The Asymmetric Effect of Wage Floors: A Natural Experiment with a Rising and Falling Minimum Wage*

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Abstract

Exploiting a unique natural experiment, we show the asymmetric effect of a large increase and an equivalent subsequent decrease to a binding minimum wage. Earnings in a leading low-wage industry increase as the minimum wage rises, but do not fall when it is lowered. This boost to workers' earnings is apparently permanent five years after the policy is revoked, providing novel evidence of earnings hysteresis from temporary labor policy. In the first year post repeal, this is consistent with downward nominal wage rigidity. But, the elevated earnings persist long term, even in high inflation times, contrary to the prevailing view that the pay gap relative to unaffected workers should be eroded by inflation. Our analysis suggests this persistence stems from changed wage-setting norms in the labor market. These results highlight the role of public policy in shaping wage expectations, and demonstrate the potential of even transitory interventions in achieving permanent gains for workers (play it while you got it).

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1 Introduction

It is a near truism that minimum wages, if they change, move only in one direction — up. But what happens to firm pay and employment when a wage floor covering all workers suddenly falls? This paper studies such an unusual scenario, a very large increase in the minimum wage followed by an equivalent, unexpected, and precipitous drop. We assess the economic effects of the policy, leveraging this shock to reveal new insights on the broader functioning of the labor market.

Our identification strategy is made possible by the proliferation in the last decade of county and city-specific minimum wages across the United States, and the conflict between different levels of government that has consequently arisen. In Johnson County, Iowa, this conflict led to a significant reduction in the statutory minimum wage. Figure 1 shows the anomalous recent history of the minimum wage there. In late 2015, the Johnson County local government initiated dramatic increases in the minimum wage — from $7.25 up to $8.20, then $9.15, and finally $10.10 — that unfolded in just over a year.[1] For 17 months in total, an elevated local minimum wage was in effect in the county (exceeding the $7.25 federal rate that had otherwise governed in the state) — until the state government abruptly made it illegal for sub-state localities to have their own minimum wage level. As a result, upon the sudden repeal in spring of 2017, the minimum wage in Johnson fell by almost $3, going back to the state level of $7.25. This unique event was followed in the ensuing years by two unusual periods: first, the COVID-19 pandemic recession in 2020, and then a period of high inflation cresting at 9% in mid-2022.

As an initial step in examining the labor market effects of this large temporary minimum wage, we employ a synthetic control approach. In particular, we compare the labor market behavior in the restaurant sector — the leading minimum-wage-employing industry — in Johnson County relative to that in a set of similar counties. The primary data utilized are administrative records on earnings and employment at the county level.

The main result of this analysis of our natural experiment is novel evidence of wage-setting hysteresis from temporary labor policy: the minimum wage increase causes a permanent increase in affected worker pay, with the earnings gains being sustained long (5 years) after the precipitating policy has ceased. First, the introduction of the higher minimum wage has a direct impact on restaurant earnings in Johnson County while the policy is in effect, increasing pay by approximately 10%. In the first year after the minimum wage is repealed, there is no indication of an earnings reduction for affected workers, with earnings remaining higher than

[1] This total increase of 39% in the minimum wage is considerably larger than the minimum wage increases studied in most existing work, though, in the last decade research has analyzed some cases of comparably large minimum wage increases (see Dube and Lindner (2024) for a review).

synthetic Johnson by a similar magnitude as when the policy was in effect. Longer term, we find that the earnings boost endures even five years after the minimum wage reduction, both through the COVID-19 recession and the high inflation episode of late 2021 through 2022. Further analysis suggests that earnings of new hires also increased in response to the policy, with not only incumbents enjoying the legacy of higher overall earnings, but, also new workers who had not been with the same employer when the local minimum wage policy was in place.

In terms of employment, we do not find a significant change in the number of employed restaurant workers while the elevated minimum wage is in effect. With no jobs immediately lost from the increase, there is, definitionally, no "rebound" when the minimum wage is lowered, and, indeed, in the year after the revocation took place there remains no significant change in employment. Starting more than three years post-implementation and into the COVID-19 years, employment exhibits somewhat larger fluctuations; however, these fluctuations remain modest and statistically insignificant. Our results imply a statistically insignificant own-wage elasticity (Dube and Zipperer, 2024) that is close to zero: -0.04 (s.e. 0.73).

With the aim of understanding how these elevated earnings can be at least partially financed, we also investigate the effect of the temporary labor market policy on prices — a key margin of potential firm adjustment to counteract the increased labor cost. To do so, we construct a newly collected database of dish-level prices by digitizing restaurant menus from online platforms. Results offer evidence of another asymmetry in the price response to the minimum wage. First, we observe that higher labor costs are passed through to consumers with a gradual increase in prices following the minimum wage hike. These findings align with existing literature showing full pass-through of increased minimum wage costs (e.g., Harasztosi and Lindner, 2019; Renkin et al., 2022), though the level of precision allows for a wide range of pass-through estimates. Second, in line with earnings failing to fall after the repeal, we observe no apparent price reduction when the policy is no longer in place. To the best of our knowledge, our price analysis presents the first evidence in the literature assessing asymmetric price responses to the minimum wage (Kline, 2025), complementing the main contribution regarding earnings asymmetry.

After documenting the main findings of persistent earnings effects from temporary minimum wage policy, in the second part of the paper we leverage this unique setting to investigate: (i) when such wage-setting hysteresis occurs, and (ii) why firms pay such high wages years after they no longer legally have to. In terms of when hysteresis occurs, we consider whether announcement or actual implementation are necessary —finding announcement without implementation is not enough, as revealed by the analysis of two additional local minimum wage increases that were repealed just before taking effect. In terms of why this phenomenon may arise, we propose a conceptual framework that highlights two main hypotheses, which we empirically investigate.

One centers on firm productivity: that firms discover the productivity benefits of higher pay during the policy period. The other focuses on worker-side expectations: that workers update their perception of "fair" wages, constraining firms' ability to reverse pay increases even after the legal constraint expires.

A group of explanations relate to the first hypothesis that firms could freely lower real pay after the policy was repealed, but they prefer not to do so. These mechanisms share an underlying theme of firm learning from forced experimentation (e.g., Bloom et al., 2013; 2015; Larcom et al., 2017).[2] Under this thinking, abiding by the higher, legally-required minimum wage taught them something about the value to them (in higher revenue/reduced costs) of paying workers more (Emanuel and Harrington, 2025), which turned out to be more beneficial than they had anticipated (Lazear, 2000). This includes mechanisms that appeal to efficiency wages (Akerlof, 1982; Akerlof and Yellen, 1990); turnover cost reduction due to greater job attachment by employees receiving higher pay (Dube et al., 2016); and, firm hiring of more productive workers when they pay a higher wage (Butschek, 2022).

In contrast, another set of explanations centers around the second hypothesis, where firms are willing to reduce pay, but then find new non-legal (norm-based) constraints that prevent them from doing so. That is, firms may not have learned anything new about how to operate more profitably from the temporary minimum wage. Rather, workers may have learned what to expect as a fair wage and this permanently changed their reservation wage and future bargaining behavior, consistent with the lab experimental findings by Falk et al. (2006). In such a case, firms continue to offer high wages after the minimum wage falls not because it otherwise increases their profits, but, because they realize that they will lose their workers, who have come to demand higher pay and more of the total surplus, should they choose a lower, now legally allowed wage. This would imply that the policy is able to achieve a sustained impact on earnings through shifting perceptions about wage-setting in the labor market, and not just through its enforcement capacity.

In order to interpret our main results and understand the source of hysteresis, we propose a simple framework. This helps highlight the main potential channels inducing hysteresis, and

[2] Bloom et al. (2013) show that externally assigned management practices led to large efficiency and quality gains at firms that had been previously left on the table because of informational barriers. These firms extended the management improvements from treated plants to their other plants, offering revealed preference evidence of their positive impact. Bloom et al. (2015) document that rapidly increasing office rental prices in Shanghai induced a large Chinese company to randomly offer the option of working from home (WFH) to some of its employees. The experiment revealed a substantial increase caused by WFH in terms of performance and employee satisfaction. As a consequence, after the experimentation period, the company decided to roll out the policy to the whole company, further increasing gains; Larcom et al. (2017) explore a totally different context - public transit commuter routes - finding that forced disruptions to the commute route lead to replacement of a commuter's sub-optimal (in terms of time spent) commute path with an improved one.

allows us to provide a simple quantification conditional on the observed facts. We then present empirical evidence specific to these channels that is in line with the results implied by the framework.

The framework features imperfect competition in the labor and product markets. Workers have idiosyncratic preferences over employers (Card et al., 2018), and can be employed in one of two sectors. A reference wage induced by minimum wage policy affects the attractiveness of jobs in the exposed sector, potentially shifting aggregate sector labor supply. At the same time, firms hire workers facing an upward sloping labor supply, and charge a markup over marginal cost. We allow firm productivity to depend on pay policy, potentially increasing when wages are higher. Given our setting, where firms face a market-level change in the wage floor, the framework highlights the joint importance of the market-level labor supply, and product demand elasticities in driving the documented effects in the exposed sector (Harasztosi and Lindner, 2019).

We apply our framework to quantify the extent to which sustained higher earnings are due to shifts in worker expectations or firm productivity. The key identification insight is simple. In equilibrium, a higher reference wage raises wages and prices, while higher productivity lowers prices. This opposite prediction lets us use the estimated joint behavior of wages, prices, and employment after the repeal to discipline the quantification exercise. As a result, interpreting the main findings through the framework, the data leave little room for a positive productivity improvement to be the dominant explanation. In contrast, the main conclusion from the exercise is that shifted worker expectations seem to be the main component explaining the hysteresis.

We next conduct several empirical tests to provide further evidence for the plausibility of the results implied by the framework. On the one hand, we assess the mechanisms related to firm productivity. To do so, we consider whether the policy may have increased efficiency via two related channels: turnover and worker quality. In terms of cost reduction via turnover, we find a small decrease after the policy is implemented, but this is quantitatively small and not statistically significant. In terms of worker composition, we investigate whether the age of employees in the exposed sector changed after the policy. We find a small shift towards slightly older individuals but, again, this is quantitatively small and cannot account for a meaningful share of the observed higher pay. Overall, our results do not support the hypothesis that firms' productivity increased substantially as a result of paying higher wages to their workers.

On the other hand, we focus on reservation wages to shed light on whether the findings highlighted above reflect a shift in workers' perceptions. We consider a direct administrative measure of reservation wages drawn from unemployment filing records and show that as minimum wages rise, reservations wages for low-income workers are positively and permanently

shocked, with no evidence of erosion over time. This evidence leverages state-level minimum wage events over two decades and thus does not correspond directly to our treated county. However, it provides a validation of the mechanism in a more general setting, further enhancing its plausibility. We find that reservation wages are permanently shifted when considering both one-time minimum wage increases, which are closer to the policy variation in our county of interest, and more common gradual minimum wage hikes.

Taken together, the findings broadly align with the hypothesis that the policy was able to shift perceptions in the labor market—even after legal incentives were no longer present—, and are difficult to reconcile with an alternative firm-productivity-based explanation.

This paper contributes to several strands of literature. First, it adds to an increasing body of evidence documenting permanent effects of temporary policies, or, hysteresis — the failure of an effect to reverse itself when its underlying cause has reversed (e.g., Blanchard and Summers, 1986; Charness and Gneezy, 2009; Giné et al., 2010; Bryan et al., 2014; Miller, 2017; Brandon et al., 2017; Ito, 2015; Costa and Gerard, 2021; Benzarti et al., 2020). In terms of hysteresis induced by labor policy, Saez et al. (2021) document *employment* hysteresis from a payroll tax cut affecting labor demand, and Goldin and Margo (1992) provide suggestive evidence that the short-lived National War Labor Board impacted wage compression in the decades after World War II. However, work showing quasi-experimental evidence on *earnings* hysteresis, as presented in this paper, is limited to contemporaneous work by Quach (2025) which documents a persistent impact on earnings from an overtime policy change that was only announced but never took effect. In contrast, this paper studies a leading policy targeted exclusively at the low-wage labor market and which took effect for more than a year, with a much longer post-repeal period during which the hysteresis is documented (including during a spell of high inflation). To our knowledge, our work represents the first quasi-experimental evidence of *earnings* hysteresis from the temporary implementation of a major labor market policy —the minimum wage. Moreover, leveraging additional natural experiments we conclude that implementation, rather than announcement alone, plays a key role in our context in causing the permanent increase (persisting more than five years after policy repeal) in low-wage worker earnings.

Second, it contributes to the literature on downward nominal wage rigidity and the phenomenon's interaction with high inflation (e.g., Keynes, 1936; Tobin, 1972; Bewley, 1999; Akerlof et al., 1996; Card and Hyslop, 1997; Schmitt-Grohé and Uribe, 2013; Dupraz et al., 2019; Kaur, 2019; Hazell and Taska, 2025). Theories of downward nominal wage rigidity would rationalize the observed absence of a wage drop upon the removal of the Johnson minimum wage because of a constraint firms face in lowering workers' nominal wages. In an inflationary environment, however, firms can freeze nominal pay in order to achieve reductions in real wages over time and

therefore reduce their costs (Tobin, 1972; Elsby, 2009). We contribute by testing empirically how the effects of the policy evolve under different inflation scenarios. And, our finding is novel. We document for the first time persistence of treated workers' wage differential (absent any legal constraint) *even in a context of high inflation*.[3]

Third, our paper relates generally to the empirical study of the labor market effects of the minimum wage (e.g., Card and Krueger, 1994; Giuliano, 2013; Cengiz et al., 2019; Derenoncourt and Montialoux, 2021; Dustmann et al., 2022; Azar et al., 2024). It also contributes to the work on labor policy compliance, which raises the question of why the minimum wage substantially affects firms' pay decisions in the first place, given that the cost of non-compliance due to legal incentives is limited (Stansbury, 2025). This aspect is relevant for high-income economies, but may be especially important for middle and low income countries with low enforcement capacity (Derenoncourt et al., 2025). Our evidence on significant long-term effects on earnings, even in an inflationary environment when wage floors should be less binding, and even when the legal incentives are completely repealed, suggests compliance may be driven by the ability of the minimum wage to shift fairness perceptions and resulting wage-setting norms. Most directly, it relates to the seminal work by Falk et al. (2006), who emphasize the role of reservation wages and fairness concerns for understanding minimum wage impacts. The authors test in the lab the effects of a temporary minimum wage of the type studied in this paper. They show that an increased lab minimum wage positively updates lab workers' reservation wage in a way that persists even after the minimum wage is removed.[4] Responding to this changed reservation wage, firms maintain a permanent increase in wages even after the wage floor falls. Our paper contributes to the literature by providing the first empirical confirmation of this finding outside the lab drawn from a quasi-experimental design that directly mirrors their lab setup. Not only do we confirm their key prediction — that profit-maximizing firms will continue to pay a much higher wage after the removal of a minimum wage than before its introduction — but in distinguishing among alternative mechanisms, we find that our evidence aligns most closely with their reservation wage hypothesis.

More specifically within the context of minimum wage policy, our work also relates to the literature on sub-minimum wages (e.g., Freeman et al., 1981; Katz and Krueger, 1992; Bockerman

[3]The 12-month percent change in the US Consumer Price Index was 2.2% in April 2017, the first month after the repeal. During the peak of the high inflation period, it reached 9.1% in June 2022. More consistent with the standard view, Kaur (2019) finds that when inflation is over 6%, prior positive shocks to wages (such as, in our case, the minimum wage increase in Johnson) do not lead to significant increases in real wages (or specifically, she "cannot reject that lagged positive shocks have no impact on current real wages"). Our results suggest otherwise.

[4]In the context of the German minimum wage — a national increase in the legal minimum — Fedorets and Shupe (2021) find partial support for a positive impact of the increase on reservation wages of job seekers, although in their case the effect seems to vanish after a few years. No reduction in the minimum wage is observed in this setting.

et al., 2024), which examines policies allowing employers to pay below the statutory minimum to a subset of the labor force, such as teenagers or students. These studies find generalized low utilization of the sub-minimum wage by employers, raising a similar question as to why profit maximizing firms pay more than legally required in such instances. We contribute by providing further insights from a case where all firms in the market are allowed to pay less to *all* workers than before the policy. Our findings reveal that the sustained earnings effect is present for both incumbent and new entrant workers, with a growth rate of nominal pay that remains approximately equal to that of unaffected workers even years after the policy was repealed. The evidence presented suggests that firms may face a constraint when trying to reverse the effects of the policy, not being able to differentiate the rate of pay *growth* relative to workers performing similar work in other regions or different tasks within the same firm. In line with existing evidence (e.g., Dube et al., 2019; Ashenfelter and Jurajda, 2022; Giupponi and Machin, 2025; Brochu et al., 2025), we view these findings as consistent with pay equity considerations both within and across employers.

The rest of the paper proceeds as follows. Section 2 explains the institutional context of the reform under study, and describes the data used. Section 3 presents the empirical methodology. Section 4 reports the main results. Section 5 develops a simple framework to interpret the main findings, and uses it to quantify the sources of hysteresis. Section 6 concludes.

2 Institutional Setting and Data

2.1 Background

Though a rarity in the United States, reversals of minimum wage policies have occurred on occasion. Before the establishment of the first federal minimum wage in 1938, some states had already enacted minimum wage policies in the early 1900s, and, were continually taken to court by opponents demanding "freedom of contract." Exceptional cases where a court struck down an active minimum wage include the District of Columbia in 1923 and New York in 1936 (Fishback and Seltzer, 2021). In more recent decades, states have had wide latitude to set their own minimum wages (above the federal rate) without interference, and, top-down reversals of minimum wages have only happened under circumstances where sub-state jurisdictions (counties or cities, led by Democrats) have set higher local minimum wages and then been challenged by a state government (led by Republicans). To our knowledge, this has happened in 6 other cases outside of Johnson County, Iowa: Kansas City MO, Lexington KY, Linn County IA, Louisville KY, St. Louis MO, and Wapello County IA. In every one of these instances, all taking place in the past decade or so, the local minimum wage increase that was revoked was either not

more than a dollar or was not allowed to be in effect for more than a few months (and sometimes for just days). The case of Johnson County, Iowa is remarkable relative to these other episodes for both the size of the minimum wage increase – allowing for a policy that actually bites – and its duration.[5]

On the 1st of November 2015, the local minimum wage in Johnson County, Iowa was raised from the federal level of $7.25 to $8.20. After that, it was further increased twice — on the 1st of May 2016 and the 1st of January 2017 — reaching a new floor of $10.10. It was repealed by the Republican-led state legislature on the 1st of April 2017, falling back to the initial level of $7.25. Figure 1 depicts the minimum wage schedule for Johnson County over this time period.[6] Given the sharp and prolonged nature of the policy change, the episode in Johnson County is the best test case for detecting any lingering effects of a minimum wage increase that was reversed and that was both originally big enough — almost $3, or, 39% above the prior state minimum wage — to substantially raise labor costs, and binding for long enough (17 months) to plausibly be perceived as permanent by firms. Press record of the repeal effort by state Republicans indicates that it was only in the last couple of months of the local policy reign (in the beginning of 2017) that the legislature began to craft a repeal, thus, making for a rather sudden and unexpected reversal.[7]

Following the reversal by the state, the Johnson County government was left without any legal power to set the minimum wage and instead resorted to publicly imploring firms to keep wages at the high (no longer legally required) Johnson schedule, and to, in the future, follow a suggested local schedule of increases. Just after the repeal, press reports indicate anecdotally that many businesses said they would keep wages at $10.10 or higher (Iowa City Press-Citizen,

[5]In these other cases of revoked minimum wage increases, there was either no detectable initial bite for low-wage workers from the minimum wage increase, a necessary precondition to study any hysteresis from the policy, or, the conditions for good identification are not met. Regarding the former, this is presumably because the increase in the minimum wage was not large enough to be binding (not more than a dollar in contrast to Johnson's $2.85 increase) or not in effect for long enough to actually be detectable in quarterly pay (e.g., 4 days in the case of Kansas City). Regarding the latter, where the (revoked) minimum wage increase was of similar size to that in Johnson County, it occurred in a state that itself raised the minimum wage not long after revocation of the local policy, thus preventing us from studying the lingering effect of the initial local policy absent another subsequent reform that would itself sustain higher wages for low-earning workers (in other words, representing a bad identification environment for our purposes, with a failure of parallel trends being observed as well). Taken together, these factors make Johnson County, Iowa, a really exceptional and ideal setting for this study.

[6]For tipped workers in Johnson County, who at baseline in 2015 had a sub-minimum wage of 60% of the level for other workers, the schedule of increases in Johnson also moved their minimum wage up dramatically and at the same rate with each step in the local increase, keeping their tipped worker wage floor at 60% of the new broader minimum wage level.

[7]The legislature sent the repeal bill, HR 295, to the Governor on March 29, 2017, and it was signed the next day, and, became effective on April 1. The bill went through a tight turnaround from introduction to passage, with it first being introduced to the legislature for discussion on Feb 14, following a subcommittee review that began on Feb 7 (the earliest legislative record of the effort).

2017).[8] Employers pointed out the ethical and practical difficulties of lowering wages after the reversal, taking away from employees the raises that they had previously obtained. One employer explained, "Once you're there, you're there. I don't see how you go back."[9]

According to media accounts, the decision by employers to keep wages high, which at first may have been motivated by such downward nominal wage rigidity concerns plus the social benefits of publicly supporting the $10.10 wage amidst public advocacy and pressure,[10] persisted in subsequent years. Some employers noted that paying even above the repealed minimum wage level of $10.10 was now crucial for staying competitive.[11] In terms of more specific rationales that inform and dovetail with the mechanisms for any sustained real wage gains that we explore in this paper, we find several articulated in the press. Some employers emphasized firm-learning type mechanisms that indicate firms discover from local minimum-wage-law-exposure that continuing with higher wages in the years that follow may actually benefit the firm. For instance, multiple reports indicate that firms see higher wages as leading to more loyal and committed workers - e.g., as one local business owner noted "keeping workers happy also keeps them around" - with few "adverse effects of the higher wage" reported by local Chamber of Commerce officials.[12] Other accounts indicate an expanded sense that workers deserve more and have raised their earnings expectations, or, reservation wage. For instance, a local county supervisor, explaining the sustained wage gains, reports that "workers have taken more control and are demanding more from their employers."[13] As one worker explained in 2022, "no one is going to work for $7.25 nowadays,"[14] echoing the sentiments of another local waiter - "if I was getting paid minimum wage, I would feel like I deserved more."[15]

[8] In this section we provide a brief summary of views expressed by employers, employees, and local officials in response to the minimum wage reversal. The reader is referred to Appendix D for a more structured collection of views as documented by local media.

[9] Travis Stovie, a local employer in the food industry, shared this perspective with The Gazette. See The Gazette (2017b).

