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

Emiliano Huet-Vaughn Jon Piqueras

January, 2026

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).

*Emiliano Huet-Vaughn: Pomona College, IZA (emiliano.huet-vaughn@pomona.edu); Jon Piqueras: Universidad Carlos III de Madrid, Institute for Fiscal Studies (jon.piqueras@uc3m.es). First version: November 2023. We thank Ginevra Casini, Francesco Cavenaghi, Federico Pepe and Ronny Zhu for exceptional research assistance. We are also grateful for helpful comments from seminar participants at the All California Labor Conference at Stanford University, the 8th Bank of Italy-CEPR Workshop on Labor Market Policies and Institutions, the Workshop on Labor Markets and Public Policy at Bocconi University, Cal State Fullerton, CEPR Paris Symposium 2025, the European Labor Symposium for Early Career Economists, the Federal Reserve Bank of Kansas City, the Helsinki Workshop in Labor Economics, the International Association for Applied Econometrics Annual Meeting at the University of Macedonia, UC Berkeley, UC Santa Cruz, Universidad Carlos III de Madrid, and the Sixth World Labor Conference SOLE-EALE-AASLE 2025.

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, the hypotheses that we put forward, and empirically investigate, are twofold. One centers on firm-side learning:

that firms discover the 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.

We conduct several empirical tests to determine which of these mechanisms may be at play in our context. On the one hand, we assess the plausibility of the mechanisms related to firm learning. To do so, we leverage wage posting information from multi-establishment firms that

[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.

both posted restaurant job vacancies in Johnson prior to the revocation of the raised minimum wage *and* that also posted such vacancies in another county where their establishment is governed only by the federal minimum wage. If the policy raised profits, we would expect these firms to begin raising pay at their non-Johnson establishments as well sometime after 2015, as they learn that higher wages are profitable — consistent with well-documented evidence on the diffusion of profitable practices across establishments within the same firm (e.g., Bloom et al., 2013; 2020; Friebel et al., 2017; Giroud et al., 2024). Conversely, those firms outside Johnson that were never exposed to the elevated minimum wage, thus missing out on the revelation that higher pay can be profit enhancing, would not be expected to raise their workers' wages following a policy that they never experienced. Our results do not support the prediction that firms learn about the profitability of paying higher wages to their workers. Instead, they align with broader US evidence indicating that increases in the minimum wage generally do not lead to higher profits (Vergara, 2025).

On the other hand, to investigate the second group of explanations, we focus on reservation wages and related measures of worker expectations. We provide several pieces of evidence suggesting that the findings highlighted above reflect a shift in workers' perceptions. First, 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. The quantitative analysis is complemented by consistent qualitative evidence from interviews in local newspapers. Second, leveraging longitudinal individual-level data, we investigate whether income gains in Johnson are portable when individuals move to other locations. Specifically, we compare the changes in income of low-wage workers who move out of Johnson County after exposure to the elevated minimum wage relative to movers from other counties. The reservation wage mechanism predicts that they should take their upwardly shocked wage expectations with them and hold out for higher-paying jobs than movers from other counties would. The firm-learning mechanisms predict no such thing since the firms that predominate in the non-Johnson counties were never treated and thus firm-side explanations are irrelevant. Despite some empirical limitations, the evidence appears broadly consistent with the reservation wage mechanism. Taken together, these findings suggest that the policy contributed to a persistent

shift in perceptions around fair pay, even after legal incentives were no longer present.

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]

[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.

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 set up. 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 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

[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.

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 describes the institutional context of the reform under study. Section 3 describes the data used. Section 4 presents the empirical methodology. Section 5 reports the main results. Section 6 explores the potential mechanisms, and Section 7 concludes.

2 Institutional 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]

[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

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, 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

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).

[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 B 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).

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]

3 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

[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 B1.

[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).

[16]See the Bureau of Labor Statistics county-level QCEW files at https://www.bls.gov/cew/.

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. 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 is disaggregated by tenure with a firm, allowing us to complement the main results with additional insights on the earnings for new hires.

Posted Wages. 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).[17] When the posted wage is recorded as a range, we use the minimum of that range.

[17]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 in a moderately sized county like Johnson).

Reservation Wages. In testing a proposed mechanism underlying our results, we also draw on audit data from the Benefit Accuracy Measurement (BAM) program.[18] 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 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 scraped, 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 C.

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, 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

[18] For more on BAM data see e.g., Woodbury (2002), Jaimovich et al. (2020), and Skandalis et al. (2022).

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

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.

Third, 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.

4 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 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 can then estimate the parameter $\tilde{\tau}_{ect} = Y_{ect}^I - \hat{Y}_{ect}^N$. 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

[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).

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

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

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 Figure A3, 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.

5 Results

5.1 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

[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.

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 4, 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 A4. 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, Panel (b) of Figure A4 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

[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.

earnings effects of the minimum wage policy corresponding to Figure A4, 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 A4, 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 A4), 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 A5, 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 between treated and control units, deliver very similar estimates to those in Figure 3 and Figure 4, underscoring the robustness of our findings.

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 A6 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 3), the

[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.

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 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

[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).

[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.

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 D, 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.

5.2 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 web scraping procedure (see Section 3). Nonetheless, the novel database constructed offers valuable insights into price-setting behavior. We estimate the following regression equation:

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

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

[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.

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).[34] In the next section we explore the mechanisms that may underlie our hysteresis results.

6 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 existing literature offers some possible explanations.

In terms of when temporary labor policies give rise to wage effects that are persistent over time, the evidence 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.

In terms of mechanisms explaining why such hysteresis may take place, one cluster of explanations involves mechanisms that appeal in some way to firms learning from forced

[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.

[34] This finding is challenging to reconcile with the canonical monopsony model, described in Kline (2025), though the addition of efficiency wage considerations (which would *flatten* the effective labor supply to the firm, thereby reducing the incentives of employers to suppress wages after the minimum wage is raised) or shifts to worker reservation wages as the minimum wage increases (thus shifting labor supply to the firm) would alter the predictions of this standard model. Evidence for these additional channels are discussed in the next section.

experimentation (e.g., Bloom et al., 2015; Larcom et al., 2017). That is, by being required by law to pay a higher minimum wage, firms may learn that offering higher pay is actually profitable and continue to do so after the revocation of the requirement.[35] Explanations of this type are varied. For instance, efficiency wage theories predict greater productivity from existing workers in response to higher wages (Akerlof, 1982; Stiglitz, 1984; Ku, 2022; Coviello et al., 2022). Higher pay is also known to reduce turnover costs due to increased job attachment (Dube et al., 2016) and reduction in time needed to fill vacancies (Cullen et al., 2023). Other work indicates higher minimum wages lead to worker compositional shifts through the hiring of more productive workers (Butschek, 2022). In each of these cases, the mechanism could motivate firms to continue on the elevated wage path even after they are no longer required to be, because, again, they learn it is profitable to do so.

Distinct from these mechanisms is an alternative hypothesis. Rather than firms learning something new about how to operate more profitably, the new minimum wage may have taught workers what to expect as a "fair" wage and this permanently changes their reservation wage and future bargaining behavior, consistent with the lab experimental findings in Falk et al. (2006).[36] In such a case, firms continue to offer high wages after the minimum wage falls not because it increases their profits, but, because they realize they are facing workers who have come to demand higher pay and more of the total surplus, and who they will not be able to attract or keep should they choose a lower (now legally-allowed) wage.[37]

6.1 When Does Earnings Hysteresis Occur?

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

[35]This relates to the role of X-efficiency (Leibenstein, 1966) in understanding the impact of minimum wage policies, and presupposes some sub-optimizing behavior of firms in the status quo (Lazear, 2000; Emanuel and Harrington, 2025).

[36]That an increased minimum wage would actually raise workers' expectations about the wage they should expect may not be surprising given the wrong understanding that workers have about their pay options (Jäger et al., 2024; Cullen et al., 2025).

[37]Note the reservation wage channel has a clear prediction on increased nominal pay after the repeal. The effects found through the inflationary period would also imply reservation wages are permanently impacted in real, and not only nominal terms. A natural explanation consistent with this result is pay equity considerations (Dube et al., 2019), where employers are not able to differentiate the growth rate of pay between different workers. A complementary channel is the recommended (evolving) non-binding wage guidelines that have been advertised by Johnson local officials since 2017 — recommendations which local officials and businesses attest, in conversations with the authors, to be purely informational and which carry no negative consequences from the government if ignored.

would take effect shortly after passage).[38]

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 4 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 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

[38]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.

a policy taking place may induce persistent earning gains.

6.2 Firm-Learning Based Mechanism

To develop a test that discriminates between alternative mechanisms, we first 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.[39] Specifically, we focus on posted wages of firms that both operated in Johnson County prior to the revocation of the minimum wage (*i.e.*, before the second quarter of 2017) and that also had establishments outside of Johnson County.[40] Our interest is in the development of these firms' wage postings in their non-Johnson County establishments in comparison to other firms operating outside of Johnson County (and that never operated in Johnson County prior to the second quarter of 2017). The first group can be thought of as firms "treated" by indirect exposure to the high local minimum wage policy (since some of their stores had to deal with operating under it), while the second group was not exposed and serves as a control.

What do the different mechanisms outlined above predict with respect to the wages posted in non-Johnson establishments by treated versus control firms? Under a firm-learning based mechanism, if low-wage firms discover that their Johnson County establishments are more profitable as a result of paying their workers more, one might expect these firms to raise wages in their non-Johnson establishments in order to experience similar gains there.[41] Accordingly, an empirically testable prediction is that the posted wage differential in non-Johnson counties should grow between treated and control firms after the minimum wage increase relative to before the policy onset.[42] The reservation wage mechanism, by contrast, predicts no such divergence

[39] As described in Section 3, 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 (online and offline) offered wage.

[40] Firm presence in either place is measured by the firm posting a job with wage information there.

[41] The diffusion of best practices across establishments within the same firm is well documented in the existing literature. Friebel et al. (2017) show that a profitable bonus pay policy, initially experimentally assigned to only some branches, is eventually rolled out by management to all of their establishments. Bloom et al. (2020) find that beneficial management practices (such as inventory management and quality control) learned at one establishment are passed onto others run by the same firm. Giroud et al. (2024) find the productivity gains for a new entrant into an agglomerated region are also passed on to distant establishments run by that firm.

[42] Growth in the gap between treated and control firms after the introduction of the minimum wage would also be consistent with "national wage setting" if firms attempt to harmonize pay for workers across establishments by bringing non-Johnson establishments into line with Johnson regulations. In this case, the gap growth would not necessarily reflect any learning from forced experimentation about what is profitable, but arise as result of the constraint by firms to set equal wages across establishments in different locations. However, note that firms engaging in national wage setting typically comprise a small share of the labor market, and local shocks to wages do not lead to wage adjustment in other establishments on average when considering all firms as shown by Hazell et al. (2022). As a consequence, we do not expect the uniform wage setting component to dominate the firm-learning

because the treated firms are not experiencing any increase in profit from paying workers more. Rather, under this alternative mechanism, they are effectively forced to raise Johnson wages even though it reduces their profit because the policy and subsequent increase in the reservation wage for Johnson County workers is binding. As workers outside Johnson County have no shock to their reservation wage, it would not be optimal to pay them more.

