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

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DECEMBER 2023

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## ABSTRACT

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

Exploiting a unique natural experiment, we show the asymmetric effects of a large increase and an equivalent subsequent decrease to a binding minimum wage. Wages in a leading low-wage industry increase as the minimum wage rises, but do not fall when it is lowered. This boost for low-wage workers' earnings is apparently permanent five years after the policy is revoked, providing novel evidence of hysteresis in wage setting from temporary labor policy. In the first year post repeal this is consistent with downward nominal wage rigidity. But, the elevated earnings persist even in high inflation times, contrary to the prediction from existing work that real wage reductions under high inflation should erode the nominal wage gap relative to unaffected firms. Our findings thus challenge the conventional view that inflation "greases the wheels" of the labor market in the face of downward nominal wage rigidity, and, demonstrate the value of even transitory labor market policy in achieving permanent gains for workers (play it while you got it).

**JEL Classification:** J3, J8, E24, E31

**Keywords:** hysteresis, minimum wage, downward nominal wage rigidity

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# 1 Introduction

It is a near truism that minimum wages, if they change, move only in one direction – up. But what happens to firm pay and employment when a wage floor covering all workers suddenly falls?

This paper studies such an unusual scenario, a very large increase in the minimum wage followed by an equivalent, unexpected, and precipitous drop. We document asymmetric responses to the increase and decrease in minimum wage, and leverage this shock to reveal new insights on broader labor market dynamics.

Our main result 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 wage gains being sustained long (5 years) after the precipitating policy has ceased. Our findings also challenge conventional wisdom about the interaction of downward nominal wage rigidity and inflation, specifically, the countervailing impact that inflation is thought to have on real wages in the face of such nominal rigidities (Tobin, 1972). Finally, we investigate the possible mechanisms at play, finding that our evidence is most consistent with the effect the policy shock has on worker reservation wages à la the fairness hypothesis of Falk et al. (2006); we provide the first empirical confirmation of their predictions from a quasi-experimental design that directly mirrors their lab experiment. Overall, the work reveals the value of implementing even transitory labor policy as it can have lasting positive footprints on the earnings of low-wage workers.

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 local Johnson government initiated dramatic increases in the minimum wage that unfolded in just over a year - from \$7.25 up to \$8.20 then \$9.15 and finally \$10.10.<sup>1</sup> For 17 months in total, an elevated local minimum wage was in effect in the county (above the \$7.25 federal rate that had otherwise governed in the state) – until the state government suddenly 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, getting back to the state level of \$7.25. This

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<sup>1</sup>This total increase of 39% in the minimum wage is far 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 (Clemens and Strain, 2021).

unique event was followed in the ensuing years by two unusual periods, first a Pandemic Recession in 2020, and then a period of high inflation cresting in 2022, that allow us to study the persistence of the minimum wage policy's footprint in the face of first a negative demand shock and then rapidly rising prices.

To examine the labor market effects of this large temporary minimum wage, we employ a synthetic control approach ([Abadie and Gardeazabal, 2003](#); [Abadie et al., 2010](#)) where we compare the labor market behavior in the restaurant sector (the leading minimum wage-employing industry) of Johnson County relative to that in a large set of similar counties. To obtain a comprehensive picture, we leverage near-census information on wages and employment at the county level, together with firm-level job posting records.

The main results on wage setting hysteresis from the temporary minimum wage policy are stark. First, the introduction of the higher minimum wage has a direct impact on restaurant wages in Johnson county while the policy is in effect, increasing pay by 6–7%. In the first year after the minimum wage is repealed, there is no indication of a wage reduction for affected workers, with wages remaining higher than 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 Pandemic Recession and the high inflation of late 2021 thru 2022. While theories of downward nominal wage rigidity ([Keynes, 1936](#); [Bewley, 1999](#)) clearly offer an explanation for the short-term persistence of raised wages for Johnson county workers after firms were no longer required to pay them, such theories also predict that inflationary episodes should serve to reduce real wages in Johnson relative to non-treated places, and thus “grease the wheels of the labor market” ([Tobin, 1972](#); [Akerlof et al., 1996](#); [Card and Hyslop, 1997](#)). Yet, this is plainly not the case. Further analysis on firms' job posting behavior offers suggestive evidence that employers increased posted wages in response to the policy shock and continued doing so after the policy reversal. While this latter evidence is less precisely estimated, the findings are consistent with not only incumbents enjoying the legacy of higher overall wages from retired minimum wage policy, but, also new workers.

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, naturally, no “rebound” when the minimum wage is lowered: in the year after revocation there remains no significant change in employment in either an economic or statistical sense. Beginning more than three years post-implementation (and almost two years post-revocation), there is a downward trend in employment which

gets more pronounced upon the onset of COVID-19 and its associated public health and demand shocks. A delayed reduction in employment would potentially be consistent with theoretical models of adjustment costs, however, the overall negative estimate of -14% is not statistically significant. In terms of posted vacancies, we find suggestive evidence of a reduction, consistent with [Dube et al. \(2016\)](#) and [Kudlyak et al. \(2023\)](#), beginning with the implementation of the policy and continuing after the policy reversal, although results are imprecise and cannot rule out a zero impact on vacancies.<sup>2</sup> In comparison to the much clearer wage results, the employment results are thus more nuanced. We think the most reasonable interpretation is one of no contemporaneous or medium term impact of the minimum wage policy on total restaurant employment, with uncertain and only suggestive evidence of a delayed longer term legacy of reduced employment and a reduction in new hires.

