The Effects of Labor Cost Increases on Retail: Evidence from 52 Local Minimum Wage Hikes

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Abstract

How do labor cost increases affect retail businesses, and how do these effects vary across contexts? Using a large, store-level panel of retail businesses from 2019-2022, we estimate the joint distribution of effects from policy-induced labor cost changes—that is, local minimum wage hikes—on retail stores in 52 separate event studies, recovering the full joint distribution of policy impacts on revenue, employment, transaction volume, and median transaction amounts. We recover a median revenue decline of 0.3%, a median employment decline of 1.3%, a median transaction volume decline of 0.5%, and a median average transaction amount increase of 0.04%—but with a wide dispersion of effects across contexts for all four outcome dimensions. We then perform secondary analyses on the estimated joint distribution of effects and find that 1) point estimates suggest that every 1% increase in labor costs is associated with a 0.2% decline in quantity demand, and 2) the association between employment changes and demand is highly significant and but approximately 4x-5x weaker than the association between price changes and demand, suggesting that consumers are far more sensitive to price changes, on a percentage basis, than to changes in the service environment.

Keywords: retail, minimum wage, staffing, pricing

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1 Introduction

One of the perennial challenges in the life of a retail store manager is dealing with rising labor costs (Nordhaus 2006). Especially in tight labor markets, such in the immediate aftermath of the COVID-19 pandemic, increases in prevailing wages are unavoidable and present managers with hard trade-offs: either one has to raise prices, reduce staffing levels, accept lower profits, or some combination of the above (BLS 2023). This difficulty is further compounded by the fact that optimal adjustment requires insight into hard-to-measure elasticities of retail store demand to price changes and to staffing level adjustments.

At the same time, the landscape of local labor policy in the United States has changed dramatically in recent years. In response to the stagnant federal minimum wage, dozens of cities, counties, and states have independently taken it upon themselves to enact increases to their local minimum wages. This proliferation of policy variation creates an exceptional opportunity to move beyond narrow, case-based studies and generate broader, generalizable evidence on how retail firms adjust to rising labor costs.

In this paper, we exploit this opportunity and estimate the revenue, transaction and employment impacts from 52 city- and county-level minimum wage increases, and then furthermore investigate how these impacts are associated across effect dimensions. This large-scale analysis offers first-of-its-kind evidence not only on the distribution of impacts from labor cost increases across a newly rich set of outcome dimensions, but also affords new insight into how variance in effects along one dimension, such as employment, relates to variance in effects across other dimensions, such as store revenue, revealing systematic relationships that would be otherwise impossible to learn from one-off policy evaluations.

First, we use a panel dataset of store-level revenue and transactions for hundreds of thousands of retail stores across the US, along with data on 52 sub-state-level minimum wage hikes, to estimate the demand effects of 52 separate event studies between the years of 2019 and 2022.[1] We use the synthetic differences-in-differences approach of Arkhangelsky et al. (2021) to estimate event-study effects of each increase in a consistent manner across all of these contexts and to allow for weaker assumptions on parallel trends, although we also present results using simple differences-in-differences estimation.[2] With this approach, we estimate a median revenue decline of 0.3%, but with a wide dispersion: across the distribution of 52 effect estimates, we find a 25th percentile effect of a 2.5% decline in revenue, but a 75th percentile effect of a 1.7% increase in revenue. Similarly, we estimate a median transaction volume decline of 0.5%, but with a 25th percentile effect estimate of a 3.1% decline and a 75th percentile effect estimate of a 2.6% increase. Finally, we recover a median average transaction

[1]While one may be concerned by the overlap between this period and the COVID-19 pandemic, the distribution of results is similar when restricting to only pre-COVID-19 wage increases, presented in Appendix B.

[2]Both synthetic differences-in-differences and simple differences-in-differences recover similar effect distributions, and furthermore feature qualitatively identical second-stage relationships between estimated effect dimensions. Full simple differences-in-differences results are presented in Appendix C.

amount increase of 0.04%, with a 25th percentile effect estimate of a 0.5% decrease and a 75th percentile effect estimate of a 0.8% increase.[3]

Second, we leverage a proxy measure for employment, based on observations of "long duration visits" from cell phone trace data, in order to estimate the corresponding impact of these same 52 minimum wage increases on staffing levels. Specifically, we follow Pandit (2023) and use the number of four-hour-or-longer visits, tracked at the establishment level using GPS trace data, as a coarse proxy for the level of employment at a given store. We find a median employment decrease of 1.3% following a minimum wage hike, but with a 25th percentile effect estimate of a 3.1% decline and a 75th percentile effect estimate of a 2.4% increase. This wide dispersion of employment effects mirrors the broad range of estimates previously recorded in the large empirical minimum wage literature (Card and Krueger 1995, Lemos 2008, Neumark 2018), although we do find a high density of significant negative effects in our set of sub-state (city- and county-level) event studies, in contrast to earlier work that found generally null employment effects when examining state-level minimum wage changes at scale (Cengiz et al. 2019), possibly because of systematic differences between state-level and local-level minimum wage change effects. Further, as our

[3]We interpret this last effect dimension, average transaction amount, as a coarse proxy measure for the degree of price adjustment following the labor cost increase: although the relationship between average transaction amount changes and price changes will be attenuated to the extent that price increases lead to decreases in purchase basket size, standard economic theory predicts that larger average price changes will occur in areas with lower average price elasticity of demand, and so we posit that variation across event studies in average transaction amount effects (i.e., larger price increases and smaller proportionate basket shrinkage, or smaller price increases and larger proportionate basket shrinkage) will still approximately correspond to variation in the degree of businesses' price adjustment across contexts.

employment metric is based on a visit-based proxy, measurement noise in our data may be expected to lead to attenuation bias in these estimates, suggesting that this estimated distribution may best be interpreted as a conservative lower-bound on the true employment effect distribution.

Finally, with this joint distribution of estimated minimum wage impacts in hand, we perform secondary analyses to examine the existence (or not) of systematic relationships between each of these estimated effects and the size of the minimum wage increase, and with each other. First, we find that, while the size of the minimum wage increase shows only insignificant associations with demand impacts, point estimates suggest that every 1% increase in labor costs is associated with a 0.2% decline in employment, closely aligned with prior empirical literature (Dube and Zipperer 2024). Further, every 1% increase in labor costs is associated with a 0.2% decline in quantity demand, a relationship that has previously only been examined in single-setting empirical studies (Harasztosi and Lindner 2019). We also uncover highly significant relationships between price adjustment and employment adjustment (respectively) and business outcomes: every percentage point increase in employment is associated with approximately 0.5 percentage point reductions in revenue and transaction quantity losses, while every percentage point increase in median transaction amount (our proxy for price adjustment) is associated with approximately 2 percentage point reductions in revenue and transaction quantity losses. This estimated relationship between employment levels and retail revenue is also, to the best of our knowledge, novel to the empirical literature, and suggests that the extent of price adjustment a key determinant of how businesses weather labor cost increases.

Taken together, our results offer new insight into the consequences of labor cost increases—particularly minimum wage hikes—on retail store outcomes across a wide range of dimensions. By estimating the joint distribution of effects on revenue, transactions, and staffing, we show that firm demand is most meaningfully associated with price increases that result from minimum wage hikes, while adjustments in overall employment levels are associated with much more modest effects. For businesses, this analysis provides crucial insight into the relative elasticities of demand to employment and to pricing shifts. Our results also suggest that price adjustments are often particularly harmful to business revenue, suggesting that investments in marketing and brand-building exercise that afford more pricing power may be instrumental for helping businesses to weather labor cost hikes. For policymakers, our results highlight a core tradeoff for policies that try to mitigate potential disemployment effects of minimum wage increases by making it harder to fire workers, as contract structures or other policies that introduce rigidity into the labor market may significantly increase the negative impact that businesses endure from labor cost hikes.

The rest of this article is structured as follows. Section 2 describes the relationship of the present work to prior literature. Section 3 details the data. Section 4 describes the empirical specification. Section 5 presents the large-scale reduced-form evidence of the effects of 52 local minimum wage increases on observed retail store transaction quantities and on proxy measures for price and employment. Section 6 presents secondary analyses on the estimated joint distribution of effects across event studies. Section 7 concludes.

2 Relation to Prior Literature

This paper contributes to the large and contested literature on the effects of the minimum wage, particularly on employment, revenue, and pricing outcomes. Seminal studies such as Card and Krueger (1993) and Neumark and Wascher (2000) have produced mixed evidence on disemployment effects, while more recent work (e.g., Giuliano (2013), Dube et al. (2010)) highlights important heterogeneity in firm and worker responses. Surveys by Neumark and Wascher (2008) and Card and Krueger (1995) underscore the prevalence of near-zero average employment effects across the literature. Among the most directly comparable studies to ours are Cengiz et al. (2019), who use a difference-in-differences approach on 138 state-level minimum wage increases from 1979 to 2016 to estimate employment effects across the wage distribution, and find essentially no net job loss in the low-wage sector on average, as well as Pandit (2023), who introduces the 4+ hour visit measure as a proxy for store-level retail employment and report a similar range of disemployment estimates across many event studies. Beyond employment, our study relates to Harasztosi and Lindner (2019), who examine an unusually large and persistent wage increase in Europe and demonstrate that even substantial policy changes yield only small net effects on employment, as well as Brummund (2017), who analyze confidential data from a US retail store chain and find evidence of declining revenues after minimum wage increases, and Mayneris et al. (2018), who perform large-scale analysis of firm response to a single minimum wage increase in China; for a full literature review of this price pass-through literature, see Lemos (2008) and Neumark (2018). Our study builds on and extends this work by examining a much wider range of business- and policy-relevant outcome dimensions and investigating how they relate to one another, with a focus on recovering insights for businesses, policymakers and researchers hoping to understand how to navigate minimum wage or other labor cost increases. This large-scale, multi-dimensional estimation, and secondary analysis of how results relate across dimensions, is novel to the empirical minimum wage literature and reveals how firm strategy and local conditions shape the extent to which minimum wage increases result in business losses or employment cuts.

This paper also contributes to the literature on store pricing and revenue responses to labor cost shocks. Prior work (e.g., Fougère et al. (2010) and Aaronson et al. (2008)) documents that restaurants often pass increased labor costs onto consumers through higher prices. Complementary evidence from Ashenfelter and Jurajda (2022)—which focuses on standardized menu items—suggests that price adjustments are an important channel of response. By decomposing overall revenue effects into distinct impacts on proxies for price and employment, our study advances the literature by providing a more comprehensive account of how labor cost shocks are absorbed in the retail sector, and how business adjustment translates to demand changes.

Finally, this work relates to the business and marketing literature on the elasticity of retail demand to price and to employment levels. This includes Andreyeva et al. (2010) who investigate the effect of price increases on retail demand, and Sulek and Hensley (2004), who use a survey-based approach to assess the importance of service on declared intentions to return to a restaurant a second time, as well as Anderson and Magruder (2012) and Luca (2011), who examine the relationship between perceived service quality and demand. While price and service-quality elasticities of retail demand have been previously studied, our holistic analysis of the relationship between price adjustment, employment adjustment, and broader revenue and demand changes are novel to the empirical literature. Moreover, we provide a clear estimate of the sensitivity of demand to shifts in employment levels using a large sample of quasi-experiments of policy-induced changes in labor costs, offering insights for both researchers and managers into the elasticity of demand to employment changes.

