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ABSTRACT

Simple theoretical models of job search can imply that a higher minimum wage increases the number of job seekers for affected jobs. Researchers often appeal to these models in defending or explaining non-negative estimated employment effects of minimum wages. We use novel data on job search in U.S. states to examine the effect of minimum wage increases on the number of job seekers for low-skilled positions. We find little if any evidence that higher minimum wages increase job search for low-skilled jobs, and more evidence that higher minimum wages decrease the number of workers seeking employment in these jobs.

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1 Introduction

Analyses of the effects of minimum wages in job search models have focused on job search by the unemployed, which – in some simple formulations of job search models – is predicted to increase in response to a higher minimum wage. The job search response among the unemployed could lead to higher employment, because it could lead firms to open up more job vacancies and fill more jobs (e.g., Flinn, 2006; Manning, 2021). However, this prediction does not necessarily hold in search models where the job-finding rate is endogenous (e.g., Mincer, 1976), and the prediction is also undermined by models that expand job characteristics beyond the wage (Clemens, 2021). Moreover, Faberman et al. (2022) show that job search while on the job is widespread and constitutes a large majority of search activity for low-skilled occupations, and the predicted effects of minimum wage increases on search while employed may be negative – an additional reason a higher minimum wage may not increase search or employment. Thus, understanding how minimum wages impact job search is informative about the potential for higher minimum wages to generate non-negative or positive employment effects.

We use novel data from one of the three largest job search platforms in the United States, covering January 2021 to September 2023, to estimate the effects of minimum wage increases on the number of job seekers for low-skilled occupations. While the existing empirical literature on search behavior predominantly relies on self-reported survey data (the Current Population Survey (CPS), the Survey of Income and Program Participation (SIPP), or the American Time Use Survey (ATUS)), our dataset is unique in that we directly observe job seekers' behavior on the platform via tracking user interactions.[1]

There are three main advantages of our job search data over existing survey data. First, our data captures actual behavior through clicks, avoiding self-reporting error.[2] Second, our ability to focus on specific low-wage occupations within a single platform provides a core ad-

[1]Our job search measure represents the number of unique platform users, identified via tracking cookies, who clicked on at least one job posting for a given job title in a state-month. Thus, we do not measure search effort or intensity, such as the number of clicks or applications submitted, but what we interpret as the number of job seekers.

[2]We are unable to observe search intensity at the individual level. The platform identifies unique users with tracking cookies, but the data we receive are aggregated by job title, state, and month. Within each job title, the tracking cookie prevents double-counting, so a person clicking multiple postings with the same title in a state-month is counted only once. Thus, while we capture the number of distinct platform users searching within each job title, we cannot construct a measure of per-person search effort.

vantage over existing studies that do not measure the jobs where searching occurs, but rather measure aggregate search.[3] In contrast, our data measures search for occupations most likely to be affected by minimum wage changes. Nonetheless, our results are complementary to similar findings reported by Adams et al. (2018); while their survey data measures aggregate search behavior, they are able to stratify by education. Third, our data capture search behavior regardless of employment status, which is critical in light of the evidence in Faberman et al. (2022) that the majority of job search comes from already-employed workers. By way of contrast, the CPS data used by Adams et al. (2018) do not capture any search activity by employed workers (who are not asked about search). Their SIPP data show only 2.6% of employed workers searching – a likely undercount since the SIPP only asks those who did not work in the prior four weeks about job search, it does not ask all continually employed respondents. As the authors note, even the ATUS time-use data understate on-the-job search since workers record only their primary activity, meaning someone searching while at work would almost surely be coded as working rather than searching.

A good part of our analysis focuses on two "unscheduled" minimum wage increases – in Nebraska and Hawaii. By "unscheduled," we mean that the increase was not a subsequent step legislated earlier, or an indexed increase. For this analysis, we use a difference-in-differences strategy that compares changes in job search in the treated states to changes in other states whose minimum wages did not change. In these core analyses, we find no evidence that higher minimum wages increase job search, and instead that, if anything, minimum wage increases are associated with substantial decreases in the number of job seekers for low-skilled occupations. Including additional analyses of other state minimum wage increases – although these provide less clean "experiments" because the increases are scheduled and hence known in advance – we continue to find essentially no evidence that higher minimum wages increase search for low-skilled jobs, and more evidence pointing to negative effects.

In our view, these findings challenge a core contention in the minimum wage literature – that a higher minimum wage induces a positive search response among unemployed workers, which can in turn generate non-negative employment effects. Across many analyses, we do not find

[3] However, we cannot observe whether reduced search in these categories is offset by increased search elsewhere.

any evidence of statistically significant positive search responses, and the evidence points more to negative effects. Although we cannot parse the reasons (i.e., we cannot rule out a negative response owing to lower returns to search for the unemployed, or a response to deteriorated job characteristics), we believe that the evidence against a positive search response may be most attributable to on-the-job workers being responsible for a large part of job search efforts (consistent with Faberman et al., 2022). Employed job seekers likely outnumber unemployed job seekers, and they may be especially inclined to remain in their current jobs and reduce search activity when the minimum wage increases.

2 Minimum Wages and Job Search: Theory and Evidence

The neoclassical theory of labor demand predicts that a higher minimum wage will reduce employment of the least-skilled workers. There is a great deal of evidence consistent with this prediction, but there are also some estimates in the research literature that point to no effects or even positive effects on low-skill employment (Neumark and Shirley, 2022). Some researchers interpret the overall body of evidence, then, as inconsistent with the neoclassical model. They argue, instead, that frictions in labor markets give rise to a monopsony-type result where a higher minimum wage can increase low-skilled employment (e.g., Manning, 2005, 2020; Schmitt, 2015; Wiltshire et al., 2024). Studies that cite monopsony-like explanations of positive or non-negative minimum wage effects include, for example, Card and Krueger (1994), Derenoncourt and Montialoux (2021), Dube (2019), and Giuliano (2013).[4]

According to this argument, because of labor market search frictions, "not all workers who want a job manage to get one, and not all employers who want to hire a worker manage to find one" (Manning, 2021, p. 21). Under such conditions, both unemployment and job vacancies can exist in equilibrium. A higher minimum wage reduces labor demand, ceteris paribus, but it can also increase labor supply – i.e., the number of job seekers. It is then possible that the increased number of workers searching for jobs can induce firms to post more job vacancies,

[4]Search responses to minimum wages are used more broadly in the labor economics and public finance research – not only to rationalize or explain non-negative employment effects. Job search responses to minimum wages are routinely invoked in theoretical models of the minimum wage (e.g., Gregory and Zierahn, 2022; Blömer et al., 2024; Gautier and Moraga-González, 2018). In the public finance literature, search responses are used in work aimed at determining an optimal minimum wage (e.g., Chéron et al. (2008)) and modeling of the minimum wage as part of optimal tax and transfer policy (e.g., Lavecchia, 2020, and Hungerbühler and Lehmann, 2009).

when they believe there is an increased probability of filling them with productive workers, and the net effect may be to increase low-skilled employment, in contrast with the neoclassical model's prediction.[5] Search models in this framework are often referred to as models of "dynamic monopsony."

There are other models that can create upward-sloping labor supply curves to the firm, and hence generate monopsony-like responses that can lead to a higher minimum wage increasing employment. Rebitzer and Taylor (1995) generate this response in an efficiency wage model. In their model, firms have two ways to create incentives for workers: they can monitor carefully and fire a worker if they "shirk," or they can monitor less, lowering the probability of detection of shirking, but pay an above-market wage so that the value of the job, relative to another job or being unemployed, creates incentives not to shirk. Suppose there is only one manager monitoring workers. If the firm expands employment, the manager's ability to monitor each individual worker declines, and the firm may thus raise the wage to deter shirking. This creates the same phenomenon as in the classical monopsony model – that the marginal cost of labor curve is steeper than the labor supply curve – in which case an aptly chosen minimum wage can increase employment (Stigler, 1946). Another explanation of monopsony-type effects is unique to the "table-service" part of the restaurant industry, where part of workers' compensation comes as tips (Wessels, 1997). If a restaurant adds a waiter, then on the margin (i.e., with no other changes) the tips earned per waiter decrease. In order to maintain the elevated level of employment, the restaurant would need to pay a higher wage to all tipped workers, again implying that a higher minimum wage can increase employment. A third explanation is static, explaining employer labor market power as resulting from workers' idiosyncratic tastes for different combinations of job amenities that are offered by small numbers of firms, and which are not fully priced into wages (Manning, 2020).

Each of these alternative models may apply in some contexts. However, we believe it is fair to say that a model of search and matching is the canonical model that economists rely on to generate the behavioral prediction that a higher minimum wage may increase employment, and to rationalize findings of non-negative employment effects. For example, in recent work

[5] The employment effects do not always have to be non-negative in the search-and-matching framework.

Hurst et al. (2022) use a search and matching model to analyze the distributional and long- and short-run effects of the minimum wage. Wiltshire et al. (2024) similarly emphasize search and matching frictions in their recent study of the fast-food industry.

The argument is often quite explicit:

> "Non-competitive labor market models recently have received increased attention in the minimum wage literature, in part because of the positive employment effects reported in Card and Krueger (1994) and their dynamic monopsony explanation Card and Krueger (1995)." (Dube et al., 2007, p. 524)

> "The claim that the employment effect might not be negative continues to be met with incredulity in some quarters or to be euphemistically described as "unconventional." But as soon as one acknowledges that efficiency wage effects might be important or that labor markets have frictions – ideas that appear in mainstream introductory-level textbooks and can hardly be described as unconventional – one has to acknowledge that the impact of the minimum wage on employment is theoretically ambiguous." (Manning, 2021, p. 22)

The effects of minimum wages in search models depend on the endogenous search responses of job seekers (Acemoglu, 2001). The prediction that a higher minimum wage increases the number of job seekers, all else equal, is clearest in a context where only jobless workers search. Leading examples of such models include Flinn (2006) and Manning (2005, Chapter 12). The simple prediction that arises from the restrictive focus on job search among the jobless is the cornerstone of models of the minimum wage and job search, and of the arguments that search-and-matching models can explain non-negative employment effects of minimum wages (e.g., Gavrel et al., 2010; Gorry, 2013; Lavecchia, 2020).

However, the theoretical prediction is actually ambiguous except in restrictive models. When the job finding/job arrival rate is fixed and exogenous, a higher minimum wage increases the reward to working but does not impact the likelihood of finding a job, and hence also increases the return to search. However, when the job finding rate is endogenous, the returns to search can change — and in particular, decline. For example, Mincer (1976) models the probability of finding a job in the covered sector (where the minimum wage binds) as a function of the separation rate, employment in the covered sector, and unemployed workers searching for minimum wage jobs (which can increase in response to a higher minimum wage). In his model, labor can flow out of the covered sector (and not increase as much in the uncovered sector because the wage there falls), potentially reducing overall employment and making job search less attractive. In the model in Flinn (2006), the effects of a higher minimum wage differ substantially depending on whether the "contact" rate is assumed exogenous (and Flinn notes that a general equilibrium perspective implies an endogenous contact rate, p. 1059).[6] Lavecchia's (2020) model is perhaps clearest, modeling the externality from search congestion, which can lower the job finding rate when the minimum wage increases, depending on how the higher minimum wage also impacts the job vacancy matching rate (which increases hiring and output).

Moreover, once we allow for on-the-job search, which Faberman et al. (2022) show is the bulk of job search, the prediction that a higher minimum wage increases search is further undermined.[7] Faberman et al. (2022) show that search effort from the employed is also more elastic. Thus, the net effect of minimum wage changes on the overall number of job seekers will likely depend on its effect on this largest and most responsive group of potential job seekers. The model in Liu (2022) suggests that job search from low-wage workers will decline following a minimum wage increase. This result comes from two channels that are modeled as occupational mobility but carry over to search more generally (Liu, 2022). The first is a "wage compression" channel: minimum wage increases reduce wage dispersion, decreasing the benefit from switching jobs, thereby reducing the number of job transitions.[8] The second is via employment effects: minimum wage increases reduce job postings, and the reduction in the likelihood of job accession reduces the incentive for individuals to engage in job search.[9] Thus, there is an additional theoretical reason – via wage compression – that workers in minimum wage jobs will reduce job search in response to a minimum wage increase. Given the evidence from Faberman et al. (2022) that most low-skilled job search comes from already-

[6] Flinn (2006) does not find evidence that the contact rate changed in his empirical analysis, but notes that this evidence is limited to a small policy intervention (minimum wage change).

[7] This is also noted in Flinn (2006) and Flinn and Mabli (2008).

[8] Although not relevant to our inquiry, this effect also leads to greater occupational mismatch.

[9] However, in many search models, as discussed above, postings can increase in response to a higher minimum wage.

employed individuals, the overall effect of a minimum wage increase may be more likely to decrease rather than increase search.[10]

In addition, recent work (Clemens, 2021; Clemens et al., 2021) emphasizes the role of non-wage compensation and job amenities in mediating the effects of minimum wages. The core idea is that a higher minimum wage can lead to lower benefits, perhaps most notably health insurance – consistent with the evidence in Marks (2011) and Clemens et al. (2018). A higher minimum wage can also change job amenities and conditions towards those that are more favorable to employers and less favorable to workers – like higher effort requirements, as can occur in the model of Coviello et al. (2022) and Ku (2022), less scheduling flexibility for workers, or diminished workplace safety (Liu et al., 2024; Davies et al., 2025).[11] The demand curve shifts out from these changes (because they reduce labor costs). But the changes in job characteristics to the detriment of workers would lead to the labor supply curve shifting in. The net effect is that the low-skilled labor market can arrive at a new equilibrium in which employment is not necessarily lower, and fewer workers are interested in minimum wage jobs – i.e., search is lower, not higher.[12] While these models of other responses to minimum wages are not search models per se, they provide another reason there can be non-negative employment effects of minimum wages without having to appeal to search models that could – under some circumstances – explain the same evidence.

