables as the stacked synthetic control's predictor variables, and the asymptotic standard errors are clustered by commuting zone. The estimated effects of Supercenter entry on non-retail and retail employment, total earnings, and per-worker earnings are presented in Appendix Figure A6. Together with the estimated aggregate effects (not visualized for parsimony), the event-year 5 estimates are presented in Appendix Table A3. The patterns and outcomes are same-signed and very similar in magnitude to the stacked synthetic control results, and the estimated effects on the same outcomes are statistically significant at conventional levels, though in general the results are somewhat less-precisely estimated.

Finally I present estimates using 3 versions of the general TWFE model in Equation 1. I drop the dummies for event years < 0, and estimate the effects (i) with no control counties (a "difference" research design) and without binning the (excluded) dummies for event years earlier than -5 or later than 5; (ii) the same as (i) but *with* control counties (a difference-in-differences design); and (iii) the same as (ii) but *with* a dummy for event years < -5 and another for event years > 5.[37]. The outcome variables are logged, and the estimates are in log points.

Appendix Table A11 presents the estimated effects of Supercenter entry on non-retail and retail employment, total earnings, and per-worker earnings. Depending on the model, various event year estimates of the non-retail employment effect are either positive or negative, and are not significant. The retail per-worker earnings estimates generally have an opposite sign to the stacked synthetic control and CS models, and are never statistically significant at conventional levels. In models (ii) and (iii) the retail employment estimates have a similar pattern to the stacked synthetic control and CS estimates; the event year 5 estimated effects on non-retail earnings are significant at the 10 percent level; and the estimates for non-retail per-worker earnings are larger than the stacked synthetic control and CS estimates, and significant at the 1% level.

In many cases, the TWFE estimates appear to be biased and often even wrong-signed due to the staggered treatment adoption and heterogeneous treatment effects, just as the new DiD literature cautions.

6.2 Impact of the 1996/97 Federal Minimum Wage Increases on Counties with Supercenters

Having established the results of the previous section, I next estimate the employment and earnings effects of the 1996/97 federal minimum wage increases on counties which already had Supercenters. The sample

[37] In results not reported, I also estimate the same models but with dummies for event-years -2 through -5 added. They are very similar to those presented, and are available upon request

of counties is restricted to those in which the federal minimum wage was the prevailing minimum wage both before and after the 1996/97 increase, and of these the treated sample are those counties which already had a Supercenter prior to the increase, while treatment is the increase itself. That is, treatment is the interaction of the minimum wage increase and the presence of a Supercenter, while the untreated counties were similarly exposed to the minimum wage increase but did not have a Supercenter. The estimated effects can then be interpreted as the impact of a minimum wage increase in labor markets that the above analysis indicated were likely affected by monopsony power, relative to the impact of a minimum wage increase in more competitive labor markets. This is effectively a test of whether monopsony power is indeed a mechanism significantly responsible for the results in the previous section. If Supercenters are indeed monpsonists, then we should expect to see this increase in the minimum wage cause employment to increase in places with a Supercenter–a result that would be inconsistent with competitive labor markets or other stories.

6.2.1 Stacked synthetic control estimates

Panels A and B of Figure 10 plot the estimated effects on aggregate and retail employment. Both aggregate and retail employment jumped sharply in 1997. As the first of the two-step increase in the minimum wage came into effect in October 1996, and the second came into effect in September 1997, this timing of the effect makes sense.

Appendix Table A4 quantifies the aggregate employment estimates by year. In 1997 agggregate employment significantly increased by 4.7%. By 2001 the estimated effect on aggregate employment is 6.5%, with an RMSPE-ranked p-value of 0.001 (Andrews and placebo-variance $p < 0.0001$).

Appendix Table A5 quantifies the retail employment estimates by year. In 1997 retail employment is a statistically significant 1.8% higher, and by 2001 it is 11% higher (RMSPE-ranked p-value of 0.001, Andrews and placebo-variance $p < 0.0001$).

While the increase in aggregate employment is partially due to the sharp increase in retail employment, there are also gain in non-retail employment (not shown), which increased faster than retail employment in 1997 before slightly falling back to be about 2.8% higher in 2001. If these labor markets where Supercenters operated were competitive then we should not see employment gains following a minimum wage increase. Even null employment effects could be consistent with competitive labor markets if either the new minimum wage was not binding or if employers were able to fully pass the higher labor costs onto customers. However, a clear increase in employment is only consistent with low-wage labor markets characterized by employer power. These results therefore affirm the interpretation of the Supercenter entry effects being the result of employer (monopsony) power.

I additionally consider the impact of this minimum wage increase on earnings in counties which already had a Supercenter. Panel C of Figure 10 plots the estimated effect on aggregate earnings, and the effects are quantified by year in Appendix Table A6. In 1997—when the full increase was realized—total earnings are a statistically significant 2% higher (RMSPE p = 0.002). Total earnings then begins to decline. The estimates with the complete set of predictors (column (4), and pictured in Figure 10) show that by 2001 total earnings were 2.6% lower than before the minimum wage increase (RMPSE $p = 0.04$, Andrews $p < 0.0001$, placebo-variance $p = 0.03$). However, the remaining specifications generally do not show a statistically significant negative effect by 2001, and the point estimate for the bias-corrected specification without every predictor is *positive*, though almost certainly a noisy zero (column (2)). This suggests that this negative estimated effect may using the complete set of predictors may, in fact, be a noisy zero.

The estimated effects on retail earnings are plotted in Panel D of Figure 10, and Appendix Table A7 quantifies the effects by year. As with total earnings, in 1997 retail earnings significantly higher (2.7%, RMSPE p = 0.001). Unlike total earnings, retail earnings then continue to climb, reaching 10.6% higher by 2001 (RMSPE $p = 0.001$, Andrews and placebo-variance $p < 0.0001$). This increase is also present in the results from the specification without every predicor (column (2)). This is what we would expect: an increase in the minimum wage increased retail earnings in counties that were characterized by monopsony power exercised by a large retailer.

Finally, I consider the impact on per-worker earnings. It is worth reiterating that this metric is a proxy for average wages, but is imperfect in that it does not account for hours. Panel E of Figure 10 plots the estimated effect on aggregate per-worker earnings, with the effects quantified by year in Appendix Table A8. In 1997 average earnings are an estimated 0.44% lower (RMSPE p = 0.09, Andrews $p < 0.0001$, but placebo-variance p = 0.13). They continue to decline somewhat, reaching 3.2% lower by 2001 (RMSPE p = 0.001, Andrews and placebo-variance $p < 0.0001$), though the estimated effect from the specification without the complete set of predictors is only -0.04 and likely not statistically significant (column (2)), leaving some ambgiguity about whether this decrease was indeed so large. Still, a decrease in aggregate average earnings is consistent with a monopsony power story: if a minimum wage increase resulted in a large boost in employment right around the new minimum wage, then aggregate average earnings in the labor market would slightly fall. This would be especially true if many of those new low-wage jobs offered fewer hours than the pre-treatment average.[38]

The impact on retail per-worker earnings are plotted in Panel F of Figure 10, and the results are quantified by year in Appendix Table A9. Average retail earnings were significantly higher in 1997 (0.6%, RMSPE p = 0.002), and were 4.2% higher in 1998 (RMSPE p = 0.001), consistent with an immediate earnings boost for all workers who had been earning below the new minimum wage. However, they then begin to decrease, and by 2001 are 1.2% lower than before the minimum wage increase (RSMPE p = 0.001, Andrews $p <$ 0.0001, placebo-variance p = 0.02). This is plausible if the large increase in retail employment involved on average fewer hours (likely with new low-wage positions). However, it is also worth noting that this result is not fully supported by the specification without the complete set of predictors (column (2)), which shows a non-significant increase of 0.7%.

6.2.2 Callaway and Sant'Anna estimates

I also estimate the effects of the federal minimum wage increase on counties that already had Supercenters using the Callaway and Sant'Anna DiD estimator. The results are presented in Appendix Figure A7 and quantified in Appendix Table A10.[39] As with the stacked synthetic control estimator, I estimate the effect aggregate and retail employment, total earnings, and average earnings. As with the estimated effect of Supercenter entry, the patterns results are quite similar to the stacked synthetic control estimates though less-precisely estimated. As with the stacked synthetic control estimates of the impact of the minimum wage on counties with a Supercenter, these aggregate and retail employment estimates both show large gains (8.7% and 10.6%, respectively), though only the estimate on aggregate employment is statistically significant (p-values of 0.07 and 0.38, respectively). Retail total earnings are also estimated to have grown significantly higher (15.2%, with a p-value of 0.02). The point estimate on aggregate total earnings is a noisy 1.1% higher in 2001 (p = 0.87), which offers further reason to be skeptical of the significance of the negative

[38] Again, this is consistent with the practices of Walmart, which offered even its full-time workers around 32 hours per week.
[39] Due to a conformability error, one covariate had to be dropped to estimate the effect on total earnings only.

estimated effect on total earnings using the stacked synthetic control specification with the complete set of predictors. Aggregate per-worker earnings and retail per-worker earnings are both noisy zeros, again consistent with the somewhat ambiguous small estimates from the stacked synthetic control estimates.

Summarizing: the federal minimum wage increase caused significant and robust gains in employment in counties that already had a Supercenter, consistent with Walmart Supercenters exercising monopsony power (and indicating that minimum wages can weaken monopsony power). The minimum wage increased 21% in 11 months, implying an employment elasticity with respect to the minimum wage of around 0.2 in the first full year after the increase, in counties with a Supercenter, both in aggregate and in the retail sector. By the fourth full year after the increase, the aggregate employment elasticity is around 0.3, while the retail employment elasticity is around 0.5. The impacts on earnings are somewhat more ambiguous, though this may be due to new hires working fewer hours, on average (an outcome I cannot check with my data). Still, we see large and statistically significant increases in retail average and total earnings in the first full year after minimum wage increase, which is exactly what we would expect in a monopsonistic labor market. It is worth re-stating that these are estimated effects of the impact of minimum wage increases *when a Supercenter is present*, and they say nothing about the impact of minimum wage increases in labor markets not characterized by monopsony power.

7 Conclusion

The size and ubiquity of Walmart stores, as well as the low wages earned by the firm's sales workers, has long drawn the interest of researchers and policy-makers. Yet prior research into Walmart's impact on local labor markets has suffered from serious methodological disagreements about how to address endogenous entry, yielding significant uncertainty about the effects of these stores. In this paper, I demonstrate that Walmart Supercenters exercise local monopsony power, with widespread, significant, negative impacts on workers.

I find Supercenter entry was responsible for a sharp increase in labor market concentration, boosting retail employment with no increase in average earnings. This indicates that Supercenters were able to set wages low and still hire substantial numbers of workers—a direct form of labor market power. Despite higher retail employment, I find Supercenter entry caused overall county employment to fall over 3%, five years after entry. This decline was concentrated in goods-producing establishments, where employment fell over 5% and total earnings fell nearly 9%. I argue this is the result of Walmart capturing a large share of the local retail market, allowing it to demand cost reductions from the local suppliers with whom it chose to work (another form of monopsony power) while displacing the local suppliers of local incumbent retailers with its own non-local suppliers in a way that was only possible at the time due to its industry-changing supply chain and distribution network. These negative output demand shocks faced by local producers then translated into lower labor demand, reducing employment and earnings in that sector and causing local unemployment to increase, reinforcing Walmart's ability to offer low wages and still attract workers. Finally, I find that the 1996/97 federal minimum wage increases—which were plausibly exogenous to local conditions—resulted in higher aggregate and retail employment in counties which had a Supercenter—results only consistent with labor markets characterized by monopsony power.

I utilize a stacked-in-event-time synthetic control estimator to estimate these effects, crucially constructing the donor pool for each synthetic control from a set of counties where Walmart tried to build a Supercenter but was prevented from doing so by local efforts. This strategy yields estimates that are free of endogeneity bias and do not suffer from the more-recently recognized bias issues which can plague two-way fixed effects estimators when treatment adoption is staggered and treatment effects are heterogeneous.

My results are not consistent with local labor markets remaining competitive after Supercenter entry. Rather, they demonstrate that Supercenters gradually acquired and exercised monopsony power where they operated, depressing local employment and earnings for workers in the wider local economy. Past work has shown that Walmart has higher productivity than its competitors, and that consumers can benefit from its lower prices on groceries. Notwithstanding these positive effects, I estimate the negative effects more-than offset these gains. My results offer evidence to economists and policy-makers that the arrival of large, low-wage employers may precipitate a deterioration in outcomes for local workers. They also indicate that targeted minimum-wage increases may actually improve worker outcomes in such labor markets, and more generally support the idea that the discordant estimates of employment elasticities in the minimum wage literature may, at least partially, be reconciled by accounting for local monopsony power.