3 Data

Our first main source of data is SafeGraph Spend. SafeGraph Spend is a large-scale panel of transactions and revenue for hundreds of thousands of physical stores. These data are collected from actual private credit- and debit-card transactions from many financial institutions across the United States in partnership with one of the largest transaction data aggregators in the US, described as an anonymized partner whose data services are used by many of the world's top financial institutions; SafeGraph reports that their panel is based on around 11 million monthly active debit and credit cards for around 9 million active consumers (SafeGraph 2024). Transactions are then assigned to specific store locations in a SafeGraph location dataset to create a panel of business revenue for the United States, covering hundreds of thousands of business locations from 2019 to 2022 and including both in-person purchases and online purchases routed through specific stores.[4] These transactions data are then aggregated to the store-by-date level. These data also report median transaction amount. Figure 1 visualizes the wide coverage of the retail and retail store locations across the United States in SafeGraph data. For the purposes of this study, we focus only on retail stores, as defined by the NAICS code.[5] SafeGraph provides documentation that its Spend data comes from a representative sample of the US population across states (SafeGraph 2024), but we note here that the SafeGraph sample is predominantly based on debit card transactions, and as debit card use is more common among lower-income populations, this suggests that revenue from customers may tend to be lower in the SafeGraph Spend panel than would be found in a pure random sample of state populations.[6]

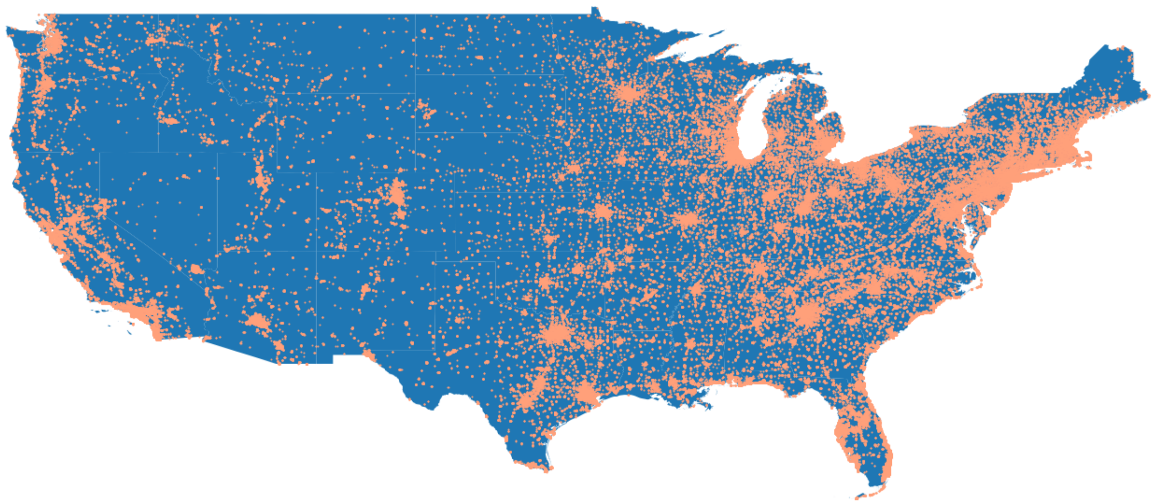
Our second main data source is SafeGraph Patterns. These are a panel of cell

[4]These transaction and revenue numbers are not modeled or based on projections; we use the "raw" spending and transaction statistics from SafeGraph.

[5]Specifically, we consider NAICS codes starting in 44, 45 and 72 as 'retail'; these categories include both stores that sell goods (44 and 45) and restaurants (72).

[6]We note however that the data do not include cash transactions, which may exclude other low-income individuals.

Figure 1: SafeGraph Spend Retail Store Locations



Notes: Distribution of physical retail store locations across the continental US. Data from SafeGraph.

phone foot traffic data for a superset of stores in SafeGraph Spend, covering over 1 million stores nationally. Built with a separate data-generating process as the revenue panel, SafeGraph Patterns is built based on collecting and combining GPS location "ping" (or "trace") data from consumers who have opted-in to sharing their location to mobile applications, and then clustering and processing these pings into monthly visit statistics for a panel of millions of locations. (The cross-section of locations is shared across Spend and Patterns, although only a subset of locations in Patterns are present in Spend.) As with the spend panel, we retain only retail stores in the panel, determined based on NAICS codes; these include both storefront retail and restaurants. These data are necessarily drawn from the population of mobile phone users in the U.S., which is widespread but may be non-representative to the extent that certain groups are more likely to have phones and more likely to share GPS trace data with mobile phone apps. We consider only retail stores and, following Pandit (2023), we focus on the number of cell phone visits that are 4 hours or more as a proxy for the employment level at each establishment. While there is certainly noise in this measurement in terms of how closely it represents total employment, we suggest that changes in this measure over time, after controlling for two-way fixed effects, is a strong correlate of actual changes in total employment at each given store.

We then join these panels with a hand-collected dataset of minimum wage increases from 2019 to 2022. Gathered from online state policy announcements and other internet news sources by research assistants, these data consist of over one hundred minimum wage increases across the US. We restrict our attention to only local minimum wage increases, defined as city- and county-level minimum wage increases, so that we may construct weighted comparison groups using stores that belong to immediately surrounding counties, generally in the same state, that do not experience contemporaneous minimum wage increases. We further restrict consideration to only minimum wage increases that occur after January 2019 and prior to July 2022, so that we have enough data to estimate both a pre- and a post-period, and exclude minimum wage changes that were within 3 months of either the March 2020 COVID-19 lockdown period or the January 2021 COVID-19 spike and associated lockdowns.[7] After these restrictions, we end up with a final minimum wage dataset of 52 separate

[7]The distribution of effects is similar if we additionally restrict consideration only to 2019 minimum wage hikes, a setting that is completely insulated from any COVID-19-related bias, presented in Appendix B.

city- and county-level increases.[8]

Table 1: Summary Statistics: Spend Panel

Average Statistics Across 52 Event-Study Datasets

	N Control Retail Stores	N Treated Retail Stores	Average Monthly Transaction Count	SD Monthly Transaction Count	Average Median Transaction Amount
Mean	33681.3	2072.8	1139.8	3921.2	253.3
SD	(25993.7)	(4170.5)	(640.4)	(1412.8)	(26.6)

	SD Median Transaction Amount	Average Monthly Revenue	SD Monthly Revenue
Mean	483.1	48905.7	290934.9
SD	(219.4)	(22853.5)	(113880.0)

Statistics for Los Angeles County, July 2019 Event Study

N Control Retail Stores	N Treated Retail Stores	Average Monthly Transaction Count	SD Monthly Transaction Count	Average Median Transaction Amount
61347	20294	1136.2	4437.8	257.3

SD Median Transaction Amount	Average Monthly Revenue	SD Monthly Revenue
467.3	49056.4	271412.6

Notes: The top panel provides average summary statistics for the 52 matched minimum wage event-study datasets, with each dataset including up to 6 months of pre-increase data and 6 months of post-increase data for every retail store in the respective sample. The bottom panel provides summary statistics for the matched minimum wage event study dataset for the Los Angeles July 2019 minimum wage increase. 'Average" denotes mean across stores, SD" denotes standard deviation across stores, so e.g. Average Mean Transaction Amount" denotes the mean (across stores) of the store-level mean (across transactions) transaction amount. Treated" retail stores are defined as stores within the given locality that experienced a minimum wage increase. Control" retail Stores are defined as stores outside of the given locality but within the same state and that did not experience a contemporaneous minimum wage increase. Standard deviations (measured across event study datasets) in parentheses. Data from SafeGraph Spend.

Finally, we merge each of these minimum wage changes to the revenue panels to

[8]Note that some of these minimum wage increases are later or earlier minimum wage increases in the same locality, if multiple minimum wage increases were implemented in a given locality this period. However, no localities increased minimum wage floors multiple times in a single 12-month period; therefore, to both cleanly separate observations between different minimum wage increases and retain consistency across settings, in our estimation we focus on a relatively short window of 6 months prior and 6 months after the minimum wage increase. Further details are provided in the subsequent section.

Table 2: Summary Statistics: Patterns Panel

Average Statistics Across 52 Event-Study Datasets

	N Control Retail Stores	N Treated Retail Stores	Average 4-Hour+ Visit Count	SD 4-Hour+ Visit Count
Mean	111077.9	9579.4	159.3	1148.5
SD	(109873.5)	(23894.6)	(37.7)	(502.1)

Statistics for Los Angeles County, July 2019 Event Study

N Control Retail Stores	N Treated Retail Stores	Average 4-Hour+ Visit Count	SD 4-Hour+ Visit Count
89149	123667	211.2	1304.2

Notes: The top panel provides average summary statistics for the 52 matched minimum wage event-study datasets, with each dataset including up to 6 months of pre-increase data and 6 months of post-increase data for every retail store in the respective sample. The bottom panel provides summary statistics for the matched minimum wage event study dataset for the Los Angeles July 2019 minimum wage increase. "Average" denotes mean, "SD" denotes standard deviation. "Treated" retail stores are defined as retail stores within the given locality that experienced a minimum wage increase. "Control" retail stores are defined as retail stores outside of the given locality but within the same state and that did not experience a contemporaneous minimum wage increase. Standard deviations (measured across event study datasets) in parentheses. Data from SafeGraph Patterns.

create 52 discrete event-study datasets, one for each minimum wage increase. Summary statistics for these data are presented in Tables 1 and 2. In the top panel, we present the average summary statistics across all 52 matched event-study datasets, along with standard deviations of the summary statistics across event studies. In the bottom panel, we present the summary statistics for an example case-study of the particular minimum wage increase in Los Angeles County in July 2019. In each of these event-study datasets, we define "treated" retail stores defined as those within the given locality that experienced a minimum wage increase and "control" retail stores as retail stores in immediately surrounding counties of the given locality that did not experience a contemporaneous minimum wage increase.

4 Econometric Specification

To consistently estimate the reduced-form effect of these minimum wage increases on retail store revenue across these 52 localities, we rely on the synthetic differences-in-differences methodology of Arkhangelsky et al. (2021). This method optimally reweights control observations to match the averages of treatment observations in the pre-period and then estimates the effect of treatment by comparing the treated group to this reweighted control, allowing for consistent estimation of treatment effects even in the presence of differential pre-trends; we rely on this approach to permit the weakest possible assumptions on the structure of the data, given that selecting for settings with null pre-trends has been shown to introduce problematic biases in inference (Roth 2022).

To estimate synthetic differences-in-differences effects, we estimate weights $\{\hat{\omega}_i\}$ to align the pre-exposure trends in retail store spending between the treatment and control groups, as well as weights $\{\hat{\lambda}_t\}$ to balance pre-exposure and post-exposure periods; then we estimate

$$\left(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = argmin_{\tau,\mu,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i \hat{\lambda}_t \right\} \tag{1}$$

where Y_{it} is the outcome (e.g. log retail store revenue), μ is a constant term, α_i is the unit fixed effect, β_t is the time fixed effect, and $W_{it} \in \{0, 1\}$ is a binary variable that is 1 for treated and 0 otherwise, and $\hat{\tau}$ measures the average effect of treatment exposure. In both primary and robustness specifications, we restrict the estimation sample period to include an equal number of post-treatment months (6) for each locality to ensure consistency in treatment window duration without any overlap, given that some localities introduce minimum wage increases 12 months apart.[9] [10]

With this approach, we are able to rigorously estimate effects from all 52 event studies, without modifying the procedure at all across settings. This allows us to infer estimates across a large variety of settings without any researcher degrees-of-freedom differing between studies, and enables us to present a broad distribution of estimates across many business-relevant dimensions, building on earlier work that performed similar exercises but only for employment-related outcomes (Cengiz et al. 2019, Pandit 2023).[11]

For robustness, given that synthetic differences-in-differences is a relatively new methodology in the research literature, we also present complete results for all steps

[9]We generally use 6 months pre-treatment as the pre-period, except in cases where the 6 months prior overlap with a previous event-study window, as is the case for consecutive increases 12 months apart; in those cases, we use the preceding 5 months as the pre-period. We also constrain the pre-period if it overlaps with a COVID-19 lockdown period, defined as March 2020 or January 2021.

