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Monopsony Power and Poverty: The Consequences of Walmart Supercenter Openings

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ABSTRACT

Monopsony Power and Poverty: The Consequences of Walmart Supercenter Openings[*]

Prior research suggests that Walmart Supercenters exert substantial power over the low-wage labor market, though the consequences of Supercenter openings on household incomes and public finances are less clear. This study uses restricted-access Panel Study of Income Dynamics data from 1970 to 2019 to study how Walmart Supercenter openings affect poverty, tax liabilities, and receipt of income transfers. Using a stacked difference-in-differences approach, we find that the opening of a Supercenter leads to a 2 percentage point (16%) increase in poverty. This increase is channeled through declining annual earnings and persists for 10 years following the Supercenter's entry. Increases in poverty are particularly strong for younger and less-educated adults, and for adults with pre-treatment incomes below the national median. Moreover, Walmart Supercenter openings lead to a $200 (or 16%) per household per year increase in government income transfers received, and a $920 (or 5%) per household per year decrease in tax revenues.

JEL Classification:	I32, J23, J31, J42, R23
Keywords:	poverty, monopsony power, Walmart, local labor markets, economic inequality

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1 Introduction

> *"Walmart threatened us (...) They told us if they didn't build here, they'd build in a nearby town, and that would have been equally hard on Main Street. Our people were going to shop there whether it was in Independence or 25 miles away. You simply cannot beat Walmart, so we joined them"*
>
> —Bowermaster (1989) in *The New York Times*

Employers often possess considerable market power over workers, which has been widely documented as a source of depressed wage and employment growth (Manning, 2013, 2021). Shifts in the balance of market power have resulted from several mechanisms, such as declining union membership (Farber et al., 2021), but recent research has given particular attention to the role of monopsony power (Manning and Petrongolo, 2017; Azar et al., 2020b,a; Yeh et al., 2022; Bassanini et al., 2024). In many labor markets, a large share of available jobs is concentrated among a small share of employers, generating market inefficiencies that allow employers to compensate workers below their marginal value (Benmelech et al., 2022; Azar et al., 2023).

Scholars have studied the consequences of monopsony power through the use of place- and time-varying measures of employment concentration (Hirsch et al., 2018, 2020; Rinz, 2022; Martins and Melo, 2024; Qiu and Sojourner, 2023); using occupation- or task-based-varying indicators of employment concentration (Schubert et al., 2024; Dodini et al., 2024); through firm-specific variation in concentration (Webber, 2015); in online labor markets, such as Amazon Mechanical Turk (Dube et al., 2022); and through place- and time-variations in mergers (Prager and Schmitt, 2021; Thoresson, 2024) or the openings of specific monopsonistic employers, such as Walmart Supercenters

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(e.g., Wiltshire 2023). This study follows the latter approach, investigating how the introduction of a Walmart Supercenter in a county affects poverty outcomes in the U.S.

Recent studies of the effects of Walmart Supercenters have established that Walmart, as America's largest employer, exerts monopsonistic power in local labor markets (Wiltshire, 2023); however, the social and economic consequences of this monopsony power are less clear. Previous studies examined the effect of the Supercenters on employment and earnings at the county level (Basker, 2005a; Dube et al., 2007; Neumark et al., 2008), but without clear implications for Walmart's effects on poverty and public finances.

Distinct from prior studies, we use person-level data (rather than county aggregates) from the restricted-access version of the Panel Study of Income Dynamics (PSID), a source of panel data that documents household composition and labor market characteristics throughout individuals' lives. Using the restricted-access data with local geographic identifiers, we can identify individuals in counties where Walmart Supercenters opened and follow these individuals (and their household members) before and after the store openings. This setting allows us to match individuals, across a broad set of covariates, living in a county where a Walmart Supercenter opened with individuals living in a county where a Supercenter did not open. That is, we create a more-similar set of controls than county aggregates would allow. Our strategy alleviates concerns that Walmart may select into places with different trends of poverty, which would make the identification of treatment effects empirically challenging. As we discuss further in Section 3, after our matching of treatment- and control-group individuals, we apply stacked difference-in-differences estimators to evaluate the effects

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of Walmart Supercenter openings.

We find that the opening of the first Walmart Supercenter in a county leads to an increase in poverty by around 2 percentage points (a 16 percent increase relative to the pre-treatment mean). The effect persists over time and increases in magnitude, as we find that poverty is still 3 percentage points higher in treated counties 10 years after the Walmart opening. Increases in poverty rates are larger for individuals who are without a college degree, younger, unemployed, or live in lower-income households (prior to treatment). Individuals unemployed before the Walmart Supercenter's arrival are the most severely affected, with a 4.9 percentage point increase in the likelihood of being in poverty in the decade following a Supercenter opening. Overall, our results indicate that local monopsony power increases households' likelihood of poverty.

Investigating the mechanisms behind these results, we find that Walmart Supercenters lead to a large and persistent decrease in annual labor income. In particular, annual household earnings decline by $4,230 (6 percent of their pre-treatment values) after the opening of a Supercenter. Earnings losses widen during the 10 years after a Supercenter opening. This finding is in line with previous evidence showing that an increase in labor market concentration leads to a reduction in earnings, including for Walmart Supercenters specifically (Wiessner and Wiessner, 2024).[1]

The consequences of Walmart Supercenter openings are not limited to private incomes, however. Consistent with declining earnings and rising poverty, we find that

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[1]Walmart, in particular, has been subject of accusations to systematically undermine the power of labor to pay low wages as widely reported by *Reuters, Bloomberg*, and others (Wiessner and Wiessner, 2024; Purifoy, 2024) and investigated by the United Nations Special Rapporteur (De Schutter, 2023; Mundy, 2023).

Supercenter openings generate a negative fiscal externality in the form of larger sums of publicly-funded income transfers and reduced tax revenues. Supercenter openings increase income transfers received by $200 per household per year (a 16 percent increase relative to pre-treatment), and reduce tax revenues by $920 per household per year (a 5 percent decline). Our conservative, back-of-the-envelope calculations suggest that Supercenters impose an average annual cost of $3 billion to public finances through this combination of increased transfer payments and reduced tax revenues.

Our study contributes to the literature on the consequences of monopsony power. Here, most studies have found that higher labor market concentration can decrease wages and employment. There is some limited evidence that trade unions and especially minimum wages can mitigate this effect (Benmelech et al., 2022; Azar et al., 2023; Qiu and Sojourner, 2023). We contribute to this literature by using person- and household-level panel data to study whether these negative employment effects also translate into poverty increases, and whether this generates a fiscal externality on public finances.

We also contribute to a literature that has examined the economic consequences of Walmart Supercenter openings. This literature has been motivated by the relatively large size of Walmart as the main employer in the U.S., as well as its introduction of new labor management practices. In particular, studies have found that the opening of a new Supercenter leads to price reductions (also because other retailers reduce prices) (Hausman and Leibtag, 2007; Basker and Noel, 2009) and an improvement in store management (Matsa, 2011). However, studies have also found conflicting effects on employment and wages (Basker, 2005a; Dube et al., 2007; Neumark et al.,

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2008), with the most recent evidence showing strong negative effects on both (Wiltshire, 2023).

We add to this literature in several ways. First, we move beyond county aggregates when studying the consequences of monopsony power, and Walmart Supercenter entries specifically. Estimates of mean county-level effects conceal potentially large heterogeneities in effects across different demographic groups and do not allow researchers to rule out compositional effects (i.e., changes in the population of counties through migration) in estimating Walmart's effects. Our individual-level data allow us to estimate differences in Supercenter effects across education, age, and other characteristics to provide finer analyses on whom Walmart Supercenters primarily affect. Moreover, our use of panel data allows us to identify individuals' pre-treatment economic circumstances to produce effect heterogeneities across levels of pre-treatment income and employment.

Second, we move beyond individual labor market consequences. Most studies interested in employment and income effects focus on a narrow set of county-level indicators including employment, earnings, and labor force participation. Personal earnings and employment losses may not affect consumption or well-being, however, if other members of the household compensate for such losses through increased work intensity, or if government income transfers can offset the lost earnings. Measuring household-level poverty outcomes is thus a useful indicator for understanding the broader welfare implications of Walmart Supercenter entries. Our measure of poverty is closely comparable to the Supplemental Poverty Measure (SPM), as explained in Section 3.

Third, we study the negative externalities of Walmart for public finances. Should Walmart entries drive down earnings, means-tested income transfers may com-

pensate for declining earnings, while affected individuals may contribute less to the state in tax revenue.[2] Both of these mechanisms—more transfers and smaller tax liabilities—weigh on public budgets. In studying their combined effect, we provide new evidence on how local monopsony power generates negative fiscal externalities.

2   Walmart as a Source of Monopsony Power

The study of monopsony power in labor markets, first theorized by Robinson (1933), seeks to understand employers' power to set wages below workers' marginal revenue product of labor paid in competitive labor markets (Manning, 2003, 2013; Manning and Petrongolo, 2017; Langella and Manning, 2021). Many studies find negative impacts of monopsony power (often proxied through employer concentration) on employment and job security, as well as productivity and wages (Azar et al., 2020b; Marinescu et al., 2021; Benmelech et al., 2022; Berger et al., 2022; Qiu and Sojourner, 2023; Bassanini et al., 2023, 2024; Martins and Melo, 2024; Schubert et al., 2024; Dodini et al., 2024). Negative effects are often concentrated among lower earners, which increases earnings inequality (Webber, 2015; Manning, 2021; Rinz, 2022).

Walmart serves as an appropriate case study in employer monopsony power in local labor markets as the largest private-sector employer in the U.S. and the world's largest retailer (Azar et al., 2023; Dube et al., 2018; Naidu et al., 2018). The majority of Walmart jobs are concentrated in Walmart Supercenters, each typically employing

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[2]Hicks (2015) examined correlations of Walmart stores and income transfers at the state level, though we are unaware of plausible-causal evidence that studies how monopsony power affects public finances.

over 350 workers, which amounts on average to about 20 percent of a county's retail employment and 2.5 percent of its total employment before a Supercenter's opening (Holmes, 2011; Wiltshire, 2023). Supercenters have experienced rapid growth over the recent decades: their count increased from 68 stores in 1994 to just over 1,900 stores by 2005 and 3,571 stores by 2019. In the 1990-2005 period alone, Walmart has added over one million workers to its payroll, accounting for 50 percent of all retail employment growth and 4 percent of total employment growth in the U.S. (Wiltshire, 2023). The growth of Supercenters significantly outpaced that of competitors and resulted in a drop in the market share of traditional grocery stores from 90 percent in 1988 to 44 percent in 2019 (Redman, 2019). For comparison, Target Superstores achieve only about 8 percent of Walmart's sales. Large retail and department stores are particularly affected by declining demand and downward-price competition with Walmart, especially in rural markets (Artz and Stone, 2006; Basker and Noel, 2009; Matsa, 2011; Hicks et al., 2012; Atkin et al., 2018).

We are not the first to study employer monopsony power in local labor markets using the example of Walmart Supercenters. Wiltshire (2023) describes the underlying mechanism of monopsony power due to Supercenter entries: the Supercenter opening causes a direct positive shock to employment by introducing a large employer into the area. However, this positive effect can be outweighed by indirect negative spillovers if the employer operates in a non-tradable sector, competes with incumbent local firms, and is a significant actor in the local market. All three conditions are met by Walmart Supercenters. Additionally, when Supercenters represent a large portion of a supplier's sales, their opening can affect employment in the local supply chain by pressuring local

suppliers to cut costs and redirect production toward Walmart's established networks of non-local suppliers. Wilmers (2018) provides evidence for this mechanism, which has gained importance over the past decades through "Fissuring of the workplace" (Weil, 2014) and seems particularly relevant for Walmart as the archetypical large upstream firm with considerable buyer power (Lichtenstein, 2012).

Wiltshire (2023) reinforces the assertion that Walmart holds considerable monopsony power: Supercenter openings reduce local employment and earnings. As a consequence, Walmart is able to absorb minimum wage increases without laying off workers. An exogenous minimum wage increase in counties with a Supercenter even leads to large employment gains, which suggests that Walmart pays wages below the marginal revenue product of labor, a sign of the presence of monopsony power exercised by Walmart Supercenters. Separately, Dube et al. (2007) provide empirical evidence showing that Walmart has monopsony power with negative consequences for earnings (reducing earnings on average, by 0.5 to 0.9 percent). Complementing the quasi-experimental evidence, Dube et al. (2018) conduct an online stated preference experiment with Walmart workers where they find evidence for employer monopsony power. Combined, the findings indicate that the dominance of Walmart Supercenters allows them to reduce competition for labor with negative consequences for local labor markets.

3 Data and Research Design

3.1 Data

Our primary data source is the Panel Study of Income Dynamics (PSID). We use a restricted-access version of the PSID that provides county identifiers for each household each year, and merge it with data on the timing of Walmart Supercenter openings by county from Holmes (2011). The PSID includes rich data on a variety of outcomes that are difficult to observe across counties in a harmonized manner - including post-tax/transfer poverty status, government-provided income transfers, tax liabilities, household composition, labor market outcomes, and more.

The PSID has collected data on individuals since the late 1960s beginning with a nationally representative sample and incorporating new respondents over time. Until 1997, data was collected yearly, whereas it became biennial afterward: since 1997 the PSID has taken place in "odd" years.

From 1988 to 2006, we observe the entries of a total of 1,980 Walmart Supercenters across 1,296 counties in the U.S.[3] We document the annual count of these entries (specifically, the first entry per county) in Appendix Table A1.

We supplement the PSID with further information on tax liabilities, tax-based refunds, and near-cash benefits from the Supplemental Nutrition Assistance Program, which are available in the Cross-National Equivalent Files (CNEF). Thus, we can com-

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[3]We have not been able to identify a database of Walmart Supercenter openings from 2007 onward, hence our endpoint of 2006. Later, we provide evidence that removing data from any given year does not influence our findings, offering suggestive evidence that the exclusion of the few Supercenter openings in these missing years should not meaningfully alter our conclusions.

pute the post-tax/post-transfer incomes of each household in each year. Specifically, the CNEF files supplement the PSID with estimates of federal and state income tax liabilities (including refundable tax credits, like the Earned Income Tax Credit) using the NBER TAXSIM model.[4]

3.2 Outcomes

Our primary outcome of interest is a person's poverty status, measured as whether the person's household income exceeds the poverty threshold. We measure household income using post-tax/transfer income that includes all cash-based income sources that appear in the Official Poverty Measure (OPM), but we also add near-cash benefits (e.g., those from the Supplemental Nutrition Assistance Program), refundable tax credits (e.g., Earned Income Tax Credit), and tax liabilities. Our income definition is thus closer to that used in poverty estimates produced at the OECD or Luxembourg Income Study. Moreover, our income definition is closely comparable to that of the Supplemental Poverty Measure (SPM), though we do not measure more-minor income transfers (such as benefits from the Low Income Home Energy Assistance Program) and we do not deduct for expenses as the SPM does.[5] CNEF simulates tax credits and liabilities into the PSID using TAXSIM, as described above, while all other income values are self-reported.