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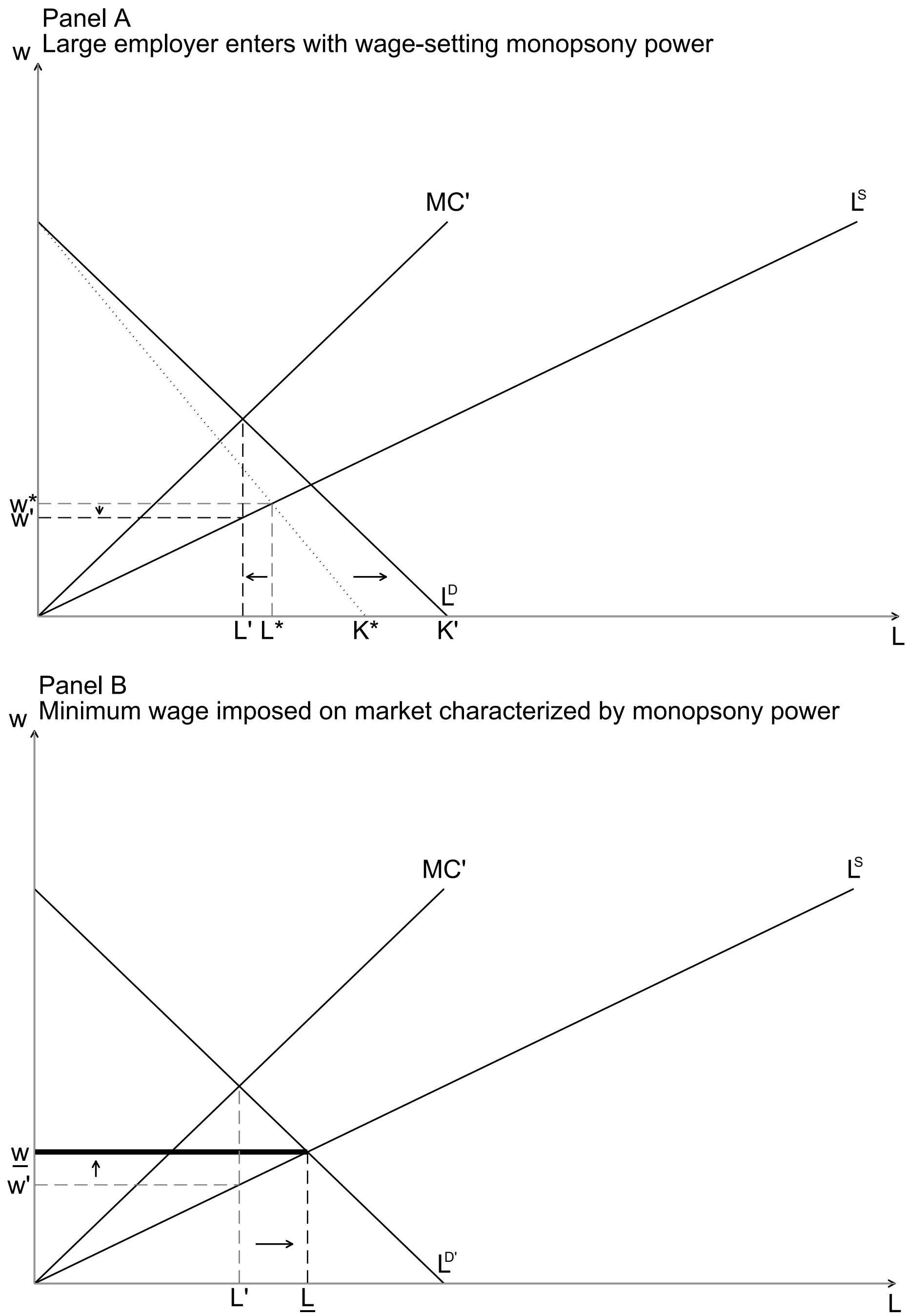
Figures and Tables

Figure 1: Walmart annual net income, employees, and Supercenters

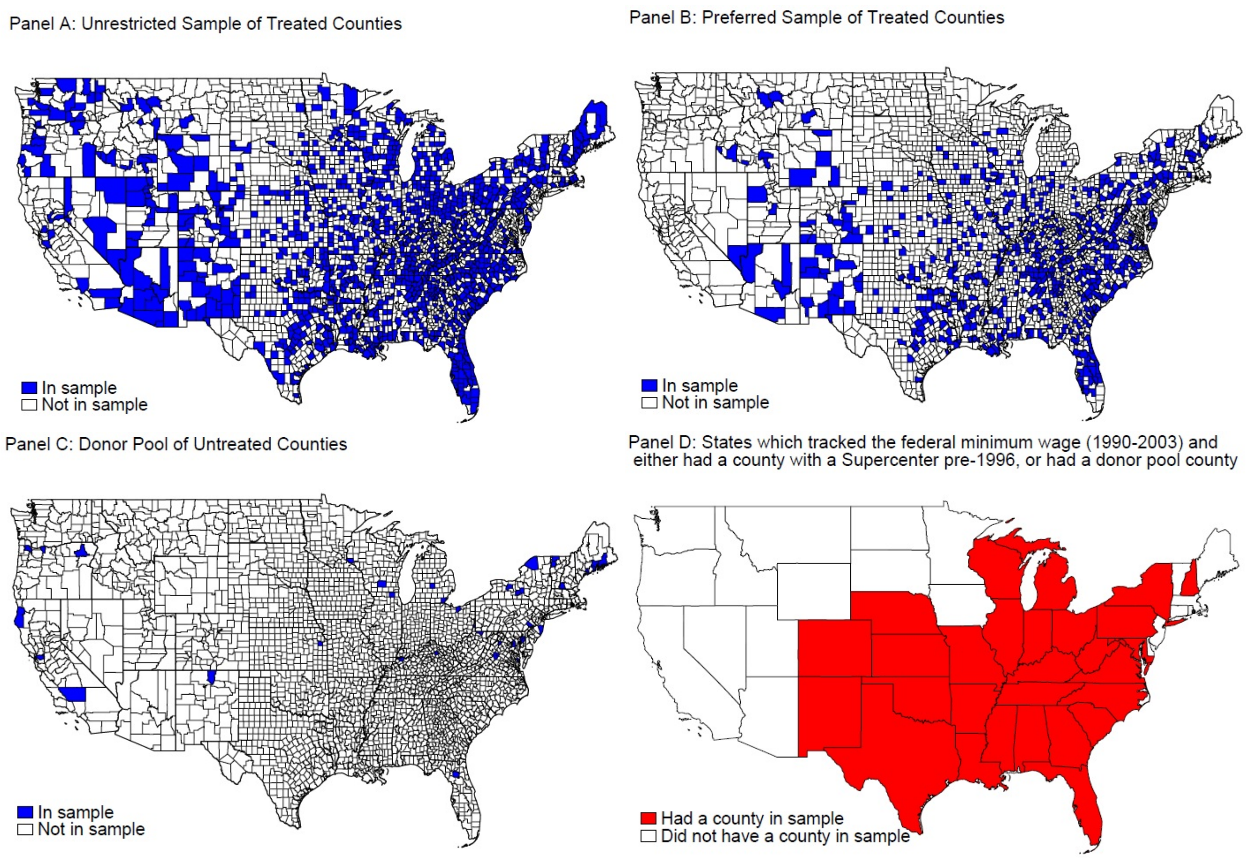
Note: Data from Holmes (2011) and Walmart Annual Reports, 1995-2006. Store numbers as of December 31 of each calendar year for the contiguous U.S., calculated from date provided by Holmes (2011). Employees and net income as of January 31 following each calendar year. Net income in PCEPI deflated, 2017 USD. U.S. employee figures are imputed for 1992, 1993, 1995, and 1996, when only global employee counts and stores by type and country are reported.

Figure 2: Labor market effects of monopsonist entry and a subsequent minimum wage introduction



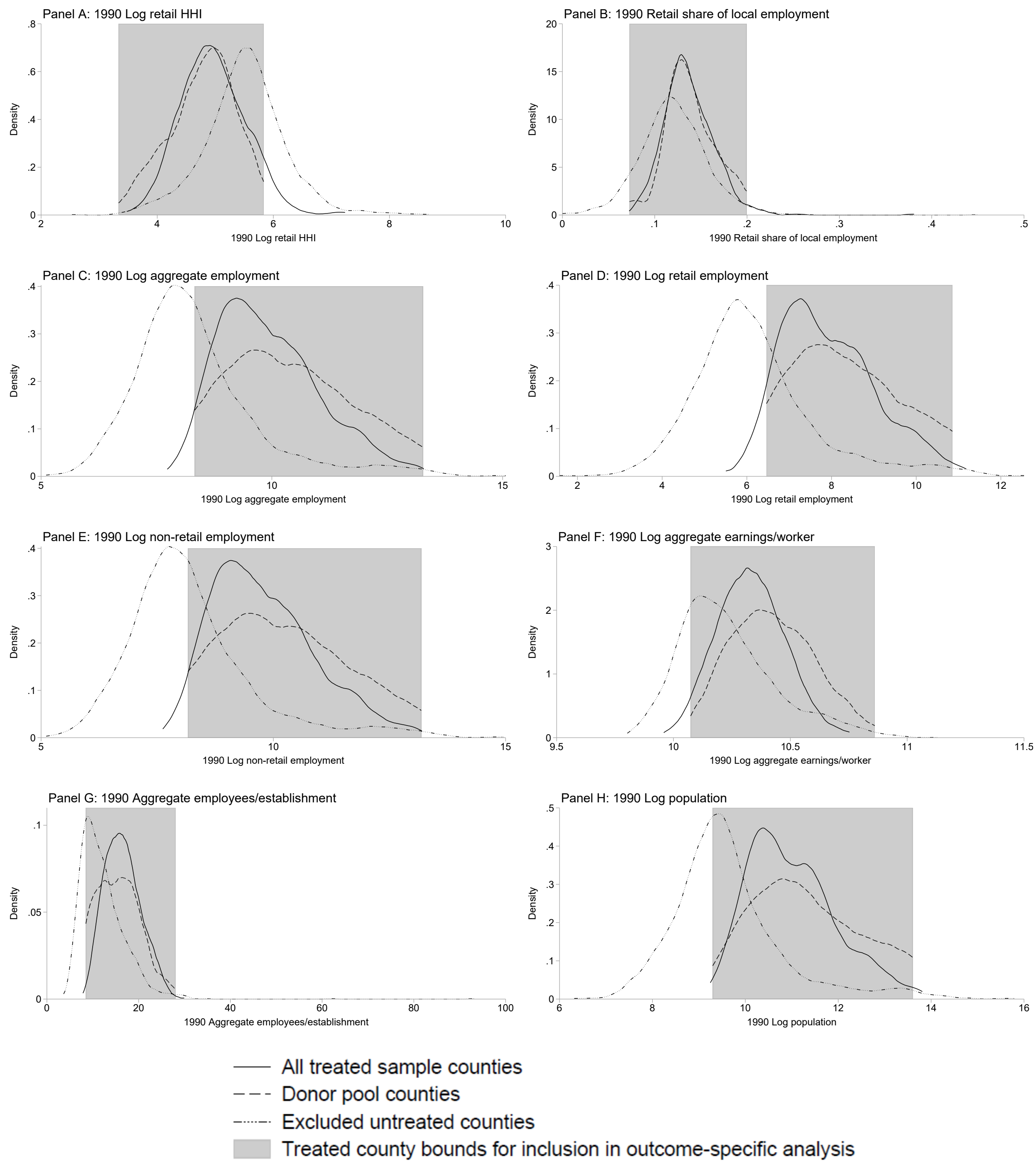
Note: Panel A shows the changes in labor demand, wages and employment when a large firm enters and exercises wage-setting monopsony power. Labor demand increases (L^D to $L^{D'}$, but the monopsonist's marginal cost curve is MC' instead of L^s, leading to lower wages (w^* to w') and employment (L^* to L'). Panel B shows the impact of introducing a binding minimum wage, $\underline{w}$ on this monopsonistic labor market, with the imposed minimum wage equal to the efficient wage in a competitive market. Employment increases from L' up to $\underline{L}$ as the marginal cost curve up to $\underline{L}$ is now $\underline{w}$.

Figure 3: Samples of interest



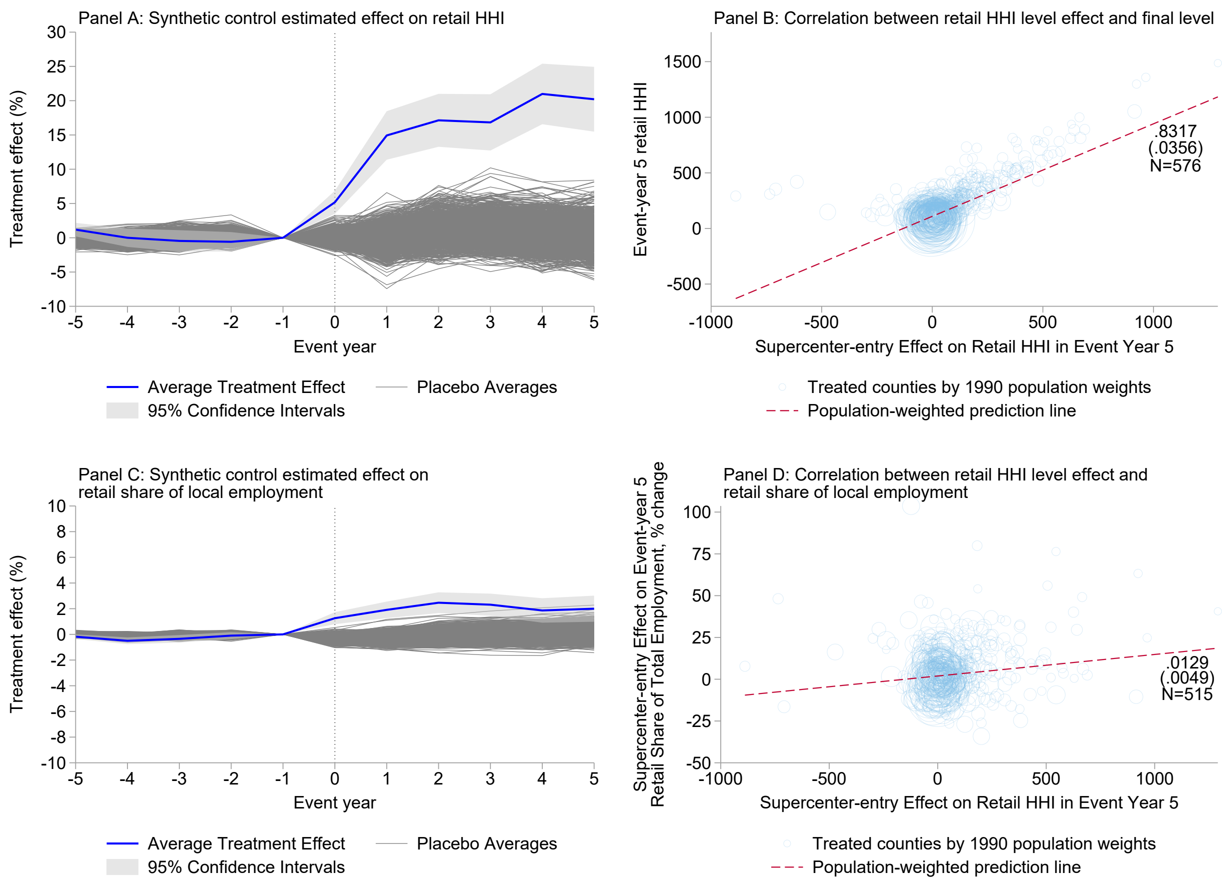
Note: Supercenter entry data from Holmes (2011). I use consistent 1990 county boundaries. I combine Virginia's independent cities with their surrounding counties, and the five New York boroughs into a single unit. Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The unrestricted set of treated counties is shown in Panel A, and includes the 1,162 counties which received their first Walmart Supercenter between 1995 and 2005. The (preferred) sample of treated counties is shown in Panel B, and includes the 505–578 counties which received their first Walmart Supercenter between 1995 and 2000 and which had a 1990 population between 10,000 and 1 million people. The donor pool counties are shown in Panel C, and include the 39 counties in which Walmart attempted to open the county's first Supercenter between 1990 and 2005 but where local political efforts prevented them from doing so. Panel D shows the states which tracked the federal minimum wage over the period 1990-2003 and which either had a county with a Supercenter before 1996 (when the federal minimum wage rose) or had a county in the donor pool.