Thus, it is theoretically ambiguous whether search frictions imply that a higher minimum wage increases job search. And consideration of a richer set of job attributes than just the wage – as emphasized by Clemens (2021) – also does not generate a clear prediction that higher minimum wages increase search. In other words, ultimately the impact of a higher minimum wage on job search is an empirical question – one that the novel data in this paper are uniquely

[10]These search responses – both from wage compression affecting employed workers and from reduced job-finding rates affecting unemployed workers – may vary with labor market conditions. Effects are likely smaller in tight markets, where job-finding prospects remain strong, and larger in slack markets, where the ratio of job seekers to vacancies increases. This remains an important area for future research (see Clemens and Strain (2021) for evidence on employment effects varying by labor market conditions).

[11]For related evidence, see Nagler and Winkler (2026).

[12]In addition to jobless individuals and those employed in minimum wage jobs, a third group of labor market participants comprises those employed in non-minimum wage jobs. While our data cannot distinguish between these worker types, the literature acknowledges their relevance. For example, Phelan (2019) demonstrates that minimum wage increases can alter the composition of compensation by changing the mix of wages and amenities, potentially making certain lower-wage jobs more attractive to workers previously employed in jobs paying somewhat above the minimum wage.

situated to answer.

Despite the many papers using search and matching models, there is very little direct evidence that higher minimum wages increase search for low-skilled jobs. Some research provides indirect evidence, such as whether individual labor force participation responds positively to a higher minimum wage. Boffy-Ramirez (2019) finds that labor force participation decreases in response to a higher minimum wage, which could imply reduced search. Dube et al. (2016) report evidence that a higher minimum wage reduces both separations and accessions among affected workers. They argue that this is consistent with a model with search frictions – in particular, a job ladder model where higher minimum wages reduce the arrival rate of better-paying jobs. Wursten and Reich (2023) present evidence that higher minimum wages increase commuting via automobile by Black workers, which they interpret as reflecting a wider job search radius. We do not regard any of this evidence as establishing decisively whether aggregate job search increases in response to higher minimum wages.

We have identified two papers that provide more direct evidence on time spent searching for jobs – on both the intensive and extensive margins. Piqueras (2023) uses combined data from the CPS and the ATUS and reports that higher minimum wages increase intensive search effort (time spent searching). However, he finds that individuals who increased search effort did not find jobs faster. Through the lens of a search model, he interprets this as reflecting a corresponding reduction in labor market tightness. Adams et al. (2018) also use CPS and ATUS data, as well as data from the SIPP. They find no impact of higher minimum wages on the likelihood of searching (their estimate is sufficiently precise to rule out even small increases) and only short-term spikes in search effort by those already looking for work.[13]

These last two papers provide perhaps a little support to the idea that increased job search because of a minimum wage increase could, in principle, lead to rising employment. However, Piqueras (2023) fails to find an employment effect, and Adams et al. (2018)'s results do not provide evidence of the sustained increase in search that would seem necessary to motivate firms to create additional jobs. Moreover, neither paper directly measures changes in job applications. Also, time-use data cannot distinguish whether more time spent searching reflects

[13] In the published version of this paper, Adams et al. (2022) use only the ATUS data.

job seekers applying to more jobs in an expanded set of job postings, or additional search due to increased difficulty in obtaining a job offer (because the minimum wage reduces the number of jobs posted). In search models, the first effect could indicate that minimum wage increases lead to more job seekers, which could induce firms to post more vacancies and hire more workers, whereas the latter effect could simply reflect neoclassical labor demand effects. Finally, as noted above, both Adams et al. (2018) and Piqueras (2023) rely on survey data that does not fully capture search by employed workers – who make up the majority of job seekers in low-wage labor markets – thereby potentially missing important behavioral responses among those already working in minimum wage jobs.

Our empirical findings of reduced search activity following minimum wage increases can be interpreted through the conceptual frameworks outlined above. The negative search response is inconsistent with theoretical predictions of increased search from higher wages and instead suggests that other mechanisms play a more important role in determining search behavior. As discussed above, this evidence is consistent with either: (1) reductions in job vacancy rates that lowers the expected returns to search for unemployed workers; (2) wage compression effects that reduce job search incentives for currently employed workers, who constitute the majority of searchers in low-wage occupations (Faberman et al., 2022), or (3) deterioration in non-wage job characteristics that make minimum wage employment less attractive overall.

3 Data

3.1 Data from Online Job Search Platform

Our primary data source is one of the three largest job search platforms in the United States, with hundreds of millions of monthly visits and millions of job postings across all major occupational categories. The platform contains job postings compiled through a combination of direct employer submissions, sponsored listings, and aggregating (scraping) publicly available job ads from company career pages and other job boards. It reaches over 90 percent of online U.S. job seekers, suggesting that any substitution across platforms is likely to be modest in scale.

We obtained monthly data at the state level on the number of job seekers accessing individual job postings from January 2021 through September 2023. On this platform, job postings are sorted into occupational groups based on the job search platform's algorithm. This should ensure similarity across states. We target occupations likely to be affected by the minimum wage, paralleling the existing literature on minimum wages and employment.

In particular, we focus on occupations with a low hourly wage to make it more likely that minimum wages would affect wages in those jobs. We identified the occupations with mean hourly wages below $18 per hour using the May, 2023, National Occupational Employment and Wage Estimates (U.S. Bureau of Labor Statistics, 2023a).[14]

We collected data on job seekers for job titles that match with low-wage occupations and that have at least 10,000 monthly job seekers nationwide on the platform. In particular, we obtained job search data for the following job titles: cashier, retail salespersons, janitor, cleaner, receptionist, crew member, cook, dishwasher, and host.[15] To avoid unnecessary specificity, our state-specific analysis aggregates these occupations into either retail and cleaning or food-related occupational categories, as follows:[16]

- Retail and Cleaning Occupations: cashier, retail salespersons, janitor, cleaner, receptionist
- Food-related Occupations: crew member, cook, dishwasher, host

For any given state-month observation, our data provide a measure of search activity on the extensive margin, indicated by the number of unique job seekers for job listings in an occupational category. The number of job seekers is measured by counting the unique platform users – identified via tracking cookies – who clicked on a job posting's thumbnail. Clicking provides additional information about the job, which we use as evidence of individual-level job search activity for that job. The tracking cookies ensure that in the data we do not double-count a platform user who clicks on multiple job postings with the same title; within each job title, each unique job seeker is only counted once. However, our aggregation of job titles within occupations may lead to limited multiple counting of the same unique user if, for example, that

[14]Table B.2 in Appendix B shows the occupations with the lowest mean hourly wages.

[15]"Crew member" typically refers to individuals employed in entry-level positions within the food service industry. Their responsibilities often include customer service, basic operational tasks, and maintenance duties. Nine out of the 10 top employers in the United States hiring workers with this job title were fast-food restaurants.

[16]Table B.3 shows the comparison of the job titles and the occupations (and wages) associated with them.

person clicked on job postings with the crew member, cook, and dishwasher job titles within the same month. We anticipate any such effect would be relatively constant across space and time and should not impact our estimated effects of minimum wages.[17]

While it is true that smaller independent establishments in low-wage industries such as restaurants still rely on “help wanted” signs and onsite applications (Azar et al., 2024), recent evidence suggests that the sector is rapidly shifting toward online recruitment channels. Reports from the National Restaurant Association, including full-service and fast-food restaurant operators, indicate that major chain restaurants (especially multi-unit operators) have largely migrated to online systems to handle applications and speed up hiring (National Restaurant Association, 2025). For example, in 2024, 86 percent of applicants for Southern Rock Restaurants, which owns McAlister’s Deli franchises, used their mobile devices to apply for a job. Chain restaurants like Wendy’s and McDonald’s (especially corporate-owned or larger franchise groups) direct prospective employees to apply through their official career websites.

Our data cover January 2021 through September 2023. This start date avoids using data from the first year of the Covid-19 pandemic, and coincides with the beginning of the labor market recovery from the pandemic. The seasonally-adjusted U.S. labor force participation rate of 61.3% in January 2021 was the lowest value of the pandemic, apart from the immediate crash and rebound experienced in April 2020. Starting from January 2021, the U.S. labor force participation rate increased at a generally steady rate until September 2023, when it peaked at 62.8% (U.S. Bureau of Labor Statistics, 2023b).[18]

3.2 Minimum Wages, Background and Treatment Timing

During the period we study, Hawaii and Nebraska enacted "unscheduled" minimum wage increases with sufficient pre-treatment data for analysis. As noted earlier, these minimum wage increases differ from "scheduled" minimum wage changes that include increases previously established by specific legislative or ballot initiative language, typically mandating a multi-year set of increases, or increases – typically much smaller – resulting from indexing the minimum

[17] We also present some limited analyses later for individual job titles, where this multiple counting cannot occur; the results are similar.

[18] Descriptive statistics are reported in Table B.1.

wage level.[19]

Hawaii's minimum wage increase was the result of state legislation, HB2510, which made the state the first to legislate a minimum wage of $18 per hour (slated to reach this level in 2028) (Pepper, 2022). The last previous minimum wage increase from $9.25 to $10.10 per hour had occurred on January 1, 2018 (the last of several steps that had been legislated in 2014).[20] HB2510 was introduced on January 26, 2022, passed the House on March 8, passed the Senate in substantially different form on April 12, passed out of conference committee on April 29, and passed full votes of both chambers in early May (Hawai'i State Legislature, 2022). Hawaii's governor had already indicated his support and signed the legislation into law on June 22. The new minimum wage of $12.00 per hour (an increase of 18.8%) became effective on October 1, 2022.

This timeline suggests that workers had several months' advance notice that minimum wages might rise. Relative to the previous legislative actions, the conference committee vote attracted the most media attention and likely served as the clearest signal of the coming minimum wage increase.[21] Because expectations of a change in minimum wages in the near future could affect job search in the present (Karabarbounis et al., 2022; Jha et al., 2024), we use May as the treatment month for Hawaii.

Nebraska's minimum wage increase was the result of a ballot initiative (Nebraska Initiative 433, 2022) that passed on November 8, 2022, with 58.66% support (Ballotpedia, 2022). Initiative 433 was filed on August 20, 2021, and initiative organizers claimed to submit 152,000 signatures on July 7, 2022 (Ballotpedia, 2022). On September 6, 2022, the Nebraska secretary of state reported the campaign had submitted 97,245 valid signatures (out of 86,776 needed)

[19]For discussion of and evidence on indexed minimum wages, see Clemens and Strain (2018). Virginia passed legislation to increase its minimum wage in February 2020. We do not include Virginia in our core analysis of unscheduled minimum wage increases because we would not have pre-treatment data.

[20]At the time of HB2510's passage, Hawaii had already mostly eliminated the tipped credit. The minimum wage for workers receiving gratuities rose from $9.35 per hour to $11.00 per hour on October 1, 2022. HB2510 also scheduled subsequent increases for the non-tipped minimum wage of $14 per hour on January 1, 2024, $16 per hour on January 1, 2026, and $18 per hour on January 1, 2028. It scheduled increases for the tipped minimum wage of $12.75 per hour on January 1, 2024, $14.75 per hour on January 1, 2026, and $16.50 per hour on January 1, 2028 (Hawai'i State Legislature, 2014).

[21]Many news articles covered the approval of HB2510 in early May 2022. Hawaii Public Radio, for example, stated that "The House and Senate approved the measure by wide margins. The bill now goes to Gov. David Ige, who has said he supports an $18 minimum wage." See more here: https://www.hawaiipublicradio.org/local-news/2022-05-03/lawmakers-vote-to-raise-minimum-wage-from-10-10-for-the-first-time-in-over-4-years.

and was eligible for the November ballot (Ballotpedia, 2022). Since there was no way for workers to know for sure that the ballot initiative would pass before November, we use November 2022 as the treatment month for Nebraska.[22]

Initiative 433 raised Nebraska's minimum wage 16.7%, from $9.00 per hour to $10.50 per hour, on January 1, 2023.[23] The last previous minimum wage increase was from $8.00 per hour to $9.00 per hour on January 1, 2016, resulting from the November 4, 2014, passage of another ballot initiative – Initiative 425, which passed with 59.47% of the vote (Ballotpedia, 2014).

Our analysis of these two unscheduled minimum wage increases involves a difference-in-differences strategy that compares the changes in low-wage job search in Hawaii and Nebraska to a group of control states that have not experienced nominal minimum wage changes since the last federal minimum wage increase in 2009. We focus on these control states with no recent history of minimum wage changes to avoid potential contamination of the controls by longer-term effects of the minimum wage, which have been highlighted in some recent research (Meer and West, 2016; Aaronson et al., 2018).[24]

Minimum wage increases in some of the remaining states are incorporated into additional analyses discussed below. But we exclude them from our core analyses of the Hawaii and Nebraska minimum wages because they had previously scheduled minimum wage changes during the period of analysis, changed their minimum wage laws in a relatively recent window before the time period of our analysis, or had an unscheduled minimum wage increase right before the period of analysis (this is only the case for Virginia).[25] Information on the minimum wage treatments and controls is provided in Figure 1 and Table 1.