We implement a difference-in-differences strategy to empirically assess the relevance of this firm-based mechanism.[43] Table 3 shows the results. First, column (1) indicates that firms directly affected by the minimum wage policy increase their posted wages for jobs located within Johnson. This specification compares the wage posting behavior of establishments inside Johnson vs. establishments of a control group of firms that only posted outside the county. The 10.7% increase in earnings, similar to our main estimates in Section 5.1, implies that the policy is binding, and thus may in principle have the ability to shift pay policy in other establishments. We test this channel in column (2), where it is shown that directly affected firms do not seem to change their posted wages in counties outside Johnson relative to unaffected firms. The estimated impact, β_1 from equation (6), is 0.004 (s.e. 0.029).[44] Thus, results do not support the firm-learning based mechanism prediction that firms are extending the pay increase experienced in Johnson County to their non-Johnson County establishments.[45]

effect and, in any case, we do not find that the policy impacts growth in the wage gap, which we view as stronger evidence against the firm-learning mechanism given the presence of this additional channel.

[43]We estimate the following regression equation:

$$y_{ict} = \beta_0 + \beta_1 \cdot D_c \cdot \mathbb{1}[t \geq 2015q4] + \gamma_i + \rho_c + \lambda_t + \epsilon_{ict} \tag{6}$$

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 (and thus exposed to the policy) and the value of zero otherwise, $\mathbb{1}[\cdot]$ is an indicator function, 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. Data spans over the period 2013-2018 and comprises counties located in Iowa and the nearby states without a state-level minimum wage, Kansas and Wisconsin (alternatively, using jobs posted in all states under the federal minimum wage yields a similar pattern of results, though this leads to poorer pre-trends).

[44]Including occupation fixed effects in Equation 6 yields essentially the same (null) result, with a coefficient of -0.014 (s.e. 0.032).

[45]The 95% confidence interval in column (2) of Table 3 rules out a change of about 5.5% in posted wages after the reform. To be clear, this finding may result from minimum wages not affecting productivity in the first place, or from the potential increase in productivity as a consequence of the policy not being enough to increase profits after accounting for the increased wage bill. While we are not able to assess the necessary condition that the minimum wage increases profits in the treated establishments in our setting, broader findings from the US literature generally suggest that minimum wage increases do not lead to higher profits (Dube and Lindner, 2024; Vergara, 2025). Moreover, in line with our results, related evidence from a large multi-establishment US firm with ex-ante nationwide compensation scheme indicates that the minimum wage only shifted wages at directly affected establishments, with unaffected ones maintaining the same remuneration scheme as before the policy change (Coviello et al., 2022). Nonetheless, regardless of the specific reason, in our case, the firm-learning mechanism is not supported by the data.

6.3 Worker Reservation Wage Mechanism

The previous result casts doubt on the firm-learning-by-forced-experimentation mechanism. We now offer suggestive evidence for the importance of the worker reservation wage channel. Specifically, we investigate the link between the minimum wage and (i) reservation wages, and (ii) movers' income.

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).[46] So, we do the next best thing and test the prediction of the mechanism out of sample, 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).[47] 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.[48] 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.[49]

[46] 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 County, Iowa.

[47] 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.

[48] 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.

[49] Specifically, low-wage respondents are identified by their answer to the BAM survey question asking about their usual hourly wage (see Section 3).

We implement a stacked design (Cengiz et al., 2019) for the event-study analysis.[50]

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.[51] 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.[52] Panel (b) of Figure 6 plots the gap in 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.[53] These findings show a clear positive link between the minimum wage and reservation wages, highlighting the strong empirical validity of the mechanism. Of course, in this case the one-time increase in the minimum wage is permanent, whereas in Johnson it was temporary. Therefore, while the evidence presented using BAM data is consistent with the hypothesis that a policy-induced shift in reservation wages underlies the earnings hysteresis documented in Johnson County, it does not constitute a direct test. Given the absence of reservation wage data identifying individuals in Johnson County, we next complement this analysis by turning to microdata covering Johnson that measures individual downstream outcomes correlated with reservation wages.

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

$$y_{hst} = \sum_{\tau=-3, \tau \neq -1}^{\tau=4} \beta_\tau \cdot D_{hs} \cdot \mathbb{1}[t=\tau] + \mu_{hs} + \rho_{ht} + u_{hst} \tag{7}$$

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).

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

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

[53] 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.

Income Changes of Movers. We exploit the longitudinal structure of the L2 data and its measure of household income.[54] 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 (rather than solely by persistent changes in firm behavior), then we would expect these movers who left Johnson County after exposure to the policy to carry their positively shocked reservation wage with them, and, to hold out for higher-paying jobs when facing new (untreated) firms upon relocating. Thus, that would imply they enjoy larger earnings gains in their new counties relative to movers from other, untreated counties.[55]

We empirically test this prediction of the reservation wage mechanism among movers applying a difference-in-differences strategy at the individual unit of analysis. Treatment status is assigned based on the individual's location being Johnson County immediately prior to the reform (as revealed by voter registration address) and not after the reform. That is, the treated group 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 (excluding Johnson) during the same period. The outcome variable is log household income. To mitigate the influence of outliers, particularly very high income growth individuals who are unlikely to actually be affected by the minimum wage, we trim the sample at the 1st and 90th percentiles of the distribution of log income changes.[56] The analysis is based on one pre-minimum-wage-event (mid-2015) year and one post-repeal (mid-2017) year of data.[57] The empirical strategy identifies the effects of the policy on income changes under the assumption that, absent the reform, movers from Johnson would have exhibited similar trajectories as movers from other counties.[58]

[54]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/.

[55]Since the minimum wage hike in Johnson simultaneously affected both Johnson's low-income workers and the firms employing them, this movers test allows us to focus only on (a subset of) treated workers, to assess the long-term income effects of the policy, *independent* of firm behavior. This test mirrors the analysis in Section 6.2 which instead isolated a potential firm-side independent impact of the treatment on affected firms *sans* treated workforce.

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

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

[58]Selection concerns thus warrant caution in interpreting the results. One such concern is that movers from Johnson are naturally more likely to move to high income counties for reasons other than minimum wage policy and this would explain earnings growth above that of the control group. However, when controlling by pre-reform average income in the destination county, results are very similar, indicating the higher individual earnings we see upon moving from Johnson are probably not driven by individuals there being more likely to move to high income counties. Additionally, the reform could affect the selection of individuals into migration, making it difficult to isolate the impact of minimum wage exposure from potential changes in the composition of movers. However, empirically, there is not a significant change brought on by the minimum wage increase in the propensity of low-income Johnson residents to move over the period of study.

The results are shown in Table 3. In column (3), the sample is restricted to include only low-income earners (bottom 20% of earners in the state in the pre-treatment year) who went on to move to another county in Iowa. Results for this sample indicate that the estimated increase in income for movers from Johnson County is on the order of about 9%. This effect is significant at the 5% level and consistent with a positively-shocked reservation wage for workers following exposure to the higher minimum wage in Johnson. For comparison, column (4) reports the estimated change in income for low-income workers who stayed in Johnson County (compared to a group of low-income stayers in other counties). The estimated income increase for this group of individuals staying in Johnson is very similar in magnitude and significance to that of movers out of Johnson. As a placebo test, in column (5), we also consider high-income movers out of Johnson and compare them to other high-income movers out of other counties. As expected, there is no significant increase in their income, as these individuals were unaffected by the minimum wage shock and thus did not experience changes in their reservation wages. Overall, these results are consistent with the reservation wage mechanism. However, because we observe income rather than reservation wages directly, these particular findings offer suggestive rather than conclusive evidence of the underlying mechanism.

Taken together, we view the evidence presented in Section 6 as supportive of the reservation wage channel. Despite some empirical challenges, the findings broadly align with the hypothesis that the policy was able to shift perceptions in the labor market, and are difficult to reconcile with an alternative firm-learning-based explanation.

7 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.

References

Abadie, A., A. Diamond and J. Hainmueller, "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program," *Journal of the American statistical Association* 105 (2010), 493–505.

———, "Comparative politics and the synthetic control method," *American Journal of Political Science* 59 (2015), 495–510.

Abadie, A. and J. Gardeazabal, "The economic costs of conflict: A case study of the Basque Country," *American Economic Review* 93 (2003), 113–132.

Abadie, A. and J. L'Hour, "A penalized synthetic control estimator for disaggregated data," *Journal of the American Statistical Association* 116 (2021), 1817–1834.

Abadie, A. and J. Vives-i Bastida, "Synthetic controls in action," (2022).

Abowd, J. M., R. K. Gittings, K. L. McKinney, B. Stephens, L. Vilhuber and S. D. Woodcock, "Dynamically consistent noise infusion and partially synthetic data as confidentiality protection measures for related time series," *US Census Bureau Center for Economic Studies Paper No. CES-WP-12-13* (2012).

Akerlof, G. A., "Labor contracts as partial gift exchange," *The Quarterly Journal of Economics* 97 (1982), 543–569.

Akerlof, G. A., W. T. Dickens, G. L. Perry, R. J. Gordon and N. G. Mankiw, "The macroeconomics of low inflation," *Brookings Papers on Economic Activity* 1996 (1996), 1–76.

Akerlof, G. A. and J. L. Yellen, "The fair wage-effort hypothesis and unemployment," *The Quarterly Journal of Economics* 105 (1990), 255–283.

Allcott, H., L. Boxell, J. Conway, M. Gentzkow, M. Thaler and D. Yang, "Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic," *Journal of Public Economics* 191 (2020), 104254.

Ashenfelter, O. and Š. Jurajda, "Minimum wages, wages, and price pass-through: The case of McDonald's restaurants," *Journal of Labor Economics* 40 (2022), S179–S201.

Autor, D., A. Dube and A. McGrew, "The unexpected compression: Competition at work in the low wage labor market," Technical Report, National Bureau of Economic Research, 2023.

Azar, J., E. Huet-Vaughn, I. Marinescu, B. Taska and T. Von Wachter, "Minimum wage employment effects and labour market concentration," *Review of Economic Studies* 91 (2024), 1843–1883.

Batra, H., A. Michaud and S. Mongey, "Online job posts contain very little wage information," Technical Report, National Bureau of Economic Research, 2023.

Ben-Michael, E., A. Feller and J. Rothstein, "The augmented synthetic control method," *Journal of the American Statistical Association* 116 (2021), 1789–1803.

Benzarti, Y., D. Carloni, J. Harju and T. Kosonen, "What goes up may not come down: asymmetric incidence of value-added taxes," *Journal of Political Economy* 128 (2020), 4438–4474.

Bewley, T. F., *Why wages don't fall during a recession* (Harvard university press, 1999).

Blanchard, O. J. and L. H. Summers, "Hysteresis and the European unemployment problem," *NBER Macroeconomics Annual* 1 (1986), 15–78.

Bloom, N., B. Eifert, A. Mahajan, D. McKenzie and J. Roberts, "Does management matter? Evidence from India," *The Quarterly journal of economics* 128 (2013), 1–51.

Bloom, N., J. Liang, J. Roberts and Z. J. Ying, "Does working from home work? Evidence from a Chinese experiment," *The Quarterly journal of economics* 130 (2015), 165–218.

Bloom, N., A. Mahajan, D. McKenzie and J. Roberts, "Do management interventions last? Evidence from India," *American Economic Journal: Applied Economics* 12 (2020), 198–219.

Bockerman, P., T. Juuti, T. Kosonen and H. Keränen, "Are Firms Willing to Pay Lower Wages? A Quasi-Experiment on Subminimum Wage Policy," (2024).

Borusyak, K., X. Jaravel and J. Spiess, "Revisiting Event Study Designs: Robust and Efficient Estimation," *Available at SSRN 2826228* (2022).

Brandon, A., P. J. Ferraro, J. A. List, R. D. Metcalfe, M. K. Price and F. Rundhammer, "Do the effects of nudges persist? theory and evidence from 38 natural field experiments," Technical Report, National Bureau of Economic Research, 2017.

