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

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Job Market Paper

November 6, 2021

Abstract

This paper considers the extent and impact of monopsony power exercised by Walmart Supercenters. I focus on Walmart as it has long been the largest private-sector employer in the U.S., and as it pays very low wages. Previous research into the firm's labor market impact has yielded incongruous results, with little consensus on how to address identification concerns regarding endogeneity of store entry. A more recent literature has also demonstrated that widely-used estimators are often subject to numerous sources of bias when units are treated at different times. I address these identification concerns by adopting a stacked-in-event-time 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 find Supercenter entry caused significant reductions in local aggregate employment and earnings. Retail employment concentration grew, as retail employment initially jumped up before reverting to pre-entry levels. 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 in their local markets for labor, with negative consequences for workers.

Keywords: Walmart, monopsony, wage income, job loss, local labor markets
JEL: J2, J31, J42, R23

1 Introduction

Walmart has long been pointed to as the archetype of a low-wage firm which exercises harmful labor market power. Yet prior research has disagreed about how to satisfactorily identify the firm's impact on local labor markets, leading to disharmonious estimates of the effects (Basker, 2005, 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 the sharp rise in income inequality over the same period has been driven largely by growing labor-income inequality (Hoffmann et al., 2020), 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 first by exploiting the rollout of Walmart Supercenters across the U.S., showing that Supercenter entry into counties had effects on local labor markets that cannot justifiably be described as 'competitive'. I find that entry caused large and significant declines in overall local employment, earnings, and labor force participation, while the positive shock to retail employment was not long-sustained. I then show that an exogenous minimum wage increase yielded large employment gains where Supercenters were already present, again inconsistent with competitive labor markets. These results admit negative spillover effects on local labor markets that extend well beyond a Supercenter's walls. My results 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 Azar et al. (2019).

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. 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. Or, 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. Such 'monopsonist' employers 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

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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, 2019; Ben-Michael et al., 2021). 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 and event-study 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., 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. More significantly, as I demonstrate in Wiltshire (2021b) and unlike the estimators proposed by Sun and Abraham (2020); Callaway and Sant'Anna (2020); De Chaisemartin and d'Haultfoeuille (2020) and Borusyak et al. (2021), my estimating strategy returns unbiased estimates even when conditional pre-trends are non-linear and heterogeneous across cohorts—though at the cost of being much more computationally burdensome.

Crucially, I offer a novel solution to address the significant 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 on those unobservable variables Walmart may use to identify locations it wishes to enter.

Walmart is the natural choice to focus on when looking for evidence of monopsony power that exacerbates income inequality. This is because the firm has long been by far the largest private-sector employer in the United States, and because its employees have long earned particularly low wages. Between 1990 and 2005 Walmart added 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 smaller "Discount" stores—especially grocery. 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% 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: even today, self-imposed minimum wages at Amazon, Costco, and Target are $15/hour or more, while that at Walmart stores is just $11/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 shrinkage 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), leading some smaller local retailers to exit (Basker, 2005; 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 (2005), 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 (2005) found local retail subsector employment slightly increased with Walmart entry, with the increase halving over the next five years.[1] 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 (2005)'s instrument (planned entry). Basker

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

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(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, it is often passingly cited as a *potential* monopsonist (Naidu et al. (2018); Azar et al. (2019); Kahn and Tracy (2019); Dube et al. (2020)). Yet a recent working paper from Derenoncourt et al. (2021)—which finds positive cross-employer wage elasticities when Walmart and others raised their self-imposed minimum wages—is to my knowledge the only study other than this one to provide any direct evidence that Walmart's wage-actions affect local labor markets.

Employment and earnings 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., 2019, 2020; Kahn and Tracy, 2019; Dube et al., 2020; Rinz, 2020).

In such cases, interventions that might otherwise be distortionary may actually return the local labor market to efficiency: while plenty 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., 2019; Cengiz et al., 2019; Dube and Lindner, 2021). Monopsony power has often been invoked to explain these latter results (Card and Krueger, 1995; Naidu et al., 2018; Azar et al., 2019). 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. (2019) suggest, the sign and magnitude of minimum wage employment effects may largely boil down to whether local employers exercise labor market power.

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This suggests a basic framework within which any labor market effects of Supercenters can be interpreted: if local labor markets remain competitive when Supercenters enter, then we should see non-negative effects on local employment—because of the accompanying positive shock to labor demand and productivity, and associated spillovers. Even if average earnings fall, labor markets may still be competitive provided entry simply brings new workers into the employed labor force at below-average wages. Moreover, any subsequent, binding, exogenous increase in the minimum wage should distort the price of labor above MRPL, and cause employment to fall. Conversely, if Supercenters exercise monopsony power, then we could expect entry to cause local employment, earnings, and labor force participation (LFP) to all *fall*, and employment to become more concentrated. Moreover, in such non-competitive labor markets, a binding, exogenous minimum wage increase could be absorbed with null or even positive local employment effects.

This paper contributes to the economic literature in three ways. First and foremost, I provide specific evidence that the largest private-sector employer in the U.S. 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 incongruous set of results in that literature, demonstrating that 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, and consider the impact on the entirety of an affected labor market rather than restricting the analysis to retail and occasionally other sectors. Third, I add to the minimum wage literature by demonstrating that binding minimum wage increases led to employment gains in labor markets where these low-wage monopsony employers were present. This affirms a key takeaway from Azar et al. (2019), 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. I also offer suggestive evidence that Walmart's ability to exercise this labor market power may have been exacerbated by the availability of the EITC, at great public expense.

This paper proceeds as follows. Section 2 outlines how large retailers might impact local labor markets. Section 3 briefly Walmart Supercenters, their labor needs, their remunerative practices, and the firm's workforce. 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 (Burdett and Mortensen, 1998; Bhaskar and To, 1999; Manning, 2003a) and applied research (Katz and Krueger, 1992; Card and Krueger, 1994, 1995; Azar et al., 2020, 2019; Kahn and Tracy, 2019; 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.[2] 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) for a thorough recent review of the literature)[3]—all of which Manning (2003b) argues essentially boil down to labor market "thin"-ness (or employer 'concentration') in one or more dimensions.

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).[4] 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

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[2]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.

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

[4]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 (Bartik et al., 2020; Barrero et al., 2021).

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 squeezes local suppliers to cut costs (e.g. if it accounts for a large proportion of its suppliers' sales and thus wields some power over its supply chain),[5] and if its sourcing practices redirect 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.[6] This is not only because local multipliers (Moretti, 2010) are likely to be smaller, but also because the entry of such a firm 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 a degree of monopsony power. Thus 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.[7] More recently, Azar et al. (2019) find evidence of monopsony power by looking at employment effects of minimum wage increases in more concentrated occupational labor markets, measured by job vacancies posted online. Numerous recent studies have found non-negative or only

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

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

[7]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) suggests a re-think may be in order.

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; Azar et al., 2019; Cengiz et al., 2019; Dube and Lindner, 2021)—though some of these research designs and results are contested (Neumark et al., 2014; Neumark and Shirley, 2021). Analysis of Seattle's 37% minimum wage increase over nine months did find negative employment effects (Jardim et al., 2017), though not among food-service workers (Reich et al., 2017). Finally, in perhaps the most relevant recent paper on minimum wage increases in the U.S., Cengiz et al. (2019) find 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.[8][9] In fact, 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.

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

[9]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 Supercenters

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 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: Supercenters employed "350 or more Associates on average" (Wal-Mart Stores, Inc., 2006a).[10]

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., 2006b). 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 realized anew each year. At 350 employees per store and 69% average annual turnover, the average Supercenter

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

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.[11] 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.[12] 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).[13] 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, in the U.S. Walmart currently pays its workers a voluntary minimum wage of $11 per hour, while Amazon, Target, and Costco—some of Walmart's largest competitors—guarantee their workers

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[11]Covers active employees with ≥ 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.

[12]Walmart's 2005 Equal Employment Opportunity Commission EEO-1 report also shows the firm's store employees are disproportionately comprised of minorities and women. Appendix Figure A6 presents the main table from that report.

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

a minimum of \$15/hour—a 36% premium.[14] 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.[15] However, if Supercenter entry introduces a degree of monopsony power into previously competitive labor markets then, as discussed earlier, 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 by Walmart squeezing its local suppliers to cut costs (Bloom and Perry, 2001; Wilmers, 2018) (a consequence of monopsony power over suppliers), and through consequent diversion of local market share away from local suppliers of incumbent competitors and to its own national and international suppliers, which may lead local goods-producers to reduce employment and wages.

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, and the non-tradeable (or local) nature of brick-and-mortar retail services, 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., 2019; Kahn and Tracy, 2019; 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.[16]

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[14]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 this proportion, though it is not clear where the estimates in Cascio (2006) come from.

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

[16]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.

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 employment and earnings, in aggregate and for service-providing sectors and goods-producing sectors each as a whole, and also for retail trade specifically. I additionally consider the effect on aggregate labor force participation and local employment concentration, and examine whether the availability of the Earned Income Tax Credit may have amplified the earnings effects. I also consider whether these effects are greater in non-urban counties, where labor markets are thinner and monopsony power is more likely.

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). Thus all in-sample 'treated' and 'control' counties here experienced the binding federal minimum wage increases, while treatment is, here, 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 (2005); Dube et al. (2007) and Neumark et al. (2008),[17] 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.[18] 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 number of recent papers (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., 2021) have made clear that additional caution should be exercised with the 'Supercenter entry' treatment, given entry occurred at different times for different counties. Common research designs when using panel data with such staggered treatment adoption involve OLS or IV regression with unit and time fixed effects (FEs)—referred to as 'difference-in-differences' (DD) or 'event study' designs, depending on whether treatment effects are modelled as dynamic. Many such designs use a 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. Event periods $e < l \leq 0$ before or $e > m > 0$ after treatment may be binned, and β_{-1} is always excluded. 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.[19] 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 DD estimator with staggered adoption is in fact a weighted average of all 2×2 DD estimators, meaning that already-treated cohorts can serve as controls for other treated cohorts. When treatment effects are dynamic (change over time), 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

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

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

[19] I present naive estimates from various specifications of Equation 1 in Appendix A.7. They deviate significantly from estimates recovered using the stacked-in-event-time synthetic control estimator.

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 DD/event study 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.[20]

In Wiltshire (2021b) I present simulations which demonstrate these issues and compare the performance of the various estimators. I first show how, when treatment adoption is staggered across units and treatment effects are heterogeneous across cohorts and event years, naive TWFE event study estimators generally yield biased estimates of the average treatment effect on the treated (ATT). I then demonstrate that the alternative estimators designed to address this are unbiased for the ATT when conditional pre-trends are linear and common across cohorts.[21] Finally, I show that these estimators are all *biased* for the ATT when conditional pre-trends are non-linear or non-common across cohorts (violating the parallel pre-trends assumptions).[22] In contrast, I show that even when conditional pre-trends are non-linear and non-common across cohorts, the stacked-in-event-time synthetic control estimator (described below) yields unbiased estimates of the ATT.[23]

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 the now well-known synthetic control estimator to accommodate many treated units (Cavallo et al., 2013; Acemoglu et al., 2016b; Abadie and L'Hour, 2019; Peri et al., 2021) and staggered treatment timing (Dube and Zipperer, 2015; Ben-Michael et al., 2021). Intuitively, the synthetic control estimator constructs 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).[24] Given a 'donor

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

[21]I also show that a TWFE event study estimator for the ATT has minimal bias when pre-trends meet these conditions—provided there is an untreated or not-yet-treated cohort of control units and the treated cohorts are balanced in post-treatment event time.

[22]The one exception to this is that when conditional pre-trends are non-common across cohorts but are known to be linear, the De Chaisemartin and D'Haultfœuille (2020) estimator with specified linear cohort trends is unbiased for the ATT. However, this specification of this estimator is the *most* biased when conditional pre-trends are non-linear.

[23]This comes at the cost of being much more computationally demanding than the other estimators.

[24]Unlike k-nearest neighbor matching estimators, Abadie and L'Hour (2019) 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 estimators, however, standard synthetic control weights are sparse, non-negative and sum to one, which ensure against extrapolation bias and

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 dynamic 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 political 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 (2019); Abadie (2021) and described below. For each outcome and each treated county, I estimate bias-corrected ATTs, then average these estimates by event year across all treated counties.

4.2.1 Formal Setting

Formally, I observe data for a total of $I+J$ counties, with treated counties $j = 1, ..., I$, and a carefully selected set of untreated "donor pool" counties $j = I+1, ..., I+J$, some combination of which are comparable to each treated county, and all of which are plausibly *not* selected into non-treatment based on unobservables correlated with outcomes of interest. I observe all counties through through calendar year $t = T$. Each treated county $i = j \leq I$ is observed a strictly positive number of years prior to treatment. Treatment occurs in calendar year $t = T_{0i} + 1 \leq T$, which can vary over the treated variables.[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. The $k \times J$ matrix $\mathbf{X}_0 = [\mathbf{X}_{I+1}, ..., \mathbf{X}_{I+J}]$

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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 and do not require sparsity.

[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.

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

contains the values of the predictors for the donor pool counties. For each $\{j,t\}$, define Y_{jt}^N as the potential outcome if j is $\{N\}$ot treated, and Y_{it}^{Int} as the potential outcome if j receives an $\{Int\}$ervention (is treated) and is observed in $t > T_{0i}$ (again, $i = j \leq I$). The marginal treatment effect of interest for i in $t > T_{0i}$ is:

$$\tau_{it} = Y_{it}^{Int} - Y_{it}^N \tag{2}$$

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

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

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 1 in T_{0i}, so as to make τ_{ie} comparable across all i. The ATT of interest in e, and its estimator, are defined in Equations (4) and (5):

$$\overline{\tau}_e = \sum_{i=1}^{I} \gamma_i \tau_{ie} = \sum_{i=1}^{I} \gamma_i (Y_{ie} - Y_{ie}^N) \tag{4}$$

$$\hat{\overline{\tau}}_e = \sum_{i=1}^{I} \gamma_i \hat{\tau}_{i,e} = \sum_{i=1}^{I} \gamma_i (Y_{ie} - \hat{Y}_{ie}^N) \tag{5}$$

with weights $\gamma_i \geq 0 \ \forall \ i$ and $\sum_i^I \gamma_i = 1$ on the treated counties.[27]

The classic synthetic control estimator for Y_{it}^N 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:

$$\hat{Y}_{it}^N = \sum_{j=I+1}^{I+J} \hat{w}_{j,i} Y_{jt} \ \forall \ t \tag{6}$$

These weights, $\hat{\mathbf{W}}_i = (\hat{w}_{I+1,i}...\hat{w}_{I+J,i})'$, 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.[28] That is, given $v_{1,i}, ..., v_{k,i}$, the synthetic control estimator for i selects $\hat{\mathbf{W}}_i$ to minimize:

$$\left(\sum_{h=1}^{k} v_{hi}(X_{h,i} - w_{I+1,i}X_{h,I+1} - ... - w_{I+J,i}X_{h,I+J})^2 \right)^{1/2} \ \text{s.t.} \ \sum_{j=I+1}^{I+J} w_{j,i} = 1, \ w_{j,i} \geq 0 \ \forall \ j \in \{I+1, ..., I+J\} \tag{7}$$

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[27]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.

[28]I use the regression-based method of selecting the $v_{h,i}$ weights, described in Kaul et al. (2015).