[22]Economists or politically-knowledgeable readers might reply that the vast majority of ballot initiatives to increase the minimum wage are successful. However, we do not assume that the average minimum wage worker would know and act on this information.

[23]Initiative 433 did not change Nebraska's tipped wage credit. The minimum wage for workers receiving gratuities remains $2.13 per hour (equal to the federal minimum wage for such workers). It also scheduled additional minimum wage increases to $12.00 per hour on January 1, 2024, $13.50 per hour on January 1, 2025, and $15.00 per hour on January 1, 2026. It indexed further annual increases to the CPI-U of the Midwest Region.

[24]There was inflation during our sample period, which eroded the value of minimum wages in both treatment and control states. However, as long as this inflation is similar across states, which seems true to the first-order, inflation should not lead to any parallel trends violation.

[25]Table B.4 in the appendix shows the minimum wages for each state from 2018 to 2023 and indicates how a given state is classified for our different analyses.

4 Empirical Strategy

4.1 Baseline Analysis

We first consider an ordinary least squares regression with two-way fixed effects (state and month-year) where all states are included. We examine the effect of changes in minimum wages on the number of job seekers for low-skilled jobs. We estimate the specification:

$$Log(\text{JobSeekers})_{it} = \beta_1 \times Log(\text{MinimumWage})_{it} + \gamma_i + \sigma_t + e_{it}, \tag{1}$$

where i indexes the state and t indexes each unique month-year. $Log(\text{JobSeekers})_{it}$ is the natural log of the number of unique job seekers in each of our two occupation categories in a given state and month-year.[26] $Log(\text{MinimumWage})_{it}$ is the natural log of the nominal minimum wage for each state in each month-year. γ_i is a vector of state fixed effects, σ_t is a vector of month-year fixed effects, and e_{it} is the error term.

In this initial analysis, we include all 50 states regardless of when legislation was passed to change their minimum wages or whether the changes in minimum wages were scheduled or unscheduled. There are potential complications in this analysis. For example, the effects of scheduled minimum wage increases may differ because expectations of job seekers in these states could lead to changes in behavior before the actual minimum wage increases occur. In addition, estimation of minimum wage effects using this approach could be prone to biases highlighted in the recent difference-in-differences econometrics literature (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021), such as dynamic treatment effects contaminating later untreated observations that serve as controls. There are solutions to these issues applicable to the simpler analyses we do of the Hawaii and Nebraska minimum wage increases, which are not applicable to the more general setting with frequent and continuous minimum wage changes. Nonetheless, this evidence is useful as a point of comparison with past studies of the effects of minimum wages on other outcomes (mainly employment) that use this empirical strategy – Gopalan et al. (2021) is a recent example – and as a point of comparison for the staggered-treatment analyses that follow.

[26]We use a logged dependent variable because the scale of the number of job seekers varies greatly based on the population size of each state.

4.2 Primary Staggered Treatment Analysis

In our core analysis, we jointly analyze the Hawaii and Nebraska minimum wage increases using difference-in-differences specifications with state and month-year fixed effects. The comparison group consists of the 20 states with no nominal minimum wage changes during the analysis period. Our baseline specification is:

$$Log(\text{JobSeekers})_{it} = \beta_1 \times \text{Treated}_{it} + \gamma_i + \sigma_t + e_{it}. \tag{2}$$

Here, Treated_{it} equals one for a treated state in months following the relevant treatment date and zero otherwise. For Hawaii and Nebraska, treatment timing is defined as the point at which job search behavior was likely to respond to the anticipated minimum wage increase, rather than the subsequent implementation date; Section 3.2 describes these dates. β_1 captures the average post-treatment difference in log job seekers between the treated states and the comparison states, conditional on state and month-year fixed effects.

We first estimate equation (2) using two-way fixed effects (TWFE) with both Hawaii and Nebraska included as treated states. However, because the two states adopt treatment at different times, this specification involves staggered treatment adoption. Recent work shows that TWFE estimates can be difficult to interpret when treatment effects vary across cohorts or over event time (De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). We use the the doubly robust (augmented inverse probability weighting) method described in Callaway and Sant'Anna (2021) (C&S) as our preferred staggered-adoption approach, and report the TWFE estimates as a benchmark. The C&S estimator allows for estimation and inference on interpretable causal parameters even in the presence of heterogeneous treatment effects and dynamic effects, avoiding the potential problems that can arise when standard TWFE regressions are interpreted as causal effects in staggered-adoption settings. We implement this estimator using both Hawaii and Nebraska as treated states, and, as discussed earlier, the control states are those where the federal minimum wage applied throughout the period.[27] Because there are only two treated states, we implement randomization inference that accounts for Hawaii and Nebraska being

[27]We also report analyses for each of these two state individually in Appendix A.

treated at separate times. This is described in Section 4.4.

4.3 Additional Treated States

Although our primary analysis focuses on the large, unscheduled minimum wage increases in Hawaii and Nebraska, it is useful to examine whether the evidence is qualitatively consistent across a broader set of minimum wage changes during the sample period. We therefore identify additional states that implemented a discrete, relatively large minimum wage increase between January 2021 and September 2023. To ensure a reasonably well-defined event, each increase must be preceded by a period with either no change or only very small increases (approximately 1–2 percent), and any larger prior increase must have occurred more than one year earlier. We treat the implementation of the first large increase as the event of interest, since several of these changes were scheduled or index-linked.[28]

The additional treated states are Delaware, Florida, Maine, Montana, Ohio, Rhode Island, South Dakota, and Virginia.[29] Table 1 reports the treatment month, dollar increase, percentage change, and type of minimum wage change for each event.

For this extended analysis, treatment timing is defined by the implementation month for all ten events, including Hawaii and Nebraska, to maintain a common treatment definition across the pooled sample. We estimate a simple aggregate C&S ATT across the ten treatment events using the same 20 never-treated states as the comparison group. We estimate a simple aggregate Callaway–Sant'Anna analogue of the treatment effect coefficient, which averages the cohort-time effects, across all ten events.[30] This pooled estimate provides complementary evidence on the relationship between minimum wage increases and job search across a broader and more heterogeneous set of policy changes; it is distinct from the primary Hawaii–Nebraska analysis that uses announcement dates to capture the anticipated effects of the two unscheduled increases. We also report state-specific estimates as supplementary evidence. Inference for the

[28]See Table B.4 for details on these minimum wage increases.

[29]Virginia's May 2021 minimum wage increase is included in the extended analysis because its implementation occurs within the sample window and is observed with pre-implementation data. However, because the increase was announced immediately before the beginning of our sample period, Virginia is not included in the core analysis focused on Hawaii and Nebraska.

[30]This aggregate is not an arithmetic average of the ten state-specific estimates reported in Panel B of Tables 5 and 6; rather, it is the pooled Callaway–Sant'Anna aggregate constructed from the underlying cohort-time treatment effects.

pooled aggregate estimate uses the CV3 state-level cluster jackknife (Karim et al., 2026), while the state-specific estimates are accompanied by randomization-inference results, as described in Section 4.4.

4.4 Inference

For the joint, staggered-treatment analysis of Hawaii and Nebraska we do not report conventional state-clustered standard errors. Because treatment is assigned at the state level and these designs contain only two treated states, conventional cluster-asymptotic inference can fail.[31] Instead, we conduct randomization inference by reassigning the relevant treatment timing to control states and comparing the observed estimate with the resulting empirical randomization distribution. This parallels recent minimum wage research by Clemens et al. (2025).

We construct the joint empirical randomization distribution using the pool of 20 control states, excluding Hawaii and Nebraska, iteratively selecting all possible unordered pairs of control states. In each iteration, one control state is assigned Hawaii's treatment timing (May 2022) and the other is assigned Nebraska's treatment timing (November 2022); we form all unordered pairs of the 20 control states. The relevant model is then re-estimated using these two placebo-treated states, and the resulting coefficient is stored as a joint placebo ATT. Because the procedure considers all unordered combinations of two control states from the 20 available, it generates $\binom{20}{2} = 190$ unique placebo ATT estimates.[32] These estimates form the joint empirical randomization distribution against which the observed staggered-treatment estimate is compared.

We report the percentile of the observed ATT in this distribution, ordered from most negative to most positive, along with the placebo distribution and randomization-based confidence intervals. We report both asymptotic and empirical 90% confidence intervals. The asymptotic confidence interval is centered on the mean placebo ATT and uses a normal approximation $\mathrm{CI}_{90} = \bar{\beta}_{\text{placebo}} \pm 1.64 \times SD_{\text{placebo}}$. Here $\bar{\beta}_{\text{placebo}}$ is the mean of the 190 joint placebo ATT

[31] See MacKinnon and Webb (2019, 2020). For the same reason, the corresponding event-study figures report point estimates without conventional cluster-robust confidence intervals. This issue does not apply to the all-state baseline specification, which includes 50 state clusters and for which conventional state-clustered inference is more credible.

[32] For the state-specific Hawaii and Nebraska analyses reported in Appendix A, we instead reassign the relevant treatment timing separately across the 20 control states and report the corresponding rank in each single-state placebo distribution.

estimates and SD_{placebo} is their standard deviation. The empirical 90% confidence interval instead spans the central 90% of the joint empirical randomization distribution. These confidence intervals reflect uncertainty under hypothetical reassignment of treatment timing rather than conventional sampling variation.[33]

The staggered treatment analysis with the additional treated states is likewise assigned at the state level, but the design includes ten treated states and 30 state clusters overall. Conventional asymptotic and multiplier-bootstrap inference can also over-reject in this setting since the number of treated clusters is not large (Karim et al., 2026). We therefore use the CV3 state-level cluster jackknife proposed by Karim et al. for this empirical setting. To align with the notation in specification (2), let $\widehat{\beta}_1^{\text{C\&S}}$ denote the full-sample simple aggregate C&S estimate of the treatment effect, and let $\widehat{\beta}_1^{\text{C\&S},(-h)}$ denote the corresponding estimate after omitting state h. The CV3 variance estimator is

$$\widehat{\text{Var}}_{\text{CV3}}\left(\widehat{\beta}_1^{\text{C\&S}}\right) = \frac{H-1}{H}\sum_{h=1}^{H}\left(\widehat{\beta}_1^{\text{C\&S},(-h)} - \widehat{\beta}_1^{\text{C\&S}}\right)^2 \tag{3}$$

where $H = 30$ is the number of state clusters. We obtain each $\widehat{\beta}_1^{\text{C\&S},(-h)}$ by re-estimating the complete two-step C&S procedure after omitting one state. Thus, each replication recomputes the relevant cohort-time effects and the simple aggregation weights using the reduced sample. We report the square root of equation (3) as the CV3 standard error and construct the associated t-statistic, p-value, and confidence interval using a t distribution with $H - 1 = 29$ degrees of freedom.

5 Results

5.1 Baseline Analysis: All States

Table 2 provides the results associated with specification (1) and include all U.S. states. This specification provides a national baseline that parallels prior research. These baseline results

[33] For these randomization-inference exercises, the placebo SD can be interpreted as a design-based measure of dispersion, where the uncertainty captured is with respect to hypothetical treatment reassignment. This is conceptually distinct from conventional asymptotic standard errors, which characterize uncertainty by appealing to the behavior of the estimator as the number of independent clusters becomes large. Under a sharp null hypothesis of no treatment effect, the placebo distribution measures the dispersion in estimated effects generated by reassigning the observed treatment across comparison states while preserving the empirical treatment-timing design.

suggest that minimum wage increases are associated with decreased job search in low-wage occupations. The estimated elasticity of job search for retail and cleaning jobs with respect to changes in the minimum wage is -0.144 during our period of analysis. The estimated elasticity of job search for food-related jobs is -0.276. Both estimates are economically meaningful and display moderate precision, although only the estimate for retail and cleaning occupations is significant (at the 10% level).[34]

5.2 Staggered Treatment Analysis

Before turning to the primary staggered-treatment analysis, Figures 2 and 3 provide descriptive evidence on the evolution of job search in the two original treated states relative to the control-state average. In each figure, the treated and control series are normalized to 100 in the final pre-treatment month, separately by occupation group.

Figure 2 shows that, prior to the May 2022 announcement of Hawaii's minimum-wage increase, job-search trends in Hawaii closely tracked those of the control states. After the announcement, Hawaii continued to follow the broader control-state pattern but diverged downward in both occupation groups, consistent with a decline in job search. The decline appears to emerge more quickly among retail and cleaning occupations than among food-related occupations. Figure 3 shows a similar pre-treatment alignment for Nebraska. Following treatment, job seekers in Nebraska deviated modestly downward from the control-state trend in both food-related and retail and cleaning occupations. Table 3 summarizes these patterns using pre- and post-treatment mean comparisons. The simple difference-in-differences estimates are negative in both states and occupation groups, ranging from -0.084 to -0.130 log points.

We now turn to the analysis of the minimum wage increases in Nebraska and Hawaii. We first consider the TWFE difference-in-differences estimation (specification (2)). We also utilize the difference-in-differences estimator described in Callaway and Sant'Anna (2021).[35] In all

[34] Although not reported in the paper, we have estimated our specifications with demographic controls, including the median age of respondents, the share of female respondents, the share of white respondents, the share of households residing in metropolitan areas, and the mean number of children per household. Descriptive statistics for these controls are reported in Table B.1. Results are nearly identical with the inclusion of these controls.