Figure 4: Density plots of 1990 observations of select variables
Treated sample, donor pool, excluded untreated sample, and outcome-specific bounds for inclusion



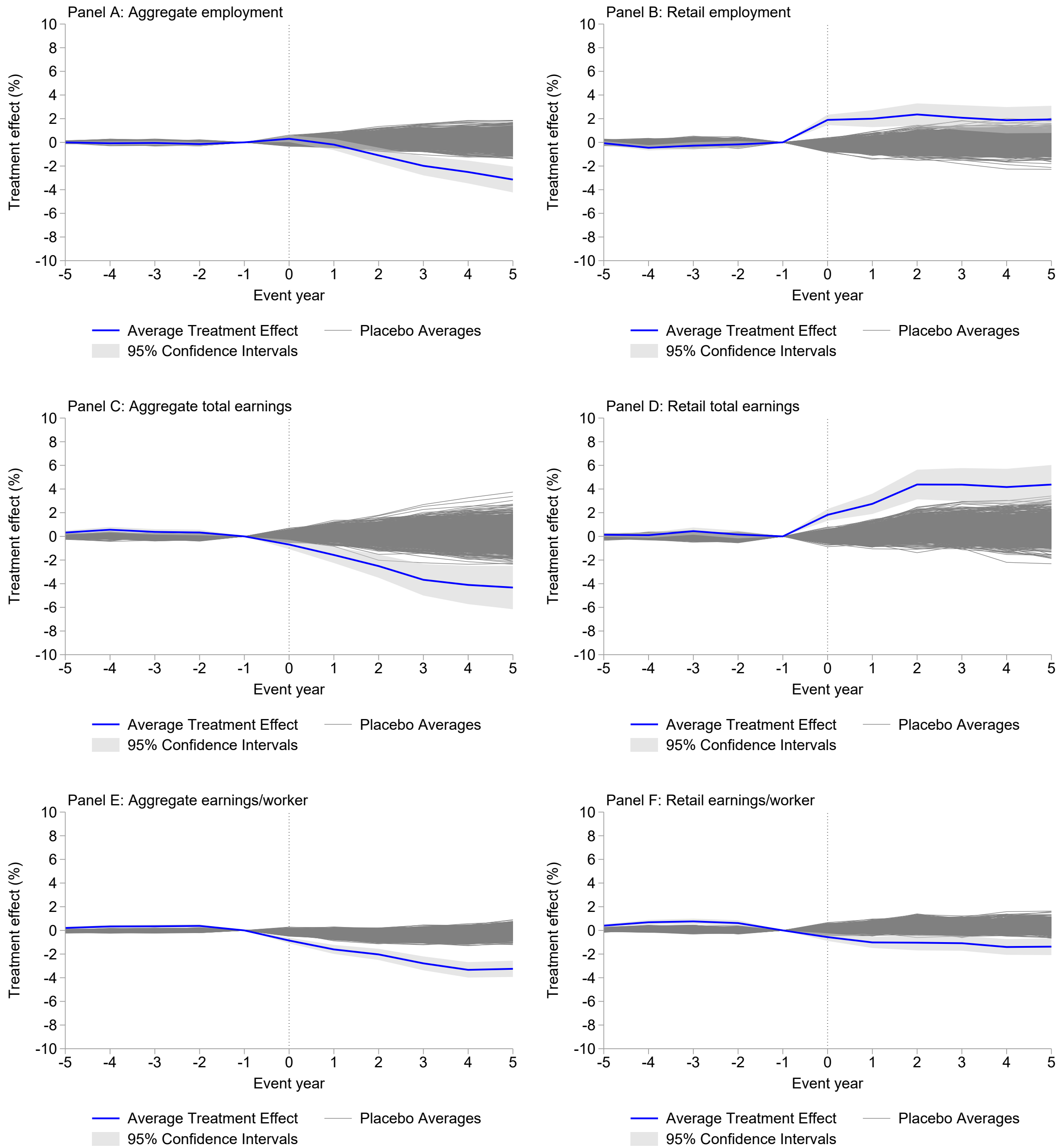
Note: Density plots of 1990 observations of select variables by sample (all 582 included treated counties, the donor pool, and excluded untreated counties). The indicated bounds on the treated sample (which determine inclusion in the outcome-specific analysis) are the minimum and maximum values in the donor pool. This ensures each 1990 observation of a treated county outcome can be reconstructed as a convex combination of a subset of donor pool counties. Densities of retail HHI, aggregate employment, retail employment, non-retail employment, per-worker earnings, and population are logged for ease of visual comparison.

Figure 5: Effects of Supercenter entry on retail employment concentration
Synthetic control estimates and correlations with retail HHI estimates



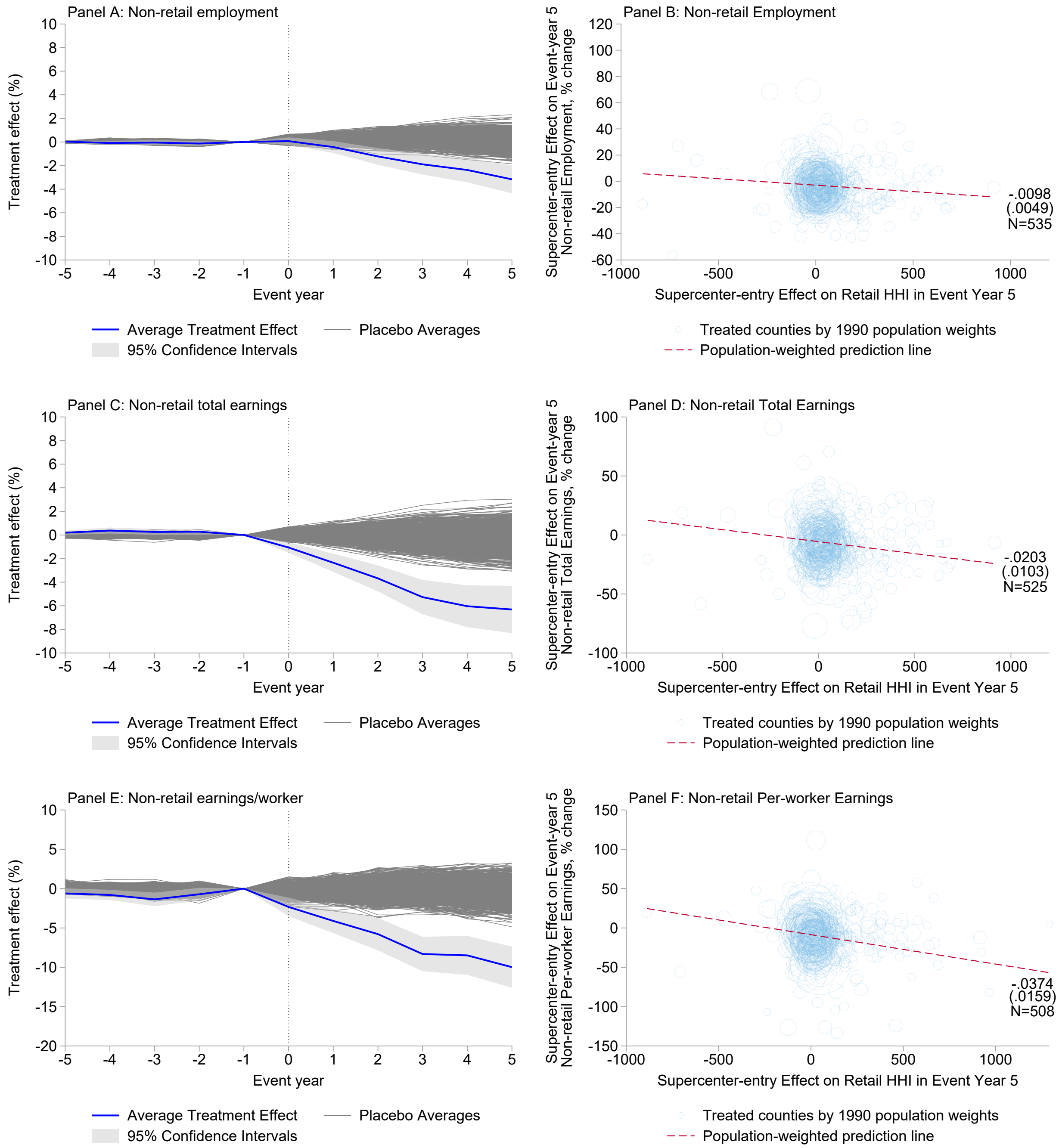
Note: Estimated using HHI calculated from the Dun's Market Indicators data from Dun & Bradstreet (DNB), as well as employment data from the QCEW and LAUS and Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. Panels A and C show the stacked synthetic control estimated percent effect on average retail employment concentration (HHI) and on retail's share of total local employment, respectively. The samples contain 525–576 treated counties and 39 donor pool counties, depending on the outcome variable. The bias-correction procedure is applied. The thick, blue line is the path of average treatment effects. The grey lines are paths of random samples of 1,000 placebo average treatment effects. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the proportion change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control. Panels B and D respectively show the correlation between the estimated effect (in level terms) in event-year five and (B) the corresponding retail HHI level in that event year, and (D) the estimated effect on retail's share of local employment.

Figure 6: Effects of Supercenter entry on aggregate and retail employment and earnings
Synthetic control estimates



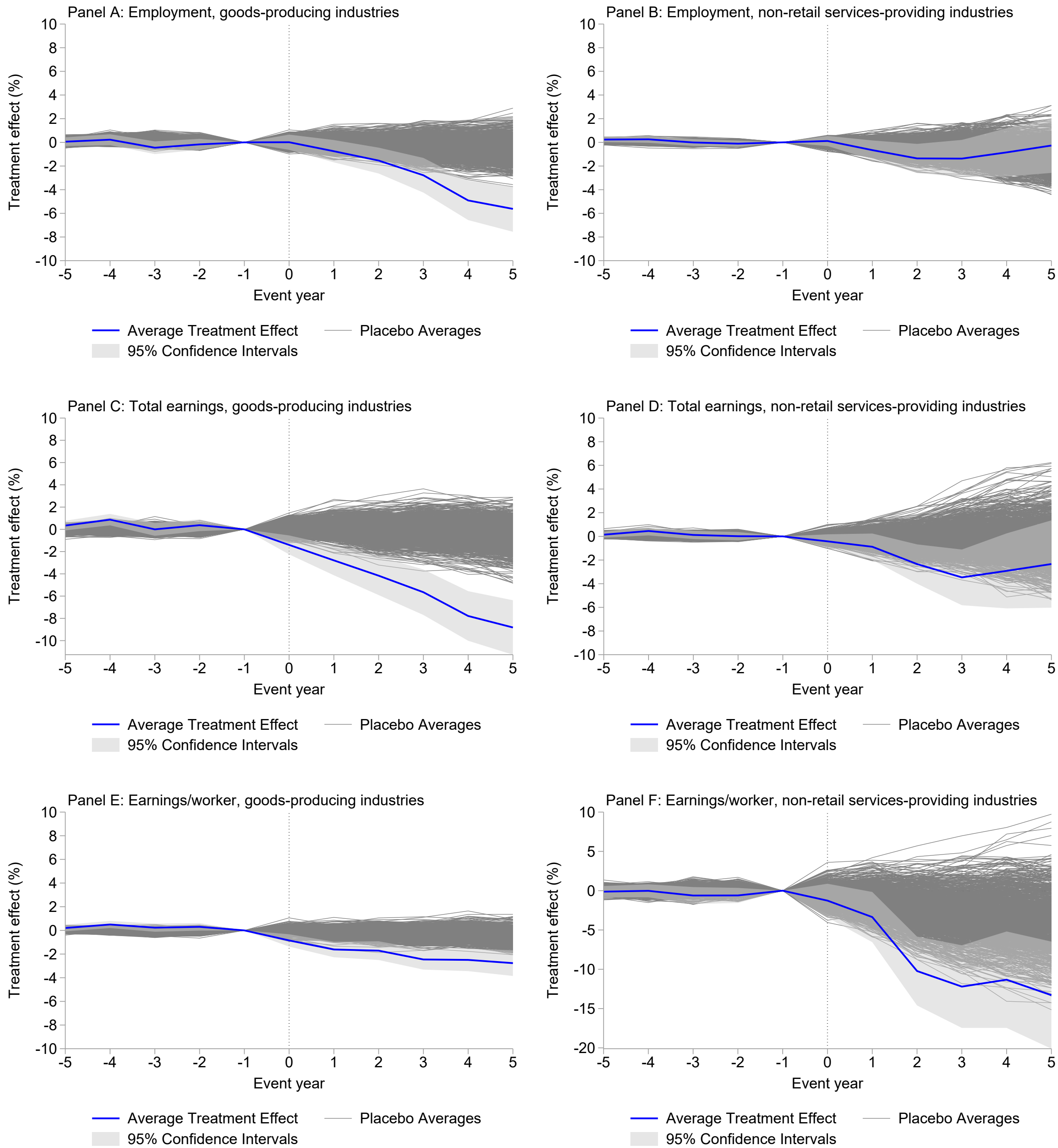
Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, blue line is the path of ATT estimates. The grey lines are paths of random samples of 1,000 placebo ATT estimates. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the proportion change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control. Panels A, C, and E are, respectively, the effects on aggregate employment, aggregate total earnings, and aggregate earnings per worker. Panels B, D, and F show the same respective outcomes for the retail industry only.