[10]We remark that this short time window may lead our impact estimates to be conservative lower-bounds to the extent that minimum wage effects may take time to manifest.

[11]That said, we are still restricted to investigating US cities and counties that introduced minimum wage increase policies, which tend be those with liberal-leaning area populations. We present these results as a step forward in generalizability of minimum wage effect findings, but caution that there may still be reasonable concerns about the representativeness of these estimates for settings that are very different from those that raise the minimum wage in these years, that are outside of the scope of this article to address.

of our analyses using simple differences-in-differences, presented in Appendix C. Distributions of effects are highly similar, and furthermore, estimated secondary relationships between effect dimensions are qualitatively identical and remain highly significant. In this specification, we implement differences-in-differences estimation as a simple two-way fixed effects regression with unit and time fixed effects:

$$Y_{it} = \mu + \alpha_i + \beta_t + \tau W_{it} + \varepsilon_{it} \tag{2}$$

where all variables are defined as above, and ε_{it} captures idiosyncratic shocks to the store-level outcome. This specification assumes parallel trends in the absence of treatment, a stronger assumption than that required by the synthetic differences-in-differences approach. However, the similarity of results across the two methods provides reassurance that our findings are not driven by model selection.

Finally, we remark that for employment outcomes, we use a proxy measure (number of 4+ hour visits) for employment that has a high proportion of zero-valued observations. In order to minimize the number of observations that are excluded when taking a logarithmic transformation of this variable, we estimate effects for this outcome by combining observations across periods into a simple two-period difference-in-differences estimator, summing the number of visits in the months before and in the months afterwards and then taking the logarithm of the sum. Since synthetic differences-in-differences is not applicable to two-period differences-in-differences, we use the standard differences-in-differences estimator for all analyses of employment effects. As noted above, since synthetic differences-in-differences and simple differences-in-differences produces similar estimate distributions, with qualitatively identical secondary relationships across effect distributions, we expect that this small methodological deviation for the estimation of employment effects would not substantively affect our findings.

5 Reduced-Form Estimates from 52 Event Studies

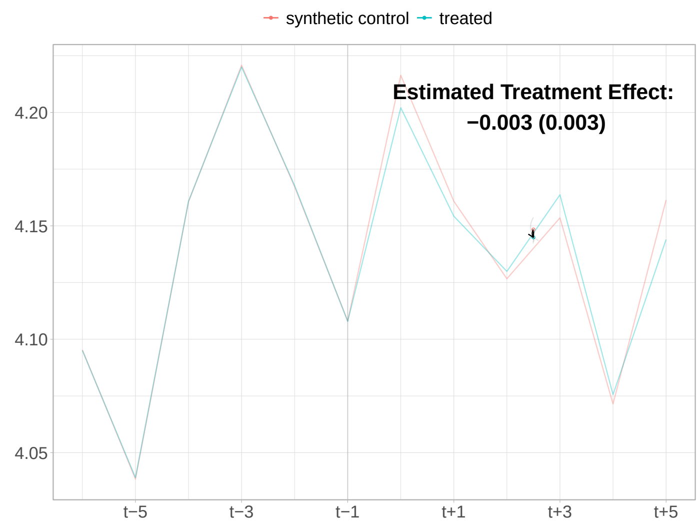
5.1 Transaction Quantity and Revenue Effects

We begin by investigating the effects of minimum wage increases on store revenue and transaction quantity. These outcomes quantify the overall loss to businesses arising from rising labor costs. Moreover, for policymakers and researchers, declines in realized transactions can serve as a lower-bound proxy for deadweight loss associated with the policy under standard efficiency loss interpretations.

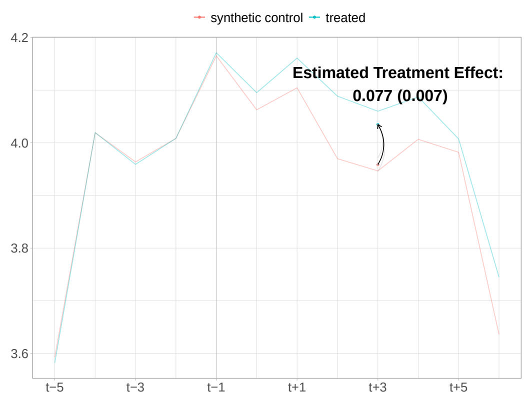
First, we present detailed synthetic differences-in-differences figures for three transaction quantity event studies in Figure 2, showing the pre-period and post-period trends for both treated groups and the optimally reweighted control for each example. In panel (a) we present the trends for our primary case study, the minimum wage increase in Los Angeles County in July 2019, while in panel (b) we present the trends for the minimum wage increase in Chicago in July 2021, and in panel (c) the trends for Mountain View's minimum wage increase in January 2022. In each

Figure 2: Effects of Minimum Wage Increases on Log Store Transaction Quantity

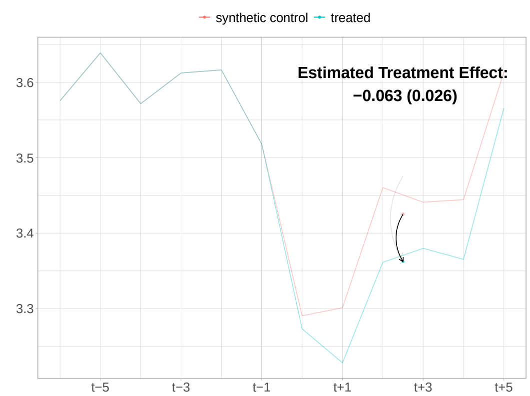
(a) Los Angeles County, July 2019



(b) Chicago, July 2021



(c) Mountain View, January 2022



Notes: Effect of minimum wage increases on retail store transaction quantity based on synthetic differences-in-differences estimator for three example settings. X-axis shows relative time in months, y-axis shows log transactions. In each figure, red line describes the log transaction path of synthetic control retail stores (based on the sample of retail stores in the surrounding counties without a contemporaneous minimum wage increase) and the blue line describes the log transaction path of retail stores that belonged to the described locality that raised minimum wage at time t. Panel (a) describes effects for Los Angeles County in July 2019, panel (b) describes effects for Chicago in July 2021, panel (c) describes effects for Mountain View in January 2022. Precise numerical estimates presented in Tables A1 and A2. Transaction data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

case, the synthetic differences-in-differences procedure achieves a close fit between the treatment group and the reweighted control observations in the pre-period, even as transaction quantity fluctuates considerably with seasonality and other shifters. What occurs afterwards, however, varies considerably across settings: in the first and third highlighted cases, Los Angeles County in July 2019, demand only declined by an insignificant 0.03%, while in Mountain View in January 2022, demand declined by a highly significant 6.3%; but in the third highlighted case, Chicago in July 2021, transaction quantity increases 7.7% after the minimum wage increase.

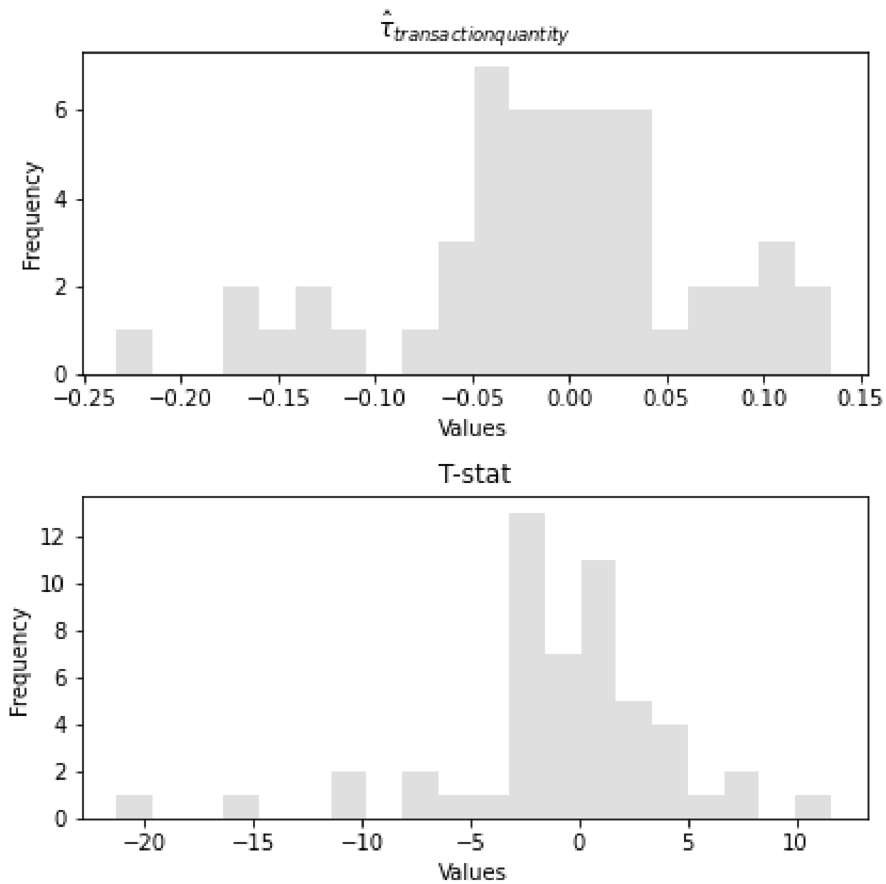
This variation is broadly representative of the wide spread of estimates across the full set of contexts studied. The full distribution of transaction quantity and revenue effects across all 52 local minimum wage event-studies is presented in Figures 3 and 4, respectively; the complete list of estimates and standard errors for every study is detailed in Appendix Tables A1 and A2. We recover a mean decline of 1.1% and a median decline of 0.5% in transaction quantity, but with a wide dispersion.[12] Similarly, we recover a mean revenue decline of 1.5% and a median revenue decline of 0.3%, but also with a wide distribution.[13]

This high variance of our estimated effects, while perhaps surprising, is in line with an earlier minimum wage literature that often finds a wide disparity of effects across different implementations and contexts Card and Krueger (1993), Lemos (2008), Neu-

[12]Excluding event-studies that coincide with COVID-19 leads to a highly similar distribution, with a mean transaction quantity decline of 1.6% and a median decline of 1.5%. See Appendix B for details.