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 (this removes at most one donor county from each donor pool), 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 (2019)—analogous to that explored in Abadie and Imbens (2011) for bias-correction in matching estimators. Specifically, for each i I first determine $\hat{\mathbf{W}}_i$ from synthetic control estimation on the uncorrected outcome values. Let $\mu_i(x) = E[Y|X = x, D = 0]$ where D is a dummy for treatment and the expectation is over the donor pool for i. Let $\hat{\mu}_i(x)$ be an estimator of $\mu_i(x)$, estimated using OLS regression of the outcome variable in each $t \leq T$ on the complete set of predictor variables. I can then calculate $\tilde{Y}_{it} = Y_{it} - \hat{\mu}_{it}(X_i)$ and $\tilde{Y}_{it}^N = \sum_{j=I+1}^{I+J} \hat{w}_{j,i}(Y_{jt} - \hat{\mu}_{it}(X_j))$, and convert these to event time.[29] I provide a Stata package, `allsynth`, to automate implementation of this procedure (Wiltshire, 2021a). The estimator of the bias-corrected average treatment effects on the treated, of Supercenter entry, for each e, is then:

$$\hat{\bar{\tau}}_{BC_e} = \sum_{i=1}^{I} \gamma_i \hat{\tau}_{BC_{ie}} = \sum_{i=1}^{I} \gamma_i (\tilde{Y}_{ie} - \tilde{Y}_{ie}^N) \tag{8}$$

Each $\hat{\bar{\tau}}_{BC_e} \times 100$ is an estimate of the percentage average treatment effect on the treated in event year e. Note this setup nests the case of the "classic" synthetic control estimator for τ_{1t} with a single treated unit, $I = 1$.

4.3 Inference

Test statistics based on permutation tests are the current standard for synthetic control inference, but the literature is rapidly evolving. 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

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[29] Appendix Figure A8 shows the effects of the bias-correction procedure on canonical results from Abadie et al. (2010).

et al., 2013; Abadie et al., 2015; Dube and Zipperer, 2015; Ferman and Pinto, 2017; Firpo and Possebom, 2018; Abadie and L'Hour, 2019), while an additional series of papers consider alternatives (Doudchenko and Imbens, 2016; Hahn and Shi, 2017; Chernozhukov et al., 2019; Zhang, 2019).

For each outcome I 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 (2019); Abadie (2021). 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 RMSPE 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 RMSPE may be significantly under-powered.[30] Both identify tests in the spirit of Andrews (2003) as preferable alternatives—though here this test might be under-powered with at most 10 pre-treatment years. Chernozhukov et al. (2019) propose an alternative approach based on *moving block* permutations, such that a null hypothesis of no effect can be rejected if the test statistic for the actual synthetic control estimate is larger than the great majority of those based on moving-block permutations. Given the uncertainty around which test is best, I implement all three and present the results along with plots of the in-space permutation distributions. Details are in Appendix A.6.

5   Data

Data cover 1990 to 2005. I take earnings, employment, and establishment figures from the Quarterly Census of Employment and Wages (QCEW). These data, from the U.S. Bureau of Labor Statistics (BLS), are fed

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[30]As 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.

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 political 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 A4. 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.[31]

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 A4). 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 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 583 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 542–581 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 Figures 4 and 5, for visual comparison. 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 mean. Median county population in the donor pool is approximately 50% larger than in the treated sample, but the two groups are broadly similar in all per-worker/-establishment terms, rates, and retail share of total employment, as well as retail HHI. By contrast, the median county in the excluded untreated sample has one quarter 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

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[31]See Appendix A.2 for further discussion of the donor pool counties.

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. The data are (mostly) collected by the firm in the course of its business as a credit reporting agency. The primary advantage of these data is the level of granularity they provide: establishment level observations of employment permit calculation of one measure of labor market concentration. The data are discussed at length in Howland et al. (1982) and Carlson (1995).[32] 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.[33] Appendix Figure A7 compares national DNB and QCEW employee counts by year.

I take minimum wage data from the Tax Policy Center. For the 'minimum wage increase' treatment I restrict the sample as described in Section 4. This yields 25 donor pool counties and 176 to 193 treated counties for this treatment, depending on the outcome. Additional data are described in Appendix A.1.

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[32]See, also, Carlton (1983). The DNB data 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.

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

6 Results and Discussion

6.1 Impact of Supercenter Entry on Labor Market Outcomes

I begin by estimating the effect of Supercenter entry on local employment and earnings. For each outcome, Figure 8 and Figure 10 plot the event-year paths of the estimated average treatment effects on the treated (ATT) against the (sample) distribution of 1,000 placebo average effects. Estimates are all relative to the event year preceding treatment and have the bias-correction procedure applied. Effects are estimated in percentage terms, though I also use these to simulate median treated-county effects in level terms.

Panel A of Figure 8 shows Supercenter entry caused aggregate county employment fell steadily and sharply in the years following Supercenter entry. At -2.9%, the ATT by the fifth year after entry is more extreme than every placebo average. This is equivalent to a median simulated county-wide employment loss of 613 workers by the fifth year after entry. Panel A of Figure 9 shows this ATT was broad-based across cohorts, and not just the result of a few treatment years. Column (1) of Table 3 shows the each of the three *p*-values are 0.1 or smaller. Given the construction of the Andrews *p*-values and the fact that there are at most 10 pre-treatment periods, a $p_{Andrews}$ of 0.1 or smaller indicates a high degree of significance. This decrease in aggregate employment came even as retail employment increased 2.2% in the year of Supercenter entry, when Walmart began hiring to staff the Supercenter—though this gain nearly halved by the fifth year after entry (it remained 1.4% higher, Panel B and column (2)). Equivalent to a long-run gain of 36 retail jobs in the median county, this retail employment pattern is very similar to that found by Basker (2005). With these local employment patterns, the share of aggregate employment working in the retail sector grew 3.5% five years after Supercenter entry (Panel C and column (3)). Finally, Panel D and column (4) show the labor force participation rate had fallen 1.6% lower five years after Supercenter entry, with the decline beginning in the third year after Supercenter entry. Panels C and D of Figure 9 show these last two ATTs were again generally broad-based across cohorts. The *p*-values broadly indicate the estimates are highly significant.

To summarize: Supercenter entry caused aggregate local employment to fall and workers to gradually exit the labor force. The employment losses were realized outside the retail sector, yielding greater local employment concentration in the retail sector. Unless Supercenter entry also boosted wages and induced a leisure-increasing income-effect, these results are inconsistent with competitive labor markets.

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In fact, Supercenter entry caused earnings to fall. Panel A of Figure 10 shows that Supercenter entry caused aggregate total local earnings to fall (-5.2% five years after entry), while Panel B of Figure 9 shows this ATT was broad-based across cohorts. The results are quantified in column (1) of Table 4. Retail total earnings, on the other hand, immediately increased 1.5% before entirely falling back to pre-entry levels (Panel B and column (2)). Given the decline in aggregate employment, it is not surprising that aggregate total earnings fell. As Panel C and column (3) show, however, they fell even in per-worker terms (-2.3% five years after entry, equivalent to a median simulated county-wide earnings loss of $758 per worker by the fifth year after Supercenter entry.), meaning Supercenter entry reduced overall employment and also reduced earnings among the consequently smaller share of individuals who were employed.

These are large effects realized outside the retail sector. To clarify where they come from, I estimate the employment and earnings effects for goods-producing industries and for service-providing industries, each as a whole.[34] Figure 11 and Table 5 present the results. The estimated effects on employment in goods-producing and service-providing industries five years after entry are -4.5% and -7.5% respectively, each significant according to all three tests. As a reminder, this occurred even as staffing a new Supercenter represented 2.5% and 20% of average pre-entry median aggregate and retail employment, respectively, and as total hiring to keep them staffed through five years following entry (given 69% average annual turnover) respectively represented 11% and 88% of the same. The point estimate of the effect on total earnings in goods-producing industries is -4.9%, significant according to all three tests, while that in service-providing industries is -6.7%, significant according to the Andrews and Moving Block *p*-values (though not the RM-SPE *p*-value). These point estimates are all somewhat larger in absolute terms than the overall effects. This is because their synthetic controls are matched according to employment in each industrial agglomeration rather than overall, which means they are better counterfactuals for counties where employment is dispro-portionately concentrated in one or the other type of agglomeration, compared to the overall estimates (as a reminder, the effects are the weighted-average estimated effects across all treated counties). As Figure 11 shows, there is also greater variance in the distribution of average placebo effects compared to the graphs for overall employment and overall total earnings, which likely contributes to the larger point estimates.

Such large effects across local economies demonstrate the wide impact of monopsony exercised by

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[34]As noted earlier, suppressions become a concern for many industries individually in the QCEW data, but the issue is negligible in aggregate, for the retail sector, and for the 'goods-producing' and 'service-providing' agglomerations of industries.

Supercenters. This is, perhaps, not all that surprising to see in service-providing industries as Walmart Supercenters offer non-traditional retail services (e.g. tire and lube, photo processing, basic financial services) as well as grocery and general merchandise, they may depress labor demand from incumbents competing in those other sectors, reducing the arrival rate of job offers from those service-providing firms.

The more-suprising effects on goods-producing industries likely come from Walmart squeezing its own local suppliers to cut costs such that they reduce wages and employment among their own workforces (e.g. Bloom and Perry (2001); Wilmers (2018)), and even moreso from the negative demand shocks faced by local manufacturers who supplied incumbent local retailers: as Supercenters captured local retail market share, Walmart's suppliers would have displaced local producers, with consequent local labor market impacts 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 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. This would have depressed the labor demand of local producers, giving Supercenters an even larger share of local demand for less-skilled labor, and exacerbating any negative impact on local multiplier effects (e.g. Moretti (2010). It would also help explain the magnitude of the aggregate estimates.

The employment and earnings results together tell a consistent story of Walmart Supercenters exercising monopsony power. As a robustness check, I re-estimate key outcomes using only non-urban (NU) treated and donor pool counties, where labor markets are thinner and monopsony power is more likely. Estimation using the same 10 predictor variables is still possible as there remain 15 NU counties in the donor pool, but the RMSPE becomes extremely under-powered due to the substantially greater likelihood that an actually-treated unit is positively weighted as a donor in the placebo runs. Table 6 presents the results for NU aggregate employment, aggregate total earnings, aggregate per-worker earnings, and per-capita EITC

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receipts in columns (1)-(4) respectively. In each case the treatment effects show the same pattern and are approximately as large or larger (in absolute terms) than when including urban counties. The Andrews and moving block p-values all indicate the results are significant.[35]

An additional way to measure labor market concentration is to consider the county retail employment-level Herfindahl-Hirschman Index (HHI) based on firm shares of a market. Notwithstanding the difficulty of determining how a market should be delineated, HHI is a common measure of market concentration, where s_i is firm i's share of some market, and $HHI = \sum_{i=1}^{N} s_i^2$. An ideal measure here 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 active labor market involvement. [36] This may 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, and estimate the impact of Supercenter entry as above.[37] I present the results in Figure 12 and Table 7. Five years after Supercenter entry, local retail employment level-concentration HHI increased 15.9%, on average (33.7% in non-urban counties). For the full sample estimate, all three p-values indicate a high degree of significance, while the Andrews and moving block p-values indicate the significance of the NU results.[38]

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[35]As noted, the RMSPE tests for the NU county analysis are very under-powered with only 15 donor pool counties.

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

[37]I add covariates to control for aggregate and retail establishments.

[38]Again, the RMSPE tests for the non-urban county analysis are very under-powered with only 15 donor pool counties.

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

I next estimate the employment and earnings effects of the 1996/97 federal minimum wage increases on counties which already had Supercenters. As treatment adoption is not staggered here, I estimate both a TWFE DD estimator and a stacked-in-calendar-time synthetic control estimator (though, as Abadie et al. (2015) note, the DD estimator may still be subject to extrapolation bias).[39]

I first consider the DD estimates. Tables 8 and 9 present the results for employment and earnings respectively. The aggregate and retail estimates are in columns (1) and (2) for the full sample of counties, and columns (3) and (4) for only non-urban counties. The estimates are nearly all positive and significant, and highly consistent for both measures of percentage change of employment.[40] They also often grow with time. Over the five years following the initial increase (1996-2001), the estimated effect of boosting the minimum wage on both aggregate and retail employment was around 5% in the full sample of counties with a Supercenter, and nearly 4% in NU counties with a Supercenter. The estimated effect on aggregate earnings was 5-8% in the full sample of counties with a Supercenter (around 5% in NU counties with a Supercenter), and 4-5% on retail earnings in the full sample of counties with a Supercenter (3-5.5% in NU counties).

I next estimate the impact of the federal minimum wage increase through 2001 using a synthetic control approach with units treated simultaneously, using controls from the DD regressions and outcomes in 1990–1992 as predictor variables. This allows synthetic control estimation of the impact for the full sample only (the donor pool of non-urban counties is too small to use with covariates). Figure 13 and Table 10 presents the results. Five years after the minimum wage increase, aggregate employment had risen 4.5% in counties with a Supercenter, with most of the increase coming in 1997. This is slightly smaller than but generally consistent with the DD estimates, which may suffer from extrapolation bias. The retail sector estimates for employment and earnings are 8.7% and 10.1%, five years after the minimum wage increase. These are somewhat larger than the DD estimates, but this is unsurprising as the retail-sector effects appear to increase throughout the post-treatment period (Panels B and D), while the DD estimates are an average over the period and may suffer from extrapolation bias. The synthetic control approach shows no impact on aggregate total earnings, which contrasts with the significant positive estimates from the DD approach.

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[39]I describe the DD model estimation details in Appendix A.4.

[40]Retail earnings normalized to 1 in 1995 were not significantly higher in 1998. All other estimates were positive and significant.

Given the minimum wage is much more likely to be binding for workers in the retail sector than for most workers, a null aggregate earnings effect alongside a retail earnings increase seems more plausible, and the DD estimates should be viewed with some skepticism. Given the relatively short pre-treatment period (6 years), and given the relatively small number of donor pool counties given the restrictions on the sample (only from states which tracked the federal minimum wage over the period), both the Andrews and the RMPSE test may be under-powered. Nonetheless, the positive estimated effects are broadly significant using at least two of the three $p-$values (that on aggregate employment is 0.106), and the Andrews p-value are all 0.167—the smallest they could be without being zero, given only 6 pre-treatment periods.

As with the estimated effects of Supercenter entry, these results are not consistent with competitive labor markets but *are* consistent with Walmart Supercenters exercising monopsony power (and indicate a minimum wage increase weakens this power). The minimum wage was increased 21% over the two years, implying a 6-year aggregate employment elasticity with respect to minimum wage of 0.2 to 0.4 in the full sample of counties with a Supercenter, and from 0.15 to 0.25 in non-urban counties with a Supercenter. It is worth re-stating that these are estimated effects of the impact of minimum wage increases *when a Supercenter is present*; they may not reflect the impact in markets where labor market power is less evident.

6.3 Walmart Supercenter Entry and the Earned Income Tax Credit (EITC)

While Supercenter entry may have crowded out labor demand from incumbent local firms, giving Super-centers an outsized role in the market for less-skilled labor, it is also possible that the firm's ability to exercise monopsony power was exacerbated by the availability of the EITC. Conditional on numbers of dependent children and spousal income, the EITC schedule has a flat-topped pyramid shape: as earnings increase, the amount for which a filer is eligible increases linearly from zero before plateauing, then linearly decreases until it is completely phased out.[41] Bitler et al. (2017) looked at the distributional effects of the EITC during periods of increased unemployment and found the EITC effectively served as a cushion against negative income shocks for married couples with children, but not for single parents. Meanwhile, Leigh (2010) and Rothstein (2010) examined the incidence of the EITC and found that downward wage adjustments in response to EITC-induced labor supply increases allowed employers to capture a significant portion of the

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[41]I discuss the program in some detail in Appendix A.5. Figure A3 shows the 2001 EITC schedule, Walmart employee earnings from Table 1 shown to indicate the EITC amounts for which these workers would be eligible, conditional on numbers of children.