[35] Another popular alternative is the estimator described in Wooldridge (2021). However, in our context this estimator yields identical estimates to TWFE. As Wooldridge shows, in a linear balanced-panel model, the coefficient from a conventional TWFE regression is numerically identical to the coefficient from a two-way Mundlak regression that includes: (i) the same time-varying regressors, (ii) their unit-specific time averages, and (iii) their period-specific cross-sectional averages. The equivalence holds when both regressions use the same observations

cases, Hawaii and Nebraska are compared against the control states where the federal minimum wage prevailed throughout the sample period.

Panel A of Table 4 shows the ATT estimated via a standard TWFE analysis, and Panel B shows the ATT estimated via the Callaway and Sant'Anna (2021) method. The average minimum wage increase across the treated states was 17.75% (18.8% for Hawaii and 16.7% for Nebraska). It is also worth noting that the treatment period in Hawaii was 17 months compared to 11 months in Nebraska. Each estimator shows an economically meaningful decrease in the number of job seekers associated with the minimum wage increases (statistically precise for the retail and cleaning occupations). The parameters estimated using the Callaway and Sant'Anna method are smaller than those estimated via standard TWFE. However, all estimates show a substantial decline (between 6% and 11%) in the number of job seekers in treated states. The implied elasticity of job seekers with respect to the minimum wage ranges from about -0.32 to -0.67 across specifications and occupation groups when we compute elasticities using the Hawaii and Nebraska minimum-wage changes as denominator bounds.[36]

Table 4 also reports the results from the randomization inference. Recall that in each case, the placebo treatment corresponds to the assignment of two control states as treated with timing corresponding to the Hawaii and Nebraska minimum wage increases. The statistical evidence is stronger for retail and cleaning occupations. The estimated treatment effect's percentile is 1 for both estimators, implying statistical significance at the 5% level, and the estimate is always outside the 90% confidence intervals. For food-related occupations the percentile ranges from 5 to 12, so the estimates, while negative, are not always statistically significant at the 10% level, although they are clearly among the larger (negative) estimates compared to the placebo estimates. For illustrative purposes, the full empirical randomization distributions for the Panel A estimates are reported in Figure 4. Overall, our staggered adoption analysis suggests that the effect of similar, unscheduled minimum wage increases in two relatively dissimilar states was

and regressors (the double-demeaned regressors have sufficient variation for the coefficient to be identified). The residualized treatment variable is the same under both procedures, so the reported point estimates would coincide exactly. In a standard setting, the standard errors would still differ, but not in the randomized inference we use.

[36]These values are calculated as arc elasticities. We first convert each staggered difference-in-difference log-point estimate into a percent change in job seekers. Because the C&S analysis combines two treated states with different minimum-wage increases, there is no single treatment "dose"; we therefore report a range by scaling the job seeker response using the arc percent changes for both Hawaii and Nebraska separately, which bound the relevant minimum-wage change in the staggered treatment setting.

to meaningfully decrease the number of workers searching for low-wage jobs.

We next examine event-study plots associated with the staggered adoption analysis from the C&S method (corresponding to Panel B of Table 4). Figure 5 shows the event-study plots for both food-related occupations and retail and cleaning occupations. The red points to the right of the vertical dashed line indicate the value of the estimated ATTs for each post-treatment month. In both occupation groups we see evidence of a decline in the number of job seekers. There is no apparent trend in the pre-treatment period (shown to the left of the vertical line). This supports the parallel trends assumption necessary for our causal estimation. The estimation of the ATTs shows statistical noise in both the pre- and post-treatment periods. However, the lack of a consistent trend in the pre-treatment period, and the clear decline in the post-treatment period, help bolster the results shown in Table 4.

5.3 Additional Treated States

Next, we turn to the evidence from the additional treated states. For this analysis, we also present estimates for Nebraska and Hawaii using implementation dates, to correspond more closely to the definition of the minimum wage treatment for the other states. In Tables 5 and 6 we report the estimates for each state, along with the inference results. Each estimate is from the TWFE estimator for the single treated states, relative to the controls (given that there is no staggered adoption for these analyses). For additional evidence, we show Callaway–Sant'Anna event-study plots below, pooling the states.

Looking at the single-state estimates in Panel B, the most important finding is that the estimates are quite consistently negative. For retail and cleaning (Table 5), all but three of the estimates are negative, and the positive ones (for Ohio, Florida, and South Dakota) are small, in the 0.02 to 0.03 range, and not statistically significant. For food-related occupations (Table 6), the evidence is qualitatively similar, although there are four positive estimates and one (for Ohio) is a bit larger. Moreover, for the eight treatment states other than Hawaii and Nebraska, a number of the negative estimates are larger in absolute value, and some are statistically significant at the 10% level—Maine and Montana for retail and cleaning occupations, and Maine for food-related occupations. Overall, then, we regard the evidence from the other states as consistent with our findings for Nebraska and Hawaii: quite clearly there is no statistical evidence of a positive search response, and there is more evidence of a negative search response.[37] Moreover, we find no indication of positive effects, aside from a handful of very small positive effects for some states. To be sure, though, when we estimate effects for a single state and a single increase, the estimates are not that precise, and the larger positive estimates imply sizable elasticities.

Panel A reports the results for the Callaway–Sant'Anna aggregate ATT across states, with CV3 state-level cluster-jackknife inference. Our inference procedure rules out positive effects as large, in absolute value, as the bigger negative effects we estimate, based on the CV3 confidence intervals in Tables 5 and 6.

We assess the feasibility and influence of the CV3 procedure using leave-one-state-out replications. All 30 replications are estimable in both occupational samples. Because four treatment cohorts contain only one state, omitting a state from one of these cohorts removes that cohort from the corresponding replication and changes the set of cohort-specific effects contributing to the aggregate ATT. We therefore assess the full set of leave-one-state-out estimates. This allows us to examine both the influence of individual states and whether these changes in cohort composition alter the pooled conclusion. We therefore view CV3 as an improvement in finite-sample inference, while recognizing that the leave-one-state-out replications do not all correspond to precisely the same fixed aggregate ATT.

The leave-one-state-out estimates indicate that no omission changes the sign of the aggregate ATT. For food-related occupations, the full-sample aggregate ATT is -0.0250, and the leave-one-state-out estimates range from -0.0428 when Virginia is omitted to -0.0123 when Rhode Island is omitted. For retail and cleaning occupations, the full-sample aggregate ATT is -0.0441, and the corresponding estimates range from -0.0507 to -0.0251, with the largest change occurring when Rhode Island is omitted. Thus, no single state determines the qualitative conclusion, although several states meaningfully affect the magnitude of the aggregate estimates.

Finally, Figure 6 shows the Callaway–Sant'Anna event-study plots. These event-study plots point to negative effects that are quite persistent post-treatment, with, as in Tables 5 and 6,

[37] We have examined event-study plots for each of these states, like Figures 2 and 3. For each state, the prior trends look quite parallel. These figures are available from the authors upon request.

stronger evidence of this for retail and cleaning occupations. We interpret these event-study plots as reinforcing the absence of evidence of positive search responses and as providing suggestive evidence of negative effects, consistent with the results for Hawaii and Nebraska in Figure 5.

5.4 Considering Individual Occupations

Last, we report results for narrower jobs within the food-related and retail and cleaning occupations. This is informative about whether the results are perhaps driven by a narrow set of jobs. This analysis also avoids the potential problem associated with counting the same job seeker more than once, since the data provides the unique number of job seekers for each job title (see discussion in Section 3). Given that the alternative to the TWFE estimator made little difference in the analyses reported in Table 4, here we use the TWFE estimator (continuing to include both states).

As reported in Table 7, for jobs in the food-related occupations, we find a large decline in job seekers for crew member jobs (-0.20).[38] This estimate implies an elasticity of the number of job seekers for crew member jobs relative to changes in the minimum wage of between -1.05 to -1.18.[39] The estimated effects for hosts and cooks are negative but smaller, while the estimate for dishwashers is positive but close to zero. Most important, there is no statistical evidence in this disaggregated analysis of food-related occupations indicating that higher minimum wages increase search.

The results for jobs in the retail and cleaning classification are even more consistent. The estimate is negative in every case, and for four of the five jobs the estimate is of a similar magnitude to the aggregate estimate.[40]

We also report the average numbers of monthly job seekers for each job. This shows which occupations may play a bigger role in our main estimates. Retail salespersons is the largest category, followed by cashiers, receptionists, crew members, and hosts. For all of these larger categories, the estimates are negative. And the only positive estimate (albeit small), is for

[38]This is likely the group with lowest wages and more likely to be affected by changes in minimum wages.
[39]These values are calculated as arc elasticities, as described above for the analysis of the combined two treated states.
[40]Our focus here is on the point estimates; we do not report all of the corresponding randomized inference results.

dishwashers, one of the smaller categories. Overall, then, our conclusion is robust. There is no evidence that higher minimum wages increase search, and a good deal of evidence of a negative effect on search.

6 Conclusion

Since the launch of the New Minimum Wage Research in the early 1990s, some economists have used theoretical job-search models to attempt to explain positive (or non-negative) estimates of minimum wage effects on employment. We present novel evidence on the effects of minimum wages on job search using data from a large online job platform, and focus in particular on the effects of two recent unexpected minimum wage increases on job search activity for low-wage jobs.

We find little if any evidence (and no statistically significant evidence) of the positive effect of higher minimum wages on job search for low-skilled jobs that is often claimed or asserted in appeals to labor market search models to explain non-negative employment effects. Instead, the evidence is more consistent with minimum wage increases lowering job search or perhaps having no detectable impact. These results provide evidence consistent with the wage compression channel in the search model of Liu (2022), wherein a reduction in wage dispersion due to the minimum wage dampens job search activity among the employed. Additionally, these findings align with the empirical results of Faberman et al. (2022), which highlight the prevalence and elasticity of on-the-job search, particularly among low-skilled workers. And if the decline in search occurs for unemployed workers specifically – which we cannot tell in our data – this could also be attributable to the mechanism highlighted by Clemens (2021) in which non-wage characteristics of jobs worsen.

Our most credible results – for the unscheduled minimum wage increases in Hawaii and Nebraska – indicate possibly substantial decreases in low-wage job search activity caused by minimum wage increases. Notably, our results suggest an average elasticity of low-wage job search activity with regard to minimum wage changes that is more negative than -0.3, and likely more negative than -0.5. Such estimates are at least two and perhaps greater than four times as large as the common range of estimates for the employment elasticity (-0.1 to -0.3

(Brown et al., 1982) and -0.148 (Neumark and Shirley, 2022)). Our results for other states with less clean minimum wage "experiments" either point to negative effects or no effects of minimum wages on job search. We find little indication of positive effects, aside from a handful of small positive effects for some states, and no statistically significant evidence of positive effects.

Our results are limited by the time period for which data were available and by the fact that some job search for low-wage jobs occurs in-person or through social networks, rather than via online job platforms. However, there is little reason to believe that job search activities would sharply diminish on online platforms while remaining constant or increasing via alternate channels. As a result, our work represents a challenge to the search-and-matching models predicting that higher minimum wages increase job search, as well as a challenge to appeals to these models to attempt to explain non-negative estimates in some studies of the effects of minimum wages on employment. Additional research and data will be useful to obtain more definitive evidence on the effects of minimum wages on job search.