Brochu, P. R., D. A. Green, T. Lemieux and J. H. Townsend, "The minimum wage, turnover, and the shape of the wage distribution," Technical Report, National Bureau of Economic Research, 2025.

Bryan, G., S. Chowdhury and A. M. Mobarak, "Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh," *Econometrica* 82 (2014), 1671–1748.

Burning Glass Technologies, "Database of Vacancies posted online," (2018).

Butschek, S., "Raising the bar: minimum wages and employers' hiring standards," *American Economic Journal: Economic Policy* 14 (2022), 91–124.

Callaway, B. and P. H. Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics* 225 (2021), 200–230.

Card, D. and D. Hyslop, "Does inflation" grease the wheels of the labor market"?," in *Reducing inflation: Motivation and strategy* (University of Chicago Press, 1997), 71–122.

Card, D. and B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *The American Economic Review* 84 (1994), 772–793.

CBS2 Iowa, "Johnson County's new minimum wage suggestion still lower than livable," *CBS2 Iowa* (2021), available at https://cbs2iowa.com/news/local/johnson-countys-new-minimum-wage-suggestion-still-lower-than-livable.

Cengiz, D., A. Dube, A. Lindner and B. Zipperer, "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics* 134 (2019), 1405–1454.

Charness, G. and U. Gneezy, "Incentives to exercise," *Econometrica* 77 (2009), 909–931.

Clemens, J., L. B. Kahn and J. Meer, "Dropouts need not apply? The minimum wage and skill upgrading," *Journal of Labor Economics* 39 (2021), S107–S149.

Costa, F. and F. Gerard, "Hysteresis and the welfare effect of corrective policies: Theory and evidence from an energy-saving program," *Journal of Political Economy* 129 (2021), 1705–1743.

Coviello, D., E. Deserranno and N. Persico, "Minimum wage and individual worker productivity: Evidence from a large US retailer," *Journal of Political Economy* 130 (2022), 2315–2360.

Cullen, Z., M. Hoffman and F. Koenig, "Labor Shortages and Firm Search," Technical Report, 2023.

Cullen, Z., B. Pakzad-Hurson and R. Perez-Truglia, "Pushing the Envelope: The Effects of Salary Negotiations," (2025).

Davies, M., R. J. Park and A. Stansbury, "Minimum wages and workplace injuries," *Available at SSRN* (2024).

Derenoncourt, E., F. Gérard, L. Lagos and C. Montialoux, "Inequality, minimum wage spillovers, and the informal sector," *Unpublished manuscript* 6 (2025).

Derenoncourt, E. and C. Montialoux, "Minimum wages and racial inequality," *The Quarterly Journal of Economics* 136 (2021), 169–228.

Des Moines Register, "Will a higher minimum wage raise prices?," *Des Moines Register* (2016), available at https://eu.desmoinesregister.com/story/news/2016/09/26/higher-minimum-wage-raise-prices/90853742/.

Doudchenko, N. and G. W. Imbens, "Balancing, regression, difference-in-differences and synthetic control methods: A synthesis," Technical Report, National Bureau of Economic Research, 2016.

Dube, A., L. Giuliano and J. Leonard, "Fairness and frictions: The impact of unequal raises on quit behavior," *American Economic Review* 109 (2019), 620–663.

Dube, A., T. W. Lester and M. Reich, "Minimum wage shocks, employment flows, and labor market frictions," *Journal of Labor Economics* 34 (2016), 663–704.

Dube, A. and A. Lindner, "Minimum wages in the 21st century," *Handbook of Labor Economics* 5 (2024), 261–383.

Dube, A. and B. Zipperer, "Own-wage elasticity: Quantifying the impact of minimum wages on employment," Technical Report, National Bureau of Economic Research, 2024.

Dupraz, S., E. Nakamura and J. Steinsson, "A plucking model of business cycles," Technical Report, National Bureau of Economic Research, 2019.

Dustmann, C., A. Lindner, U. Schönberg, M. Umkehrer and P. Vom Berge, "Reallocation effects of the minimum wage," *The Quarterly Journal of Economics* 137 (2022), 267–328.

Elsby, M. W., "Evaluating the economic significance of downward nominal wage rigidity," *Journal of Monetary Economics* 56 (2009), 154–169.

Emanuel, N. and E. Harrington, "The Payoffs of Higher Pay: Labor Supply and Productivity Responses to a Voluntary Minimum Wage," Technical Report, Working Paper, 2025.

Falk, A., E. Fehr and C. Zehnder, "Fairness perceptions and reservation wages: the behavioral effects of minimum wage laws," *The Quarterly Journal of Economics* 121 (2006), 1347–1381.

Fedorets, A. and C. Shupe, "Great expectations: Reservation wages and minimum wage reform," *Journal of Economic Behavior & Organization* 183 (2021), 397–419.

Ferman, B., "On the properties of the synthetic control estimator with many periods and many controls," *Journal of the American Statistical Association* 116 (2021), 1764–1772.

Ferman, B. and C. Pinto, "Inference in differences-in-differences with few treated groups and heteroskedasticity," *Review of Economics and Statistics* 101 (2019), 452–467.

———, "Synthetic controls with imperfect pretreatment fit," *Quantitative Economics* 12 (2021), 1197–1221.

Fishback, P. V. and A. J. Seltzer, "The rise of American minimum wages, 1912–1968," *Journal of Economic Perspectives* 35 (2021), 73–96.

Freeman, R. B., W. B. Gray and C. Ichniowski, "Low-Cost Student Labor: The Use and Effects of the Subminimum Wage Provisions for Full-time Students," Technical Report, 1981.

Friebel, G., M. Heinz, M. Krueger and N. Zubanov, "Team incentives and performance: Evidence from a retail chain," *American Economic Review* 107 (2017), 2168–2203.

Giné, X., D. Karlan and J. Zinman, "Put your money where your butt is: a commitment contract for smoking cessation," *American Economic Journal: Applied Economics* 2 (2010), 213–235.

Giroud, X., S. Lenzu, Q. Maingi and H. Mueller, "Propagation and amplification of local productivity spillovers," *Econometrica* 92 (2024), 1589–1619.

Giuliano, L., "Minimum wage effects on employment, substitution, and the teenage labor supply: Evidence from personnel data," *Journal of Labor Economics* 31 (2013), 155–194.

Giupponi, G. and S. Machin, "Company wage policy in a low-wage labor market," (2025).

Goldin, C. and R. A. Margo, "The great compression: The wage structure in the United States at mid-century," *The Quarterly Journal of Economics* 107 (1992), 1–34.

Harasztosi, P. and A. Lindner, "Who pays for the minimum wage?," *American Economic Review* 109 (2019), 2693–2727.

Hazell, J., C. Patterson, H. Sarsons and B. Taska, "National wage setting," Technical Report, National Bureau of Economic Research, 2022.

Hazell, J. and B. Taska, "Downward Rigidity in the Wage for New Hires," *American Economic Review* (2025), forthcoming.

Iowa City Press-Citizen, "Residents rally in support of minimum wage increase," *Iowa City Press-Citizen* (2015), available at https://eu.press-citizen.com/story/news/local/2015/07/23/residents-rally-support-minimum-wage-increase/30598833/.

———, "Johnson County minimum wage group claims over 140 businesses committed to paying $10.10," *Iowa City Press-Citizen* (2017), available at https://eu.press-citizen.com/story/news/local/2017/09/18/johnson-county-minimum-wage-group-claims-over-140-businesses-committed-paying-10-10/676725001/.

———, "Johnson County, Iowa City jobs: Entry-level workers find much more than minimum wage," *Iowa City Press-Citizen* (2022), available at https://eu.press-citizen.com/story/news/2022/06/20/johnson-county-iowa-city-jobs-entry-level-workers-find-much-more-than-minimum-wage/7645992001/.

Ito, K., "Asymmetric incentives in subsidies: Evidence from a large-scale electricity rebate program," *American Economic Journal: Economic Policy* 7 (2015), 209–237.

Jäger, S., C. Roth, N. Roussille and B. Schoefer, "Worker beliefs about outside options," *The Quarterly Journal of Economics* 139 (2024), 1505–1556.

Jaimovich, N., D. Ferraro, F. Molinari and C. Young, "The Safety Net as a Springboard? A General Equilibrium based Policy Evaluation," (2020).

Katz, L. F. and A. B. Krueger, "The effect of the minimum wage on the fast-food industry," *ILR Review* 46 (1992), 6–21.

Kaul, A., S. Klößner, G. Pfeifer and M. Schieler, "Standard synthetic control methods: The case of using all preintervention outcomes together with covariates," *Journal of Business & Economic Statistics* 40 (2022), 1362–1376.

Kaur, S., "Nominal wage rigidity in village labor markets," *American Economic Review* 109 (2019), 3585–3616.

KCRG, "Some Johnson Co. businesses in favor of higher minimum wage, employees feel the benefits," *KCRG-TV* (2017), available at https://www.kcrg.com/content/news/Some-Johnson-Co-businesses-in-favor-of-higher-minimum-wage-employees-feel-the-benefit
html.

Keynes, J. M., "The general theory of interest, employment and money," (1936).

Kline, P. M., "Labor Market Monopsony: Fundamentals and Frontiers," *Handbook of Labor Economics* 6 (2025), forthcoming.

Ku, H., "Does minimum wage increase labor productivity? Evidence from piece rate workers," *Journal of Labor Economics* 40 (2022), 325–359.

Larcom, S., F. Rauch and T. Willems, "The benefits of forced experimentation: Striking evidence from the London underground network," *The Quarterly Journal of Economics* 132 (2017), 2019–2055.

Lazear, E. P., "Performance pay and productivity," *American Economic Review* 90 (2000), 1346–1361.

Leibenstein, H., "Allocative efficiency vs." X-efficiency"," *The American Economic Review* 56 (1966), 392–415.

McGovern, T., "United States General Election Presidential Results by District, Ward, or County from 2008 to 2024," (2012), available at https://github.com/tonmcg/US_County_Level_Election_Results_08-24Tony McGovern's election repository.

Miller, C., "The persistent effect of temporary affirmative action," *American Economic Journal: Applied Economics* 9 (2017), 152–190.

Quach, S., "Wage Hysteresis and Entitlement Effects: The Persistent Impacts of a Temporary Overtime Policy," *The Quarterly Journal of Economics* (2025), qjaf008.

Raffiee, J., D. Fehder and F. Teodoridis, "Revealing the revealed preferences of public firm CEOs and top executives: A new database from credit card spending," *Strategic Management Journal* 43 (2022), 2042–2065.

Rao, N. and M. Risch, "Who's Afraid of the Minimum Wage? Measuring the Impacts on Independent Businesses Using Matched US Tax Returns," (2025).

Renkin, T., C. Montialoux and M. Siegenthaler, "The Pass-Through of Minimum Wages into U.S. Retail Prices: Evidence from Supermarket Scanner Data," *The Review of Economics and Statistics* 104 (2022), 890–908.

Rubin, D. B., "Estimating causal effects of treatments in randomized and nonrandomized studies.," *Journal of Educational Psychology* 66 (1974), 688.

Saez, E., B. Schoefer and D. Seim, "Payroll taxes, firm behavior, and rent sharing: Evidence from a young workers' tax cut in Sweden," *American Economic Review* 109 (2019), 1717–1763.

———, "Hysteresis from employer subsidies," *Journal of Public Economics* 200 (2021), 104459.

Schmitt-Grohé, S. and M. Uribe, "Downward nominal wage rigidity and the case for temporary inflation in the eurozone," *Journal of Economic Perspectives* 27 (2013), 193–212.