Figure 7: Effects of Supercenter entry on non-retail employment and earnings
Synthetic control estimates and correlations with retail HHI estimates



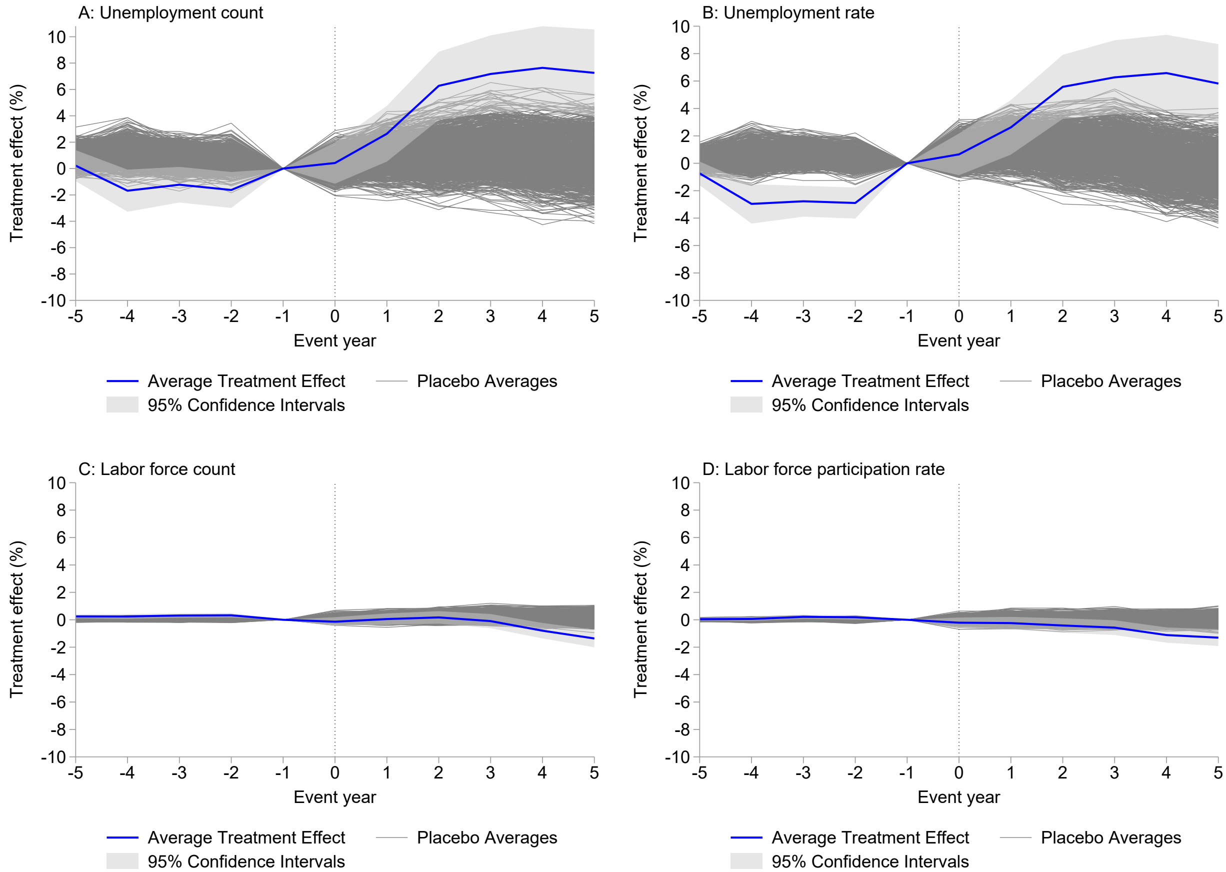
Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, blue line is the path of ATT estimates. The grey lines are paths of random samples of 1,000 placebo ATT estimates. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the proportion change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control. Panels A, C, and E are, respectively, the effects on employment, total earnings, and earnings per worker in non-retail industries. Panels B, D, and F show the correlation between the estimated retail HHI effect (in level terms) in event-year 5 and these event-year 5 estimates for non-retail services-providing industries.

Figure 8: Effects of Supercenter entry on disaggregated non-retail employment and earnings
Synthetic control estimates



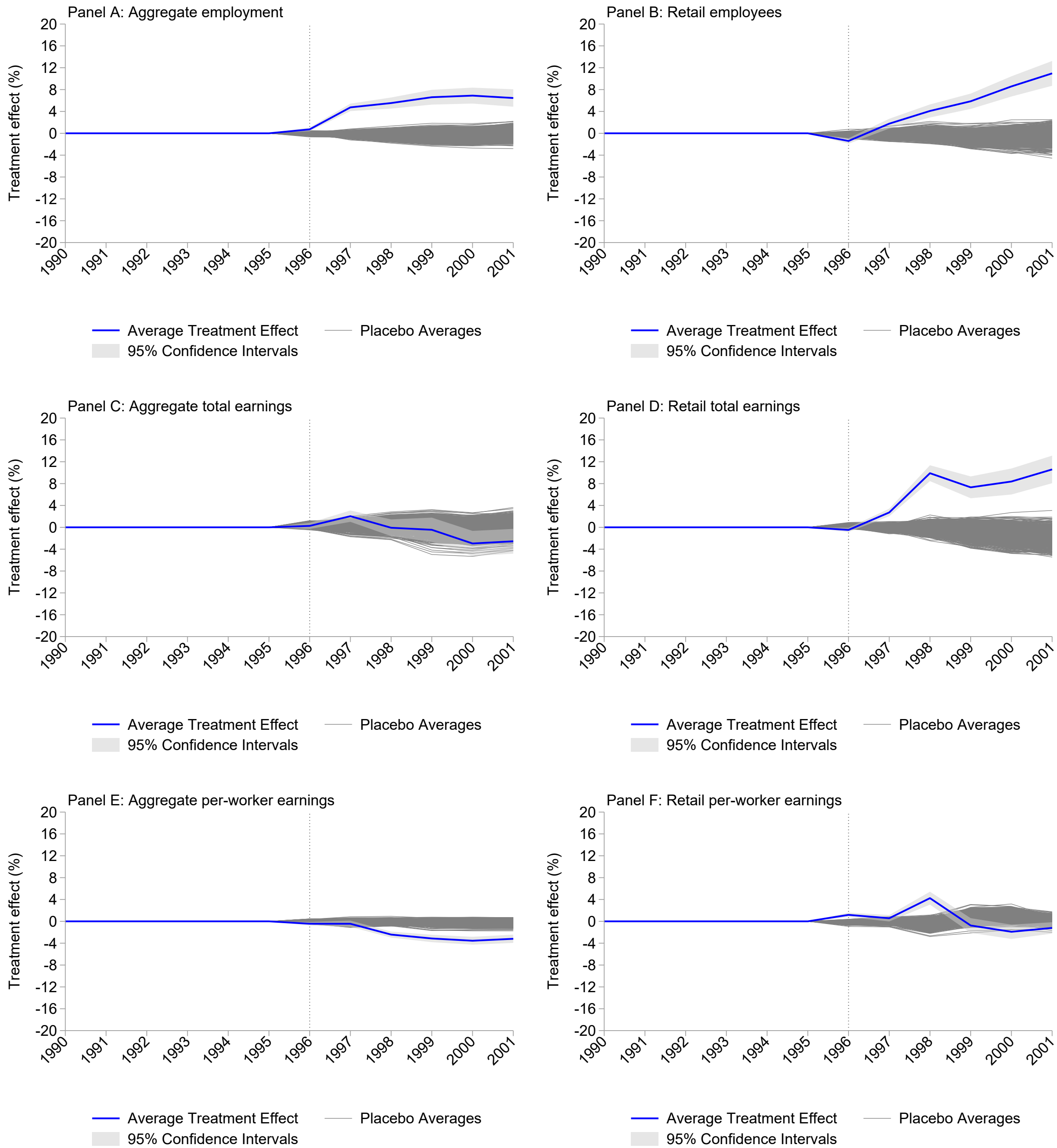
Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, blue line is the path of ATT estimates. The grey lines are paths of random samples of 1,000 placebo ATT estimates. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the proportion change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control. Panels A, C, and E are, respectively, the effects on employment, total earnings, and earnings per worker in goods-producing industries. Panels B, D, and F show the same respective outcomes for non-retail services-providing industries.

Figure 9: Effects of Supercenter entry on labor force activity and EITC receipts
Synthetic control estimates



Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, local receipts of federal EITC transfers from BEA Regional Economic Accounts, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 38–39 donor pool counties. The bias-correction procedure is applied. The thick, blue line is the path of ATT estimates. The grey lines are paths of random samples of 1,000 placebo ATT estimates. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry (meaning the LFP rate and unemployment rate estimates are *percent* changes in the *rates*, not *percentage point* changes). The estimated treatment effects are the proportion change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control. Panels A–D are, respectively, the effects (percent change) on the number of unemployed individuals, the unemployment rate, the number of individuals in the labor force, and the labor force participation rate.

Figure 10: Effects of 1996/97 federal minimum wage increase in counties with a Supercenter
Synthetic control estimates



Note: Estimated using employment data from the QCEW, labor force data from LAUS, normalized to one in the 1995. Supercenter entry timing and location from Holmes (2011). Data on state and federal minimum wages from the Tax Policy Center. Treated counties are those which received a Supercenter before 1996; untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The bias-correction procedure is applied. The thick, blue line is the path of ATT estimates. The grey lines are paths of random samples of 1,000 placebo ATT estimates. The light grey area shows the 95% confidence intervals from the placebo-average-variance estimates. All figures normalized to one in 1995. Restricted to states with a minimum wage which tracked the federal minimum wage over 1990-2005. All regressions are weighted using the 1990 county population and control for annual total population. Counties further restricted to those with 1990 population between 10,000 and 1,000,000. This yields a full sample of 182–188 treated counties and 25 control counties.

Table 1: Mean Annual Earnings of Employed Workers: Walmart (WM), Retail Sector, and Overall

Year	Full-time/ Part-time	WM Employees in Sample	WM % Women	Mean WM Earnings	Mean Retail Earnings	Mean Overall Earnings
1999	FT	354,933	73.36	$16,144	$24,845	$35,203
1999	PT	70,314	69.10	$7,499	$8,426	$15,719
2000	FT	392,949	72.45	$16,882	$25,174	$35,902
2000	PT	75,050	67.75	$7,694	$8,269	$16,214
2001	FT	438,320	71.32	$17,592	$27,293	$38,904
2001	PT	80,537	66.66	$7,877	$9,323	$18,377

Note: Walmart (WM) figures calculated for 'tenured' hourly Walmart employees backed out from court submissions for Wal-Mart Stores, Inc. v Dukes (see Drogin (2003)) and exclude Sam's Club employees. (Weighted) mean U.S. overall and U.S. retail annual earnings for full-time and part-time employees in the contiguous U.S. aged 15-65 taken from the CPS ASEC. All income figures in current USD.

Table 2: Median and Mean County Values of Select Variables in 1990, by Sample

	Treated Sample		Donor Pool		Untreated Excluded Counties	
	Median	Mean	Median	Mean	Median	Mean
Total employees	18,217	43,190	32,603	80,233	3,335	24,788
Real total earnings/worker (2017 USD)	30,771	31,261	33,700	34,151	26,839	28,432
Retail Employees	2,490	5,475	3,986	10,263	389	2,908
Real retail earnings/worker (2017 USD)	20,762	21,069	22,437	23,281	18,824	19,379
Employees per establishment	16	17	16	15	11	12
Retail share of total employment (%)	13	14	13	14	12	12
Retail HHI (out of 10,000)	133	149	126	138	249	325
Unemployment rate (%)	6	6	5	6	6	6
Labor force participation rate (%)	63	63	65	66	61	60
Total population	52,457	95,952	75,645	176,116	12,824	58,220
N	505–578	505–578	39	39	1,684	1,684

Note: Data from the QCEW, LAUS, Intercensal Population Estimates, and Dun & Bradstreet DMI. All dollar figures in 2017 USD, deflated by the Personal Consumption Expenditures: Chain-type Price Index. The median value per sample is *individually selected for each variable listed*, so the per-worker figures *should not* be calculated as the ratio of the level value and total employees or population respectively—though those figures are quite similar to these. The sample of treated counties includes the 582 counties which received their first Walmart Supercenter between 1995 and 2000, and which had a 1990 population between 10,000 and 1 million people. The treated sample is then further restricted, for each variable, such that all remaining treated counties have 1990 values within the bounds of the donor pool, yielding between 505 and 578 treated counties, depending on the variable of interest (including outcomes not reported here). The donor pool of untreated counties includes the 39 counties in which Walmart attempted to open the county's first Supercenter between 1990 and 2005 but where local political efforts prevented them from doing so. The set of untreated excluded counties include those counties in which Walmart never tried or succeeded in building a first Supercenter between 1990 and 2005. Not considered here are those counties which received a first Supercenter before 1995 or after 2000 (to allow a panel of treated counties balanced in event time in the five years either side of treatment).

Table 3 Effects of Supercenter entry five years after opening
Retail employment-level concentration
Synthetic control estimates

	(1)	(2)	(3)	(4)
A: Retail employment HHI				
Estimated treatment effect (%)	14.4104	20.2522	17.5178	20.2090
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0020	0.0360	0.0010	0.0050
Placebo-variance p-value	<0.0001	<0.0001	<0.0001	<0.0001
B: Retail share of total local employment				
Estimated treatment effect (%)	1.6005	1.9710	1.8002	1.9994
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.1389	0.1429	0.0410	0.0879
Placebo-variance p-value	0.0096	0.0003	0.0041	0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using HHI calculated from the Dun's Market Indicators data from Dun & Bradstreet, employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains 505–578 treated counties and 39 donor pool counties. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the percent change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control.

Table 4: Effect of Supercenter entry five years after opening
Aggregate and retail employment and earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
A: Aggregate employment				
Estimated treatment effect (%)	-1.9876	-3.1119	-1.7673	-3.1419
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.0599	0.0410	0.0390	0.0190
Placebo-variance p-value	0.0053	$<$0.0001	0.0122	$<$0.0001
B: Aggregate total earnings				
Estimated treatment effect (%)	-1.1133	-3.3439	-1.5706	-4.3229
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.6583	0.4545	0.5485	0.2687
Placebo-variance p-value	0.3482	0.0008	0.1990	$<$0.0001
C: Aggregate earnings/worker				
Estimated treatment effect (%)	-1.6970	-2.9093	-2.0240	-3.2479
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.1568	0.2488	0.5195	0.2727
Placebo-variance p-value	0.0002	$<$0.0001	$<$0.0001	$<$0.0001
D: Retail employment				
Estimated treatment effect (%)	1.2059	2.8719	1.1118	1.9408
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.1538	0.1708	0.2238	0.1558
Placebo-variance p-value	0.1274	$<$0.0001	0.1874	0.0010
E: Retail total earnings				
Estimated treatment effect (%)	1.4422	4.4363	0.7482	4.3795
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.1019	0.0949	0.0829	0.0679
Placebo-variance p-value	0.1770	$<$0.0001	0.5102	$<$0.0001
F: Retail earnings/worker				
Estimated treatment effect (%)	-0.3317	-1.3227	-0.2063	-1.3727
Andrews *p*-value	$<$0.0001	$<$0.0001	$<$0.0001	$<$0.0001
RMSPE-ranked *p*-value	0.9950	0.7303	0.9970	0.8252
Placebo-variance p-value	0.4587	0.0009	0.6239	0.0002
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the percent change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control.

Table 5: Effect of Supercenter entry five years after opening
Non-retail employment and earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
A: Non-retail employment				
Estimated treatment effect (%)	-1.5554	-3.2443	-1.9131	-3.1655
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0070	0.0140	0.0120	0.0150
Placebo-variance p-value	0.0348	<0.0001	0.0095	<0.0001
B: Non-retail total earnings				
Estimated treatment effect (%)	-1.8420	-5.0254	-2.8200	-6.3139
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.3916	0.0789	0.2438	0.0759
Placebo-variance p-value	0.1495	<0.0001	0.0316	<0.0001
C: Non-retail earnings/worker				
Estimated treatment effect (%)	-7.2472	-9.3294	-9.0344	-9.9832
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.1528	0.0949	0.0230	0.0430
Placebo-variance p-value	0.0009	<0.0001	<0.0001	<0.0001
D: Employment, goods-producing industries				
Estimated treatment effect (%)	-5.3918	-5.7273	-6.1601	-5.6225
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0979	0.0080	0.0380	0.0230
Placebo-variance p-value	<0.0001	<0.0001	<0.0001	<0.0001
E: Total earnings, goods-producing industries				
Estimated treatment effect (%)	-7.7034	-9.6337	-8.0697	-8.8181
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.1479	0.0410	0.0889	0.0390
Placebo-variance p-value	<0.0001	<0.0001	<0.0001	<0.0001
F: Earnings/worker, goods-producing industries				
Estimated treatment effect (%)	-2.2492	-1.6159	-2.9546	-2.7661
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.1548	0.0240	0.1459	0.1279
Placebo-variance p-value	0.0029	0.0180	<0.0001	<0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the percent change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control.