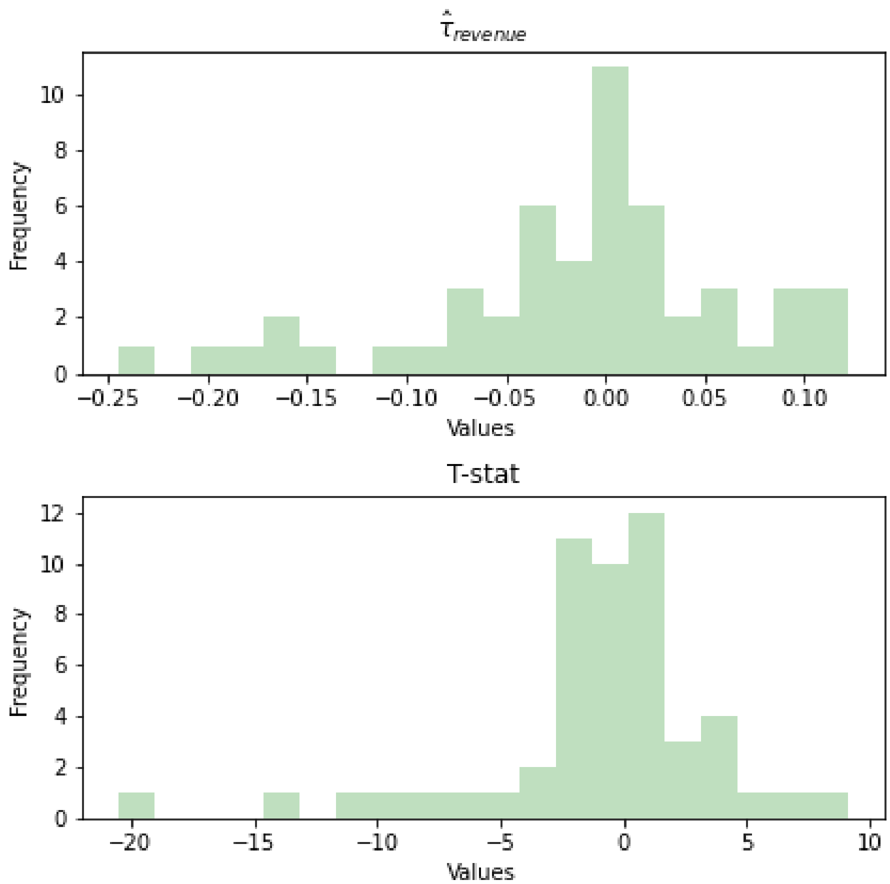
[13]Excluding event-studies that coincide with COVID-19 leads to a highly similar distribution, with a mean revenue decline of 1.3% and a median decline of 0.5%. See Appendix B for details.

Figure 3: Histograms of Transaction Quantity Effects



Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on synthetic differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Transactions data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

Figure 4: Histograms of Revenue Effects



Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on synthetic differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Revenue data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

mark (2018). The present study offers strong empirical support for a truly wide variation in effects across contexts, suggesting that this observed range is not driven by different researcher choices with the data but arises even when the exact same estimation protocol is applied to a large number of contexts. This wide range also helps reconcile our negative average estimate with both the positive and negative point estimates found in earlier work between minimum wage and business revenue (Brummund 2017, Harasztosi and Lindner 2019): our distribution suggests that positive revenue effects are not abnormal, presumably arising in contexts where minimum wage changes lead to price increases and with less-than-proportional demand declines; but these effects are nonetheless atypical, and policymakers and businesses are well-advised to expect that minimum wage increases will lead to revenue declines in the median case. The present work provides key context for interpreting such single-case estimates in light of the full distribution of impacts.

5.2 Median Transaction Amount Effects

Next, we examine the impact of labor cost increases (minimum wage policy changes) on median transaction amount, as reported in the SafeGraph Spend panel at the store-by-month level. Unlike revenue and transaction counts, which reflect the overall business impact of labor cost increases, changes in median transaction amount reflect a combination of retailers' price adjustments and consumers' basket size responses.

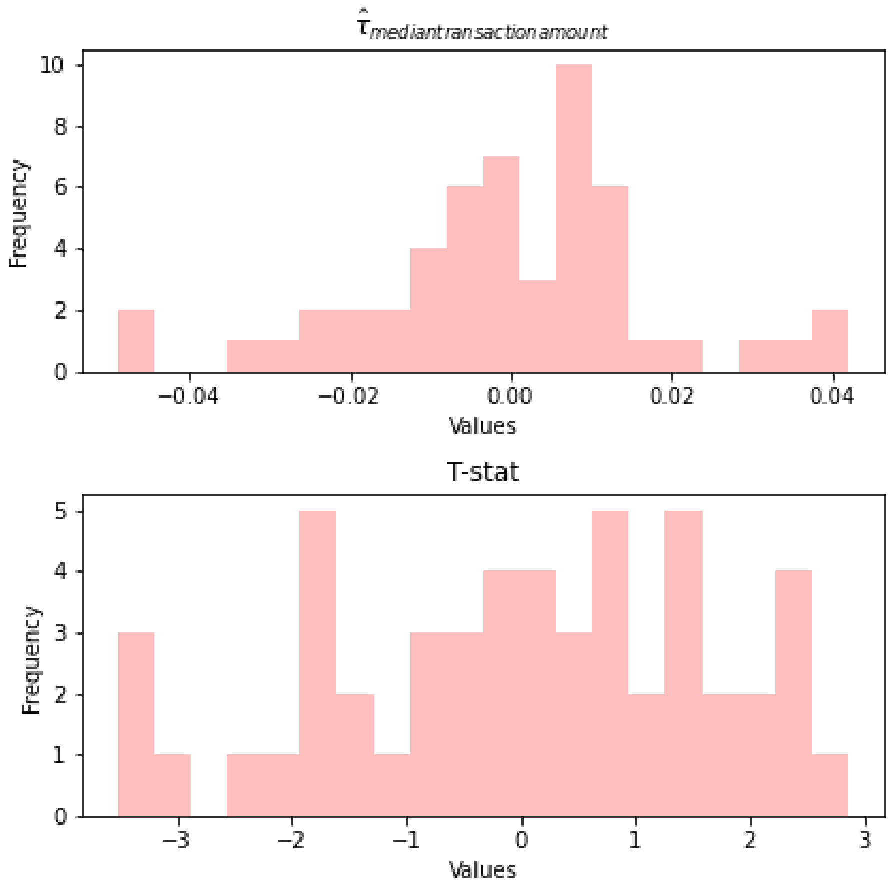
According to standard economic theory, we expect heterogeneity in these responses across localities to approximately vary according to the local price elasticity of demand (among other factors). That is to say, in markets with relatively inelastic demand, retailers are more likely to pass through labor cost increases as higher prices, with relatively modest declines in quantity purchased per transaction. Conversely, in areas with more price-elastic consumers[14], retailers may face stronger consumer pushback, resulting in smaller price increases and larger proportional reductions in basket size. Therefore, we here interpret changes in median transaction amount as a proxy for the extent of price adjustment: larger increases suggest greater price pass-through with limited basket contraction, while smaller or negative changes indicate more limited pricing power and more pronounced reductions in consumer quantity demanded.

Results are presented in Figure 5. We estimate a mean median transaction amount increase of –0.02% and a median increase of 0.04%.[15] Visually, we find a tighter dispersion of effect estimates as compared to our first analysis of revenue and transaction quantity effects, with effects more tightly centered and balanced around zero; t-statistics for average transaction amount range from –3 to 3, as compared to a much wider range for transaction quantity and revenue, and similar numbers of estimates fall above and below zero, as compared to predominantly negative effects on revenue and transaction quantity.

[14]This may arise for example due to differences in consumers across areas, or alternatively due to differences in the composition of stores across areas, with some localities featuring retail that is more concentrated in price-elastic categories.

[15]Excluding event-studies that coincide with COVID-19 leads to a slightly more positive distribution, with a mean increase of 0.60% and a median increase of 0.82%. See Appendix B for details.

Figure 5: Histograms of Median Transaction Amount Effects



Notes: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store median transaction amount based on synthetic differences-in-differences estimator. Precise numerical estimates presented in Tables A1 and A2. Data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

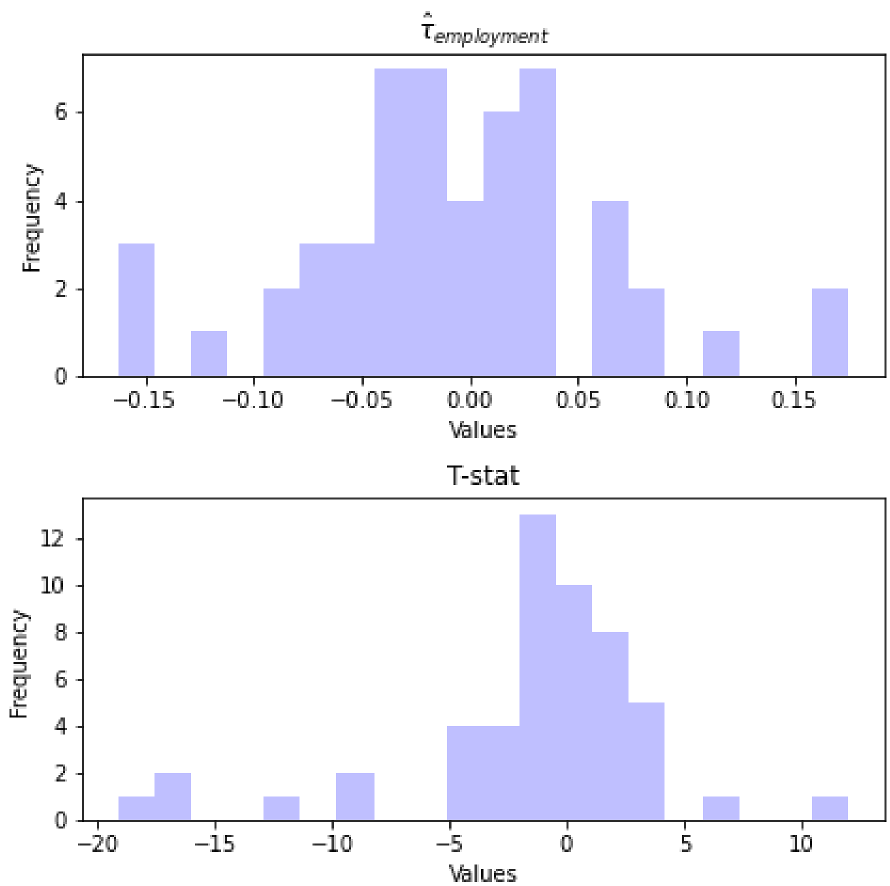
5.3 Employment Effects

Next, we examine the impact of labor cost increases (minimum wage policy changes) on employment, as measured by proxy in the SafeGraph Patterns data using the number of 4+ hour visits, following Pandit (2023). This measure, while approximate, allows us to (coarsely) measure the change in total employment from the policy change; but we note that since this proxy measure is undoubtedly noisy in its correspondence to store-level employment, effect estimates should be interpreted as a conservative lower-bound due to the likely presence of non-negligible attenuation bias. Nonetheless, as noted in Pandit (2023), there are strengths to this measure, in particular since it is a rare *store-level* employment metric, allowing us to estimate effects in a differences-in-differences design that controls for store-level fixed effects, thereby residualizing out any store-level confounds, in addition to time fixed effects.

Results are presented in Figure 6. We estimate a mean employment decline of 0.7% and a median employment decline of 1.3%.[16] Quantitatively, we find a similarly wide dispersion of effect estimates as compared to our first analysis of demand effects, and similarly with a more left-skewed distribution that includes many more negative effect estimates than positive (although both directions of effects would be consistent with findings in prior minimum wage literature (Card and Krueger 1993, 1995, Neumark and Wascher 2000, Dube et al. 2010, Cengiz et al. 2019, Pandit 2023)).