[10] Following the initial repeal, a door-to-door canvass of some local businesses was undertaken by The Center for Worker Justice of Eastern Iowa, asking businesses to pledge to maintain the $10.10 wage and distributing signs to display in their windows to signal their commitment to "moving our community forward" (The Gazette (2017a), Iowa City Press-Citizen (2017)).

[11] That the market demands an even higher wage than $10.10 as time went on was a logic regularly employed. For instance, Chad Simmons, a local employer, shared with CBS2 Iowa CBS2 Iowa (2021): "We do not follow the minimum wage but what we do follow is the market. The market has forced us to keep competitive and pay $12 an hour to start."

[12] See the quote to CBS2 Iowa (2021) in row 10 and to Iowa City Press-Citizen (2017) in row 5 of Table D1.

[13] Johnson County Supervisor Rod Sullivan shared this perspective with the Iowa City Press-Citizen. See Iowa City Press-Citizen (2022).

[14] Grayson Lottes, an employee of a local restaurant, shared his perspective with the Iowa City Press-Citizen. See Iowa City Press-Citizen (2022).

[15] Kennedy Donovan, an employee in Iowa City. See The Daily Iowan (2024).

2.2 Data

Below we describe the data sources used that allow us to learn about different dimensions affected by the policy.

Employment and Earnings. We leverage information on employment and average weekly earnings from the Quarterly Census of Employment and Wages (QCEW) over the years 2011-2022.[16] This dataset contains a quarterly near-census of employment counts and weekly earnings at the county-industry level for employers subject to unemployment insurance laws (covering more than 95 percent of all U.S. jobs). Our main analysis focuses on an important 3-digit low-wage industry: Food Services and Drinking Places (NAICS 722), known to contain the largest share of minimum-wage-level workers (Rao and Risch, 2025).

Earnings for New Hires, Turnover, and Worker Composition. We also draw on the publicly available Quarterly Workforce Indicators (QWI) dataset, which provides near-universal coverage of private-sector jobs by linking administrative records from the Longitudinal Employer-Household Dynamics (LEHD) program. QWI reports detailed information on employment and earnings at the county-industry level. However, although this dataset provides higher disaggregation than QCEW, the information is released after a process of noise-infusion due to confidentiality protection, which effectively results in substantially lower estimate precision when zooming in on small levels of aggregation (Abowd et al., 2012). We thus primarily leverage this information to supplement our main findings by analyzing outcomes not available in QCEW. In particular, the QWI data allows us to complement the main results with additional insights on the earnings for new hires, turnover and age of workers.

Reservation Wages. In testing a proposed mechanism underlying our results, we also draw on audit data from the Benefit Accuracy Measurement (BAM) program.[17] BAM data, available at the state level, is collected by public officials to determine the accuracy of paid and denied claims in the major Unemployment Insurance (UI) programs in the US, based on surveying a small number of UI applicants. The key survey question of interest for us is one about the lowest hourly wage the applicant is willing to accept in a job: "what is the lowest rate of pay you will accept for a job?" (recorded in dollars per hour). An additional question about the respondent's normal hourly wage when working helps us to determine likely minimum-wage

[16] See the Bureau of Labor Statistics county-level QCEW files at https://www.bls.gov/cew/.
[17] For more on BAM data see e.g., Woodbury (2002), Jaimovich et al. (2020), and Skandalis et al. (2022).

affected workers: "in the last 18 months, what has been your normal wage for the work you do?" (also recorded in dollars per hour).

Restaurant Prices. In order to assess the degree to which restaurant prices are adjusted in response to the minimum wage increase and repeal, we collect an original historical restaurant price data set using a combination of the Wayback Machine and time-stamped menu images available on popular online crowd-sourced business review platforms. Only some of the restaurants in Johnson have menus stored in the 2011-2022 period of our analysis, but for those that do, we can timestamp the dish-level price. Across 75 restaurants in Johnson County, Iowa, and 228 in 40 other counties drawn from the donors used in our synthetic control analysis, we analyze price changes in 78,232 dish-restaurant-year level observations. Additional details on the price dataset can be found in Appendix E.

Additional Data Sets. We employ three additional datasets for different purposes. First, we leverage data from BLS Occupational Employment and Wage Statistics (OEWS). We use the hourly wage information on 21 major occupations at the MSA level, and focus on years 2012, 2015, 2018 and 2021 to match with treatment timing given that each annual estimate contains 3-year averages including the year in question and the two preceding years (e.g., Clemens et al., 2021). We utilize it to complement the main analysis on earnings hysteresis.

Second, with the aim of testing one of the proposed mechanisms, we utilize job posting data from Burning Glass Technologies (Burning Glass Technologies, 2018) from 2013 through mid-2018. The data covers the near universe of online U.S. job vacancy postings (culled from some 40,000 websites). Burning Glass cleans the data to remove vacancy duplicates and to extract key characteristics for each vacancy. A main advantage of this dataset is that it contains offered wage information (when firms include it as part of the job ad), and, also information on the firm — and establishments within the firm — posting each job ad. This allows us to investigate whether firms affected by a high minimum wage in Johnson, which may be positively surprised about its profitability, extend the higher pay policy to other establishments. Following our focus above, we study posted wages in the highly-exposed restaurant industry (NAICS 722).[18] When

[18]While the Burning Glass dataset is impressive in its scope, it has two limitations for our purpose. First, during much of the time period we are studying, the restaurant sector utilized online platforms to search for jobs at a lower rate than other sectors, as documented in Azar et al. (2024), making this data less representative of the overall (online and offline) job postings. Thus, if there are disparities in the volume of new postings or wage rates across online and offline sources, our conclusions will be less representative of the total. Second, within the Burning Glass posting data, the wage information during our time period is particularly sparse, being present for only a little over 4% of adverts in the industry. Still, given the millions of total adverts in the BG data this leaves us with a moderate amount of wage information (over a hundred thousand observations nationally with wage information in the restaurant sector during the time period studied, and, hundreds of individual postings with wage information

the posted wage is recorded as a range, we use the minimum of that range.

Third, we employ 2015-2017 data from L2, a leading commercial provider of consumer and voter data.[19] The L2 data provides longitudinal yearly information on all registered voters in the United States (about 70% of the population) across a variety of dimensions, many sourced from credit card spending records acquired from the credit reporting agency Experian. For our purposes, we use it to track household income changes for those workers who move out of treated and untreated counties in Iowa in service of a test of the mechanism underlying our main results. The household income estimates from L2 are derived from a "combination of expenditure patterns and other sources of reported income when available from financial transactions (e.g., loan applications)" (Raffiee et al., 2022), and as such, do not represent exact incomes.

Fourth, we utilize data on presidential vote shares in the 2012 election from McGovern (2012) to select control counties more similar to Johnson County, Iowa on this dimension, where it is an outlier in terms of Democratic Party support.

3 Empirical Approach

In order to understand the labor market effects of the temporary local minimum wage policy, we start by visually inspecting the evolution of average earnings around this unique event. Figure 2 shows raw average earnings in the restaurant sector for our treated county, Johnson, relative to a set of similar counties located in states which are just subject to the federal minimum wage. Before the policy was enacted, outcomes for both groups exhibited parallel behavior. When the first minimum wage was enacted, wages in Johnson increased relative to the rest, creating a gap that remained approximately constant until the end of our sample period. Additionally, Figure A1 compares Johnson to only two other counties in Iowa, Polk and Lee, where analogous laws to raise the local minimum wage were passed, but never went into effect after the statewide ban following the Johnson event. The comparison with these counties, while more likely to be subject to idiosyncratic shocks given the limited number of counties, provides *prima facie* an arguably good counterfactual and shows a very similar pattern. Taken together, this initial evidence suggests a clear and permanent positive effect of the policy, which we formally investigate below.

Methodology. We construct our counterfactual for what the restaurant sector in Johnson County would have been like absent the minimum wage increase by adopting a synthetic control

in a moderately sized county like Johnson).

[19] L2 data is proprietary. Further details regarding the dataset and purchase options can be found at https://www.l2-data.com/.

approach (Abadie and Gardeazabal, 2003; Abadie et al., 2010). This method utilizes a data-driven strategy to construct a control county for Johnson as a weighted average of the outcomes of non-treated counties present in the donor pool.

Formally, we adopt a potential outcomes model (Rubin, 1974). Let Y_{sct}^N be the potential outcome for sector $s \in \{e, o\}$ (exposed and other) in county $c \in \{1, ..., C\}$ and time $t \in \{1, ..., T\}$ in the absence of intervention. Let Y_{sct}^I be the potential outcome for sector s in county c and time t under the intervention. We will refer to $c = 1$ as the treated county and $s = e$ as the sector that is exposed to the policy, which receives treatment at time $t = T_0 + 1$. Outcome Y_{sct} is observed, which we normalize to 100 in $t = T_0$.[20] We posit a grouped factor model to describe the structure of the potential outcomes in the spirit of Ferman (2021) and Abadie and Vives-i Bastida (2022). There are F common factors depending on sectors s denoted by $f(s) \in \{1, ..., F\}$, and G common factors depending on counties c denoted by $g(c) \in \{1, ..., G\}$, so that we have:

$$Y_{sct}^N = \lambda_{f(s)t} + \delta_{g(c)t} + \varepsilon_{sct} \tag{1}$$

$$Y_{sct}^I = \tau_{sct} + Y_{sct}^N \tag{2}$$

where $\lambda_{f(s)t}$ and $\delta_{g(c)t}$ are common factors, ε_{sct} is an error term, and τ_{sct} is the treatment effect of the policy, which is expected to be zero for other (non-exposed) sectors of the economy in all counties, and for the exposed sector in untreated counties.

Since $Y_{sct}^I = Y_{sct}$ is observed for the treated county, we can estimate Y_{sct}^N for this county using a synthetic control estimator:

$$\hat{Y}_{sct}^N = \sum_{j=2}^{C} w_j^{sc} Y_{sct} \tag{3}$$

Following Abadie et al. (2010), given a set of weights $v_1^{sc}, \ldots, v_k^{sc}$ determining the relative importance of the k predictors (enumerated below), the set of synthetic control weights $\widehat{\mathbf{W}}^{sc} = (\hat{w}_2^{sc} \cdots \hat{w}_C^{sc})'$ is selected so that the distance between sector s in county c and its donor pool units using pre-treatment data is minimized.[21] With the appropriate weights $\widehat{\mathbf{W}}^{sc}$ selected, one

[20] This specification is equal to Wiltshire et al. (2025) in the synthetic control context, and related to Doudchenko and Imbens (2016) and Ferman and Pinto (2021). It is also similar to the one used by Harasztosi and Lindner (2019) and Saez et al. (2019) in their difference-in-differences settings, which helps interpretation and provides better fit when there is large variation in levels in the cross section.

[21] Specifically, the following expression is minimized

$$\left(\sum_{h=1}^{k} v_h^{sc} (X_{h,sc} - (w_2^{sc} X_{h,s2} + \cdots + w_C^{sc} X_{h,sC}))^2 \right)^{1/2}$$

subject to $\sum_{j=2}^{C} w_j^{sc} = 1$ and $w_j^{sc} \geq 0 \, \forall \, j \in \{2, \ldots, C\}$, where these constraints permit interpretation of the synthetic control as a weighted average of the outcome values of the counties in the donor pool. In our implementation, predictor weights v_h^{sc} are estimated using the regression based method (Kaul et al., 2022).

can then estimate the parameter $\tilde{\tau}_{ect} = Y^I_{ect} - \hat{Y}^N_{ect}$. Here, $\tilde{\tau}_{ect}$ identifies the causal effect of the minimum wage under the assumption that county-level effects, $\delta_{g(c)t}$, are well approximated by the synthetic counterfactual. However, in practice, since the counterfactual is built using only sectors in untreated counties, it may be the case that county-level shocks are unique to the treated county, and so no donor units load on the same factor as the treated unit (i.e., $g_{(1)t} \neq g_{(c)t} \; \forall \; c \in \{2, \ldots, C\}$).

To account for the possibility of contemporaneous effects that are unique to the county and common across sectors, but unrelated to the minimum wage policy, we express our estimates relative to the behavior exhibited by the non-exposed sectors within the county. This allows us to transparently assess empirically the importance of contemporaneous county-level shocks, and use the rest of the economy as a within-county control unit. We estimate the time-specific treatment effect on the exposed sector of the treated county as:

$$\hat{\tau}_{ect} = (Y^I_{ect} - \hat{Y}^N_{ect}) - (Y^I_{oct} - \hat{Y}^N_{oct}) \tag{4}$$

This approach identifies the treatment effect on the exposed sector of the treated county in the presence of contemporaneous county-level shocks that are constant across sectors. In a setting where county-level shocks arise from a factor that is unique to the treated unit, a simpler procedure using only information on the exposed sector (i.e., $\tilde{\tau}_{ect} = Y^I_{ect} - \hat{Y}^N_{ect}$) would yield a biased estimate so that $\tilde{\tau}_{ect} \neq \hat{\tau}_{ect}$.

In practice, we operationalize this approach by estimating sector-specific (restaurant and others) synthetic counterfactuals and then taking the difference between observed and synthetic outcomes across sectors in each period. We perform this procedure separately for our two outcomes of interest: earnings and employment. Conceptually, this is analogous to a triple difference estimator where the first difference-in-differences estimate compares the outcome in the minimum-wage-affected sector pre and post reform for treated vs. synthetic county, and, the triple difference comes from comparing this expression to the difference-in-differences estimate from the other non-affected sectors pre and post reform across treated and synthetic county.

Alongside our sequence of benchmark estimates $\hat{\tau}_{ect} = \tilde{\tau}_{ect} - \tilde{\tau}_{oct}$, we also report the individual estimates for both the exposed restaurant sector, $\tilde{\tau}_{ect} = Y^I_{ect} - \hat{Y}^N_{ect}$, and the other sectors, $\tilde{\tau}_{oct} = Y^I_{oct} - \hat{Y}^N_{oct}$. When it comes to the primary conclusions on earnings hysteresis, there is little divergence between the sectoral-differenced result and the result using only information on the restaurant sector.

In relation to inference, we follow the permutation approach proposed by Abadie et al. (2010; 2015), which is based on the rank of the empirical distribution of the ratio of post/pre-reform mean squared prediction error (RMSPE). Moreover, in addition to classic synthetic control

estimates, we also report bias-corrected estimates to account for discrepancies in predictor values between a treated unit and its donor pool units (Ben-Michael et al., 2021; Abadie and L'Hour, 2021), though this does not change our conclusions in any meaningful way.[22]

Sample selection and predictors. For our baseline specification, the donor pool comprises counties from states that do not require firms to pay more than the federal minimum wage (19 states besides Iowa that have not updated their minimum wage above $7.25 since the last federal change in 2009), excluding the few other counties in Iowa which had passed their own (eventually undone) local minimum wage law by early 2017 or counties which border Johnson (to exclude places possibly affected by policy spillovers). Furthermore, we restrict the donor pool to counties in these places with average pre-treatment restaurant employment between 1000 and 10000.[23] The set of donor counties is shown in the map in Figure A2.

Table 1 presents summary statistics of Johnson County, relative to those in the donor pool and all US counties. This table illustrates that Johnson is an outlier in terms of Democratic Party vote share in the 2012 presidential election. As documented in the literature (e.g., Allcott et al., 2020), counties with high Democratic Party vote share experienced more COVID-19 cases per capita relative to those with more Republican Party support, and also implemented stay-at-home policies earlier. This suggests that COVID-19 may have impacted restaurant employment heterogeneously in the later years of our analysis depending on the level of political support. We explore this in Panel (b) of Figure A2, where it is evident that higher support for the Democratic party is associated with larger restaurant employment drops during COVID-19 and afterwards. Johnson is not included in the construction of the figure but belongs to the fourth quartile of the distribution. This indicates that based on pre-reform characteristics, it was predicted to have a larger than average drop in employment, and so motivates the predictor choice to account for this fact.

Procedurally, for each outcome-sector pair, we construct a separate synthetic control. Within each sector, both outcomes — earnings and employment — are modeled using a common set of predictors from their same sector. These predictors are annual (sector-specific) averages of both earnings and employment over the pre-reform period.[24] Moreover, for all outcome-sector pairs, we also include the county Democratic Party vote share in the 2012 presidential election guided by the descriptive evidence mentioned above.

[22] For the empirical implementation, we use the `allsynth` package, version 1.2 (Wiltshire, 2025).

[23] For reference, Johnson County has a pre-treatment average restaurant employment of around 6700.

[24] For 2015, the year when the first minimum wage was enacted in the fourth quarter, the predictors are the average of both earnings and employment in the first two quarters since the third one is used for normalization.

4 Results

4.1 Main Results

The main estimates of the effect of the temporary minimum wage policy on earnings are shown in Figure 3. We aggregate the quarterly estimates into 6-month averages to obtain effects at the half-yearly level. The effects in Johnson County are presented in solid green, and the muted grey lines refer to placebo estimates where the non-treated donor counties are compared to their own synthetic versions (using the same method of selecting their respective synthetic counterfactuals as is done for Johnson).[25] For easier visual inspection, in this and later figures, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE).[26] In the year following the introduction of the minimum wage, earnings increase sharply by around 10% relative to the control unit. After removal of the legal minimum, the earnings remain elevated at roughly the same level in the years immediately following the policy reversal. These positive effects remain unaltered until the end of the sample period, surviving even through the COVID-19 pandemic, and also through the period of unusually high (9%) inflation in 2022. As shown in Table 2, considering the whole post-event period (from the first full quarter when the minimum wage was in place until the end of 2022) we find an overall positive earnings effect of 12.6% with a p-value of 0.01. Considering only the period after the policy was repealed, the estimate is of similar size and significance (at 13.3% with a p-value of 0.01).

Figure 3 presents the difference in estimated treatment effects for earnings between the restaurant sector and other sectors, as described in Section 3, accounting for concerns that the results on the restaurant sector alone may be partially driven by a confounding county-level shock that hit Johnson County subsequent to the minimum wage policy. The underlying earnings results for the restaurant sector alone as well as for the rest of the economy excluding restaurants, respectively, can be seen in Panels (a) and (b) of Figure A3. Panel (a) of the figure (restaurant sector alone) tracks very closely with the results in Figure 3. Panel (b) of the figure (other sectors in Johnson, excluding the restaurant sector) clearly does not.

Intuitively, if the evident effect seen in the restaurant sector is caused by the minimum wage policy effect on the low-earning restaurant workforce, we should not see an effect from the policy emerging over that period more broadly for higher paying sectors of the economy. Reassuringly,

[25] See Tables A1 and A2 for the specific weights and counties used to build the synthetic counterfactual for Johnson County.

[26] Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure.

Panel (b) of Figure A3 depicts a clear null effect, with earnings in other sectors of our treated county following closely those of the synthetic counterfactual. This provides evidence that in terms of earnings, the estimated effect does not reflect a more generalized higher earnings growth path in sectors not exposed to the policy, such as, for example, one caused by higher countywide inflation in Johnson. Moreover, note that the empirical design also accounts for nationwide shocks such as the post-COVID-19 tightening of the labor market, which may be more likely to affect low-wage workers (Autor et al., 2023), by estimating effects relative to other counties similarly exposed to these macroeconomic trends. Panel (a) of Table 2 also reports the earnings effects of the minimum wage policy corresponding to Figure A3, separately for the restaurant sector, and for other sectors (during the post-onset period overall, during the time the elevated minimum wage binds, and in the post-repeal period).

In relation to employment effects, the main results are depicted in Figure 4. We do not find statistically or economically significant effects overall, while the policy is in place, or during the post-repeal period. Averaging over the whole post-policy period, there is an overall insignificant employment effect of -0.45% (p-value of 0.87). Together with the estimate from Figure 3, this implies an own-wage elasticity of -0.04 (s.e. 0.73).[27] The underlying employment results for the restaurant sector alone as well as for other sectors of the economy, can be seen in Panels (c) and (d) of Figure A3, respectively. The analysis on the evolution of employment for the other sectors in Johnson County, excluding the restaurant sector, provides some evidence of a possible broad-based downward employment trend from 2019 onward in Johnson County, though, this is not statistically significant (the average effect is -6.6% with a p-value of 0.28, and only slightly larger if focusing only on the post-repeal era — see Panel (b) of Table 2). When examining restaurant employment directly (Panel (c) of Figure A3), we see a very similar qualitative and quantitative pattern to the one observed in the other sectors of the economy: the estimates exhibit a downward trend with an insignificant average effect of -7% (p-value of 0.34). In context, this statistically insignificant but negative trend seen in the restaurant sector alone is likely not due to the impact of the minimum wage policy, given the timing and, critically, the fact that the same pattern is observed in higher wage sectors not affected by the minimum wage policy. Rather, it can be better attributed to unrelated county-level shocks affecting all sectors in Johnson in the later years of our sample period.

In Figure A4, we also provide bias-corrected synthetic control versions (Ben-Michael et al., 2021) of the main results. These estimates, which account for discrepancies in predictor values

[27] As with prior work (Harasztosi and Lindner, 2019; Azar et al., 2024), we use the delta method assuming independence of earnings and employment estimates to compute the own-wage elasticity. We base the standard error for the respective earnings and employment effects, needed for this calculation, on the standard deviation of the placebo estimates underlying Figure 3 and Figure 4.

between treated and control units, deliver very similar estimates to those in Figure 3 and Figure 4, underscoring the robustness of our findings.

Earnings of New Hires Furthermore, we explore whether the uncovered effect on sustained earnings in the long term only applies to incumbents or if it is also enjoyed by newly hired employees. As Figure A5 shows, the earnings increase is also observed for employees newly hired by firms, both when considering all hires (Panel (a)) or only new hires excluding recalls (Panel (b)). However, given the higher variance of the series coming from a different dataset (QWI), explained in part by the noise-infusion procedures applied to the public data and reporting lags (see Section 2.2), the estimates' precision is more limited.[28] Nonetheless, the dynamic pattern and the magnitude of the earnings effect (9.6% - 11.2%) is closely aligned with the results on all workers obtained with the higher quality QCEW data.

Extension to All Low-Wage Occupations While restaurants are known to hire many of the lowest wage occupations affected by minimum wage increases (e.g., line cooks, waiters, busboys), there are of course other occupations outside the sector that are affected by the minimum wage as well. We now show the robustness of our findings when considering impacts across the occupation distribution. The advantage here, relative to the approach using earnings from industries, is that we can leverage information on hourly wages by occupation, which provides a direct exposure measure of the minimum wage which we use to identify groups of individuals affected by the policy. This allows us to detect effects of the policy that might otherwise be missed when studying other industries where these low-wage occupations are only a small share of total workers. Moreover, it allows us to measure hourly wages directly to complement the prior QCEW-based earnings results (which combine possible changes in hours and changes in hourly wages). The disadvantage, in comparison with the QCEW analysis, is that the occupation data we use is available only at the MSA level and comes from a survey drawn from a much smaller sample.