Table 6: Effect of Supercenter entry five years after opening
Labor force activity
Synthetic control estimates

	(1)	(2)	(3)	(4)
A: Unemployment count				
Estimated treatment effect (%)	10.7325	6.8159	9.3922	7.2594
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0140	0.2607	0.0150	0.0629
Placebo-variance p-value	<0.0001	0.0002	0.0002	<0.0001
B: Unemployment rate				
Estimated treatment effect (%)	10.2526	7.1546	11.1452	5.8197
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0849	0.1249	0.1199	0.5105
Placebo-variance p-value	<0.0001	<0.0001	<0.0001	0.0001
C: Labor force count				
Estimated treatment effect (%)	-1.0612	-1.0971	-1.1888	-1.3640
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.8232	0.6643	0.7772	0.6853
Placebo-variance p-value	0.1013	0.0006	0.0746	<0.0001
D: Labor force participation rate				
Estimated treatment effect (%)	-0.7727	-1.4678	-0.6712	-1.3034
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.5385	0.2428	0.6593	0.2398
Placebo-variance p-value	0.0646	<0.0001	0.1077	<0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 38–39 donor pool counties. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry (meaning the LFP rate and unemployment rate estimates are *percent* changes in the *rates*, not *percentage point* changes). The estimated treatment effects are the percent change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control.

A Appendix

A.1 Donor Pool Counties

Most of the donor pool counties either already had a (much smaller) Walmart Discount store, or received one between 1990 and 2005, but Walmart was unable to open a Supercenter in any of them before the end of 2005. The presence of a Discount store does not disqualify these counties from the donor pool for three reasons. First, as noted, some Discount stores employed as few as one fifth the employees of a typical Supercenter, and competed in fewer subsectors. Second, most of the treated counties already had a Walmart Discount store or received one during the period. The treatment of interest is the first Supercenter entry into a county because Supercenter entry is a substantively different event than Discount store presence or entry. Third, if anything, the arrival of a Discount store would likely have same-signed but smaller effects compared to Supercenter entry, which would bias any estimated treatment effects toward zero. Thus any significant estimated treatment effects may, in fact, be conservative. Table A1 presents the list of donor pool counties, along with the year a Supercenter was first proposed in each county.

A.2 Synthetic Control Predictors

To estimate the effects of Supercenter entry, I specify a set of predictor variables including ten covariates: the five-year average (over 1990–1994, before *any* included counties received their first Supercenter), of county total employment, county retail employment, county total per-worker earnings, county retail per-worker earnings, the retail share of total county employment, total county population, the unemployment rate, the employment rate, the labor force participation rate, and the white share of total county population. I also include the values of the outcome variable in each of 1990, 1991, 1992, and 1993. For each treated county and all 39 donor counties, all variables except rates are normalized to the value in the final pre-treatment year for each synthetic control estimation. As a robustness check I also estimate effects but with the 1993 outcome value dropped as a predictor.

All predictor variable values are observed for every included unit (I do not want any predictor variable values from 1995 onward, since this is when the earliest included treated counties began being treated). Yet with 14 predictor variables the curse of dimensionality makes it less likely that any estimated synthetic control will be non-unique given (a maximum of) 39 donor pool counties (see Abadie and L'Hour (2021)). I intentionally exclude additional pre-treatment values of the outcome variable both to ensure the covariates are able to receive positive weighting in every synthetic control (Kaul et al., 2022) and to allow the bias-corrected pre-treatment outcomes to potentially deviate from zero in these excluded years.

To estimate the effects of the 1996/97 federal minimum wage increase, I specify a similar set of predictors: the six-year average (over 1990–1995, before *any* the minimum wage increase), of county total employment, county retail employment, county total per-worker earnings, county retail per-worker earnings, the retail share of total county employment, total county population, the unemployment rate, the employment rate, the labor force participation rate, and the white share of total county population. I also include the values of the outcome variable in each of 1990–1994. As a robustness check I also estimate effects but without the 1993 and 1994 outcome values added as predictors.

A.3 *p*-value Construction

A.3.1 Andrews *p*-values

Andrews (2003) originally proposed an end-of-sample instability test in another context, assuming that the observations are stationary and ergodic under the null, such that a null hypothesis of no effect can be viewed as a hypothesis that the time series is stationary. Hahn and Shi (2017) and Zhang (2019) propose its use for synthetic control estimators—the latter explicitly when working with many treated units. In my case, I counties are treated at event period $e = 0$. Let $\tau_{ie} = \sum_{i=1}^{I} \gamma_i (Y_{ie} - Y_{ie}^N)$ as defined in Equation (7). Define a test statistic $\hat{S} = \sum_{i=1}^{I} \gamma_i (\hat{\tau}_{i,E})^2$, where γ_i are the 1990 population weights described in Section 4, and E is the final post-treatment event period for which there exists a balanced sample of treated units. Let the null hypothesis be a path of zero average treatment effects, and let the permutation distribution be defined by $S(e) = \sum_{i=1}^{I} \gamma_i (\hat{\tau}_{i,e})^2$ for each $e \leq -1$. Then the Andrews p-value can be can be computed as:

$$p_{Andrews} = \frac{1}{D} \sum_{e=-D}^{-1} \mathbb{1}\{S(e) \geq \hat{S}\}$$

where D is the number of pre-treatment event periods for which there exists a balanced sample of treated units.

A.3.2 In-Space Placebo RMSPE-ranked p-values

Similar to Cavallo et al. (2013); Abadie and L'Hour (2021) and Abadie (2021), and assuming treatment assignment is uniformly distributed, I derive in-space placebo RMSPE p-values from (a sample of) the empirical distribution of 'placebo' average treatment effects, each of which is averaged across a full set of treatment permutations for each treated unit i. Specifically, for each i treatment is re-assigned "in space" to each untreated unit $j \in \{I+1,...,I+J\}$ in the donor pool for i, while i and the remaining donors comprise the donor pool for j. This yields $I \times J$ paths of marginal estimated placebo treatment effects, $\hat{\tau}_{ji}$, for each donor for each treated unit (where, in general, $\hat{\tau}_{ji} \neq \hat{\tau}_{jk}\ \forall\ k \in \{1,...,I\}$, $k \neq i$). From these, there are J^I possible 'placebo averages' of exactly I marginal placebo treatment effects, such that each treated unit i is in the donor pool for *exactly one* of the contributing $\hat{\tau}_{ji}$. This becomes an impossibly large number of possible averages as I and J grow, as in my case. I therefore randomly draw 1,000 of these placebo averages, $\hat{\hat{\tau}}_g = \{\hat{\hat{\tau}}_{g,-D},...,\hat{\hat{\tau}}_{g,E}\}$—where $g \in \{2,...,1001\}$, D is the number of pre-treatment event periods over which a balanced sample can be averaged, and E is the number post-treatment event periods (excluding the period when treatment occurs) over which a balanced sample can be averaged—to create the sample distribution of placebo averages against which the single estimated ATT path, $\hat{\hat{\tau}}_1$, can be compared. The ATT and the placebo ATT paths can also be bias-corrected. The $RMSPE_g$ summary statistic for each $g \in \{1,...,1001\}$ can then be calculated as:

$$RMSPE_g = \frac{\sum_{e=0}^{E}(\hat{\hat{\tau}}_{g,e})^2/(E+1)}{\sum_{e=-1}^{-D}(\hat{\hat{\tau}}_{g,e})^2/D}$$

With these in hand, the $RMSPE$ p-value for the path of $\hat{\tau}_{1,t}$ through period T can be calculated as:

$$p_{RMSPE} = \frac{\sum_{g=2}^{1001} \mathbb{1}[RMSPE_g \geq RMSPE_1]}{1001}$$

A.3.3 Placebo-variance-based p-values

Estimate of the placebo-variance-based p-values again uses in-space placebo estimates, as in Appendix section A.3.2, and also assumes homoskedasticity across units (within each event year). A central limit theorem applies with many treated units and staggered treatment adoption, and with the sample distribution of placebo averages drawn with replacement. The approach, detailed as Algorithm 4 in Arkhangelsky et al.

(2021) (potentially applied to each individual post-treatment period), then follows straightforwardly.

A.4 TWFE Difference-in-Differences Model for the Minimum Wage Increase Treatment

For the treatment of the federal minimum wage increase interacted with pre-1996 Supercenter presence, I estimate a difference-in-differences model: $Y_{i,t} = \alpha_i + \delta_t + \psi \mathbb{1}\{t \geq 1996\} + \beta(S_i \times \mathbb{1}\{t \geq 1996\}) + \gamma \mathbf{X}_{i,t} + u_{i,t}$, where α_i and δ_t are county and time fixed effects, S_i is a dummy indicating if a county had a Supercenter before 1996, and $\mathbb{1}\{t \geq 1996\}$ is an indicator for the considered period 1996 onward (once the minimum wage increase had occurred). $\mathbf{X}_{i,t}$ is a vector of controls. The effect of interest is β, on the interaction between S_i and $\mathbb{1}\{t \geq 1996\}$. I consider the percentage impact by estimating the outcome variable normalized to one in 1995, as well as the natural logarithm of the outcome variable's annual value. I consider three treatment periods: 1996-1998, 1996-2001, and 1996-2005. As the increases were implemented later in the year (October 1996 and September 1997), 1998 is the first full year after the second increase. Standard errors are clustered at the county level in all cases.

A.5 Additional Tables and Figures

Table A1: Donor pool counties

Municipality	State	County	Year Project First Proposed
Antioch	California	Contra Costa	2003
Eureka	California	Humboldt	1997
Bakersfield	California	Kern	2002
San Francisco	California	San Francisco	1999
Gainesville	Florida	Alachua	2003
Lake-in-the-Hills	Illinois	McHenry	2001
Lawrence	Kansas	Douglas	2001
Paris	Kentucky	Bourbon	2002
Henderson	Kentucky	Henderson	2000
Ellsworth	Maine	Hancock	2000
Rockland	Maine	Knox	1999
Topsham	Maine	Sagadahoc	2000
Belfast	Maine	Waldo	2000
Chestertown	Maryland	Kent	2001
Accokeek	Maryland	Prince George's	1999
Stevensville	Maryland	Queen Anne's	1999
St. Johns	Michigan	Clinton	1999
Bedford	Michigan	Monroe	2001
Keene	New Hampshire	Cheshire	1993
Lacey	New Jersey	Ocean	2003
Taos	New Mexico	Taos	2003
Cortland	New York	Cortland	2003
Victor	New York	Ontario	2003
Potsdam	New York	St Lawrence	1998
Ithaca	New York	Tompkins	1999
Lorain	Ohio	Lorain	2003
Hood River	Oregon	Hood River	2001
Island City	Oregon	Union	2002
Hillsboro	Oregon	Washington	2003
Lower Gwynedd	Pennsylvania	Montgomery	2000
Williston	Vermont	Chittenden	1990
St. Albans	Vermont	Franklin	1993
Charlottesville	Virginia	Albemarle	1999
Kilmarnock	Virginia	Lancaster	1998
Front Royal	Virginia	Warren	2002
Morgantown	West Virginia	Monongalia	2000
Wheeling	West Virginia	Ohio	2003
Stoughton	Wisconsin	Dane	2003
River Falls	Wisconsin	Pierce	2002

Note: These counties had a Walmart Supercenter project proposed between 1990 and 2003, but local efforts (political or legal) delayed or prevented a Supercenter from entering the county before the end of 2005 (in some cases, the intitial proposal was for a *Discount* store). Many of these counties already had a Walmart *Discount* store, or received one during the period of interest.

Table A2: Effect of Supercenter entry five years after opening
Employment and earnings in services-providing industries, with and without retail
Synthetic control estimates

	(1)	(2)	(3)	(4)
A: Employment, services-providing industries				
Estimated treatment effect (%)	0.9453	0.1678	0.0529	-0.7180
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.4745	0.9820	0.8881	0.3676
Placebo-variance p-value	0.4297	0.8676	0.9668	0.4570
B: Total earnings, services-providing industries				
Estimated treatment effect (%)	-0.5147	-1.5255	-2.1818	-1.5561
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.1508	0.3566	0.2727	0.4256
Placebo-variance p-value	0.7702	0.3005	0.2668	0.2796
C: Earnings/worker, services-providing industries				
Estimated treatment effect (%)	-1.1304	-1.5204	-1.4116	-1.7276
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.0190	0.0380	0.0869	0.0829
Placebo-variance p-value	0.0148	0.0002	0.0033	<0.0001
D: Employment, non-retail services-providing industries				
Estimated treatment effect (%)	1.4819	-0.0655	-0.1105	-0.2669
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.2228	0.8791	0.4476	0.6973
Placebo-variance p-value	0.2861	0.9559	0.9433	0.8257
E: Total earnings, non-retail services-providing industries				
Estimated treatment effect (%)	0.4555	-2.1144	-2.5871	-2.3365
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.2917	0.2018	0.1638	0.2667
Placebo-variance p-value	0.8300	0.2526	0.2566	0.1984
F: Earnings/worker, non-retail services-providing industries				
Estimated treatment effect (%)	1.4065	-9.5036	-2.5599	-13.2946
Andrews *p*-value	<0.0001	<0.0001	<0.0001	<0.0001
RMSPE-ranked *p*-value	0.8831	0.1788	0.6324	0.0569
Placebo-variance p-value	0.9103	0.0102	0.8273	0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. All outcomes normalized to own-value in the final pre-treatment year prior to Supercenter entry. The estimated treatment effects are the percent change in the outcome variable in the fifth year following Supercenter entry, relative to the synthetic control.