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[4]The model applied first identifies tax units within each household, but eventually sums tax unit values to the household level to maintain a consistent unit of analysis with all other income variables.

[5]Specifically, the income transfer programs included are cash benefits from AFDC/TANF, SSI, unemployment insurance, worker's compensation, and SNAP, while all tax credits are instead built into the tax liabilities indicator presented subsequently

Given that we cannot replicate the SPM poverty thresholds consistently in the PSID, we apply the OPM thresholds. The OPM thresholds are "fixed" over time, in that they are updated annually only for inflation. The OPM thresholds vary according to household size and structure to account for economies of scale. Our poverty measure is thus a hybrid of the SPM and OPM concepts.

We also measure a household's income transfers (inclusive of the set of transfers detailed above), tax liabilities, and a series of personal labor market outcomes that may affect poverty status and transfers received. These labor market outcomes include employment, retail employment, and labor earnings. We winsorize our three continuous indicators – household income, income transfers, and tax liabilities – at the 5th and 95th percentiles.

3.3   Identification and Inference

We mostly follow Wing et al. (2024) and estimate a trimmed aggregate average treatment effect, using a stacked difference in differences approach (SDD) after matching treatment- and control-group individuals on observable characteristics. Given that the PSID sample is not balanced, using other estimators may be problematic: attriters may be different from the rest of the population, and as such, event study estimates may be hard to interpret if dynamic treatment effects reflect compositional changes in the control group across event-times. SDD allows us to construct stacks that ensure compositional balance throughout the event study—i.e., we can restrict the sample to treatment and control units, which we can observe for an arbitrary number of years

around treatment. Moreover, SDD lets us flexibly restrict the sample to subgroups of interest and investigate conditional average treatment effects. Other estimators do not easily accommodate this objective, as control units may have different covariate values for the same event-time, if they serve as controls for individuals treated in different years. In any case, we show that our results are broadly consistent with those obtained using the estimator of Callaway and Sant'Anna (2021) in Appendix D.

Treatment is defined as the first time an individual i is present in a county c when the location experiences its first Walmart Supercenter entry.[6] We exclude individuals who, upon entry, had previously lived in a different county where Walmart had already established a Supercenter, and restrict our treatment years to 1993 onwards.

Formally, for each treatment cohort with treatment year equal to g, we construct a stack, also denoted by g, with the following inclusion criteria for individual i:

$$i \in g \quad \text{if} \quad \begin{cases} \text{Treated: } D_i = g \text{ and } t_{i,min} \leq g - 5 \text{ and } t_{i,max} \geq g + 5 \\ \\ \text{Control: } D_i > 2006 \text{ and } t_{i,min} \leq g - 5 \text{ and } t_{i,max} \geq g + 5 \end{cases}$$

Where D_i is the treatment year, $t_{i,min}$ the first calendar year in which i is observed, and $t_{i,max}$ the last. We then build a dataset appending each stack, and estimate treatment effects for each event-time in a 20-year window.[7] By avoiding problematic comparisons and the use of previously treated individuals as controls, our estimates circumvent the issues of the TWFE specification in the presence of treatment effect hetero-

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[6]We also consider Supercenter entries in places which already had a discount store—which could, for instance, be converted into a Supercenter

[7]The same control individual may appear in several stacks g. The treated appear only once in the dataset, as we do not use them as controls.

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geneity (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2024).

We further restrict our sample to individuals who, at year g, were aged 15 to 55. Our final sample includes 4,688 individuals treated from 1993 to 2006 in 481 different counties[8], and 4,307 controls in 351 counties. The control group is exclusively made up by individuals of who (1) had not lived, before g, in a different county where Walmart was already established, and (2) in year g, were living in counties where a Walmart Supercenter entry did not take place.

One key advantage of our setup is that we can leverage individual-level information to find appropriate controls. Walmart's entry is not random, and, importantly, the company's motives are not public (we discuss this more below and in Appendix Table A2). This creates an issue of selection on unobservables that may result in violations of the parallel trends assumption. Our data allows us to refine our control group selection by comparing similar individuals in potentially different counties. Following Heckman et al. (1997), we perform kernel-based propensity score matching four years before treatment. That is, for each stack, we match every treated individual to controls with a sufficiently similar propensity score. Then, we reweight controls based on their number of matches.[9] This (1) makes treated and control groups more similar (we demonstrate this in Appendix A), and (2) reduces pre-trend concerns in some of our variables of interest. Furthermore, matching 4 years before treatment lets us exam-

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[8]Note that the number of Walmart-receiving counties in our final sample is larger, as both control and treatment individuals can move to any county after treatment, including eventually treated counties in which we do not observe any individuals at the time of treatment.

[9]More precisely, let i be a control and j some treated unit. $i(j) = 1$ if i is matched as a control for j. The weight assigned to i will be $\sum_{j;i(j)=1} \frac{1}{N_j}$, where N_j is the count of matches for treated unit j.

ine anticipation effects of a Walmart entry and, importantly, alleviate issues related to regression to the mean in DiD settings with matching. Finally, the matching weights constructed separately for each stack automatically satisfy the reweighting strategy proposed by Wing et al. (2024), as we show in Appendix A.

We implement a weighted OLS with the following form, clustering standard errors at the treatment level (county where treated):

$$\text{outcome}_{i,g,\tau} = \alpha_t + \gamma_\tau + \lambda_{i,g} + \sum_{\tau=-5}^{-2} \delta_\tau \times D_{g,i} + \sum_{\tau=0}^{5} \beta_\tau \times D_{g,i} + U_{i,g,\tau}$$

Where α_t, γ_τ and $\lambda_{i,g}$ are calendar-time, event-time and individual-stack fixed effects, respectively, δ_τ collect the pre-treatment coefficients, and β_τ the post-treatment coefficients of interest. $U_{i,g,\tau}$ are the residuals.

Note that τ are pooled at the 2-year level - that is, $\tau = -1$ includes both 1 and 2 years before the event, whereas $\tau = 0$ includes years 0 and 1 after the event; and so on.[10] In our figures, we likewise pool estimates at the two-year level but include actual years-to-event in the x-axis for readability.

We also provide aggregations of the β_τ coefficients by estimating their means.[11] To check the validity of the parallel trends assumption, we separately estimate linear

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[10]As the PSID has been biennial since 1997, we assign treatment one year ahead for individuals treated in even years from 1997 onwards. This does not matter for interpretability, as we aggregate time-to-event coefficients at the two-year level. By doing so, we can display the Average Treatment Effect aggregated at the time-to-event level without worrying about compositional changes from one event-time to the next (for example, if we did not aggregate, all odd event-times would be made up exclusively by even treatment years before 1997).

[11]In doing so, our estimate is benchmarked against $\tau = -1$ - i.e., the "base period" $g - 1$ - and not the whole pre-period as in a traditional pre-post specification. This follows the advice of Callaway et al. (2024).

pre-trend coefficients in our main specifications, and report the results in Appendix Table B1.

3.4   Where do Walmart Supercenters Open?

Our identification strategy to understand the consequences of Supercenter openings relies on the place- and time-varying nature of Supercenter openings. In this respect, it is worth mentioning that Walmart's decision to open Supercenters in specific counties at a given point in time is clearly not random. Previous studies have shown that geography played an important role in this respect. Specifically, Walmart initially expanded its network in the Southeast and lower Midwest regions (a pattern also evident from Figure 1), following its initial opening in Benton county (Dube et al., 2007; Neumark et al., 2008). Figure 1 documents geographic and temporal variation in the opening of Walmart Supercenters.

Figure 1: Walmart Supercenter openings by county and period

Walmart Openings

Date of first Walmart Supercenter opening by county

Year

■ 1988–1994
■ 1995–2000
■ 2001–2006
■ NA
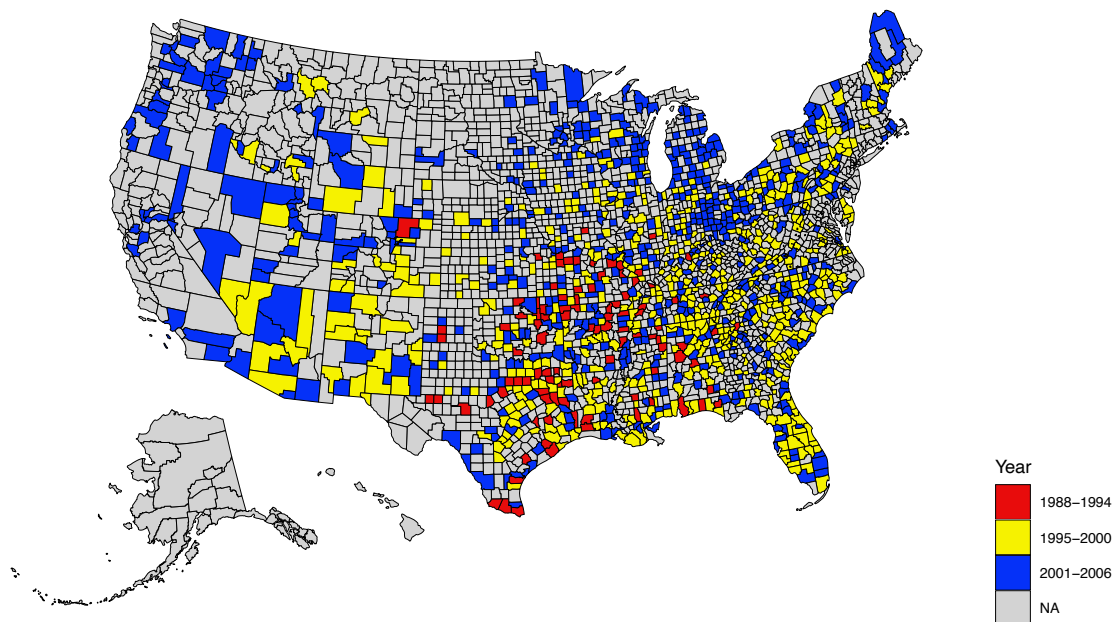
To understand what other factors influenced Walmart's decision to open in specific counties, we conduct a county-level analysis regressing Walmart opening against pre-opening, county-level information from the Census Bureau.[12] We include year fixed effects in all analyses, while presenting separately results with and without state fixed effects. This is to isolate the within-state Walmart county selection process from the

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[12]Specifically, we assign information from the 1990 or 2000 Census to counties, based on whether Walmart opened a Supercenter in that county in the ensuing decade. Control counties are those where Walmart did not open over the same period of time. Note that the analysis does not cover the universe of Walmart openings, owing to missing county identifiers for small counties in the Census.

wider network expansion strategy that took place across the U.S. We find that, within states, Walmart selected more rural regions, while no statistically significant differences emerge with respect to the age or racial composition of the population. Walmart also selected counties with lower educational attainment, while there were no differences in pre-opening employment rates. Both within and across states, Walmart did not specifically decide to open in counties with higher average household income. However, selected counties had on average higher levels of Social Security income, but lower income from transfers (Appendix Table A2).

Overall, these results show that Walmart entered relatively more rural and less-educated counties, while no stark differences emerge with respect to the socio-economic and labor market characteristics of the selected counties. While this provides some first evidence of the fact that treated and control counties are comparable along many dimensions of interest, it is also worth noting that differences in pre-treatment characteristics do not necessarily represent a threat to identification in a DiD setting. This is especially true in the present context, given that we match similar individuals in potentially different counties in our estimation strategy. Moreover, we conduct validity tests supporting the parallel trends assumption for each of our analyses.

4 Findings

4.1 The Effects of Walmart Supercenter Openings on Poverty

Figure 2 presents an event study that documents the effects of Walmart Supercenter openings on poverty. During the 10 years before a Supercenter entry, the counties that eventually receive the Supercenter have comparable trends in poverty status relative to the control counties. (In Table B1, we conduct validity tests supporting the parallel trends assumption.)

After the Supercenter entry, however, poverty rates increase for the treatment group and persist for up to 10 years after entry. Point estimates range from 1 to 3 percentage points in the post-treatment window. Averaging over the post-treatment window, we find that the Supercenter entry leads to a 2 percentage point increase in poverty, or a 16 percent increase relative to the pre-treatment mean for the treatment group (see Table 1).

Figure 2: Estimated effects of Walmart Supercenter openings on poverty
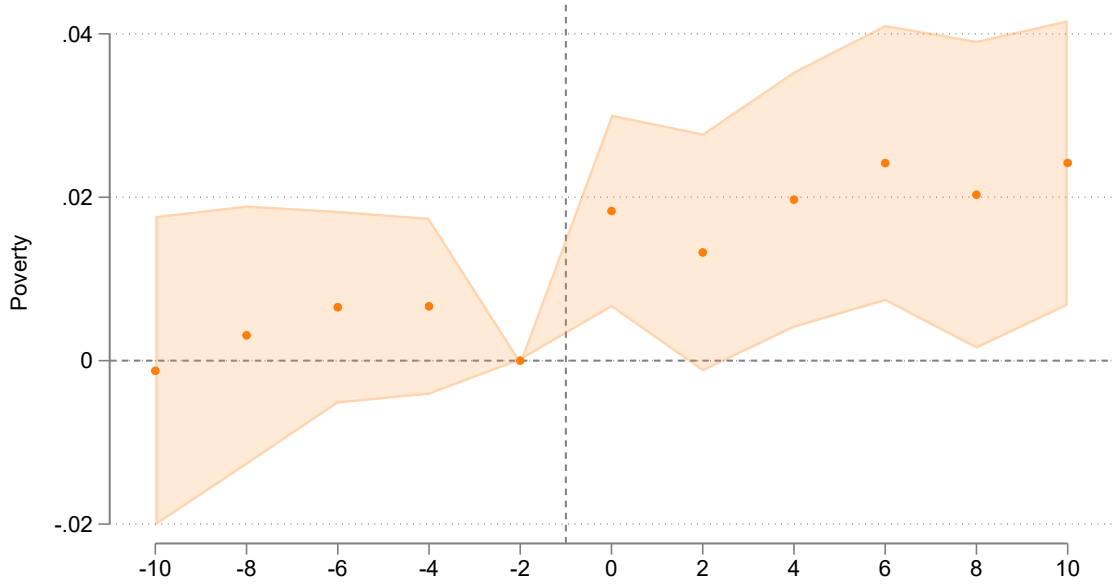
Note: We cluster standard errors at the county-where-treated level. Poverty is measured as household income (inclusive of taxes and transfers, including SNAP and EITC benefits) relative to the OPM poverty line. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on poverty, with the shaded region representing 95 percent confidence intervals.