Skandalis, D., I. Marinescu and M. N. Massenkoff, "Racial inequality in the US unemployment insurance system," Technical Report, National Bureau of Economic Research, 2022.

Stansbury, A., "Incentives to comply with the minimum wage in the United States and the United Kingdom," *ILR Review* 78 (2025), 190–216.

Stiglitz, J. E., "Theories of wage rigidity," Technical Report, National Bureau of Economic Research, 1984.

The Daily Iowan, "Guest opinion: Research favors workers, unions," *The Daily Iowan* (2017), available at https://dailyiowan.com/2017/09/19/guest-opinion-research-favors-workers-unions/.

———, "Johnson County's symbolic minimum wage to increase in July," *The Daily Iowan* (2019), available at https://dailyiowan.com/2019/04/10/johnson-countys-symbolic-minimum-wage-to-increase-in-july/.

———, "Johnson County Board of Supervisors call for minimum wage increase," *The Daily Iowan* (2024), available at https://dailyiowan.com/2024/04/09/johnson-county-board-of-supervisors-call-for-minimum-wage-increase/.

The Gazette, "Advocates continue working for higher minimum wages in Linn and Johnson counties," *The Gazette* (2017a), available at https://www.thegazette.com/news/advocates-continue-working-for-higher-minimum-wages-in-linn-and-johnson-counties/.

———, "Minimum wage pre-emption might not lower wages for all," *The Gazette* (2017b), available at https://www.thegazette.com/government-politics/minimum-wage-pre-emption-might-not-lower-wages-for-all/.

Tobin, J., "Inflation and Unemployment," *American Economic Review* 1/2 (1972), 1–18.

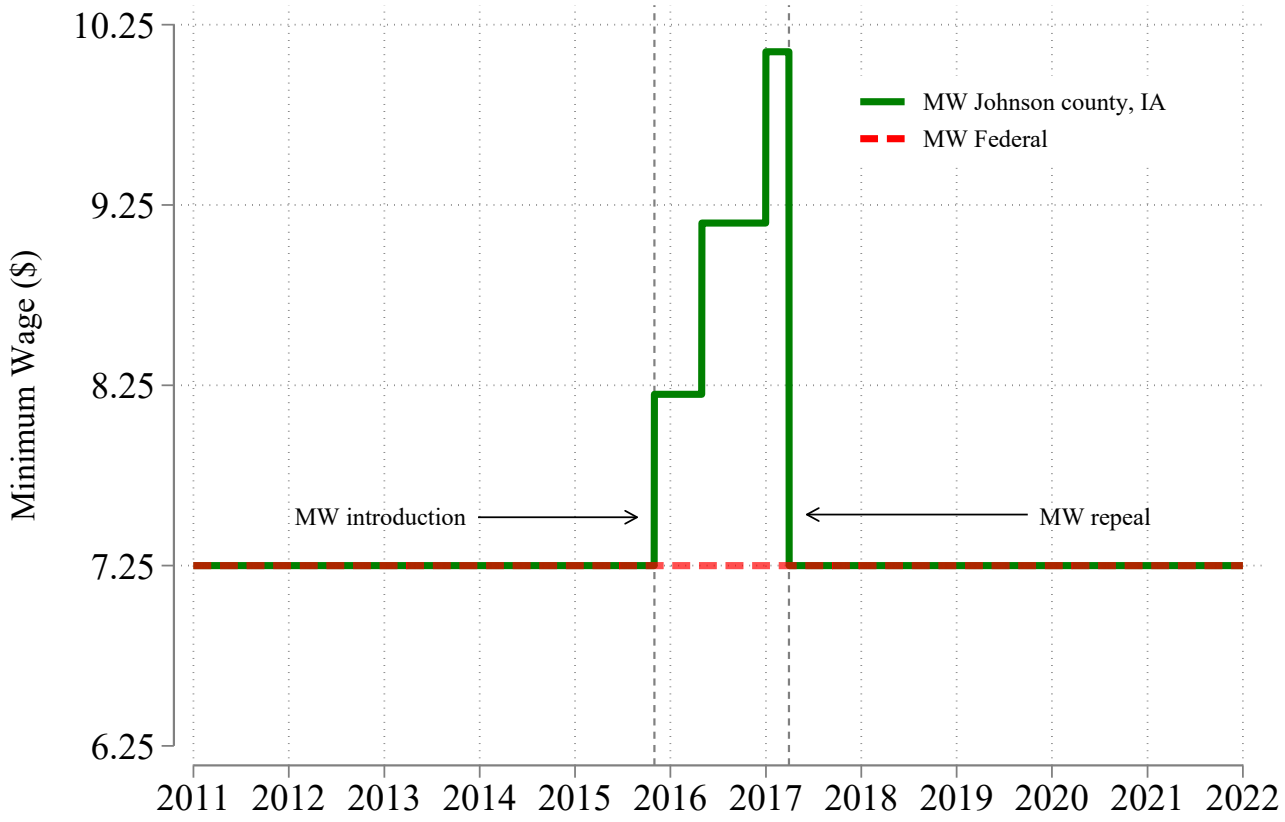
Vergara, D., "Minimum Wages and Optimal Redistribution: The Role of Firm Profits," (2025).

Wiltshire, J., "Allsynth:(stacked) synthetic control bias-correction Utilities for Stata. Working Paper," (2025).

Wiltshire, J. C., C. McPherson, M. Reich and D. Sosinskiy, "Minimum wage effects and monopsony explanations," *Journal of Labor Economics* (2025), forthcoming.

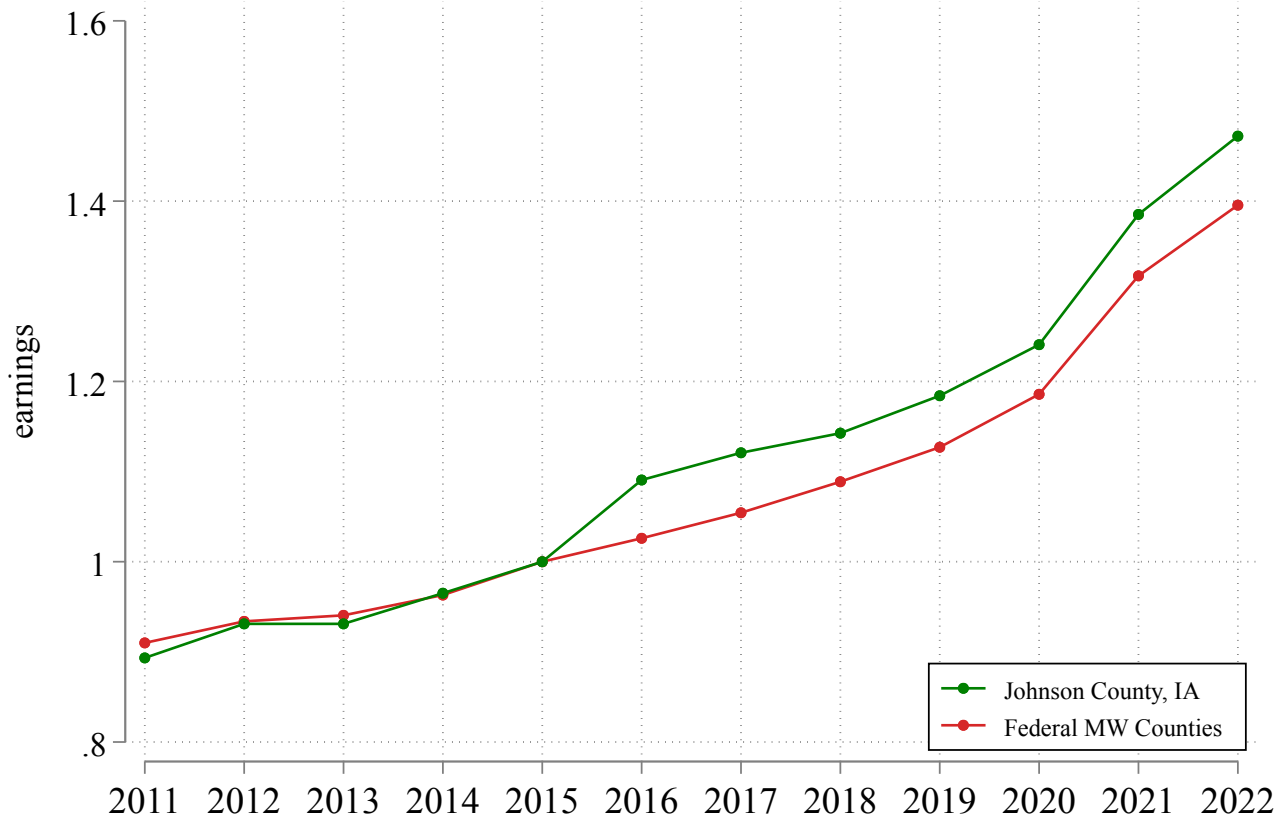
Woodbury, S. A., "Unemployment insurance overpayments and underpayments," (2002).

Figure 1: Minimum Wage in Johnson County, Iowa



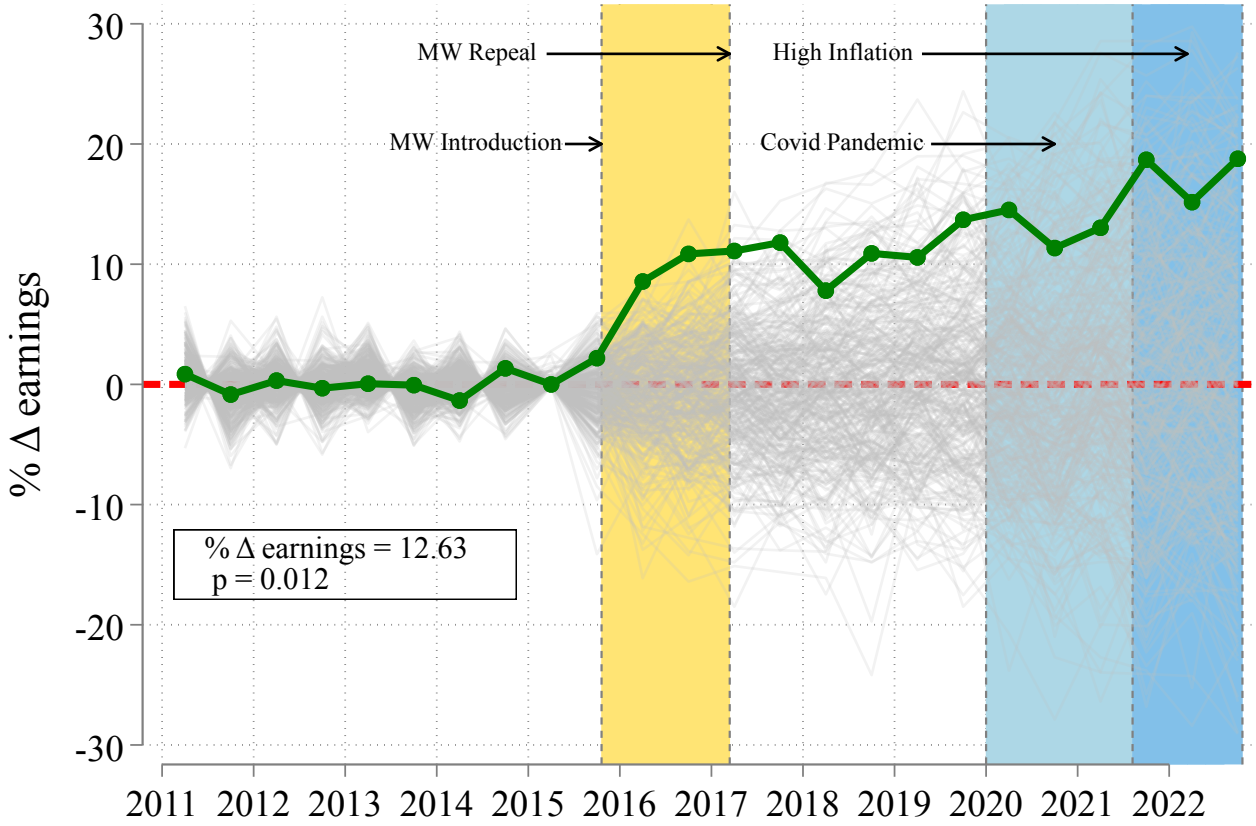
Notes: The graph shows the recent evolution of the statutory minimum wage in Johnson county, IA (green line) relative to the federal minimum wage (red line). The elevated Johnson minimum wage was first introduced on the 1st of November 2015, and then increased twice: on the 1st of May 2016 and the 1st 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