Table A3: Effect of Supercenter entry five years after opening
Employment and earnings
Callaway and Sant'Anna Difference-in-differences (CSDiD) estimates

	(1)
A: Aggregate employment	
Estimated treatment effect (100× log points)	-2.6086
Asymptotic *p*-value	0.0323
B: Aggregate total earnings	
Estimated treatment effect (100× log points)	-8.2453
Asymptotic *p*-value	0.0014
C: Aggregate earnings/worker	
Estimated treatment effect (100× log points)	-2.5424
Asymptotic *p*-value	0.1329
D: Retail employment	
Estimated treatment effect (100× log points)	5.1924
Asymptotic *p*-value	0.1735
E: Retail total earnings	
Estimated treatment effect (100× log points)	7.2859
Asymptotic *p*-value	0.0282
F: Retail earnings/worker	
Estimated treatment effect (100× log points)	-0.6970
Asymptotic *p*-value	0.7297
G: Non-retail employment	
Estimated treatment effect (100× log points)	-3.7089
Asymptotic *p*-value	0.0011
H: Non-retail total earnings	
Estimated treatment effect (100× log points)	-6.2202
Asymptotic *p*-value	0.0111
I: Non-retail earnings/worker	
Estimated treatment effect (100× log points)	-5.4222
Asymptotic *p*-value	0.4924

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 control counties. All outcomes logged, and the estimated effects are the change in 100× log points in the outcome variable in the fifth year following Supercenter entry. Standard errors are clustered by commuting zone.

Table A4: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Aggregate employment
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	0.6039	0.7984	0.1877	0.7213
Andrews *p*-value	0.0909	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.4545	0.2238	0.5994	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1997				
Estimated treatment effect (%)	3.2609	4.0261	2.4355	4.7467
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.1009	0.0340	0.0300	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1998				
Estimated treatment effect (%)	3.9964	4.8173	3.4382	5.5359
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.0909	0.0310	0.0200	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1999				
Estimated treatment effect (%)	4.7574	5.8356	4.0696	6.6050
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.0949	0.0350	0.0150	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2000				
Estimated treatment effect (%)	4.0936	6.0980	3.0860	6.8987
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.1179	0.0380	0.0200	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2001				
Estimated treatment effect (%)	3.6013	5.9887	3.0099	6.4606
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.1628	0.0420	0.0490	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A5: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Retail employment
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	1.0248	-1.5309	0.1631	-1.4022
Andrews *p*-value	0.4545	< 0.0001	0.4545	< 0.0001
RMSPE-ranked *p*-value	0.4885	0.1139	0.9520	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1997				
Estimated treatment effect (%)	3.0177	1.7430	1.4006	1.7644
Andrews *p*-value	0.1818	< 0.0001	0.1818	< 0.0001
RMSPE-ranked *p*-value	0.2897	0.2388	0.8092	0.0010
Placebo-variance p-value	0.0009	0.0009	0.0001	0.0001
1998				
Estimated treatment effect (%)	4.0328	3.9500	2.3476	4.0898
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.3317	0.1698	0.6883	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1999				
Estimated treatment effect (%)	2.9690	5.5829	1.2598	5.8637
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.5564	0.1898	0.7772	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2000				
Estimated treatment effect (%)	2.5685	7.7726	0.7383	8.5813
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6034	0.1718	0.8641	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2001				
Estimated treatment effect (%)	3.5697	9.4533	1.2396	10.9676
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6024	0.1499	0.9221	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A6: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Aggregate total earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	0.8682	-0.3476	-0.1548	0.2624
Andrews *p*-value	0.1818	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.5634	0.8092	0.7353	0.7582
Placebo-variance p-value	0.1670	0.1670	0.3510	0.3510
1997				
Estimated treatment effect (%)	3.0957	2.3276	1.0811	2.0379
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6424	0.6134	0.3546	0.0020
Placebo-variance p-value	< 0.0001	< 0.0001	0.0001	0.0001
1998				
Estimated treatment effect (%)	2.6255	0.7107	0.4221	-0.0799
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6583	0.8042	0.5275	0.0719
Placebo-variance p-value	0.4074	0.4074	0.9184	0.9184
1999				
Estimated treatment effect (%)	3.1045	1.8064	0.3666	-0.4511
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6873	0.8911	0.6913	0.1728
Placebo-variance p-value	0.1401	0.1401	0.6911	0.6911
2000				
Estimated treatment effect (%)	2.0897	0.5287	-1.3922	-2.9585
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.7403	0.9750	0.6394	0.0519
Placebo-variance p-value	0.7022	0.7022	0.0119	0.0119
2001				
Estimated treatment effect (%)	1.4718	0.9891	-1.3507	-2.5553
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.7772	0.9920	0.6494	0.0400
Placebo-variance p-value	0.4827	0.4827	0.0273	0.0273
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A7: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Retail total earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	0.7133	-0.2665	0.4096	-0.4781
Andrews *p*-value	0.4545	0.0909	0.4545	< 0.0001
RMSPE-ranked *p*-value	0.3057	0.7672	0.7502	0.1279
Placebo-variance p-value	0.2216	0.2216	0.0189	0.0189
1997				
Estimated treatment effect (%)	1.3439	2.8215	1.3926	2.7212
Andrews *p*-value	0.4545	< 0.0001	0.4545	< 0.0001
RMSPE-ranked *p*-value	0.2877	0.1998	0.4765	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1998				
Estimated treatment effect (%)	2.6633	8.7510	3.5421	9.9075
Andrews *p*-value	0.4545	< 0.0001	0.2727	< 0.0001
RMSPE-ranked *p*-value	0.1988	0.0480	0.2188	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1999				
Estimated treatment effect (%)	0.9244	7.9560	1.0719	7.3170
Andrews *p*-value	0.3636	< 0.0001	0.2727	< 0.0001
RMSPE-ranked *p*-value	0.4535	0.0659	0.4296	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2000				
Estimated treatment effect (%)	0.4006	8.7648	-0.5548	8.3856
Andrews *p*-value	0.2727	< 0.0001	0.1818	< 0.0001
RMSPE-ranked *p*-value	0.5794	0.0729	0.6204	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
2001				
Estimated treatment effect (%)	1.8000	10.2196	-1.2862	10.5962
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.6104	0.0809	0.7902	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A8: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Aggregate per-worker earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	-0.3757	0.0009	-0.2350	-0.4391
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.7203	0.9990	0.9421	0.0100
Placebo-variance p-value	0.9956	0.9956	0.0090	0.0090
1997				
Estimated treatment effect (%)	-1.0566	0.2002	-0.8704	-0.4417
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.5215	0.9500	0.8082	0.0859
Placebo-variance p-value	0.4832	0.4832	0.1323	0.1323
1998				
Estimated treatment effect (%)	-2.0515	-1.2044	-1.7584	-2.4224
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.3007	0.4685	0.6793	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1999				
Estimated treatment effect (%)	-3.0130	-0.3079	-2.9012	-3.1355
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.2348	0.5465	0.5684	0.0010
Placebo-variance p-value	0.3080	0.3080	< 0.0001	< 0.0001
2000				
Estimated treatment effect (%)	-3.4790	-0.5767	-3.2941	-3.5493
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.2118	0.5814	0.5405	0.0010
Placebo-variance p-value	0.0664	0.0664	< 0.0001	< 0.0001
2001				
Estimated treatment effect (%)	-3.3488	-0.3919	-3.3062	-3.1937
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.2008	0.6573	0.5305	0.0010
Placebo-variance p-value	0.3226	0.3226	< 0.0001	< 0.0001
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A9: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter
Retail per-worker earnings
Synthetic control estimates

	(1)	(2)	(3)	(4)
1996				
Estimated treatment effect (%)	-0.5180	2.1261	-0.3382	1.1884
Andrews *p*-value	0.0909	< 0.0001	0.0909	< 0.0001
RMSPE-ranked *p*-value	0.5215	0.2977	0.6963	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1997				
Estimated treatment effect (%)	-0.1610	3.1000	-0.7122	0.5691
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.7662	0.2368	0.6683	0.0020
Placebo-variance p-value	< 0.0001	< 0.0001	0.0581	0.0581
1998				
Estimated treatment effect (%)	-0.4791	2.4600	-0.0231	4.2439
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.8951	0.4326	0.8821	0.0010
Placebo-variance p-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
1999				
Estimated treatment effect (%)	-0.4444	2.3907	-0.2631	-0.7591
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.9930	0.5774	0.9910	0.0010
Placebo-variance p-value	0.0013	0.0013	0.2664	0.2664
2000				
Estimated treatment effect (%)	0.0150	0.8266	0.7102	-1.9105
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	0.9990	0.7602	0.9950	0.0010
Placebo-variance p-value	0.2666	0.2666	0.0043	0.0043
2001				
Estimated treatment effect (%)	0.7405	0.6738	1.1588	-1.2036
Andrews *p*-value	< 0.0001	< 0.0001	< 0.0001	< 0.0001
RMSPE-ranked *p*-value	1.0000	0.7952	0.9850	0.0010
Placebo-variance p-value	0.2624	0.2624	0.0231	0.0231
Complete set of predictor variables	No	No	Yes	Yes
Bias-corrected estimates	No	Yes	No	Yes

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, Supercenter entry timing and location from Holmes (2011), and minimum wage rates from the Tax Policy Center. The sample is restrict to counties in states which tracked the federal minimum wage over the period. The 182–188 treated counties are those which received a Supercenter before 1996; the 25 untreated counties are those which Walmart wanted to enter but were prevented from doing so by local efforts. Treatment is the federal minimum wage increases in 1996 and 1997, and the treated period begins in 1996 for counties with a Supercenter, extending to the end of the panel, as indicated. The estimated treatment effects are the percent change in each year, relative to the synthetic control.

Table A10: Effect of 1996/97 federal minimum wage increases in counties with a Supercenter Callaway and Sant'Anna Difference-in-differences (CSDiD) estimates

	(1)
A: Aggregate employment	
Estimated treatment effect (100× log points)	8.7193
Asymptotic *p*-value	0.0720
B: Retail employment	
Estimated treatment effect (100× log points)	10.5704
Asymptotic *p*-value	0.3789
C: Aggregate total earnings	
Estimated treatment effect (100× log points)	1.0675
Asymptotic *p*-value	0.8768
D: Retail total earnings	
Estimated treatment effect (100× log points)	15.2656
Asymptotic *p*-value	0.0244
E: Aggregate per-worker earnings	
Estimated treatment effect (100× log points)	-0.7191
Asymptotic *p*-value	0.8680
F: Retail per-worker earnings	
Estimated treatment effect (100× log points)	1.8797
Asymptotic *p*-value	0.7165

Note: Estimated using employment and earnings data from the QCEW, labor force data from LAUS, and Supercenter entry timing and location from Holmes (2011). Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 182–188 treated counties and 25 control counties. All outcomes logged, and the estimated effects are the change in 100× log points in the outcome variable in the fifth year following Supercenter entry—that is, in 2001. Standard errors are clustered by commuting zone.

Table A11
TWFE event study estimates of event-year treatment effects of Supercenter entry
Estimated without dummies for pre-treatment event years

	Total Employment		Total earnings		Earnings/worker	
	Non-retail	Retail	Non-retail	Retail	Non-retail	Retail
No control counties, no bins for event years < -5 and > 5: Estimated effect in log points						
Event-year 0	-0.0066	0.0161**	-0.0070	0.0124	-0.0107	0.0020
	(0.0065)	(0.0075)	(0.0070)	(0.0078)	(0.0085)	(0.0028)
Event-year 1	-0.0111	0.0166	-0.0133	0.0137	-0.0236*	0.0054
	(0.0102)	(0.0119)	(0.0115)	(0.0129)	(0.0140)	(0.0040)
Event-year 2	-0.0171	0.0155	-0.0223	0.0130	-0.0348*	0.0097*
	(0.0146)	(0.0174)	(0.0166)	(0.0185)	(0.0178)	(0.0052)
Event-year 3	-0.0223	0.0085	-0.0304	0.0058	-0.0449**	0.0118*
	(0.0195)	(0.0235)	(0.0223)	(0.0250)	(0.0228)	(0.0066)
Event-year 4	-0.0269	0.0033	-0.0368	0.0036	-0.0607**	0.0161**
	(0.0254)	(0.0306)	(0.0294)	(0.0325)	(0.0291)	(0.0079)
Event-year 5	-0.0412	-0.0087	-0.0510	-0.0033	-0.0691*	0.0198**
	(0.0327)	(0.0399)	(0.0376)	(0.0412)	(0.0362)	(0.0092)
With control counties, no bins for event years < -5 and > 5: Estimated effect in log points						
Event-year 0	0.0050	0.0313***	-0.0010	0.0248***	-0.0179	-0.0008
	(0.0054)	(0.0072)	(0.0058)	(0.0078)	(0.0126)	(0.0027)
Event-year 1	0.0039	0.0358***	-0.0063	0.0283***	-0.0326*	0.0007
	(0.0072)	(0.0092)	(0.0076)	(0.0100)	(0.0167)	(0.0036)
Event-year 2	0.0016	0.0384***	-0.0137	0.0296**	-0.0449**	0.0029
	(0.0092)	(0.0113)	(0.0096)	(0.0122)	(0.0194)	(0.0044)
Event-year 3	0.0003	0.0353***	-0.0201*	0.0248*	-0.0564**	0.0033
	(0.0115)	(0.0136)	(0.0119)	(0.0148)	(0.0225)	(0.0051)
Event-year 4	0.0006	0.0338**	-0.0241	0.0249	-0.0742***	0.0059
	(0.0144)	(0.0162)	(0.0151)	(0.0177)	(0.0264)	(0.0059)
Event-year 5	-0.0080	0.0256	-0.0338*	0.0209	-0.0819***	0.0092
	(0.0170)	(0.0187)	(0.0182)	(0.0197)	(0.0300)	(0.0063)
With control counties, with bins for event years < -5 and > 5: Estimated effect in log points						
Event-year 0	0.0061	0.0320***	-0.0002	0.0261***	-0.0188	-0.0008
	(0.0055)	(0.0072)	(0.0062)	(0.0080)	(0.0131)	(0.0028)
Event-year 1	0.0057	0.0370***	-0.0050	0.0303***	-0.0340*	0.0007
	(0.0073)	(0.0093)	(0.0081)	(0.0103)	(0.0174)	(0.0037)
Event-year 2	0.0041	0.0402***	-0.0120	0.0325**	-0.0468**	0.0029
	(0.0093)	(0.0115)	(0.0101)	(0.0126)	(0.0204)	(0.0045)
Event-year 3	0.0037	0.0379***	-0.0178	0.0286*	-0.0590**	0.0032
	(0.0115)	(0.0138)	(0.0123)	(0.0151)	(0.0240)	(0.0052)
Event-year 4	0.0047	0.0371**	-0.0212	0.0295*	-0.0773***	0.0058
	(0.0143)	(0.0163)	(0.0152)	(0.0178)	(0.0281)	(0.0060)
Event-year 5	-0.0031	0.0295	-0.0303*	0.0264	-0.0857***	0.0091
	(0.0166)	(0.0186)	(0.0180)	(0.0196)	(0.0319)	(0.0065)

Note: Estimated using logged employment and earnings data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. Estimates weighted by 1990 population The sample contains the 505–578 treated counties and 39 donor pool counties. Estimated using OLS with year and county fixed effects and with no adjustments for "forbidden comparisons"/weighting issues. Standard errors clustered by commuting zone.