For this occupation analysis we turn to OEWS data, which provides specific moments of the hourly wage distribution at the year-MSA-occupation level.[29] In particular, we use the major occupation definition, which aggregates occupations into 21 mutually-exclusive groups. With the aim of exploring the impact of the policy on the most exposed occupations, we rank the

[28] Given the more noisy nature of this series, and given the previous finding that differencing with respect to non-restaurant outcomes did not matter for earnings, here we only report single differences using only information from the restaurant sector.

[29] OEWS releases yearly estimates as an average of three years. Similar approaches using OEWS occupation information have been employed in related work such as Clemens et al. (2021) and Davies et al. (2024).

occupations based on the 25th percentile of each occupation's wage distribution in Johnson County, IA. We then assign these occupation groups to employment-weighted quintiles, and estimate quintile-specific regressions where we compare Johnson's Iowa City MSA to MSAs in non-minimum wage states, which correspond to the same ones used for the analysis above.[30]

We find large and persistent hourly wage effects which are only present in the bottom quintile — the most exposed occupations — indicating the main earnings effect documented for restaurants is generally present for the broader set of exposed occupations. These include non-restaurant occupations, and occupations not subject to tips, underscoring that the main results are not specific to the restaurant sector, are not driven by changes in hours, and also arise when tips are not part of the remuneration package. Figure A7 shows the impact of the minimum wage across the occupational distribution.[31] In Appendix F, we consider different policy exposure measures to understand the dynamic evolution of the effect. Columns (1)-(4) of Table A3 show the persistence of the wage increase in response to the minimum wage long after the policy has been revoked.

4.2 Additional Results

To complement our main findings, we proceed to examine the effect of the minimum wage increase and repeal on restaurant prices. This is a key firm margin of adjustment to raised labor costs, potentially providing information on how the sustained earnings for restaurant workers documented above can be financed after the reform. These conclusions are more tentative for two main reasons: (i) the dish-level price data are not as comprehensive as the earnings and employment data, being drawn from only 75 restaurants in Johnson and 228 outside of it, limiting the precision of our estimates; and (ii) the data may not be fully representative given that the collected prices represent the information that was available with our retrospective data collection procedure (see Section 2.2). Nonetheless, the novel database constructed offers valuable insights into price-setting behavior. We estimate the following regression equation:

$$p_{irt} = \sum_{\tau=2011, \tau \neq 2015}^{\tau=2022} \beta_\tau \cdot D_r \cdot \mathbb{1}[t = \tau] + \gamma_{ir} + \lambda_t + \epsilon_{irt} \tag{5}$$

[30] For this exercise, we estimate the following regression equation separately for each quintile j:

$$y_{omt} = \beta_1^j \cdot D_m \cdot \mathbb{1}[t > 2015] + \gamma_o^j + \rho_m^j + \lambda_t^j + \epsilon_{omt}$$

where y_{omt} refers to mean log hourly wage in occupation o, MSA region m and year t, D_m is a dummy taking value 1 for Iowa City, IA, $\mathbb{1}[\cdot]$ is an indicator function, γ_o are occupation fixed effects, ρ_m are MSA fixed effects and λ_t are year fixed effects. Each specification is weighted by average MSA-occupation pre-reform employment.

[31] The most exposed (bottom) quintile includes not only restaurant-based "food preparation and serving related" occupations, but also includes "personal care and service" and "sales and related" occupations.

where p_{irt} refers to the log price of dish i offered at restaurant r in year t, D_r is a dummy taking value 1 when the restaurant is in Johnson, $\mathbb{1}[\cdot]$ is an indicator function, and γ_{ir} and λ_t refer to dish-by-restaurant and year fixed effects, respectively.

Note that this specification leverages within restaurant-dish variation, and so even if the sample is not representative in terms of price levels due to varying availability of dishes in the online platform, the estimated effect on price changes can be informative of broader responses if the dishes sampled exhibit representative time variation in price-setting behavior.

Figure 5 reports the results. There are several takeaways. First, there is no evidence of an immediate price change upon the minimum wage increase. Second, a price increase of about 5% — consistent with more than full pass-through — does appear to manifest gradually, with firms taking time to adjust their prices. The average point estimate in the overall period since the minimum wage introduction is 0.042 (s.e. 0.007). Looking at the period from 2018 onward, when the price effect stabilizes, the estimate is 0.055 (s.e. 0.008).[32] This estimate, coupled with existing estimates from the literature for labor's marginal cost share in restaurants (Ashenfelter and Jurajda, 2022) and our own average earnings elasticity estimate, is consistent with full pass-through of the initial costs of the minimum wage increase to consumer prices, though, the level of precision allows for a wider range of pass-through estimates. Finally, consistent with the persistent earnings effect documented earlier, Figure 5 suggests the repeal of the minimum wage does not seem to result in a price decrease either, even a delayed one, as point estimates continue to hover around 5% in the following years.[33] Thus, in summary, we observe an asymmetric price response to the minimum wage — the first such evidence of this kind (Kline, 2025).In the next section we use a simple framework and additional evidence to study the forces that may underlie our hysteresis results.

5 Mechanisms

When does earnings hysteresis occur? Why do firms continue to pay much higher wages to workers after they are no longer legally required to do so? The current section explores these questions combining empirics and a conceptual framework.

[32] Anecdotal evidence appears in line with the positive pass-through result. For example, Des Moines Register (2016) documents that Brian Rorris, co-owner of Quinton's Bar and Deli, which enacted small price increases stated that "You got to adapt" referring to the recent minimum wage increase.

[33] While online menus may be updated with some lag relative to in-store pricing, possibly explaining the finding of a delay in the initial price increase upon policy implementation, allowing for a similar-sized delay for online menu updates upon revocation would put us in the 2018-2019 period and there is clearly no change around those years.

5.1 When Does Earnings Hysteresis Occur?

In terms of when temporary labor policies give rise to wage effects that are persistent over time, the evidence in the literature is scant. Recent work by Quach (2025) suggests that in the case of temporary overtime policies, an important characteristic of the event is that the law is passed and announced, with no actual implementation needed to induce earnings hysteresis. However, it is not clear whether that finding applies to other important labor market policies, whether it depends on the part of the wage distribution affected, or whether inflationary environments may affect the results.

To shed light on the importance of different policy characteristics for the earnings hysteresis outcome, we analyze two additional natural experiments. At the same time Johnson County saw its local minimum wage repealed, the state of Iowa banned all other counties in the state from enacting their own minimum wage. This resulted in two other Iowa counties, Polk and Lee, not being able to experience the higher minimum wage laws they had passed (and had announced would take effect shortly after passage).[34]

These two events, very similar otherwise to the event in Johnson regarding the specific labor market policy and economic environment, allow us to better understand the importance of announcement relative to actual implementation, ceteris paribus. To that end, we employ QCEW data and a synthetic control approach as explained in Section 3 to estimate the impact of passing and announcing a local minimum wage law increase without its actual implementation. We estimate separately the effect of the policy for each event, and obtain an average estimate by aggregating using pre-reform county average employment in the restaurant sector.

The results are depicted in Figure A8, which shows that the aborted minimum wage policy had an average effect on earnings close to zero (-1.88%, p-value 0.70) —where no effect is apparent around the time when the law should have been implemented or long term. This finding highlights that, in the context of the low-wage labor market, a major labor market policy —the minimum wage— is only able to induce hysteresis when the reform is actually implemented and both firms and workers experience the consequences of raising worker pay. The null effect for an announced but never implemented policy stands in contrast to the findings of Quach (2025), who documents persistent earnings effects from a proposed change in temporary overtime rules that did not go into effect. Beyond the specific labor policy, there is an obvious distinction between the two contexts: the affected workers in our context are the lowest paid wage workers in the

[34]Polk County, Iowa, passed a minimum wage increase (from $7.25 to $10.75) in the fourth quarter of 2016, with the first step in the approved multi-step schedule to take effect in the second quarter of 2017. Lee County, Iowa, passed a minimum wage increase (from $7.25 to $8.20) in the first quarter of 2017 scheduled to take effect in the second quarter of 2017. The state ban on sub-state minimum wages, taking effect on April 1, 2017, preempted any increase in the minimum wage in either county.

economy whereas in Quach (2025) the focus is on salaried workers carrying out white-collar tasks. Moreover, given the institutional details in the case of the overtime policy change, firms had incentives to avoid monitoring costs and implement organizational changes already before the policy takes place, which led to bosses making promises about higher future pay beforehand. Although the setting analyzed by Quach (2025) does not allow one to disentangle whether announcement or pre-reform firm commitments drove the earnings effect, the null results found in our setting —where no incentives are present to act before the policy takes place— indicate that it is more likely that wage promises were the main firm constraint responsible for the earnings increase in this other study.

In summary, shifting pay persistently at the bottom of the wage distribution may require more than just announcement, underscoring the special value of this unique setting to better understand earnings hysteresis from labor market policy. Given the importance of actual implementation for minimum wage policy, we next turn to investigating why experiencing such a policy taking place may induce persistent earning gains.

5.2 A Simple Framework for Interpreting Hysteresis

The evidence in the previous section suggests that announcement alone does not leave behind a persistent effect on pay. What seems to matter is actual exposure to a market-wide wage floor. That observation points to an underlying force induced by the policy that remains in place afterwards, which we aim to identify.

To understand why hysteresis may occur, we use a simple framework to interpret the three post-repeal data moments documented above: wages and prices remain elevated, and employment remains unchanged. The aim is to use the framework to provide a quantification of the relative importance of the different forces behind the hysteresis, conditional on the observed empirical facts. A brief description to build intuition is presented here. The reader is referred to Appendix B for the details and derivation of the developed framework.

Workers. The key object on the worker side is a reference wage, m^e, which summarizes the wage expectation workers in the exposed sector have for exposed jobs after experiencing the temporary minimum wage. An important insight is that this component operates at the sector level. Workers have idiosyncratic preferences over employers (Card et al., 2018), and their decision on whether to work in the exposed sector depends on a sector wage index $\mathcal{W}^e$ which is evaluated relative to the reference wage m^e. Aggregate labor supply at the sector level is governed by a market-level elasticity θ. A temporary market-wide wage floor can therefore

change what restaurant workers view as acceptable pay in the sector as a whole, even once the legal floor disappears.

Firms. On the production side, atomistic firms post wages and hire labor while taking sector conditions as given. Productivity is summarized by z, and labor's share in marginal cost by s_L. In this setting, a firm-side force that makes production more efficient can sustain a given wage with less pressure on prices. By contrast, if labor costs remain high without offsetting productivity gains, prices must absorb part of that pressure.

Product demand. On the product-market side, the exposed sector faces a market-level elasticity ε. Because the policy affects the exposed sector as a whole, the relevant object is substitution between the exposed sector and the rest of the economy. In the benchmark version discussed here, demand is held fixed, so ε governs how wages and prices map into employment.

Equilibrium. In the baseline equilibrium, the two underlying objects m^e and z jointly determine wages, prices, and employment. A persistent increase in m^e pushes labor supply inward. A rise in z makes production cheaper. These ingredients can be summarized with three expressions that are sufficient for the empirical interpretation in the main text.

The first expression comes from the worker-side block. If the policy permanently shifts what workers view as an acceptable wage, then labor supplied to the sector depends on pay relative to that reference point:

$$n = A\left(\frac{w}{m^e}\right)^{\theta}, \tag{6}$$

where n is employment in the exposed sector, w is the sector wage, A is a scale term, and $\theta > 0$ governs how sectoral labor supply responds to pay relative to the reference wage. This expression summarizes sector-entry condition.[35] In that environment, a higher m^e shifts sectoral labor supply inward, so firms must continue to pay more in order to attract and retain workers after repeal.

The second expression comes from the firm side. Let p denote the price of the exposed sector relative to the county-wide price level. Then prices depend on wages and productivity according to

$$\Delta \log p = s_L \Delta \log w - \Delta \log z. \tag{7}$$

[35] Appendix B makes clear that the relevant elasticity here is not merely substitution across restaurants within the county, but the elasticity governing how many workers are willing to work in the exposed sector at all relative to outside options.

A productivity improvement therefore lowers prices for a given wage, while higher wages raise prices only to the extent that labor enters marginal cost. This equation also makes clear that labor need not be the only input in production. The labor share s_L governs how strongly higher wages feed into prices, while z captures firm-side forces that offset labor costs, including true productivity gains and other cost-reducing adjustments.

The third expression comes from the demand side and determines how employment responds to wages and prices:

$$\Delta \log n = (1 - \varepsilon)\Delta \log p - \Delta \log w, \tag{8}$$

where the response is guided by the market-level elasticity of demand ε since all exposed firms face a common wage shock.

5.3 Quantifying the Sources of Hysteresis

We next use the key expressions in the framework to quantify the relative importance of worker-side expectations and productivity. We start by considering the benchmark when demand is held fixed. Appendix B shows that, in a version of the model where labor accounts for a share s_L of marginal cost, wage growth between pre- and post- minimum wage policy can be decomposed as

$$\Delta \log w = \frac{\theta}{D}\Delta \log m^e + \frac{\varepsilon - 1}{D}\Delta \log z, \tag{9}$$

where $D \equiv \theta + 1 - s_L + s_L\varepsilon$. We calibrate this expression using the post-repeal earnings and price effects from Section 4, namely $\Delta \log w = \log(1.126)$ and $\Delta \log p = \log(1.055)$. Following the evidence in the literature (e.g., Harasztosi and Lindner (2019); Ashenfelter and Jurajda (2022)), we set $s_L = 1/3$, consider $\varepsilon \in \{0.05, 0.10\}$ and $\theta \in \{0.5, 1\}$, and evaluate two employment targets: no change and a decline of around 5 log points.

We then relax the fixed-demand restriction and allow for a reduced-form demand shifter, B. This richer specification helps the framework to better match the employment response. In that case, wage growth can be decomposed as

$$\Delta \log w = \frac{1}{D}\Delta \log B + \frac{\theta}{D}\Delta \log m^e + \frac{\varepsilon - 1}{D}\Delta \log z. \tag{10}$$

Results. When labor corresponds to one-third of marginal cost, the observed wage-price pair implies $\Delta \log z = \frac{1}{3}\Delta \log w - \Delta \log p \approx -0.014$. In other words, the data leave little room for a positive productivity improvement to be the source of wage hysteresis. Given the framework and the facts, productivity must be either roughly unchanged or slightly lower, but not higher.

The parsimonious benchmark is in Panel A of Table 3. If the quality-demand shifter is held fixed, the model attributes roughly 90 to 94 percent of wage hysteresis to norms, with the remainder coming from the fact that fitted productivity is slightly negative rather than positive. The cost of that parsimony is that the model predicts an employment decline of about 0.068 to 0.070 in log terms. One possibility is that, given the uncertainty around the near-zero point estimate we document in the main text, the true employment effect could be modestly negative. In that scenario, the norm-dominant decomposition in Panel A is the most natural benchmark.

An alternative interpretation is considered in Panel B of Table 3. If employment is taken to be exactly unchanged, the model needs a sizeable positive demand shifter, about 0.068 to 0.070 in log terms, to offset the employment decline that the cost side would otherwise generate. In that case, norms still matter considerably, but quality-adjusted demand also becomes quantitatively important. However, if the true employment effect is even moderately negative, e.g. around 0.05 in log terms, the required demand shifter becomes small, only about 0.017 to 0.019, and the decomposition moves back toward a largely norm-driven account of wage hysteresis.

Taken together, the overall result is consistent across different implementation assumptions: the wage-price facts strongly support a persistent norm or reservation-wage shift, while positive productivity does not seem a plausible explanation for the observed post-repeal combination of wages and prices.

5.4 Empirical Evidence on the Sources of Hysteresis

The framework narrows the set of plausible explanations, implying a leading role for worker side norms rather than firm productivity, given the set of facts observed. We now provide additional pieces of empirical evidence on the different components, which are in line with the predictions above.

Reservation Wages of UI Recipients. To our knowledge, the only data source with a measure of wage expectations for low-wage workers — and with sufficient temporal and geographic coverage to study the effects of minimum wage shocks on these expectations *directly* — is the Benefits Accuracy Measurement (BAM) data from the Unemployment Insurance program. The survey that underlies BAM data asks UI applicants for the lowest hourly wage that they would accept at a job. This serves as the outcome in our direct test of the reservation wage mechanism. Unfortunately, in the available BAM data, a respondent's county is not recorded (instead only the state is).[36] So, we do the next best thing and test the prediction of the mechanism out of sample,

[36] Even were county information shared, there are too few audited workers — less than 500 a year in all of Iowa, and, only a fraction of them low-wage — to provide sufficient coverage in a medium-size county like Johnson

exploring whether there is a general empirical link between public policy and reservation wages at the state level over the last two decades.

Specifically, we look at sizable state-level minimum wage changes that took place across the United States and use an event-study design to test whether they have a positive effect on the reported reservation wages of minimum-wage level respondents. For the analysis, we utilize the prominent state-level minimum wage increase events identified in Dube and Lindner (2024).[37] We further restrict the set of events studied to one-time increases in the minimum wage which are not followed by subsequent increases, since this is the most similar source of policy variation in relation to our main experiment, and where wage erosion is more likely to occur in case it takes place.[38] To focus on individuals that are highly exposed to the minimum wage policy, we restrict the sample to the bottom 5% of UI applicants in the state-year hourly wage distribution.[39] We implement a stacked design (Cengiz et al., 2019) for the event-study analysis.[40]

The results leveraging the one-time minimum wage events are presented in Figure 6. Panel (a) presents the effect of the prominent minimum wage increase events on the reservation wage for minimum wage individuals.[41] Event estimates are close to zero prior to the reform, implying that the outcomes in treated and control states exhibited parallel trends. After the minimum wage is increased, the estimated impact is positive and statistically significant, stabilizing at $t+1$ which is when the gap in the minimum wage stabilizes. This heightened reservation wage then goes on to persist for the additional 4-year estimation period.[42] Panel (b) of Figure 6 plots the gap in

County, Iowa.

[37]To be included in the set of events, the state minimum wage increase must have been both at least 25 cents and must have represented at least a 5% increase above the existing minimum wage while taking place in a year without a federal minimum wage increase; control states used for each event are those that have no state minimum wage increase in the three years preceding the event and no state minimum wage increase in the post-period. The events studied also are selected to have clean pre-event periods without other legislated state minimum wage increases taking place. See Appendix E of Dube and Lindner (2024) for the full set of restrictions involved in the selection of a total of the 60 sizable state-level minimum wage increase events. Given the BAM data we have used for our primary outcome begins in 2002, only 34 of these events are used in the initial analysis.

[38]We also complement this exercise by leveraging the full set of state-level events including more gradual, multi-stepped minimum wage hikes, with similar findings as described below.

[39]Specifically, low-wage respondents are identified by their answer to the BAM survey question asking about their usual hourly wage (see Section 2.2).

[40]We estimate a regression equation of the following form:

$$y_{hst} = \sum_{\tau=-3,\tau\neq-1}^{\tau=4} \beta_\tau \cdot D_{hs} \cdot \mathbb{1}[t=\tau] + \mu_{hs} + \rho_{ht} + u_{hst} \tag{11}$$

where y_{hst} corresponds to the outcome (log minimum wage or log reservation wage) in event-dataset h, state s and event-time t; D_{hs} is a dummy taking value 1 for state s being treated in event-dataset h, $\mathbb{1}[\cdot]$ is an indicator function, and μ_{hs} and $\rho_{h\tau}$ refer to event-dataset-state and event-dataset-time fixed effects, respectively. This design uses only within event variation, which prevents negative weighting issues highlighted in the recent difference-in-differences literature (e.g., Callaway and Sant'Anna, 2021; Borusyak et al., 2022).

[41]For this specification, we leverage 6 events.

[42]Note this finding is different than that in Fedorets and Shupe (2021). In that work, the authors find a similar

the statutory minimum wage between treatment and control states over time, which depicts the one-time pattern described above, and mirrors the dynamics observed for reservation wages.[43] In addition, Appendix C also reports evidence using movers in the L2 data. Low-income workers who leave Johnson after exposure to the policy experience larger income gains than comparable movers from other Iowa counties, while high-income movers do not. This pattern is consistent with treated workers carrying higher wage expectations with them, rather than the effect being tied only to firms located in Johnson.

Together, these findings show a clear positive link between the minimum wage and reservation wages, highlighting the strong empirical validity of the mechanism.

Turnover and Worker Composition. We investigate whether the results might be explained by a persistent productivity improvement inside treated firms in Appendix Figure A6. This analysis uses the Quarterly Workforce Indicators (QWI), which we leverage because it provides labor-market outcomes not available in QCEW. The empirical design is the same as for the main results above.

Panel (a) of Appendix Figure A6 plots the turnover rate. If the temporary minimum wage generated persistent efficiency gains by making jobs substantially more stable, one would expect turnover to decline even after repeal. However, the results indicate only a small and statistically insignificant effect. Panel (b) plots the average age of employees, a proxy used for investigating changes in worker quality as a result of the policy. We detect a small shift in the age of individuals employed in the restaurant sector. However, this is quantitatively small, and cannot account for a large share of the main earnings increase as shown in Panel (c), which depicts age-predicted earnings. Across the three panels, we find little evidence of the type of sustained compositional upgrading consistent with the productivity-based story. This weakens the case that post-repeal wage persistence is being driven primarily by productivity gains generated within firms.[44]

Taken together, the findings are difficult to reconcile with a firm-productivity-based explanation. In contrast, they broadly align with the hypothesis that the policy did not induce long-term

positive shock to reservation wages (in Germany) following a minimum wage increase, but, unlike here, the reservation wage (in real terms) returns to pre-shock level after 3 years. Our results demonstrate clear persistence of elevated real reservation wages with no decline after 5 years post reform.

[43] Leveraging the full set of gradual minimum wage changes, the results show a similar strong relationship between the minimum wage and reservation wages. Panel (a) of Figure A9 presents the estimated impact on reservation wages, which is positive and statistically significant, increasing over time. Panel (b) of Figure A9 plots the gap in the minimum wage between treatment and control states over time, indicating why this increase takes place over time: the initial minimum wage increase events analyzed tend to be followed by multiple subsequent increases.

[44] Appendix C reports additional evidence from Burning Glass job postings. Firms that were exposed to the Johnson policy do not appear to raise posted wages at their untreated establishments outside Johnson relative to other firms. That result is difficult to square with a firm-learning explanation in which the policy teaches employers that paying more is generally profitable and should be propagated across establishments.

effects via legal enforcement alone, but by being able to shift perceptions in the labor market.

6 Conclusion

This paper studies the effects of a unique natural experiment: a large increase in the minimum wage followed by an unexpected reversal with a correspondingly large drop in the wage floor. Our main finding is that a relatively transitory policy footprint can cast a very long shadow, with workers experiencing permanent earnings gains five years after the policy ends. This offers novel evidence of earnings hysteresis resulting from temporary labor market policy. While theories of downward nominal wage rigidity may account for the lack of wage reversal after high pay is no longer legally required, such theories predict that inflationary episodes should serve to reduce the pay gap relative to unaffected workers over time. Notably, this is not the case. The wage gains persist even in a period of generalized high inflation, with no sign of a negative real wage adjustment.