To understand the mechanism underlying the wage hysteresis – why are firms paying such high wages when they no longer legally have to – we consider several possible explanations drawn from the existing literature. Some of these mechanisms share an underlying theme of firm learning from forced experimentation ([Larcom et al., 2017](#)).<sup>3</sup> Under this thinking, abiding by the higher minimum wage taught them something about the value to them (in higher revenue/ reduced costs) of paying workers more, which turned out to be more beneficial than they had anticipated. 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 at (perhaps drawn from neighboring non-Johnson county restaurants or other sectors internally) when they pay a higher wage ([Butschek, 2022](#)). Alternatively, 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 in [Falk et al. \(2006\)](#). 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 that they will lose their workers, who have come to demand a higher pay and

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<sup>2</sup>Note that a reduction in posted vacancies, building over time, may be consistent with an eventual reduction in employment, but, it is not necessarily associated such a reduction as the stock of employees is affected not only by the flow in of employees but also the flow out. Evidence in the minimum wage literature (e.g. [Dube et al., 2016](#)) tends to find lower quit rates in response to a higher minimum wage, which could also cause an offsetting reduction in vacancies without an effect on total employment.

<sup>3</sup>[Larcom et al. \(2017\)](#) explores 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.

more of the total surplus, should they choose a lower, now legally allowed wage.

For a separating test to determine which of these mechanisms may be at play, we leverage the wage posting behavior of multi-establishment firms that 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 operates with only the federal minimum wage. Mechanisms in the first category above (*i.e.* those that predict learning-by-forced-experimentation and increased firm profit from paying workers more) would expect these firms to some time after 2015 begin raising their wages in their non-Johnson establishments as well, as they learn it profits them to do so. In comparison, those firms outside Johnson that never were exposed to the revelatory, profit-enhancing, elevated minimum wage would not be expected to raise their workers' wages following a policy that they never experienced. In contrast, the reservation wage mechanism would predict the two kinds of firms (those exposed and not exposed to the Johnson minimum wage) should not dramatically deviate post-2015 from the baseline difference in their wage offerings (as there is no profit to be gained by the exposed, or "treated", firms and no need to lift wages for non-Johnson workers who did not experience a shock to their reservation wage). In fact, as we show, the evidence is more consistent with the latter pattern.

This paper contributes to three 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](#); [Saez et al., 2021](#); [Benzarti et al., 2020](#)). In terms of hysteresis induced by labor policy, [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, while, to our knowledge, existing quasi-experimental evidence is limited to [Saez et al. \(2021\)](#) who document *employment* hysteresis from an active labor market policy (a payroll tax-cut) affecting labor demand. Our work represents the first quasi-experimental evidence of *wage* hysteresis from temporary labor market policy, specifically, a permanent increase in low-wage worker earnings from a short-run minimum wage increase, persisting more than five years after policy repeal.

Second, it contributes to the macro-labor literature on downward nominal wage rigidity and the phenomena'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](#)). 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. The prevailing view is that when inflation is high, however, firms can freeze nominal pay in order to achieve reductions in real wages and therefore reduce costs. [Elsby \(2009\)](#) traces the argument back to [Tobin \(1972\)](#), summarizing the intuition as follows: "if workers are reluctant to accept reductions in their nominal wages, a certain amount of inflation may grease the wheels of the labor market by easing reductions in real labor costs that would otherwise have been prevented." We contribute by testing empirically how the effects of the policy evolve under different inflation scenarios. And, our finding is novel. While the observed absence in wage change after the policy repeal can clearly be explained by nominal wage rigidity, in contrast to the prior literature, we also document for the first time persistence of the wage differential *even in high inflation periods*.<sup>4</sup>

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](#); [Azar et al., 2023](#); [Dustmann et al., 2022](#)). 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.<sup>5</sup> 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 between alternative mechanisms we find evidence more consistent with the [Falk et al. \(2006\)](#) reservation wage mechanism than other leading mechanisms. Beyond [Falk et al. \(2006\)](#), our work relates

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<sup>4</sup>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 result suggest otherwise.

<sup>5</sup>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 the effect seems to vanish after a few years. No reduction in the minimum wage is observed in this setting.

to the literature on sub-minimum wages ([Freeman et al., 1981](#); [Katz and Krueger, 1992](#)), which refer to policies allowing lower than minimum wage pay 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. In these cases, horizontal pay equity concerns may play an important role when, as is common, workers of different ages work together, and so firms may face a constraint when trying to pay differently to workers performing similar work within the firm. 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, and, thus, this horizontal pay equity constraint at the firm level is not binding in our setting (and so is not a factor in explaining our main result). In light of our findings, we believe that horizontal pay equity considerations, while certainly important, may not be the only explanation for the under-utilization of sub-minimum wages and that reservation wages may be an additional or nesting explanation.

The rest of the paper proceeds as follows. Section [2](#) describes the data and the institutional background. Section [3](#) presents the results. Section [4](#) discusses the potential mechanisms, and Section [5](#) concludes.

## 2 Background and Data

**Institutional Setting.** 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 sub-federal minimum wages have 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: Louisville KY, Lexington KY, Linn County IA, Wapello County IA, St. Louis MO, and, Kansas City MO. In every one of these instances the local minimum wage increase that was revoked was not more than a dollar, and, in most cases it was also 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 and the duration. On 1st November 2015, the local minimum wage was raised from the Federal level of \$7.25 to \$8.20. After that, it was further increased twice — on 1st May 2015 and 1st January 2017— reaching \$10.10. It was repealed by the Republican-led state legislature on 1st April 2017, going back to the initial level of \$7.25. See Figure 1 for the Johnson County minimum wage schedule over this time period.<sup>6</sup> Given the sharp and prolonged nature of the shock, the episode in Johnson county is the best test case for detecting any immediate or lingering (post-reversal) effects of a minimum wage increase that was actually 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 not be perceived as transitory by firms (Sorkin, 2015). 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.<sup>7</sup> Moreover, the large mandated increase to the wage bill enables us to study a situation where adjustment costs or inertia should play a minor role given the sizable costs of inaction (Chetty et al., 2011). 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.