Figure A1: Walmart employees by job, sex, and ethnicity

WAL-MART STORES, INC.
Office of Diversity

2005 EEO1 Survey Results

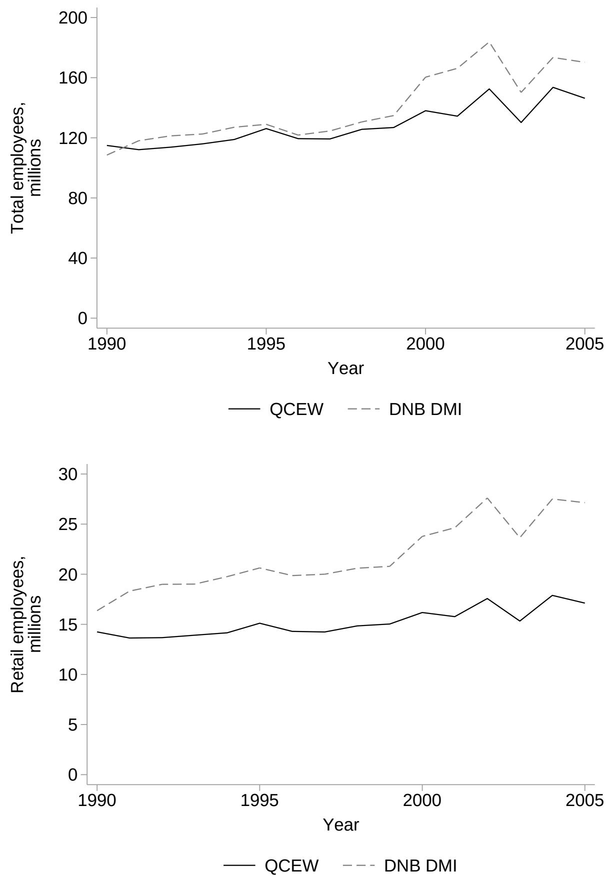
2005 EEOC Report

			****************** MALE ******************						****************** FEMALE ******************						All
		Total	White	African American	Hispanic	Asian	Native American	Ttl Male	White	African American	Hispanic	Asian	Native American	Ttl Female	Minority Assocs
1. Officials and Managers	Headcount	61,503	30,004	3,609	2,868	885	264	37,630	18,390	2,960	1,702	589	232	23,873	13,109
	% of Ttl	100.00%	48.78%	5.87%	4.66%	1.44%	0.43%	61.18%	29.90%	4.81%	2.77%	0.96%	0.38%	38.82%	21.31%
2. Professionals	Headcount	10,895	4,015	237	142	505	31	4,930	4,724	422	134	654	31	5,965	2,156
	% of Ttl	100.00%	36.85%	2.18%	1.30%	4.64%	0.28%	45.25%	43.36%	3.87%	1.23%	6.00%	0.28%	54.75%	19.79%
3. Technicians	Headcount	25,672	3,157	350	443	251	39	4,240	16,792	2,051	1,700	684	205	21,432	5,723
	% of Ttl	100.00%	12.30%	1.36%	1.73%	0.98%	0.15%	16.52%	65.41%	7.99%	6.62%	2.66%	0.80%	83.48%	22.29%
4. Sales Workers	Headcount	728,237	125,322	29,351	22,428	5,963	2,002	185,066	365,391	98,489	58,330	14,674	6,287	543,171	237,524
	% of Ttl	100.00%	17.21%	4.03%	3.08%	0.82%	0.27%	25.41%	50.17%	13.52%	8.01%	2.02%	0.86%	74.59%	32.62%
5. Office and Clerical	Headcount	58,589	8,037	1,029	894	261	92	10,313	37,818	4,962	3,978	1,063	455	48,276	12,734
	% of Ttl	100.00%	13.72%	1.76%	1.53%	0.45%	0.16%	17.60%	64.55%	8.47%	6.79%	1.81%	0.78%	82.40%	21.73%
6. Craft Workers (Skilled)	Headcount	2,965	2,016	264	269	18	16	2,583	307	47	20	5	3	382	642
	% of Ttl	100.00%	67.99%	8.90%	9.07%	0.61%	0.54%	87.12%	10.35%	1.59%	0.67%	0.17%	0.10%	12.88%	21.65%
7. Operatives (Semi-skilled)	Headcount	40,024	17,368	2,954	2,376	442	188	23,328	11,673	2,484	1,904	442	193	16,696	10,983
	% of Ttl	100.00%	43.39%	7.38%	5.94%	1.10%	0.47%	58.29%	29.17%	6.21%	4.76%	1.10%	0.48%	41.71%	27.44%
8. Laborers	Headcount	308,181	128,074	41,809	27,195	4,792	2,641	204,511	69,161	17,738	12,870	2,491	1,410	103,670	110,946
	% of Ttl	100.00%	41.56%	13.57%	8.82%	1.55%	0.86%	66.36%	22.44%	5.76%	4.18%	0.81%	0.46%	33.64%	36.00%
9. Service Workers	Headcount	110,844	40,041	10,184	6,967	1,517	544	59,253	36,094	7,822	6,047	1,091	537	51,591	34,709
	% of Ttl	100.00%	36.12%	9.19%	6.29%	1.37%	0.49%	53.46%	32.56%	7.06%	5.46%	0.98%	0.48%	46.54%	31.31%
Total	Headcount	1,346,910	358,034	89,787	63,582	14,634	5,817	531,854	560,350	136,975	86,685	21,693	9,353	815,056	428,526
	% of Ttl	100.00%	26.58%	6.67%	4.72%	1.09%	0.43%	39.49%	41.60%	10.17%	6.44%	1.61%	0.69%	60.51%	31.82%

Note: When adding percentages, differences are due to rounding.

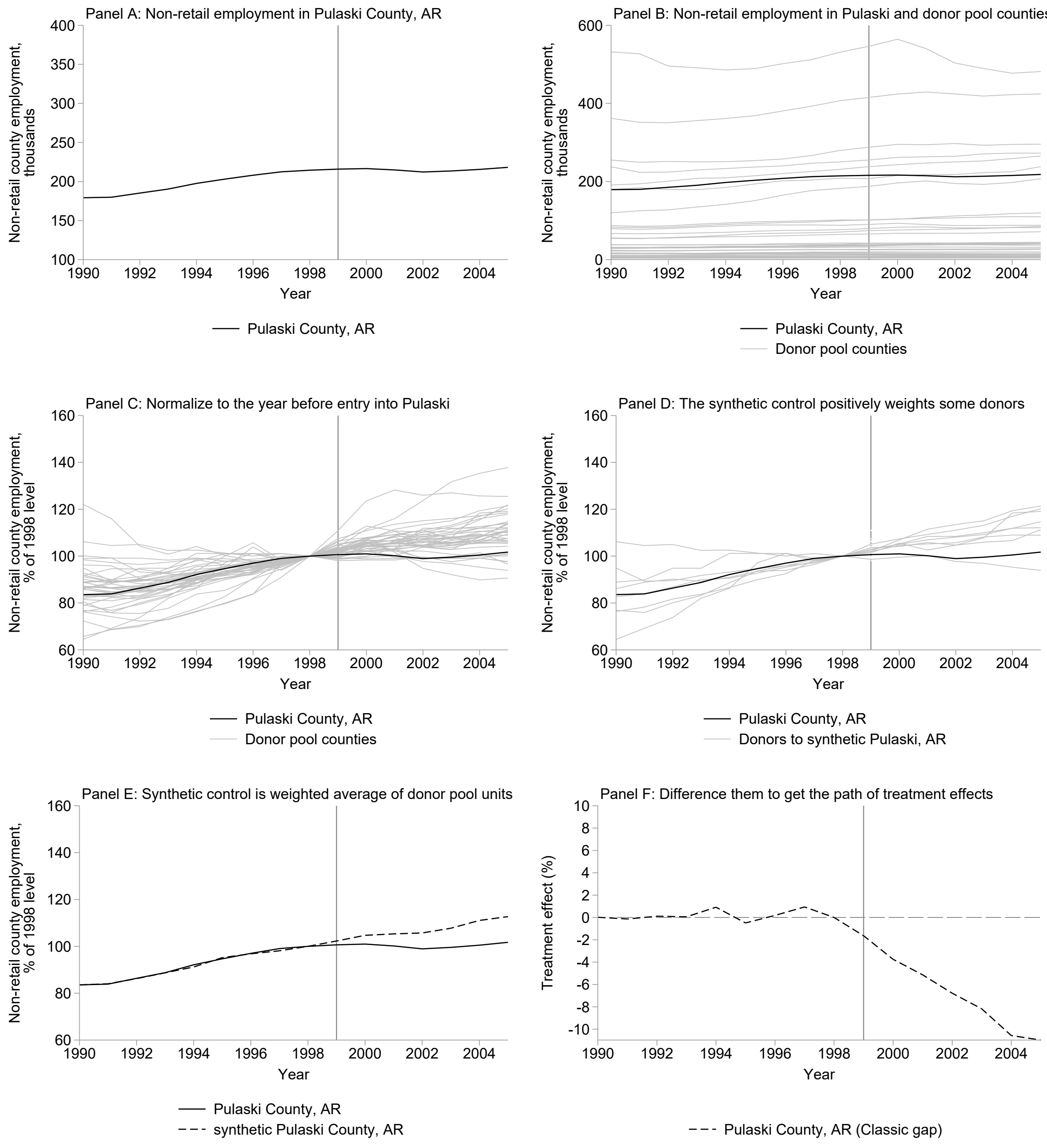
Note: Taken from Walmart's 2005 Equal Employment Opportunity Commission EE0-1 report Wal-Mart Stores, Inc. (2006b). The table shows that women comprised 61% of Walmart's total workforce in that year. 54% of the firm's employees were sales workers, and of those three-quarters were women. Minority women, more than half of them black, comprised nearly a quarter of the firm's sales workers, while minorities as a whole comprised 32% of Walmart's workforce, concentrated mainly among sales workers, laborers, and service workers. Over 85% of Walmart's employees were concentrated in these three job types.

Figure A2: Total and retail employees
QCEW vs Dun & Bradstreet DMI data



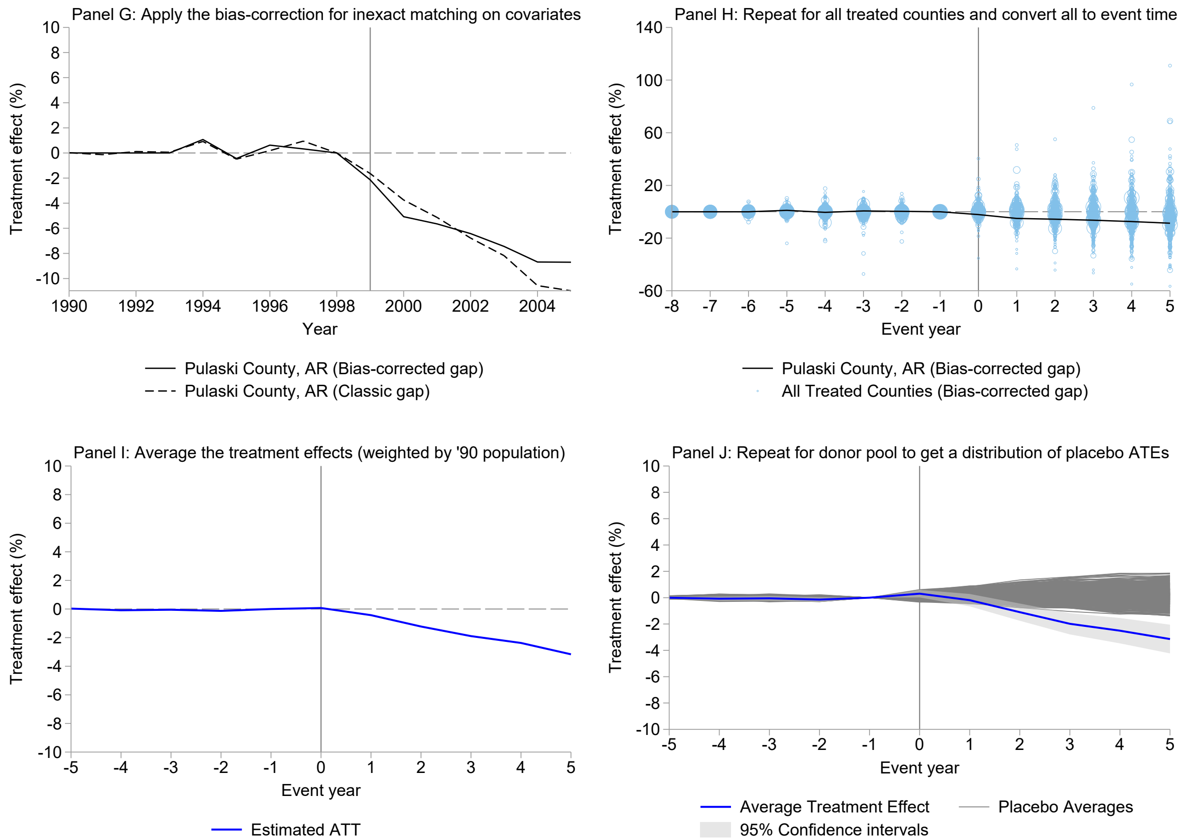
Note: Calculated using data from the QCEW and the Dun's Market Indicators data from Dun & Bradstreet (DNB). The top panel shows total reported employees, by year. The bottom panel shows total reported *retail* employees, by year, with the DNB figures restricted to establishments reporting up to 1,000 employees. Retail is identified in the QCEW using the 2-digit NAICS codes 44 and 45, and in the DNB data using the 2-digit SIC codes 52–59.