EITC incidence at the expense of less-skilled workers—especially ineligible single women. Workers who lost their jobs following Supercenter entry may thus have been willing to accept lower-wage jobs if they were eligible for an EITC that would partially replace any wage reduction. This would have increased the labor supplied at lower wages, allowing a monopsonist to set wages even lower and still attract enough workers (capturing some of the incidence of the EITC). A simple "One-stop shopping" model in Appendix A.5.1 illustrates how the EITC could exacerbate wage declines in the presence of monopsony power.

Panel D of Figure 10 shows the impact of Supercenter entry on per-capita EITC receipts. After staying constant in the year of entry (EITCs are claimed when filing taxes in the following calendar year), per-capita EITC receipts then rapidly begin climbing, and were 9.1% higher five years after Supercenter entry. This would also be expected if new workers entered employment in lower wage jobs, but I have shown that LFP actually declined following Supercenter entry. If this substantial growth in EITC receipts came from new workers entering employment in low-wage jobs after Supercenter entry, then even more higher-paid workers would have had to exit the labor force (as both aggregate employment and LFP fell). The concern about the RMSPE being under-powered becomes visually obvious here: the distribution of placebo averages is clearly negatively-skewed after treatment as the synthetic control units estimated for the placebo runs often gave substantial weight given to the actually-treated units as donors. Given the large positive effects seen in a great number of the individual counties, their inclusion as donors spuriously inflated the placebo treatment effects downward, which negatively skewed the permutation distribution of placebo averages, with the consequence being a reduction in statistical power. Nonetheless, all three *p*-values indicate statistical significance (column (4) of Table 4), with an RMSPE *p*-value of 0.0789.

Per-worker EITC receipts clearly increased sharply following Supercenter entry. I next calculate a back-of-the-envelope, lower-bound estimate of the total increase in EITC receipts associated with Supercenter entry. For each treated county I multiply total EITC receipts in the year before entry by the per-worker estimate for each event year over the period, and simulate a path of treatment effects in level terms. I sum these values across counties and event years to get a lower bound estimate of the aggregate added public cost of the EITC program associated with Supercenter entry over the period 1995-2005: $6.35 billion (PCE deflated, 2017 USD)—equivalent to more than 7% of Walmart's aggregate real net profit over the period.[42]

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[42]This neglects the impact of any additional Supercenters opened in counties which already had a Supercenter before 1995.

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. Specifically, I find Supercenter entry caused overall county employment to fall 2.9% and county earnings to fall 5.2% five years after entry. Retail's share of local employment increased, while labor force participation fell. I also 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. I utilize a stacked-in-event-time synthetic control estimator to estimate these effects, cruicially 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 plague difference-in-differences research designs 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. Availability of the Earned Income Tax Credit may have exacerbated Walmart's ability to exercise monopsony power. These 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, and increase local utilization of the EITC. 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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Figure 1:
Walmart Supercenters drove the company's growth and profitability
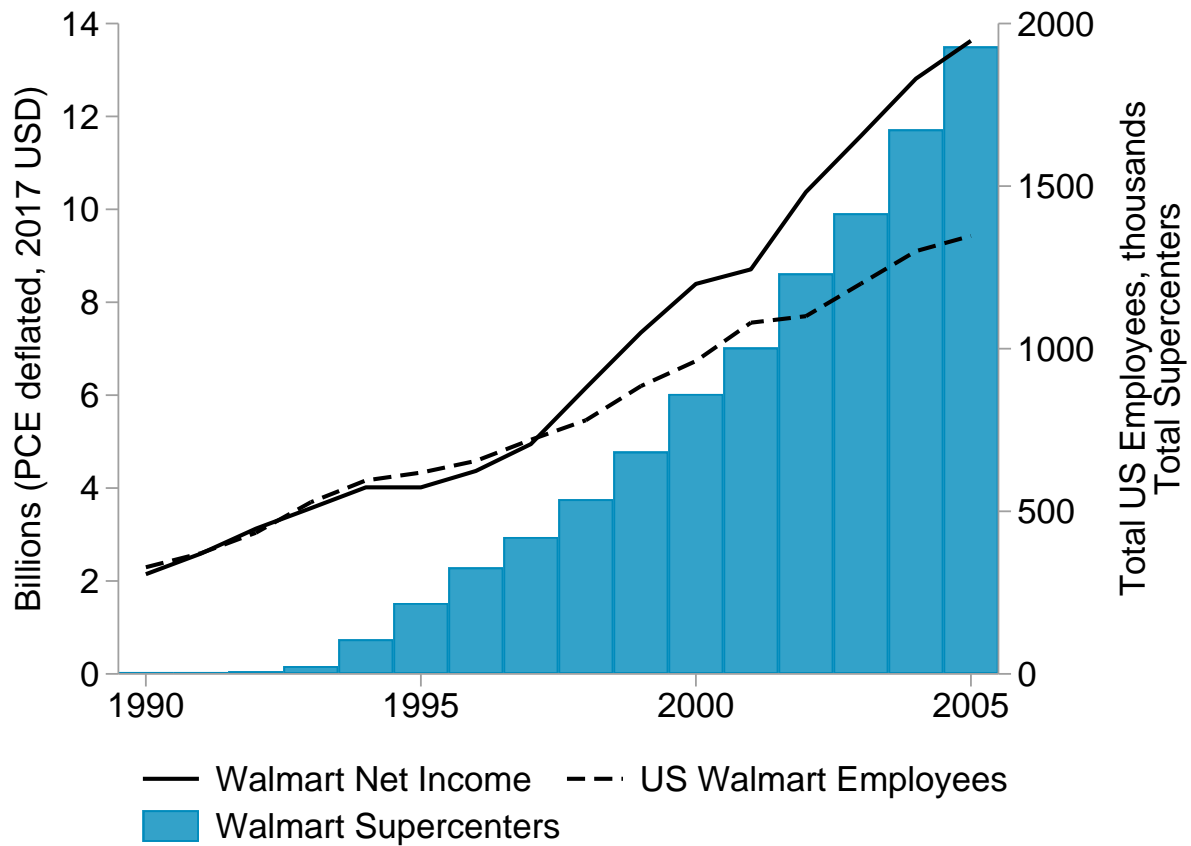
Note: Data from Holmes (2011) and Walmart Annual Reports, 1995-2006. Store numbers as of December 31 of each calendar year for the contiguous U.S., calculated from date provided by Holmes (2011). Employees and net income as of January 31 following each calendar year. Net income in PCEPI deflated, 2017 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

Panel A
Large firm enters, exercises monopsony power

Panel B
Minimum wage imposed on market characterized by monopsony power
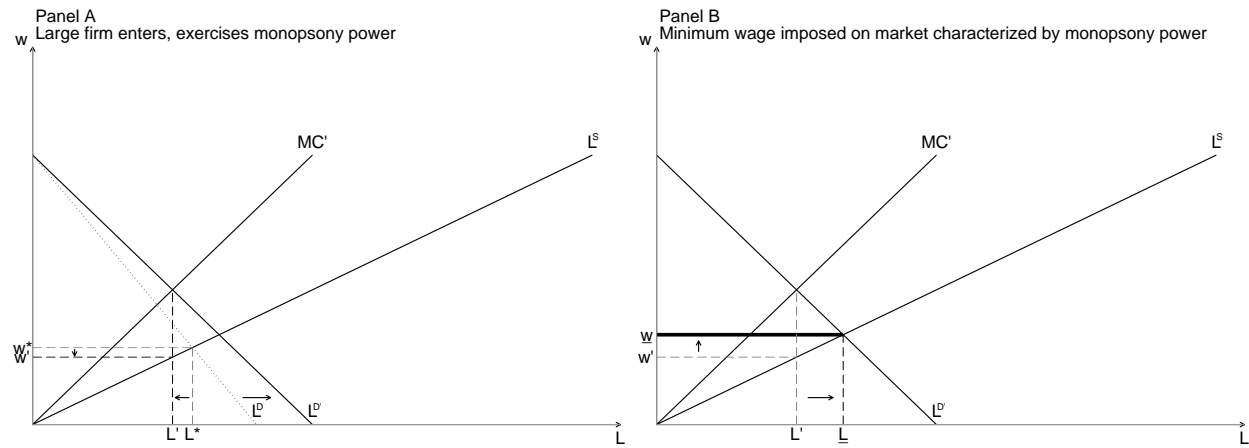
Note: Simulations from base model, presented in Section A.5.1. Panel A shows the difference in labor demand, wages and employment when a large firm enters and exercises monopsony power. Labor demand increases (L^D to $L^{D'}$, but the monopsonist's marginal cost curve is MC' instead of L^s, leading to 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

Panel A: Unrestricted Sample of Treated Counties
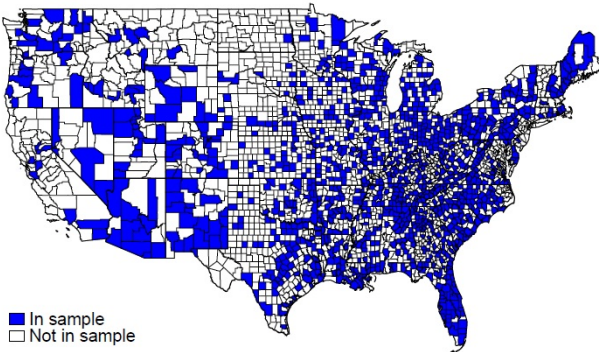
Panel B: Preferred Sample of Treated Counties

■ In sample
☐ Not in sample

■ In sample
☐ Not in sample
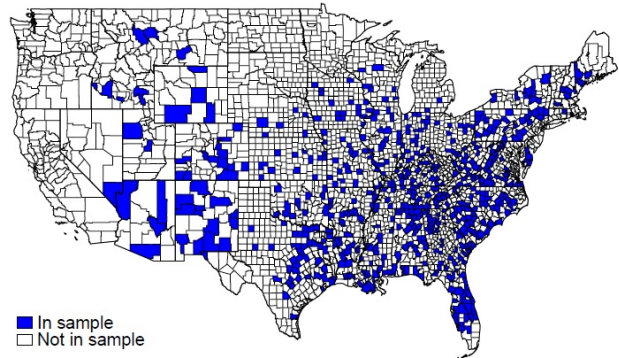
Panel C: Donor Pool of Untreated Counties
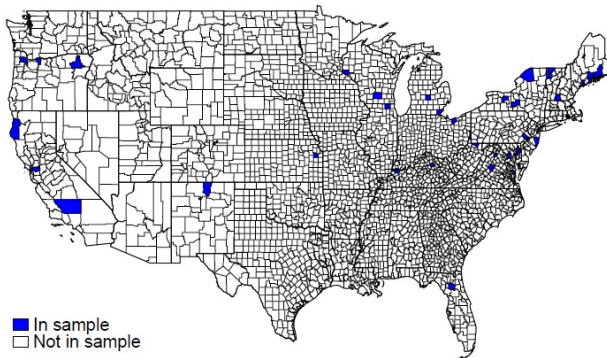
Panel D: States which tracked the federal minimum wage (1990-2003) and either had a county with a Supercenter pre-1996, or had a donor pool county

■ In sample
☐ Not in sample

■ Had a county in sample
☐ Did not have a county in sample
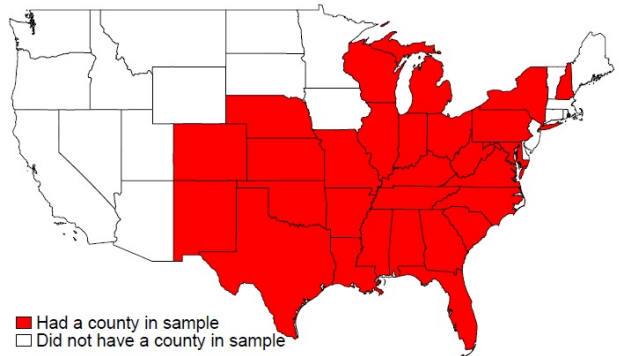
Note: Supercenter entry data from Holmes (2011). I use consistent 1990 county boundaries. I combine Virginia's independent cities with their surrounding counties, and the five New York boroughs into a single unit. Donor pool counties are those where Walmart tried to open a Supercenter over the period 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 542–581 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
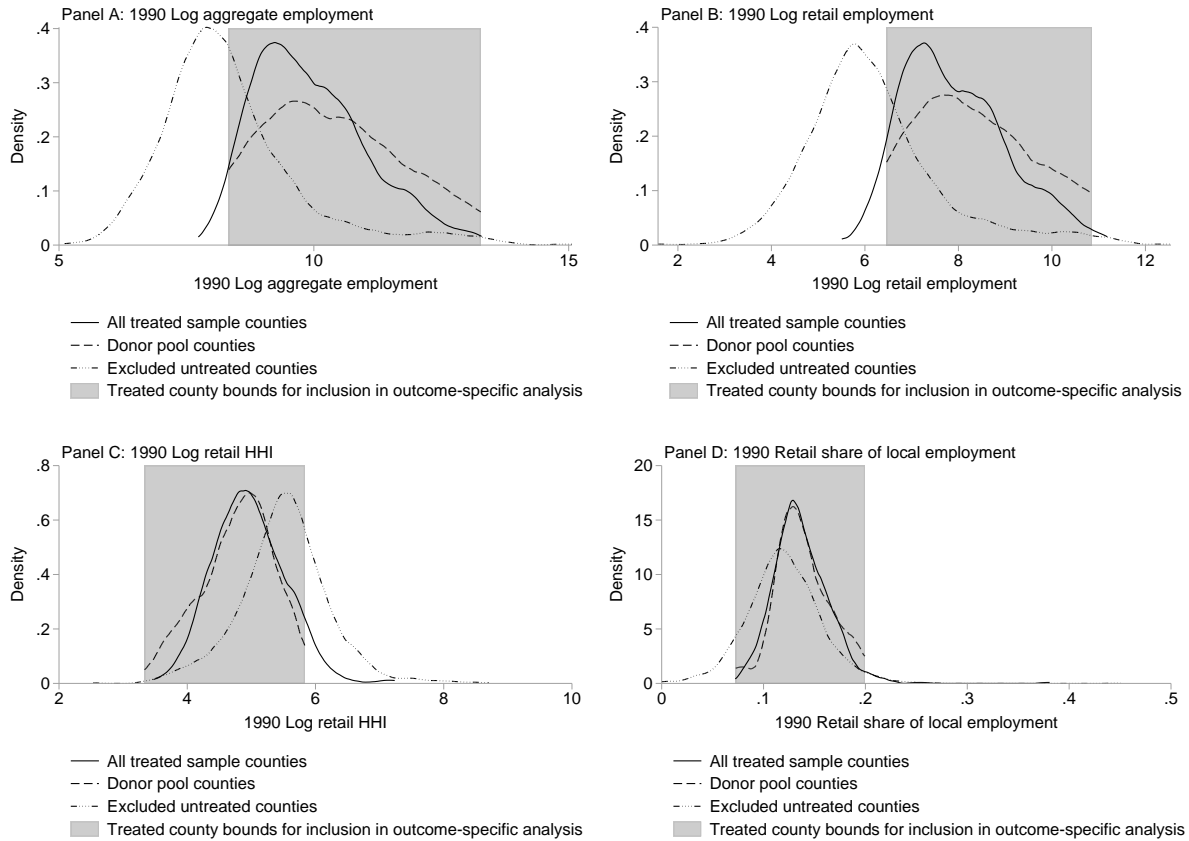
Note: Density plots of 1990 observations of select variables by sample (all 583 included treated counties, the donor pool, and excluded untreated counties). The indicated bounds on the treated sample (which determine inclusion in the outcome-specific analysis) are the minimum and maximum values in the 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 aggregate employment, retail employment, and retail HHI are all logged for ease of visual comparison.