We use two main data sources that allow us to learn about different dimensions affected by the policy.

**Employment and Wages.** We leverage information on employment and wages from the Quarterly Census of Employment and Wages (QCEW) over the years 2011-2022.<sup>8</sup> This dataset contains a quarterly near-census of employment counts and weekly wages for employers subject to unemployment insurance laws (covering more than 95 percent of all U.S. jobs).

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<sup>6</sup>For tipped workers in Johnson county, who at baseline in 2015 had a sub-minimum wage of about 60% of the level for other workers, the schedule of increases in Johnson also moved their minimum wage up dramatically as well with each step in the local increase, keeping their tipped worker wage floor at about 60% of the new broader minimum wage level.

<sup>7</sup>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. This represented a tight turnaround from introduction to passage, with the bill being first 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).

<sup>8</sup>See the Bureau of Labor Statistics county-level QCEW files at <https://www.bls.gov/cew/>.

Our main analysis focuses on information at the county level, and for an important 3-digit low-wage industry: Food Services and Drinking Places (NAICS 722), known to contain one of the largest shares of workers affected by minimum wage increases. We complement the dataset with county-level shares of employment and average wages at the super-sector level, which corresponds to a high-level aggregation of NAICS sectors.<sup>9</sup>

**Job postings.** To learn about the effect of the policy on new jobs and wages, we use job posting data from Burning Glass Technologies (Burning Glass Technologies, 2018) from 2013 thru the middle of 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. We are primarily interested in the number of vacancies in the restaurant industry (722 NAICS) and in the posted wage for the job. While the Burning Glass data 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. \(2023\)](#), 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 good deal of wage information (over a hundred thousand observations). In cases of a posted wage range we use the minimum of that range.

### 3 Baseline Results

In order to assess the labor market effects of the temporary local minimum wage policy in Johnson County, Iowa, we construct our counterfactual 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 for counties present in the donor pool. For our baseline specification, we use potential donor counties from states that do not require firms to pay more than the federal minimum wage

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<sup>9</sup>Excluding the public sector, we use the following supersectors: Natural Resources and Mining; Construction; Manufacturing; Trade, Transportation, and Utilities; Information; Financial Activities; Professional and Business Services; Education and Health Services; Leisure and Hospitality; Other Services

(19 states besides Iowa that have not updated their minimum wage above \$7.25 since the last federal change in 2009) and counties in the rest of Iowa with the exclusion of those counties that border Johnson (to exclude places possibly affected by policy spillovers) and three additional counties in Iowa that had in early 2017 also attempted to increase their minimum wage before being stopped by the state.<sup>10</sup> Moreover, we restrict the donor pool to counties in these places with average pre-treatment employment in the sector of interest (restaurants) between 2000 and 11000.<sup>11</sup>

Our main outcomes are average wages and employment in the restaurant industry at the county level. We work with a specification where we normalize both variables by their value in the quarter pre-reform.<sup>12</sup> In terms of predictors, we use averages of both wage and employment for each year in the pre-reform period, together with averages over the whole pre-period of wages and employment shares in each supersector. The inclusion of sector wages and industry shares is motivated by recent literature ([Dube and Lindner, 2021](#)) showing the importance of accounting for differences in industry composition and wage distribution.<sup>13</sup> We estimate synthetic counterfactuals at the quarterly frequency, and take annual averages to obtain estimates at the annual level. 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 mean squared prediction error (RMSPE). 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](#)) using the implementation found in the `allsynth` package ([Wiltshire, 2021](#)), though this does not change our conclusions in any meaningful way.

The estimates of the effect of the temporary policy change on wages are shown in Figure 2. 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. 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

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<sup>10</sup>In Wapello county the local minimum wage was in effect for 3 months before repeal; in Lee and Polk counties it was yet to go into effect when the state law prohibiting local minimum wages became effective law.

<sup>11</sup>Johnson county has a pre-treatment average restaurant employment of around 6700, so this constitutes an approximately symmetric window on both sides of the size of our treated unit.

<sup>12</sup>This specification is similar to the one used by [Harasztsi and Lindner \(2019\)](#); [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. It is also equal to [McPherson et al. \(2023\)](#) in the synthetic control context, and related to [Doudchenko and Imbens \(2016\)](#) and [Ferman and Pinto \(2021\)](#). Our results are similar when matching in levels, although somewhat less precise.

<sup>13</sup>[Dube and Lindner \(2021\)](#) show that without such controls they do not get sensible results on the upper tail of the wage distribution, considered a key falsification test ([Autor et al., 2016](#); [Cengiz et al., 2019](#)).

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). In the year following the introduction of the minimum wage, wages increase sharply by around 7% (6% for the bias-corrected estimate) relative to the control unit. After removal of the legal minimum, the wages 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 Pandemic, and also through the period of unusually high inflation. Considering the whole post-policy period, from the first full quarter after introduction of the minimum wage until the end of 2022, we find an overall positive effect of 7.51% with a p-value of 0.053 as reported in Figure 2 (for reference, the bias-corrected estimate is 6.18 with a p-value of 0.052).