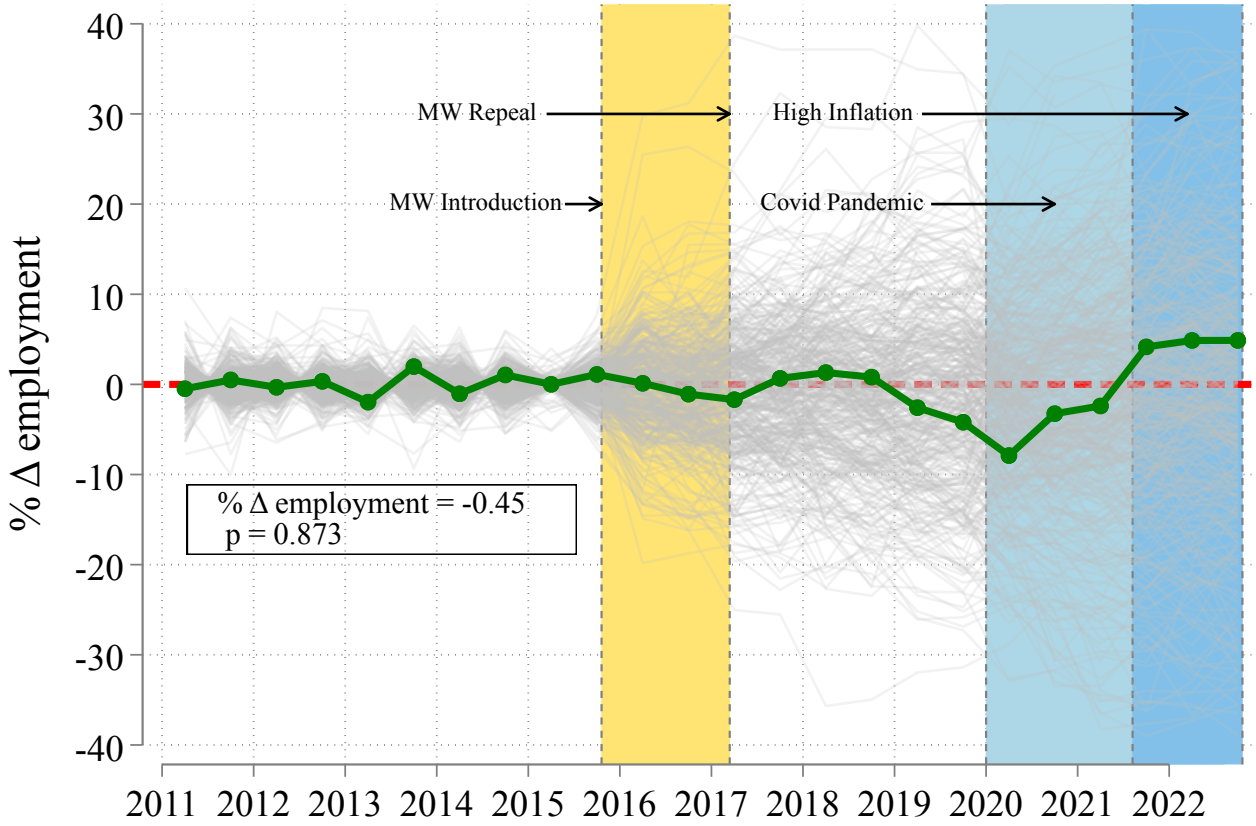
Notes: The graph shows the evolution of average earnings in the restaurant sector (NAICS 722) around the timing of the large, unexpectedly temporary minimum wage policy implemented in Johnson County, Iowa (this minimum wage schedule is depicted in Figure 1). The green line represents the average earnings 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



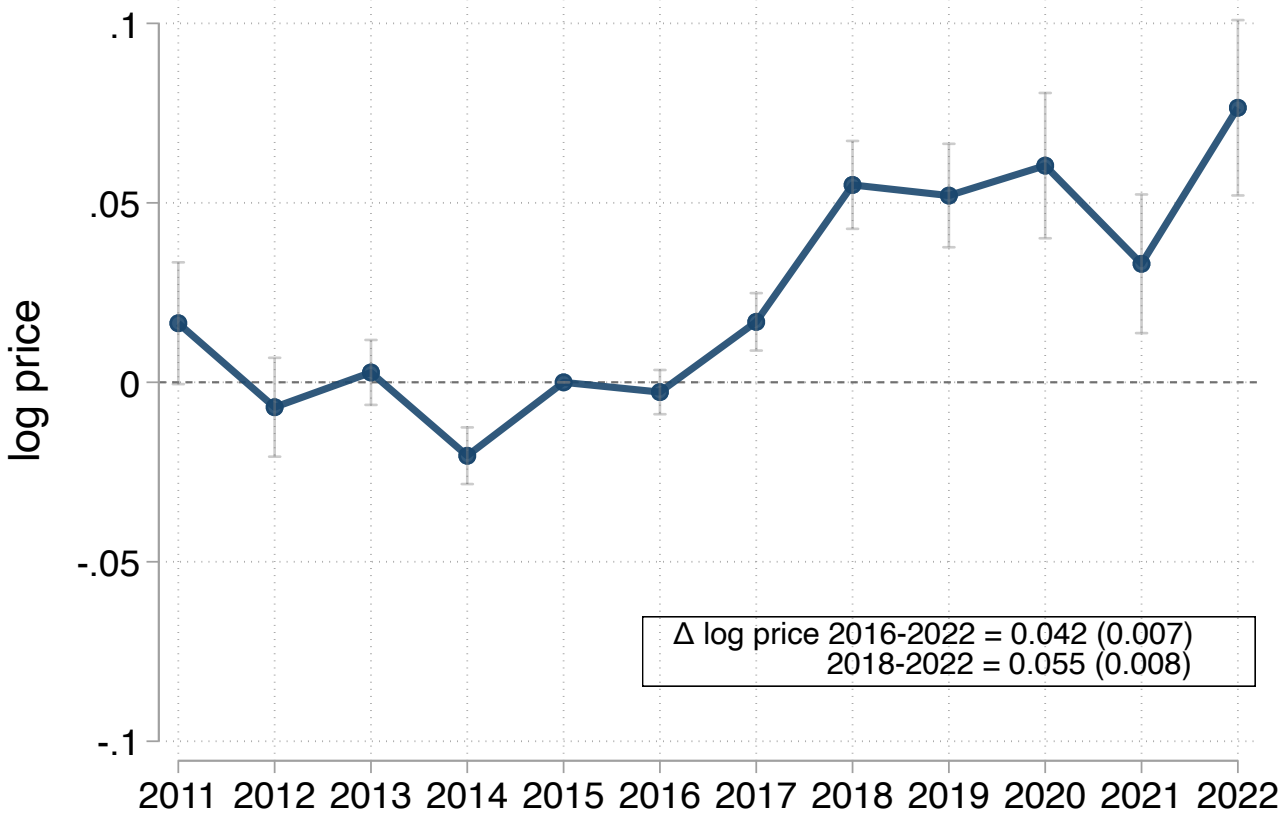
Notes: The graph shows the impact of temporary minimum wage policy on restaurant earnings in Johnson County, Iowa. The green line depicts the relative gap in average earnings in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (coefficients $\hat{\tau}_{ect}$ in Section 4). 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 A4 plot the estimates that underlie this figure as described in the main text.

Figure 4: Effect on Employment



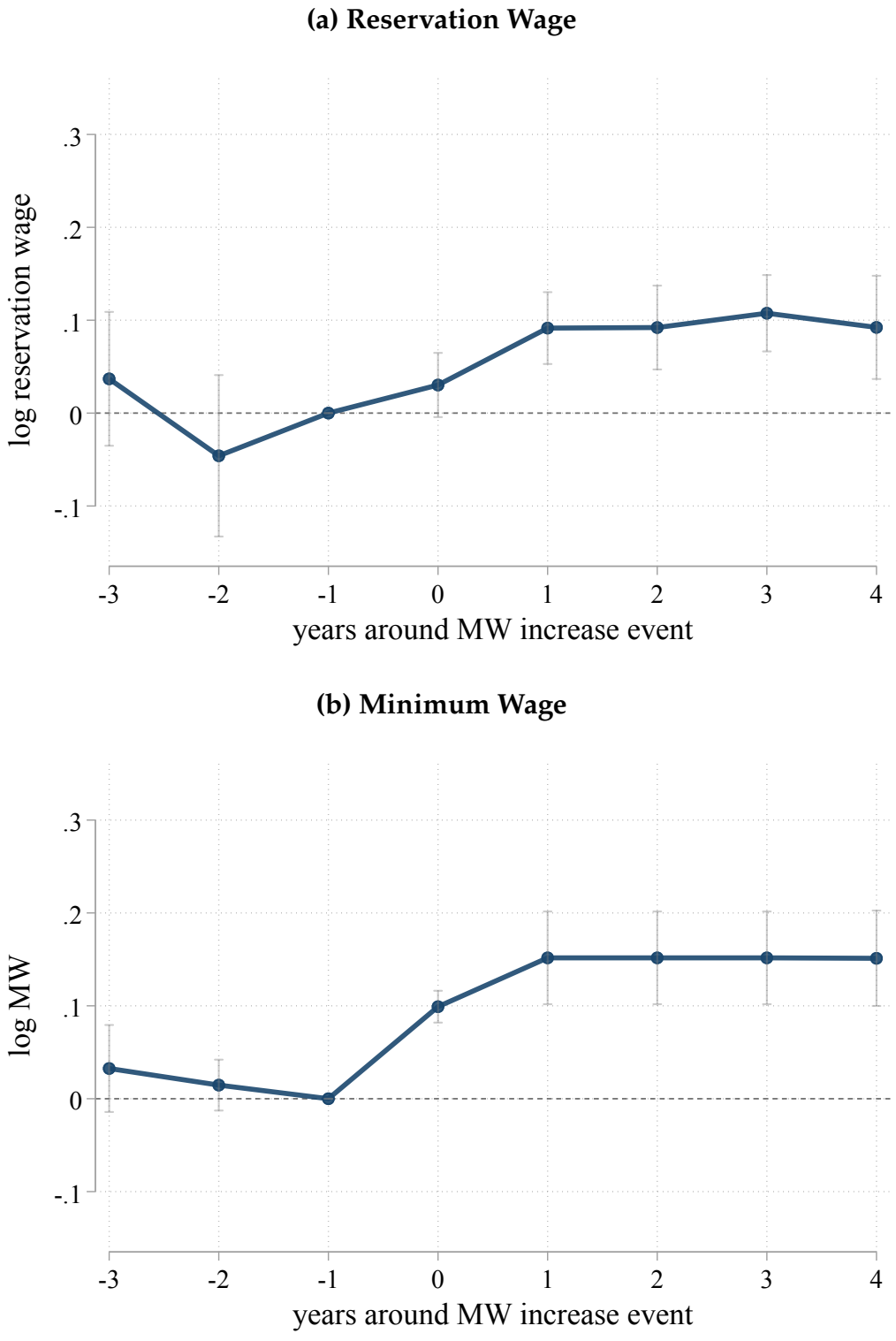
Notes: The graph shows the impact of temporary minimum wage policy on restaurant employment in Johnson County, Iowa. The green line depicts the relative gap in average employment in Johnson relative to synthetic Johnson, constructed using a synthetic control approach as described in the main text (coefficients $\hat{\tau}_{ect}$ in Section 4). 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 A4 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 4 (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 4. 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 A4. Data source: QCEW.