[16] Excluding event-studies that coincide with COVID-19 leads to a highly similar distribution, with a mean decrease of 0.5% and a median decrease of 0.9%. See Appendix B for details.

Figure 6: Histograms of Employment Effects



Notes: Histogram of effects $\hat{\tau}$ of minimum wage increases on the number of retail store 4+ hour visits, as a proxy for the number of employees, based on synthetic differences-in-differences estimator. Precise numerical estimates presented in Tables A1 and A2. Data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

This wide dispersion of employment effects mirrors the substantial heterogeneity documented in the large empirical literature on minimum wage policy (Card and Krueger 1995, Lemos 2008, Neumark 2018, Cengiz et al. 2019, Pandit 2023). However, in minor contrast to earlier work that investigated state-level changes and did not use specific store-level data, namely Cengiz et al. (2019)—but in line with work that does use such micro-data, namely Pandit (2023)—we find that while our distribution includes non-negligible densities of both positive and negative employment effects, on average effects are negative, rather than negligibly small or effectively zero. This suggests that, in contrast to state-level minimum wage changes, sub-state level local minimum wage changes may often have small, but nonzero, disemployment effects, in contrast to generally null disemployment effects recovered in prior literature on state-level shifts (ibid.).[17]

6 Secondary Regression Analyses

Given this high degree of variation across contexts, a natural follow-up inquiry is to examine possible explanations for the differential effects that we recover from our set of event studies. In this section, to account for differing levels of precision across event studies, we use simple inverse-variance-weighted regression to examine systematic relationships across effect dimensions, following Higgins and Green (2011).

[17] Alternatively, one may argue that this suggests that our store-level microdata allows for cleaner identification by comparing affected cities or counties to surrounding counties, as compared to state-level analyses that rely on control groups (i.e., other states) that may be more likely to suffer from unobserved confounding variation.

This method gives greater weight to more precise estimates, and downweights noisier estimates, when computing the overall relationships between effects.

6.1 Effect Estimates and Minimum Wage Increase Magnitude

As the relative size of minimum wage increase varies meaningfully across contexts, we begin by inspecting perhaps the most intuitive explanation for variation in effects, that variation in the magnitude of the minimum wage increase may explain some proportion of the differences in effects that we observe.

Table 3: Regressions of Log Minimum Wage Increase on Estimated Effects

	Median Transaction Amount	Employment	Transaction Quantity	Revenue
Log MW Increase	-0.011 (0.048)	-0.205 (0.278)	-0.211 (0.250)	-0.251 (0.259)
N	52	52	52	52
R^2	0.001	0.011	0.014	0.018

Notes: Inverse-variance-weighted least squares regressions of each estimated effect on the log change in minimum wage. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included but not shown.

Across the board, we find insignificant statistical relationships between the size of the minimum wage increase and all of our measured effects, across all four dimensions of revenue, transaction count, median transaction amount, and employment, as shown in Table 3, which compares the log minimum wage change ($log(MW_{after}) - log(MW_{before})$ to the respective estimated $\hat{\tau}$. That said, the point estimate is negative between employment effects and minimum wage increase size, suggesting that larger minimum wage increases are insignificantly associated with larger employment declines, which accords with economic intuition; moreover, the estimate of a 0.2 elasticity of employment changes to minimum wage changes is closely in line with prior literature (Dube and Zipperer 2024). Similarly, we find negative point estimates between the magnitude of the minimum wage increase and transaction quantity and revenue effects, suggesting that every 1% increase in labor costs leads to an approximately 0.2% decline in transaction quantity and revenue. Given that we only have a sample of just over 50 event study estimates, we our sample may be underpowered to recover significant results, but we hope that our point estimates may nonetheless be an informative prior for the empirical relationship between labor cost hikes and local demand, for both policymakers and managers alike.

6.2 Price and Employment Effects Against Revenue and Transaction Quantity Effects

Next, we examine the extent to which variation in price (median transaction amount) and employment effects relates to variation in business losses from the labor cost increases, as measured by revenue and transaction quantity effects. With this analysis, we hope to recover further empirical guidance on how business responses are (or are not) systematically associated with greater or smaller losses in local retail.

Results are presented in Table 4. In the top panel, we present the estimated relationship between employment effects and revenue and transaction quantity effects,

Table 4: Regression of Business Adjustment Effects on Demand Changes

	Revenue	Transaction Quantity
Employment	0.438** (0.150)	0.409** (0.152)
N	52	52
R^2	0.146	0.127

Notes: Regression of revenue and transaction quantity effects on employment effect estimates, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.

	Revenue	Transaction Quantity
Median Trans. Amount	-1.882** (0.759)	-2.358*** (0.715)
N	52	52
R^2	0.109	0.179

Notes: Regression of revenue and transaction quantity effects on median transaction amount effect estimates, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.

	Revenue	Transaction Quantity
Employment	0.334* (0.157)	0.279* (0.152)
Median Trans. Amount	-1.437* (0.768)	-2.053*** (0.726)
Log MW Change	-0.177 (0.244)	-0.177 (0.229)
N	52	52
R^2	0.208	0.254

Notes: Joint regression of revenue and transaction quantity effects on employment, median transaction amount, and log minimum wage change, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.

respectively, showing a highly significant positive relationship between employment and demand effects, showing that larger employment declines are significantly associated with stronger businesses losses. Our point estimates suggest that every percentage point of additional disemployment is associated with an addition half percentage point of revenue or transaction quantity loss.

In the center panel, we present the estimated relationship between median transaction amount effects, which we interpret as capturing the extent of price adjustment, and business losses. We here recover a much larger significant negative relationship between the degree of median transaction amount (price) adjustment and associated business effects, suggesting that demand is far more sensitive to price changes. Point estimates suggest that every percentage point increase in median transaction amount is associated with three percentage points larger revenue or transaction quantity loss.[18]

In the bottom panel we present estimation results from a multivariate regression that includes both effect dimensions, as well as the size of the minimum wage change, as regressors, to determine in a single model the extent to which these effects are associated with differential change is revenue and transaction quantity, controlling for

[18]We acknowledge here that we measure both employment and price shifts by proxy, and so one may concerned that both of these effects are attenuated towards zero. As such, both of these may best be interpreted as lower-bound estimates on the true elasticities of demand to either price or employment changes. We also note that one may be concerned that the relative comparison between these two elasticities may be impacted by relative differences in attenuation bias for either proxy measure. However, given the massive differential between these effects—with price changes estimated to be associated with 4x-5x larger changes on business demand than employment changes—we argue that it is exceedingly unlikely that measurement error would differ to such a massive degree as to define this difference. Nonetheless, we caution that the exact relative magnitude may be informed by the relative degree of attenuation bias between the proxies we rely on for either metric.

these other margins of adjustment. While point estimates are slightly smaller than in the univariate models, they are qualitatively highly similar, with every additional percentage point of price increase associated with a 1.5 percentage point larger revenue loss and every additional percentage of employment decline associated with a 0.3 percentage point larger revenue loss. The point estimate on the effect of the size of the minimum wage change on revenue losses also remains highly similar. Altogether, we find these three factors are able to explain 20% to 25% of variation in businesses losses following minimum wage increases—considerably more than the roughly 2% variation that was statistically explained by variation in the size of the minimum wage increase alone.

7 Conclusion

Rising labor costs present a major challenge for retail stores, requiring difficult trade-offs between raising prices, reducing staff, or accepting lower profits. Understanding how consumers respond to such adjustments is crucial for informing retail store managers as well as policymakers considering minimum wage laws. This paper provides one of the most comprehensive investigations to date of how retail businesses respond to rising labor costs, specifically in the form of local minimum wage increases. By estimating the effects of 52 discrete minimum wage hikes from 2019 to 2022 across U.S. cities and counties, we construct a large-scale, consistent empirical foundation for understanding the consequences of wage floors on retail store outcomes. Our findings are both practically relevant and theoretically illuminating, offering new insight into how retail firms adjust along multiple margins: employment, pricing (average transaction amount), and ultimately, transaction quantity and revenue.

At a broad level, our first-step analysis reveals meaningful heterogeneity in the effects of local minimum wage increases on retail store outcomes. While the median estimated effects suggest relatively modest declines in revenue (0.3%), transaction counts (0.5%), and employment (1.3%), and a very modest increase in average transaction amount (0.04%), these central estimates mask a wide distribution of impacts across different contexts. In several settings, we observe sizable revenue or employment losses; in others, effects are statistically indistinguishable from zero or even suggest mild increases.

We then perform secondary analyses on our estimated joint distribution to examine the relationships between these outcome dimensions, and between these effects and the size of the local policy change. We find that, while point estimates are insignificant, every 1% increase in labor costs is associated with a 0.2% decline in employment, closely matching prior empirical literature; and moreover, we find that every 1% increase is associated with a 0.2% decline in transaction quantity, offering novel large-scale evidence on the elasticity of business demand to labor cost changes.

Moreover, we find that differences in the way businesses adjust, namely the relative magnitudes of their price and employment responses, appear to be much more strongly associated with the observed variation in business performance. We estimate highly significant relationships between both employment changes and price adjustments with overall business outcomes, but with markedly different magnitudes: every percentage point decline in employment is associated with approximately 0.5 percentage points of additional revenue loss, while every percentage point increase in median transaction amount is associated with approximately 2 percentage points of additional revenue decline. These results suggest that consumer demand in retail settings is significantly more sensitive to price changes than to service quality changes associated with staffing adjustments.

These findings have important implications for both business strategy and public policy. For managers, the results provide crucial insight into the relative elasticities of demand to employment and pricing shifts, revealing that consumers are significantly more sensitive to price changes than to service quality changes associated with staffing adjustments. This suggests that when facing labor cost increases, firms may need to carefully weigh the trade-offs between maintaining service levels through higher staffing versus preserving demand through price restraint.

For policymakers, our results highlight a core tradeoff for policies that attempt to mitigate potential disemployment effects of minimum wage increases. While employment cuts do negatively impact business performance, the much larger sensitivity of demand to price changes suggests that policies making it harder to reduce employment—such as stricter labor contracts or firing restrictions—may force firms to absorb labor cost increases primarily through price adjustments, potentially leading to significantly larger negative impacts on business revenue and transaction volumes. This finding suggests that policymakers should carefully consider how labor market rigidities might amplify the business costs of minimum wage policies, as the inability to adjust along the employment margin may force more damaging adjustments along the price dimension.

That said, this study leaves several open questions and directions for future work. First, our use of median transaction amount as a proxy for price is necessarily imperfect, especially in contexts where consumers change basket size in response to price increases. Future work with item-level pricing data could more cleanly separate price effects from compositional shifts. Second, we rely on a proxy for employment levels (long-duration visits); future research using payroll or HR records could provide more granular and accurate estimates of staffing changes. Third, extending this analysis to include firm exit, entry, or investment decisions could deepen our understanding of the longer-run dynamic consequences of labor cost changes. Fourth, incorporating differences in labor market structure, such as different levels of monopsony power, could help link our findings more closely with earlier minimum wage literature that focused on monopsony as another driver of differential effects across contexts. Finally, future research could examine differential effects by store type or consumer demographic, including whether adjustments burden certain communities or worker groups more than others.