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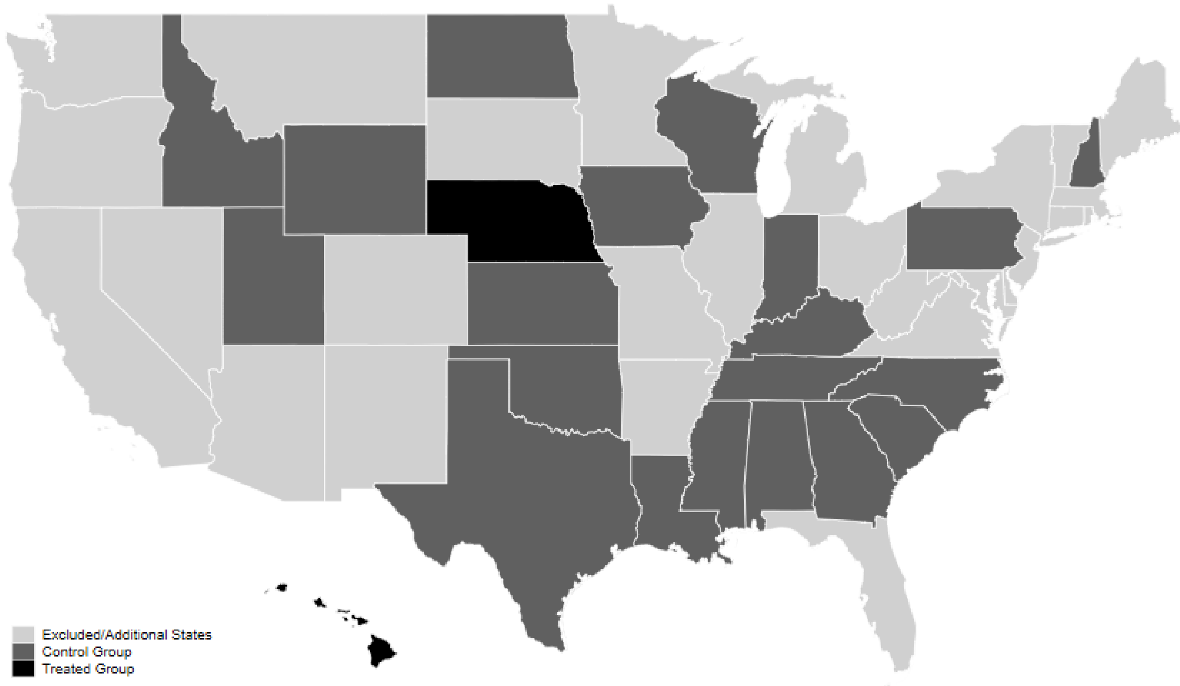
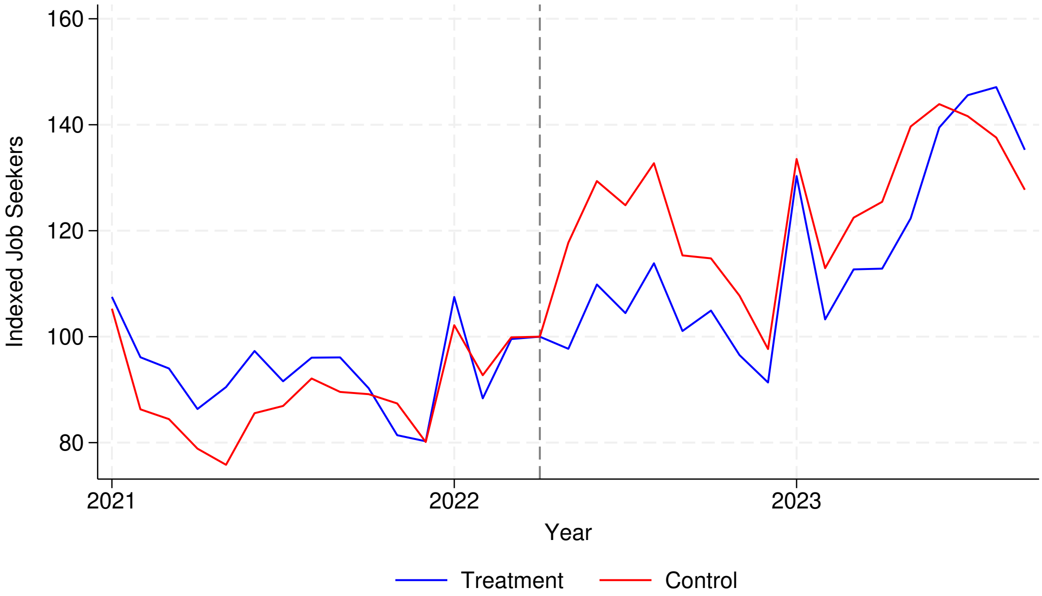
Figure 1: Treated and Control States



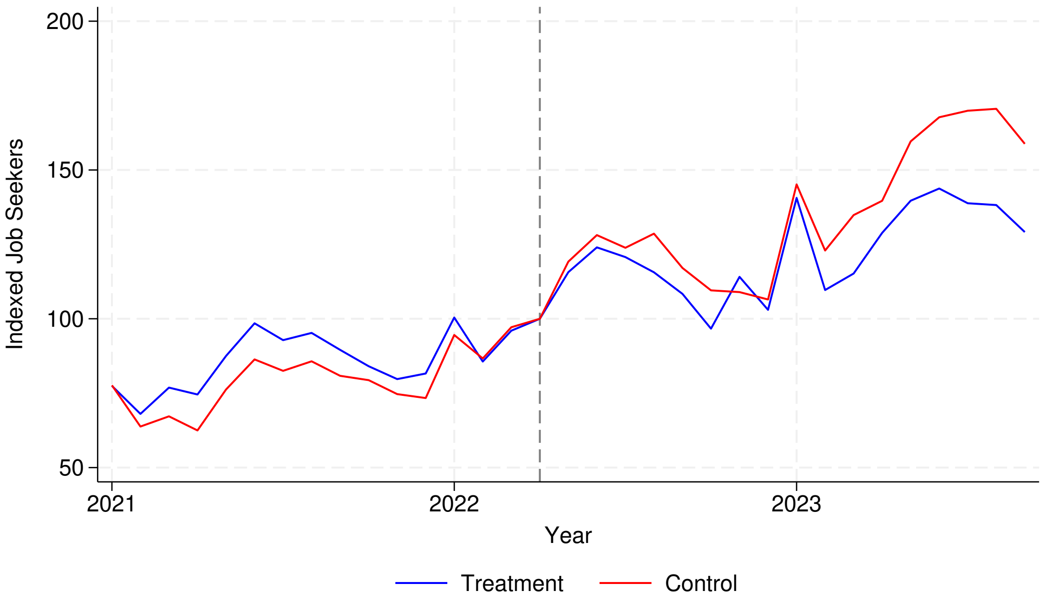
Notes: The map above shows the treated states for our state-specific analysis (Hawaii and Nebraska). It also shows the control states where the federal minimum wage prevailed throughout the period, and other states with minimum wage increases that we exclude from our state-specific analysis (including Alaska – not shown) or include in some of our additional analyses.

Figure 2: Indexed Job-Seeker Trends: Hawaii vs. Control States

(a) Retail/Cleaning occupations



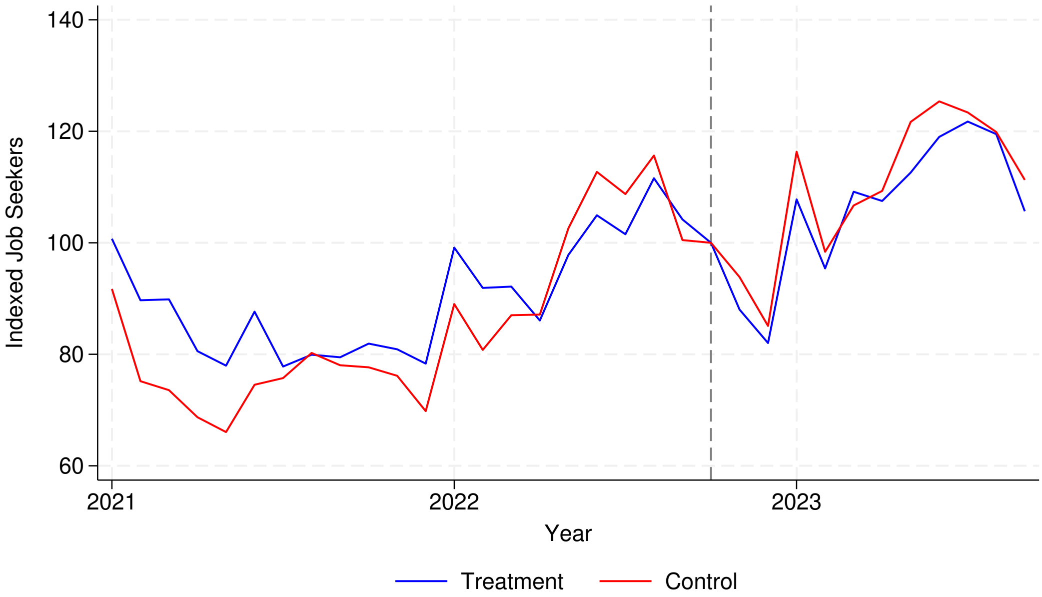
(b) Food-related occupations



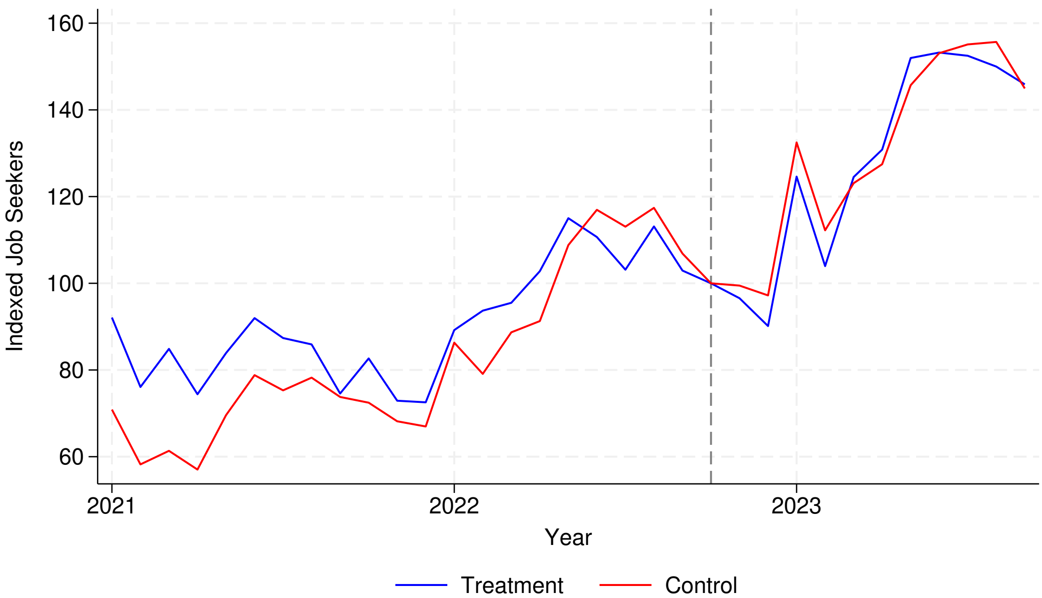
Notes: Each panel plots average monthly job seekers for Hawaii (treatment) and the control states (control), indexed so that each group equals 100 in the last pre-treatment month (vertical dashed line). Treatment timing is based on the May 2022 minimum wage announcement. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Figure 3: Indexed Job-Seeker Trends: Nebraska vs. Control States

(a) Retail/Cleaning occupations



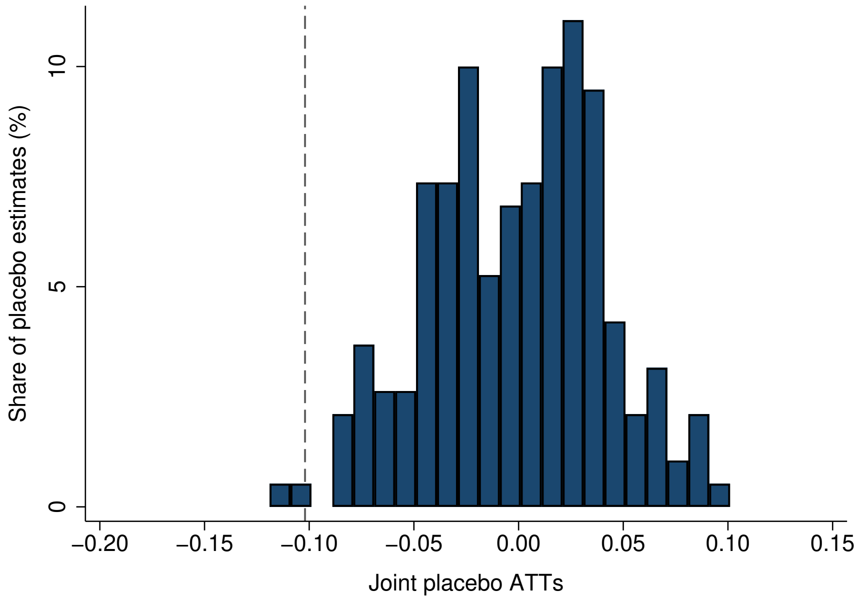
(b) Food-related occupations



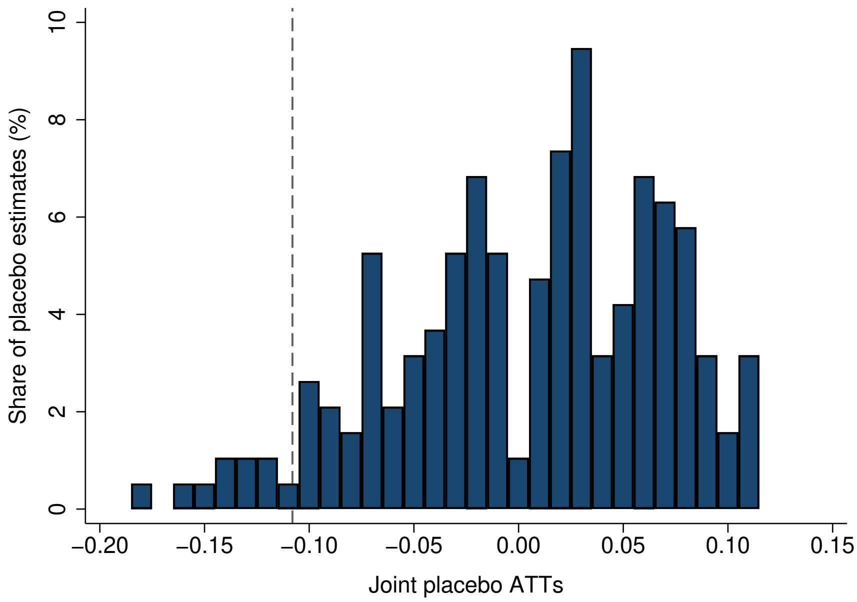
Notes: Each panel plots average monthly job seekers for Nebraska (treatment) and the control states (control), indexed so that each group equals 100 in the last pre-treatment month (vertical dashed line). Treatment timing is based on the November 2022 minimum wage announcement. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Figure 4: Joint Empirical Randomization Inference Distributions