How do these results vary by subgroup? And, in turn, which subgroups are driving our overall effects? Table 1 provides the point estimates from the difference-in-differences models by education status, age, pre-treatment employment status, and pre-treatment income status. In each case, the percentage point increase in poverty is largest for the group that was more disadvantaged prior to the Supercenter's entry, suggesting that the Supercenters exacerbate pre-existing disparities. Specifically, we

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Table 1: Estimated effects of Walmart Supercenter openings on poverty, by subgroup

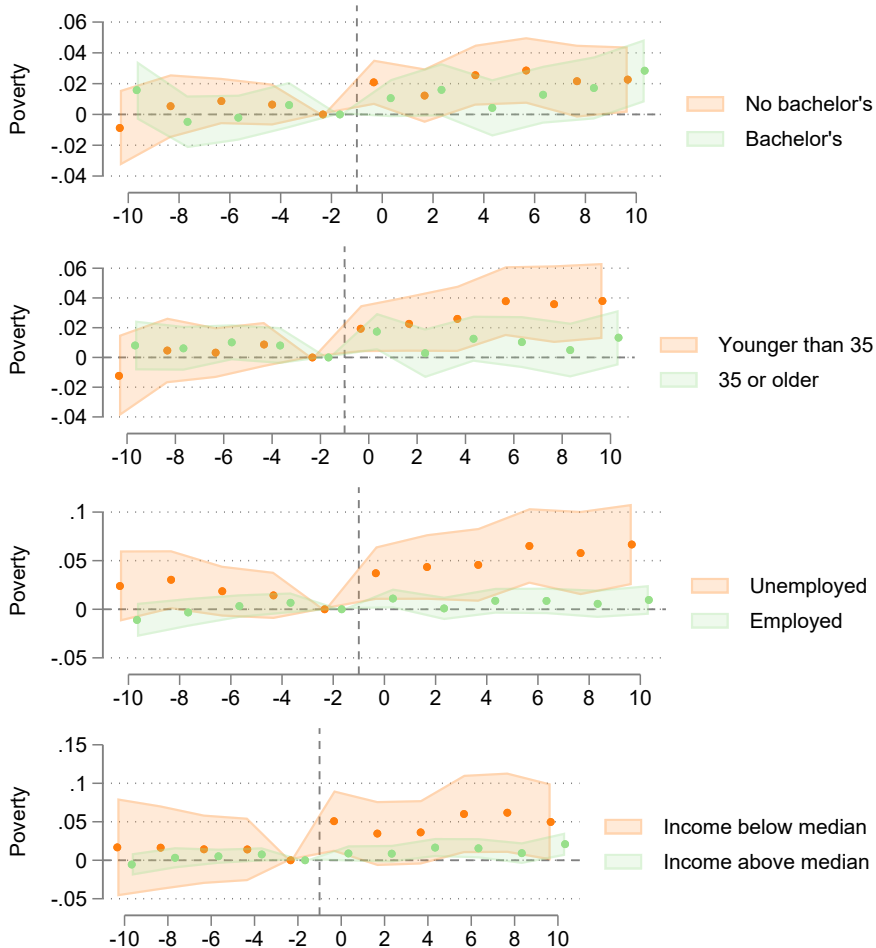|	Poverty								
	All	Low education		Young		Jobless		Low income	
		Yes	No	Yes	No	Yes	No	Yes	No
Treat × Post	0.020***	0.022***	0.015**	0.030***	0.010	0.053***	0.007	0.049**	0.013***
	(.007)	(.008)	(.007)	(.009)	(.007)	(.016)	(.005)	(.019)	(.005)
Poverty rate treat	0.125	0.155	0.043	0.159	0.090	0.222	0.086	0.381	0.039
Poverty rate control	0.133	0.169	0.043	0.170	0.094	0.270	0.078	0.453	0.033
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4308	2979	1315	2635	2402	2206	3337	1809	3501

Note: All column-defining characteristics are measured two years before treatment. Each specification includes individual-stack fixed effects (FE), calendar-year FE, and a post-treatment dummy. We cluster errors at the county-where-treated level. "Low education" represents no Bachelor's degree. "Young" represents individuals under the age of 35. "Jobless" indicates not having a job in the pre-treatment reference year. "Low income" represents a household income net of taxes below the national median for the respective pre-treatment calendar year.

find evidence of a 2.2 percentage point (14 percent) increase in poverty for individuals with only a high school degree, a 3 percentage point (19 percent) increase for young individuals (under age 35), a 5 percentage point (24 percent) increase for adults who were jobless when the Supercenter entered, and a 4.9 percentage point (13 percent) increase for adults with pre-treatment household incomes below the national median.

Given that poverty is a household-level indicator, and that Walmart Supercenters are likely to have cross-sector employment consequences, we should not expect the effects of Supercenters on poverty to be confined to the individuals most likely to work at Walmart. Though adults older than age 35 and those who were employed when the Supercenter entered the county largely avoided increases in poverty relative to comparable adults in counties where a Supercenter did not enter, adults with more than a high school degree and adults with pre-treatment household incomes above the median experienced increases in poverty (1.5 percentage point and 1.3 percentage point

increases in poverty, respectively). As we discuss later, we also present results on a sample of adults who were employed in retail for at least one post-treatment year (for our treatment group, this represents the closest proxy we have to employment at Walmart); for this group, we find a 2.4 percentage point increase in poverty due to a Supercenter entry (Appendix Figure E1).

Figure 3: Event study of estimated effects of Walmart Supercenter openings on poverty, by subgroup

Note: We cluster standard errors at the county-where-treated level. Poverty is measured as household income (inclusive of taxes and transfers, including SNAP and EITC benefits) relative to the OPM poverty line. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on poverty, with the shaded region representing 95 percent confidence intervals.

Figure 3 provides the event study specification by subgroup. Beyond the changes in poverty summarized in Table 1, Figure 3 supports our pre-treatment par-

allel trends assumption for each of the subgroups that we examine. Moreover, the findings show that the dynamic effects are particularly notable (and increasingly large) for younger adults and adults who were jobless at baseline.

4.2   Mechanisms: Why Do Supercenters Affect Poverty?

The entry of Walmart Supercenters into a county increases individuals' likelihood of poverty, and particularly so for individuals with greater pre-treatment disadvantages. We now investigate the labor market mechanisms through which these effects occur.

Figure 4: Estimated effect of Walmart Supercenter entry on earnings, employment, and retail work
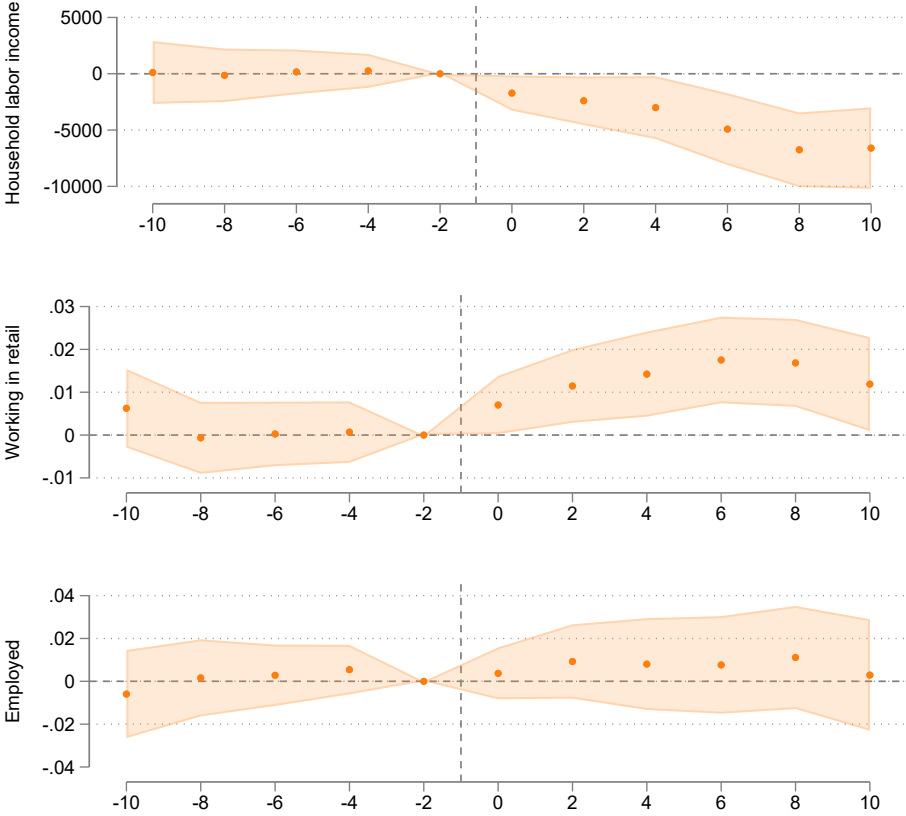
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.The sample is restricted to individuals aged 15–55 years at the time of treatment.

Figure 4 presents the event study plots of our primary labor market outcomes of interest: labor income, retail employment, and overall employment. Focusing first on labor income, we find a steady decline in annual household earnings for adults (aged 15 to 55) after a Walmart Supercenter entry. Specifically, we find a decline of $4,230 (6

percent relative to pre-treatment earnings) in household earnings for the average adult in the post-treatment window. For a full-time, full-year worker, this is equivalent to a decline in hourly wages of approximately $2 per hour. Though we show household earnings here to align with the unit of analysis when evaluating poverty, we also find negative and statistically significant declines of 6 percent when evaluating personal labor earnings. The decline in earnings due to an increase in monopsony power is consistent with evidence from Dube et al. (2007), Wiltshire (2023), and Neumark et al. (2008).

We present subgroup analyses in Appendix Table C1. In absolute terms, the declines in household earnings were larger for higher-educated, older, and higher-income workers, though this is largely an artifact of these workers having higher pre-treatment household earnings levels.[13] In relative terms, however, adults who were jobless just before the Supercenter entry see the strongest declines in household earnings ($7,620, or 12 percent of the pre-treatment mean, relative to comparable adults in counties where Walmart did not enter).

The decline in earnings coincides with an increase in the likelihood that adults work in retail (see middle panel of Figure 4). Specifically, we find that the likelihood that an adult works in retail increases by 1.3 percentage points following a Walmart Supercenter entry. This finding is consistent with past literature that suggests (1) Walmart Supercenters increase retail employment in the area where they enter (Basker, 2005a; Wiltshire, 2023) and (2) Walmart Supercenters tend to pay relatively low wages

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[13]Though some of these negative effects are due to household composition effects (e.g., a high-income worker co-residing with a low-income worker who is more directly affected by a Supercenter entry), the negative effects also likely represent the cross-sector spillover effects of Supercenter entries and Walmart's power over local suppliers. (Wiltshire, 2023)

(Wiltshire, 2023; Neumark et al., 2008; Dube et al., 2022). The increase in retail employment persists for over a decade following the Walmart Supercenter's entry. Our subgroup analyses in Appendix Table C1 find that the larger percentage-point gains in retail employment are concentrated among lower-educated adults (1.5 p.p.), young adults (1.6 p.p.), adults who were jobless when the Supercenter opened (1.8 p.p.), and adults who had pre-treatment incomes above the median (1.8 p.p.).

The lower panel of Figure 4 suggests that Supercenter entries have positively-signed, but statistically insignificant effects on overall employment among adults in treated counties. That overall employment increased less than retail employment suggests declining employment in other sectors and a reallocation of labor toward retail jobs. With respect to our subgroup analyses, we find substantively small and statistically insignificant increases in overall employment for lower-educated workers, young workers, and adults who were jobless upon Walmart's entry; for all three subgroups, the overall increases in employment are driven primarily by increases in retail employment, which is consistent with Walmart Supercenters offering (low-pay) employment to less-experienced workers. We present event study plots for all subgroup analyses in Appendix C, though we acknowledge that several of our subgroups have too small of samples to produce estimates with high precision.

4.3 Fiscal Externalities: How Supercenters Affects Taxes and Transfers

Given that the introduction of Walmart Supercenters reduces earnings and increases poverty, their entry is also likely to carry negative fiscal externalities. This can occur given that the government will spend more on income transfers (recall that transfers are inclusive of benefits from AFDC/TANF, SSI, unemployment insurance, worker's compensation, and SNAP), and receive less in tax revenues (inclusive of increases in tax credits, such as those from the EITC) when labor earnings decline.

Figure 5 presents the event study plot that documents the effects of Walmart Supercenters on public transfers received (top panel) and household taxes paid (bottom panel). We do not observe divergent trends prior to treatment for either outcome (this is further supported in Appendix Table B1). Increases in public transfers, and reductions in taxes paid, occur after the Supercenter entries and persist for up to 10 years afterward.

Figure 5: Effect of Walmart Supercenter entries on income transfers and tax liabilities
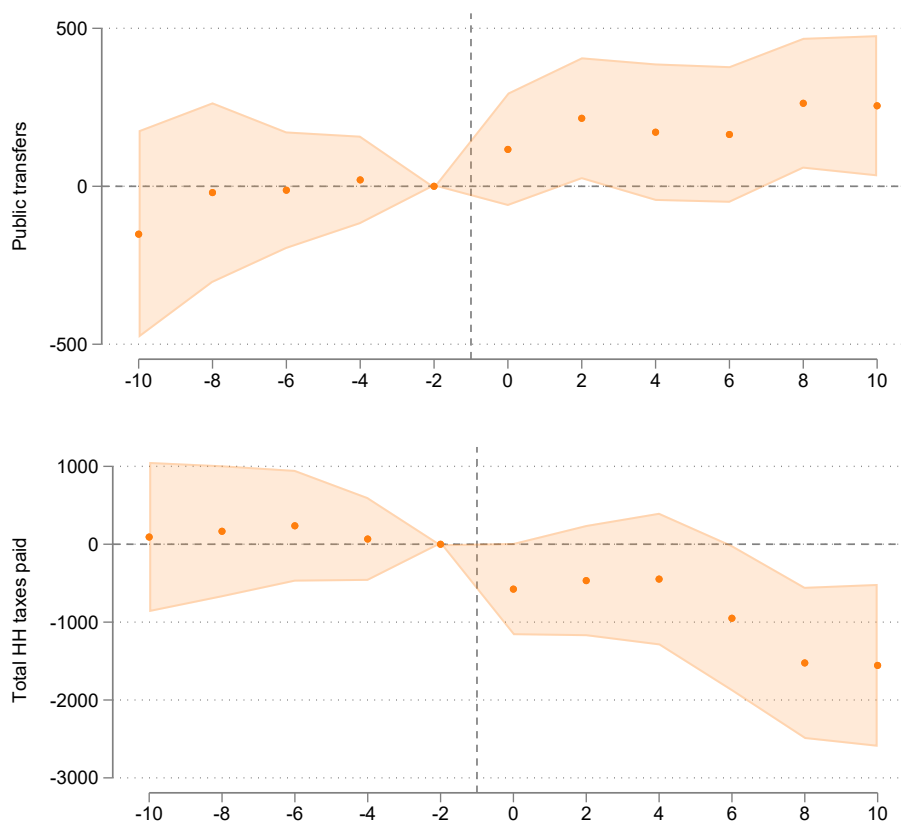
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcomes, with the shaded region representing 95 percent confidence intervals. Public transfers are inclusive of benefits from AFDC/TANF, SSI, unemployment insurance, worker's compensation, and SNAP. All tax credits, such as the EITC, are built into tax liabilities.