Understanding the impact of labor cost shocks on retail is crucial for both store managers and for policymakers aiming to support businesses. The recent flurry of local minimum wage changes affords researchers a novel opportunity to gather large-scale evidence on these effects, and to study the relationships between different effect dimensions. By estimating the joint distribution of outcome effects across dozens of real-world policy changes, we offer novel insights that both clarify previous debates and open new avenues for understanding firm resilience in the face of labor cost shocks.

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Appendix A:

Further Details on Reduced-Form Results

In this section, we present detailed results on the full set of event studies investigated in our reduced-form results, and for the subset of our event studies that occur prior to the COVID-19 pandemic. For each event study, we use synthetic differences-in-differences from Arkhangelsky et al. (2021) to estimate the effect of the minimum wage increase on retail store revenue for the given locality (county or municipality) that hiked the minimum wage, with the synthetic control group based on retail stores in surrounding counties that did not experience a contemporaneous minimum wage hike. We include event studies for all localities in our collected sample and during our examined period except those that overlapped (within 3 months) with a major COVID-19 lockdown period of March 2020 or January 2021.

In Tables A1 and A2 below, we present the full set of synthetic differences-in-differences estimates for all 52 local minimum wage increases inspected, $\hat{\tau}$, across all outcomes measures examined. We include information both on the timing of the minimum wage increase as well as the locality examined. Overall we find a high proportion of significant effects across all dimensions examined, well above the 5% ratio of significant effects that one might expect through random chance, even though some of our outcome metrics are measured with non-negligible noise, such as our employment proxy measure based on 4+ hour visits.

Table A1: Event Study Results: 2019 and 2020

Synthetic Differences-in-Differences Estimates

Year	**Month**	**Locality**	$\hat{\tau}_{emp}$	$\hat{\tau}_{med\_trans\_amount}$	$\hat{\tau}_{trans}$	$\hat{\tau}_{revenue}$
2019	7	Alameda	-0.020 (0.024)	0.020 (0.011)	-0.000 (0.020)	0.005 (0.023)
2019	7	Berkeley	0.032* (0.015)	0.009 (0.007)	0.008 (0.015)	0.015 (0.016)
2019	7	Emeryville	0.024 (0.033)	0.011 (0.038)	-0.014 (0.027)	-0.004 (0.022)
2019	7	Malibu	0.067 (0.041)	0.000 (0.022)	-0.064* (0.028)	-0.075* (0.038)
2019	7	Milpitas	-0.005 (0.020)	0.000 (0.009)	-0.041* (0.017)	-0.031 (0.020)
2019	7	Pasadena	-0.026* (0.013)	-0.005 (0.008)	-0.015 (0.010)	-0.014 (0.012)
2019	7	San Francisco	-0.009 (0.006)	0.010* (0.004)	-0.007 (0.006)	-0.003 (0.007)
2019	7	San Leandro	0.007 (0.021)	0.009 (0.009)	0.019 (0.016)	0.017 (0.019)
2019	7	Santa Monica	-0.044** (0.015)	0.029* (0.011)	-0.039** (0.014)	-0.021 (0.016)
2019	7	Chicago	-0.016** (0.005)	-0.005 (0.003)	0.005 (0.005)	0.001 (0.006)
2019	7	Portland	-0.068** (0.021)	0.008 (0.012)	-0.043* (0.019)	-0.032 (0.023)
2019	7	Minneapolis	-0.018 (0.010)	0.004 (0.008)	-0.033** (0.011)	-0.032* (0.014)
2019	7	Los Angeles County	-0.032*** (0.003)	-0.002 (0.002)	-0.003 (0.003)	-0.006 (0.003)
2019	7	Cook County	0.000 (0.006)	-0.009** (0.003)	0.009* (0.005)	-0.001 (0.005)
2019	7	Montgomery County	0.036*** (0.009)	0.011* (0.005)	-0.015* (0.007)	-0.005 (0.009)
2020	7	Alameda	0.027 (0.033)	-0.010 (0.012)	0.016 (0.026)	0.007 (0.029)
2020	7	Emeryville	-0.035 (0.051)	-0.006 (0.025)	0.126 (0.076)	0.100 (0.079)
2020	7	Malibu	0.162** (0.053)	-0.021 (0.024)	-0.138** (0.048)	-0.166** (0.054)
2020	7	Milpitas	-0.063* (0.029)	-0.006 (0.016)	0.026 (0.022)	0.011 (0.028)
2020	7	Pasadena	-0.160*** (0.019)	0.014 (0.011)	-0.233*** (0.021)	-0.245*** (0.024)
2020	7	San Francisco	-0.162*** (0.010)	0.013* (0.006)	0.083*** (0.012)	0.088*** (0.013)
2020	7	San Leandro	0.010 (0.026)	-0.002 (0.014)	0.006 (0.022)	0.012 (0.024)
2020	7	Chicago	-0.115*** (0.007)	0.007 (0.004)	-0.081*** (0.008)	-0.080*** (0.009)
2020	7	Minneapolis	-0.163*** (0.014)	-0.022 (0.012)	-0.046* (0.022)	-0.062* (0.024)
2020	7	Los Angeles County	-0.082*** (0.004)	0.006* (0.002)	-0.164*** (0.008)	-0.164*** (0.008)
2020	7	Cook County	-0.031*** (0.007)	0.003 (0.004)	-0.043*** (0.006)	-0.046*** (0.007)
2020	7	Montgomery County	-0.059*** (0.013)	0.006 (0.006)	0.011 (0.011)	0.008 (0.012)

Notes: Effect of minimum wage increases on retail store outcomes based on synthetic differences-in-differences estimator for all studied settings in 2019 and 2020. Revenue data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table A2: Event Study Results: 2021 and 2022

Synthetic Differences-in-Differences Estimates

Year	**Month**	**Locality**	$\hat{\tau}_{emp}$	$\hat{\tau}_{med\_trans\_amount}$	$\hat{\tau}_{trans}$	$\hat{\tau}_{revenue}$
2021	7	Berkeley	0.063* (0.025)	-0.023* (0.012)	0.066** (0.023)	0.049 (0.026)
2021	7	Emeryville	-0.081 (0.052)	-0.030 (0.016)	0.005 (0.028)	0.003 (0.035)
2021	7	Milpitas	0.062* (0.029)	0.035* (0.016)	-0.117*** (0.023)	-0.099*** (0.027)
2021	7	San Francisco	0.038*** (0.011)	-0.016** (0.005)	0.038*** (0.009)	0.029** (0.010)
2021	7	Chicago	0.075*** (0.006)	-0.013*** (0.004)	0.077*** (0.007)	0.066*** (0.007)
2021	7	Minneapolis	0.087*** (0.014)	-0.034*** (0.010)	0.135*** (0.016)	0.116*** (0.019)
2021	7	Los Angeles County	0.020*** (0.005)	-0.003 (0.002)	-0.008 (0.004)	-0.010* (0.005)
2021	7	Montgomery County	0.013 (0.013)	0.002 (0.006)	0.036*** (0.009)	0.040*** (0.010)
2022	1	Flagstaff	-0.056* (0.028)	-0.019 (0.011)	-0.158*** (0.022)	-0.189*** (0.025)
2022	1	Belmont	0.033 (0.062)	0.041 (0.036)	0.028 (0.055)	0.077 (0.066)
2022	1	Cupertino	0.122 (0.062)	-0.049 (0.032)	0.115** (0.044)	0.060 (0.043)
2022	1	El Cerrito	-0.043 (0.056)	-0.047 (0.025)	-0.170*** (0.038)	-0.205*** (0.046)
2022	1	Los Altos	0.175* (0.072)	0.000 (0.047)	-0.066 (0.041)	-0.072 (0.056)
2022	1	Mountain View	0.067 (0.037)	0.042** (0.015)	-0.063* (0.026)	-0.031 (0.029)
2022	1	Oakland	-0.065*** (0.015)	0.013 (0.008)	-0.031* (0.016)	-0.041* (0.018)
2022	1	Palo Alto	-0.008 (0.031)	0.016 (0.012)	-0.021 (0.030)	-0.025 (0.032)
2022	1	Redwood City	-0.031 (0.032)	-0.004 (0.014)	0.115*** (0.028)	0.120*** (0.033)
2022	1	Richmond	0.009 (0.026)	-0.006 (0.014)	0.107*** (0.027)	0.122*** (0.029)
2022	1	San Diego	0.021** (0.008)	0.006 (0.004)	-0.139*** (0.009)	-0.141*** (0.010)
2022	1	San Mateo	-0.026 (0.030)	0.012 (0.015)	0.082** (0.025)	0.088** (0.029)
2022	1	Santa Clara	-0.033 (0.025)	0.006 (0.013)	0.029 (0.026)	0.021 (0.029)
2022	1	Denver	-0.020 (0.012)	-0.001 (0.005)	0.034** (0.011)	0.019 (0.012)
2022	1	Portland	-0.017 (0.032)	-0.009 (0.012)	-0.044* (0.022)	-0.061* (0.026)
2022	1	Seattle	-0.056*** (0.011)	-0.012* (0.005)	0.051*** (0.009)	0.037*** (0.010)
2022	4	Tucson	0.039** (0.014)	0.008 (0.006)	-0.014 (0.010)	-0.008 (0.012)

Notes: Effect of minimum wage increases on retail store outcomes based on synthetic differences-in-differences estimator for all studied settings in 2021 and 2022. Revenue data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

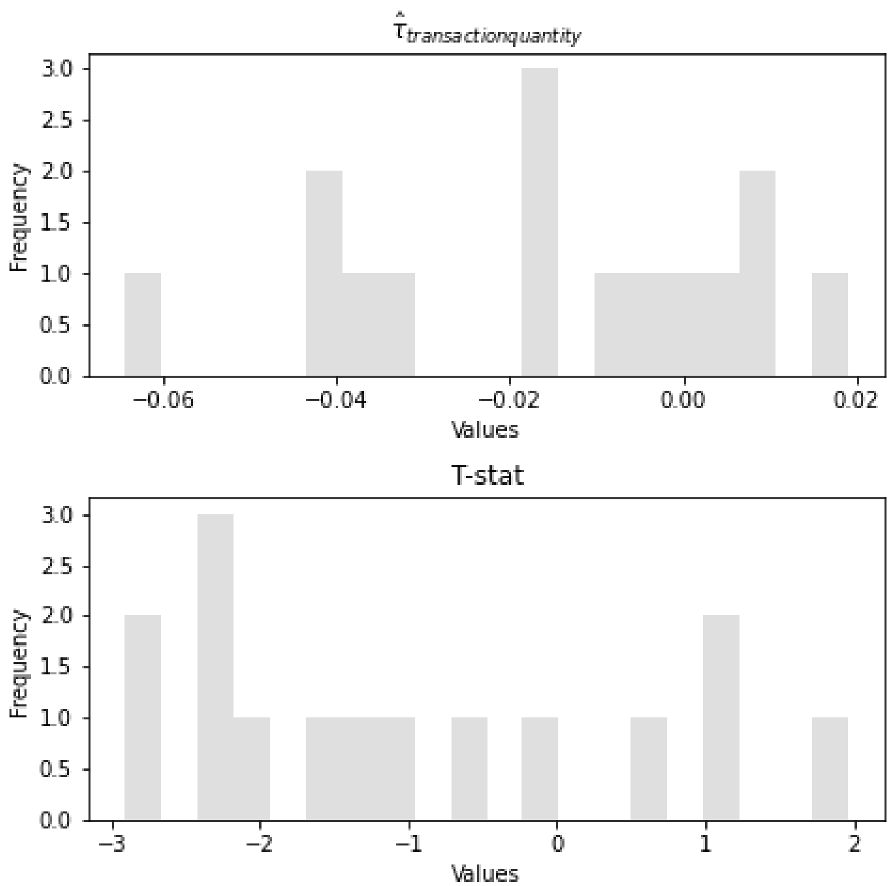
Appendix B:

Excluding COVID-19 — Distributions for Only 2019

While we exclude event studies that closely overlap with lockdown periods in the above sample, one may remain concerned that the majority of our event studies still take place during the COVID-19 pandemic and therefore may be biased or otherwise unrepresentative of effects in other periods. To empirically investigate this, we inspect the more limited distribution of effects arising from 2019 event studies, which are thereby unaffected by COVID and any associated lockdowns. Results are presented in Figures B1, B2, B3, and B4. While these only-2019 distributions are necessarily much sparser, they are similarly centered and qualitatively similar to the effect distributions when using later years.