In relation to the employment effects, results are depicted in Figure 3. We do not find statistically or economically significant effects while the policy is in place, during the removal year or even almost two years after removal. Beginning in 2019, the estimates exhibit an insignificant downward trend, with the magnitude becoming more pronounced over time. Averaging over the whole post-policy period there is an overall insignificant employment effect of -14% with a p-value of 0.21 (while the bias-corrected estimate is of equivalent size with a p-value of 0.36).<sup>14</sup>

Figure 4 presents results on the posted wage for new restaurant industry jobs from the Burning Glass online job posting data. Figure 5 presents results on the number of new job postings in the industry from the same data. The specifications used to generate these Burning Glass results are the same as above with the addition of predictors averaging the posted wage (for Figure 4) and the number of vacancies (for Figure 5) for each year in the pre-reform period; we additionally remove the sector wages and industry shares as predictors since pre-trend fit is poor when they are included. The posted wage results are consistent with new workers benefiting from higher offered wages even after the local minimum wage increase has been repealed (though this is not a statistically significant effect). In terms of the volume of new vacancies posted in the restaurant sector, the pattern is consistent with reduced new hires through 2018, which if continued over time might explain the delayed

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<sup>14</sup>While there are theoretical explanations that would predict a delayed negative employment response following a permanent minimum wage shock (Sorkin, 2015; Aaronson et al., 2018), existing empirical evidence remains inconclusive in this respect (Meer and West, 2016; Harasztosi and Lindner, 2019). Moreover, note that these models postulate that one-time nominal minimum wage increases, as in our case, are effectively temporary shocks due to inflation, and thus observed long-term responses should be relatively small. Thus, a potential disemployment effect would be consistent with the theoretical models only if the reservation wage mechanism that we have in mind (see Section 4) actually means a permanent shift in effective real labor cost – regardless of statutory revocation of the minimum wage – making the Johnson-induced wage shock effectively permanent. Though, again, the employment effect is not estimated to be statistically significant here.



reduction in employment seen after 2019 in Figure 3 (though again the reduction in new vacancies seen in Figure 5 is not statistically significant).

## 4 Mechanisms

Why do we see wage hysteresis? What makes firms continue to pay much higher wages to workers after they are no longer legally required to? The existing literature offers several possible mechanisms.

One cluster of explanations are those mechanisms that appeal in some way to firm learning from forced experimentation (Larcom et al., 2017). That is, by being required by law to pay a higher minimum wage, firms learn that offering higher pay is actually profitable and continue to do so after the revocation of the requirement. This presupposes some sub-optimizing behavior of firms in the status quo. 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). 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 no longer required, 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 changed their reservation wage and future bargaining behavior, consistent with the lab experimental findings in Falk et al. (2006).<sup>15</sup> 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 that they will lose their workers, who have come to demand a higher pay and more of the total surplus, should they choose a lower, now legally allowed wage.<sup>16</sup>

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<sup>15</sup>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., 2022).

<sup>16</sup>Note the reservation wage channel has a clear prediction on increased pay after the repeal, however the effects we find through high-inflation periods would also imply that reservation wages are permanently impacted in real, and not only in nominal terms. Driving this could be a natural updating of reservation wages in response to changing prices, or, a change in reservation wages induced by the signal sent through the recommended (evolving) non-binding wage guidelines that have been advertised by Johnson local officials since 2017. Regarding this latter channel, Falk et al. (2006) also include a treatment in their experiment with a

To develop a test that discriminates between alternative mechanisms we turn to Burning Glass wage posting data in the restaurant industry (NAICS 722). Specifically we look at 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 (with firm presence in either place measured by firm posting a job with wage there). Our interest is in the development of their wage postings in their non-Johnson county establishments in comparison to other firms operating outside of Johnson county that never operated in Johnson county prior to the second quarter of 2017. The first group can be thought of as firms “treated” by 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 should happen to the difference in non-Johnson wage postings for treated versus control firms? Under the efficiency wage, turnover cost, and reallocation mechanisms, low-wage firms in Johnson county discover that paying their low-wage workers more yields gains (of various sources) to firms. If firms discover that their Johnson county establishments are less costly or more profitable as a result of the higher worker wage, then the logic of these mechanisms predicts they should raise wages in their non-Johnson firms in order to experience similar gains there. Thus, 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.<sup>17</sup> The reservation wage mechanism, by contrast, predicts no such divergence because the treated firms are not experiencing any increase in profit from paying workers more. Rather, they are effectively forced to do so well after 2017, even though it reduces their profit, because of the policy-induced increase in the reservation wage for Johnson county workers. Workers outside of Johnson county have no shock to their reservation wage so there is no need to pay them more and no desire to otherwise since it does not increase profit under this mechanism alone.

Figure 6 shows what we observe empirically. It plots the gap in treatment and control firms’ lowest posted log wage in non-Johnson counties before and after the increase in the minimum wage in Johnson County, Iowa. Data comes from the rest of Iowa and the nearby

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nonbinding recommended wage guideline that resembles what Johnson county officials have done since they were stripped of their statutory authority. The effects on experimental earnings from the wage guideline in Falk et al. (2006) are similar to the wage floor treatment.

<sup>17</sup>Growth in the gap between treated and control firms would also be consistent with Hazell et al. (2022) if firms are attempting to harmonize pay for workers across establishments (though this explanation would not necessarily involve any learning from forced experimentation about what is profitable).



states without a state-level minimum wage (Kansas and Wisconsin).<sup>18</sup> The specification includes county, quarter, and employer fixed effects. As the figure reveals, the difference in the log wage between treatment and control firms is stable prior to the 4th quarter of 2015 (when the treated firms were exposed to the elevated Johnson county minimum wage). After exposure to the policy, these treated firms do not seem to change what they pay their workers outside of Johnson county. Averaging across all post-treatment periods, there is a statistically insignificant 0.004 estimate (p-value of 0.89), or, less than half of a percent change in the wage gap. Thus, the reservation wage mechanism is supported and the alternative mechanisms that predict an increase in non-Johnson county establishment wage are rejected. Specifically, we can rule out the possibility that firms are extending the observed 8% increase in earnings experienced in Johnson county to their non-Johnson county establishments, as upper and lower bounds of the 95% confidence interval rule out an approximately 5% or greater reduction and a 5.5% or greater increase in posted wages.

## 5 Conclusion

The battle between the minimum wage and inflation has been one on the minds of economists for generations. Stigler, for instance, playfully noted in 1946 that “the minimum wage provisions of the Fair Labor Standards act of 1938 have been repealed by inflation”. While this accepted view makes sense, in this work we present evidence to the contrary.