Table 3: Mechanisms

	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 evidence on the mechanism(s) discussed in Section 6 that may drive our main result. 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 Section 6.2. 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 (consistent with the main finding in Figure 3). 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 (where they face an unexposed workforce), challenging a prediction of the firm-learning mechanism. Both columns are obtained from a difference-in-differences specification as in equation (6). The remaining columns (3)-(5) report tests that relate to the predictions of the alternative reservation wage mechanism (described in Section 6.3) in a context where any firm-learning-based mechanism is expected to be shut down because treated workers primarily face firms not treated in Johnson. 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 pre and post defined as such in the ensuing columns in the table as well and Ferman and Pinto (2019) standard errors, appropriate for the case of a single treated county and heteroskedasticity, being used). The outcome variable is log income trimmed at the 1% and 90% level of the distribution of the log difference in (post-pre) income to ensure outliers, particularly very high-income-growth individuals, in the right tail of the distribution are not driving the average results (with the same trimming cutoff from Column (3) used in the ensuing columns of the table for consistency). 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 (top decile) movers out of Johnson, who were never bound by the minimum wage, and compare them to other high-income movers out of other counties. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

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

Emiliano Huet-Vaughn Jon Piqueras

Contents

A Additional Figures and Tables **46**

B News Coverage of Minimum Wage **58**

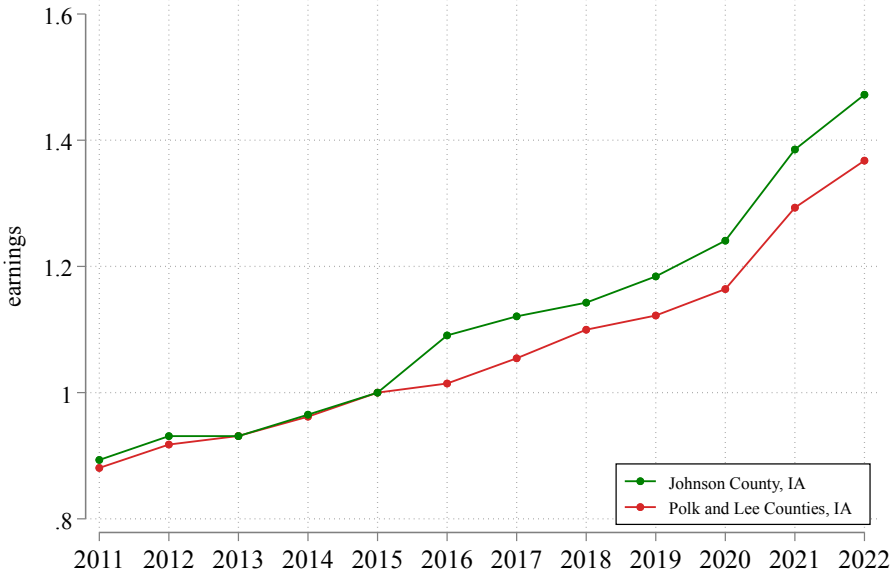
C Restaurant Price Series Data Collection **61**

D Occupation-Based Approach **63**

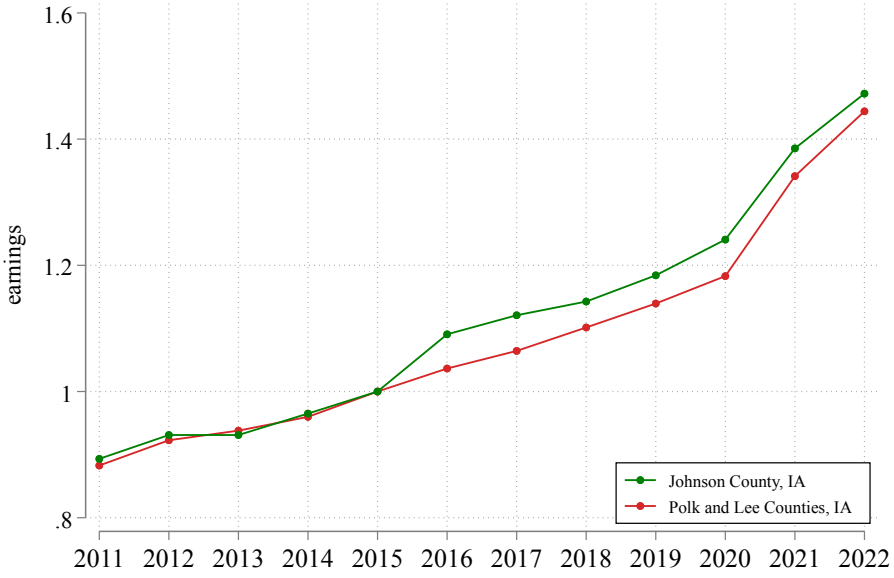
Appendix A Additional Figures and Tables

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

(a) Earnings - Unweighted

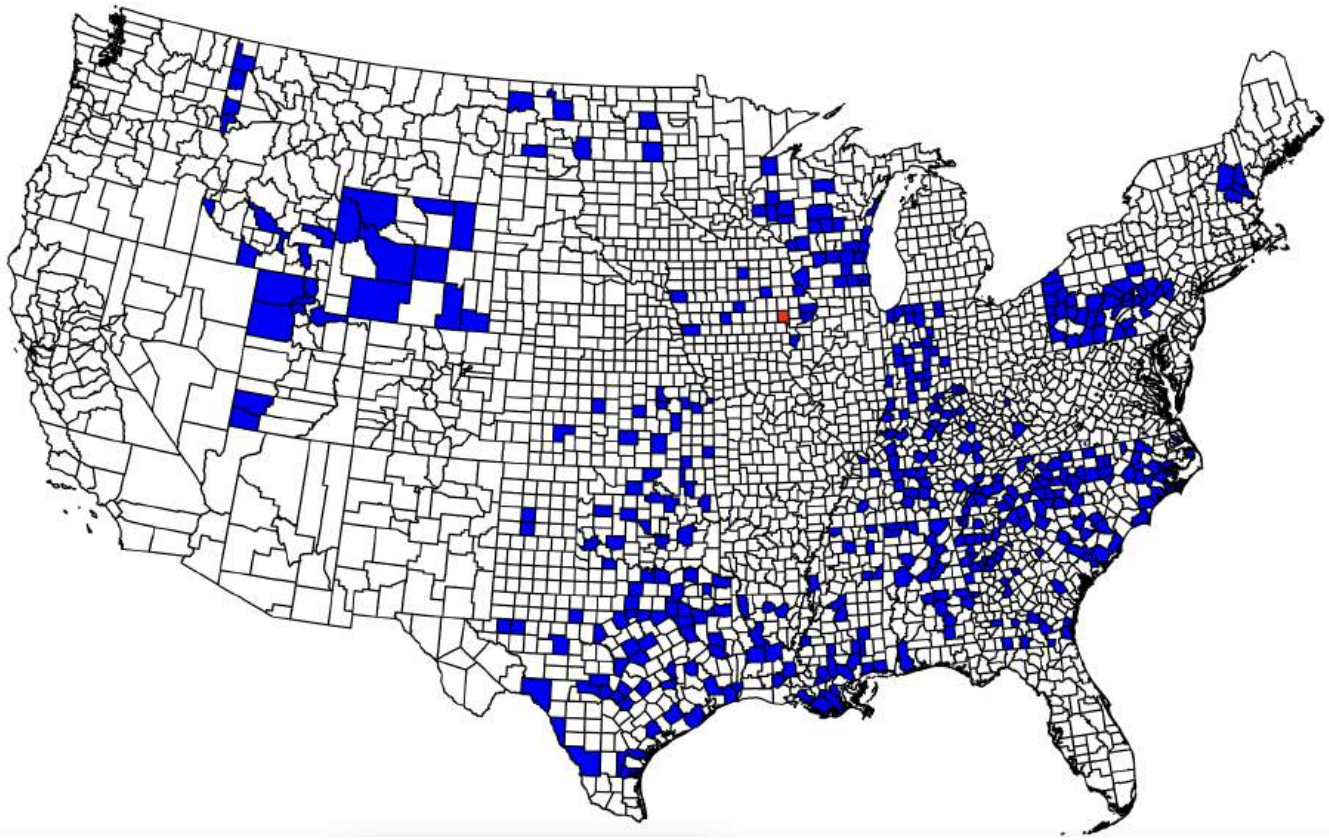


(b) Earnings - Weighted



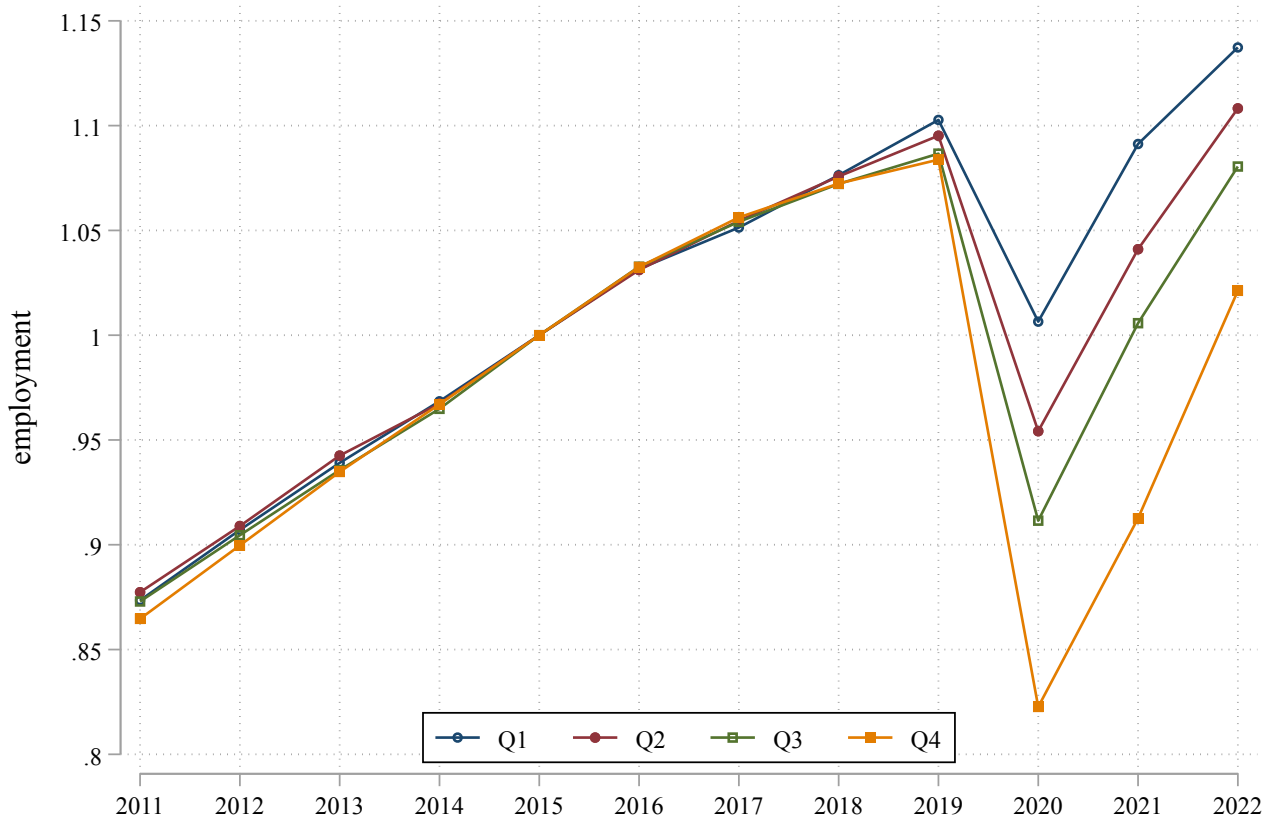
Notes: The graph shows the evolution of average earnings in the restaurant sector (NAICS 722) around the timing of the large, unexpectedly temporary minimum wage policy implemented in Johnson County, Iowa (this minimum wage schedule is depicted in Figure 1). The green line represents the average earnings 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: Counties in Donor Pool



Notes: The map presents the donor counties (in blue) used in the construction of synthetic Johnson (underlying Figures 3 and 4). Johnson County, Iowa is in red.