Figure 5:
Density plots of 1990 observations of select variables (cont'd.)
Treated sample, donor pool, excluded untreated sample, and outcome-specific bounds for inclusion
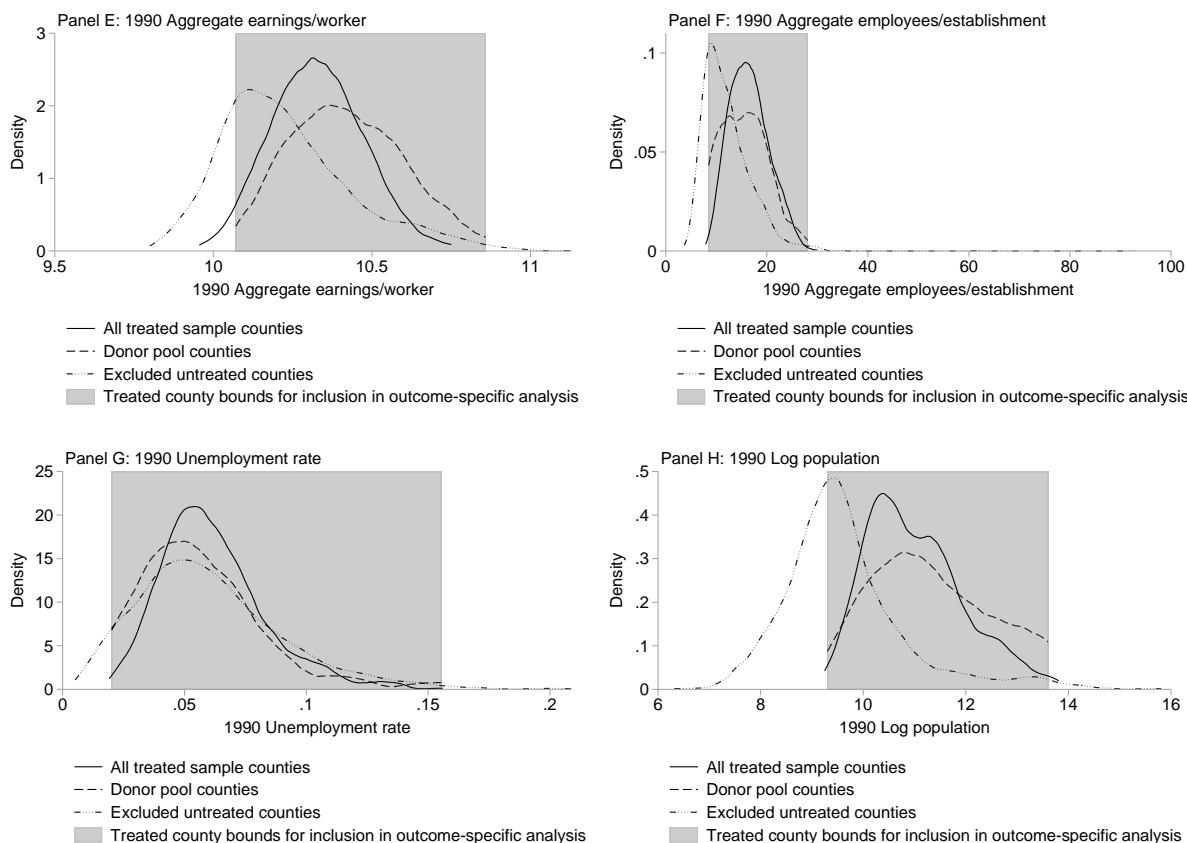
Note: Continuation of the visual comparison of samples begun in Figure **??**. Density plots of 1990 observations of select variables by sample (all 583 included treated counties, the donor pool, and excluded untreated counties). The indicated bounds on the treated sample (which determine inclusion in 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 aggregate per-worker earnings and population are all logged for ease of visual comparison.

Figure 6:
Step-by-step example of (bias-corrected) stacked synthetic control approach
Focus on aggregate employment in Vanderburgh County, IN (Supercenter entry in 1997)
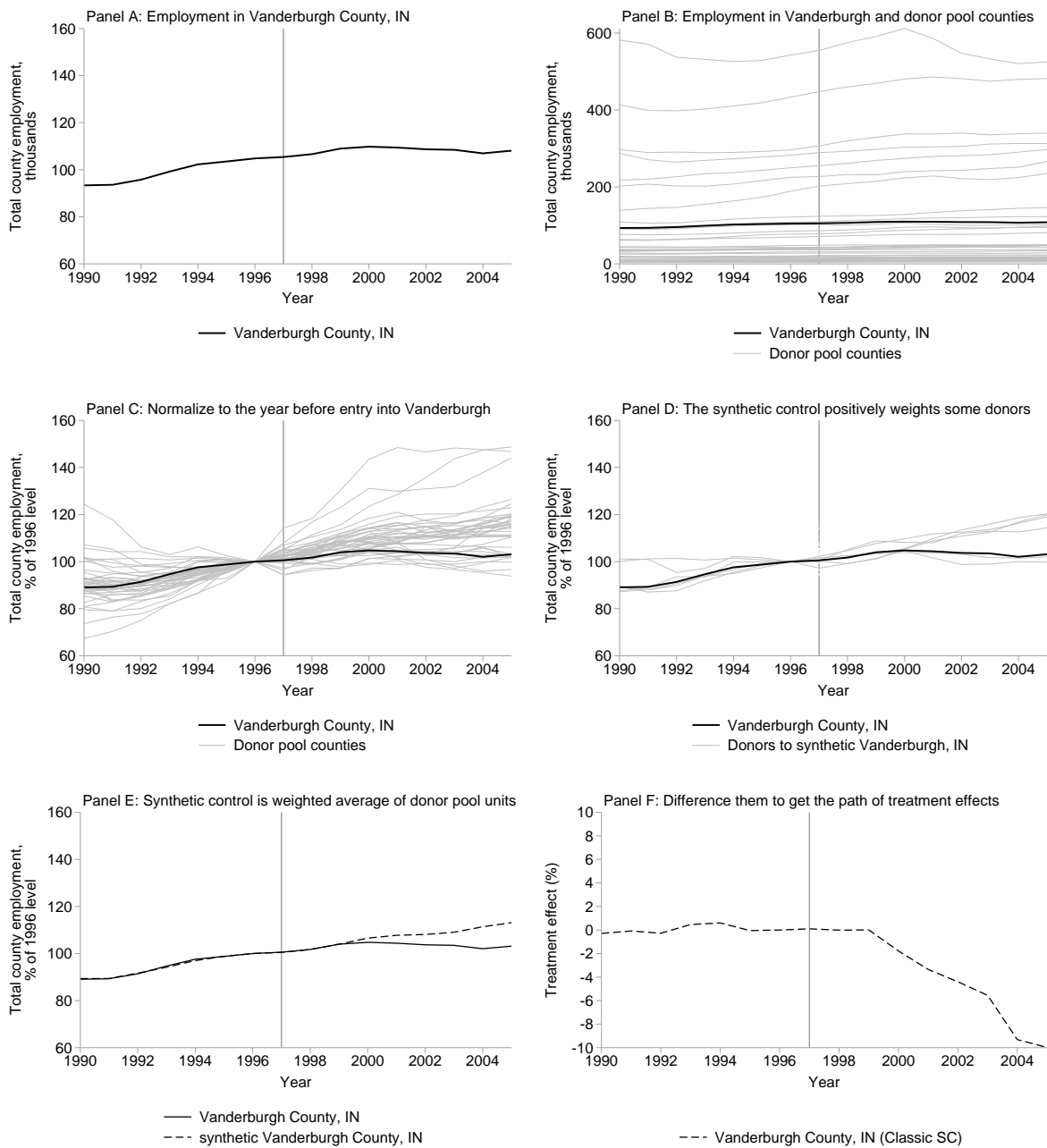
Note: Visualization of the synthetic control methodology continues in Figure 7. Panel A shows aggregate employment in Vander-bugh County, which received its first Supercenter in 1997; Panel B shows the same, also for donor pool counties; Panel C shows the same as Panel B, normalized to 100 in 1996 (the year before Vanderburgh County was treated); Panel D shows the same as panel C, but only for Vanderburgh and positively-weighted donors in Vanderburgh's synthetic control; Panel E shows aggregate employment in Vanderbugh 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 aggregate employment in Vanderburgh—the difference between the two lines in Panel E.

Figure 7:
Step-by-step example of (bias-corrected) stacked synthetic control approach (cont'd.)
From the effect on aggregate employment in Vanderburgh to the ATE across all treated units
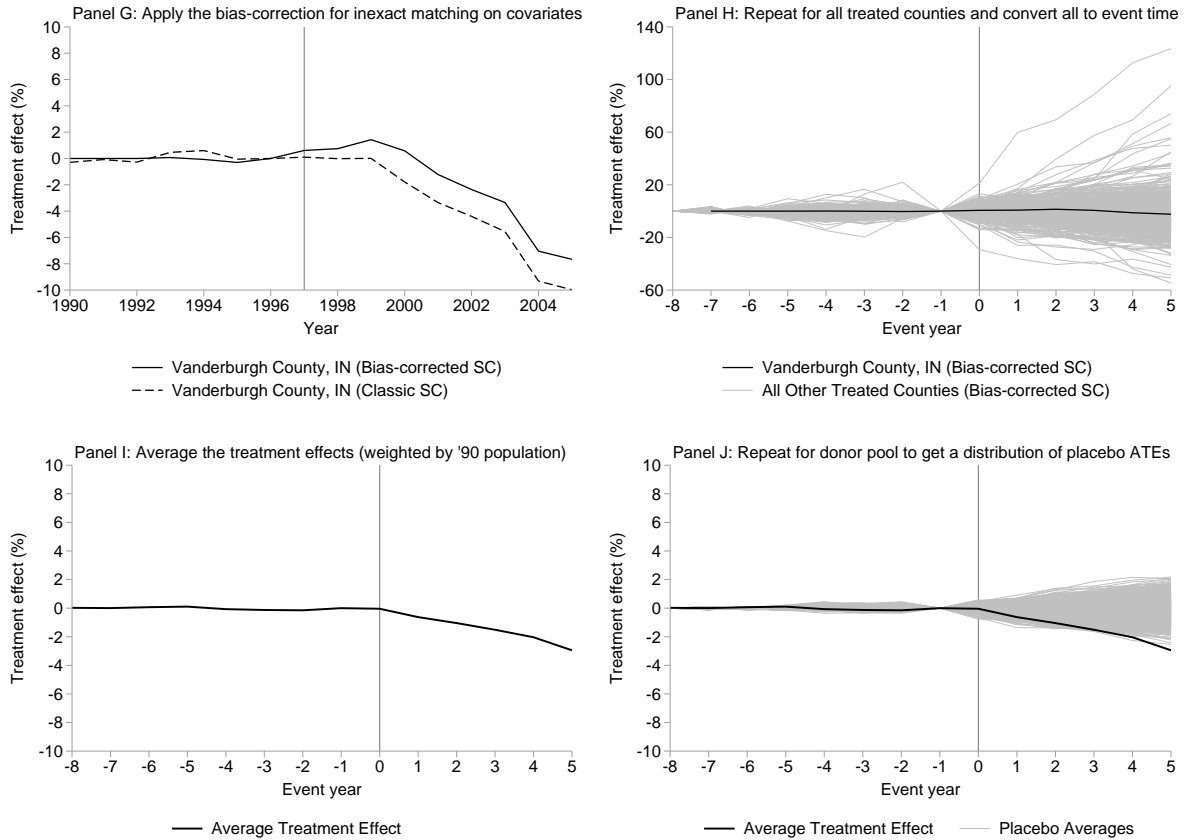
Note: Continuation of the visualization of the synthetic control methodology begun in Figure 6. This demonstrates the methodological process of the synthetic control approach, focusing on aggregate employment in Vanderburgh County, IN before repeating the process for all treated counties. Estimated using employment data from the QCEW and 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. Panel G shows the same as Panel F (Figure 6) 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 aggregate employment sample, converted to event time (event year 0 is the year of Supercenter entry); Panel I shows the path of average treatment effects (weighted by 1990 population), which is the estimand of interest; Panel J plots the ATE from Panel I against 1,000 randomly drawn placebo average treatment effects (from placebo treatment of the donor pool units).

Figure 8:
Employment and labor force effects of Supercenter entry
Synthetic control estimates

Note: Estimated using employment data from the QCEW and 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, dark line is the path of average treatment effects. The grey lines are paths of random samples of 1,000 placebo average treatment effects. All figures normalized to one in the 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. Panel A is the effect on aggregate employment; Panel B is the effect on retail employment; Panel C is the effect on the retail share of local employment; Panel D is the effect on labor force participation rate.

Figure 9:
Event-year 5 county and cohort-average treatment effects of Supercenter entry
Synthetic control estimates for select variables
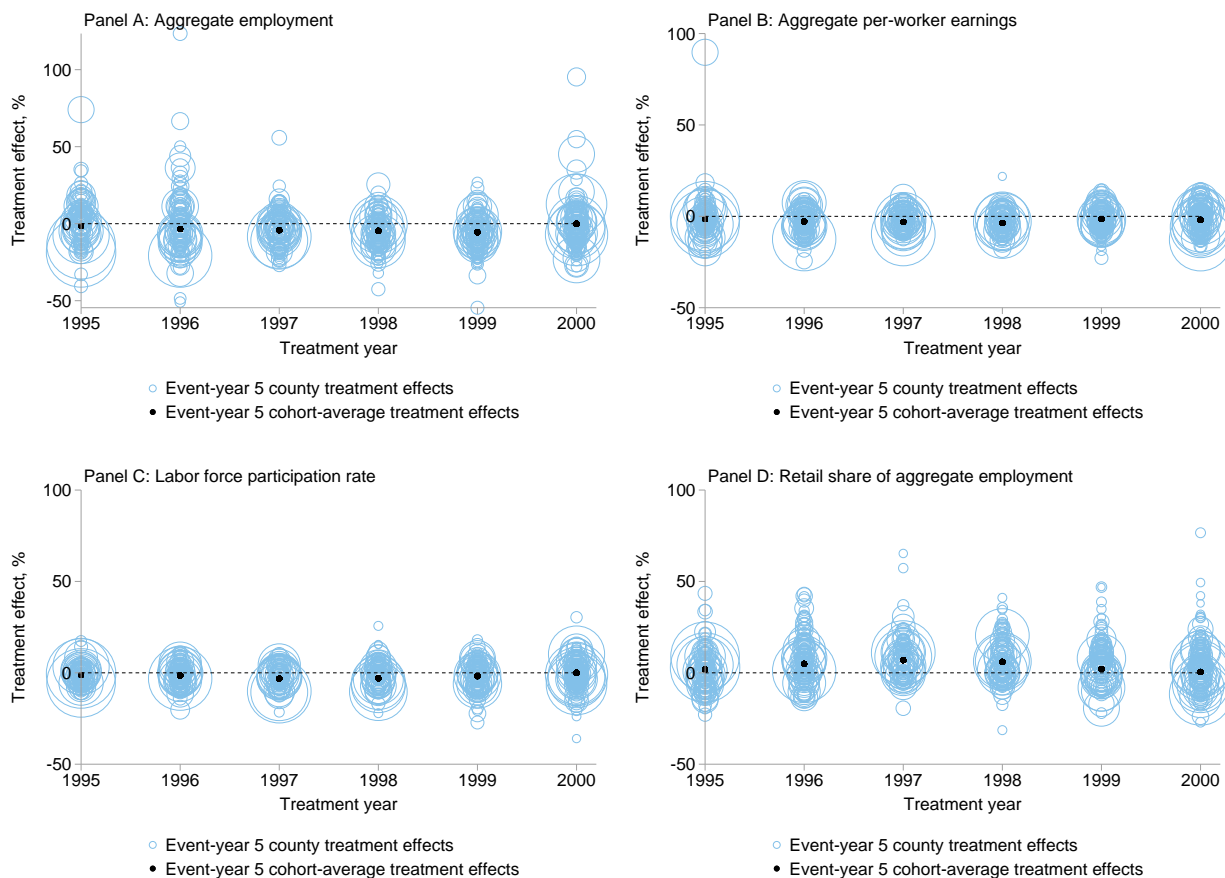
Note: Estimated using employment data from the QCEW and LAUS, as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains the 542–581 urban and non-urban 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-average treatment effects (weighted by the 1990 county population). Panel A shows these effects on aggregate employment; Panel B on aggregate per-worker earnings; Panel C on the labor force participation rate; Panel D the retail sector's share of local aggregate employment. The large, negative effect on Jasper County, IA retail employment share (in the 1999 cohort) is the result of an inexplicable level drop (of 3,300) in recorded retail employment in 1998. This may be due to measurement error as local aggregate employment did not follow suit, but if anything it biases the estimate toward zero (and is very noisy and trending upward in the pre-treatment period). A few similar apparent outliers in Panels A and B likewise would influence the estimates toward zero.