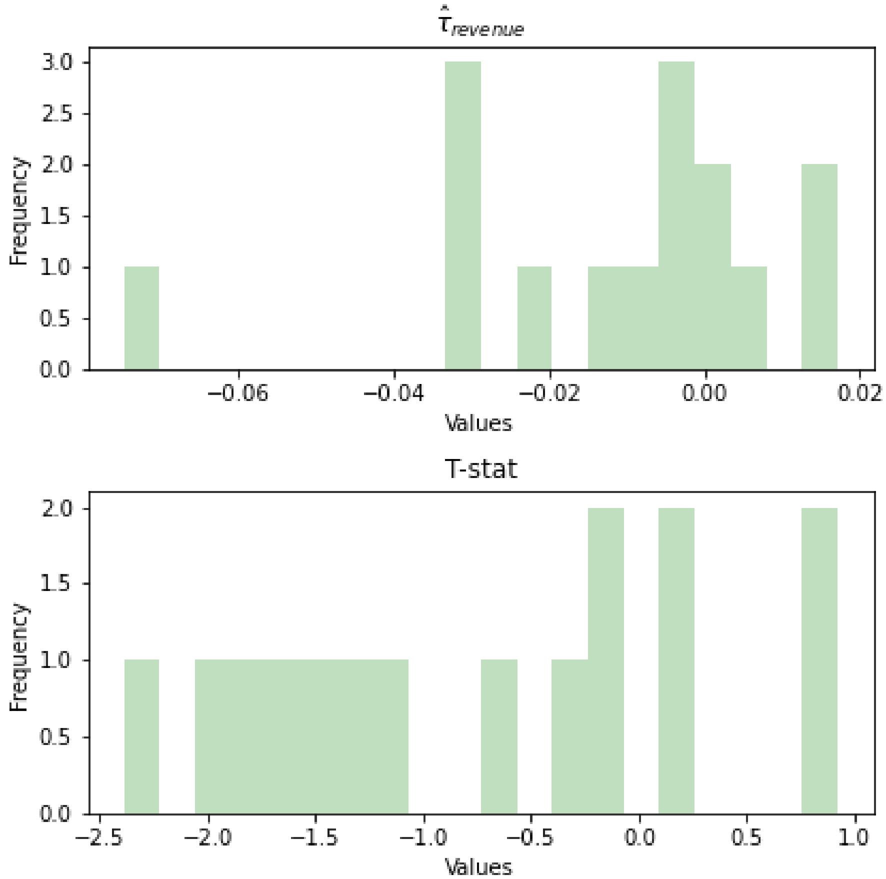
For example, the mean employment effect is -0.69% across all years but only -0.48% in the 2019-only sample, and the median effect shifts from -1.27% to -0.94%. Mean and median effects on total spend are -1.47% and -0.34% with COVID data included, versus -1.25% and -0.54% in the pre-COVID-only sample. Median price effects are small in both samples, with a median of 0.04% with COVID and 0.82% in the 2019-only sample. Finally, transaction count effects remain negative throughout: the mean is -1.12% with COVID versus -1.55% without, and the median is -0.50% versus -1.45%. Together, these comparisons indicate that while the 2019-only sample is smaller and somewhat more conservative in magnitude, the central tendencies of estimated effects are stable and suggest robustness to concerns about COVID-period bias.

Figure B1: Histograms of Transaction Quantity Effects: Only 2019



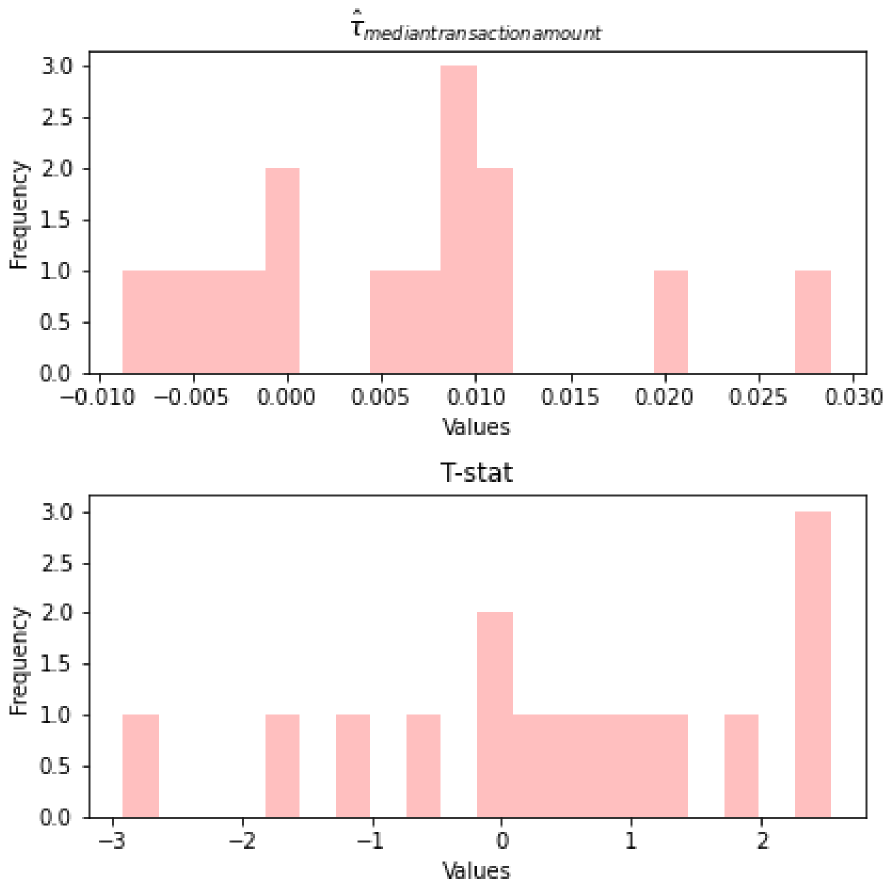
Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on synthetic differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Transactions data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements, restricted to those in 2019.

Figure B2: Histograms of Revenue Effects: Only 2019



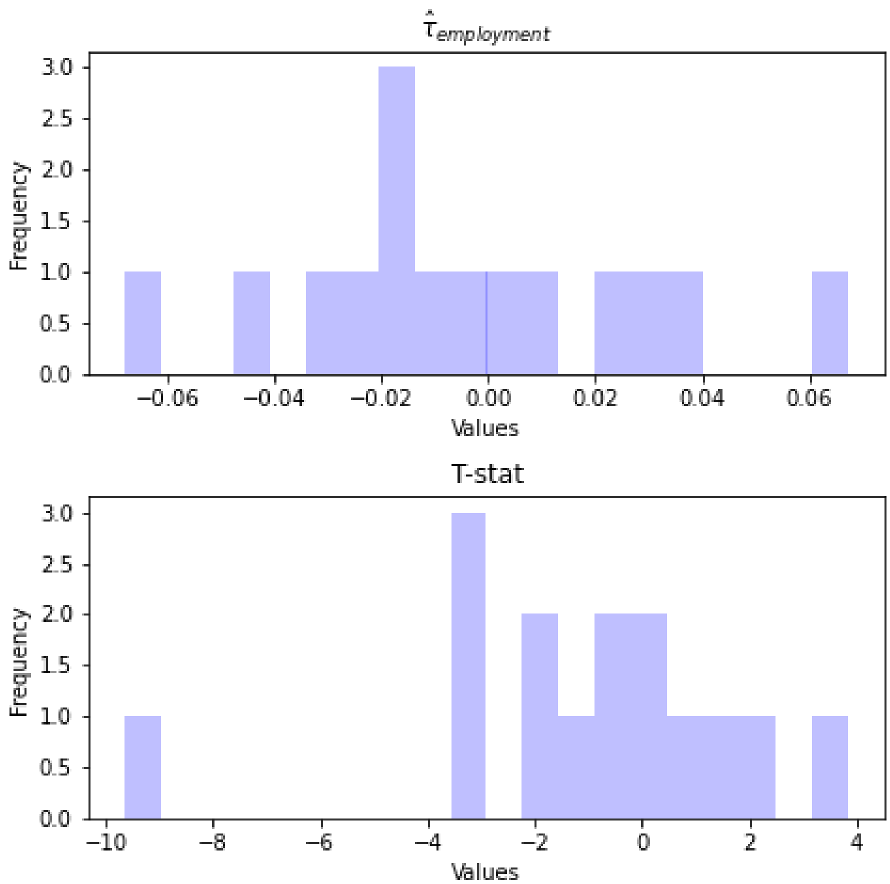
Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on synthetic differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Revenue data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements, restricted to those in 2019.

Figure B3: Histograms of Median Transaction Amount Effects: Only 2019



Notes: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store median transaction amount based on synthetic differences-in-differences estimator. Precise numerical estimates presented in Tables A1 and A2. Data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements, restricted to those in 2019.

Figure B4: Histograms of Employment Effects: Only 2019



Notes: Histogram of effects $\hat{\tau}$ of minimum wage increases on the number of retail store 4+ hour visits, as a proxy for the number of employees, based on synthetic differences-in-differences estimator. Precise numerical estimates presented in Tables A1 and A2. Data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements, restricted to those in 2019.

Appendix C:

Simple Differences-in-Differences Estimates

In our main analysis, we rely on the synthetic differences-in-differences method of Arkhangelsky et al. (2021) in order to minimize our reliance on assumptions of parallel trends, given that we are examining a large sample of event studies and it is likely that parallel trends may not hold in some portion of that sample; but selecting on parallel trends may itself lead to problematic bias (Roth 2022). Nonetheless, given that synthetic differences-in-differences is a recent methodology, we also present our full pipeline of results using simple differences-in-differences, estimated using two-way fixed effects.