We leverage a unique natural experiment for identification, 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 is novel evidence of wage hysteresis from temporary labor market policy. While theories of downward nominal wage rigidity could explain the persistence of raised wages for workers treated by the policy (even when high wages are no longer legally required), such theories predict that inflationary episodes should serve to reduce real wages in treated places relative to non-treated places. Surprisingly, this is not the case: the wage gains persists even in a period of generationally high inflation, with no sign of a negative real wage adjustment, i.e. an erosion of the wage gap that treated workers enjoy relative to unaffected workers. A leading candidate explanation for our results is a model where reservation wages are permanently affected by the temporary minimum wage policy, as in [Falk et al. \(2006\)](#). This model

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<sup>18</sup>Using the whole country as a control leads to poor pre-trends.

seems to explain the facts better than some alternative theories.

More broadly, our findings provide greater understanding of the dynamics of wage-setting behavior, indicating that equilibrium wages depend on the history of policy changes and not only on current policy - even when it contradicts past policy. For policymakers, especially those operating in environments with divided governments and uncertain policy survival, the upshot 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 reigns for the moment, play it if you've got it.

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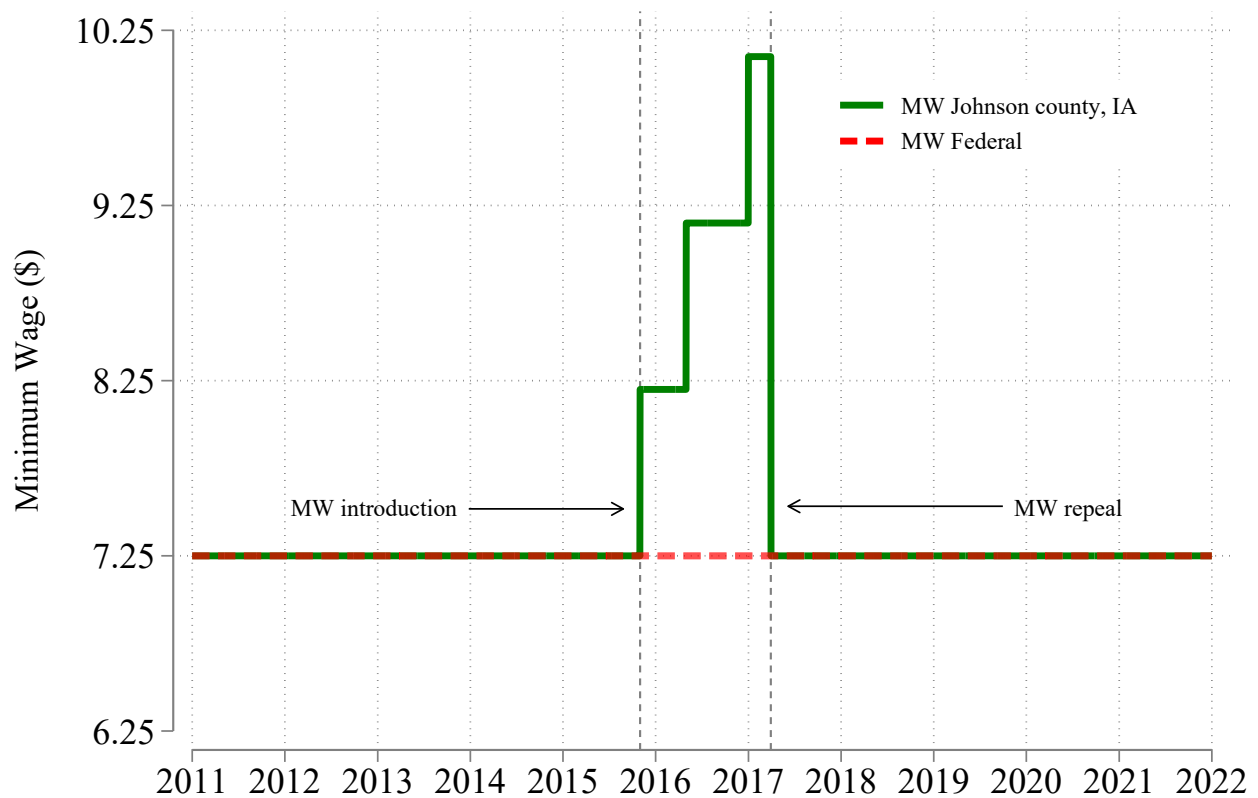
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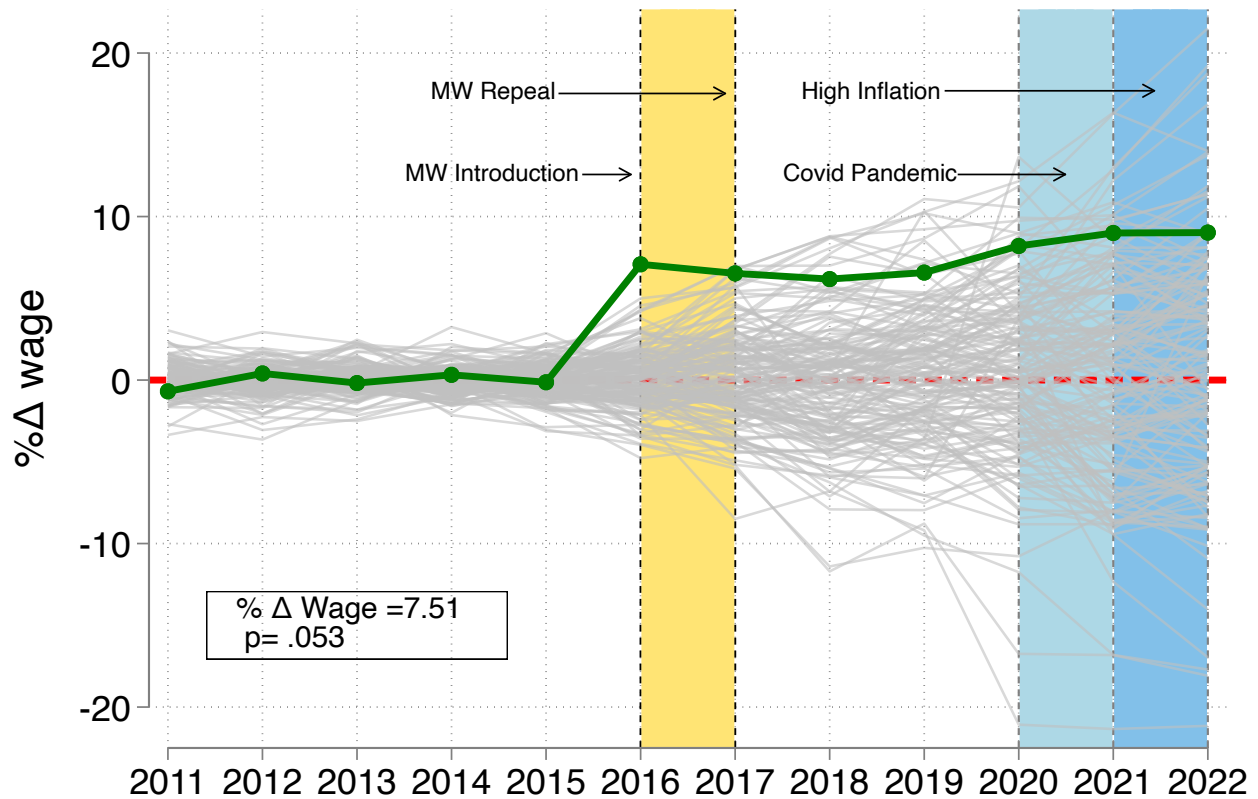
**Figure 1: Minimum Wage**