Our evidence indicates that the minimum wage was able to shift workers' reservation wages, and through that have a persistent effect on earnings, consistent with the seminal work by Falk et al. (2006). This underscores the ability of public policies to shape expectations, achieving a lasting impact on the labor market through altering wage-setting norms — rather than legal incentives alone. This suggests that enforcement may be less of a binding constraint than typically assumed, helping to explain the broad impact of minimum wage policies on workers' pay, even in contexts where formal enforcement is weak (Stansbury, 2025). Importantly, these insights apply not only to advanced economies, but also to developing countries where enforcement capacity may be more limited (Derenoncourt et al., 2025).

More broadly, our findings provide a greater understanding of the dynamic nature of wage-setting behavior, indicating that equilibrium wages depend on the history of policy changes and not only on current policy — even if it undoes past reforms. For policymakers, especially those operating in environments with divided governments and uncertain policy survival, the implication of these results is also clear: even transitory labor market reforms can achieve earnings gains for workers that last long after the policy lifetime. In other words, if one has the policymaking reins for the moment, play it if you've got it.

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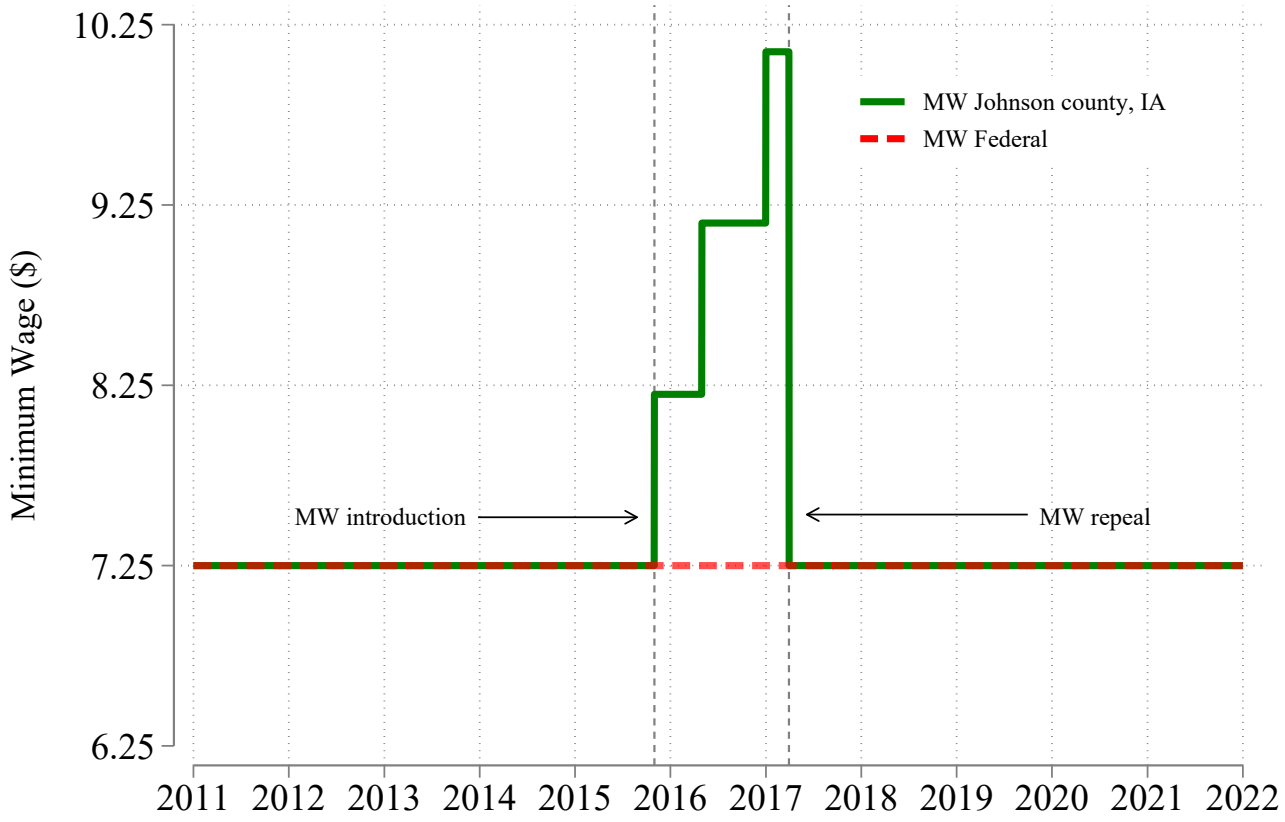
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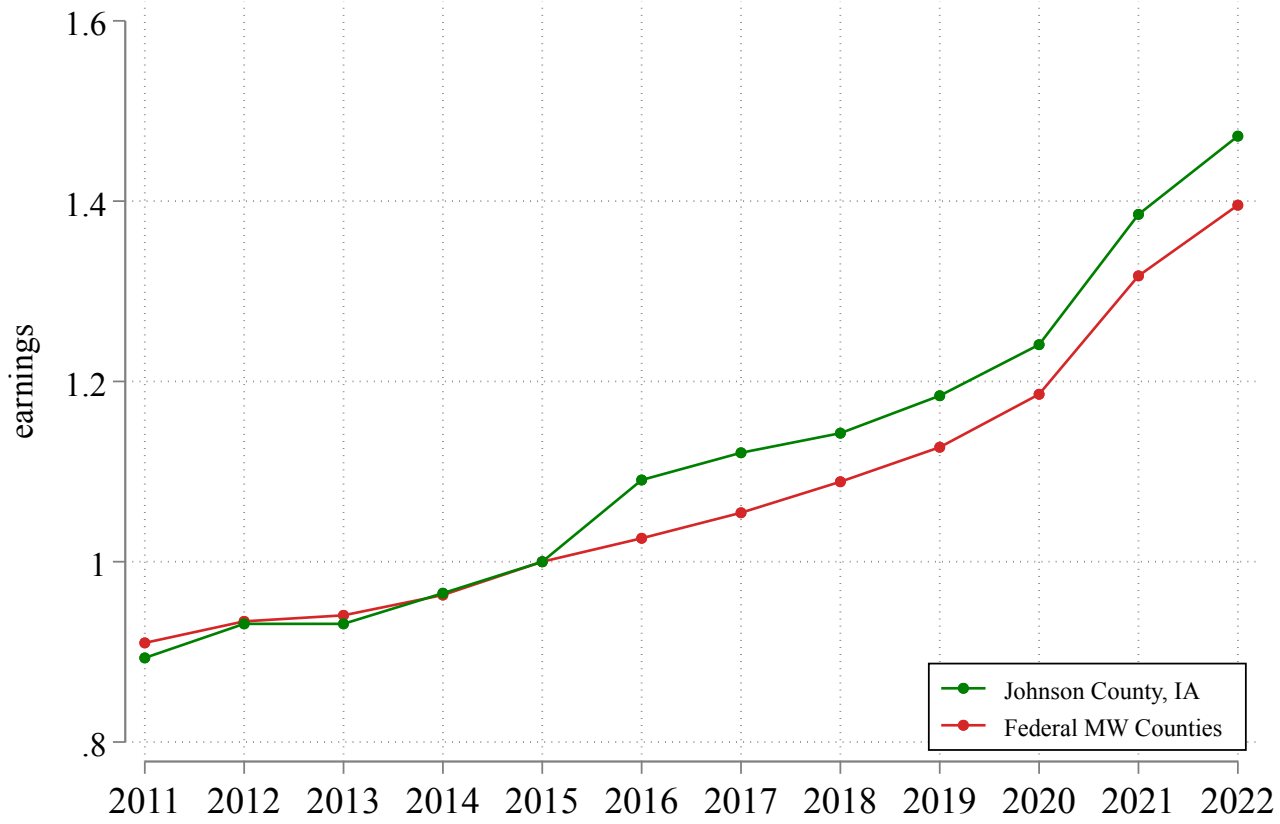
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Figure 1: Minimum Wage in Johnson County, Iowa



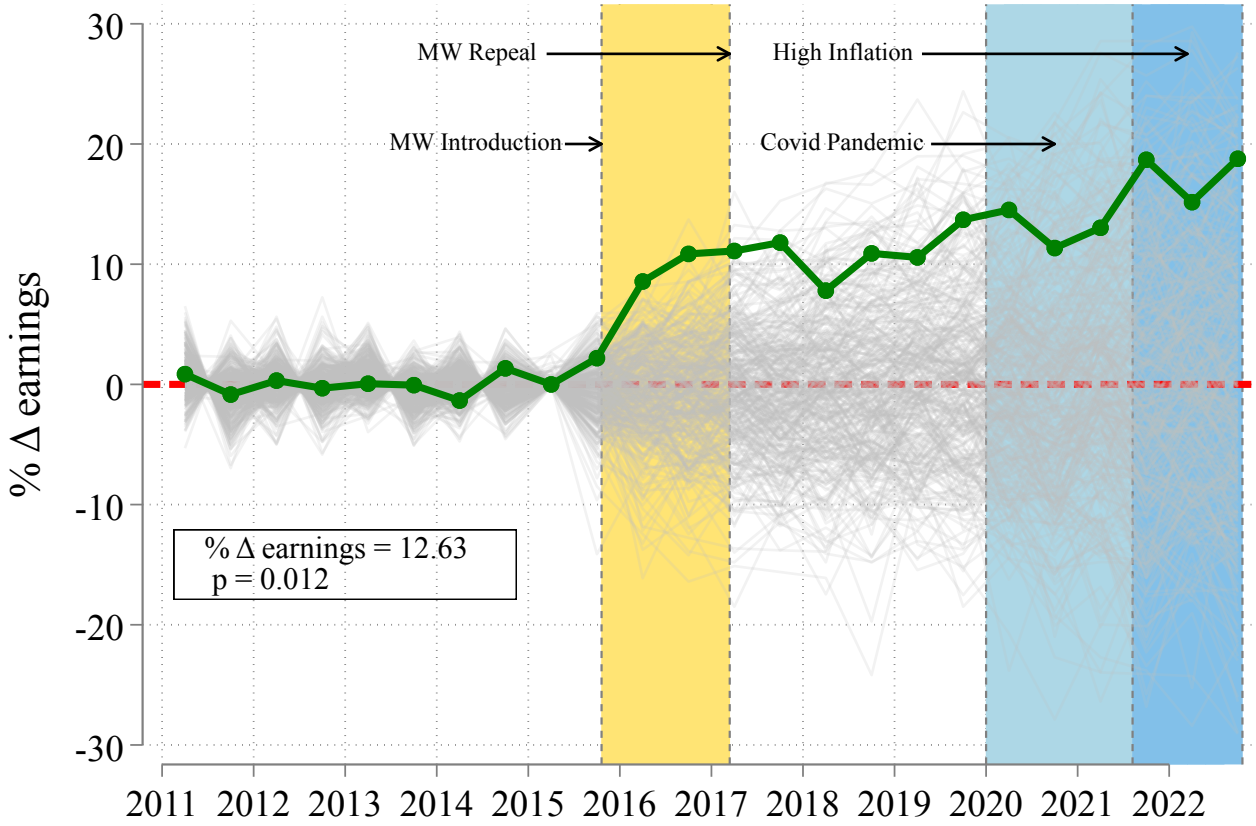
Notes: The graph shows the recent evolution of the statutory minimum wage in Johnson county, IA (green line) relative to the federal minimum wage (red line). The elevated Johnson minimum wage was first introduced on the 1st of November 2015, and then increased twice: on the 1st of May 2016 and the 1st of January 2017. It was repealed by the state on the 1st of April 2017, going back to the initial level binding for Iowa, the federal minimum wage.

Figure 2: Average Restaurant Earnings Around Change in Minimum Wage



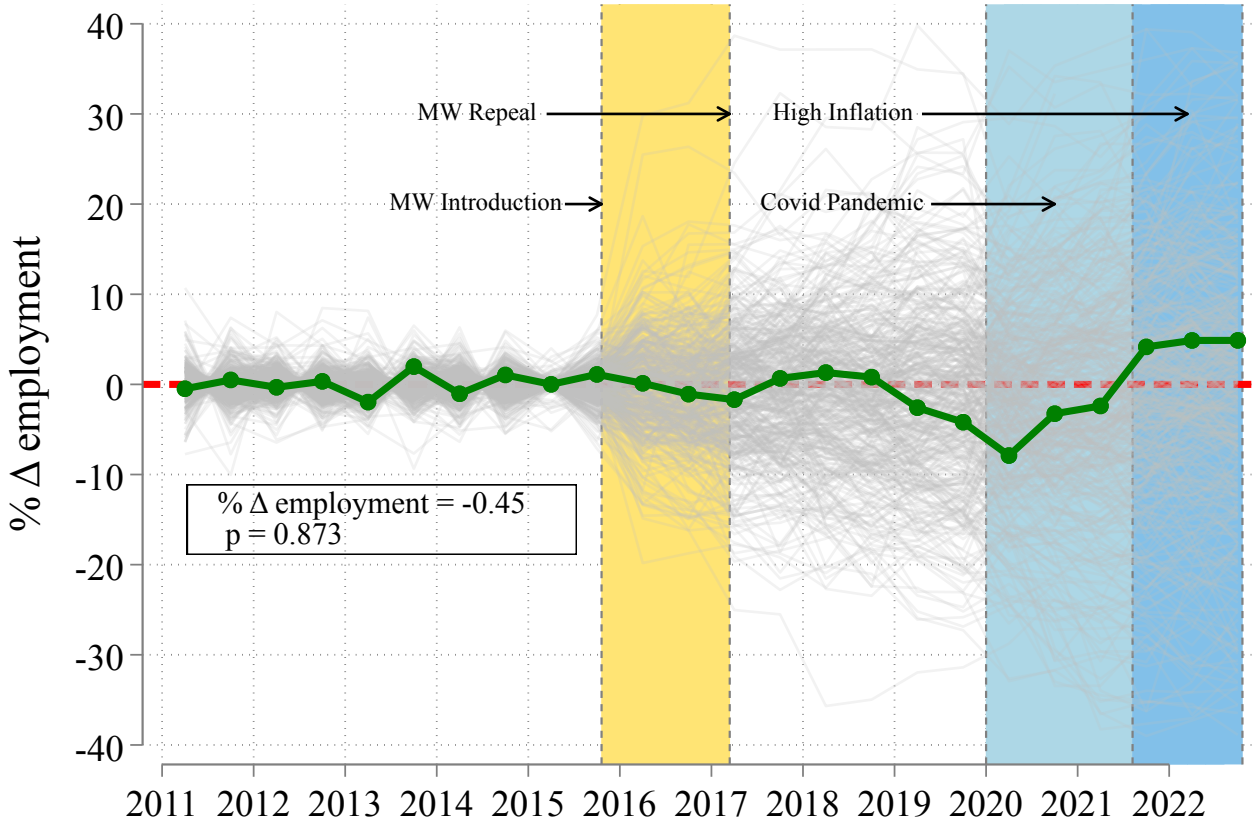
Notes: The graph shows the evolution of average earnings in the restaurant sector (NAICS 722) around the timing of the large, unexpectedly temporary minimum wage policy implemented in Johnson County, Iowa (this minimum wage schedule is depicted in Figure 1). The green line represents the average earnings in the sector in Johnson. The red line refers to the employment-weighted average earnings in the sector for the rest of the counties located in states where the maximum minimum wage is the federal one and whose average pre-2016 restaurant employment is between 1000 and 10000 workers (following the restrictions used in our main synthetic control analysis). The data used come from the Quarterly Census of Employment and Wages (QCEW). Raw averages are normalized to one in the year prior to the first full year of the minimum wage increase.

Figure 3: Effect on Earnings



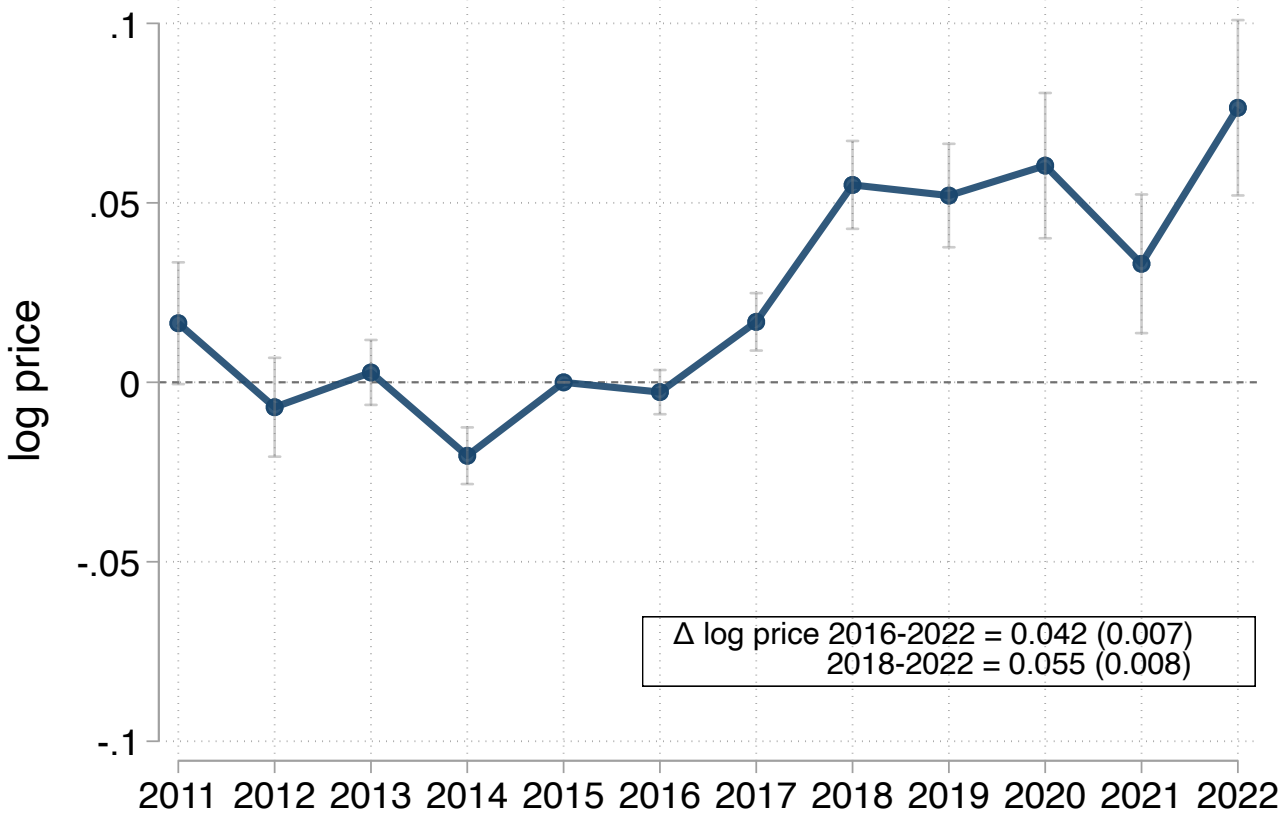
Notes: The graph shows the impact of temporary minimum wage policy on restaurant earnings in Johnson County, Iowa. The green line depicts the relative gap in average earnings in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (coefficients $\hat{\tau}_{ect}$ in Section 3). Gray lines refer to placebo estimates, where we plot the relative gap in average earnings for non-treated units relative to their synthetic version. For easier visual inspection, in this and later figures, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Census of Employment and Wages (QCEW). The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The two blue areas correspond to the COVID-19 recession and the high inflation period. The outcomes are aggregated in six-month time periods from quarterly frequency. Panels (a) and (b) of Figure A3 plot the estimates that underlie this figure as described in the main text.

Figure 4: Effect on Employment



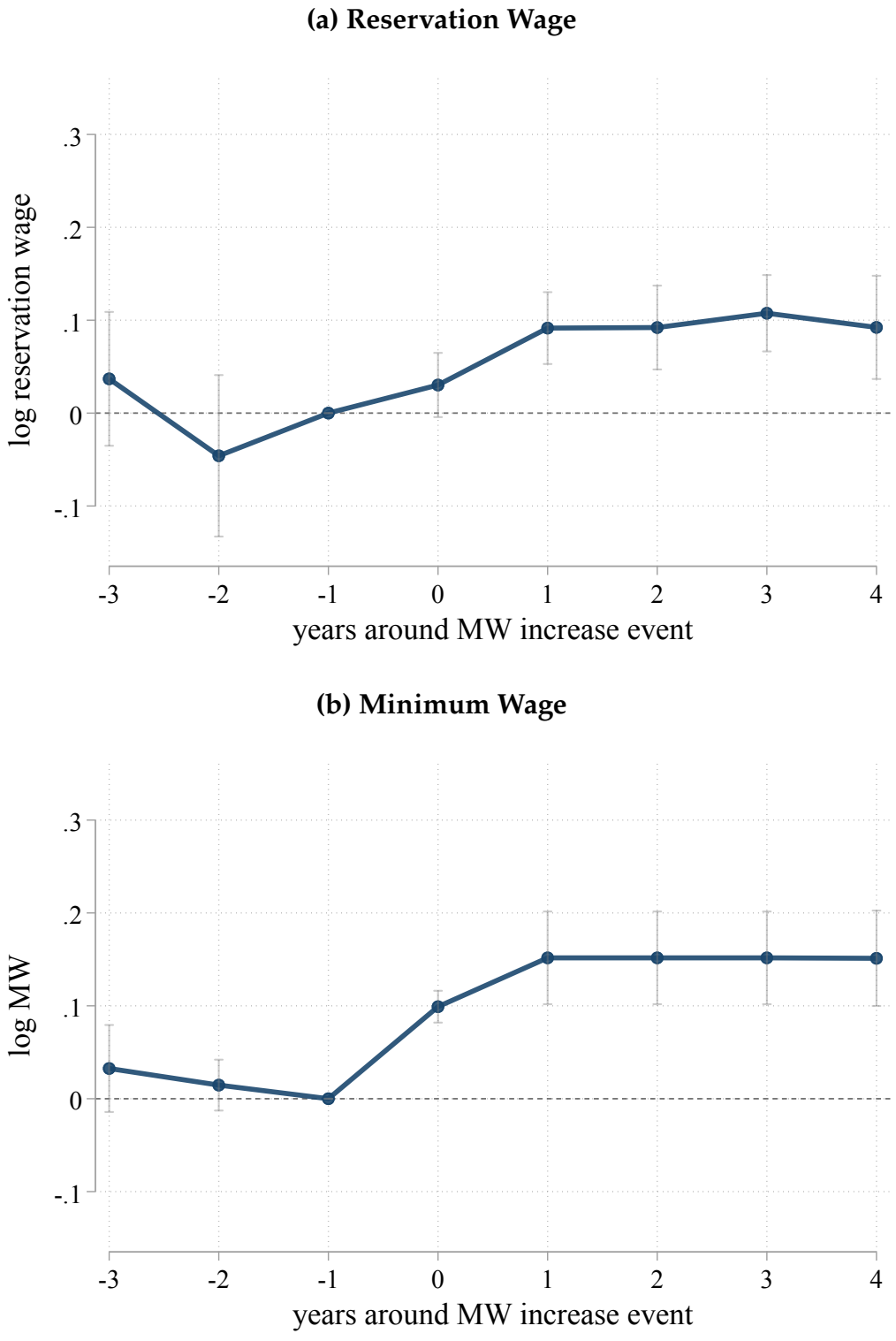
Notes: The graph shows the impact of temporary minimum wage policy on restaurant employment in Johnson County, Iowa. The green line depicts the relative gap in average employment in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (coefficients $\hat{\tau}_{ect}$ in Section 3). Gray lines refer to placebo estimates, where we plot the relative gap in average employment for non-treated units relative to their synthetic version. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Census of Employment and Wages (QCEW). The yellow area corresponds to the period where employers were legally obliged to pay above federal minimum wage. The two blue areas correspond to the COVID-19 recession and the high inflation period. The outcomes are aggregated in six-month time periods from quarterly frequency. Panels (c) and (d) of Figure A3 plot the estimates that underlie this figure as described in the main text.