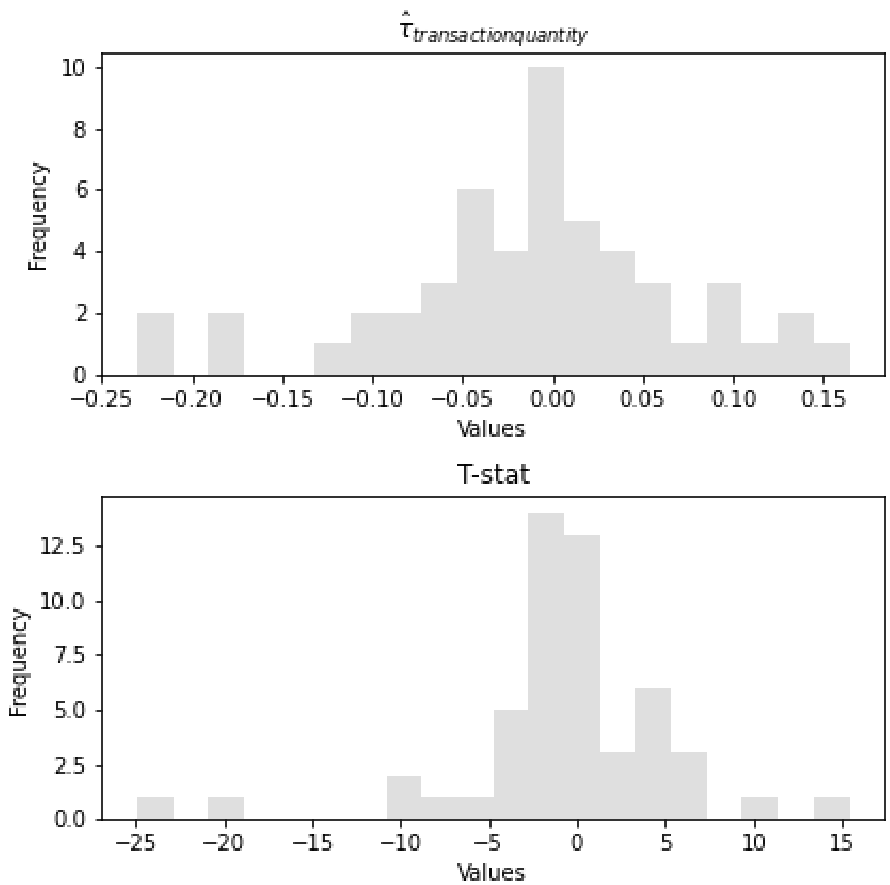
C.1 Effect Distributions

Effect distributions from simple differences-in-differences are presented in Figures C1, C2 and C3.[19] While the precise numbers differ, the distributions are qualitatively highly similar across outcome dimensions. For total spend, the synthetic DiD mean is -1.47% and median -0.34%, while simple DiD gives -1.27% and -0.31%, respectively. Median transaction amounts effects remain minimal in both approaches, with synthetic DiD yielding a near-zero mean (-0.02%) and normal DiD only slightly

[19] As employment effects are estimated using two-period differences-in-differences in our main analysis due to the high prevalence of zero-valued observations, we use simple differences-in-differences in all cases. Given this, we do not reproduce that figure here, and instead only present distribution figures for estimates that differ.

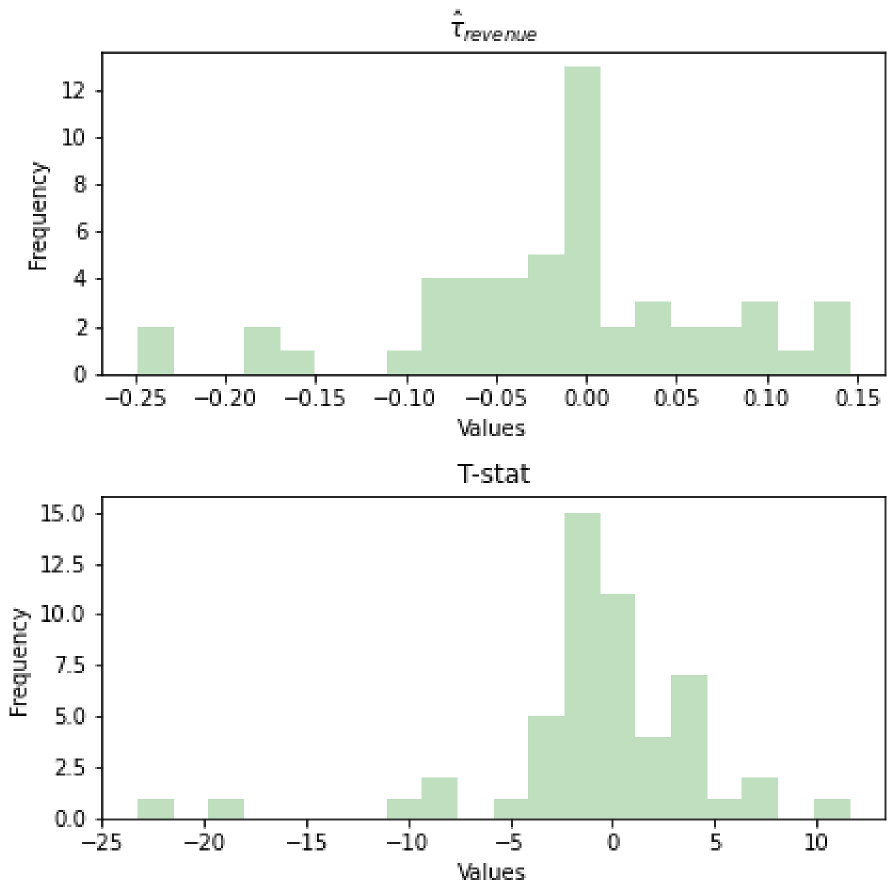
more negative (-0.09%). Finally, overall transaction effects are negative across both methods: synthetic DiD yields a mean of -1.12% and median -0.50%, while normal DiD estimates are -1.04% and -0.79%, respectively. Taken together, the robustness of these distributions to the choice of method underscores the stability of our findings.

Figure C1: Histograms of Transaction Quantity Effects: Simple DiD



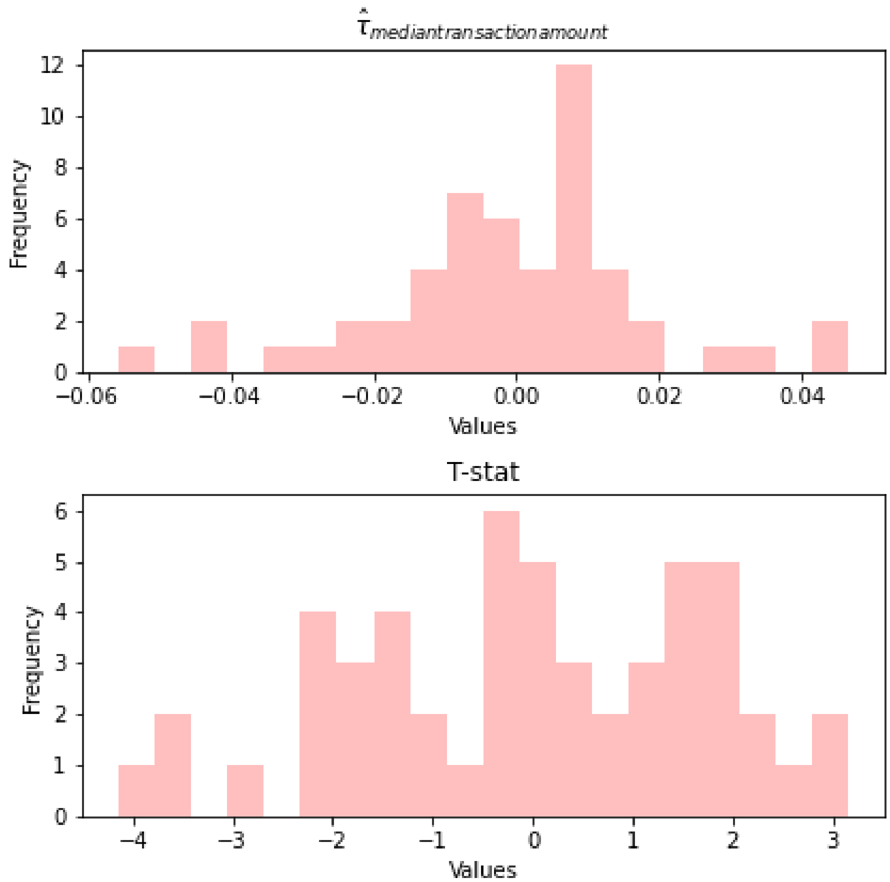
Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on two-way fixed effects differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Transactions data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

Figure C2: Histograms of Revenue Effects: Simple DiD



Notes: Top panel: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store transactions based on two-way fixed effects differences-in-differences estimator. Bottom panel: Histogram of t-statistics of minimum wage effects. Precise numerical estimates presented in Tables A1 and A2. Revenue data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

Figure C3: Histograms of Median Transaction Amount Effects: Simple DiD



Notes: Histogram of effects $\hat{\tau}$ of minimum wage increases on retail store median transaction amount based on two-way fixed effects differences-in-differences estimator. Precise numerical estimates presented in Tables A1 and A2. Data from SafeGraph Spend panel. Minimum age increases based on data hand-collected from online state webpages and policy announcements.

C.2 Secondary Analyses

While above we show that the distributions of effects are highly similar when using either synthetic differences-in-differences or simple differences-in-differences, one may still be concerned that the secondary relationships between effect estimates may vary across estimation methodologies. To allay this concern, we here reproduce all of our secondary analyses. Across analyses, results are qualitatively identical.

First, we find also null effects between minimum wage change magnitude and effect estimates, presented in Table C1.

Table C1: Regressions of Log Minimum Wage Increase on Estimated Effects (Based on Simple DiD τ Estimates)

	Median Transaction Amount	Employment	Transaction Quantity	Revenue
Log MW Increase	-0.007 (0.050)	-0.205 (0.278)	-0.286 (0.296)	-0.345 (0.295)
N	52	52	52	52
R^2	0.000	0.011	0.018	0.027

Notes: Inverse-variance-weighted least squares regressions of each estimated effect on the log change in minimum wage. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included but not shown.

Second, we find qualitatively very similar results in our regressions of business response effects on revenue and transaction quantity. We recover a significantly negative relationship between the degree of employment adjustment (significantly positive relationship between the degree of price adjustment) and overall revenue and transaction quantity losses, presented in Table C2, with the estimated effect of price changes on revenue roughly 5x-6x larger than the estimate effect of employment changes on revenue. We furthermore find a comparatively high degree of explanatory power from a model that includes both employment effects, median transaction amount effects, and minimum wage change magnitudes on revenue and transaction quantity effects, achieving R^2 of roughly 0.27 and 0.34, respectively.

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Table C2: Regression of Business Adjustment Effects on Demand Changes (Based on Simple DiD τ Estimates)

	Revenue	Transaction Quantity
Employment	0.499** (0.170)	0.518** (0.175)
N	52	52
R^2	0.147	0.149

Notes: Regression of employment effect estimates on median transaction amount effects, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.

	Revenue	Transaction Quantity
Median Trans. Amount	-2.678*** (0.808)	-3.308*** (0.785)
N	52	52
R^2	0.180	0.262

Notes: Regression of employment effect estimates on median transaction amount effects, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.

	Revenue	Transaction Quantity
Employment	0.312 (0.175)	0.297 (0.171)
Median Trans. Amount	-2.236** (0.835)	-2.950*** (0.807)
Log MW Change	-0.288 (0.270)	-0.278 (0.257)
N	52	52
R^2	0.265	0.337

Notes: Regression of employment effect estimates on median transaction amount effects, estimated with inverse-variance weights. Standard errors in parentheses. *Significance levels:* *p<0.05, **p<0.01, ***p<0.001. Constant term included by not shown.