*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 minimum wage was first introduced on 1st November 2015, and then increased twice: on 1st May 2016 and 1st January 2017. It was repealed on 1st April 2017, getting back at the initial level: the Federal minimum wage.

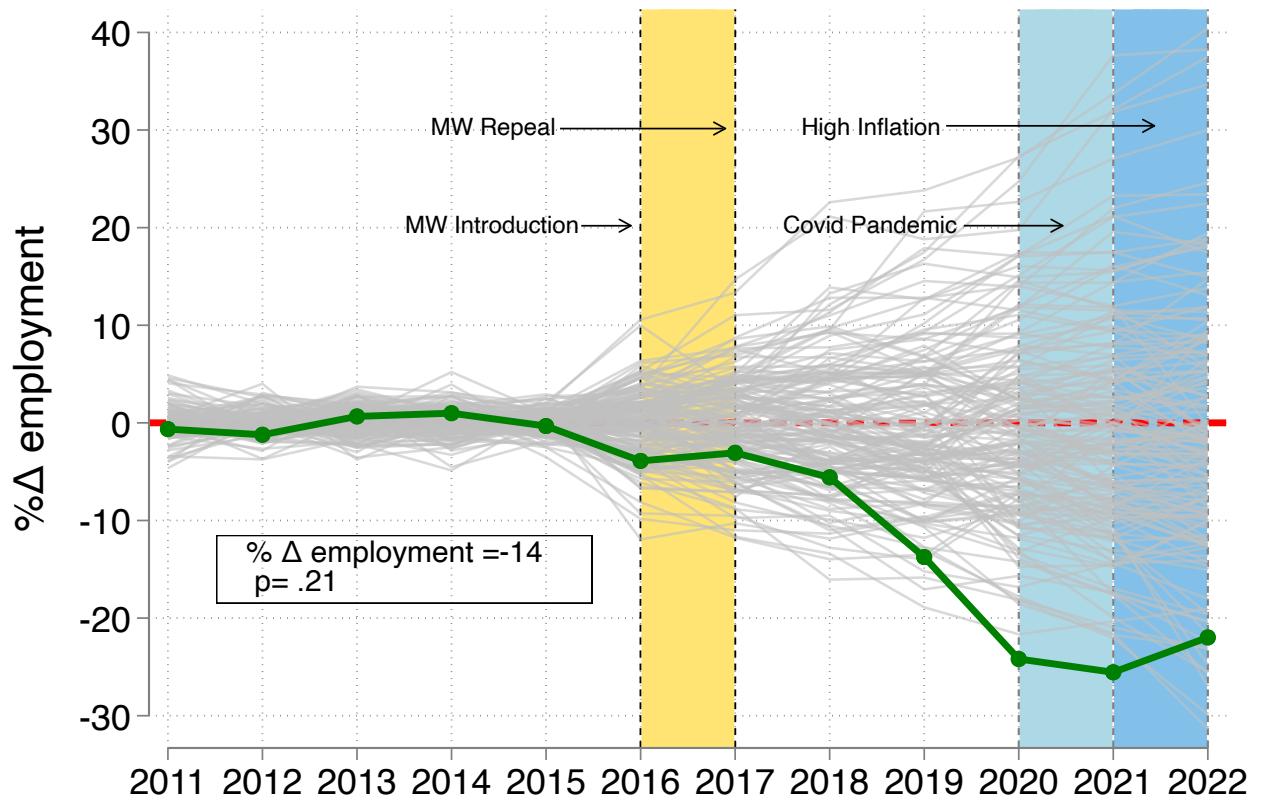


**Figure 2: Effect on Wages**



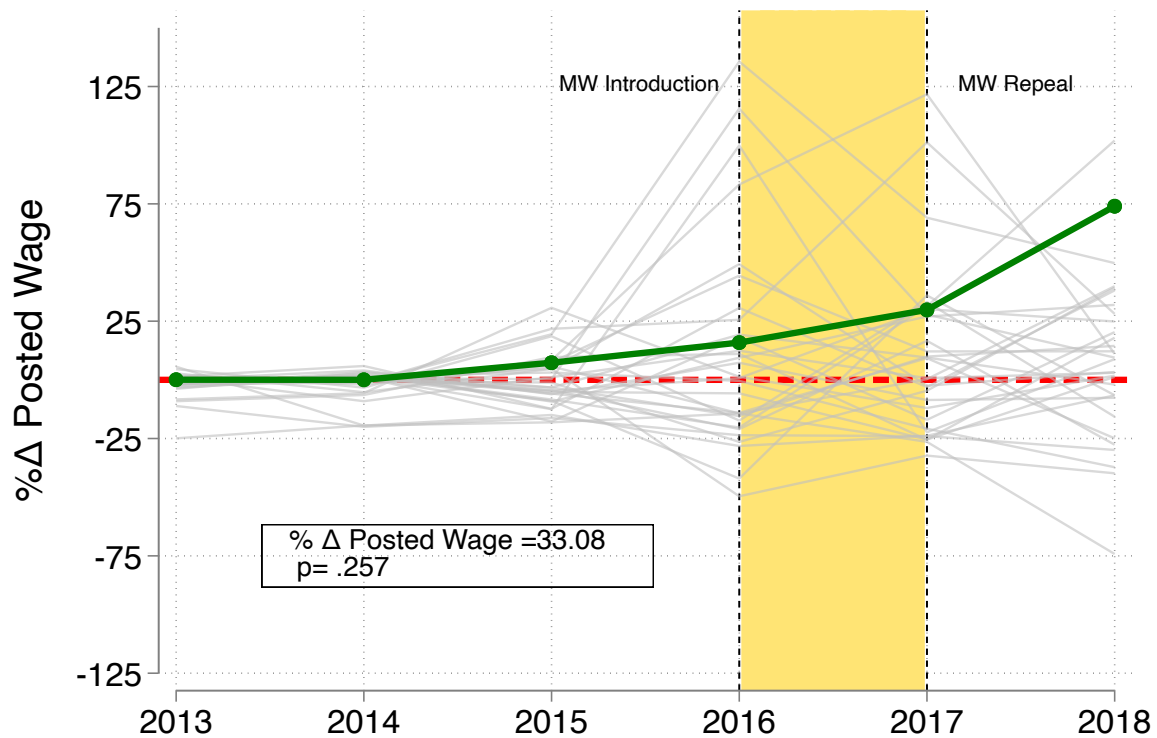
*Notes:* The graph shows the impact of temporary minimum wage policy on restaurant wages in Johnson county, Iowa. The green line depicts the relative gap in average wages in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average wages for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). 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 a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers in Johnson county were legally obliged to