Figure 5: Effect on Restaurant Prices



Notes: The figure shows the impact of a temporary minimum wage policy on restaurant prices. The dark blue line depicts the dynamic evolution of restaurant prices in Johnson County relative to 40 other control counties, obtained from estimating equation (5). The data comes from a newly collected database of dish-level prices obtained by digitizing restaurant menus from online platforms. 95 % confidence intervals from standard errors clustered at the county level are depicted in gray. In the legend, the average estimate (with standard errors in parentheses) across the post-event years is presented as is the average estimate post-stabilization (starting in 2018).

Figure 6: Reservation Wage Mechanism



Notes: The figure shows the effect of an increase in the minimum wage on the self-reported reservation wage of low-wage workers, in Panel (a). Panel (b) demonstrates the changes in the statutory minimum wage (between treated and control states) that constitute the shocks used for identification in Panel (a). These minimum wage increase events are drawn from a subset of prominent state-level minimum wage increase events catalogued in Appendix E of Dube and Lindner (2024), selected here to generate a relatively stable average post-event gap between minimum wages in the treated and control locales (within the time period for which we have outcome data on the reservation wage for UI applicants). Results come from a stacked event-study design (Cengiz et al., 2019). See the main text for further details. The data used come from the Unemployment Insurance Benefit Accuracy Measurement (BAM) program. 95 % confidence intervals from standard errors clustered at the state level are depicted in gray.

Table 1: Descriptive Statistics

	Johnson County, IA	Donor Pool	All US counties
Weekly earnings - Restaurants	249.95	266.58	319.35
Weekly earnings - Other sectors	922.49	795.22	1014.07
Employment share - Restaurants	0.09	0.09	0.08
Employment share - Other sectors	0.91	0.91	0.92
Population	139,436.00	98,714.18	100,798.41
Income per capita	45,316.60	38,873.17	45,114.26
Share of votes to Democratic Party	0.67	0.38	0.51

Notes: The table shows descriptive statistics for Johnson County, IA (column 1), the set of counties present in the donor pool used for the synthetic control approach described in Section 3 (column 2), and all US counties (column 3). Weekly earnings and employment shares are from the Quarterly Census of Employment and Wages (QCEW), where restaurants refer to sector NAICS 722, and other sectors correspond to the rest of the economy. Population is extracted from the 5-year American Community Survey (ACS). Income per capita is from the Bureau of Economic Analysis (BEA). Vote shares for the Democratic Party in the 2012 presidential election are from McGovern (2012). All values (except vote shares) refer to the average value over the pre-reform period 2011-2015.

Table 2: Impact on Labor Market Outcomes

	Overall	During MW	After MW
Panel (a): Earnings			
(1) Restaurants	11.85	9.54	12.48
	[0.041]	[0.005]	[0.043]
(2) Other Sectors	-0.78	-0.63	-0.82
	[0.772]	[0.659]	[0.760]
(1) - (2)	12.63	10.17	13.30
	[0.012]	[0.005]	[0.014]
Panel (b): Employment			
(1) Restaurants	-7.03	-2.20	-8.34
	[0.336]	[0.712]	[0.319]
(2) Other Sectors	-6.58	-1.32	-8.02
	[0.278]	[0.765]	[0.266]
(1) - (2)	-0.45	-0.89	-0.33
	[0.873]	[0.856]	[0.847]

Notes: The table shows the main estimates of the percent change in earnings (Panel (a)) and employment (Panel (b)) due to the minimum wage policy following the synthetic control approach described in the main text in Section 3. Results are presented for the restaurant sector (coefficient $\tilde{\tau}_{ect}$), other sectors excluding restaurants (coefficient $\tilde{\tau}_{oct}$), and, for the difference between the two (coefficient $\hat{\tau}_{ect}$), and refer to the average estimated impact over different periods. The third row within each panel shows the preferred estimates. Column 1 provides estimates for the entire post-event period (from the first full quarter when the minimum wage was in place until the end of our analysis sample in 2022). Column 2 provides estimates averaged across only the quarters when the elevated minimum wage was fully in effect. Column 3 provides estimates averaged across the periods after the policy was repealed. P-values are shown in brackets. The table results correspond to the results reported in Figure 3, Figure 4 and Figure A3. Data source: QCEW.

Table 3: Quantifying the Sources of Wage Hysteresis

ε	θ	Target $\Delta \log n$	Model $\Delta \log n$	$\Delta \log m^e$	$\Delta \log z$	$\Delta \log B$	Norm contrib. (%)	z contrib. (%)	B contrib. (%)
Panel A. Demand shifter held fixed ($\Delta \log B = 0$)									
0.05	0.5	0.000	-0.068	0.254	-0.014	0.000	90.5	9.5	0.0
0.05	0.5	-0.051	-0.068	0.254	-0.014	0.000	90.5	9.5	0.0
0.05	1.0	0.000	-0.068	0.186	-0.014	0.000	93.3	6.7	0.0
0.05	1.0	-0.051	-0.068	0.186	-0.014	0.000	93.3	6.7	0.0
0.10	0.5	0.000	-0.070	0.260	-0.014	0.000	91.2	8.8	0.0
0.10	0.5	-0.051	-0.070	0.260	-0.014	0.000	91.2	8.8	0.0
0.10	1.0	0.000	-0.070	0.189	-0.014	0.000	93.8	6.2	0.0
0.10	1.0	-0.051	-0.070	0.189	-0.014	0.000	93.8	6.2	0.0
Panel B. Demand shifter chosen to match the employment target									
0.05	0.5	0.000	0.000	0.119	-0.014	0.068	42.3	9.5	48.3
0.05	0.5	-0.051	-0.051	0.221	-0.014	0.017	78.8	9.5	11.8
0.05	1.0	0.000	0.000	0.119	-0.014	0.068	59.4	6.7	33.9
0.05	1.0	-0.051	-0.051	0.170	-0.014	0.017	85.1	6.7	8.3
0.10	0.5	0.000	0.000	0.119	-0.014	0.070	41.7	8.8	49.5
0.10	0.5	-0.051	-0.051	0.221	-0.014	0.019	77.7	8.8	13.5
0.10	1.0	0.000	0.000	0.119	-0.014	0.070	58.8	6.2	34.9
0.10	1.0	-0.051	-0.051	0.170	-0.014	0.019	84.2	6.2	9.5

Notes: Columns 3–7 report raw log changes. The final three columns report the contribution of each component to the observed wage increase implied by equation (10). The productivity contribution can be positive even when fitted $\Delta \log z$ is slightly negative because its coefficient in the wage decomposition is negative when $\varepsilon < 1$. In Panel A, the employment target does not feed back into the decomposition because $\Delta \log B = 0$; the fourth column therefore reports the model-implied employment change under that restriction. In Panel B, the demand shifter is chosen to match the employment target exactly.

Online Appendix

The Asymmetric Effect of Wage Floors: A Natural Experiment with a Rising and Falling Minimum Wage

Emiliano Huet-Vaughn Jon Piqueras

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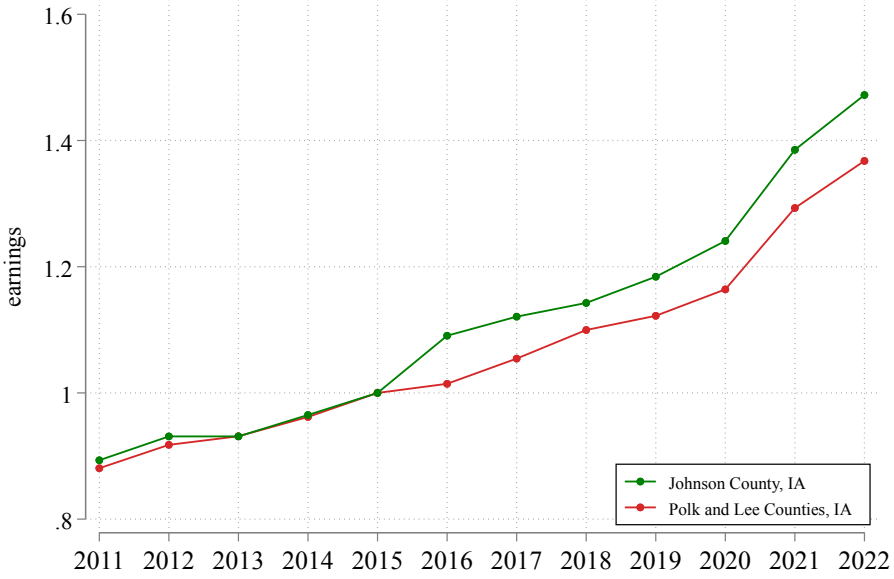
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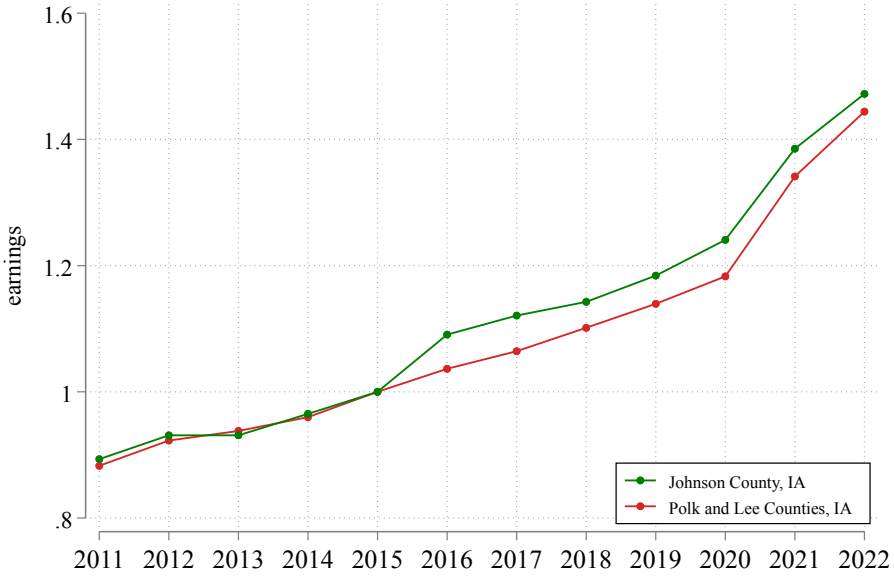
Appendix A Additional Figures and Tables

Figure A1: Average Restaurant Earnings: Johnson vs. Counties with Announced but Non-Implemented Minimum Wages

(a) Earnings - Unweighted



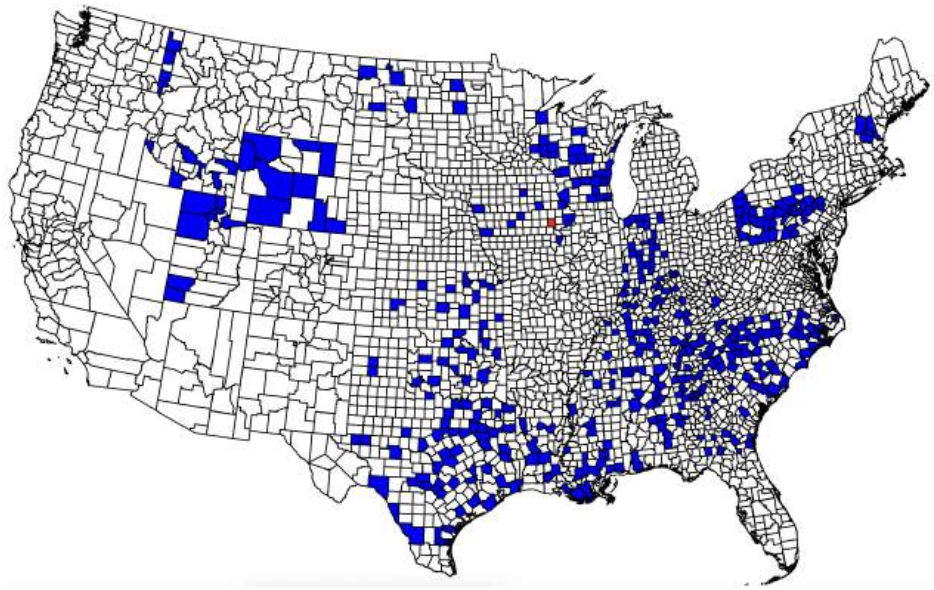
(b) Earnings - Weighted



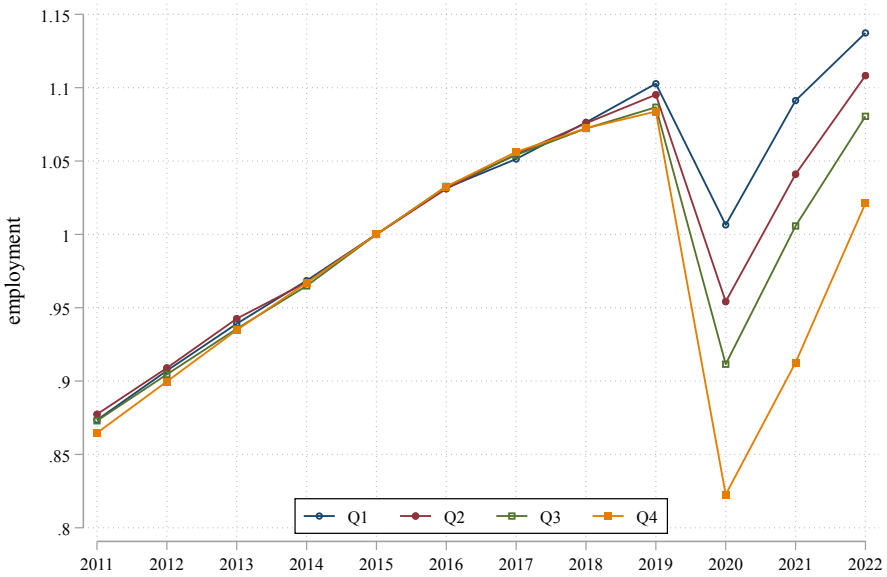
Notes: The graph shows the evolution of average earnings in the restaurant sector (NAICS 722) around the timing of the large, unexpectedly temporary minimum wage policy implemented in Johnson County, Iowa (this minimum wage schedule is depicted in Figure 1). The green line represents the average earnings in the sector in Johnson County, IA. The red line refers to earnings in two Iowa counties, Polk and Lee, where a similar law was passed to increase the minimum wage in 2017, but it never went into effect given the statewide ban after the Johnson County event. Panel (a) shows an unweighted average of these two counties, while Panel (b) depicts average earnings weighting by the pre-2016 restaurant employment in the county. The data used come from the Quarterly Census of Employment and Wages (QCEW). Raw averages are normalized to one in the year prior to the first full year of the minimum wage increase.

Figure A2: Donor Counties and Restaurant Employment by Democratic Vote Share

(a) Counties in Donor Pool



(b) Democratic Vote Share and Restaurant Employment



Notes: Panel (a) shows the donor counties used in the construction of synthetic Johnson. Panel (b) shows average restaurant employment around the minimum wage episode by quartiles of county Democratic Party vote share in the 2012 presidential election. The map in Panel (a) presents the donor counties (in blue) used in the construction of synthetic Johnson (underlying Figures 3 and 4). Johnson County, Iowa is in red. In Panel (b), raw averages are normalized to one in the year prior to the first full year of the minimum wage increase, and then weighted by pre-reform employment in the restaurant sector. The sample includes all US counties except for Johnson County, IA, which is excluded. Johnson County is not used to compute the average but belongs to the fourth quartile. Employment data comes from the QCEW, and Democratic Party vote share information from McGovern (2012). While all quartiles follow similar trends prior to COVID-19, with its onset (in 2020) there is significant divergence.

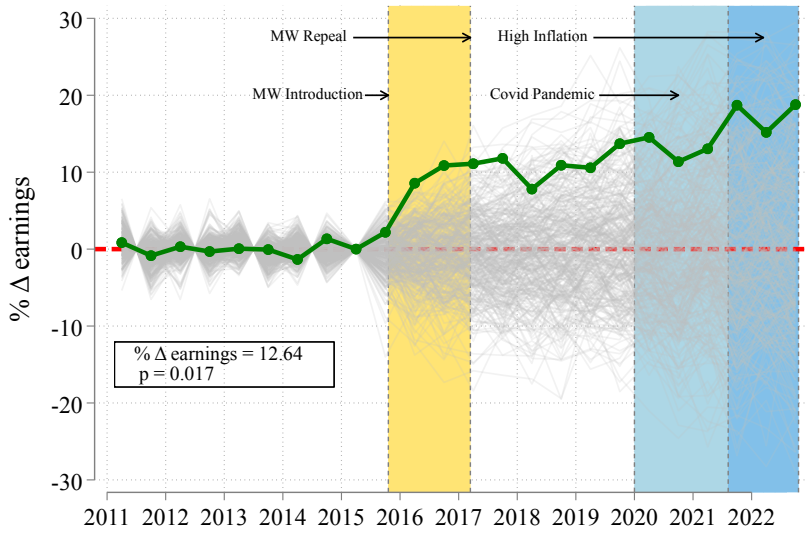
Figure A3: Effect on Earnings and Employment - By Sector



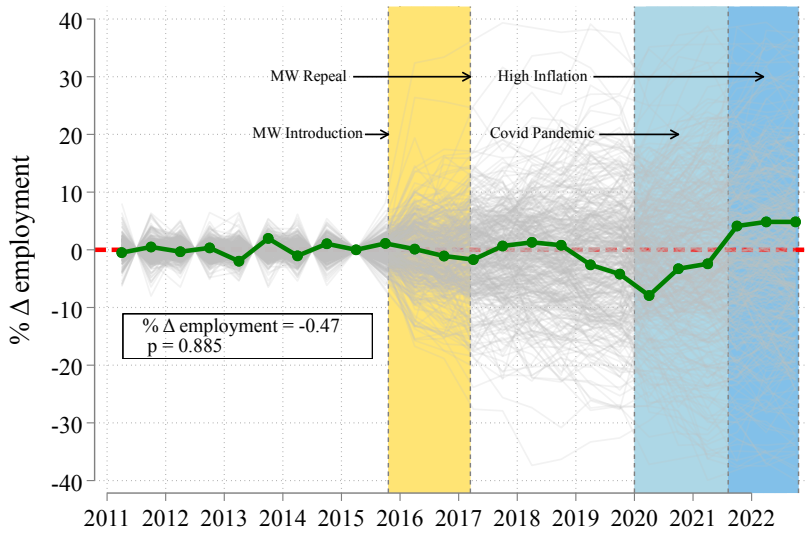
Notes: The figure shows the impact of a temporary minimum wage policy on county-level earnings and employment by sector, separately for the restaurant sector (NAICS 722) and other sectors of the economy (sectors excluding NAICS 722). Panel (a) refers to the impact of the policy on restaurant earnings, while Panel (b) depicts the impact on earnings in the rest of the economy. Panel (c) and Panel (d) refer to the impact on employment, for the restaurant sector and for the rest of the economy, respectively. The green lines depict the relative gap in average earnings or employment in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (Panels (a) and (c) correspond to coefficients $\tilde{\tau}_{ect}$, while Panels (b) and (d) correspond to coefficients $\tilde{\tau}_{oct}$, both described in Section 3.). Gray lines refer to placebo estimates, where we plot the relative gap in average earnings/employment for non-treated units relative to their synthetic version. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage including Iowa with the exclusion of the counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Census of Employment and Wages (QCEW). The yellow area corresponds to the period where employers in Johnson County were legally obliged to pay above the federal minimum wage. The two blue areas correspond to the COVID-19 recession and the ensuing high inflation period. The outcomes are aggregated in six-month time periods from quarterly frequency.

Figure A4: Effect on Earnings and Employment - Bias Corrected SC

(a) Earnings



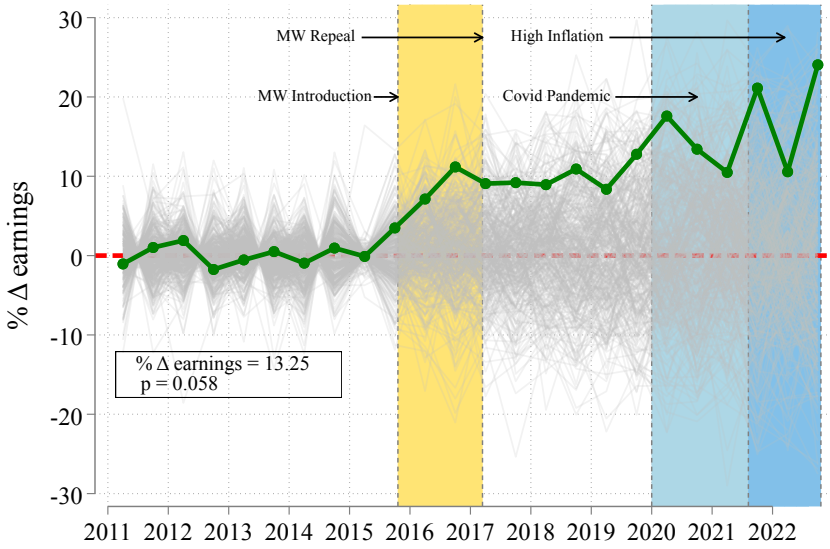
(b) Employment



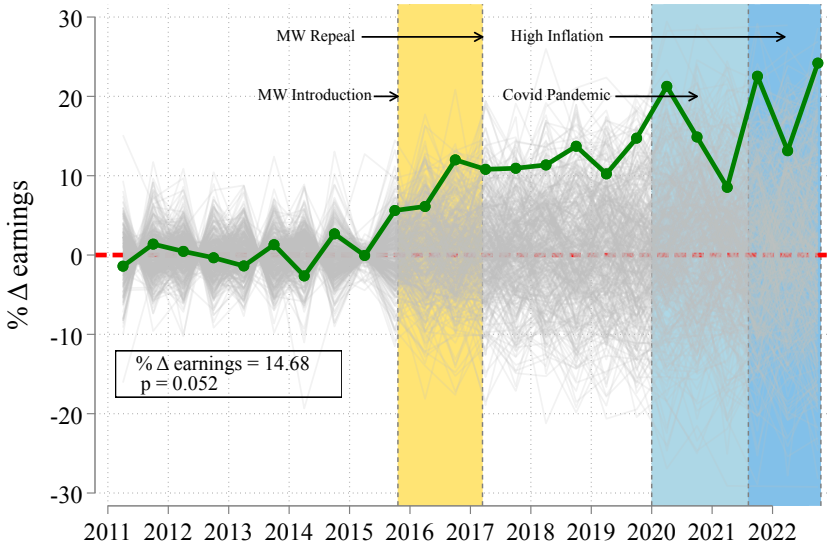
Notes: The figure shows the bias-corrected synthetic control estimates (Ben-Michael et al., 2021) of the impact of the temporary minimum wage policy on restaurant earnings (Panel (a)), and restaurant employment (Panel (b)) in Johnson County, Iowa, accounting for discrepancies in predictor values between treated and control units. The green line depicts the relative gap in average earnings/employment in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (coefficients $\hat{\tau}_{ect}$ in Section 3). Gray lines refer to placebo estimates, where we plot the relative gap in average earnings/employment for non-treated units relative to their synthetic version. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Census of Employment and Wages (QCEW). The yellow area corresponds to the period where employers were legally obliged to pay above federal minimum wage. The two blue areas correspond to the COVID-19 recession and the high inflation period. The outcomes are aggregated in six-month time periods from quarterly frequency.