We summarize these effects and present subgroup analyses in Table C2. For the average household, the opening of a Walmart Supercenter increases annual income transfers received by $200, representing a 16 percent increase in income transfers relative to the pre-treatment mean. Consistent with our poverty findings, the increases

in household income transfers received are largest for lower-educated workers and for adults who were jobless prior to Walmart's opening. Adults who were jobless at baseline see the largest increase in transfers received: $410 on average, or an 18 percent increase in household transfers received. On the one hand, the increase in transfers points to the stabilizing effect of the social safety net in mitigating what could have been larger increases in poverty. On the other hand, the findings suggest that Walmart Supercenter entries carry negative externalities in the form of increased public expenditures to compensate for declining private incomes.

With respect to tax liabilities, Walmart Supercenter entries led tax units in the average household to pay $920 less in federal and state (combined) income taxes, a 5 percent decline relative to the pre-treatment mean. Declining tax liabilities can be due to both declining personal earnings (and, thus, lower levels of taxes paid) plus an increase in refundable tax credits (such as benefits from the EITC) that accrue when personal earnings are positive yet relatively low.

We present event study specifications for each subgroup in C2. The event study figures show evidence supporting the parallel trends assumption in both the poverty (top panel) and public transfers (bottom panel) analyses for each subgroup.

Using our estimates of increased transfers and declining tax revenues, we can produce back-of-the-envelope calculations of the total fiscal externalities of Walmart Supercenter entries. We estimate a conservative (lower-bound) fiscal price tag of $2.4 million (2021 USD) per Supercenter entry per a given year in our post-treatment window. Aggregating across all observed Supercenter entries, the total fiscal externality

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amounts to \$3 billion per year in our conservative estimates.[14] Thus, the monopsony power that Walmart Supercenters exert also carries notable negative fiscal externalities.

4.4 Additional Analyses and Robustness Tests

We perform several sets of additional analyses to evaluate the consistency of our results when accounting for price reductions; across alternative model specifications; with alternative sample inclusion criteria; and with alternative decisions related to weighting and model controls.

4.4.1 Accounting for Price Reductions

Our study finds that Walmart reduces household earnings and, in turn, increases poverty rates. However, Walmart Supercenters also reduce prices (relative to competitor retailers in a local labor market). Thus, declining incomes due to Supercenter entries may not be representative of declining consumption power. To account for this possibility, we provide a range of alternative estimates of Supercenters' effects on poverty after accounting for hypothetical price reductions.

Basker (2005b) finds that Walmart reduces average retail prices by 7 percent to 13 percent in the years after Walmart's entry. Similarly, Volpe and Lavoie (2008) finds declines of 6 to 7 percent for national brand goods, and 3 to 8 percent for private

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[14]We calculate this back-of-the-envelope estimate by, first, estimating the lower-bound (from 95 percent confidence intervals) effect of Walmart Supercenter entries on total tax and transfer liabilities, which we then multiply by counties' annual population counts in the year of treatment (specifically, the total population of the county from Census estimates divided by the mean number of persons per household).

label goods. For completeness, we provide estimates that account for price reductions spanning beyond this range, namely from a 2 percent decline to a 20 percent decline.

Given that retail items are only one portion of consumers' basket of consumption necessities, we rely on weights commonly applied in the computation of the Consumer Price Index (CPI) to scale the price reductions. Food items overall account for about 16 percent of the CPI-W, while food at home accounts for around 10 percent (BLS, 2023). Given that Walmart shoppers may also save on some other non-food items and a 10 percent weight may be too low. Thus, we provide estimates in which we weigh the price reductions at 10 percent and 20 percent. Thus, in our most conservative scenario of a 20 percent price decline with a 20 percent weight in the price index, Walmart's entry increases purchasing power by 4 percent before accounting for observed earnings declines.

Figure 6: Estimated effects of Walmart Supercenter openings on poverty when adjusting for various estimates of price declines and price index weights
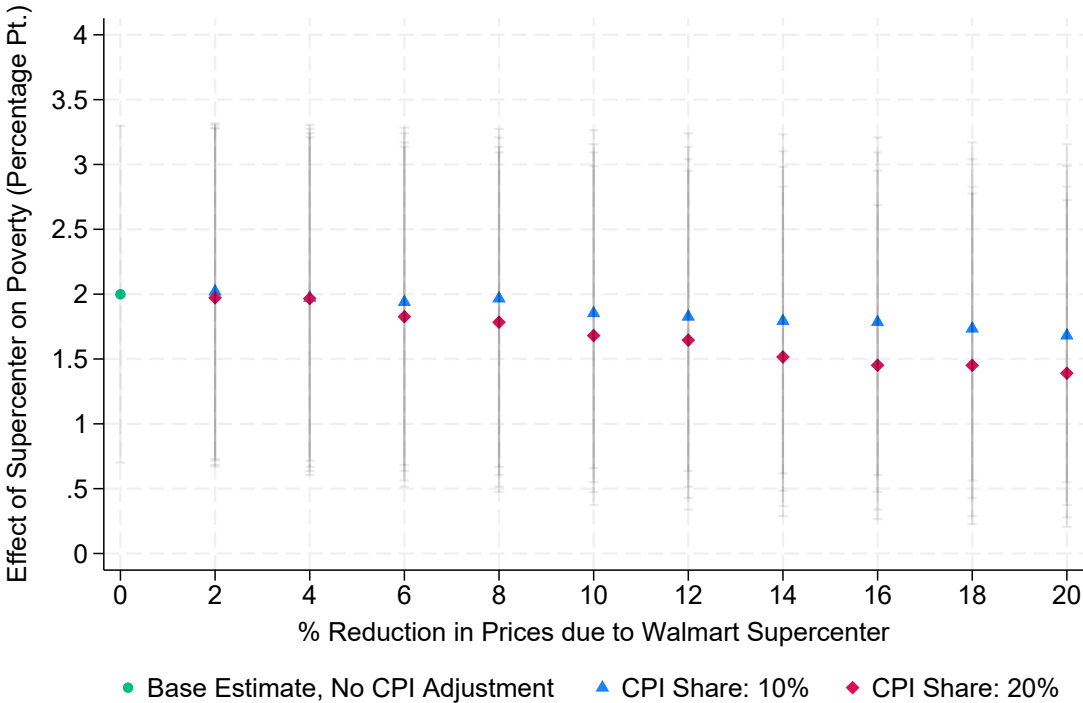
Note: All point estimates are bounded by 95 percent confidence intervals. Differences between our baseline poverty estimate and the adjusted poverty estimates are not statistically significant at conventional levels.

Figure 6 presents the alternative poverty estimates accounting for this range of price and weight decisions. As anticipated, the point estimate of Walmart's increases in poverty declines when the assumed price reduction is larger, and especially when applying the 20 percent CPI weight. At a 10 percent price decline with a 10 percent PCI weight, the estimated effect on poverty drops from 2 percentage points to 1.7 percentage points, with the difference not being statistically significant. In our most conservative estimate (20 percent price decline and 20 percent weight), the estimated increase in poverty is just under 1.5 percentage points, still a statistically significant

positive effect on poverty, yet not significantly different from our baseline estimate. Accounting for Walmart's potential price reductions does not fully offset the effects of Walmart Supercenter entries on poverty.

4.4.2 Alternative Modeling Decisions

In our primary analyses, we identify treatment status as whether a person lives in a county where a Walmart Supercenter opens. However, individuals often work and shop across county borders. As a sensitivity test, we re-estimate our primary results when defining treatment as whether a Supercenter enters one's own county or a contiguous (i.e., bordering) county. Including contiguous counties into our treatment definition increases the number of treated individuals in our sample and shifts the timing of treatment to earlier periods (largely in the mid-1990s), as expected when broadening the treatment definition. Appendix Figure D1 shows that our results are comparable when incorporating contiguous counties. Point estimates of Supercenters' effects on poverty drops from 2 percentage points in our primary analysis to 1.5 percentage points in this sensitivity test.

In Appendix Figures D2 and D3, we replicate our results using the Callaway and Sant'Anna specification for difference-in-differences estimates (with and without matching, respectively). The results are broadly consistent with our primary findings (declining earnings, increasing poverty rates, and increases in public transfers paid).

4.4.3 Alternative Sample Inclusion Criteria

In Appendix Figure E1, we re-estimate our primary findings when limiting our sample to adults who work in retail employment during at least one year in the post-treatment period. For our treatment group (individuals in counties where a Walmart Supercenter opens), working in retail after the Supercenter opens is the closest proxy we have to working directly for Walmart (though we emphasize that we cannot directly observe each individual's employer). As discussed previously, Supercenter openings are likely to have downstream consequences for workers and firms outside of the retail industry; thus, our preferred specification remains our full sample that is not limited to adults who have worked in retail. At the same time, a particularly robust set of results should find strong consequences of Supercenter openings on retail workers specifically. We find that this is indeed the case. For our sample of adults who work in retail in the post-treatment window, Supercenter openings lead to a 2.4 percentage point increase in poverty, a $7,623 decrease in annual household earnings, a $199 increase in annual income transfers received per household, and a $1,853 decline in annual tax liabilities per household. The event study in Appendix Figure E1 shows evidence supporting our parallel trends assumption for each estimate.

In Appendix E, we perform two sets of "leave-one-out" estimates to test the sensitivity of our results to the exclusion of specific states and years. We present leave-one-out point estimates using our baseline estimates but computed after dropping the given state or year. In Figure E2, we show that dropping individual states does not meaningfully affect our point estimates on poverty or transfers. In Figure E3, we

similarly find that dropping any given year in our sample does not meaningfully alter our conclusions (thus, our results are not driven by any particular outlier).

In Appendix Figures E4 and E5, we account for the fact that some of the counties in our treatment group (which, by definition, are county-years with the first entry of a Walmart Supercenter) receive subsequent Supercenters in the post-treatment window. Specifically, we document that the average treated county has three Walmart Supercenters present at 10 years after the first Supercenter entry; meanwhile, our control group has, by definition, no Walmart Supercenters in any year.

In Figure E5, we present results that relax the requirement that control counties have no Walmart openings for the whole post-period. Doing so results in parallel movement of treatment intensity after a first treatment; in Figure E5, we see that the gap in the number of Walmart Supercenters in treated and control counties does not expand if we allow the latter to eventually open Walmart Supercenters. We then provide alternative results with this relaxed criteria for the control group in Figure E6; our results are largely similar to our primary analyses.

In Appendix E7, we further restrict the timing of our sample to Walmart Supercenter entries before the year 2000. Given that we do not observe Walmart Supercenter entries beyond 2006, this assessment ensures that our primary results are not affected by the possibility of individuals in our control groups for later-treated cohorts being treated after 2006 while still serving as controls in a post-treatment window. Even when restricting to pre-2000 Supercenter entries, we find declining labor earnings, increasing poverty, increasing income transfers received, and declining tax liabilities for individuals in treated counties (though with more noise).

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4.4.4 Alternative Sensitivity Analyses

In Appendix Figure F1 and F2, we conduct Fisher Randomization Tests, commonly referred to as permutation tests (Fisher, 1935; Roth et al., 2023). More precisely, we assign treatment (first Walmart Supercenter entry) at random, for each year, keeping the distributions of first entries unchanged relative to the observed one (i.e., if there were 200 first entries in 1994, we simulated 200 random entries in 1994). We then perform our full set of analyses on the random treatment sample and repeat the process 330 times. Given potential dependencies across individuals in treated and non-treated counties that may not be accounted for in our clustered standard errors, the permutation tests offer another tool for ensuring the statistical significance of our findings. Furthermore, it allows us to ensure that uncertainty related to our matching strategy should not affect the robustness of our findings. Put differently, the main advantage is to address uncertainty in our matching strategy that is not included in the confidence intervals in our primary analysis. We plot the distribution of placebo effects in Figure F1. The treatment effects from our original analysis are consistently larger than 95 percent of the draws from our randomized-treatment permutations, allowing us to reject the null hypothesis of no treatment effects. Figure F2 provides similar estimates in histogram format and confirms our assessment of p-values. These permutation tests add further confidence that our analyses are capturing a real effect of the entry of Walmart Supercenters on poverty and taxes/transfers.

In Appendix Figure F3, we replicate our core results while controlling for state- and year-level variation in state policy generosity and institutional features. Specifically,

we account for state-year variation in minimum wage levels, state supplements to the federal EITC, trade union membership, maximum benefit levels and maximum duration of receipt from Unemployment Insurance. The inclusion of these controls does not affect our primary findings.

5 Conclusions

Past studies demonstrate that Walmart Supercenters exert monopsony power, but what are the consequences of this power imbalance for poverty? This study investigates this question using restricted-access Panel Study of Income Dynamics data spanning from 1970 to 2019. Different from prior research, we move beyond county-level estimates of personal earnings and employment to study how Supercenters affect household incomes and poverty. Moreover, we are able to study effect heterogeneity across demographic- and economic-based subgroups, and to calculate the negative fiscal externalities of Supercenter openings.

Our findings reveal that the introduction of a Supercenter is associated with a 2 percentage point (16 percent) increase in poverty, even when accounting for all major taxes and income support programs. The increases in poverty disproportionately affect younger and less-educated adults, as well as those with pre-treatment incomes below the national median. These effects, which persist for at least a decade following a Supercenter's opening, are primarily driven by reductions in annual earnings. Specifically, we find evidence of a $4,230 (6 percent) decline in annual earnings for individuals in counties where Supercenters opened. For context, this amounts to a decline of approx-

imately \$2 per hour worked for a full-time, full-year employee. The declining earnings co-exist with an increase in retail employment, consistent with past evidence that Walmart's presence increases the prevalence of low-pay retail work (Wiltshire, 2023).

Notably, the consequences of Supercenters are not limited to private incomes. We also find evidence of increased costs for the state. Specifically, we estimate that the entry of a Walmart Supercenter increases the level of annual transfers received by \$200 (16 percent) for the average household, and reduces tax liabilities by \$920 (5 percent) per household per year. Our conservative, back-of-the-envelope calculations suggest that Supercenters, in aggregate, impose an average annual cost of \$3 billion to the state through this combination of increased transfer payments and reduced tax revenues.

Our results are robust to accounting for potential price declines to Supercenter entries (Figure 6), accounting for Supercenter entries in bordering counties (Appendix Figure D1), to alternative difference-in-difference estimators, and to a variety of other sensitivity analyses documented in Appendices C through F.