pay above Federal minimum wage (see Figure 1). The two blue areas correspond to the Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

**Figure 3: Effect on Employment**



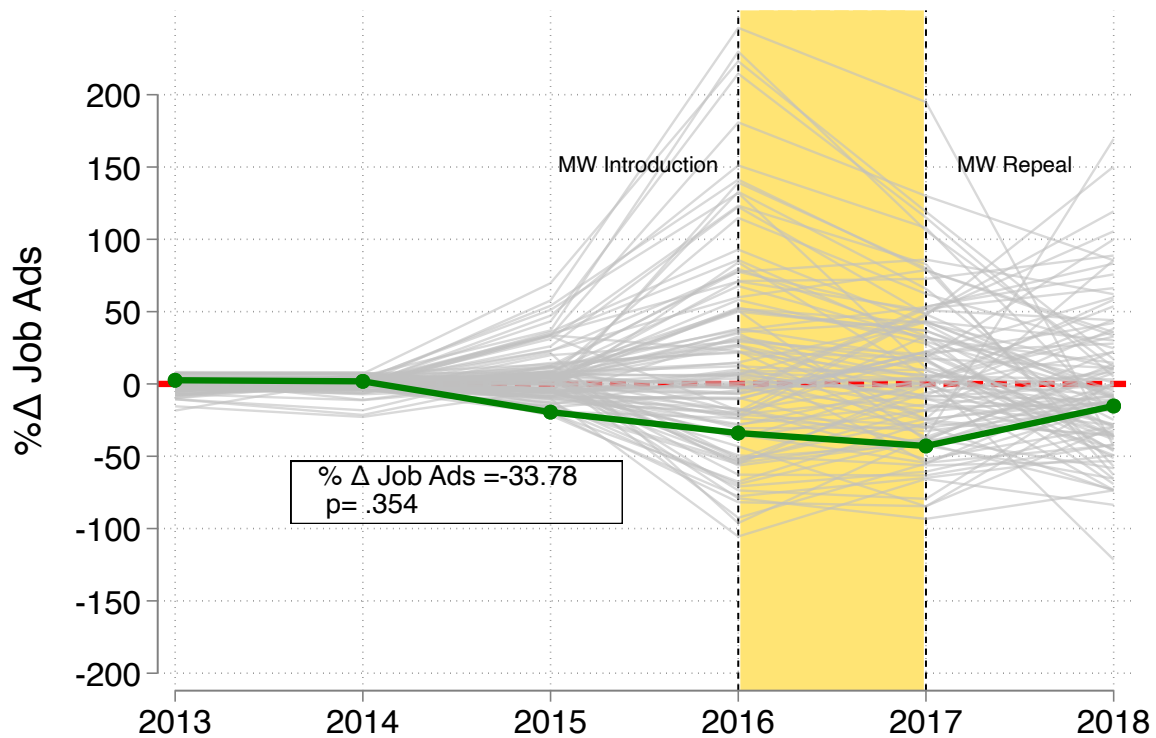
*Notes:* The graph shows the impact of temporary minimum wage policy on restaurant employment in Johnson county, Iowa. The green line depicts the relative gap in average employment in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. 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, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). 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 a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. 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 Pandemic Recession and the high inflation period. The annual outcomes are aggregated from quarterly frequency.