Figure A3:
Step-by-step example of (bias-corrected) stacked synthetic control approach
Focus on non-retail employment in Pulaski County, AR (Supercenter entry in 1997)



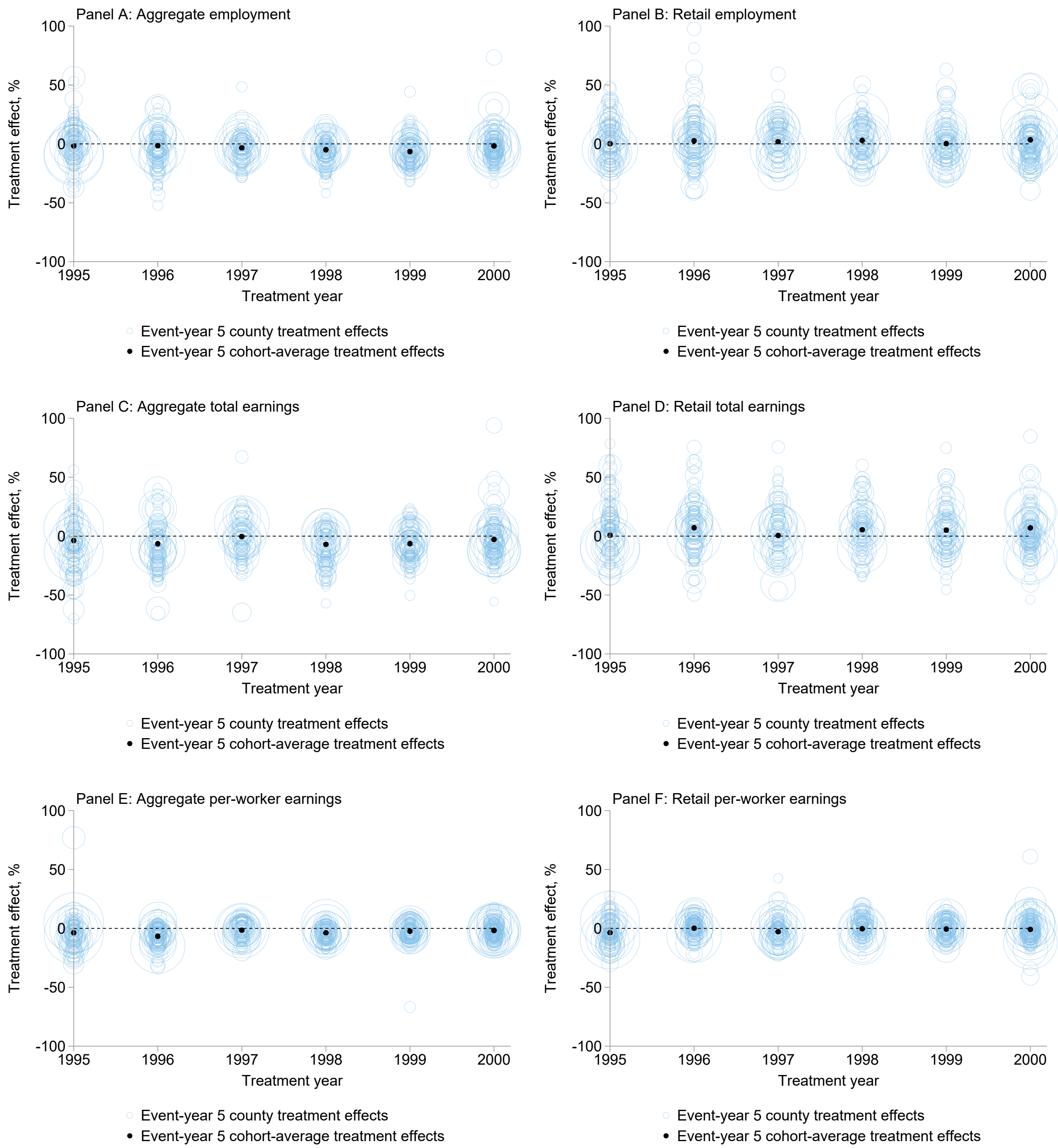
Note: Visualization of the synthetic control methodology continues in Figure A4. Panel A shows non-retail employment in Pulaski County, which received its first Supercenter in 1999; Panel B shows the same, also for donor pool counties; Panel C shows the same as Panel B, normalized to 100 in 1998 (the year before Pulaski was treated); Panel D shows the same as panel C, but only for Pulaski and positively-weighted donors in Pulaski's synthetic control; Panel E shows non-retail employment in Pulaski and in its synthetic control (the weighted average of donor pool units); Panel F shows the path of estimated treatment effects of Supercenter entry on non-retail employment in Pulaski—the difference between the two lines in Panel E.

Figure A4:
Step-by-step example of (bias-corrected) stacked synthetic control approach (cont'd.)
From the effect on non-retail employment in Pulaski to the ATT estimate across all treated units



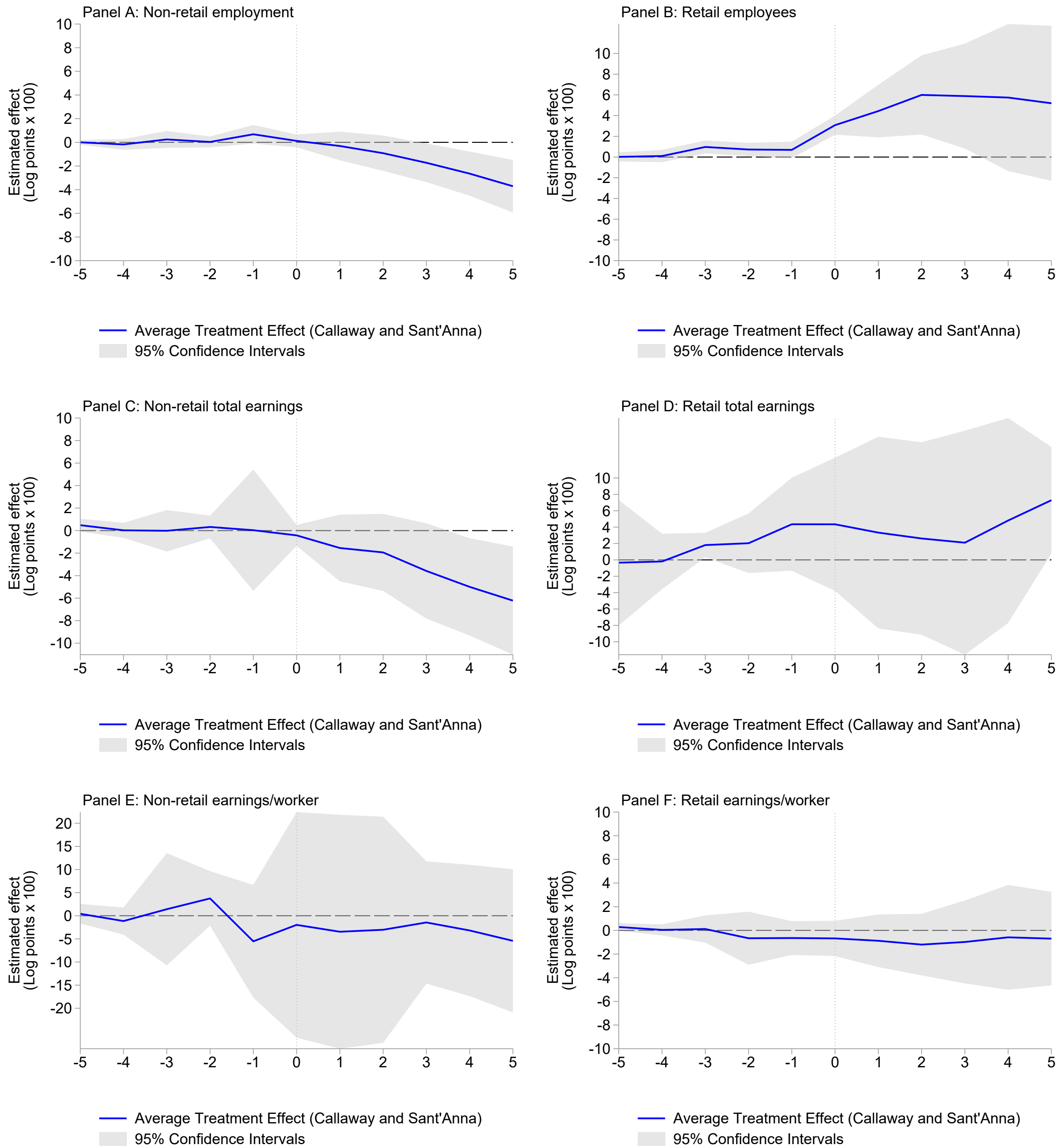
Note: Continuation of the visualization of the synthetic control methodology begun in Figure A3. This demonstrates the methodological process of the synthetic control approach, focusing on non-retail employment in Pulaski County, AR before repeating the process for all treated counties. Estimated using employment data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 567 treated counties and 39 donor pool counties. The bias-correction procedure is applied. Panel G shows the same as Panel F (Figure A3) as well as the result of applying the bias-correction procedure for inexact matching on pre-treatment covariates; Panel H shows these paths of estimated treatment effects (from Panel F) for all 567 treated counties in the non-retail employment sample, converted to event time (event year 0 is the year of Supercenter entry); Panel I shows the evolution of the estimated ATT (weighted by 1990 population), which is the estimand of interest; Panel J plots the ATT estimate from Panel I against 1,000 randomly drawn placebo average treatment effects (from placebo treatment of the donor pool units).

Figure A5: Event-year 5 county and cohort-average treatment effects of Supercenter entry
Synthetic control estimates for select variables



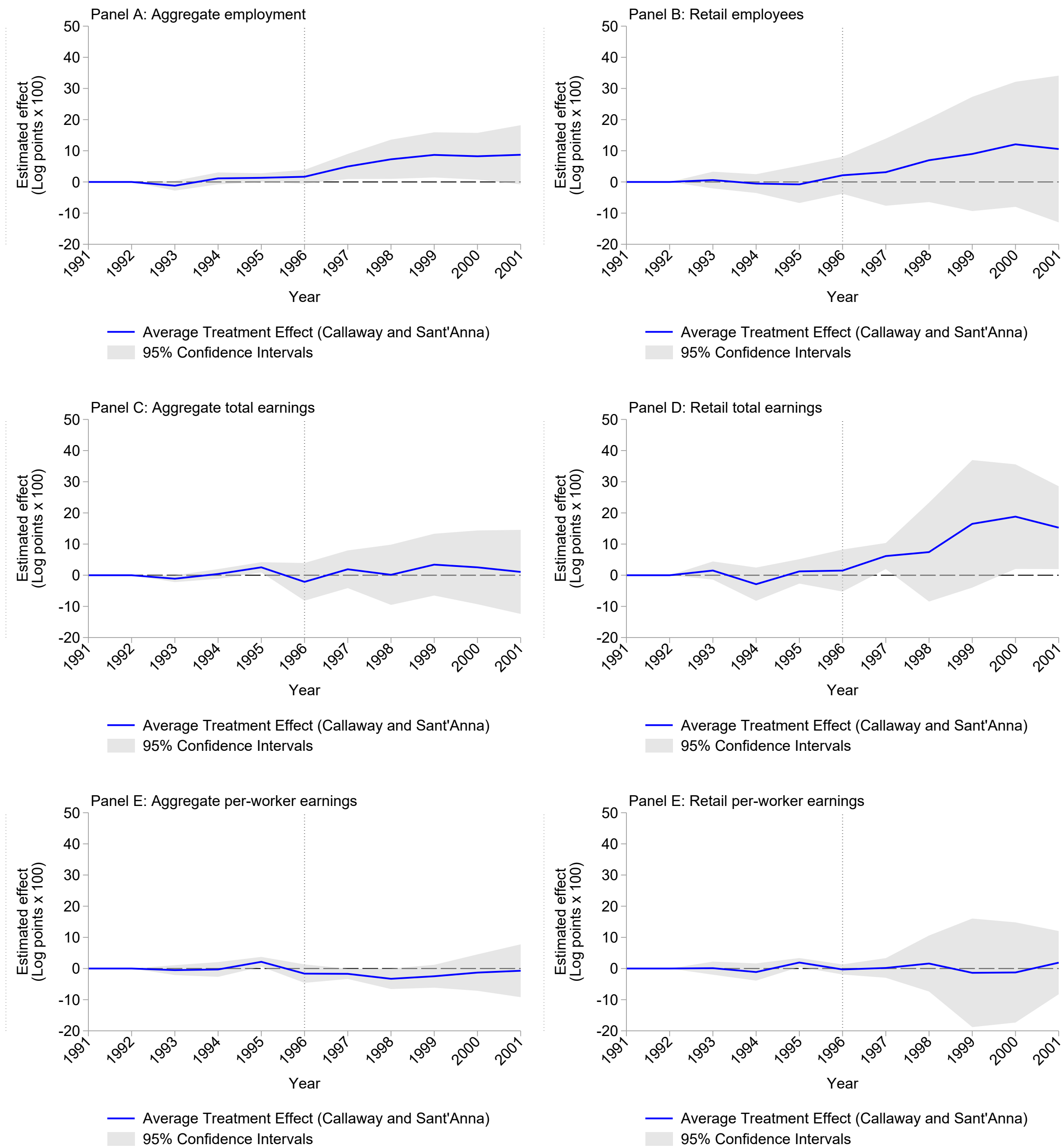
Note: Estimated using employment data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. 97 counties in the treated sample received their first Supercenter in 1995, 99 in 1996, 78 in 1997, 91 in 1998, 97 in 1999, and 122 in 2000. The bias-correction procedure is applied. The bubbles are centered around the individual county treatment effects estimated in the fifth year after Supercenter entry (relative to the year before Supercenter entry), and the bubble size represents their 1990 population. The dashed horizontal line indicates zero, and the dark circles are the cohort-ATT estimates (weighted by the 1990 county population). Panels A, C, and E are, respectively, these effects on aggregate employment, aggregate total earnings, and aggregate earnings per worker. Panels B, D, and F show the same respective outcomes for the retail industry only.

Figure A6: Effects of Supercenter entry on non-retail and retail employment and earnings
Callaway and Sant'Anna estimated effects



Note: Estimated using employment data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Data on state and federal minimum wages from the Tax Policy Center. Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 505–578 treated counties and 39 donor pool counties. Estimated using the Callaway and Sant'Anna (2020) estimator, with effects on log outcomes, with 1990 population weights. Asymptotic standard errors are estimated and clustered at the commuting zone level. The thick, blue line is the path of ATT estimates. The light grey area shows the 95% confidence intervals. The estimated treatment effects are 100× the log point effect. Panels A, C, and E are, respectively, the effects on non-retail employment, non-retail total earnings, and non-retail earnings per worker. Panels B, D, and F show the same respective outcomes for the retail industry only.

Figure A7: Effects of 1996/97 federal minimum wage increases in counties with a Supercenter Callaway and Sant'Anna estimated effects



Note: Estimated using employment data from the QCEW, labor force data from LAUS, as well as Supercenter entry timing and location from Holmes (2011). Data on state and federal minimum wages from the Tax Policy Center. Control counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains 182–188 treated counties and 25 donor pool counties. Estimated using the Callaway and Sant'Anna (2020) estimator, with effects on log outcomes, with 1990 population weights. Asymptotic standard errors are estimated and clustered at the commuting zone level. The thick, blue line is the path of ATT estimates. The light grey area shows the 95% confidence intervals. The estimated treatment effects are 100× the log point effect. Panels A, C, and E show the estimated effect on aggregate employment, total earnings, and average earnings, respectively. Panels and C and E are, respectively, the effects on non-retail earnings and non-retail employment. Panels B, D, and F show the effects on retail employment, total earnings, and average earnings, respectively. The effect on aggregate total earnings required one covariate be dropped due to a conformability error using the full specification.