(a) Retail/Cleaning Occupations



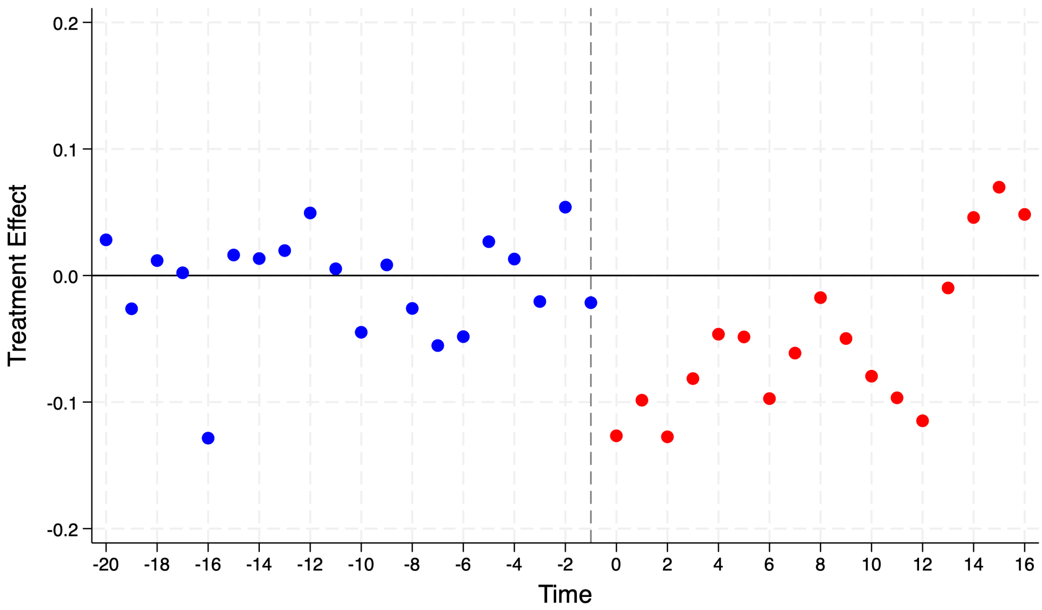
(b) Food-related Occupations



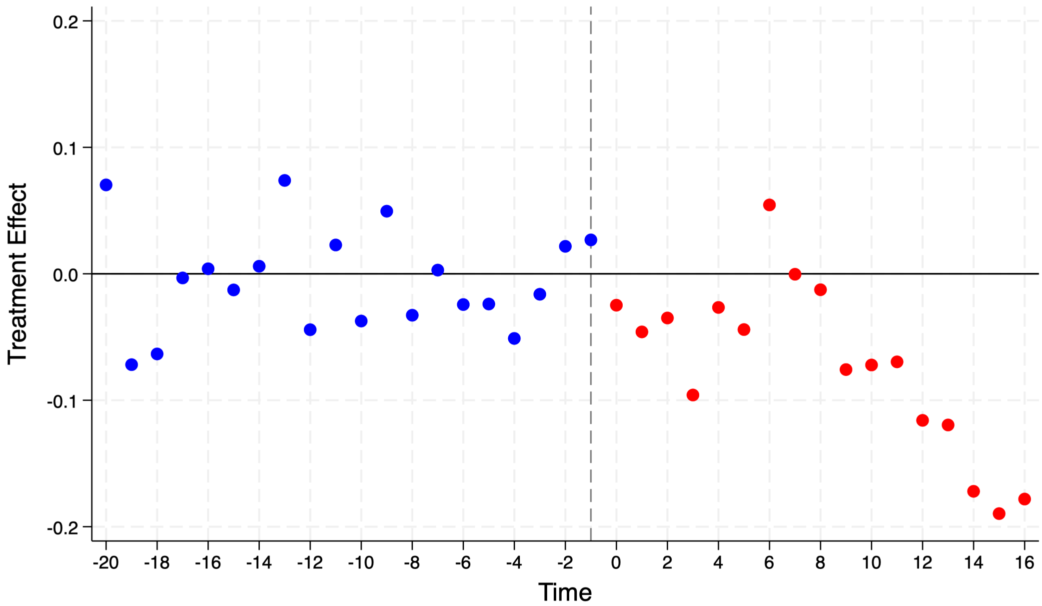
Notes: Each panel plots the joint empirical randomization distribution of 190 placebo ATT estimates, corresponding to all unordered pairs of the 20 control states. In each placebo iteration, one selected state is assigned Hawaii's May 2022 treatment timing and the other is assigned Nebraska's November 2022 treatment timing, and the TWFE specification is re-estimated. The vertical line indicates the observed joint TWFE ATT reported in Panel A of Table 4. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Figure 5: Hawaii and Nebraska Event Study: Callaway and Sant'Anna Method

(a) Retail/Cleaning Occupations



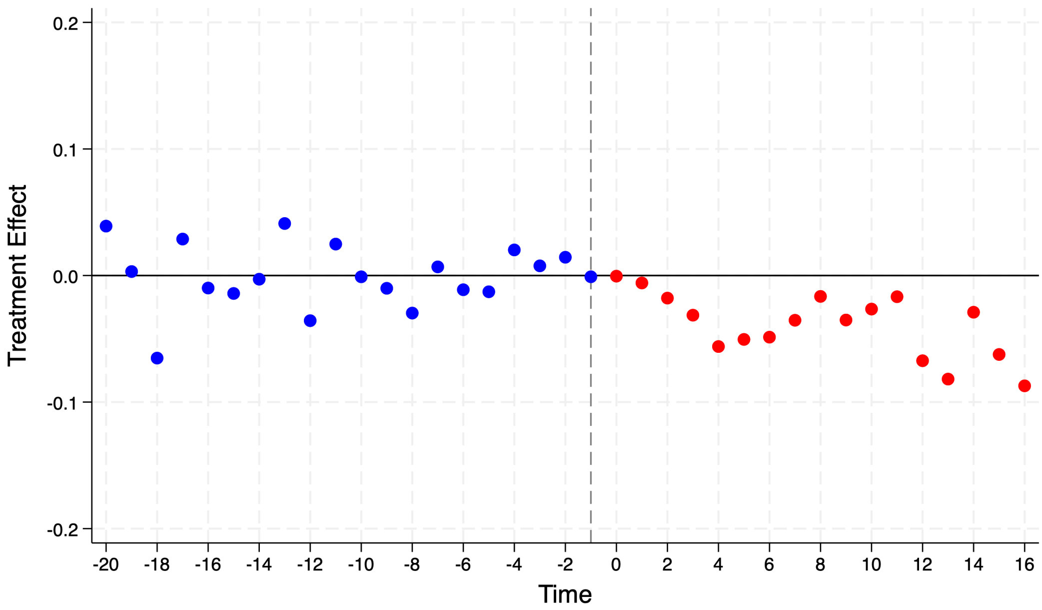
(b) Food-related Occupations



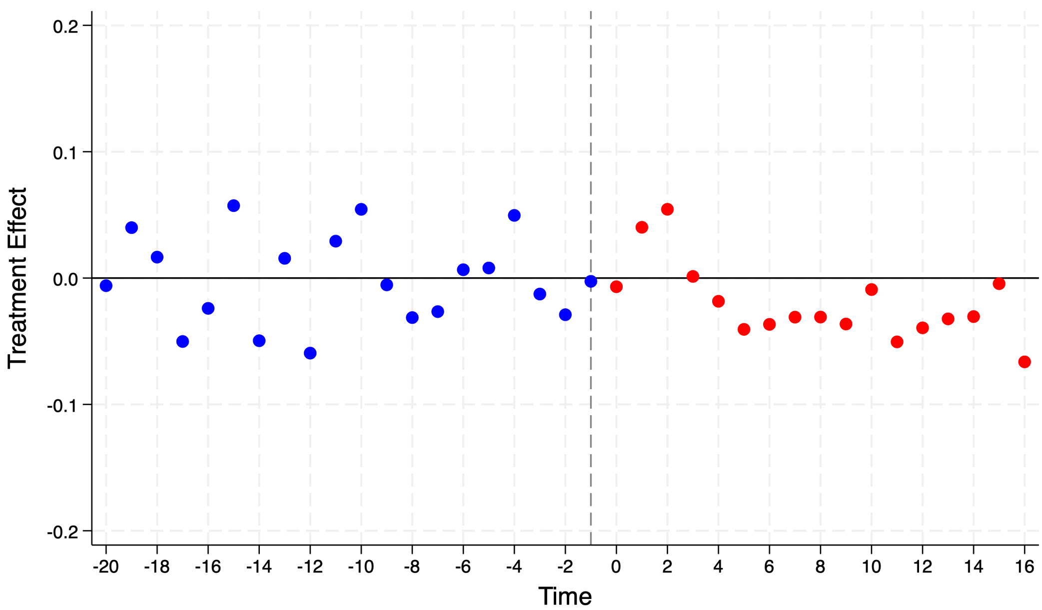
Notes: This figure shows the event-study plots based on the Callaway and Sant'Anna method shown in Panel B of Table 4. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Figure 6: Additional Treated States Event Study: Callaway and Sant'Anna Method

(a) Retail/Cleaning Occupations



(b) Food-related Occupations



Notes: This figure shows the event-study plots based on the Callaway and Sant'Anna method shown in Panel A of Table 5 and Panel A of Table 6. Treatment timing is based on the implementation date of the minimum wage per Section 4.3 and Table 1. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Table 1: Minimum Wage Increases in Hawaii, Nebraska, and Additional Treated States

State	Treatment Month	MW Change	Percent	Type of MW Change
Delaware	January 2022	\$9.25 - \$10.50	13.5%	Unscheduled - prior increase
Florida	October 2021	\$8.65 - \$10.00	15.6%	Unscheduled - prior increase
Hawaii (*Announcement*)	May 2022	\$10.10 - \$12.00	18.8%	Unscheduled - clean
Hawaii (*Implementation*)	October 2022	—	—	—
Maine	January 2022	\$12.15 - \$12.75	4.9%	Scheduled - inflation indexed
Montana	January 2022	\$8.75 - \$9.20	5.1%	Scheduled - inflation indexed
Nebraska (*Announcement*)	November 2022	\$9.00 - \$10.50	16.7%	Unscheduled - clean
Nebraska (*Implementation*)	January 2023	—	—	—
Ohio	January 2022	\$8.80 - \$9.30	5.7%	Scheduled - inflation indexed
Rhode Island	January 2022	\$11.50 - \$12.25	6.5%	Unscheduled - prior increase
South Dakota	January 2022	\$9.45 - \$9.95	5.3%	Scheduled - inflation indexed
Virginia	May 2021	\$7.25 - \$9.50	31.0%	Implemented

Notes: Table reports the timing and magnitude of the first large minimum wage increase observed between January 2021 and September 2023 for each additional treated state. Treatment timing is defined by the implementation date of the increase. Additional treated states are included only if the increase is preceded by a period with either no change or only very small increases (approximately 1–2 percent) and any larger prior increase occurred more than one year earlier. "Unscheduled – clean" denotes a discrete policy change not mechanically linked to indexation and not preceded by any minimum wage increase. "Unscheduled – prior increase" denotes a discrete policy change that follows small minimum wage increases in preceding years. "Scheduled – inflation indexed" denotes increases generated by state legislated inflation indexation. Virginia is classified as "Implemented" because the actual May 2021 increase occurs after the data begins, while the announcement date coincides with the beginning of the data.

Table 2: Baseline Analysis of All States,
Effects of Minimum Wage Increases on Log(Job Seekers)

	Retail/Cleaning Occupations	Food-related Occupations
Log (Minimum Wage)	-.144	-.276
	(.075)	(.173)
Observations	1,646	1,646
Number of States	50	50

Notes: Each column reports TWFE (OLS) estimates from the baseline specification in equation (1), relating the log number of unique job seekers accessing job postings in the indicated occupation group to the log nominal state minimum wage. All specifications include state and month-year fixed effects and use all 50 states. Standard errors are clustered at the state level and reported in parentheses. Sample consists of monthly state-level observations from January 2021 through September 2023.

Table 3: Descriptive Statistics on Log(Job Seekers): Levels and Changes

	Pre-treatment	Post-treatment	Difference	Difference-in-Differences
Panel A – Hawaii				
Retail/Cleaning Occupations				
Treatment Group	10.461	10.671	0.209	**-0.092**
Control Group	11.626	11.928	0.302	
Food-related Occupations				
Treatment Group	10.115	10.445	0.330	**-0.130**
Control Group	10.781	11.241	0.461	
Panel B – Nebraska				
Retail/Cleaning Occupations				
Treatment Group	10.759	10.936	0.177	**-0.084**
Control Group	11.693	11.954	0.260	
Food-related Occupations				
Treatment Group	10.054	10.423	0.369	**-0.084**
Control Group	10.867	11.320	0.453	

Notes: Each panel reports pre- and post-treatment mean log job seekers in the indicated treated state and control states. Treatment begins with the May 2022 minimum wage announcement in Hawaii and the November 2022 announcement in Nebraska. The reported simple difference-in-differences equals the treated state's pre–post change minus the corresponding change among control states. Outcomes measure unique job seekers accessing job postings in each occupation group. Sample consists of monthly state-level observations from January 2021 through September 2023.