**Figure 4: Effect on Posted Wages for New Hires**



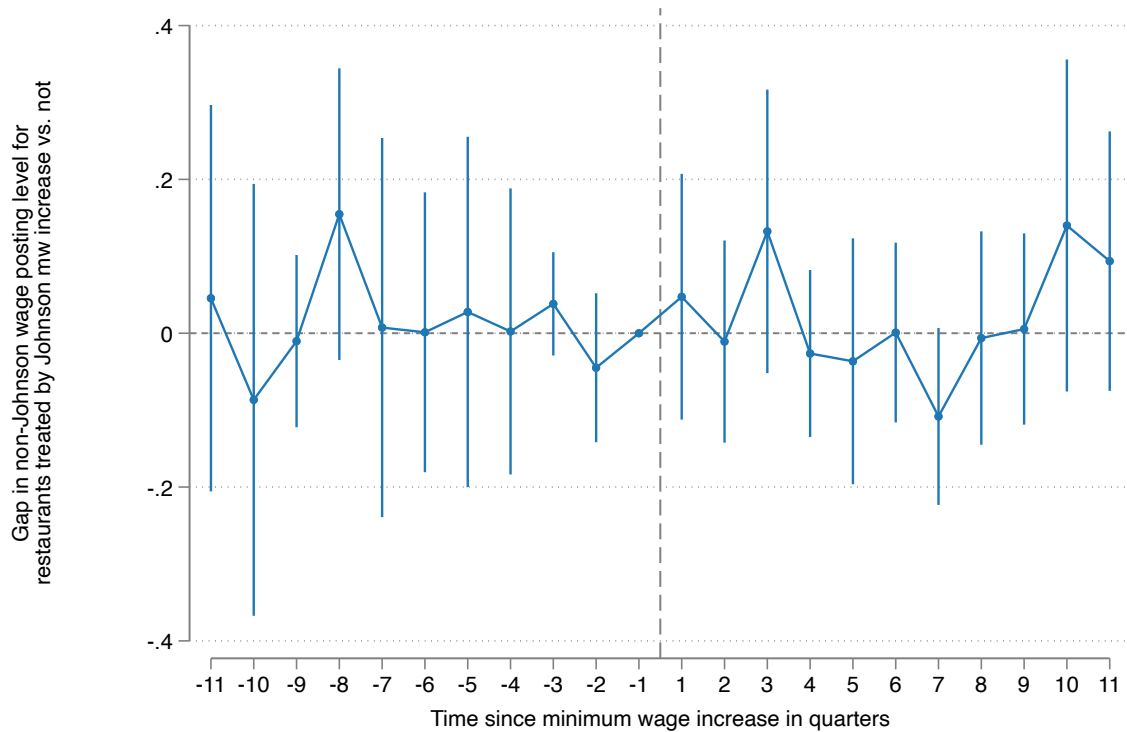
*Notes:* The graph shows the impact of temporary minimum wage policy on the advertised wage in new online jobs postings in the restaurant sector in Johnson county, Iowa. Data comes from Burning Glass data that includes wage information (with the lowest posted wage in the wage range used when a range is given). The green line depicts the relative gap in average posted wage in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average posted wage for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). 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 a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The annual outcomes are aggregated from quarterly frequency.

**Figure 5: Effect on Number of Job Postings**



*Notes:* The graph shows the impact of temporary minimum wage policy on online job postings in the restaurant sector in Johnson county, Iowa. Data comes from Burning Glass data. The green line depicts the relative gap in average number of online job ads in Johnson relative to synthetic Johnson, constructed using a Synthetic Control approach as described in the main text. Gray lines refer to placebo estimates, where we plot the relative gap in average number of online job ads for non-treated units relative to their synthetic version. For easier visual inspection, placebo estimates with relatively bad pre-treatment fit (pre-MSPE larger than three times the one in Johnson) are not shown, but the p-value shown in the plot is computed using all units and accounts for pre-treatment fit (ratio of post-MSPE to pre-MSPE). 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 a local minimum wage repeal (either of an effective or anticipated local policy). Furthermore, the pool is restricted to counties with average pre-reform employment in the sector of study between 2000 and 11000. The yellow area corresponds to the period where employers were legally obliged to pay above Federal minimum wage. The annual outcomes are aggregated from quarterly frequency.

**Figure 6: Mandated Minimum Wage Increase for One Establishment Induces no Change in Advertised Pay in External Establishments**



*Note:* The graph shows the development of the average gap in log posted wage for non-Johnson county restaurant jobs between treated and control firms. Treatment is defined by exposure of a firm that operates both inside and outside Johnson county to the Johnson county minimum wage law (i.e. posting of jobs in Johnson prior to repeal of the Johnson minimum wage). Control firms are those that operated outside Johnson county but not in Johnson county before repeal (and thus were never exposed to the Johnson minimum wage). The firms operate establishments in a county in Iowa outside of Johnson or in neighboring states that also have no state minimum wage, Kansas and Wisconsin. The vertical dashed line represents the onset of the local minimum wage in Johnson (it took effect in quarter 1). The difference-in-difference results are scaled relative to the gap in posted wage just prior to the local policy onset (quarter -1). The specification includes county, quarter, and employer fixed effects. Averaging across all post-treatment periods, there is a statistically insignificant 0.004 estimate (p-value of 0.89), or, less than half of a percent change in the posted wage gap of treated and control firms, consistent with the reservation wage interpretation of the mechanism underlying the main wage hysteresis result (see the main text). With the 95% confidence intervals we can rule out an approximately 5% or greater reduction and a 5.5% or greater increase in posted wages by treated firms to their non-Johnson establishments.