Figure A5: Effect on Earnings of New Hires

(a) Earnings of Hires - Restaurants



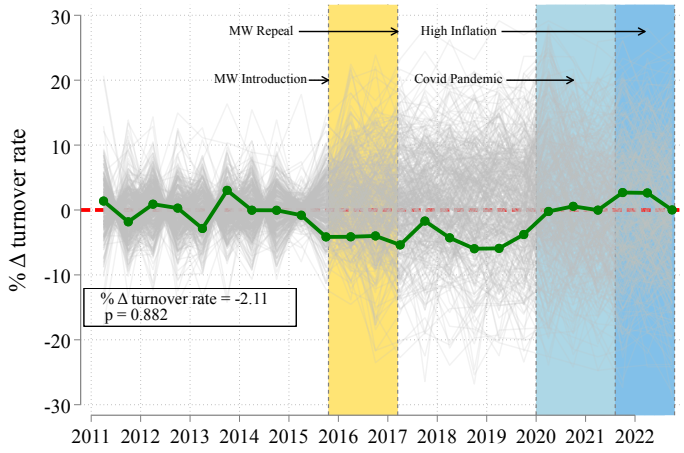
(b) Earnings of New Employees - Restaurants



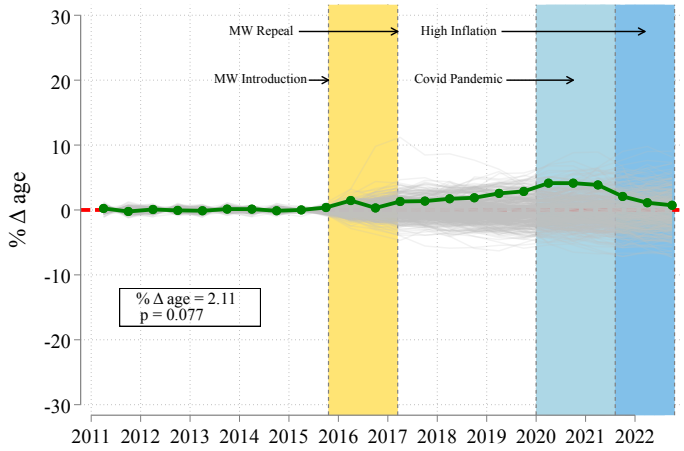
Notes: The figure shows the impact of the temporary minimum wage policy on earnings of individuals hired in the restaurant sector including recalls (Panel (a)), and new individuals hired in the restaurant sector excluding recalls (Panel (b)) in Johnson County, Iowa. The green line depicts the relative gap in average earnings in Johnson relative to synthetic Johnson, constructed using a synthetic control approach leveraging information only from the restaurant sector as described in the main text (coefficients $\tilde{\tau}_{ect}$ in Section 3). Gray lines refer to placebo estimates, where we plot the relative gap in average earnings for non-treated units relative to their synthetic version. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding the counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Workforce Indicators (QWI). The yellow area corresponds to the period where employers were legally obliged to pay above federal minimum wage. The two blue areas correspond to the COVID-19 recession and the high inflation period. The outcomes are aggregated in six-month time periods from quarterly frequency.

Figure A6: Effect on Turnover and Workforce Composition

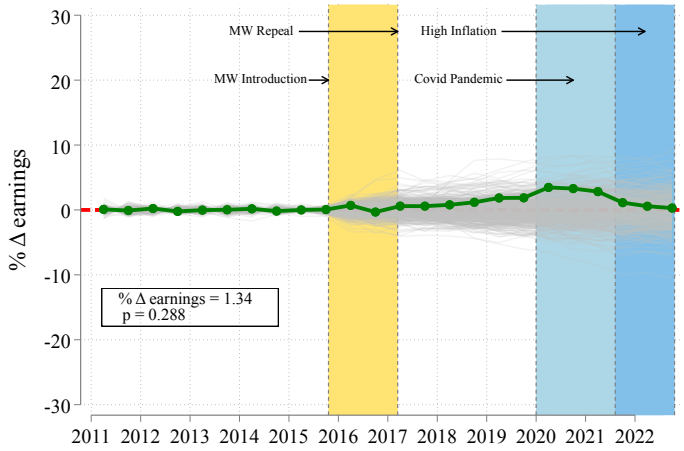
(a) Turnover Rate



(b) Age of Employees

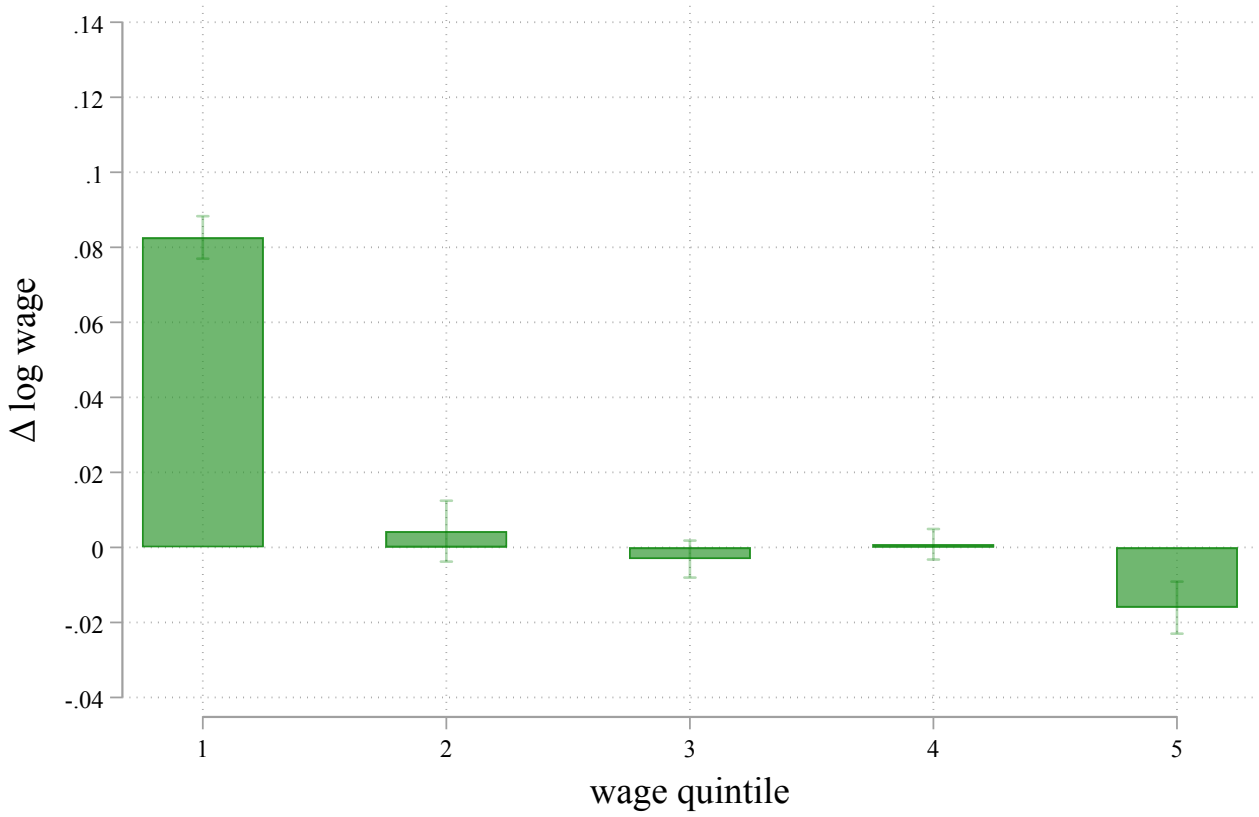


(c) Age-Predicted Earnings



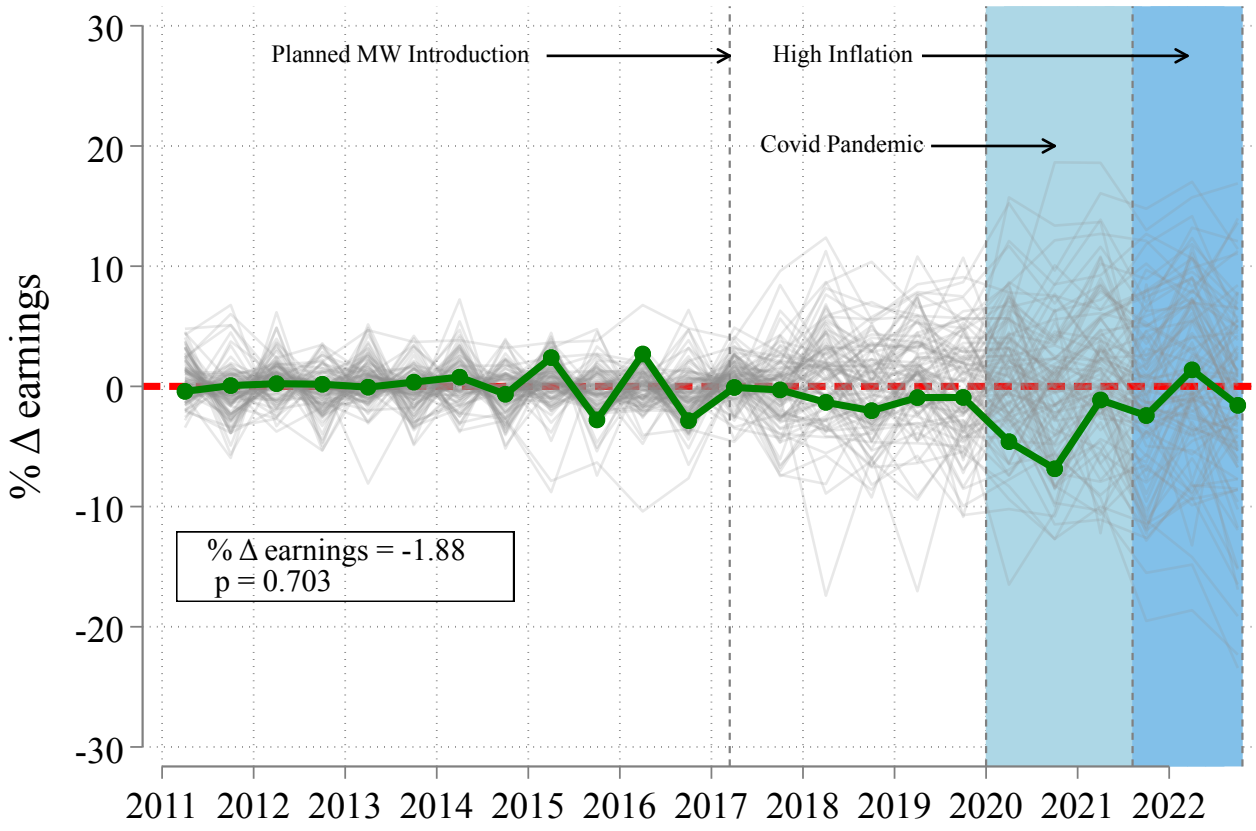
Notes: The figure shows the impact of the temporary minimum wage policy on turnover and workforce-composition outcomes for the restaurant sector in Johnson County, Iowa. Panel (a) shows turnover rate, Panel (b) average age of employees, and Panel (c) age-predicted earnings. The green lines depict the relative gap in each outcome in Johnson relative to synthetic Johnson, constructed using a synthetic control approach leveraging information only from the restaurant sector (coefficients $\tilde{\tau}_{ect}$ in Section 3). Gray lines refer to placebo estimates, where we plot the relative gap in each outcome for non-treated units relative to their synthetic version. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). Similarly, very few point estimates that lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors consists of counties in states where the state minimum wage does not exceed the federal minimum wage and excluding counties in Iowa that border Johnson or that experienced an aborted intended or incipient local minimum wage of their own. Furthermore, the pool is restricted to counties with average pre-reform restaurant employment between 1000 and 10000. The data used come from the Quarterly Workforce Indicators (QWI). The yellow area corresponds to the period where employers were legally obliged to pay above the federal minimum wage. The two blue areas correspond to the COVID-19 recession and the high inflation period. Outcomes are aggregated in six-month time periods from quarterly frequency.

Figure A7: Effect on Wages: Occupation-Based Approach



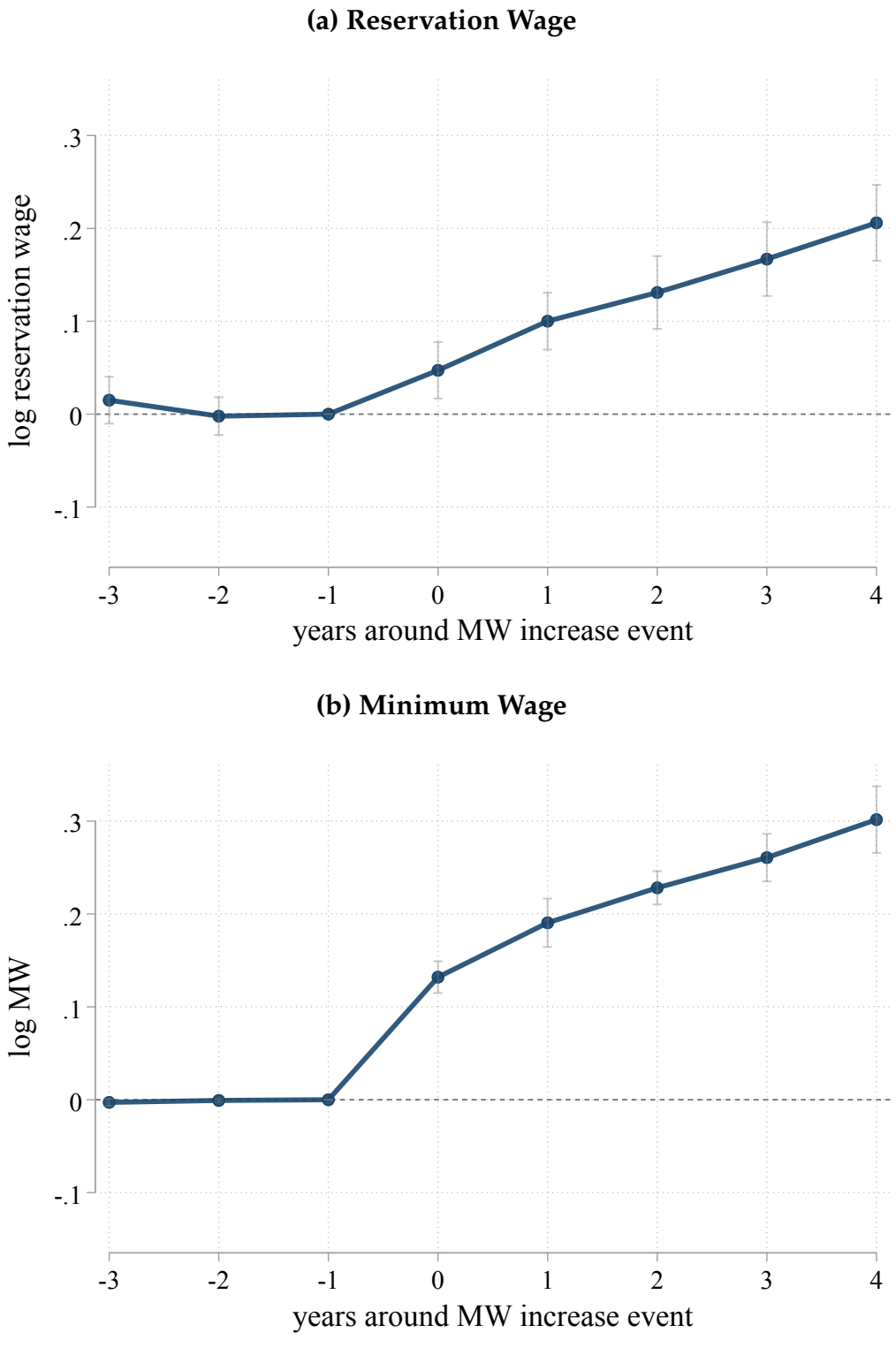
Notes: This figure examines the distributional impact of the policy on hourly wages in the Iowa City MSA using occupation-level data from the OEWS. Iowa City MSA is entirely contained in Johnson County. We rank occupations by the 25th percentile of their hourly wage occupation wage distribution in Iowa City and assign them into employment-weighted quintiles. The figure reports the estimated impact separately for each quintile of the wage distribution. Since data in OEWS correspond to three-year averages, here we use four data points, namely 2012, 2015, 2018, and 2021, to best match the onset of timing of the policy shock under study. For more detailed information on data construction, please see Appendix F. The reported estimate captures the average impact on 2018 and 2021 relative to the pre-reform period. Standard errors are clustered at the MSA level and 95% confidence intervals are depicted on top of the bars. All specifications include MSA, major occupation, and year fixed effects, and are weighted by average MSA-occupation pre-reform employment (see the end of Section 4.1 for additional details).

Figure A8: Policy Announcement Without Implementation



Notes: The graph shows the impact of the announced – but never enacted – minimum wage policies in Polk and Lee counties in Iowa. The green line depicts the relative average gap in earnings in the these two counties relative to a synthetic counterfactual, formed by using the same methods described in Section 3 for the main analysis of Johnson County, Iowa, but here instead identifying Polk and Lee counties as the "treated" counties. In order to provide an average estimate across Polk and Lee counties, we first employ a synthetic control approach at the county-sector level and obtain a county-level estimate, using the same predictors as described in the main text and assessed over the period before the planned enactment of the policy in Spring 2017 (coefficients $\hat{\tau}_{ect}$ in Section 3). We then average the estimates for the two counties weighting by average pre-reform employment in the restaurant sector of each county. Gray lines refer to placebo estimates, where we plot the relative gap in average earnings for non-treated units relative to their synthetic version. To provide a sensible analogue to the average estimate across Polk and Lee counties, we obtain these placebo estimates by first randomly sampling 100 counties from each donor pool, corresponding to each treated county, and then computing the average estimate for each pair of counties using the same weights as for our treated counties. For easier visual inspection, the placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for the pre-treatment fit (ratio of post-MSPE to pre-MSPE employing the final sample of 100 random county-pairs). Similarly, very few point estimates which lie outside the depicted y-axis range are not shown, but are taken into account for the inference procedure. The pool of potential donors for each of the two events uses the same restrictions as used in the main Johnson analysis (see Figure 3), while also excluding Johnson County, Iowa, and restricting donors to be drawn from counties with average pre-reform restaurant employment between 5000 and 30000 for Polk, and 500 and 3000 for Lee. These restrictions are intended to account for the different dimension of the restaurant sectors in both counties (17973 employees in Polk relative to 880 in Lee). The data used come from the Quarterly Census of Employment and Wages (QCEW). The two blue areas correspond to the COVID-19 recession and the high inflation period. The dashed line refers to the moment of intended onset of the minimum wage increase in the second quarter of 2017 (which was aborted just prior to onset). The outcomes are aggregated in six-month time periods from quarterly frequency.

Figure A9: Reservation Wage Mechanism (All Events)



Notes: The figure shows the effect of an increase in the minimum wage on the self-reported reservation wage of low-wage workers, in Panel (a), while Panel (b) demonstrates the changes in the statutory minimum wage (between treated and control states) that constitute the shocks used for identification in Panel (a). These minimum wage increase events are composed of all of the prominent state-level minimum wage increase events catalogued in Appendix E of Dube and Lindner (2024) that transpire in the time period for which we have outcome data (reservation wage for UI applicants). Results come from a stacked event-study design (Cengiz et al., 2019). See the main text for further details. The data used come from the Unemployment Insurance Benefit Accuracy Measurement (BAM) program. 95 % confidence intervals from standard errors clustered at the state level are depicted in gray.

Table A1: Donor Counties for Restaurant Sector

Weight	County
Panel (a): Earnings	
0.375	Webb County, Texas
0.141	Clayton County, Georgia
0.137	Maverick County, Texas
0.122	Pasquotank County, North Carolina
0.098	Stark County, North Dakota
0.038	Jackson County, North Carolina
0.025	Teton County, Wyoming
0.023	Williams County, North Dakota
0.020	Centre County, Pennsylvania
0.010	Door County, Wisconsin
0.010	Dougherty County, Georgia
Panel (b): Employment	
0.375	Webb County, Texas
0.141	Clayton County, Georgia
0.137	Maverick County, Texas
0.122	Pasquotank County, North Carolina
0.098	Stark County, North Dakota
0.038	Jackson County, North Carolina
0.025	Teton County, Wyoming
0.023	Williams County, North Dakota
0.020	Centre County, Pennsylvania
0.010	Door County, Wisconsin
0.010	Dougherty County, Georgia

Notes: The table shows the counties that received a positive weight in the construction of the synthetic counterfactual for the restaurant sector (NAICS 722) of Johnson County, Iowa, as described in Section 3, with Panel (a) corresponding to the estimates shown in Panel (a) of Figure A3, and Panel (b) corresponding to the estimates shown in Panel (c) of that figure.