Figure 10:
Effects of Supercenter entry on earnings and EITC receipts
Synthetic control estimates

Note: Estimated using employment and earnings data from the QCEW and EITC receipts from BEA, 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, dark line is the path of average treatment effects. The grey lines are paths of random samples of 1,000 placebo average treatment effects. All figures normalized to one in the 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. Panel A is the effect on aggregate total earnings; Panel B is the effect on retail total earnings; Panel C is the effect on the aggregate per-worker earnings; Panel D is the effect on Earned Income Tax Credit receipts.

Figure 11:
Effects of Supercenter entry on employment and earnings
Goods-producing and services-providing industries
Synthetic control estimates

Panel A: Goods-producing industries, employment

Panel B: Services-providing industries, employment

Panel C: Goods-producing industries, total earnings

Panel D: Services-providing industries, total earnings

Note: Estimated using employment and earnings data from the QCEW, 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, dark line is the path of average treatment effects. The grey lines are paths of random samples of 1,000 placebo average treatment effects. All figures normalized to one in the 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. Panel A is the effect on aggregate total earnings; Panel B is the effect on retail total earnings; Panel C is the effect on the aggregate per-worker earnings; Panel D][=]s the effect on Earned Income Tax Credit receipts.

Figure 12:
Effects of Supercenter entry on retail employment-level concentration (HHI)
Synthetic control estimates
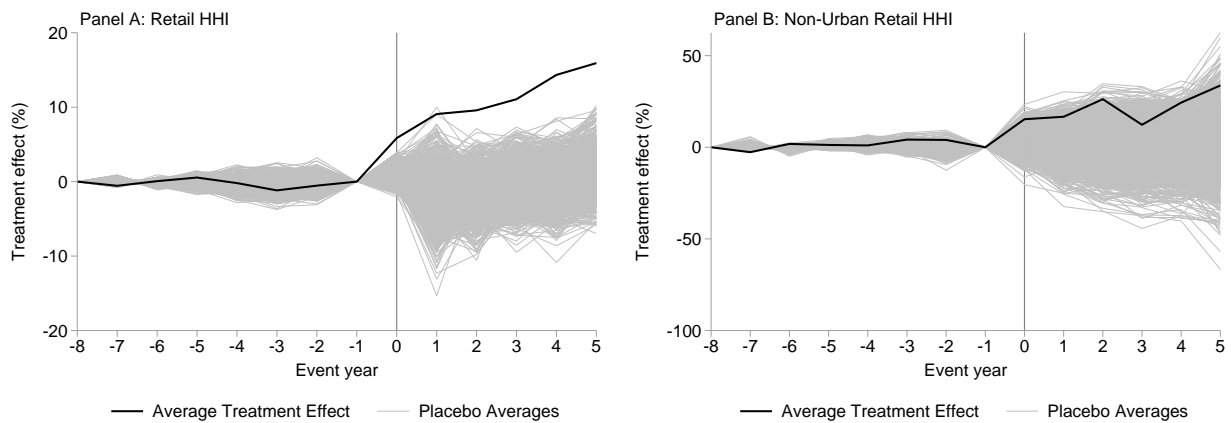
Note: Estimated using HHI calculated from the Dun's Market Indicators data from Dun & Bradstreet (DNB), as well as Supercenter entry timing and location from Holmes (2011). Donor pool counties are those where Walmart tried to open a Supercenter over the period but was blocked from doing so by local efforts. The sample contains 582 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. The thick, dark line is the path of average treatment effects. The grey lines are paths of random samples of 1,000 placebo average treatment effects. All figures normalized to one in the 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. Panel A is the average effect on retail HHI in the entire sample; Panel B is the average effect on retail HHI in non-urban counties.

Figure 13:
Effects of 1996/97 federal minimum wage increase in counties w/ a Supercenter
Synthetic control estimates

Panel A: Aggregate employment

Panel B: Retail employees

Panel C: Aggregate total earnings

Panel D: Retail total earnings
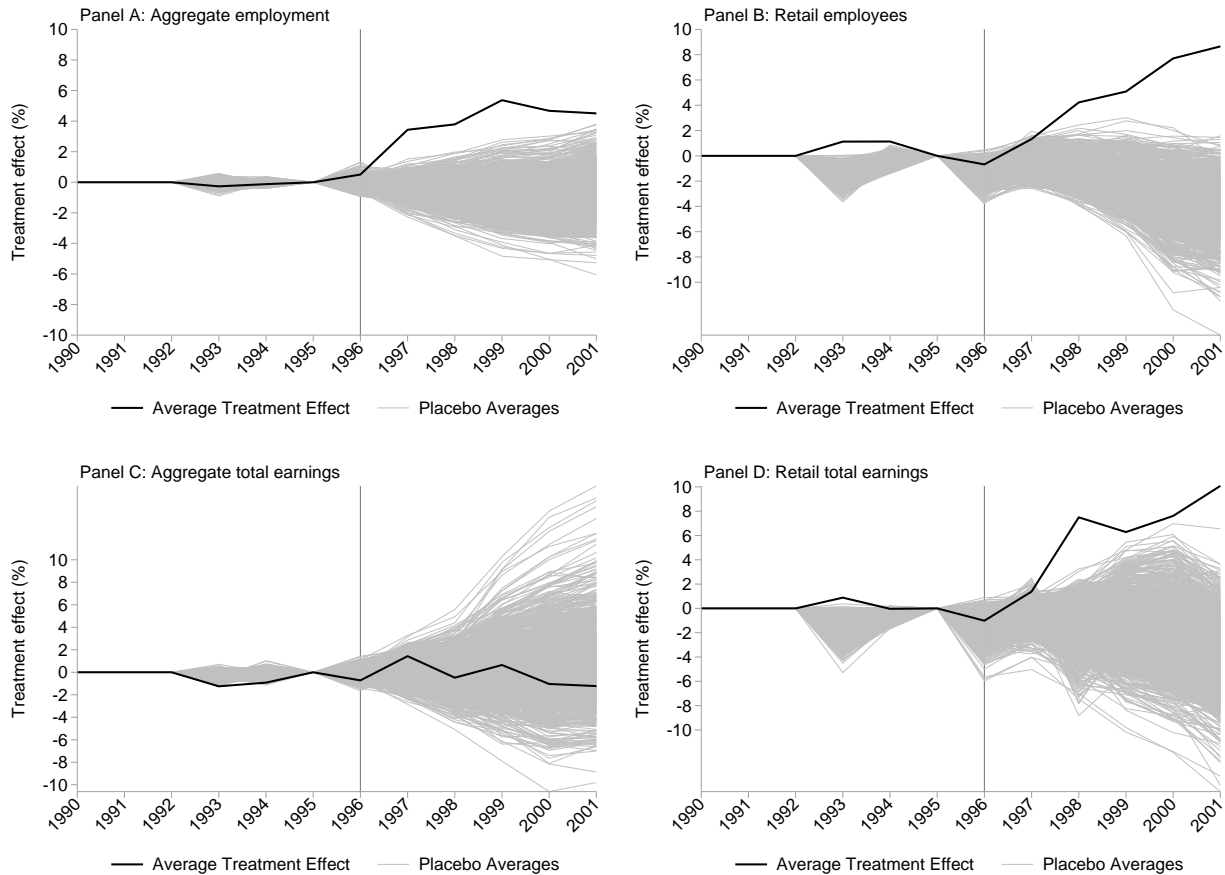
Note: Estimated using employment data from the QCEW, normalized to one in the 1995. Supercenter entry timing and location from Holmes (2011). Data on state and federal minimum wages from the Tax Policy Center. Treated counties are those which received a Supercenter before 1996; untreated counties are 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. 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 195 treated counties and 25 control counties, and a non-urban sample of 109 treated counties and 8 control counties. No non-urban county in the sample has a 1990 population below 113,000.

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	18,188	43,114	32,603	80,233	3,338	24,776
Real total earnings/worker (2017 USD)	30,685	31,170	33,612	34,062	26,769	28,358
Retail Employees	2,490	5,474	3,986	10,263	390	2,907
Real retail earnings/worker (2017 USD)	20,694	21,015	22,378	23,219	18,776	19,328
Employees per establishment	16	17	16	15	11	12
Retail share of total employment (%)	13	14	13	14	12	12
Retail HHI (/10,000)	133	149	126	138	250	325
Unemployment rate (%)	6	6	5	6	6	6
Total population	52,100	95,786	75,645	176,116	12,827	58,194
N	542–581	542–581	39	39	1,685	1,685

Note: Data from the QCEW, Intercensal Population Estimates, and Personal Current Transfer Receipts (CAINC35). 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 583 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 542 and 581 treated counties, depending on the variable of interest. 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:
Effect of Supercenter entry five years after opening
Employment and labor force
Synthetic control estimates

	Aggregate Employment	Retail Employment	Retail Share of Local Employment	Labor Force Participation Rate
	(1)	(2)	(3)	(4)
Treatment effect	-0.0295	0.0136	0.0349	-0.0163
Andrews *p*-value	0.1000	0.1000	0.0000	0.2000
Moving block *p*-value	0.0667	0.0000	0.0000	0.0000
RMSPE *p*-value	0.0609	0.0659	0.0190	0.0070

Note: Estimated using employment data from the QCEW and 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. All figures normalized to one in the 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.

Table 4:
Effect of Supercenter entry five years after opening
Earnings and EITC receipts
Synthetic control estimates

	Aggregate Total Earnings	Retail Total Earnings	Aggregate per-worker Earnings	EITC Receipts per capita
	(1)	(2)	(3)	(4)
Treatment effect	-0.0523	0.0004	-0.0234	0.0907
Andrews *p*-value	0.0000	0.0000	0.1000	0.1000
Moving block *p*-value	0.0000	0.0000	0.0000	0.0000
RMSPE *p*-value	0.2248	0.0599	0.3157	0.0789

Note: Estimated using employment and earnings data from the QCEW and EITC receipts from BEA, 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. Earnings figures deflated to 2017 USD using the PCEPI, and all figures normalized to one in the 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.

Table 5:
Effect of Supercenter entry five years after opening
Goods-producing and services-providing industries
Synthetic control estimates

	Goods-Producing Industries		Services-Providing Industries	
	Employment	Total Earnings	Employment	Total Earnings
	(1)	(2)	(3)	(4)
Treatment effect	-0.0452	-0.0492	-0.0752	-0.0670
Andrews *p*-value	0.1000	0.0000	0.0000	0.0000
Moving block *p*-value	0.0000	0.0000	0.0000	0.0000
RMSPE *p*-value	0.0010	0.0410	0.0120	0.3766

Note: Estimated using employment and earnings data from the QCEW, 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 542–581 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied. Earnings figures deflated to 2017 USD using the PCEPI, and all figures normalized to one in the 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.

Table 6:
Effect of Supercenter entry five years after opening
Non-urban counties
Synthetic control estimates

	Aggregate Employment	Aggregate Total Earnings	Aggregate per-worker Earnings	EITC per worker
	(1)	(2)	(3)	(4)
Treatment effect	-0.0733	-0.0677	-0.0208	0.1431
Andrews *p*-value	0.1000	0.1000	0.1000	0.0000
Moving block *p*-value	0.0000	0.0000	0.0000	0.0000
RMSPE *p*-value	0.1089	0.8042	0.9271	0.7483

Note: Estimated using employment and earnings data from the QCEW, 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 303 non-urban treated counties and 15 non-urban donor pool counties. The bias-correction procedure is applied. Earnings figures deflated to 2017 USD using the PCEPI, and all figures normalized to one in the 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.

Table 7
Effects of Supercenter entry five years after opening
Retail employment-level concentration (HHI)
Synthetic control estimates

	Retail HHI	Non-Urban Retail HHI
	(1)	(2)
Treatment effect	0.1592	0.3373
Andrews *p*-value	0.1000	0.1000
Moving block *p*-value	0.0000	0.0000
RMSPE *p*-value	0.0030	0.3147

Note: Estimated using HHI calculated from the Dun's Market Indicators data from Dun & Bradstreet, 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. The sample contains 582 urban and non-urban treated counties and 39 donor pool counties. The bias-correction procedure is applied, and all figures normalized to one in the 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.

Table 8
Estimated employment effects of 1996/97 federal minimum wage increases
Counties with a Supercenter vs donor pool
Difference-in-differences estimates

	All Counties		Non-Urban Counties	
	Aggregate (1)	Retail (2)	Aggregate (3)	Retail (4)
1990 - 1998				
Employment, %	2.2709*	2.6245*	3.9043***	3.6867***
	(1.2406)	(1.3520)	(1.3309)	(1.2675)
100 × Log Employment	2.3226*	2.9632**	3.9230***	4.1581***
	(1.3319)	(1.4230)	(1.3749)	(1.3163)
1990 - 2001				
Employment, %	4.9529**	5.2983***	3.6321***	3.8399***
	(2.0189)	(2.0097)	(1.1806)	(1.1677)
100 × Log Employment	4.5648**	5.2844***	3.7344***	4.3417***
	(1.9105)	(1.9340)	(1.1971)	(1.1633)
1990 - 2005				
Employment, %	6.3534**	6.9579**	3.2672**	3.8862***
	(2.7512)	(2.7180)	(1.3173)	(1.2298)
100 × Log Employment	5.3437**	6.3321**	3.4413**	4.2189***
	(2.4149)	(2.4486)	(1.3224)	(1.2175)
N	220	117	220	117

Note: Estimated using employment data from the QCEW, 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. 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. *Employment, %* = 100 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 195 treated counties and 25 control counties, and a non-urban sample of 109 treated counties and 8 control counties. No non-urban county in the sample has a 1990 population below 113,000. Standard errors clustered by county.