We acknowledge that our study has important limitations. First, any underreporting of means-tested transfers in the PSID may bias our estimates of poverty increases, particularly if underreporting was affected by a Walmart Supercenter entry. Given a lack of evidence of place-based variation in benefit underreporting, however, it is unlikely that, say, misreporting of SNAP benefits may meaningfully affect our findings. Second, we are unable to identify the specific employers of workers in our sample; as such, we cannot distinguish between the direct and indirect estimates of Walmart Supercenter openings. Given that Walmart Supercenters are likely to have

spillover consequences for non-Walmart workers, this concern does not carry meaningful consequences for our baseline findings. Moreover, we are able to present results for adults who work in retail after a Supercenter enters (see Appendix Figure E1, and we find particularly strong increases in poverty and decreases in annual earnings for this group relative to retail workers in counties where Walmart Supercenters did not enter). With these limitations in mind, our findings nonetheless contribute to the growing literature on monopsony power: this study uniquely documents how the presence of a monopsonist employer affects poverty and public finances.

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Part

Appendix

Table of Contents

A   Research Design

Table A1: Number of first entries
of Walmart Supercenters over time

Year	Number of first entries
1988	2
1989	2
1990	2
1992	2
1993	16
1994	82
1995	112
1996	109
1997	94
1998	116
1999	146
2000	178
2001	142
2002	228
2003	185
2004	258
2005	255
2006	51

Table A2: County-level analysis on the determinants of Walmart opening

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of men	-2.589	0.861						
	(1.672)	(1.666)						
Average age	-0.006	0.011						
	(0.008)	(0.009)						
Share of white	0.244*	0.158						
	(0.134)	(0.137)						
Share of non-hispanid	-0.103	-0.148						
	(0.143)	(0.142)						
Share of rural	0.178	3.098*						
	(2.035)	(1.854)						
Share attending school			-0.259	-0.542				
			(0.490)	(0.523)				
Share with grade 12 or below			1.092***	0.845***				
			(0.300)	(0.312)				
Share employed			0.238	0.614				
			(0.463)	(0.501)				
Average family income					-0.000	-0.000		
					(0.000)	(0.000)		
Average wage income							-0.014***	-0.002
							(0.004)	(0.004)
Average Social Security income							0.208***	0.182***
							(0.052)	(0.063)
Average income from Welfare							-0.039	-0.138*
							(0.076)	(0.078)
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	762	762	762	762	762	762	762	762
R-squared	0.025	0.369	0.045	0.374	0.021	0.362	0.064	0.375

Notes: The table presents coefficient estimates and standard errors from different regressions run at the county level. Data comes from the 1990 and 2000 versions of the Census. The dependent variable is equal to one if Walmart opened a Supercenter in the county during the decade following the Census observation (i.e., between 1990 and 1999 for the 1990 Census and between 2001 and 2009 for the 2000 Census), and zero otherwise. For each county, the independent variables are computed as population averages using sampling weights. The analysis does not include the full sample of Walmart openings that will be used in the rest of the analysis, given that not all counties are identifiable in the Census. *** denotes significance at the 1% level.

Table A3: Mean of characteristics by treatment status, with and without propensity score matching weights

Variable name	No matching				PSM weights			
	Control	Treated	Difference	P-value	Control	Treated	Difference	P-value
Labor Earnings	41312	34523	-6747	0.007	37501	34809	**-2685**	0.190
	(69400)	(48169)	(2482)		(50880)	(48411)	(2046)	
Retail	.057	.057	**-.001**	0.913	.06	.056	-.004	0.512
	(.232)	(.232)	(.005)		(.238)	(.23)	(.006)	
Hourly wage	24.6	21.4	-3.23	0.003	23.5	21.5	**-2.05**	0.034
	(32.1)	(30.1)	(1.09)		(31.2)	(30.1)	(.967)	
Hours worked	1410	1405	-5.97	0.883	1407	1406	**-1.38**	0.974
	(1038)	(1051)	(40.7)		(1042)	(1054)	(42.1)	
Employed	.73	.714	-.015	0.375	.724	.715	**-.01**	0.580
	(.444)	(.452)	(.017)		(.447)	(.452)	(.018)	
Poor	.125	.106	-.019	0.350	.121	.104	**-.018**	0.377
	(.331)	(.308)	(.021)		(.326)	(.305)	(.02)	
Poor pre-transfers	.161	.151	-.01	0.686	.158	.149	**-.009**	0.715
	(.367)	(.358)	(.026)		(.364)	(.356)	(.026)	
Transfers	2514	2305	**-205**	0.440	2784	2291	-494	0.049
	(6545)	(6693)	(265)		(7215)	(6635)	(250)	
Living ch. head	.268	.264	**-.004**	0.811	.277	.266	-.01	0.480
	(.443)	(.441)	(.015)		(.448)	(.442)	(.015)	
Age	34	32.9	-1.05	0.009	33.7	33.1	**-.576**	0.194
	(11.5)	(12)	(.401)		(11.7)	(12)	(.443)	
Female	.554	.551	-.003	0.773	.551	.553	**.001**	0.918
	(.497)	(.497)	(.011)		(.497)	(.497)	(.012)	
Years of edu.	12.9	12.8	-.144	0.308	12.8	12.8	**.01**	0.938
	(2.34)	(2.19)	(.141)		(2.28)	(2.19)	(.133)	
Married	.535	.507	-.029	0.294	.53	.513	**-.017**	0.545
	(.499)	(.5)	(.028)		(.499)	(.5)	(.028)	
White	.572	.578	.008	0.894	.587	.58	**-.005**	0.943
	(.495)	(.494)	(.061)		(.492)	(.494)	(.064)	
Total HH taxes paid	23935	19957	-3978	0.032	21877	20284	**-1611**	0.330
	(30874)	(26133)	(1855)		(26612)	(26321)	(1652)	
Works full time	.778	.762	-.016	0.341	.773	.762	**-.011**	0.517
	(.416)	(.426)	(.017)		(.419)	(.426)	(.017)	

Note: Bold text indicates the approach resulting in the smallest difference between treated and control groups. Across covariates, PSM weights reduce the differences between groups, except for mean transfers, share of White individuals, and retail employment share. Averages are calculated 2 years before treatment and then across stacks. P-values are obtained in a regression of the covariate on a treatment dummy two years before the treatment date, clustering errors at the county-where-treated level.

B   Supporting Evidence

Figure B1: Descriptive information on earnings for retail and non-retail workers and the poverty threshold
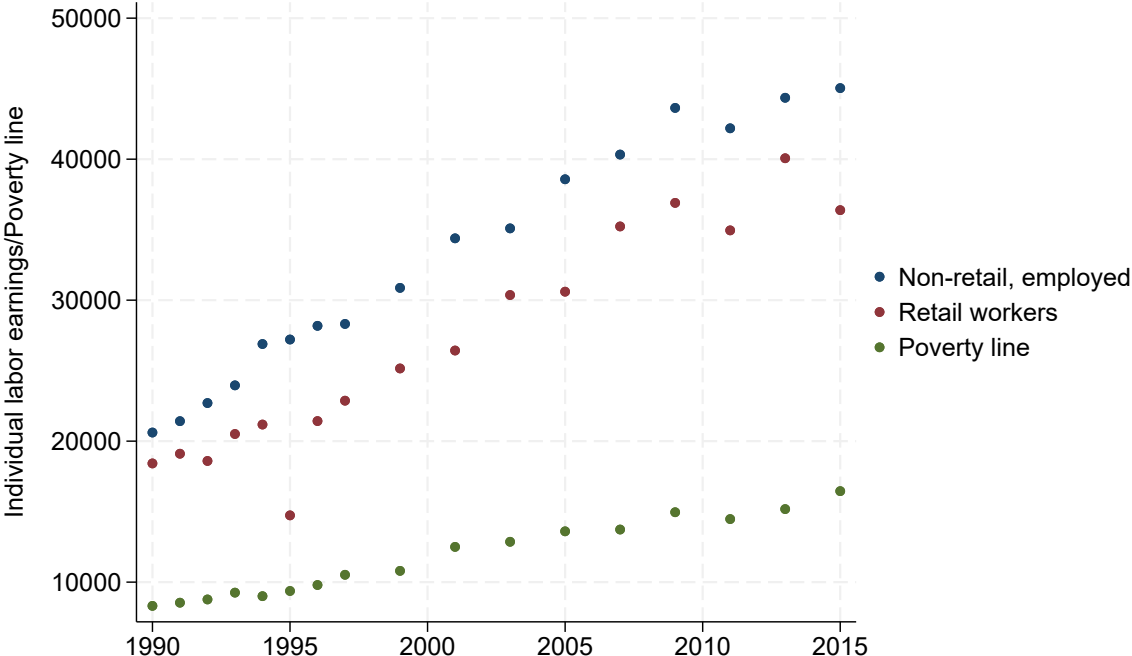
Note: Mean labor earnings are measured at the individual level while poverty is measured at the household level. The poverty line is based on the Official Poverty Measure definition, though all poverty assessments in the manuscript use a post-tax/transfer measure of household resources.

Figure B2: Event study with an alternative employment measure: hours worked per week is higher than 30
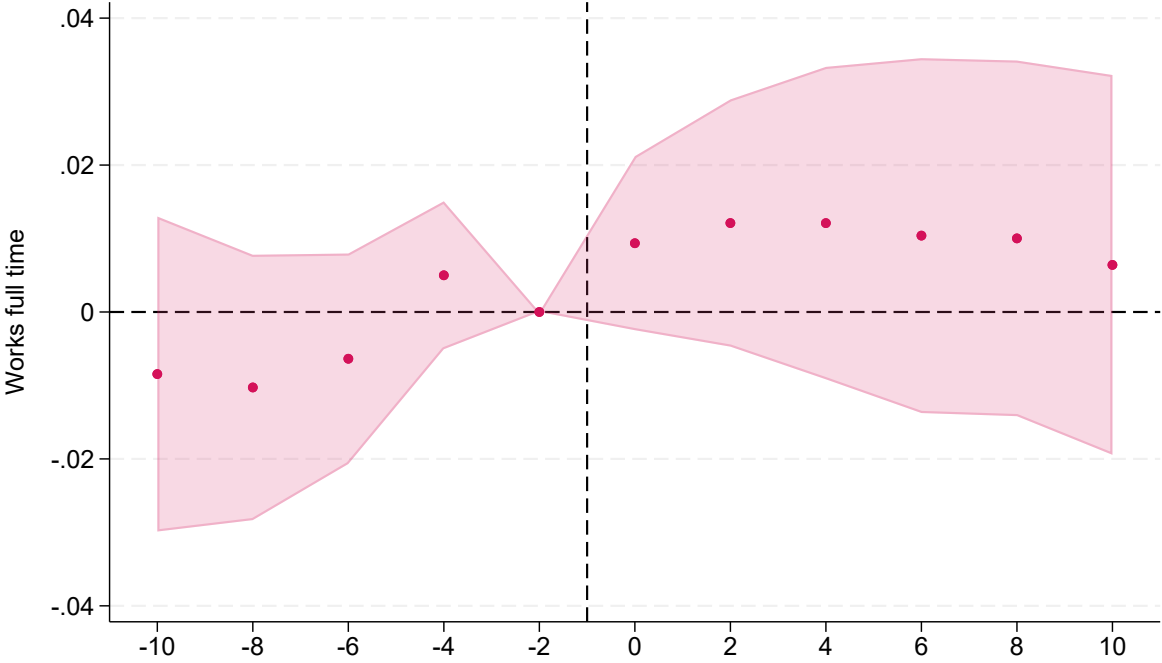
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.

Table B1: Estimates of differential pre-treatment trends in outcomes

	Poverty	Transfers	Taxes paid	Labor earnings	Retail	Employment
Trend coefficient	0.001	40.886	-44.800	146.608	-0.001	0.002
P-value of trend	0.740	0.566	0.813	0.770	0.312	0.489
Number of treated	4688	4688	4688	4688	4688	4688
Number of controls	4308	4309	4309	4309	4309	4309

Note: Errors clustered at the county-where-treated level (across stacks). The analysis supports our parallel trends assumption for each of the outcomes listed.

C   Additional Subgroup Analyses

Figure C1: Event study: Effects of Walmart Supercenter entries on earnings, retail and employment by subgroup
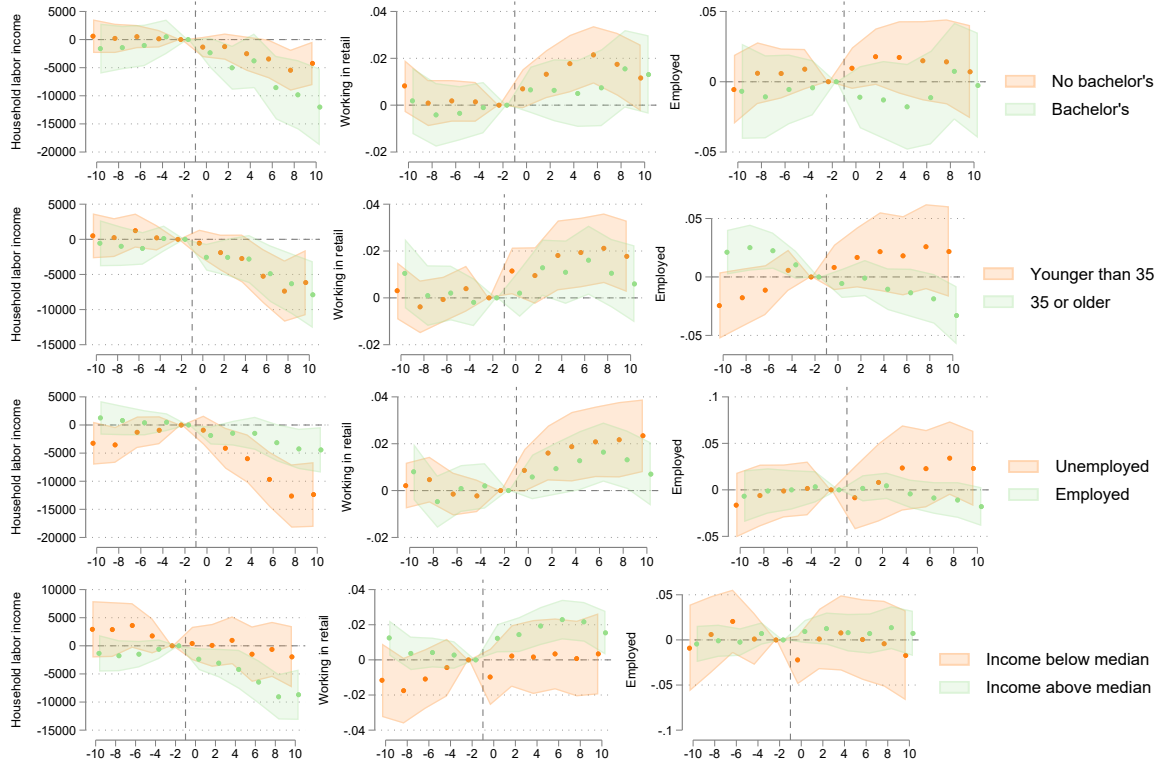
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals..