Table 4: Effects of Minimum Wage Increase on Log(Job Seekers), Hawaii and Nebraska - Staggered Treatment DiD

	Retail/Cleaning Occupations	Food-related Occupations
Panel A — TWFE		
ATT	-.102	-.108
Randomization Inference		
Percentile	1	5
Mean Placebo ATT	-.001	.005
Placebo SD	(.042)	(.064)
Asymptotic 90% CI	[-.070, .067]	[-.100, .110]
Empirical 90% CI	[-.073, .068]	[-.114, .094]
Panel B — Callaway and Sant'Anna		
ATT	-.062	-.057
Randomization Inference		
Percentile	1	12
Mean Placebo ATT	.004	.006
Placebo SD	(.028)	(.049)
Asymptotic 90% CI	[-.042, .051]	[-.074, .085]
Empirical 90% CI	[-.044, .053]	[-.088, .072]
Observations	726	726
Number of States	22	22

Notes: Each panel reports ATT estimates from the indicated specification with state and time fixed effects. Randomization inference (RI) uses a joint design that accounts for Hawaii and Nebraska adopting at different times. From the 20 control states we form all unordered pairs and, in each iteration, assign one state Hawaii's timing and the other Nebraska's timing and re-estimate the model. This yields 190 joint placebo ATT estimates that define the empirical randomization distribution. The Percentile reports the position of the observed ATT in this distribution (ordered from most negative to most positive). The Mean Placebo ATT is the average placebo estimate and the Placebo SD is their standard deviation. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Table 5: Additional Treated States: Aggregate and State-Specific Estimates (Retail/Cleaning Occupations)

Panel A – Aggregate Estimate Across All Ten Treatment Events

Estimator	**ATT**	**CV3 SE**	***t*-statistic**	***p*-value**	**95% CI**	**Observations**
C&S Aggregate ATT	-0.044	0.026	-1.727	0.095	[-0.096, 0.008]	990

Panel B – State-Specific Estimates

State	**MW Change**	**ATT**	**RI Rank**	**Mean Placebo ATT**	**Asymptotic 90% CI**	**Empirical 90% CI**
Delaware	13.5%	-.048	6	.002	[-.100, .104]	[-.077, .078]
Florida	15.6%	.028	14	-.001	[-.101, .099]	[-.074, .079]
Hawaii	18.8%	-.041	4	.002	[-.088, .092]	[-.095, .095]
Maine	4.9%	-.178	1	.009	[-.093, .111]	[-.070, .085]
Montana	5.1%	-.001	1	.000	[-.102, .102]	[-.070, .085]
Nebraska	16.7%	-.077	2	.004	[-.075, .083]	[-.064, .082]
Ohio	5.7%	.023	11	-.001	[-.103, .101]	[-.080, .075]
Rhode Island	6.5%	-.075	2	.004	[-.098, .106]	[-.075, .098]
South Dakota	5.3%	.024	11	-.001	[-.103, .101]	[-.080, .093]
Virginia	31.0%	-.045	6	.002	[-.121, .125]	[-.133, .108]

Notes: Treatment timing in both panels is based on the implementation date of each minimum wage increase, as described in Section 4.3 and Table 1. Panel A reports the simple aggregate Callaway–Sant'Anna ATT from specification (2) across all ten treated states and twenty never-treated control states. The reported standard error, t-statistic, p-value, and 95% confidence interval are based on state-level CV3 cluster-jackknife inference. Panel B reports state-specific ATT estimates from specification (2), which includes state and time fixed effects. Randomization inference (RI) reassigns the treatment timing to each of the 20 control states and re-estimates the model to form the randomization distribution. The RI Rank reports the position of the observed ATT in this ordered distribution, from most negative to most positive. The Mean Placebo ATT is the average of the placebo estimates. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Table 6: Additional Treated States: Aggregate and State-Specific Estimates (Food-Related Occupations)

Panel A – Aggregate Estimate Across All Ten Treatment Events

Estimator	**ATT**	**CV3 SE**	***t*-statistic**	***p*-value**	**95% CI**	**Observations**
C&S Aggregate ATT	-0.025	0.031	-0.820	0.419	[-0.087, 0.037]	990

Panel B – State-Specific Estimates

State	**MW Change**	**ATT**	**RI Rank**	**Mean Placebo ATT**	**Asymptotic 90% CI**	**Empirical 90% CI**
Delaware	13.5%	-.107	4	.005	[-.149, .159]	[-.145, .122]
Florida	15.6%	.009	9	.000	[-.172, .172]	[-.171, .133]
Hawaii	18.8%	-.127	3	.006	[-.137, .149]	[-.167, .099]
Maine	4.9%	-.184	1	.009	[-.145, .163]	[-.141, .123]
Montana	5.1%	-.025	8	.001	[-.153, .155]	[-.149, .115]
Nebraska	16.7%	-.072	6	.004	[-.129, .137]	[-.127, .051]
Ohio	5.7%	.061	15	-.003	[-.157, .151]	[-.153, .111]
Rhode Island	6.5%	-.080	6	.004	[-.150, .158]	[-.146, .118]
South Dakota	5.3%	.031	11	-.002	[-.156, .152]	[-.152, .113]
Virginia	31.0%	.054	13	-.003	[-.211, .205]	[-.221, .134]

Notes: Treatment timing in both panels is based on the implementation date of each minimum wage increase, as described in Section 4.3 and Table 1. Panel A reports the simple aggregate Callaway–Sant'Anna ATT from specification (2) across all ten treated states and twenty never-treated control states. The reported standard error, t-statistic, p-value, and 95% confidence interval are based on state-level CV3 cluster-jackknife inference. Panel B reports state-specific ATT estimates from specification (2), which includes state and time fixed effects. Randomization inference (RI) reassigns the treatment timing to each of the 20 control states and re-estimates the model to form the randomization distribution. The RI Rank reports the position of the observed ATT in this ordered distribution, from most negative to most positive. The Mean Placebo ATT is the average of the placebo estimates. The Asymptotic 90% CI is centered at the placebo mean and uses a normal approximation with critical value 1.64, while the Empirical 90% CI spans the central 90% of the empirical randomization distribution. RI confidence intervals reflect randomization uncertainty rather than sampling variation. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Table 7: Effects of Minimum Wage Increase on Log(Job Seekers) in Hawaii and Nebraska for Individual Occupations - TWFE

	Food-related Occupations				Retail/Cleaning Occupations				
	Crew Member	Cook	Dishwasher	Host	Cashier	Retail Salespersons	Janitor	Cleaner	Receptionist
ATT	-.20	-.04	.03	-.06	-.08	-.13	-.01	-.14	-.11
Mean # of Seekers	32,543	13,864	23,842	34,455	41,572	82,128	21,482	19,494	37,281

Notes: Each column reports the joint Hawaii–Nebraska ATT from a separate TWFE regression for the indicated job title based on specification (2). Specifications include state and month-year fixed effects and compare Hawaii and Nebraska with the 20 control states, using the May 2022 and November 2022 minimum wage announcements as the respective treatment dates. ATT estimates are reported in log points. Mean number of job seekers is the average monthly number of unique job seekers accessing postings for each job title; because outcomes are measured separately by title, a job seeker is counted only once within a given title. Sample consists of monthly state-level observations from January 2021 through September 2023.

Appendix A: State-Specific Analyses

We also report results from TWFE) estimates for Hawaii and Nebraska separately. Separate analyses of the treatments in Hawaii and Nebraska are potentially useful because the magnitude and duration of the minimum wage change (i.e., scheduled later increases) differs between the states. Because we only consider one treated state at a time in our state-specific analysis, we do not face any issues associated with heterogeneity in treatment timing described by the recent difference-in-differences econometrics literature.

Paralleling our staggered treatment analysis in the main text, for this analysis of single treated states we use randomization inference. Specifically, we re-assign the timing of minimum wage increases to control states, re-estimate our baseline specifications, and compare the distribution of these estimates (the "empirical randomization distribution") to the treatment effects we obtain. With only 20 placebo estimates, no estimate can be outside of the empirical (two-sided) 95% confidence interval, so 90% confidence intervals are more useful.

The results in Table A.1 suggest that the increase in minimum wages in Hawaii caused a decline of around 11% and 13% in the number of job seekers in retail and cleaning occupations and food-related occupations across the period from May 2022-September 2023. The computed elasticity of the change in the number of job seekers relative to the minimum wage change is -0.61 for retail and cleaning occupations and -0.72 for food-related occupations.[41]

Table A.1 also reports details on the statistical inference. For example, for retail and cleaning occupations, the rank of the estimate (from most to least negative) for the treatment state and the controls is 2, meaning that only one control state had a more negative estimate (New Hampshire, with an estimate of -0.117), while 19 states had estimates that were smaller negative, or positive (ranging from -0.076 to 0.101). These ranks correspond to exact p-values, so with 21 estimates, a rank of 2 would imply statistical significance at the 10% level but not the 5% level. Finally, the last three rows report the mean placebo ATT and the two confidence intervals. In this case, we see that the -0.110 estimate for Hawaii retail and cleaning occupations is significant at the 10% level in both cases (with the empirical confidence interval corresponding

[41] These values are calculated as arc elasticities. We convert the difference-in-difference log-point estimates to percentage changes and divide by the arc percent change in Hawaii's minimum wage.

to the conclusion just noted based on the rank of the estimate).

Table A.2 shows the results for Nebraska. The results suggest that the number of job seekers for retail and cleaning occupations declined by more than 9.0% over the 11 months (November 2022-September 2023) following the signal of the minimum wage increase. Job seekers for food-related occupations decreased by 8.7%. The computed elasticity of the change in the number of job seekers relative to the minimum wage change is -0.58 for retail and cleaning occupations and -0.54 for food-related occupations.[42] Table A.2 also reports details on the statistical inference. For retail and cleaning occupations, the rank of the estimate for Nebraska is 2, implying statistical significance at the 10% level but not the 5% level. For food-related occupations, the rank is 4, meaning the ATT for these occupations is still relative large (in absolute value) relative to the placebo estimates, but the statistical evidence is weaker (reflected also in the confidence intervals reported in the table).

Note that the estimated elasticities for retail and cleaning occupations are quite close in Hawaii and Nebraska, differing by only -0.02. In contrast, the elasticity for food-related occupations is a good deal larger in Hawaii (-0.69 vs. -0.51). It is conceivable that this is because Hawaii does not have a lower minimum wage for tipped workers, whereas Nebraska does and the tipped minimum wage did not change when the regular minimum wage went up. Thus, the Nebraska minimum wage may have resulted in somewhat more substitution from the limited-service to the full-service sector, and less of a decline in overall food-related employment.[43]

Notably, the state-specific (Hawaii and Nebraska) state-specific analyses find larger elasticities than the 50-state baseline analysis. This may be because large, discrete, unscheduled minimum wage changes produce larger effects than scheduled, discrete changes and marginal, index-driven changes. This could suggest nonlinear effects, since unscheduled minimum wage increases tend to be substantially larger than increases related to cost-of-living index changes. Alternatively, the difference may suggest that anticipatory effects mitigate the measured effect, since scheduled and index-driven changes are known in advance by both workers and employers.

[42] These values are, again, calculated as arc elasticities.

[43] This would be consistent with the evidence on tipped minimum wages in Neumark and Yen (2023).

Table A.1: Effects of Minimum Wage Increase on Log(Job Seekers) in Hawaii

	Retail/Cleaning Occupations	Food-related Occupations
Baseline		
ATT	-.110	-.131
Randomization Inference		
Rank	2	3
Mean Placebo ATT	.005	.007
Placebo SD	(.062)	(.104)
Asymptotic 90% CI	[-.097, .107]	[-.164, .178]
Empirical 90% CI	[-.076, .084]	[-.187, .119]
Observations	693	693
Number of States	21	21

Notes: Baseline estimates report the ATT from specification (2), which includes state and time fixed effects. Randomization inference (RI) constructs an empirical randomization distribution by re-assigning the treatment timing to each of our 20 control states and re-estimating specification (2). The Mean Placebo ATT is the average of these placebo estimates and the Placebo SD is their standard deviation. Rank denotes the position of the actual ATT in the ordered randomization distribution (from most negative to most positive). The Asymptotic 90% CI is centered at the mean of the placebo estimates and is constructed using a normal approximation with critical value 1.64 and the standard deviation of the randomization distribution. The Empirical 90% CI spans the central 90% of the empirical randomization distribution, from the second-lowest to the second-highest placebo ATT. With 20 placebo estimates, trimming one observation from each tail yields a 90% interval. Both RI confidence intervals reflect uncertainty under the randomization distribution rather than sampling variation. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Table A.2: Effects of Minimum Wage Increase on Log(Job Seekers) in Nebraska

	Retail/Cleaning Occupations	Food-related Occupations
Baseline		
ATT	-.093	-.087
Randomization Inference		
Rank	2	4
Mean Placebo ATT	.005	.004
Placebo SD	(.054)	(.085)
Asymptotic 90% CI	[-.084, .094]	[-.135, .143]
Empirical 90% CI	[-.087, .093]	[-.156, .101]
Observations	693	693
Number of States	21	21

Notes: Baseline estimates report the ATT from specification (2), which includes state and time fixed effects. Randomization inference (RI) constructs an empirical randomization distribution by re-assigning the treatment timing to each of our 20 control states and re-estimating specification (2). The Mean Placebo ATT is the average of these placebo estimates and the Placebo SD is their standard deviation. Rank denotes the position of the actual ATT in the ordered randomization distribution (from most negative to most positive). The Asymptotic 90% CI is centered at the mean of the placebo estimates and is constructed using a normal approximation with critical value 1.64 and the standard deviation of the randomization distribution. The Empirical 90% CI spans the central 90% of the empirical randomization distribution, from the second-lowest to the second-highest placebo ATT. With 20 placebo estimates, trimming one observation from each tail yields a 90% interval. Both RI confidence intervals reflect uncertainty under the randomization distribution rather than sampling variation. Sample consists of monthly state-level observations on unique job seekers accessing job postings from January 2021 through September 2023.