Table 9

Estimated earnings effects of 1996/97 federal minimum wage increases

Counties with a Supercenter vs donor pool

Difference-in-differences estimates

	All Counties		Non-Urban Counties	
	Aggregate (1)	Retail (2)	Aggregate (3)	Retail (4)
1990 - 1998				
Earnings, %	3.4294***	1.5696	4.3969***	2.6739*
	(1.2755)	(1.1896)	(1.4841)	(1.5409)
100 × Log Earnings	3.5689***	2.8807**	4.4302***	4.7291***
	(1.3550)	(1.2435)	(1.4796)	(1.2901)
1990 - 2001				
Earnings, %	8.3208***	4.1817**	4.8382***	3.2615**
	(3.1463)	(2.0164)	(1.4327)	(1.3583)
100 × Log Earnings	5.4148***	4.8506***	5.2695***	5.4877***
	(1.8904)	(1.7801)	(1.4019)	(1.1926)
1990 - 2005				
Earnings, %	9.9024**	5.6991**	4.9755***	3.8858***
	(4.0602)	(2.7126)	(1.6208)	(1.3505)
100 × Log Earnings	6.0964**	5.3236**	5.7662***	5.8583***
	(2.3818)	(2.1767)	(1.5056)	(1.2495)
N	220	117	220	117

Note: Estimated using employment and earnings data from the QCEW, 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. 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. *Earnings, %* = 100 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 195 treated counties and 25 control counties, and a non-urban sample of 109 treated counties and 8 control counties. No non-urban county in the sample has a 1990 population below 113,000. Standard errors clustered by county.

Table 10
Estimated employment and earnings effects of 1996/97 federal minimum wage increases
Counties with a Supercenter vs donor pool
Synthetic control estimates

	Aggregate Employment	Retail Employment	Aggregate Total Earnings	Retail Total Earnings
	(1)	(2)	(3)	(4)
Treatment effect	0.0451	0.0866	-0.0123	0.1008
Andrews *p*-value	0.1667	0.1667	0.1667	0.1667
Moving block *p*-value	0.0000	0.0000	0.0000	0.0000
RMSPE *p*-value	0.1059	0.0490	0.9880	0.0080

Note: Estimated using employment and earnings data from the QCEW, 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. Data on state and federal minimum wages from the Tax Policy Center. Treated counties are those which received a Supercenter before 1996. 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. Employment 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 195 treated counties and 25 control counties, and a non-urban sample of 109 treated counties and 8 control counties. No non-urban county in the sample has a 1990 population below 113,000. Standard errors clustered by county. The estimated treatment effects are the proportion change in the outcome variable in the fifth year following the minimum wage change, relative to the synthetic control.

A Appendix

A.1 Additional Data Sources and Details

I take county-by-year Earned Income Tax Credit (EITC) receipts from Personal Current Transfer Receipts (CAINC35) from the U.S. Bureau of Economic Analysis (BEA) Regional Economic Accounts series. These report national federal credits received, inclusive of tax offsets (EITC reductions in tax liabilities). BEA calculates these by county by first adding payment data from the Treasury Department's Monthly Treasury Statement (MTS)—which are only available as annual national totals—and tax offsets based on Internal Revenue Service Statistics of Income (SOI) data, and then allocating these national totals to states and then to ZIP codes and finally to counties based on each regional level's relative share of its parent-level SOI totals in each year. Thus the state-level totals sum to the national total, and the county-level totals sum to their state's total (see ? for details). The data reflect the county of residence of recipients of EITC amounts, rather than the county of employment, which unfortunately introduces some measurement error for my purposes.[43] Annual dollar amounts are deflated to 2017 USD, and annual county-level per-capita receipts are then calculated using the intercensal population estimates.

I take non-urban counties to be those which are classified as "Nonmetropolitan" based on the 1990 Urban-Rural Classification Scheme for Counties from the National Center for Health Statistics. Finally, I take state and federal minimum wage data from the Tax Policy Center. The federal minimum wage was $4.25/hour from 1991-1995. On October 1, 1996 it was raised to $4.75/hour, and on September 1, 1997 was raised again to $5.15/hour, where it remained through 2005. These two increase represented a 21% jump in just less than a year. 22 states had a county with a Supercenter before 1996 and had minimum wages less than or equal to the minimum wage through at least 2003, or had no minimum wage at all. 8 of these states were home to a donor pool county. With few exceptions, the federal minimum wage is binding on employers in any state which has a lower minimum wage than the federal level. Thus in these 22 states the minimum wage perfectly tracked the federal minimum wage over the period. An additional six states similarly tracked the federal minimum wage through at least 2003 and were host to a donor pool county but no county which had a Supercenter before 1996.

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[43]If anything, this would likely bias my estimates toward zero because some of the changes in EITC amounts associated with Supercenter entry are not realized in the county of entry.

A.2 Discussion of 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.

A.3 Synthetic Control Model Specification: Effects of Supercenter Entry

For each outcome, the predictor variables include six covariates: the five-year average, over 1990 - 1994 (before *any* included counties received their first Supercenter), of (the natural logarithm of): county total employment, county retail employment, county total earnings per worker, county retail earnings per worker, the retail share of total county employment, and total county population. I also include the values of the outcome variable in each of 1990, 1991, and 1992, and a five-year average of the same over 1990-1994—a total of $K = 10$ predictor variables. For the HHI estimaets, I additionally include the five-year average of (the natural logarithm of) aggregate and retail establishment counts. 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 10 predictor variables (12 for the HHI estimates), 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 (2019)). 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., 2015) and to allow the bias-corrected pre-treatment outcomes to potentially deviate from zero in these excluded years.

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 The Earned Income Tax Credit (EITC)

At $67 billion in 2018, the EITC is the largest means-tested, non-Medicaid transfer program in United States. It is a federal refundable tax credit available to low-income workers—meaning any credit in excess of this tax liability is fully paid out to workers—with the amount for which an individual is eligible heavily contingent on family earnings and parental-status, and to a lesser extent on marital-status.[44] Originally designed as a legislative end run around an anti-poverty-focused negative income tax, the EITC began with a narrow focus on offsetting increased payroll taxes and providing a work incentive to low-income individuals (Social Security Administration, 1997; Hotz and Scholz, 2003; Nichols and Rothstein, 2015). Introduced in 1975, it remained a relatively small program until the early 1990s, when it was given a new anti-poverty emphasis and was greatly expanded through a series of legislative changes, growing in size from $3.7 billion in 1987 to $47.9 billion in 2005.

While the exact EITC amount for which a filer is eligible depends on family earnings and numbers of children, for all filers the EITC schedule has a flat-topped pyramid shape: as earnings increase, the EITC amount for which a filer is eligible initially increases linearly from zero before plateauing, then linearly

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[44]Many states also offer their own EITC in addition to the federal program. The generosity of these state-level programs can vary substantially from state-to-state and year-to-year.

decreases until it is completely phased out. Most of the literature on the EITC has focused on how its introduction and expansion has increased labor force participation (LFP) among single mothers, who comprise the largest population of EITC recipients (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Meyer, 2002; Bastian, 2020) and somewhat discouraged labor force participation among married women (Eissa and Hoynes, 2004).[45] Leigh (2010) and Rothstein (2010) examine the incidence of the EITC and find that downward wage adjustments, realized in response to EITC-induced labor supply increases, allow employers to capture a significant portion of the EITC incidence at the expense of less skilled workers, especially ineligible single women. Looking at the distributional effects during periods of increased unemployment, Bitler et al. (2017) found the EITC effectively served as a cushion against negative income shocks for married couples with children, but not for single parents. Nevertheless, today the EITC is widely regarded as one of the US federal government's most effective anti-poverty tools, particularly for children (??), generally attracting support from across the political spectrum because it encourages work by effectively 'topping up' the wages of low-income earners without increasing the marginal cost of labor in the way a minimum wage increase might.

In Figure A1 I plot aggregate county-level EITC receipts for the contiguous U.S. by year, in counties which received a Supercenter before 2005 and in those which didn't, against the number of Walmart Supercenters. EITC receipts rose in both groups, but much more rapidly in counties which received a Supercenter. To give a sense of EITC eligibility before and after Supercenter entry, I combine observations of Supercenter timing and location of entry from Holmes (2011) and data from the Annual Social and Economic Supplement of the Current Population Survey (CPS ASEC, or the "March Supplement"), which observes demographic characteristics of individual respondents, certain tax-related variables simulated using the tax model of the Census Bureau, and county of residence for individuals in the largest counties (from 1996 onward). As only the largest counties are observed in these data (209 without a Supercenter as of 1996), this sample may not be representative, but it is suggestive. I separate respondents by whether they were in a county which received its first Supercenter between 1997 and 2005 (92 counties) or in a county which had not received a Supercenter before 2006 (117 counties), and whether they are in a unmarried household with children (working single parents are the most likely to qualify for an EITC). I then calculate the (weighted)

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[45]Kleven (2020) finds little evidence that LFP was affected by the EITC, and instead argues the large increase in the LFP of single mothers seen in the mid 1990s was the result of welfare reform and the business cycle.

Figure A1: U.S. EITC expenditures grew substantially faster in counties which received a Supercenter
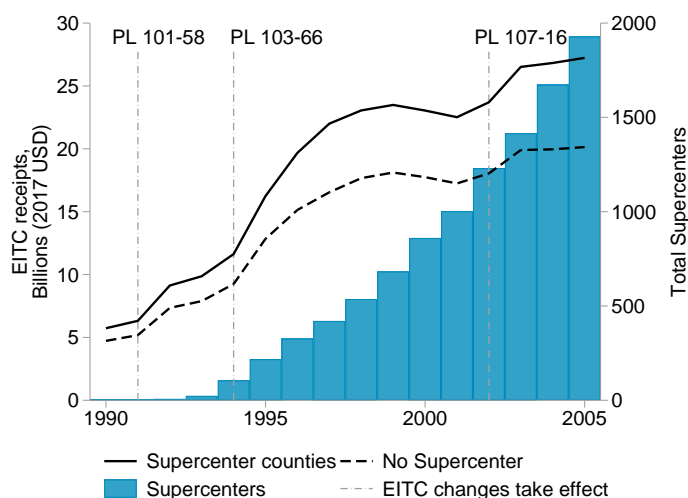
Note: Federal EITC receipts are aggregated across counties in the contiguous U.S. and deflated to (billions of) 2017 USD using the PCEPI. Walmart Supercenter totals calculated using data from Holmes (2011).

proportion of all individuals eligible for an EITC in the previous year, by county and year, who are in unmarried households, including any children. The density plots are presented in Figure A2. They show that, in observed counties which received their first Supercenter over the period, individuals from households with unmarried parents became relatively much more likely to be eligible for an EITC than their equivalents in counties which didn't have a Supercenter.

To give a sense of the EITC amounts for which Supercenter workers might be eligible, Figure A3 plots average earnings in 2001 for tenured full-time and part-time hourly Walmart employees (from Table 1) against the 2001 Federal EITC schedule for families with none, one, and two or more children. Assuming no other family income,[46] the average full-time Walmart hourly employee with one child would be entitled to an EITC of $1,708, and her part-time equivalent would be entitled to an EITC of $2,428. Average full-time and part-time Walmart hourly employees with two or more children would be entitled to EITC amounts of $3,060 and $3,151 respectively.

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[46]That is, assuming the Walmart employee is the head-of-household for tax purposes, and assuming no joint filing with an income-earning spouse

Figure A2: Counties which received a Supercenter saw a relative increase in EITC-eligible individuals from unmarried households with children
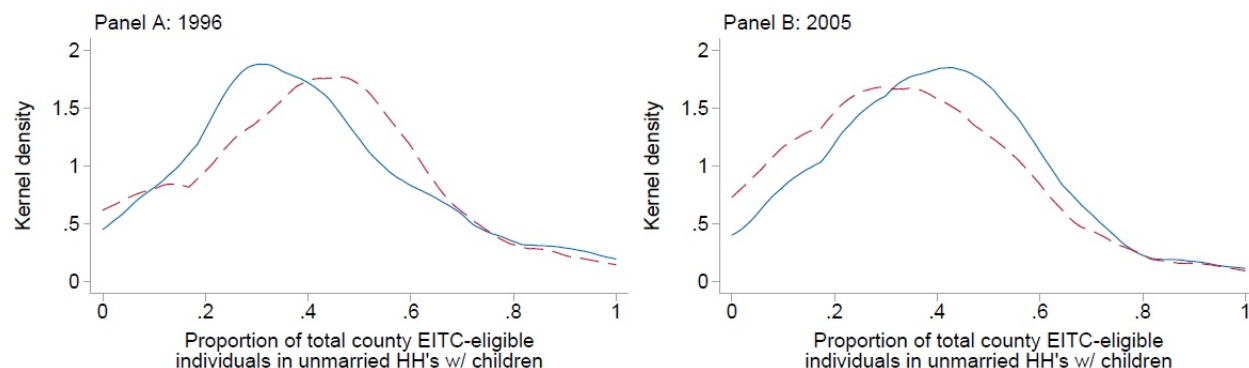
Note: Calculated from CPS ASEC data. The plots split observed counties (209 total) into those which received their first Supercenter between 1997 and 2005 and those which received no Supercenter before 2006, and within each group plot the density of the proportion of EITC-eligible individuals in unmarried households with children in 1996 (Panel A) and 2005 (Panel B).

Figure A3: Earnings of Walmart Employees vs Federal EITC Schedule, 2001
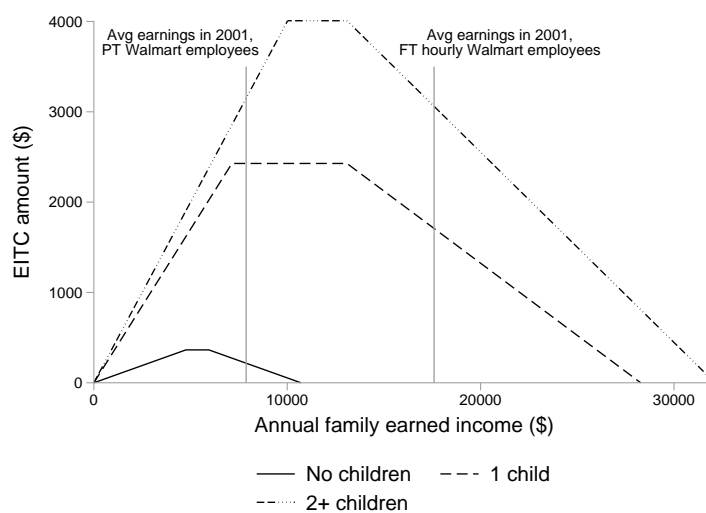
Note: Walmart earnings figures calculated from Drogin (2003) for tenured hourly Walmart employees (excluding Sam's Club). All figures in current USD.

A.5.1 Theoretical Framework: One-Stop Shopping

To fix ideas, I develop a simple static, partial equilibrium model which illustrate the channels through which higher local EITC receipts could be realized in the face of monopsony power in the labor market from a large firm that offers "one stop shopping". Let there be N individuals indexed by i, each of whom has some

positive endowment and demand $q_{i,k}^D = \frac{1}{N}(1-p_k)$ for consumption good $k \leq \overline{K}$. That is, each individual loves variety of consumption goods, regardless of earnings (subsidized by their endowments), up to a maximum number of varieties. Individuals also suffer disutility of travel time t spent to visit each retailer, with total travel time T, such that $U_i'(T) < 0$ and $U_i''(T) < 0$. Individuals choose whether or not to work, providing labor supply:

$$L_i^S = \begin{cases} 0 & \text{if } w < \gamma_i \\ 1 & \text{if } w \geq \gamma_i \end{cases}$$

$w \in [0,1]$ is the annual wage (earnings). $\gamma_i \in \Gamma$ is the reservation wage of i, with $\Gamma \sim U[0,1]$. Total labor supply is $L^S = \sum_{i=1}^N L_i^S = \sum_{i=1}^N \mathbb{1}[y(1) \geq \gamma_i] \leq N$, where y(1) is after-tax earnings given $L_i^S = 1$:

$$y(1) = \begin{cases} \alpha w & \text{if } w < v_1 \\ w + (\alpha - 1)v_1 & \text{if } v_1 \leq w < v_2 \\ w + (\alpha - 1)v_1 + (\alpha - 1)(w - v_2) & \text{if } v_2 \leq w < v_3 \\ w & \text{if } w \geq v_3 \end{cases}$$

$\alpha \geq 1$ is a simple Earned Income Tax Credit phase-in (and phase-out) rate. The EITC phases in up to wage v_1, plateaus between wages v_1 and v_2, and phases out between wages v_2 and v_3. It is available to all workers conditional only on w and the EITC threshold values v_1, v_2 and v_3. Thus $y > w$ if $0 < w < v_3$ and $\alpha > 1$, and $y = w$ if $w = 0$ or $w \geq v_3$ or $\alpha = 1$. Further, suppose α is such that $y \leq 1$. Thus $L^S = yN$, where y is also the fraction of N who choose to work.