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Table C1: Difference in differences estimates of the effects of Walmart Supercenters on labor market outcomes by subgroup

	All	Low education		Young		Jobless		Low income	
		Yes	No	Yes	No	Yes	No	Yes	No
HH labor earn. (1000's)									
Treat × Post	-4.23***	-3.04**	-6.92***	-3.99***	-4.50***	-7.62***	-2.77**	-0.44	-5.64***
	(1.21)	(1.34)	(2.11)	(1.47)	(1.51)	(1.9)	(1.27)	(2.01)	(1.47)
Outcome baseline treat	75.95	64.73	107.45	65.29	87.10	65.29	80.27	30.35	91.30
Outcome baseline control	78.69	65.07	112.59	70.52	87.14	62.51	85.39	27.65	94.72
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4309	2980	1315	2636	2402	2207	3337	1810	3501
Working in retail									
Treat × Post	0.013***	0.015***	0.009	0.016***	0.010*	0.018***	0.011**	0.000	0.018***
	(.004)	(.005)	(.006)	(.006)	(.006)	(.005)	(.005)	(.008)	(.004)
Outcome baseline treat	0.060	0.067	0.039	0.048	0.070	0.007	0.080	0.061	0.058
Outcome baseline control	0.062	0.069	0.043	0.051	0.072	0.013	0.081	0.053	0.065
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4309	2980	1315	2636	2402	2207	3337	1810	3501
Employed									
Treat × Post	0.007	0.013	-0.008	0.019	-0.014*	0.017	-0.006	-0.006	0.010
	(.009)	(.012)	(.013)	(.014)	(.008)	(.017)	(.007)	(.018)	(.009)
Outcome baseline treat	0.680	0.664	0.724	0.498	0.875	0.193	0.879	0.520	0.738
Outcome baseline control	0.685	0.664	0.737	0.507	0.858	0.193	0.877	0.480	0.751
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4309	2980	1315	2636	2402	2207	3337	1810	3501

Note: All column-defining characteristics are measured two years before treatment. Every specification includes individual-stack FE, calendar-year FE and a post treatment dummy. Errors clustered at the county-where-treated level (across stacks). Low education stands for no post-high-school education, young for age under 35, jobless for not having a job and low income for having household income net of taxes below the national median for the respective calendar year. Finally, note that the outcome baseline levels are also measured two years before stack treatment dates and correspond to weighted means of the outcome using the propensity score weights.
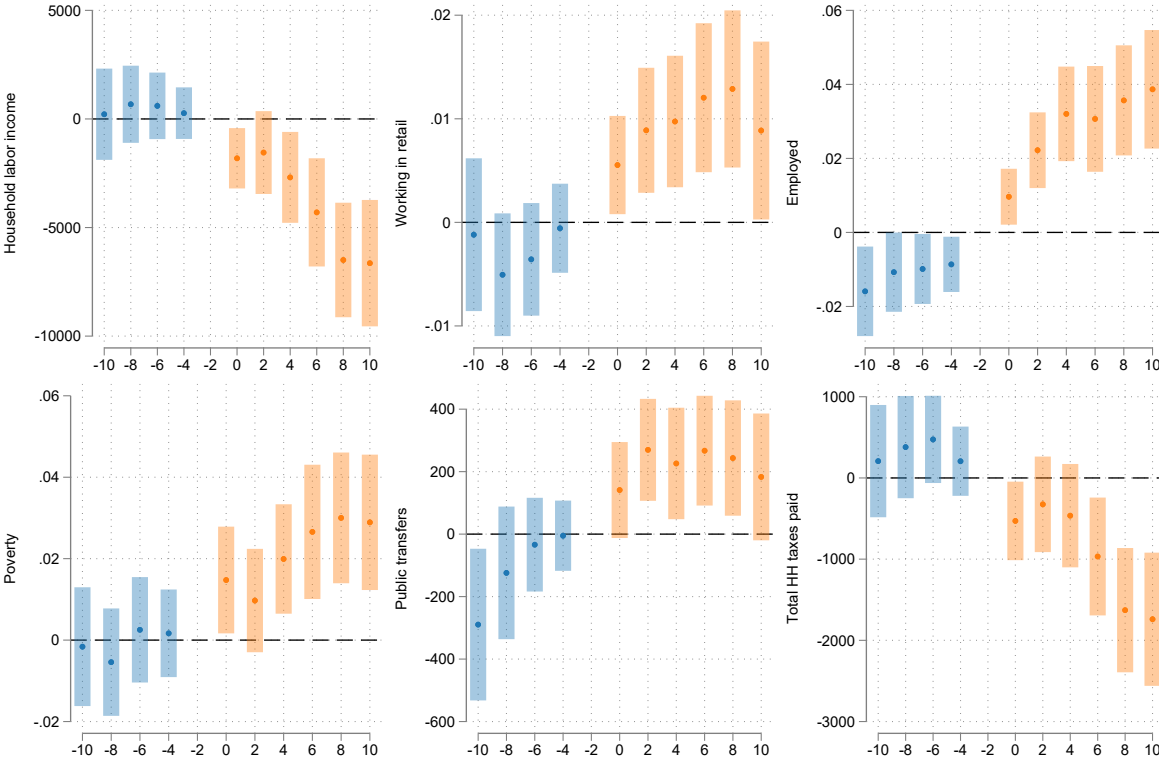
Figure C2: Event study: Effects of Walmart Supercenter entries on tax liabilities and transfers received by subgroup
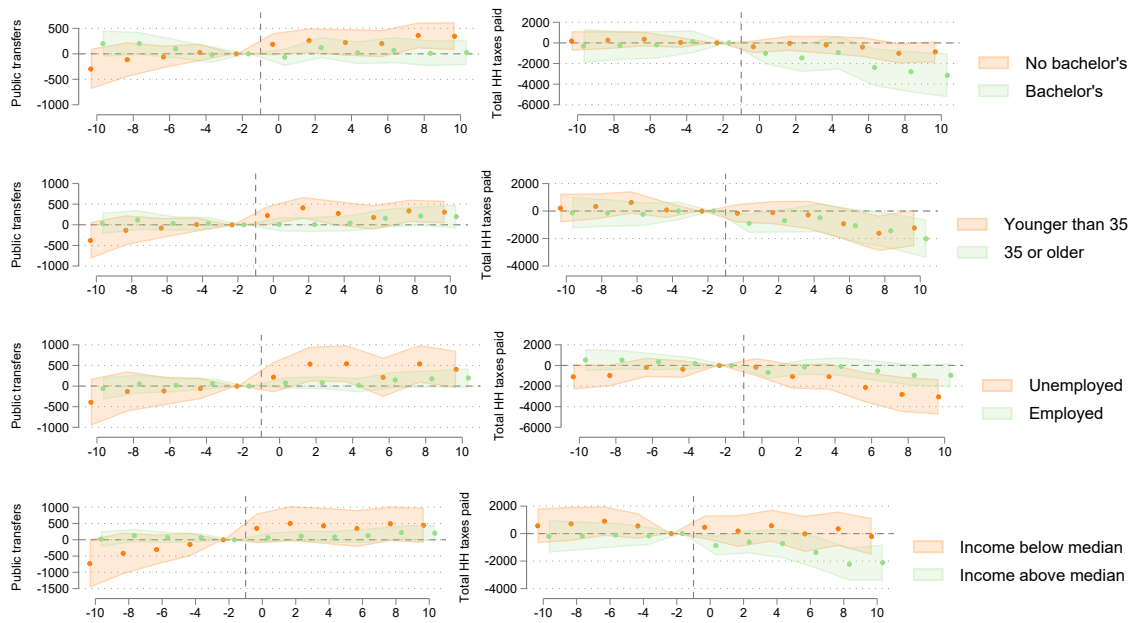
Table C2: Difference in differences estimates of the effects of Walmart Supercenters on tax and transfer outcomes by subgroup

	All	Low education		Young		Jobless		Low income	
		Yes	No	Yes	No	Yes	No	Yes	No
Public transfers (1000's)									
Treat × Post	0.20**	0.26**	0.03	0.29**	0.10	0.41**	0.12*	0.43*	0.14*
	(.08)	(.10)	(.09)	(.12)	(.08)	(.19)	(.07)	(.23)	(.08)
Outcome baseline treat	1.26	1.57	0.39	1.59	0.92	2.18	0.89	3.18	0.62
Outcome baseline control	1.70	2.10	0.69	2.11	1.29	3.18	1.10	4.43	0.84
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4309	2980	1315	2636	2402	2207	3337	1810	3501
Taxes paid (1000's)									
Treat × Post	-0.92**	-0.48	-1.95***	-0.72	-1.10**	-1.72***	-0.56	0.22	-1.32***
	(.36)	(.37)	(.71)	(.44)	(.46)	(.57)	(.38)	(.53)	(.45)
Outcome baseline treat	17.71	13.78	28.77	14.02	21.55	14.47	19.01	3.93	22.34
Outcome baseline control	19.18	14.44	30.97	16.42	22.02	14.39	21.17	3.64	24.06
Number of treated	4688	3422	1262	2310	2261	1305	3266	1180	3391
Number of controls	4309	2980	1315	2636	2402	2207	3337	1810	3501

Note: All column-defining characteristics are measured two years before treatment. Each specification includes individual-stack fixed effects (FE), calendar-year FE, and a post-treatment dummy. We cluster errors at the county-where-treated level. "Low education" represents no Bachelor's degree. "Young" represents individuals under the age of 35. "Jobless" indicates not having a job in the pre-treatment reference year. "Low income" represents a household income net of taxes below the national median for the respective pre-treatment calendar year. The outcome baseline levels are also measured two years before stack treatment dates and correspond to weighted means of the outcome using the propensity score weights.

D Alternative Identification Strategies

Figure D1: Event study: Effects of Walmart Supercenters on poverty when defining treatment as whether own county or contiguous county experience Supercenter entry
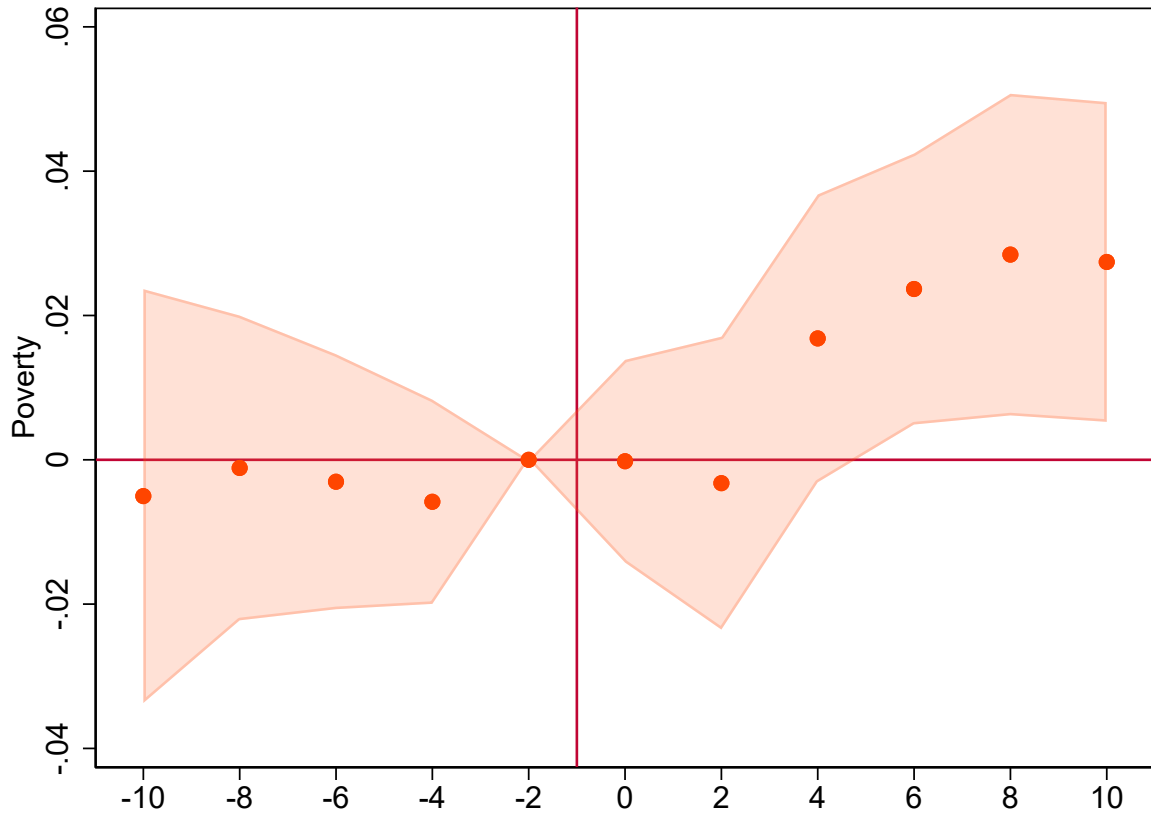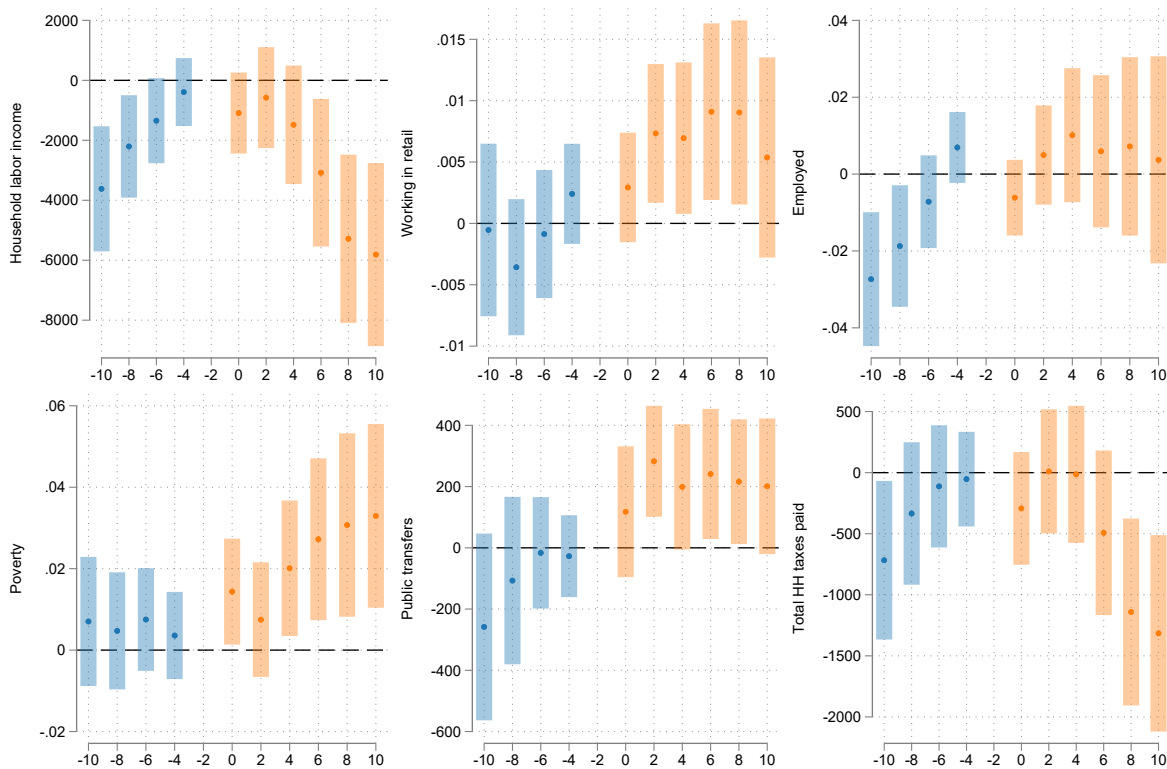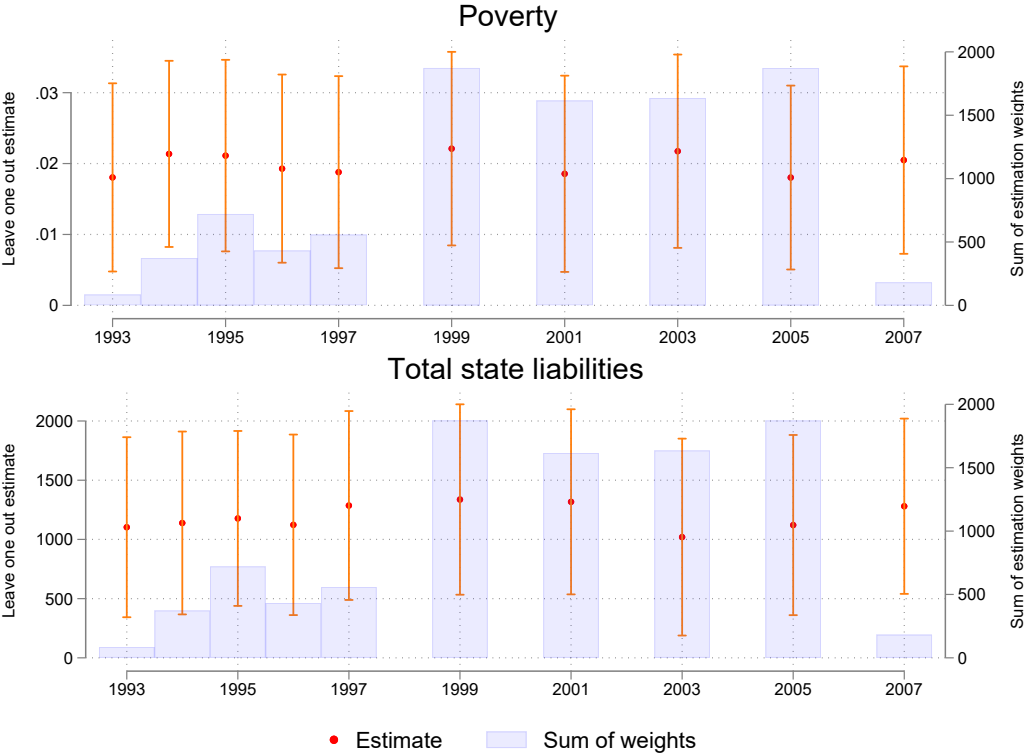

Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county or contiguous county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.

Figure D2: Callaway & Sant'Anna with matching: Estimates of Walmart Supercenters on outcomes

Figure D3: Callaway & Sant'Anna without matching: Estimates of Walmart Supercenters on outcomes

Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.

E Alternative Sample Inclusion Criteria

Figure E1: Estimated effects of Walmart Supercenter openings on outcomes among individuals worked in retail for at least one year in post-treatment period
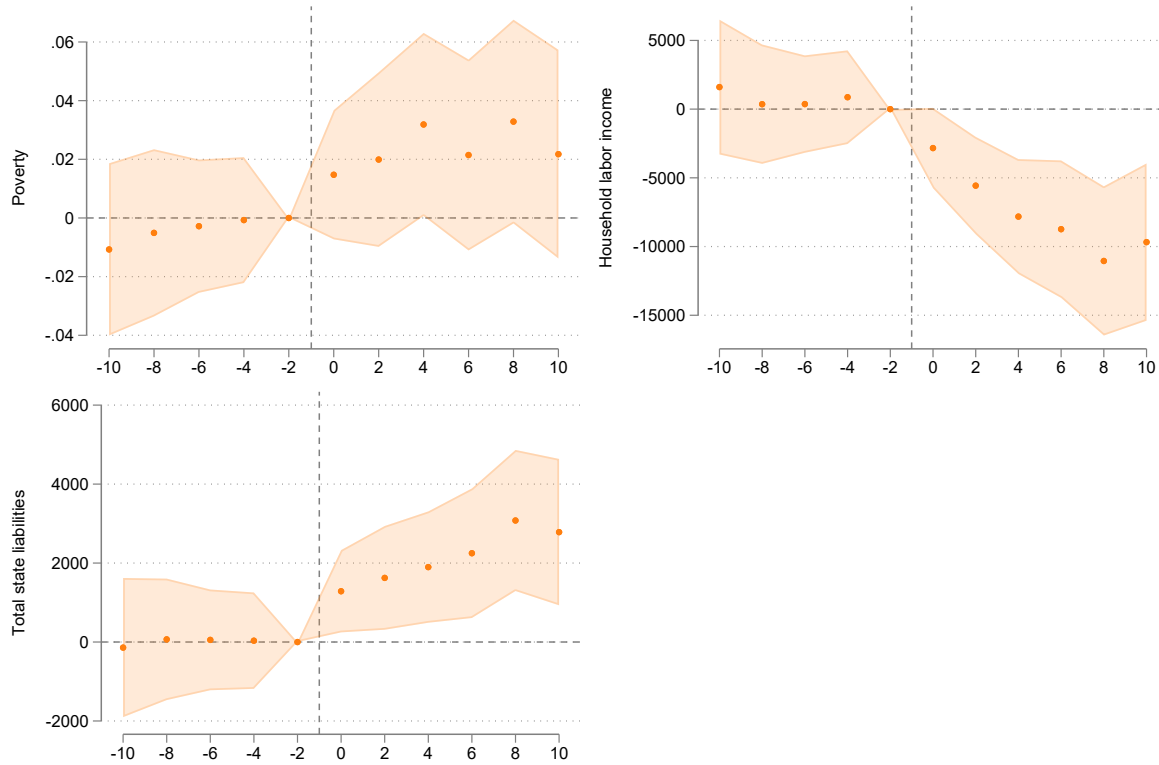
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals. The treatment group is stably defined as adults in households where at least one person worked in retail between treatment years 0 and 10.

**Figure E2:** Leave-one-out estimates, by state.
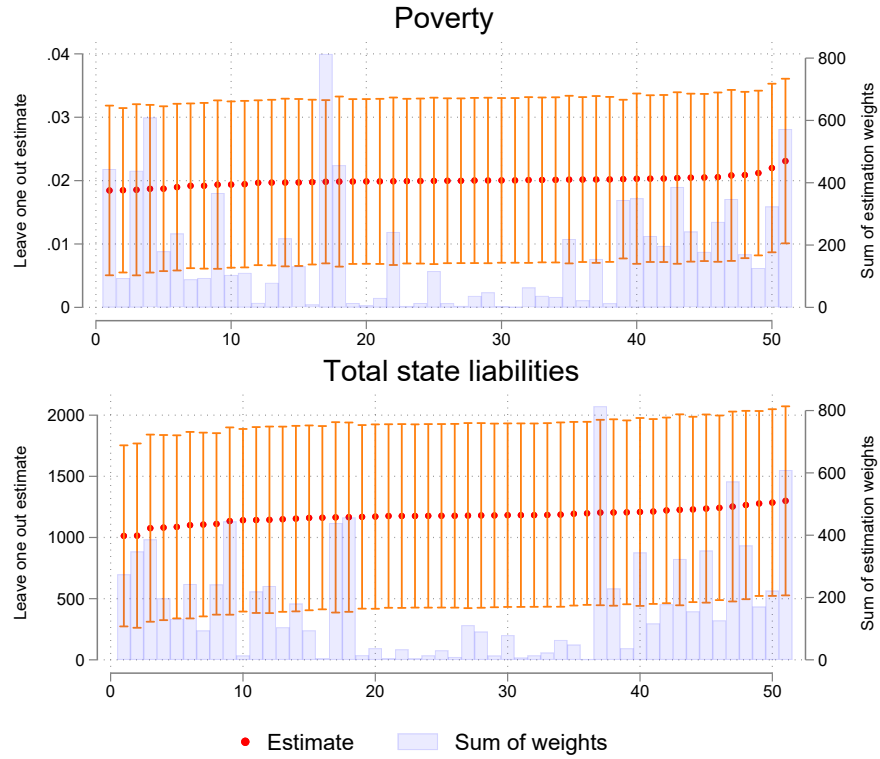
Note: We cluster standard errors at the county-where-treated level. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome with a given state removed from the analysis, and with the shaded region representing 95 percent confidence intervals.

Figure E3: Leave-one-out estimates, by stack.

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**Figure E4:** Average number of Walmart Supercenters in treated and control groups across the event time.
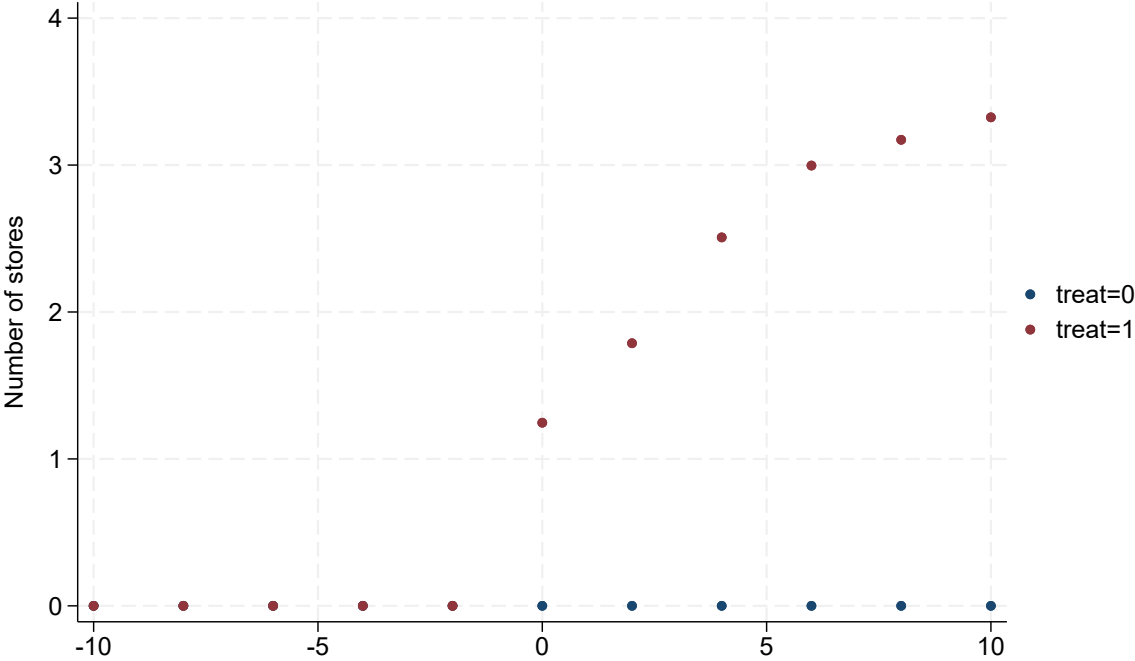
Note: The figure plots the average number of Walmart Supercenter openings per year in each county by year relative to initial treatment and treatment group. Given that some treated counties see additional Supercenter entries in post-treatment years, our subsequent analyses take steps to address this post-treatment imbalance.

Figure E5: Average number of Walmart Supercenters in treated and control groups across the event time, after allowing the control group to include individuals in counties where a Supercenter after $t >= 0$.
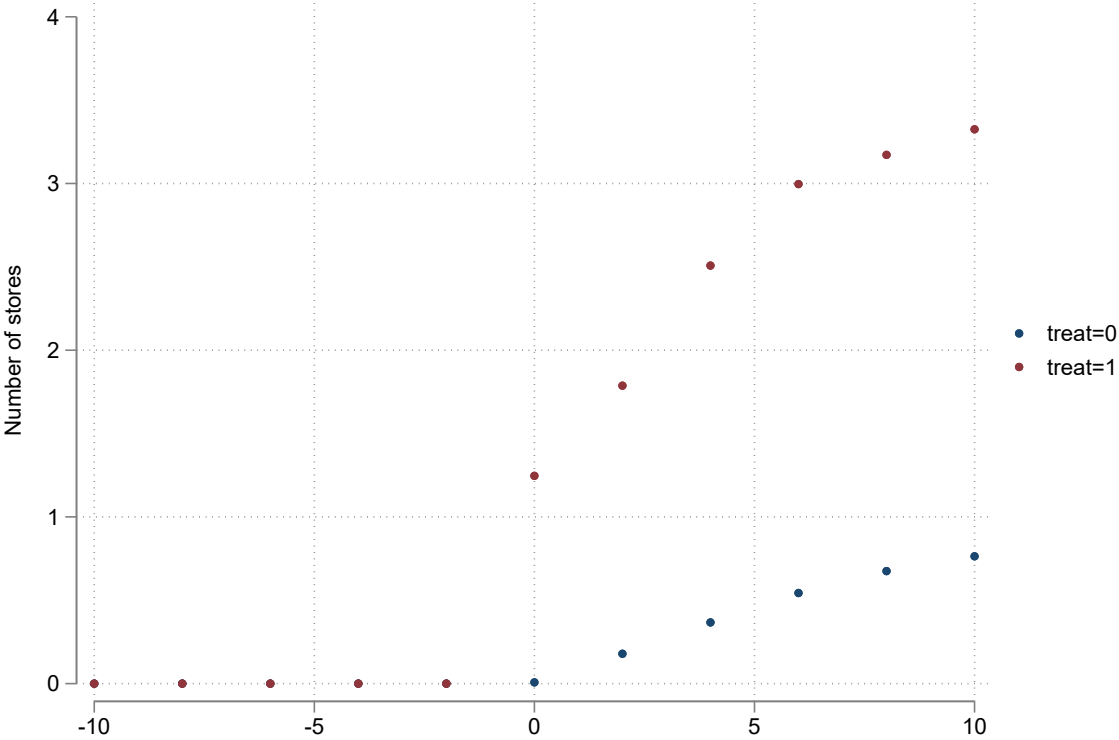
Note: The figure plots the average number of Walmart Supercenter openings per year in each county by year relative to initial treatment and treatment group after relaxing our treatment group criteria. Specifically, we allow the treatment group to include individuals in counties where a Walmart Supercenter opened after the treatment year. The subsequent figure presents event study results after altering our sample to include the expanded control group.