Table A2: Donor Counties for Other Sectors

Weight	County
Panel (a): Earnings	
0.214	Orange County, North Carolina
0.198	Dougherty County, Georgia
0.120	Douglas County, Wisconsin
0.103	Orangeburg County, South Carolina
0.103	Woodbury County, Iowa
0.088	Maverick County, Texas
0.076	Baldwin County, Georgia
0.051	Clayton County, Georgia
0.030	Summit County, Utah
0.013	Rock County, Wisconsin
0.002	Wood County, Wisconsin
Panel (b): Employment	
0.222	Orange County, North Carolina
0.189	Dougherty County, Georgia
0.119	Douglas County, Wisconsin
0.104	Orangeburg County, South Carolina
0.103	Woodbury County, Iowa
0.091	Maverick County, Texas
0.075	Baldwin County, Georgia
0.051	Clayton County, Georgia
0.032	Summit County, Utah
0.009	Rock County, Wisconsin
0.006	Wood County, Wisconsin

Notes: The table shows the counties that received a positive weight in the construction of the synthetic counterfactual for other sectors (excluding restaurants) of Johnson County, Iowa, as described in Section 3, with Panel (a) corresponding to the estimates shown in Panel (b) of Figure A3, and Panel (b) corresponding to the estimates shown in Panel (d) of that figure.

Table A3: Effect on Wages: Occupation-Based Approach

	Log Wage			
	(1)	(2)	(3)	(4)
Treated × Johnson × 2012	-0.001	0.001	0.041*	0.058*
	(0.003)	(0.006)	(0.018)	(0.028)
Treated × Johnson × 2018	0.053***	0.108***	0.323***	0.514***
	(0.004)	(0.007)	(0.022)	(0.034)
Treated × Johnson × 2021	0.098***	0.200***	0.598***	0.952***
	(0.003)	(0.006)	(0.018)	(0.027)
Definition of "Treated"	$\mathbb{1}[p25 < MW]$	GAP_{p25}	share below MW	GAP
N	13424	13424	13424	13424

Notes: This table examines the effect of the policy on hourly wages in Iowa City using occupation-level data at the MSA level from OEWS. Iowa City MSA is entirely contained in Johnson County. The specification employs four definitions of treatment. Column (1) defines occupations as treated when the 25th percentile of the major occupation's wage distribution is below the new minimum. Column (2) defines the treatment variable as the predicted percent increase in wages required to bring the 25th percentile up to the new minimum wage (corresponding to a distributional version of the GAP measure used in the literature). Column (3) defines it as the share of workers below the new minimum. Column (4) defines it as the predicted average percent increase in wages needed for all workers in the occupation below the new minimum wage to reach it (GAP measure). Columns (3) and (4) require imputation, while Columns (1) and (2) are taken directly from OEWS data. For more detailed information on the construction of those variables, please see Appendix F. Since data in OEWS correspond to three-year averages, here we use four data points, namely 2012, 2015, 2018, and 2021, to best match the onset of timing of the policy shock under study. Therefore, the estimate for the year 2018 only captures partial exposure to the maximum statutory wage floor. Standard errors are clustered at the MSA level. All specifications include MSA, major occupation, and year fixed effects, and are weighted by average MSA-occupation pre-reform employment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B Framework for Interpreting Hysteresis

This appendix develops the framework used in Section 5 to interpret the post-repeal evidence. The aim to highlight the main forces that may account for wage hysteresis after repeal. The framework is built around two features of the setting. On the product side, a market-wide wage floor is governed by the elasticity of substitution between the exposed sector and the rest of consumption, not only by substitution across firms within the exposed sector (Harasztosi and Lindner, 2019). On the labor side, a temporary market-wide minimum wage changes the attractiveness of the exposed sector itself, not merely relative wages across exposed firms.

The key insight used for identification is simple. In equilibrium, a higher reference wage m^e raises wages and prices, while a higher productivity shifter z lowers prices. This sign contrast lets us use the joint behavior of wages, prices, and employment after repeal to discipline the quantification exercise. We first consider the worker side, then consumer demand, then the firm problem, and finally the multi-input extension used to take the framework to the data.

B.1 Environment

There are two sectors, indexed by $s \in \{e, o\}$, where e denotes the exposed low-wage sector and o denotes the rest of the county economy. Workers are indexed by i and firms by j. There is one type of labor on the worker side. In the benchmark production side developed through the equilibrium subsection below, labor is also the only variable input. A multi-input extension is presented later, which allows labor to be only one component of the marginal cost, and utilize that version for the quantification.

The model is static except for one state variable, the exposed-sector reference wage

$$m^e \equiv h\left(\max_{\tau \leq T} m_\tau\right),$$

which summarizes the effective reference wage relevant for workers' exposed-sector choices after the repeal date T. The function $h(\cdot)$ is increasing in the highest real minimum wage workers in the exposed sector have experienced up to date T; so the effective reference wage equals the highest past real minimum wage. After repeal, the current statutory minimum may fall back below m^e. Let P denote the county-wide price index and P^e the price index of the exposed sector. All wages and prices below are in real terms, deflated by P, so the real relative price of the exposed sector is $p \equiv P^e/P$.

B.2 Worker Problem and Labor Supply

This subsection delivers the labor-side block that later maps wage hysteresis into a persistent shift in market-level labor supply. The key idea is that a temporary market-wide minimum wage raises the common reference wage m^e for jobs in the exposed sector, which changes the attractiveness of the exposed sector as a whole.

Firm Choice. Workers are assumed to observe posted wages and to exhibit idiosyncratic preferences over employers, following Card et al. (2018). For worker i and exposed-sector firm j, let indirect utility be

$$u_{ij}^e = \log a_j + \eta \log\left(\frac{w_j}{m^e}\right) + \varepsilon_{ij},$$

where w_j is the real wage posted by firm j, $a_j > 0$ is a firm-specific amenity term, and ε_{ij} is an idiosyncratic preference shock. The parameter $\eta > 0$ is the within-labor-market elasticity governing substitution across firms inside the exposed sector. The term m^e is a common exposed-sector reference wage induced by the minimum wage, so a higher m^e lowers the utility of exposed jobs at a given posted wage. This reference-wage specification is closely related to the fair wage view of wage setting emphasized by Akerlof and Yellen (1990).

Under the standard type-I extreme value assumption for ε_{ij}, McFadden (1973) implies that the conditional probability that worker i chooses firm j after entering the exposed sector is

$$s_{j|e}^L = \frac{a_j w_j^\eta}{\sum_{k=1}^J a_k w_k^\eta}.$$

Because m^e is common across exposed firms, it cancels out of this lower nest. Define the exposed-sector wage index by

$$\mathcal{W}^e \equiv \left(\sum_{k=1}^J a_k w_k^\eta\right)^{1/\eta}.$$

Then the lower-nest labor share can be written as

$$s_{j|e}^L = \frac{a_j w_j^\eta}{(\mathcal{W}^e)^\eta} = \lambda^e a_j w_j^\eta, \qquad \lambda^e \equiv (\mathcal{W}^e)^{-\eta}.$$

When the number of exposed firms is large, each firm is negligible in the sectoral index $\mathcal{W}^e$, so λ^e can be treated as given from the perspective of an individual firm. We adopt this atomistic-firm approximation in what follows. The lower nest therefore governs reallocation across firms inside the exposed labor market using the within-market elasticity η.

Sector Entry. Let the outside option, which includes both other sectors and nonemployment, deliver utility $u_{i0} = b_i$, where b_i is worker i's outside value. Using the lower-nest wage index just

defined, the expected value of entering the exposed sector is

$$V^e = \eta \log\left(\frac{\mathcal{W}^e}{m^e}\right) + c_\eta,$$

where c_η is the corresponding extreme-value constant. Worker i enters the exposed sector if and only if $V^e \geq b_i$. Equivalently, there is a worker-specific reservation sector wage index that makes worker i indifferent between the exposed sector and the outside option:

$$\mathcal{W}_i^{R,e} = m^e \exp\left(\frac{b_i - c_\eta}{\eta}\right).$$

Worker i enters the exposed sector if and only if $\mathcal{W}^e \geq \mathcal{W}_i^{R,e}$. This is the key worker-side object for the post-repeal setting: holding outside values fixed, a higher m^e raises the reservation sector wage index for entering the exposed sector. It is also close in spirit to the reservation-wage evidence presented in the main text, since an increase in average reservation wages can be interpreted as an increase in the average reservation threshold for the exposed-sector wage index.

To obtain a tractable upper-nest labor-supply equation, assume aggregate entry into the exposed sector is isoelastic in the sector wage index relative to the reference wage. Then aggregate labor supplied to the exposed sector is

$$M^e = \Lambda\left(\frac{\mathcal{W}^e}{m^e}\right)^\theta,$$

where θ is the across-labor-market elasticity governing reallocation between the exposed sector and the outside option. This upper-nest elasticity is distinct from the within-sector elasticity η: the former governs how many workers are willing to work in the exposed sector at all, while the latter governs how those workers sort across employers once they enter.

Firm Labor Supply. Combining the lower-nest labor share with the upper-nest sector-entry margin gives

$$n_j = M^e s_{j|e}^L = M^e \lambda^e a_j w_j^\eta.$$

Defining the labor-supply shifter $\mathcal{A}_j \equiv M^e \lambda^e a_j$, the firm-level labor supply curve is

$$n_j = \mathcal{A}_j w_j^\eta,$$

with inverse labor supply

$$w_j = \left(\frac{n_j}{\mathcal{A}_j}\right)^{1/\eta}.$$

Under this representation, a higher m^e shifts labor supply to the left through the shifter $\mathcal{A}_j$ because it lowers sector entry, while the firm-level labor supply elasticity with respect to the posted wage remains equal to η. When all exposed firms move together in equilibrium, however, the market-level entry elasticity θ becomes the key labor-supply object.

B.3 Consumer Problem and Product Demand

This subsection delivers the product-demand block. The within-sector elasticity σ governs substitution across firms inside the exposed sector, while the across-sector elasticity ε governs how a market-wide cost shock translates into pass-through and quantity adjustment.

Within-Sector Aggregation. The exposed-sector composite is a CES aggregate of differentiated firm varieties:

$$Q^e = \left(\int_{j \in e} q_j^{\frac{\sigma-1}{\sigma}} \, \mathrm{d}j \right)^{\frac{\sigma}{\sigma-1}}, \qquad \sigma > 1.$$

Standard CES expenditure minimization implies the exposed-sector price index

$$P^e = \left(\int_{j \in e} p_j^{1-\sigma} \, \mathrm{d}j \right)^{\frac{1}{1-\sigma}}$$

and firm demand

$$q_j = Q^e \left(\frac{p_j}{P^e} \right)^{-\sigma}.$$

Hence σ is the within-product-market elasticity that governs substitution across firms inside the exposed sector.

Across-Sector Allocation. At the upper nest, a representative county consumer chooses the exposed-sector composite Q^e and the outside good Q^o according to

$$Q = \left[\alpha \left(Q^e \right)^{\frac{\varepsilon-1}{\varepsilon}} + (1-\alpha) \left(Q^o \right)^{\frac{\varepsilon-1}{\varepsilon}} \right]^{\frac{\varepsilon}{\varepsilon-1}}, \qquad \varepsilon > 0.$$

Standard CES demand then implies

$$Q^e = \alpha^{\varepsilon} \left(\frac{P^e}{P} \right)^{-\varepsilon} Q, \qquad Q^o = (1-\alpha)^{\varepsilon} \left(\frac{P^o}{P} \right)^{-\varepsilon} Q.$$

Taking the ratio gives

$$\frac{Q^e}{Q^o} = \left(\frac{\alpha}{1-\alpha} \right)^{\varepsilon} \left(\frac{P^e}{P^o} \right)^{-\varepsilon} \equiv \bar{B} \left(\frac{P^e}{P^o} \right)^{-\varepsilon},$$

where $\bar{B} \equiv [\alpha/(1-\alpha)]^{\varepsilon}$. This upper nest is the product-market counterpart to the labor aggregation derived above. The within-sector elasticity σ governs firm shares and markups, while the across-sector elasticity ε governs how total demand for the exposed sector changes when all exposed firms face a common cost shock.

B.4 Firm Problem

Exposed-sector firms are small and take sector demand objects and the labor-supply shifter $\mathcal{A}_j$ as given. There is no strategic interaction. In the benchmark model, labor is the only variable input and production is linear in employment:

$$y_j = z n_j, \qquad z > 0.$$

Here z is a productivity shifter. This labor-only benchmark is useful for exposition; the next subsection then extends production to allow labor to be only one component of marginal cost. Product market clearing requires $q_j = y_j$, or equivalently $n_j = q_j/z$. Combining this identity with inverse labor supply, total labor cost as a function of output is

$$C_j(q_j) = \mathcal{A}_j^{-1/\eta} \left(\frac{q_j}{z}\right)^{1+1/\eta},$$

and marginal labor cost is

$$\text{MLC}_j \equiv \frac{\partial C_j(q_j)}{\partial q_j} = \left(1 + \frac{1}{\eta}\right) \frac{w_j}{z}.$$

This is the labor-market markdown wedge. Because the firm must raise the wage to recruit additional labor, the marginal cost of one more unit of output exceeds the wage itself.

Given CES demand, the firm chooses output to maximize $p_j(q_j)q_j - C_j(q_j)$. Under the inverse demand curve induced by the CES aggregator, marginal revenue is $\text{MR}_j = p_j(1 - 1/\sigma)$. Equating marginal revenue to marginal labor cost yields

$$p_j = \underbrace{\frac{\sigma}{\sigma - 1}}_{\mu} \text{MLC}_j = \underbrace{\frac{\sigma}{\sigma - 1}\left(1 + \frac{1}{\eta}\right)}_{\kappa} \frac{w_j}{z}.$$

Thus the within-product elasticity σ determines the markup, the within-labor elasticity η determines the markdown, and the combined wedge is κ. The market-level elasticity θ still matters for sector entry and for market-wide shocks.

B.5 Equilibrium

We now impose a symmetric exposed-sector equilibrium for the labor-only benchmark while keeping the worker-side derivation of the reservation sector wage index in the background. Under symmetry,

$$p_j = P^e, \qquad w_j = w, \qquad q_j = q, \qquad n_j = n.$$

The worker-side entry condition depends on the common ratio w/m^e. Normalizing the mass of exposed firms to one and taking P^o and Q^o as fixed, the across-sector allocation block reduces to

$$q = B p^{-\varepsilon},$$

where $B > 0$ is a reduced-form demand shifter that nests the structural CES constant, the fixed outside-sector terms, and any time variation in demand conditions, including service quality. Likewise, the upper-nest labor-supply block reduces to

$$n = A\left(\frac{w}{m^e}\right)^{\theta},$$

which corresponds to equation (6) in the main text. In the benchmark model, the pricing rule implies

$$p = \kappa \frac{w}{z},$$

and product market clearing gives $q = zn$.

Together, these equations form the symmetric benchmark system. Solving the system yields equilibrium objects of the form

$$w^* = C_w (m^e)^{\frac{\theta}{\theta+\varepsilon}} z^{\frac{\varepsilon-1}{\theta+\varepsilon}}, \qquad n^* = C_n (m^e)^{-\frac{\theta\varepsilon}{\theta+\varepsilon}} z^{\frac{\theta(\varepsilon-1)}{\theta+\varepsilon}}, \qquad p^* = C_p (m^e)^{\frac{\theta}{\theta+\varepsilon}} z^{-\frac{\theta+1}{\theta+\varepsilon}},$$

where the constants C_w, C_n, and C_p depend only on $(A, B, \kappa, \varepsilon, \theta)$. These expressions make the sign logic transparent. A higher reference wage raises wages and prices and weakly lowers employment. A productivity improvement lowers prices and, when $\varepsilon < 1$, also tends to lower wages and employment because the sector needs less labor to produce the demanded output. By contrast, the reduced-form demand shifter B raises wages and prices together and is mainly disciplined by employment. The within elasticities σ and η matter for levels through the pricing wedge κ, while the comparative statics of market-wide shocks depend primarily on the across-market elasticities ε and θ.

B.6 Multi-Input Extension for Quantification

To take the framework to the data, we allow labor to be only one component of marginal cost. Suppose firms combine labor n_j and a nonlabor input x_j, purchased at fixed real price r, according to

$$y_j = z n_j^{s_L} x_j^{1-s_L}, \qquad s_L \in (0, 1].$$

Maintaining the same inverse labor supply as above, cost minimization for a given output implies marginal cost can be written as

$$mc = s_L^{-s_L}(1 - s_L)^{-(1-s_L)} \left(1 + \frac{1}{\eta}\right)^{s_L} r^{1-s_L} \frac{w^{s_L}}{z} \equiv \bar{c} \frac{w^{s_L}}{z},$$

where $\bar{c}$ absorbs the fixed Cobb-Douglas terms, the markdown term, and the nonlabor input price. Under CES demand, the pricing rule becomes

$$p = \underbrace{\frac{\sigma}{\sigma - 1} \bar{c}}_{\tilde{\kappa}} \frac{w^{s_L}}{z}.$$

Taking logs gives the pricing equation used in the main text:

$$\Delta \log p = s_L \Delta \log w - \Delta \log z.$$

On the demand side, the same reduced-form shifter B gives $q = Bp^{-\varepsilon}$. Substituting the optimal input ratio back into production then yields

$$n = \chi \frac{Bp^{1-\varepsilon}}{w}, \qquad \chi > 0,$$

which in log changes implies equation (8) in the main text:

$$\Delta \log n = \Delta \log B + (1-\varepsilon)\Delta \log p - \Delta \log w.$$

When $s_L = 1$, the extension collapses to the labor-only benchmark. For the empirical application, however, allowing labor to be only one part of marginal cost is important because it lets the observed wage and price responses discipline the role of productivity in a transparent way.

B.7 Quantifying the Sources of Wage Hysteresis

We now compare the pre-reform equilibrium to the post-repeal period, when the current statutory minimum may have returned to its earlier level but the exposed-sector reference wage m^e can remain elevated. The goal is to use the observed post-repeal changes in wages, prices, and employment together with the framework to decompose the empirical hysteresis into contributions from worker-side norms, productivity, and quality-adjusted demand.

Calibration and Interpretation. Combining the pricing equation, labor demand, and worker-side labor supply gives the wage change decomposition used in Section 5.3:

$$\Delta \log w = \frac{1}{D}\Delta \log B + \frac{\theta}{D}\Delta \log m^e + \frac{\varepsilon - 1}{D}\Delta \log z,$$

where $D \equiv \theta + 1 - s_L + s_L\varepsilon$. Equation (10) in the main text separates three forces: a worker-side norm or reservation-wage shift m^e, a production-productivity term z, and a quality-adjusted demand shifter B. In the quantification, we use the empirical post-repeal moments from the main text, $\Delta \log w = \log(1.126)$ and $\Delta \log p = \log(1.055)$, and set $s_L = 1/3$. We vary the product-demand elasticity $\varepsilon \in \{0.05, 0.10\}$, the market-level labor-supply elasticity $\theta \in \{0.5, 1\}$, whether the demand shifter B is fixed or allowed to move, and whether the employment target is zero or a decline of 5.1 log points.

Under the one-third labor-share assumption, the observed wage-price pair pins down productivity through

$$\Delta \log z = \frac{1}{3}\Delta \log w - \Delta \log p.$$

Hence $\Delta \log z$ is invariant across the calibration grid once the wage and price moments are fixed. If the quality-demand shifter is allowed to move, the remaining objects are identified by

$$\Delta \log B = \Delta \log n - (1-\varepsilon)\Delta \log p + \Delta \log w, \qquad \Delta \log m^e = \Delta \log w - \frac{1}{\theta}\Delta \log n.$$

If instead $\Delta \log B = 0$, then the norm shift is identified by

$$\Delta \log m^e = \frac{(\theta+1)\Delta \log w + (\varepsilon-1)\Delta \log p}{\theta}.$$

The contribution shares reported in Table 3 correspond to the three terms in the wage decomposition divided by the observed wage increase.

Table 3 delivers one immediate conclusion. Once labor is only one-third of marginal cost, the wage-price pair implies $\Delta \log z \lesssim 0$ in every calibration. In other words, the data leave little room for a positive productivity improvement to be the source of wage hysteresis. The main uncertainty is therefore not whether productivity explains the facts, but whether the model should be read through the parsimonious benchmark with $\Delta \log B = 0$ or through the richer specification in which demand is also allowed to move.

The parsimonious benchmark corresponds to Panel A of Table 3. If the demand shifter is held fixed, the model attributes roughly 90 to 94 percent of wage hysteresis to norms, with the remainder coming from the fact that fitted productivity is slightly negative rather than positive. The cost of that parsimony is that the model predicts an employment decline of about 0.068 to 0.070 in log terms. One possible interpretation is that, given the uncertainty around the near-zero point estimate in the main text, the true employment effect could be modestly negative. In that case, the norm-dominant decomposition in Panel A is the natural benchmark.

Panel B allows demand to move so as to match the employment target. If employment is taken to be exactly unchanged, the model needs a sizeable positive demand shift to offset the employment decline that the cost side would otherwise generate. In that case, norms still matter considerably, but quality-adjusted demand also becomes quantitatively important. However, if the true employment effect is even moderately negative, close to the lower end of the estimates we report, the required demand shift becomes small and the decomposition moves back toward a largely norm-driven account of wage hysteresis. Taken together, the overall result is consistent across implementation assumptions: the wage-price facts strongly support a persistent norm or reservation-wage shift, while positive productivity does not appear to be a plausible explanation for the observed post-repeal combination of wages and prices.

Appendix C Additional Mechanism Evidence

This Appendix reports the additional mechanism exercises complementing the evidence presented in Section 5.4.

Firm-Productivity Based Mechanism: Job Posting. To develop a test that discriminates between alternative mechanisms, we turn to Burning Glass wage posting data in the restaurant industry (NAICS 722). Though not without known limitations (Azar et al., 2024; Batra et al., 2023), this data offers establishment-level wage information that is uniquely useful for this purpose.[45] Specifically, we focus on firms that both operated in Johnson County before the revocation of the minimum wage and also had establishments outside Johnson County.

Under a firm-productivity mechanism, if firms discover that their Johnson establishments are more profitable as a result of paying their workers more, one would expect them to raise posted wages in their non-Johnson establishments as well. By contrast, the reservation wage mechanism predicts no such divergence because the workers outside Johnson were not exposed to the policy and therefore did not experience a shift in their wage expectations.

We implement a difference-in-differences strategy to assess the relevance of this firm-based mechanism:

$$y_{ict} = \beta_0 + \beta_1 \cdot D_c \cdot \mathbb{1}[t \geq 2015q4] + \gamma_i + \rho_c + \lambda_t + \epsilon_{ict}, \tag{12}$$

where y_{ict} refers to log posted wage by firm i in its establishments in a non-Johnson county c and quarter t, D_c is a dummy taking value 1 for firms with establishments also posting in Johnson County before the revocation of the minimum wage, γ_i are firm fixed effects, ρ_c are county fixed effects, and λ_t are quarter fixed effects. Standard errors are clustered at the firm and county level. The coefficient β_1 identifies the change in posted wages of directly affected firms relative to those unaffected after the minimum wage was enacted.

The results are shown in Table C1. Column (1) indicates that firms directly affected by the policy increase their posted wages for jobs located within Johnson, implying that the policy is binding. But column (2) shows that directly affected firms do not appear to change their posted wages in counties outside Johnson relative to unaffected firms. This absence of wage propagation to untreated establishments weighs against a firm-learning interpretation of the persistent wage effect.