Case 1: Initial Competitive Equilibrium, EITC introduced In the initial scenario let $K^* < \overline{K}$ varieties be provided by small firms which can provide only one variety each such that there are $K = K^*$ small firms. These small firms are competitive in input and output markets, with identical technology $q_k^S = L_k^D$, where L_k^D is the labor demand of the firm providing variety k, and the labor supply curve coincides with the marginal cost of labor curve. Entry and exit is free, and firms set marginal cost to equal marginal revenue. Each firm earns zero profit and sets price $p_k = p = w$. Consumers thus face product price p along with travel time t to acquire each variety k, with $T^* = tK^*$. Total demand for variety k is $q_k^D = \sum_{i=1}^N q_{i,k}^D = (1-p_k) = (1-p)$, and clearing of the product market implies $q_k^D = q_k^S = L_k^D$. so clearing of the labor market implies $L^S = L^D \implies$

Figure A4: Competitive labor market with EITC
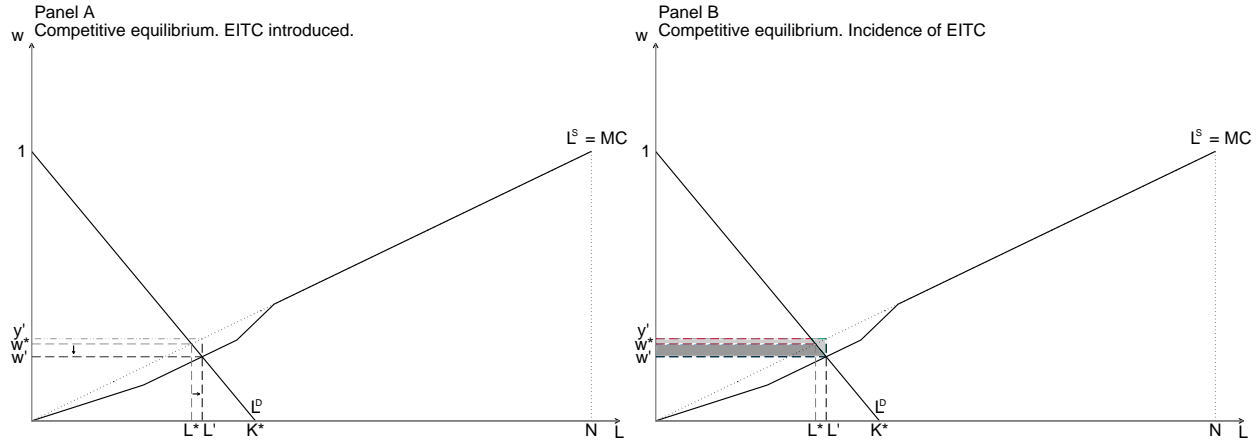
Note: Simulations from base model with an EITC, presented in Section A.5.1. Panel A shows the difference in wages, LFP, and employment with and without an EITC. In this simple case, introducing an EITC causes the wage paid by employers to decrease, but LFP, employment, and after-tax take-home earnings all increase. Panel B shows the total local cost the EITC (both shaded areas), and the incidence of the EITC, with the proportion that goes to firms shaded dark, and the proportion that goes to workers shaded light.

$yN = \sum_{k=1}^{K^*} L_k^D = \sum_{k=1}^{K^*} q_k^D = K^*(1-p)$. If $\alpha = 1$ (no EITC) then $y = w$, and $w^* = \frac{K^*}{N+K^*} = p^*$, $L^* = \frac{NK^*}{N+K^*}$

This is the base case in Panel A of Figure A4 and Panels A-C in Figure A5. Now suppose the government introduces an Earned Income Tax Credit, setting $\alpha > 1$. Wages fall, but LFP and employment rise, as do after-tax earnings. The solution is a step function. If on the plateau: $w' = \frac{K^* - N(\alpha-1)v_1}{N+K^*}$, $L' = \frac{NK^*[1+(\alpha-1)v_1]}{N+K^*}$ (which reduce to w^* and L^* when $\alpha = 1$). The total cost of local EITC receipts are shown as the combined shaded areas in Panel B of Figure A4. The light shaded area is the incidence realized by workers, while the dark shaded area is the incidence realized by firms. This illustrates the basic idea that firms and workers share the EITC through wage adjustments, as in Leigh (2010) and Rothstein (2010). This simple model doesn't distinguish among eligible and ineligible workers, but if it did then ineligible workers would take home $w' < w^*$, while eligible workers would take home $y' > w^*$, consistent with their findings that the EITC benefits eligible workers at the expense of ineligible workers in the same labor market.

Case 2: Large Firm Enters and Acts Competitively, No EITC Consider again the initial scenario in Case 1 *before* the EITC is introduced (that is, let $\alpha = 1$). Now suppose a large firm enters with technology that allows it to produce many varieties, each as $q_k^S = L_k^D$. This large firm also incurs some cost $C(K)$ which is

increasing in the number of varieties it produces. For simplicity, let the firm choose to produce K' varieties, with $K^* < K' = \overline{K}$. In this case, let the firm act competitively among its own 'departments' for each variety produced and sold, so it chooses L_k^D for each k and the marginal cost curve again coincides with the labor supply curve. The firm thus produces the maximum number of varieties which consumers love, sells them all for $p_k = p$, and sets marginal revenue equal to marginal cost. It offers the varieties to consumers all from a single large location ("one-stop shopping"). Consumers incur a price p for each k, but now only incur travel time t for all varieties purchased from the large firm, and travel time t for *each* variety purchased from a small firm, with total travel time T'. For simplicity, let $U'(T)$ be such that consumers prefer to purchase all varieties from the large firm ($T' = t < T^*$), which accepts the competitive price and wage, $w' = \frac{K'}{N+K'} = p'$ and employs $L' = \frac{NK'}{N+K'}$. This effectively constitutes a simple increase in labor demand (the initial L^D schedule is shown as a dotted line), and yields higher wages, LFP, and employment, with $w' > w^*$ and $L' > L^*$. The move from Case 1 to this case is illustrated in Panel A of Figure A5.

Case 3: Large Firm Enters and Acts as a Monopsonist, No EITC   Suppose again $\alpha = 1$ and a large firm again enters as in Case 2, and again decides to offer K' varieties, but now produces with technology $q_k^D = L_x^D$, where $L_x^D = \frac{L_M^D}{K'}$, with L_M^D the total amount of labor demanded by the large firm which it allocates evenly across each of the K' varieties it produces. That is, this firm recognizes its ability to allocate total labor employed across its varieties, and thus chooses its total labor demand L_M^D rather than each L_k^D individually—it recognizes and acts on its monopsony power in the labor market, and the marginal cost curve is now above the labor supply curve. It still selects $p_k = p$ for all k, and customers again choose to purchase only from the large firm because one-stop shopping reduces their travel time. While this constitutes an increase in labor demand, as in Case 2, the firms' selection of L_k^D to set marginal revenue equal to marginal cost is such that, provided K' is not substantially greater than K^*, rather than *increasing* as in Case 2, wages, LFP, and employment all now *decrease*, and the monopsonist 'exploits' labor by paying a wage below the marginal revenue of labor, with $w' = \frac{K'}{N+2K'}$, and $L' = \frac{NK'}{N+2K'}$. The move from Case 1 to this case is illustrated in Panel B of Figure A5. This is the classic situation with a monopsonist employer. With this setup, $p' = 2w'$ and the firm earns a positive profit.

Figure A5: Large firm entry, with and without monopsony power and EITC

Panel A
w Large firm enters. No monopsony, no EITC

Panel B
w Large firm enters. Monopsony, no EITC

Panel C
w Large firm enters. Monopsony and EITC

Panel D
w Large firm enters. Monopsony and EITC. Subsidy and exploitation
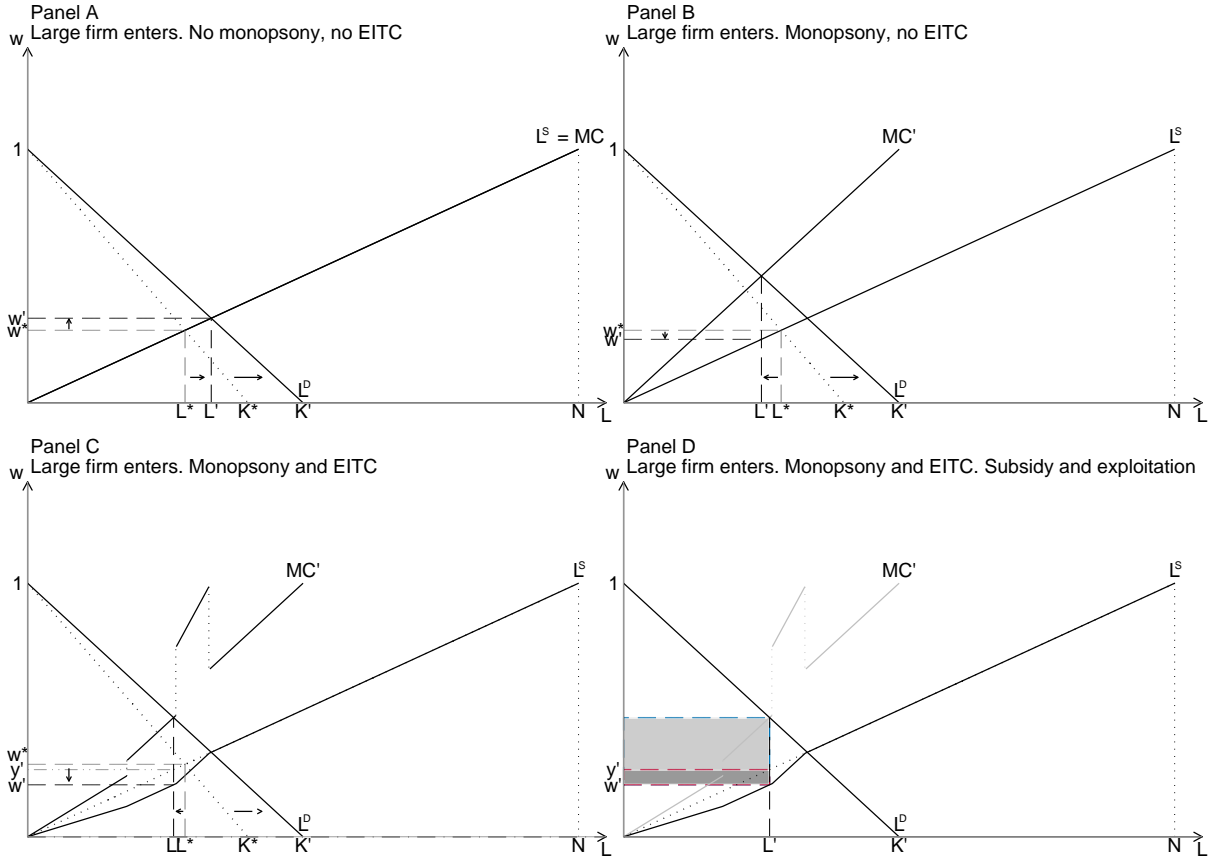
Note: Simulations from model presented in Section A.5.1. Panel A shows the case with no EITC where a large firm enters and produces up to $K' > K^*$ varieties but does not exercise monopsony power, with labor and wages both increasing. Panel B shows the case with no EITC where the large firm enters and replaces all small firms, producing K' varieties and exercising monopsony power, with labor and wages both decreasing. Panel C shows same as Panel B but with an EITC, with wages decreasing more than in Panel B but labor decreasing less. Panel D shows the consequent subsidy cost and exploitation of labor from the case in Panel C.

Case 4: Large Firm Enters and Acts as a Monopsonist, EITC Introduced Now suppose the same firm enters as in Case 3, increasing labor demand, and again suppose the government introduces an EITC such that $\alpha > 1$. Panel C of Figure A5 shows the differences this makes. The labor supply schedule now takes on a new shape for $w < v_3$ (L^S with $\alpha = 1$ is still shown as a dotted line), which results in a new marginal cost schedule for all $L(w) < L(v_3)$ with jumps at $L(v_1), L(v_2),$ and $L(v_3)$. Again, (provided K' is not too substantially greater than K^*) wages, LFP, and employment all fall. The comparative statics between this

case and Case 3 depend on α, v_1, v_2, and v_3, but in general wages and fall more while LFP and employment fall less. As illustrated in Panel C of Figure A5, $w' = \frac{K' - (N+K')(\alpha-1)v_1}{N+2K'}$ and $L' = \frac{NK'[1+(\alpha-1)v_1]}{N+2K'}$, which reduce to the values in Case 3 when $\alpha = 1$. It is also clear that the EITC would have no effect on w' or L' if the large firm did not operate as a monopsonist, as in Case 2 (because v_3 is too low for the EITC to be binding in such a case), and that wages will be lower than in Case 1 *before* the large firm entered (the differences in LFP and employment between the initial scenario in Case 1 and the final result in Case 4 depend on the parameter values which structure the EITC).

If I consider this case relative to the initial equilibrium or to Case 2 (if the alternative were that the firm chose not to or was constrained from acting on its monopsony power), then Panel D of Figure A5 shows the firm effectively captures the entire incidence of the EITC along with large monopsony rents (exploitation of labor). Compared to Case 3, it captures a portion of it. The light-shaded area is its exploitation of labor—the difference between each worker's after-tax earnings y' and the marginal revenue their labor yields for the firm. The dark shaded area is the total local increase in local EITC cost, which is here entirely caused by the firm's operation as a monopsonist. The firm captures both areas. The differences in y' and L', relative to those in Case 2, are the reductions in after-tax earnings, LFP, and employment caused by the firm's operation as a monopsonist—which could be eliminated e.g. if the firm was required to pay a minimum wage equal to w' in Case 2, eliminating the monopsony power.