Figure E6: Estimated effects of Walmart Supercenter entry on outcomes with relaxed criteria for control units
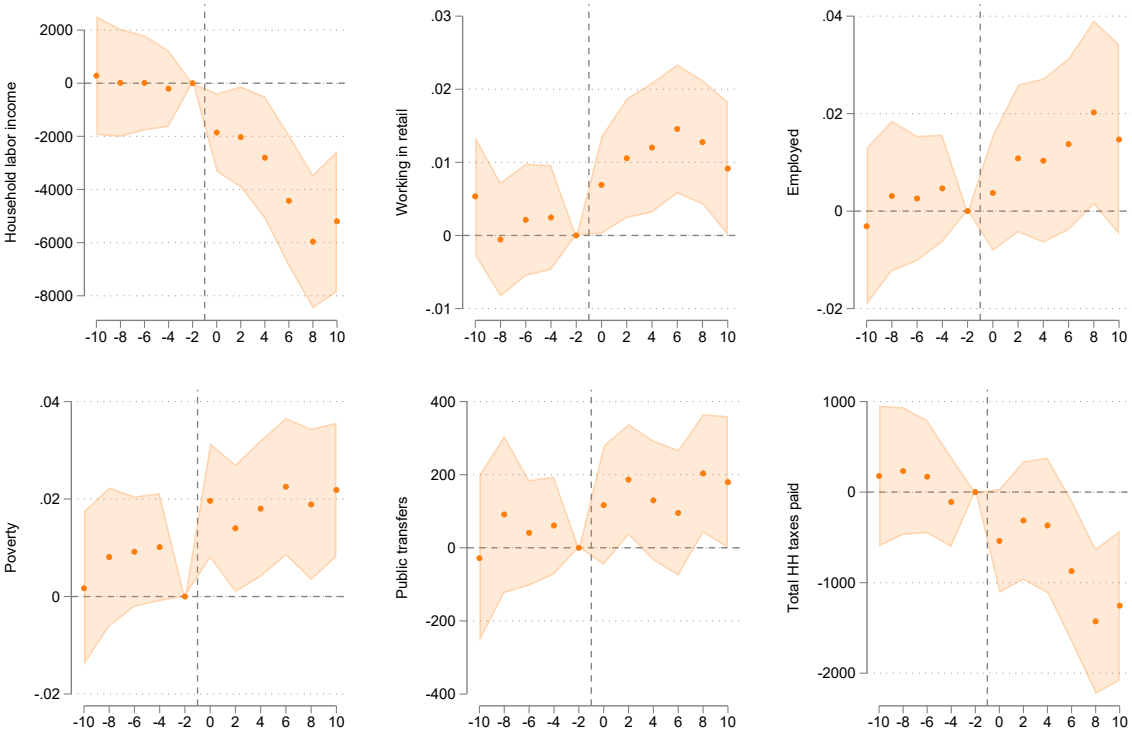
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals. The treatment group is stably defined as adults in households where at least one person worked in retail between treatment years 0 and 10.

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Figure E7: Event study: Effects of Walmart Supercenter entries on outcomes when limiting to pre-2000 entries.
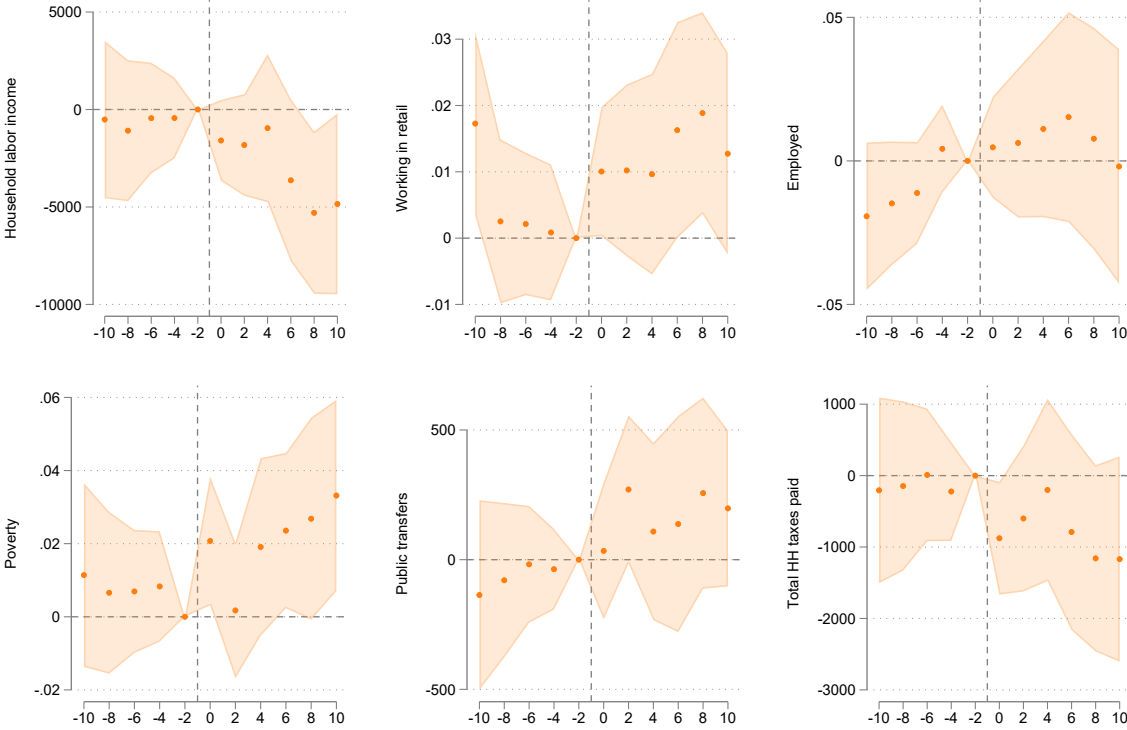
Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.

F   Additional Sensitivity Tests

Figure F1: Coefficients of Walmart Supercenter effects across 1,000 permutations with randomized assignment of treatment
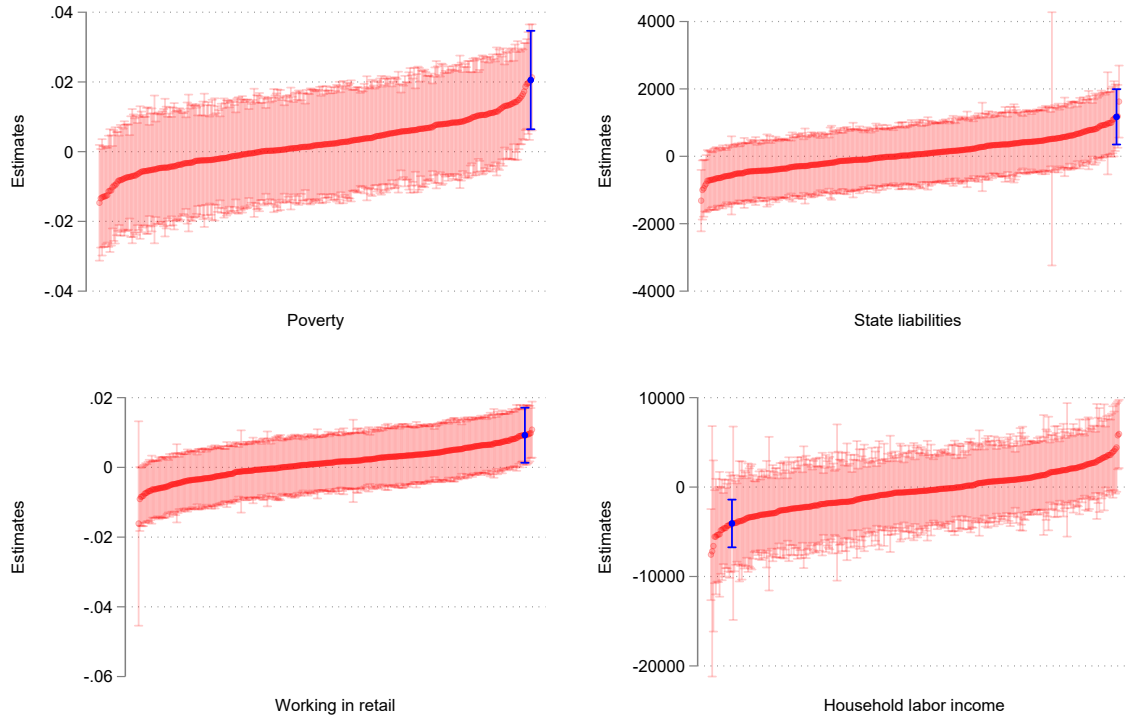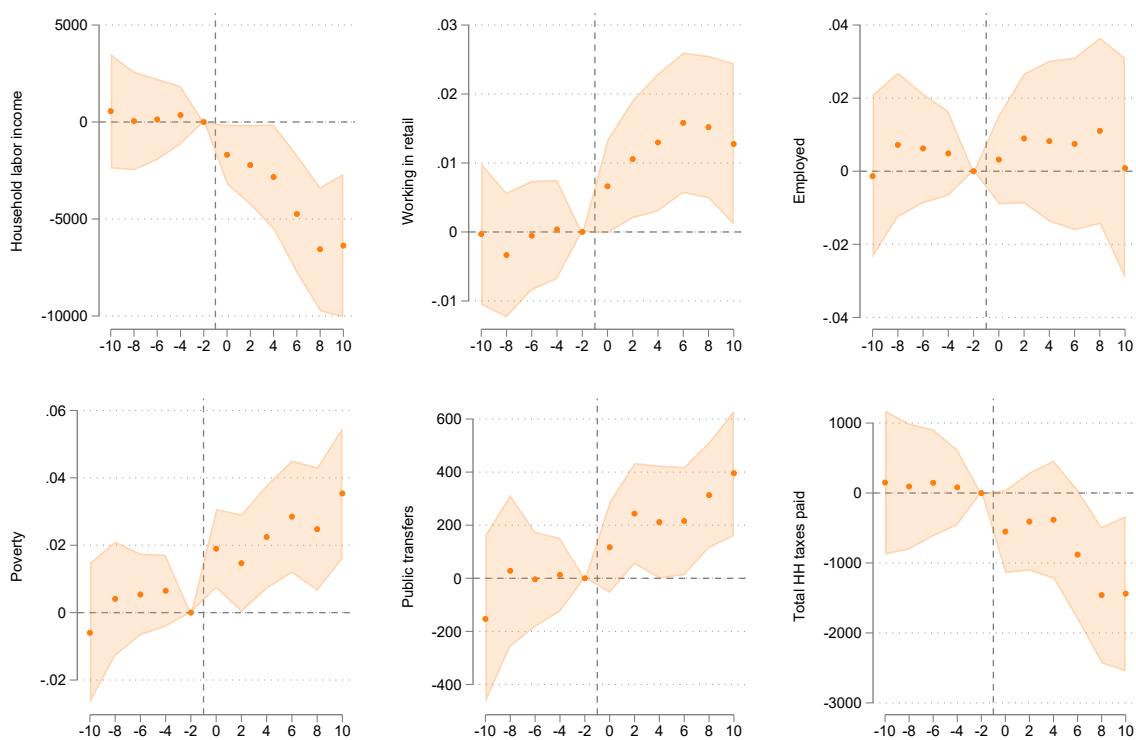

Note: The figure presents results from a Fisher Randomization Tests, or permutation tests, in which we randomly select counties to be treated in each year, while keeping the same number of treated counties in each year to match the observed treatment-by-year distribution. We perform 330 permutations (i.e., we re-randomize county treatment selection and re-run our primary results) and plot the distribution of effects. The treatment effects from our original analysis (blue lines) are larger than 95 percent of the draws from our randomized-treatment simulations.

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Figure F2: Histogram of estimated Walmart Supercenter effects on outcomes across 1,000 permutations with randomized assignment of treatment
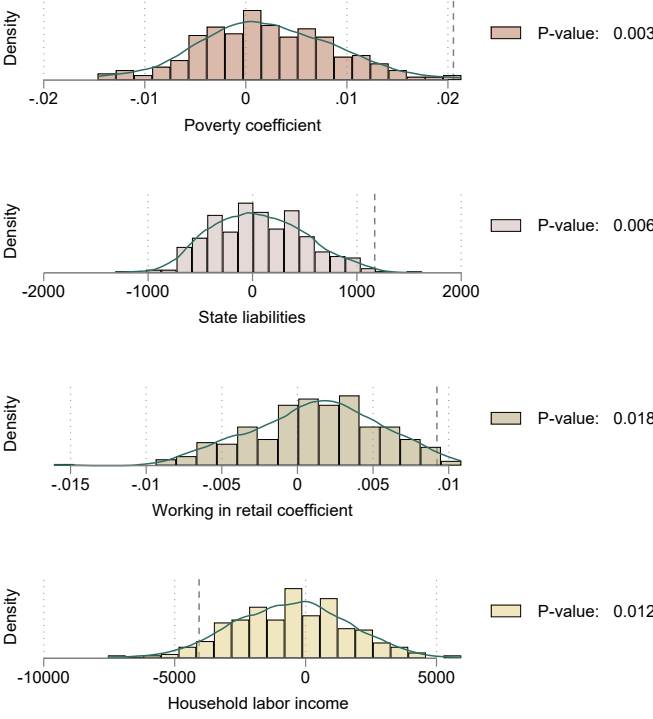
Note: The figure presents results from a Fisher Randomization Tests, or permutation tests, in which we randomly select counties to be treated in each year, while keeping the same number of treated counties in each year to match the observed treatment-by-year distribution. We perform 330 permutations (i.e., we re-randomize county treatment selection and re-run our primary results) and plot the distribution of effects. The treatment effects from our original analysis (blue lines) are consistently larger than 95 percent of the draws from our randomized-treatment permutations.

Figure F3: Event study: Estimated effect of Walmart Supercenter entries on outcomes while controlling for state-year policy outcomes

Note: We cluster standard errors at the county-where-treated level. The X-axis represents years relative to the opening of the first Walmart Supercenter in an individual's county. The plotted coefficients are the estimated effect of a Supercenter opening on the specified outcome, with the shaded region representing 95 percent confidence intervals.