[45] As described in Section 2.2, the restaurant sector utilized online platforms to search for jobs at a lower rate than other sectors during the years of our analysis (Azar et al., 2024) and only a small fraction of the job postings for all sectors (Batra et al., 2023) have wage information, so the findings here may be less representative of the overall offered wage.

Worker Reservation Wage Mechanism: Income Changes of Movers. We also exploit the longitudinal structure of the L2 data and its measure of household income.[46] We study the income dynamics of low-income workers who are directly affected by the minimum wage in Johnson County before they move to other counties in Iowa. If earnings hysteresis is driven, at least in part, by upward shifts in treated workers' reservation wages, then movers who leave Johnson after exposure to the policy should carry those higher wage expectations with them and hold out for better-paying jobs when facing new untreated firms.

We test this prediction by assigning treatment based on an individual's location being Johnson County immediately prior to the reform and not after the reform. The treated group therefore consists of individuals who moved out of Johnson County after exposure to the policy, while the control group includes individuals who moved between other Iowa counties during the same period. The outcome variable is log household income, and to limit the influence of outliers we trim the sample at the 1st and 90th percentiles of the distribution of log income changes.[47] The analysis is based on one pre-event year (mid-2015) and one post-repeal year (mid-2017).[48]

The results are shown in Table C1. In column (3), the sample is restricted to low-income earners (bottom 20% of earners in the state in the pre-treatment year) who move to another county in Iowa. For this sample, movers from Johnson experience an increase in income of about 9%. Column (4) shows that low-income workers who stay in Johnson experience a similarly sized gain. As a placebo test, column (5) considers high-income movers out of Johnson and compares them to other high-income movers; as expected, there is no significant increase in their income. Overall, these results are consistent with treated workers carrying a higher reservation wage with them, although, because we observe income rather than reservation wages directly, the evidence remains suggestive rather than definitive.

[46]The L2 data used has broad though not complete coverage of the Iowa population. It covers all registered voters, who make up about 70% of Iowa's voter population in recent years. For more on the proprietary consumer and voter products used in this analysis see https://www.l2-data.com/.

[47]For consistency, the other estimates in this analysis use samples with the same trimming cutoffs.

[48]Unfortunately, the L2 data utilized for this analysis does not provide income information for additional earlier pre-policy years.

Table C1: Additional Mechanism Evidence

	Log Posted Wages		Log Income		
	(1) Johnson Restaurants	(2) Outside Johnson Restaurants	(3) Leavers Johnson: Low-Earners	(4) Stayers Johnson: Low-Earners	(5) Leavers Johnson: High-Earners
Treated × Post	0.107**	0.004	0.096**	0.093***	-0.036
	(0.044)	(0.029)	(0.047)	(0.008)	(0.040)
N	2680	3270	7370	544480	4184

Notes: The table reports the additional mechanism evidence discussed in Appendix C. Columns (1) and (2) use 2013-2018 Burning Glass online job vacancy data on the log posted wage for restaurant jobs to explore a prediction of the firm-learning mechanism discussed in subsection C. Column (1) shows growth in the posted wage for establishments in Johnson County relative to unexposed firms outside Johnson following the minimum wage increase there. Column (2) shows that firms exposed to the minimum wage shock in Johnson do not seem to raise posted wages in their non-Johnson establishments, challenging a prediction of the firm-learning mechanism. Both columns are obtained from a difference-in-differences specification as in equation (12). The remaining columns (3)-(5) report tests related to the worker reservation wage mechanism discussed in subsection C. Specifically, using L2 data to track workers who move out of Johnson, we study the income of low-wage workers (i.e., bottom 20% of the initial Iowa income distribution) who move out of Johnson County, Iowa to other counties in Iowa following exposure to the Johnson increase in the minimum wage. These treated workers are compared to a control group of low-wage workers who move out of other counties in Iowa to a new Iowa county (excluding moves to Johnson). Column (3) reports the coefficient from a difference-in-differences regression estimate for these treated and control groups, based on one pre-event (mid-2015) year and one post-repeal (mid-2017) year of data, with Ferman and Pinto (2019) standard errors. The outcome variable is log income trimmed at the 1% and 90% level of the distribution of the log difference in income. Column (4) reports the difference-in-differences estimate on the increase in earnings for low-wage workers who stayed in Johnson County, compared to low-wage stayers in other untreated counties. In column (5) we consider a placebo treatment on high-income movers out of Johnson, who were never bound by the minimum wage, and compare them to other high-income movers out of other counties. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D News Coverage of Minimum Wage

Figure D1: Selected Newspaper Images

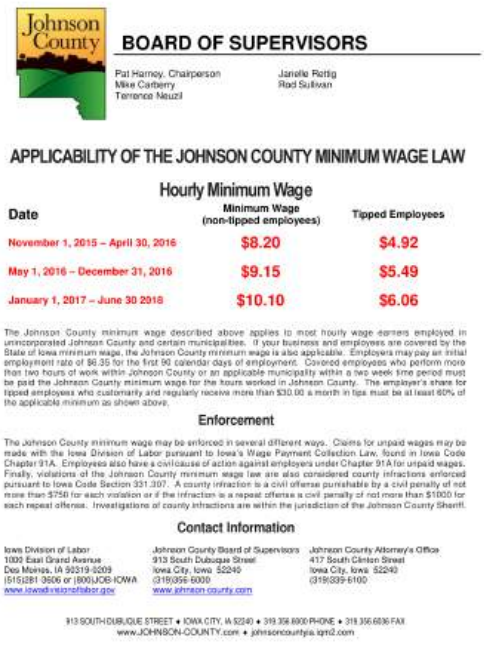
(a)



(b)



(c)



Johnson County

BOARD OF SUPERVISORS

Pat Harney, Chairperson
Mike Carberry
Terrence Neuzil
Janelle Rettig
Rod Sullivan

APPLICABILITY OF THE JOHNSON COUNTY MINIMUM WAGE LAW

Hourly Minimum Wage

Date	Minimum Wage (non-tipped employees)	Tipped Employees
November 1, 2015 – April 30, 2016	$8.20	$4.92
May 1, 2016 – December 31, 2016	$9.15	$5.49
January 1, 2017 – June 30 2018	$10.10	$6.06

The Johnson County minimum wage described above applies to most hourly wage earners employed in unincorporated Johnson County and certain municipalities. If your business and employees are covered by the State of Iowa minimum wage, the Johnson County minimum wage is also applicable. Employers may pay an initial employment rate of $6.35 for the first 90 calendar days of employment. Covered employees who perform more than two hours of work within Johnson County or an applicable municipality within a two week time period must be paid the Johnson County minimum wage for the hours worked in Johnson County. The employer's share for tipped employees who customarily and regularly receive more than $30.00 a month in tips must be at least 60% of the applicable minimum as shown above.

Enforcement

The Johnson County minimum wage may be enforced in several different ways. Claims for unpaid wages may be made with the Iowa Division of Labor pursuant to Iowa's Wage Payment Collection Law, found in Iowa Code Chapter 91A. Employees also have a civil cause of action against employers under Chapter 91A for unpaid wages. Finally, violations of the Johnson County minimum wage law are also considered county infractions enforced pursuant to Iowa Code Section 331.307. A county infraction is a civil offense punishable by a civil penalty of not more than $750 for each violation or if the infraction is a repeat offense a civil penalty of not more than $1000 for each repeat offense. Investigations of county infractions are within the jurisdiction of the Johnson County Sheriff.

Contact Information

Iowa Division of Labor
1000 East Grand Avenue
Des Moines, IA 50319-0209
(515)281-3606 or (800)JOB-IOWA
www.iowadivisionoflabor.gov

Johnson County Board of Supervisors
913 South Dubuque Street
Iowa City, Iowa 52240
(319)356-6000
www.johnson-county.com

Johnson County Attorney's Office
417 South Clinton Street
Iowa City, Iowa 52240
(319)339-6100

913 SOUTH DUBUQUE STREET ♦ IOWA CITY, IA 52240 ♦ 319.356.6000 PHONE ♦ 319.356.6036 FAX
www.JOHNSON-COUNTY.com ♦ johnsoncountyia.iqm2.com

(d)



Notes: **(a)** The minimum wage support sign can be seen at Shorts Burger & Shine on Monday, April 8, 2019. (The Daily Iowan, 2019); **(b)** Johnson County residents march in support of a minimum wage increase on Thursday, July 23, 2015. Source: (Iowa City Press-Citizen, 2015) ; **(c)** Poster of the minimum wage schedule announced in Johnson County in 2015 `https://www.johnsoncountyiowa.gov` ; **(d)** People participate in a workers' rights protest in Iowa City on Thursday, Sept. 14, 2017. Rally members advocated for higher minimum wage in protest of the state's decision to regulate county minimum wage laws (The Daily Iowan, 2017)

Table D1: Summary of Public Comments on the Minimum Wage (1/2)

#	Date	Name	Type	Role	Quotes	Context
(1)	2017-03-24	Travis Stovie	Employer	Managing director with 18 Burger King restaurants	"Once you're there, you're there. I don't see how you go back. I don't think that's feasible, that's too hard of a thing to do for any of our employees ... I just don't see it."	Talking about how he does not plan to cut raises that were forced by local wage increases, once House File 295 passed.
(2)	2017-03-24	Jessica Dunker	Employer	President and chief executive officer of the Iowa Restaurant Association	"I can't imagine anyone going backward, honestly"	Talking about difficulties of returning to a lower wage, once House File 295 passed.
(3)	2017-03-24	Tracey McWane	Employer	Owner of the Iowa City Dairy Queen	"If they started at $10.10, I'm not going to pay them less than that. I'm not going to take that away"	Talking about how he does not plan to cut raises that were forced by local wage increases, once House File 295 passed.
(4)	2017-07-04	Lesley Triplett	Employer	Owner of Dumpling Darling in Cedar Rapids and Iowa City.	"I just think that no matter what you do, you should be able to go to a good job, work hard and that should be enough if you're working full time to support yourself. With us, we always want to be the employer of choice and to do that, one of the key things is to pay a higher wage"	Explaining how she intends to keep the higher wage, says it's always been a priority to give her 30 employees a starting rate of $11 an hour.
(5)	2017-09-18	Ryan Sempf	Representative	Director of government relations and public policy at the Iowa City Area Chamber of Commerce	"I haven't heard many member businesses complain of adverse effects of the higher wage. We definitely haven't had anyone close and say it's because of the minimum wage or anything like that"	Said the chamber sent out a survey to its roughly 900 members after the Legislature nullified local wage increases in March. 78 percent of respondents said they would continue paying $10.10 or higher, while 12 percent were undecided and 10 percent said they would not continue paying above $10.10 for all employees.
(6)	2017-09-18	Rod Sullivan	Representative	Johnson County Supervisor	"I think there are people who are paying better than they were before this and realizing that it's not a bad thing for their businesses, and I think that there are people who are working and earning more money than they were. And I think that's all good."	Talking on how businesses are keeping wages high.
(7)	2017-09-18	Peter Fisher	Researcher	Researcher at the nonprofit Iowa Policy Project	"It's encouraging that many restaurants have found that they can survive and profit paying $10 an hour and that they're going to keep doing that. If they're maintaining $10.10 in what is probably the largest low-wage sector in the economy it probably tells us that it's maybe become the market wage and other places are going to have to pay that anyway"	Said there is a combination of reasons leading businesses to continue paying higher wages despite the law change: social benefits of publicly supporting the $10.10 wage, lower turnover costs due to employees earning more and staying at their jobs, and that the wage floor in the county has risen as a result of the increase. How much of that is due to public pressure from groups like the Center for Worker Justice is hard to tell, he said.

Notes: **(1)** The Gazette (2017b) ; **(2)** The Gazette (2017b) ; **(3)** The Gazette (2017b) ; **(4)** The Gazette (2017a) ; **(5)** Iowa City Press-Citizen (2017) **(6)** Iowa City Press-Citizen (2017) ; **(7)** Iowa City Press-Citizen (2017) ;

Summary of Public Comments on the Minimum Wage (2/2)

#	Date	Name	Type	Role	Quotes	Context
(8)	2018-03-21	Markess Wright	Employee	Works in food services	"I wouldn't be able to pay my rent on time if I didn't make this much money. My child would still be sleeping on an air mattress. I wouldn't have a car. I'm glad my boss supports a more livable wage"	Speaking about how his employer is keeping the wage high, he said he couldn't live off $7.25 an hour.
(9)	2018-03-21	Jonathan Wilson	Employer	Co-owner at the Bluebird Cafe	"The minimum wage increase we thought was long overdue so when it went up in Johnson County we had no problem with it. We were on board" "It's good for the county. Hopefully people are moving here because wages are higher here and expanding the labor base and the customer base so it's just growing the community"	Speaking in favour of the recent increase in the suggested minimum wage.
(10)	2022-03-25	Chad Simmons	Employer	Co-owner of SugaPeach	"We do not follow the minimum wage but what we do follow is the market. The market has forced us to keep competitive. We pay $12 an hour to start." "Keeping workers happy also keeps them around and I'm happy to do it, especially when finding workers at all is tougher now."	Speaking about the recent increase in the recommended wage.
(11)	2022-06-20	Grayson Lottes	Employee	Works for minimum wage at Iowa Chop House	"They don't have to do that, but let's be honest, no one is going to work for $7.25 nowadays. I do feel like I am being paid fairly, since I am 19 and a second-year student at the University of Iowa"	Speaks on how he is paid more than minimum wage. On a good night after workers are tipped out, he said he makes up to $25 an hour on tips alone, although on average it's between $11 and $12 a night. He said Iowa Chop House makes sure all of its workers reap the rewards of busy nights, so that everybody gets paid more and the business can attract more workers.
(12)	2022-06-20	Rod Sullivan	Representative	Johnson County Supervisor	"I think wages are on the rise in the county for a variety of factors, including market forces, the county's minimum wage and because workers have taken more control and are demanding more from their employers."	Speaks on how wages are being kept above minimum
(13)	2024-04-09	Kennedy Donovan	Employee	Employee at Yotopia	"I feel like if I was getting paid minimum wage, I feel like we kind of deserve more than that just because we're doing so much,"	Donovan said she now gets paid $11 in her current position at Yotopia in downtown Iowa City. Even though this is above the federal minimum wage, she still believes it's important to raise wages.

Notes: **(8)** KCRG (2017) ; **(9)** KCRG (2017) ; **(10)** CBS2 Iowa (2021) ; **(11)** Iowa City Press-Citizen (2022) ; **(12)** Iowa City Press-Citizen (2022) ; **(13)** The Daily Iowan (2024)

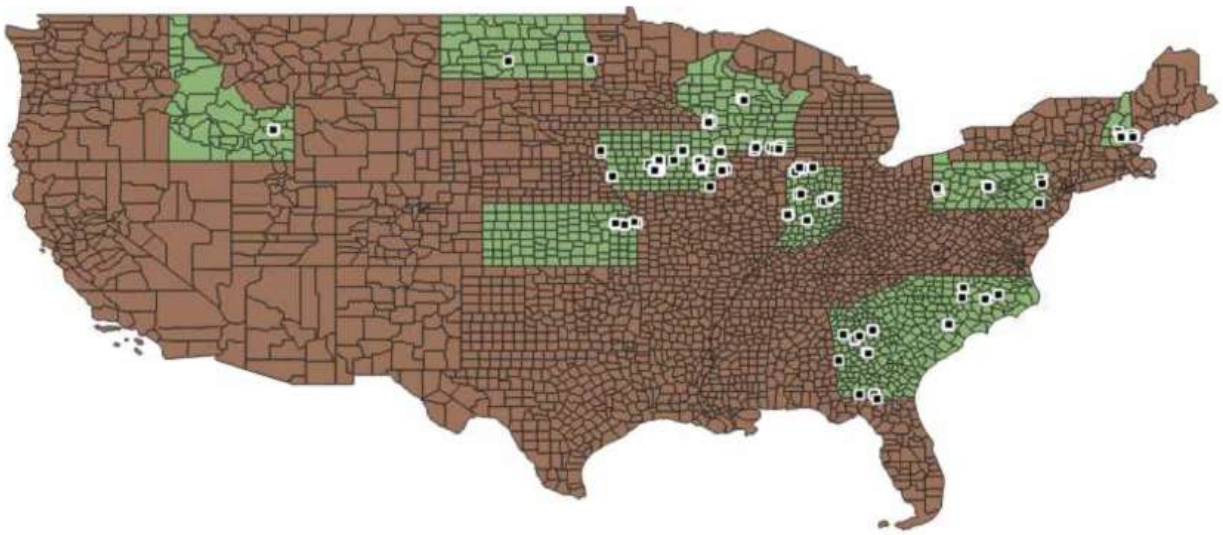
Appendix E Restaurant Price Series Data Collection

In this Appendix we describe the procedure we use to collect restaurant price data from online menus (the data used in the analysis in Section 4.2).

We start by using proprietary data from Data Axle Reference Solutions, a leading business and consumer data provider, in order to obtain a comprehensive listing of local restaurant establishments by geographic location (county). In addition to establishment address and employment information, Data Axle Reference Solutions also frequently provides a url for the restaurant's website. Utilizing the Wayback Machine, an online archive that stores snapshots of websites at different points in time, we then search for this url and for any and all stored menu snapshots available on the website that are timestamped at any point between 2011 and 2022. The infrequent and non-comprehensive storage procedures used by the Wayback Machine, and the lack of online menus for many restaurants, significantly limits the number of restaurants that can be included in the analysis. We thus supplement our menu database by occasionally using Yelp images of menus (posted by customers for a restaurant with a visible timestamp) and tourist websites available in certain counties that contain restaurant lists (which sometimes also have archived menus found via the Wayback Machine).

Upon downloading PDFs of a menu, we first use ChatGPT to collect every individual menu item and its corresponding price, storing this along with the date on which the menu was timestamped. To ensure data accuracy, for every menu item and price from a restaurant at a given point in time, a manual cross-check of this data is made against the original menu snapshots in the PDF, a time-consuming process. In total, this results in 303 unique restaurant establishments across 41 counties (none of them being national chains) with 18,629 unique restaurant-specific menu items that are used in our analysis. In addition to Johnson County, Iowa, the other 40 counties sampled include those drawn from the donor pool for our main earnings results that are most similar in population to Johnson County, Iowa. The location of all the restaurants in the price analysis sample can be found in the map below (Figure E1).

Figure E1: Geographical Distribution of Restaurants Used in Price Analysis



Notes: The map presents the geographical distribution of the restaurants for which we collected online menus used in the price analysis described in Section 4.2.

Appendix F Occupation-Based Approach

We complement the main analysis which uses industry-level (restaurant) data on earnings with data on hourly wages at the occupational level, covering occupations present in a wider range of sectors. The data come from the BLS Occupational Employment and Wage Statistics (OEWS), released at the MSA level. For our purpose, the main MSA of interest is Iowa City, which is entirely contained within Johnson County. We focus on major occupations (21 in total) in the period around the enaction of the minimum wage policy. OEWS releases annual estimates, which correspond to retrospective three-year averages (see e.g., Clemens et al. (2021)). For our analysis, we select the years 2012, 2015, 2018, and 2021 to best match the onset of timing of the policy shock under study. We restrict attention to MSAs with a balanced set of occupations, located in states only subject to the federal minimum wage.

We proceed in several ways. First, we provide a quintile-based approach, where we rank occupations into employment-weighted quintiles based on the 25th percentile of the occupation-specific wage distribution. This allows us to employ a difference-in-differences strategy comparing the evolution of mean hourly wages (within each quintile) for Iowa City versus the rest of the MSAs. The set of MSAs in the sample are drawn from states that are only subject to the federal minimum wage, mimicking the procedure in the main analysis. For each quintile j, we estimate the following model:

$$y_{omt} = \beta_1^j \cdot D_m \cdot \mathbb{1}[t > 2015] + \gamma_o^j + \rho_m^j + \lambda_t^j + \epsilon_{omt}$$

where y_{omt} refers to mean log hourly wage in occupation o, MSA region m and year t, D_m is a dummy taking value 1 for Iowa City, IA, $\mathbb{1}[\cdot]$ is an indicator function, γ_o are occupation fixed effects, ρ_m are MSA fixed effects and λ_t are year fixed effects. Specifications are weighted by average MSA-occupation pre-reform employment. Results are shown in Figure A7.

Second, we consider a dynamic version of that specification with alternative treatment measures. Specifically, we estimate models of the following form:

$$y_{omt} = \sum_{\tau=2012,2018,2021} \beta_\tau \cdot Treated_o \cdot Johnson_m \cdot \mathbb{1}[t = \tau] + \gamma_o + \rho_m + \lambda_t + \epsilon_{omt}$$

where $Johnson$ takes value 1 for Iowa City, and we define our exposure variable $Treated$ in several ways. First, it is defined to take value 1 for occupations where the 25th percentile of wages is below the new minimum wage, thus comparing these occupations with the rest of the occupations (and with other counties) in the estimation of β_τ. Second, $Treated_o$ is defined to equal the predicted percent wage increase required to bring the occupation's 25th percentile wage to the new Johnson minimum wage ($10.10). This is similar to the GAP measure (for 25th

percentile of wages), commonly used in the literature. Third, $Treated_o$ is defined as the share of individuals in the occupation below the new minimum wage. Fourth, $Treated_o$ equals the predicted average percent increase in wages needed for all workers in the occupation below the new minimum wage to reach it. The objective with the later three defnitions is not only to compare partially exposed occupations with those that are not exposed, but also to leverage a continuous measure of treatment intensity. For all specifications, regressions are weighted by average pre-reform occupation employment, and include MSA, occupation, and year fixed effects.

The first two measures are taken from the data directly, while the third and fourth ones are imputed (given OEWS data does not provide the full occupational wage distribution). The imputation procedure assumes a log-normal wage distribution within each occupation, similar to Davies et al. (2024). Using OEWS moments at different reported percentiles (10,25,75,90), and under the log-normal assumption, the standard deviation is estimated for each occupation. Specifically, we minimize the sum of squared errors between the logged OEWS wage percentiles and the log wage percentiles implied by a log-normal wage distribution with mean μ and standard deviation σ, where $\hat{\sigma}$ solves:

$$\hat{\sigma} = \min_{\sigma} \sum_{p \in P} \left(\log(\text{pct}_p) - \left(\mu + \Phi^{-1}(\frac{p}{100})\, \sigma \right) \right)^2$$

and $P = \{10, 25, 75, 90\}$, pct_p is the OEWS pth wage percentile, μ is the mean parameter/log OEWS median, and $\Phi^{-1}(\cdot)$ is the inverse standard normal CDF. The imputed share measure for a given threshold –the new statutory minimum wage– is used for the analysis. The imputed predicted average increase (a traditional GAP measure) similarly leverages the properties of the log-normal distribution. Results using the different definitions of the exposure variable are shown in Table A3.