Figure A3: Democratic Vote Share and Restaurant Employment



Notes: The graph shows the evolution of average employment in the restaurant sector (NAICS 722) around the timing of the minimum wage increase studied in the paper, with counties in the US sorted by quartiles of the county Democratic party vote share in the 2012 Presidential election. 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.

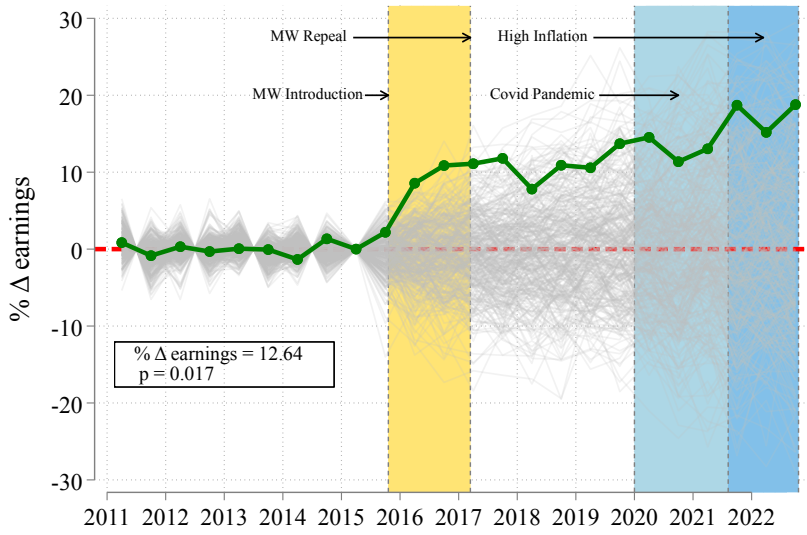
Figure A4: Effect on Earnings and Employment - By Sector



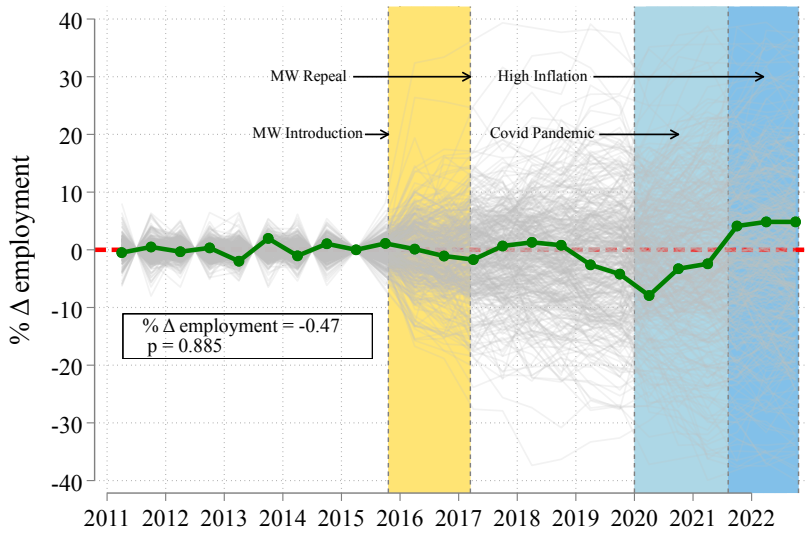
Notes: The figure shows the impact of a temporary minimum wage policy on county-level earnings and employment by sector, separately for the restaurant sector (NAICS 722) and other sectors of the economy (sectors excluding NAICS 722). Panel (a) refers to the impact of the policy on restaurant earnings, 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 4.). 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 A5: Effect on Earnings and Employment - Bias Corrected SC

(a) Earnings



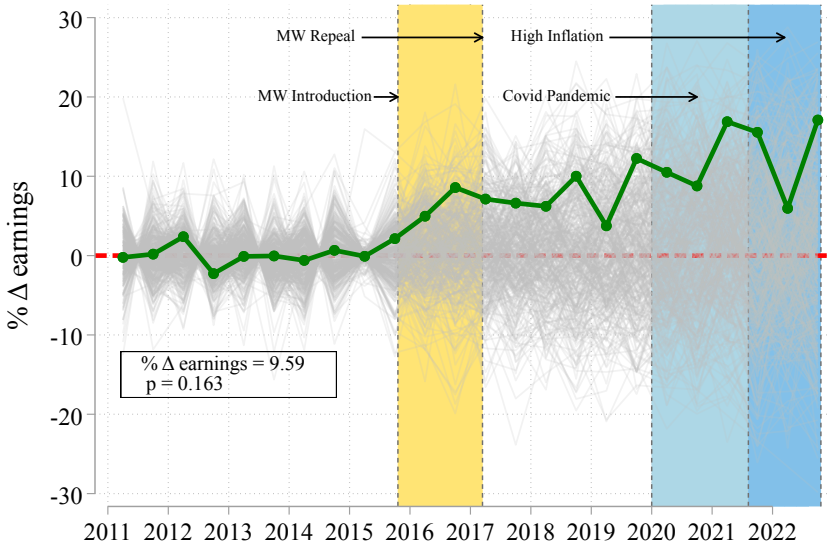
(b) Employment



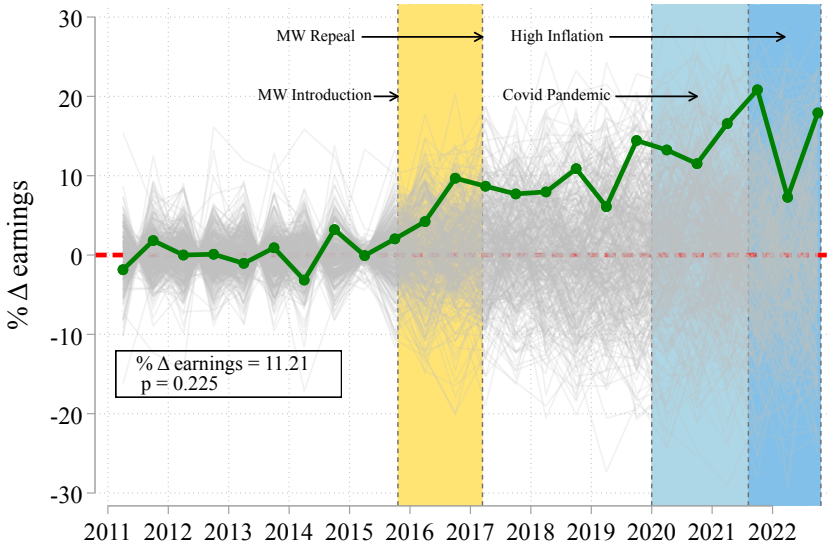
Notes: The figure shows the bias-corrected synthetic control estimates (Ben-Michael et al., 2021) of the impact of the temporary minimum wage policy on restaurant earnings (Panel (a)), and restaurant employment (Panel (b)) in Johnson County, Iowa, accounting for discrepancies in predictor values between 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 4). 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 A6: Effect on Earnings of New Hires