A.6   Andrews, Moving Block, and In-Space Placebo RMSPE *p*-values

A.6.1   Andrews *p*-values

Andrews (2003) originally proposed an end-of-sample instability test in another context, 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 (8). 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:

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

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

A.6.2 Moving Block p-values

Moving block permutation tests are proposed by Chernozhukov et al. (2019). Specifically, let P_e^N be a mean-unbiased proxy for $Y_{1,e}^N$: $Y_1^N = P_e^N + u_e$, $E[u_e] = 0$, $\forall\, e$ (here, unit 1 is an average of all units treated in event time $e = 0$). Under a sharp null hypothesis of $\hat{\tau}_{1,e} = 0 \,\forall\, e \geq 0$, each u_e can be calculated as $u_e = Y_{1,e} - P_e^N$. The test procedure assumes $\{u_e\}$ is stationary and weakly dependent. If $\{u_e\}$ is invariant under treatment, then the distribution of $\{u_e\}$ should be the same post-treatment as it is pre-treatment. Then a test statistic can be calculated: $S(\hat{u}) = \frac{1}{\sqrt{E}} \sum_{e=0}^{E} |\hat{u}_e|$, where E is the number of post-treatment event periods (excluding the event period of treatment, $e = 0$) for which there exists a balanced sample of treated units. Define block permutation π_y such that:

$$\pi_y(z) = \begin{cases} y+z & \text{if } y+z \leq 1+D+E \\ y+z-(1+D+E) & \text{otherwise} \end{cases}$$

where D is the number of pre-treatment event periods for which there exists a balanced sample of treated units. For each $\pi \in \Pi$ let $\hat{u}_\pi = (\hat{u}_{\pi(1)}, ..., \hat{u}_{\pi(E)})$ be the vector of permuted residuals. The moving block p-values can then be computed as:

$$p_{MB} = 1 - \frac{1}{|\Pi|} \sum_{\pi \in \Pi} \mathbb{1}\{S(\hat{u}_\pi) < S(\hat{u})\}$$

A.6.3 In-Space Placebo RMSPE p-values

Similar to Cavallo et al. (2013); Abadie and L'Hour (2019) and Abadie (2021), 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, $\hat{\bar{\tau}}_g = \{\hat{\bar{\tau}}_{g,-D}, ..., \hat{\bar{\tau}}_{g,E}\}$—where g $\{2, ..., 1001\}$, 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, $\hat{\bar{\tau}}_1$, 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:

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

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.7 Naive estimates using event study and difference-in-differences research designs?

I present estimates of Equation 1. I first consider a naive TWFE event study specification, with $l = -5$ and $m = 5$, and restrict my sample of treated counties to be balanced in event time. I present estimates with and without my 'donor pool' of untreated counties. Some of the estimated effects on retail employment and compensation are positive and significant soon after treatment, but often turn negative and in all cases lose significance by the fifth year after entry. Retail earnings estimates follow the opposite pattern. Estimated effects on aggregate employment, compensation and earnings are generally negative but never significant. Appendix Table A3 similarly presents naive TWFE DD estimates with two groups (treatment and control), and two periods for each treated county (pre and post), such that $l = 0$ and $m = 1$.

Table A1

Naive event study estimates of event-year treatment effects of Supercenter entry

Estimated with dummies for event years -2 through -5 included

	Employment		Compensation		Per-employee compensation		Labor force
	Aggregate	Retail	Aggregate	Retail	Aggregate	Retail	participation rate
No untreated units, unweighted							
Event-year 0	0.0023	0.0410***	0.0006	0.0334***	-0.0018	-0.0079***	0.0036
	(0.0027)	(0.0043)	(0.0033)	(0.0049)	(0.0013)	(0.0024)	(0.0023)
Event-year 1	-0.0007	0.0421***	-0.0021	0.0348***	-0.0015	-0.0073**	0.0021
	(0.0049)	(0.0072)	(0.0061)	(0.0084)	(0.0023)	(0.0034)	(0.0037)
Event-year 2	-0.0055	0.0334***	-0.0068	0.0304**	-0.0014	-0.0031	-0.0004
	(0.0075)	(0.0103)	(0.0092)	(0.0120)	(0.0033)	(0.0045)	(0.0050)
Event-year 3	-0.0082	0.0243*	-0.0092	0.0231	-0.0010	-0.0014	-0.0013
	(0.0099)	(0.0135)	(0.0121)	(0.0155)	(0.0041)	(0.0054)	(0.0059)
Event-year 4	-0.0088	0.0194	-0.0074	0.0220	0.0014	0.0026	-0.0019
	(0.0122)	(0.0166)	(0.0147)	(0.0190)	(0.0050)	(0.0065)	(0.0065)
Event-year 5	-0.0105	0.0152	-0.0077	0.0177	0.0029	0.0025	-0.0036
	(0.0144)	(0.0196)	(0.0172)	(0.0224)	(0.0058)	(0.0074)	(0.0067)
No untreated units, weighted *(1990 population)*							
Event-year 0	-0.0015	0.0118***	-0.0023	0.0099**	-0.0009	-0.0020	0.0005
	(0.0035)	(0.0039)	(0.0043)	(0.0046)	(0.0015)	(0.0025)	(0.0023)
Event-year 1	-0.0078	0.0067	-0.0094	0.0068	-0.0016	0.0001	-0.0012
	(0.0071)	(0.0075)	(0.0089)	(0.0088)	(0.0033)	(0.0042)	(0.0039)
Event-year 2	-0.0166	-0.0035	-0.0202	-0.0015	-0.0034	0.0021	-0.0042
	(0.0112)	(0.0119)	(0.0136)	(0.0136)	(0.0044)	(0.0060)	(0.0053)
Event-year 3	-0.0241	-0.0165	-0.0296	-0.0144	-0.0053	0.0021	-0.0067
	(0.0151)	(0.0161)	(0.0180)	(0.0184)	(0.0058)	(0.0077)	(0.0063)
Event-year 4	-0.0286	-0.0257	-0.0344	-0.0204	-0.0053	0.0054	-0.0088
	(0.0191)	(0.0203)	(0.0221)	(0.0229)	(0.0073)	(0.0093)	(0.0069)
Event-year 5	-0.0364	-0.0341	-0.0401	-0.0262	-0.0034	0.0080	-0.0102
	(0.0222)	(0.0243)	(0.0260)	(0.0269)	(0.0085)	(0.0105)	(0.0074)
With untreated units, weighted *(1990 population)*							
Event-year 0	0.0010	0.0160***	-0.0004	0.0141***	-0.0014	-0.0019	0.0008
	(0.0030)	(0.0037)	(0.0042)	(0.0039)	(0.0023)	(0.0022)	(0.0019)
Event-year 1	-0.0018	0.0161**	-0.0038	0.0171**	-0.0020	0.0010	0.0007
	(0.0059)	(0.0073)	(0.0083)	(0.0075)	(0.0046)	(0.0037)	(0.0035)
Event-year 2	-0.0083	0.0118	-0.0130	0.0135	-0.0046	0.0018	-0.0019
	(0.0090)	(0.0110)	(0.0122)	(0.0112)	(0.0065)	(0.0048)	(0.0047)
Event-year 3	-0.0132	0.0053	-0.0224	0.0080	-0.0091	0.0028	-0.0039
	(0.0125)	(0.0150)	(0.0163)	(0.0154)	(0.0086)	(0.0058)	(0.0058)
Event-year 4	-0.0158	0.0005	-0.0270	0.0062	-0.0108	0.0059	-0.0054
	(0.0155)	(0.0182)	(0.0199)	(0.0186)	(0.0106)	(0.0069)	(0.0068)
Event-year 5	-0.0223	-0.0045	-0.0325	0.0043	-0.0099	0.0090	-0.0066
	(0.0180)	(0.0214)	(0.0231)	(0.0217)	(0.0124)	(0.0078)	(0.0076)

Note: Estimated using employment and earnings data from the QCEW, as well as Supercenter entry timing and location from Holmes (2011). Untreated/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 542–581 urban and non-urban 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. $l = -5$. Standard errors clustered by county.

Table A2
Naive event study estimates of event-year treatment effects of Supercenter entry
Estimated without dummies for pre-treatment event years

	Employment		Compensation		Per-employee compensation		Labor force
	Aggregate	Retail	Aggregate	Retail	Aggregate	Retail	participation rate
No untreated units, unweighted							
Event-year 0	0.0019	0.0398***	-0.0008	0.0326***	-0.0027*	-0.0074***	0.0030
	(0.0030)	(0.0049)	(0.0037)	(0.0057)	(0.0015)	(0.0024)	(0.0025)
Event-year 1	0.0022	0.0440***	0.0000	0.0378***	-0.0022	-0.0062**	0.0026
	(0.0044)	(0.0066)	(0.0054)	(0.0077)	(0.0022)	(0.0030)	(0.0033)
Event-year 2	0.0006	0.0382***	-0.0013	0.0370***	-0.0018	-0.0013	0.0013
	(0.0062)	(0.0087)	(0.0076)	(0.0102)	(0.0029)	(0.0037)	(0.0041)
Event-year 3	0.0005	0.0318***	-0.0007	0.0328**	-0.0013	0.0010	0.0014
	(0.0082)	(0.0112)	(0.0099)	(0.0129)	(0.0035)	(0.0044)	(0.0046)
Event-year 4	0.0024	0.0292**	0.0038	0.0346**	0.0013	0.0054	0.0017
	(0.0101)	(0.0139)	(0.0124)	(0.0159)	(0.0045)	(0.0052)	(0.0050)
Event-year 5	0.0030	0.0271	0.0058	0.0329*	0.0029	0.0058	0.0007
	(0.0121)	(0.0166)	(0.0148)	(0.0189)	(0.0052)	(0.0060)	(0.0050)
No untreated units, weighted *(1990 population)*							
Event-year 0	-0.0027	0.0103**	-0.0035	0.0083	-0.0008	-0.0021	-0.0001
	(0.0041)	(0.0046)	(0.0051)	(0.0053)	(0.0018)	(0.0028)	(0.0025)
Event-year 1	-0.0065	0.0080	-0.0085	0.0084	-0.0019	0.0004	0.0002
	(0.0064)	(0.0072)	(0.0081)	(0.0081)	(0.0032)	(0.0037)	(0.0036)
Event-year 2	-0.0131	0.0005	-0.0173	0.0031	-0.0040	0.0026	-0.0009
	(0.0094)	(0.0108)	(0.0116)	(0.0118)	(0.0042)	(0.0047)	(0.0044)
Event-year 3	-0.0186	-0.0100	-0.0249	-0.0070	-0.0062	0.0030	-0.0017
	(0.0125)	(0.0146)	(0.0151)	(0.0159)	(0.0055)	(0.0058)	(0.0050)
Event-year 4	-0.0213	-0.0171	-0.0281	-0.0107	-0.0065	0.0065	-0.0022
	(0.0159)	(0.0186)	(0.0189)	(0.0201)	(0.0070)	(0.0068)	(0.0054)
Event-year 5	-0.0274	-0.0236	-0.0324	-0.0143	-0.0047	0.0094	-0.0023
	(0.0185)	(0.0225)	(0.0222)	(0.0237)	(0.0083)	(0.0076)	(0.0056)
With untreated units, weighted *(1990 population)*							
Event-year 0	0.0002	0.0154***	-0.0012	0.0137**	-0.0014	-0.0018	0.0000
	(0.0042)	(0.0052)	(0.0060)	(0.0053)	(0.0033)	(0.0028)	(0.0024)
Event-year 1	-0.0022	0.0153**	-0.0045	0.0160**	-0.0023	0.0006	0.0006
	(0.0060)	(0.0073)	(0.0086)	(0.0074)	(0.0048)	(0.0038)	(0.0036)
Event-year 2	-0.0081	0.0109	-0.0135	0.0118	-0.0053	0.0010	-0.0013
	(0.0080)	(0.0099)	(0.0112)	(0.0098)	(0.0061)	(0.0045)	(0.0043)
Event-year 3	-0.0126	0.0043	-0.0228	0.0058	-0.0101	0.0016	-0.0026
	(0.0105)	(0.0127)	(0.0142)	(0.0128)	(0.0076)	(0.0050)	(0.0051)
Event-year 4	-0.0149	-0.0007	-0.0273	0.0035	-0.0120	0.0043	-0.0036
	(0.0128)	(0.0152)	(0.0170)	(0.0153)	(0.0093)	(0.0058)	(0.0058)
Event-year 5	-0.0211	-0.0059	-0.0327*	0.0010	-0.0114	0.0071	-0.0042
	(0.0147)	(0.0178)	(0.0195)	(0.0176)	(0.0107)	(0.0064)	(0.0063)

Note: Estimated using employment and earnings data from the QCEW, as well as Supercenter entry timing and location from Holmes (2011). Untreated/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 542–581 urban and non-urban 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. $l = 0$. Standard errors clustered by county.

Table A3
Naive difference-in-differences estimates of effects of Supercenter entry

	Employment		Compensation		Per-employee compensation		Labor force
	Aggregate	Retail	Aggregate	Retail	Aggregate	Retail	participation rate
Unweighted							
Treatment	-0.0042	0.0514***	-0.0234	0.0503**	-0.0192**	-0.0009	0.0015
	(0.0173)	(0.0188)	(0.0216)	(0.0215)	(0.0091)	(0.0083)	(0.0071)
Weighted *(1990 population)*							
Treatment	0.0201	0.0406	-0.0147	0.0422	-0.0347*	0.0018	0.0046
	(0.0199)	(0.0250)	(0.0163)	(0.0268)	(0.0188)	(0.0108)	(0.0127)

Note: Estimated using employment and earnings data from the QCEW, as well as Supercenter entry timing and location from Holmes (2011). Untreated/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 542–581 urban and non-urban treated counties and 39 donor pool counties. Estimated using OLS with year and county fixed effects with no adjustment for weighting issues. Standard errors clustered by county.

A.8   Figures and Tables

Figure A6:
Walmart employees by job, sex, and ethnicity

2005 EEO1 Survey Results

2005 EEOC Report

		Total	White	African American	Hispanic	Asian	Native American	Ttl Male	White	African American	Hispanic	Asian	Native American	Ttl Female	All Minority Assocs
				**************** MALE ****************					**************** FEMALE ****************						
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 **?**. 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 A7:
Total and retail employees
QCEW vs Dun & Bradstreet DMI data
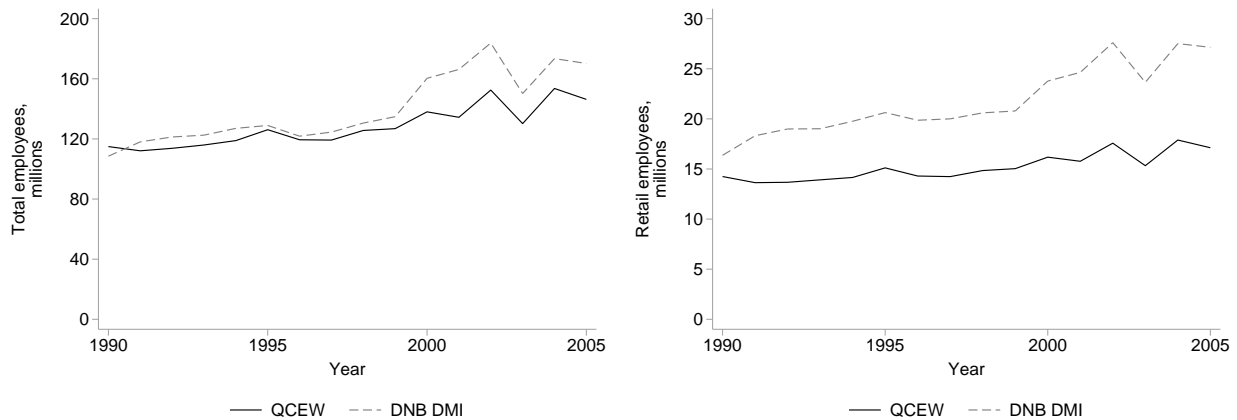
Note: Calculated using data from the QCEW and the Dun's Market Indicators data from Dun & Bradstreet (DNB). The left-hand panel shows total reported employees, by year. The right-hand panel shows total reported *retail* employees, by year, with the DNB figures restricted to establishments reporting up to 1,000 employees.

Figure A8:
Example of the effects of the synthetic control bias-correction procedure
Using California's Tobacco Control Program, Abadie et al. (2010) vs Wiltshire (2021a)

Note: Calculated using the example dataset provided with the `synth` Stata package (Abadie et al., 2011). The left-hand figure is taken from Abadie et al. (2010). The right-hand figure is generated using the `allsynth` Stata package (Wiltshire, 2021a).

Table A4:
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.