(a) Earnings of Hires - Restaurants

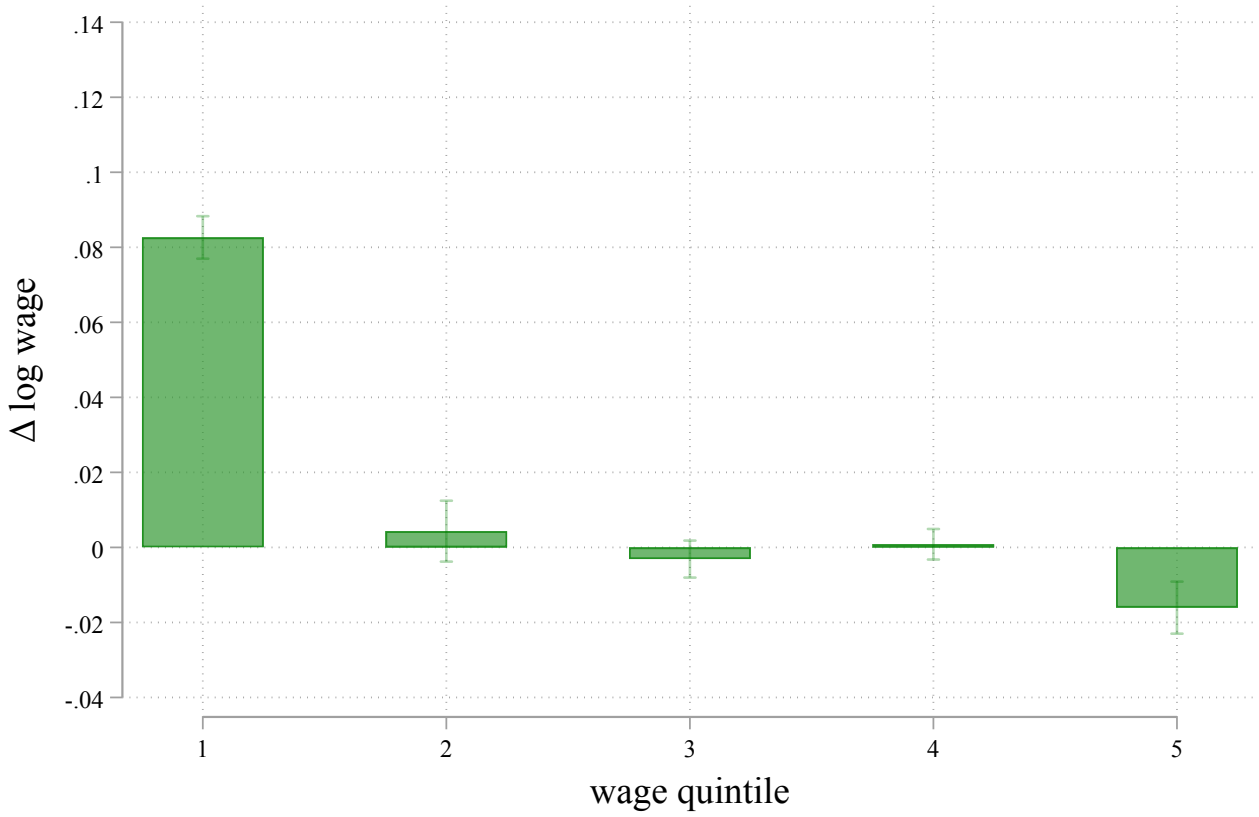


(b) Earnings of New Employees - Restaurants



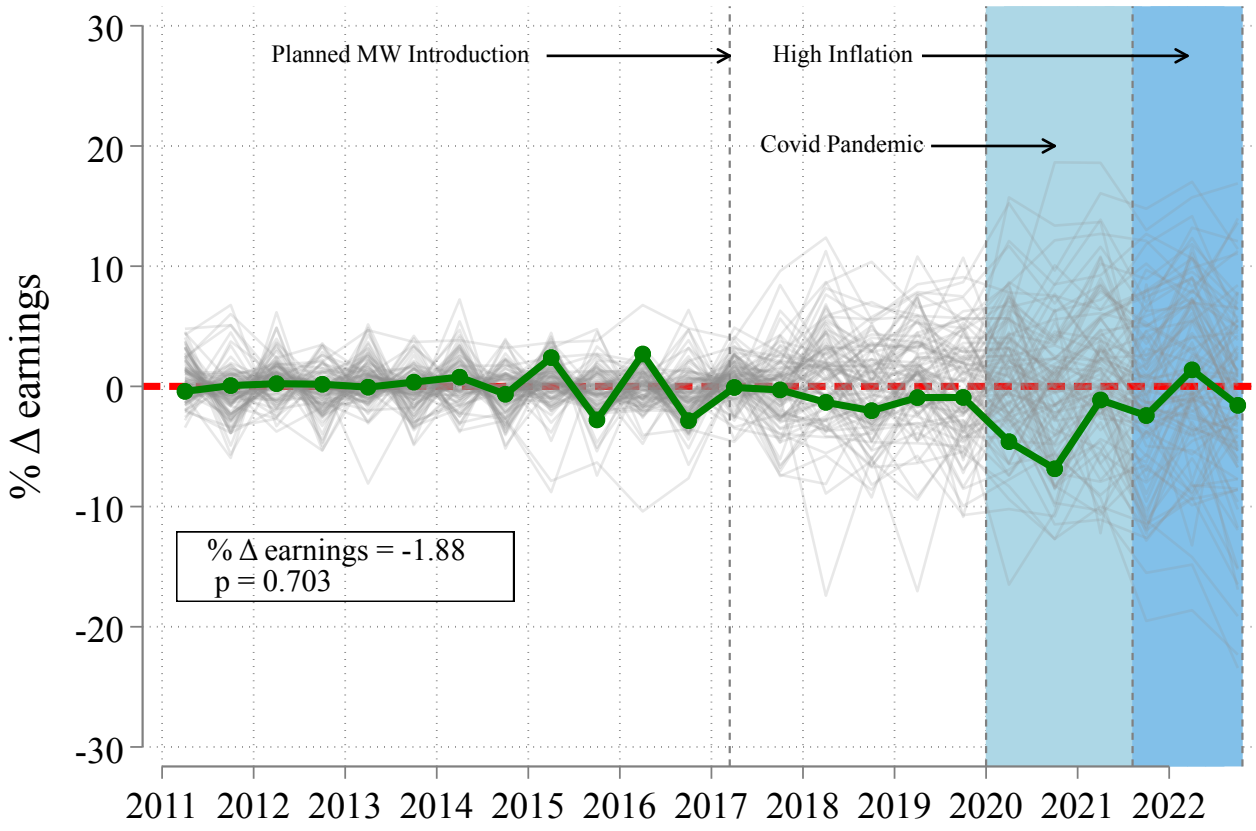
Notes: The figure shows the impact of the temporary minimum wage policy on earnings of individuals hired in the restaurant sector including recalls (Panel (a)), and new individuals hired in the restaurant sector excluding recalls (Panel (b)) in Johnson County, Iowa. The green line depicts the relative 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 4). 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 A7: Effect on Wages: Occupation-Based Approach



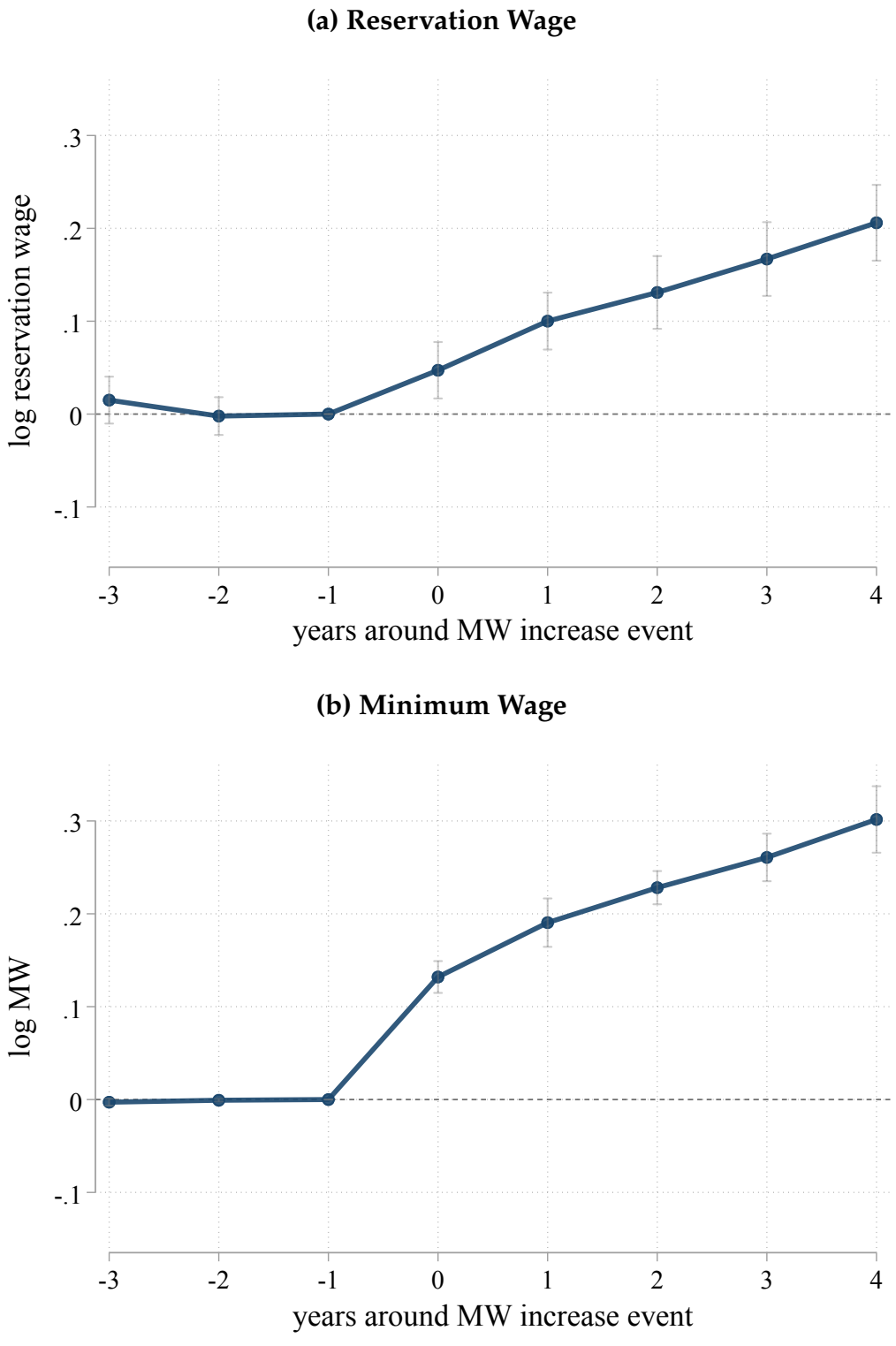
Notes: This figure examines the distributional impact of the policy on hourly wages in the Iowa City MSA using occupation-level data from the OEWS. Iowa City MSA is entirely contained in Johnson County. We rank occupations by the 25th percentile of their hourly wage occupation wage distribution in Iowa 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 D. 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 5.1 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 4 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 4). 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 4, with Panel (a) corresponding to the estimates shown in Panel (a) of Figure A4, 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 4, with Panel (a) corresponding to the estimates shown in Panel (b) of Figure A4, 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 D. 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 News Coverage of Minimum Wage

Figure B1: Selected Newspaper Images

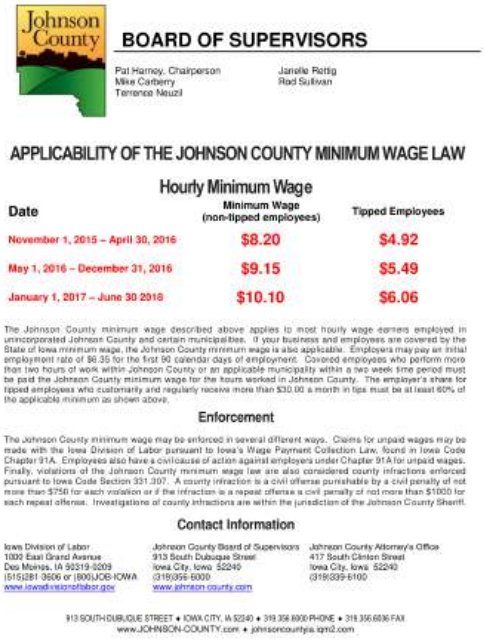
(a)



(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 B1: 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)

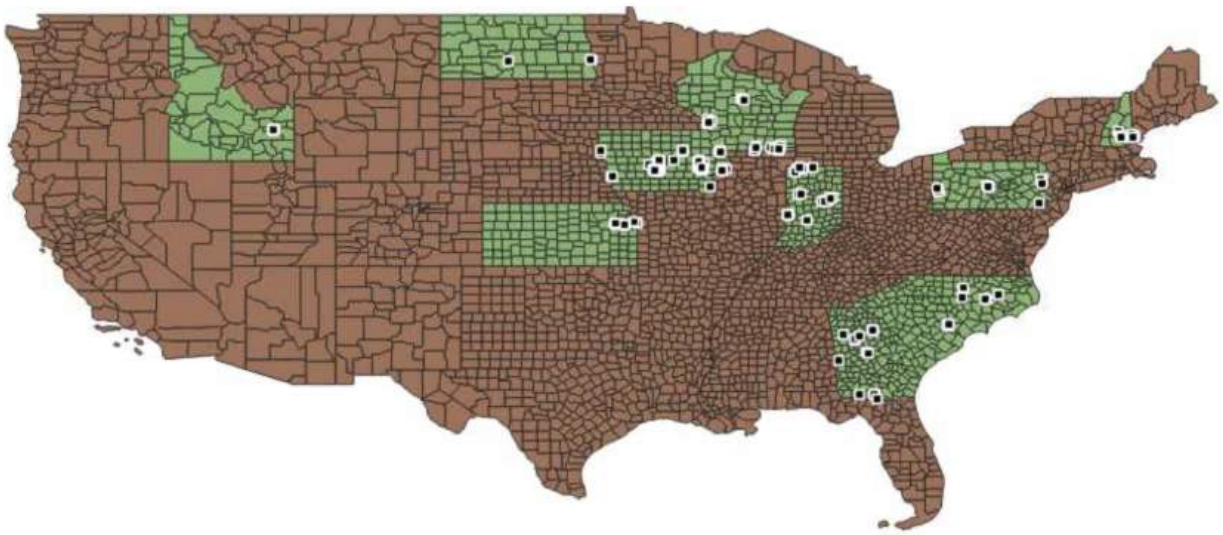
Appendix C Restaurant Price Series Data Collection

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 5.2).

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 scraped 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 C1).

Figure C1: 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 5.2.

Appendix D 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.