Appendix B: Additional Tables

Table B.1: Descriptive Statistics: Hawaii, Nebraska, and Control States

Variable	Mean	SD	Median
***Panel A.* Hawaii**			
Log (Job Seekers) - Retail/Cleaning Occupations	10.56	0.16	10.53
Log (Job Seekers) - Food-related Occupations	10.275	0.21	10.25
Median Age (years)	44.37	.99	44
Female Share (%)	49.99	.68	50
White Share (%)	24.62	1.92	24.88
Urban Share (%)	23.65	2.72	23.48
Mean Number of Children per Household	.35	.03	.35
N		33	
***Panel B.* Nebraska**			
Log (Job Seekers) - Retail/Cleaning Occupations	10.81	0.14	10.82
Log (Job Seekers) - Food-related Occupations	10.17	0.23	10.12
Median Age (years)	38.23	1.91	38
Female Share (%)	50.06	1.14	49.81
White Share (%)	89.67	1.58	89.71
Urban Share (%)	21.46	1.93	21.4
Mean Number of Children per Household	.31	.04	.31
N		33	
***Panel C.* Control States**			
Log (Job Seekers) - Retail/Cleaning Occupations	11.77	1.01	11.87
Log (Job Seekers) - Food-related Occupations	11	0.99	11
Median Age (years)	40.84	3.58	41
Female Share (%)	51.18	1.29	51.22
White Share (%)	82.18	10.74	85.46
Urban Share (%)	16.49	8.80	16.8
Mean Number of Children per Household	.30	.05	.29
N		660	

Notes: This table reports the mean, standard deviation, and median for our state-specific outcome variable, Log(Job Seekers), during the period of analysis, January 2021 through September 2023. Log(Job Seekers) refers to the logarithm of the number of unique monthly platform users actively searching for job postings in the relevant occupation groups. We also report state-level demographic characteristics drawn from the Current Population Survey (CPS), including median age, the share of females, the share of whites, the share of households residing in metropolitan areas, and the mean number of children per household. Treated states are broadly similar to the control states along these dimensions, with the notable exception of racial composition: while the control states are on average 82% white, the corresponding share in Hawaii is approximately 25%. We re-estimated our specifications with these demographic controls included; the addition of demographic controls had a negligible impact on the estimates.

Table B.2: Occupations with Lowest Mean Hourly Wages

Occupation Code Name	Occupation title	Mean Hourly Wage
772	Shampooers	14.07
687	Cooks, Fast Food	14.31
699	Fast Food and Counter Workers	14.48
753	Amusement and Recreation Attendants	14.54
751	Ushers, Lobby Attendants, and Ticket Takers	14.67
798	Cashiers	14.77
709	Hosts and Hostesses, Restaurant, Lounge, and Coffee Shop	14.78
752	Miscellaneous Entertainment Attendants and Related Workers	14.89
676	Lifeguards, Ski Patrol, and Other Recreational Protective Service Workers	15.07
708	Dishwashers	15.22
1219	Laundry and Dry-Cleaning Workers	15.33
704	Other Food Preparation and Serving Related Workers	15.37
746	Gambling and Sports Book Writers and Runners	15.4
783	Childcare Workers	15.42
743	Entertainment Attendants and Related Workers	15.5
1220	Pressers, Textile, Garment, and Related Materials	15.55
878	Hotel, Motel, and Resort Desk Clerks	15.66
1363	Parking Attendants	15.72
705	Dining Room and Cafeteria Attendants and Bartender Helpers	15.74
694	Food Preparation Workers	15.85
695	Food and Beverage Serving Workers	15.89
612	Home Health and Personal Care Aides	16.05
742	Animal Caretakers	16.12
702	Food Servers, Nonrestaurant	16.27
1228	Sewers, Hand	16.28
691	Cooks, Short Order	16.31
678	School Bus Monitors	16.33
685	Cooks and Food Preparation Workers	16.35
738	Animal Care and Service Workers	16.49
686	Cooks	16.52
1328	Ambulance Drivers and Attendants, Except Emergency Medical Technicians	16.55
680	Food Preparation and Serving Related Occupations	16.58
796	Retail Sales Workers	16.59
1366	Automotive and Watercraft Service Attendants	16.6
755	Locker Room, Coatroom, and Dressing Room Attendants	16.63
721	Maids and Housekeeping Cleaners	16.66

Table B.2: Bottom Occupations by Mean Hourly Wage (*Continued*)

Occupation Code Name	Occupation title	Mean Hourly Wage
756	Entertainment Attendants and Related Workers, All Other	16.67
624	Physical Therapist Aides	16.74
952	Graders and Sorters, Agricultural Products	16.77
1222	Sewing Machine Operators	16.83
1337	Taxi Drivers	16.88
611	Home Health and Personal Care Aides	
761	Funeral Attendants	16.92
1217	Textile, Apparel, and Furnishings Workers	16.93
1388	Cleaners of Vehicles and Equipment	16.95
711	Miscellaneous Food Preparation and Serving Related Workers	17.03
712	Food Preparation and Serving Related Workers, All Other	17.03
1391	Packers and Packagers, Hand	17.05
457	Floral Designers	17.07
1167	Bakers	17.09
747	Gambling Service Workers, All Other	17.14
719	Building Cleaning Workers	17.23
688	Cooks, Institution and Cafeteria	17.27
882	Library Assistants, Clerical	17.29
799	Gambling Change Persons and Booth Cashiers	17.32
690	Cooks, Restaurant	17.34
718	Building Cleaning and Pest Control Workers	17.36
776	Baggage Porters and Bellhops	17.36
1365	Transportation Service Attendants	17.37
956	Farmworkers and Laborers, Crop, Nursery, and Greenhouse	17.37
720	Janitors and Cleaners, Except Maids and Housekeeping Cleaners	17.43
786	Recreation Workers	17.44
771	Manicurists and Pedicurists	17.54
701	Waiters and Waitresses	17.56
1226	Shoe Machine Operators and Tenders	17.58
892	Receptionists and Information Clerks	17.59
830	Telemarketers	17.64
804	Retail Salespersons	17.64
853	Gambling Cage Workers	17.67
1372	Passenger Attendants	17.68
1170	Meat, Poultry, and Fish Cutters and Trimmers	17.71
1224	Shoe and Leather Workers	17.72
961	Forest and Conservation Workers	17.72
1336	Shuttle Drivers and Chauffeurs	17.75

Table B.2: Bottom Occupations by Mean Hourly Wage (*Continued*)

Occupation Code Name	**Occupation title**	**Mean Hourly Wage**
954	Miscellaneous Agricultural Workers	17.75
1225	Shoe and Leather Workers and Repairers	17.80
1232	Textile Cutting Machine Setters, Operators, and Tenders	17.81
957	Farmworkers, Farm, Ranch, and Aquacultural Animals	17.82
696	Bartenders	17.83
1234	Textile Winding, Twisting, and Drawing Out Machine Setters, Operators, and Tenders	17.84
790	Personal Care and Service Workers, All Other	17.88
789	Miscellaneous Personal Care and Service Workers	17.88
1109	Tire Repairers and Changers	17.92
634	Veterinary Assistants and Laboratory Animal Caretakers	17.94
1230	Textile Machine Setters, Operators, and Tenders	17.96
947	Agricultural Workers	17.97

Notes: This table shows all occupations with mean hourly wage below $18 in the United States, based on May, 2023, National Occupational Employment and Wage Estimates (U.S. Bureau of Labor Statistics, 2023a).

Table B.3: Comparison of BLS Categories and Occupations Used in this Study

BLS occupational category	**Mean hourly wage**	**Our searched job titles**
Cooks and Food Preparation Workers	\$16.35	cook, crew member
Food and Beverage Serving Workers	\$15.89	crew member
Other Food Preparation and Serving Related Workers	\$15.37	host, dishwasher
Retail Sales Workers	\$16.59	retail salespersons
Cashiers	\$14.78	cashiers
Janitors and Cleaners, Except Maids and Housekeeping Cleaners	\$17.43	janitors, cleaners
Receptionists and Information Clerks	\$17.59	receptionist

Notes: This table shows the mean hourly wage for each of the occupations associated with the job titles chosen for our study, based on May, 2023, National Occupational Employment and Wage Estimates (U.S. Bureau of Labor Statistics, 2023a).

Table B.4: Minimum Wage for U.S. States

State Name	MW 2018	MW 2019	MW 2020	MW 2021	MW 2022	MW 2023	Group
Alabama	7.25	7.25	7.25	7.25	7.25	7.25	Control
Alaska	9.84	9.89	10.19	10.34	10.34	10.85	Excluded
Arizona	10.50	11	12	12.15	12.80	13.85	Excluded
Arkansas	8.50	9.25	10	11	11	11	Excluded
California	11	12	13	14	15	15.50	Excluded
Colorado	10.20	11.10	12	12.32	12.56	13.65	Excluded
Connecticut	10.10	10.10	11	12	13	14	Excluded
Delaware	8.25	8.75	9.25	9.25	10.50	11.75	Additional
Florida	8.25	8.46	8.56	8.65	10	11	Additional
Georgia	7.25	7.25	7.25	7.25	7.25	7.25	Control
Hawaii	10.10	10.10	10.10	10.10	10.10	12	Treatment
Idaho	7.25	7.25	7.25	7.25	7.25	7.25	Control
Illinois	8.25	8.25	9.25	11	12	13	Excluded
Indiana	7.25	7.25	7.25	7.25	7.25	7.25	Control
Iowa	7.25	7.25	7.25	7.25	7.25	7.25	Control
Kansas	7.25	7.25	7.25	7.25	7.25	7.25	Control
Kentucky	7.25	7.25	7.25	7.25	7.25	7.25	Control
Louisiana	7.25	7.25	7.25	7.25	7.25	7.25	Control
Maine	10	11	12	12.15	12.75	13.80	Additional
Maryland	9.25	10.10	11	11.75	12.50	13.25	Excluded
Massachusetts	11	12	12.75	13.50	14.25	15	Excluded
Michigan	9.25	9.45	9.65	9.65	9.87	10.10	Excluded
Minnesota	9.65	9.86	10	10.08	10.33	10.59	Excluded
Mississippi	7.25	7.25	7.25	7.25	7.25	7.25	Control
Missouri	7.85	8.60	9.45	10.30	11.15	12	Excluded
Montana	8.30	8.50	8.65	8.75	9.20	9.95	Additional
Nebraska	9	9	9	9	9	10.50	Treatment
Nevada	8.25	8.25	8.25	9	9.75	10.50	Excluded
New Hampshire	7.25	7.25	7.25	7.25	7.25	7.25	Control
New Jersey	8.60	8.85	11	12	13	14.13	Excluded
New Mexico	7.50	7.50	9	10.50	11.50	12	Excluded
New York	10.40	11.10	11.80	12.50	13.20	14.20	Excluded
North Carolina	7.25	7.25	7.25	7.25	7.25	7.25	Control
North Dakota	7.25	7.25	7.25	7.25	7.25	7.25	Control
Ohio	8.30	8.55	8.70	8.80	9.30	10.10	Additional
Oklahoma	7.25	7.25	7.25	7.25	7.25	7.25	Control
Oregon	10.25	10.75	11.25	12	12.75	13.50	Excluded
Pennsylvania	7.25	7.25	7.25	7.25	7.25	7.25	Control
Rhode Island	10.10	10.50	10.50	11.50	12.25	13	Additional
South Carolina	7.25	7.25	7.25	7.25	7.25	7.25	Control
South Dakota	8.85	9.10	9.30	9.45	9.95	10.80	Additional
Tennessee	7.25	7.25	7.25	7.25	7.25	7.25	Control
Texas	7.25	7.25	7.25	7.25	7.25	7.25	Control
Utah	7.25	7.25	7.25	7.25	7.25	7.25	Control
Vermont	10.50	10.78	10.96	11.75	12.55	13.18	Excluded

Table B.4: Minimum Wage for USA states (*Continued*)

State Name	**MW 2018**	**MW 2019**	**MW 2020**	**MW 2021**	**MW 2022**	**MW 2023**	**Group**
Virginia	7.25	7.25	7.25	7.25	11	12	Additional
Washington	11.50	12	13.50	13.96	14.49	15.74	Excluded
West Virginia*	8.75	8.75	8.75	8.75	8.75	8.75	Excluded
Wisconsin	7.25	7.25	7.25	7.25	7.25	7.25	Control
Wyoming	7.25	7.25	7.25	7.25	7.25	7.25	Control

Notes: Minimum wages listed above are as of January 1st for the given year. All states with stable minimum wages from 2018 to 2023 are included in the control group with the exception of West Virginia, which changed its minimum wage soon before 2018. Results shown in this paper are very similar if we include West Virginia in the control group. Nebraska and Hawaii had unscheduled changes in their minimum wages in 2022 or 2023 and thus are included in the treatment group. Delaware, Florida, Maine, Montana, Ohio, Rhode Island, and South Dakota are included in the "additional" treatment group because each has a discrete, relatively large minimum wage increase in the sample period that is preceded by no change or only very small increases (approximately 1–2 percent), with any larger prior increase occurring more than one year earlier. Virginia's minimum wage increase, by contrast, was unscheduled and announced immediately prior to the start of the analysis period. Although the lack of pre-announcement data prevents Virginia from being included in our state-specific analysis, the implementation of Virginia's minimum wage increase occurs within the sample window and is observed with pre-implementation data. For the additional treated states, treatment timing is defined by the implementation date of the minimum wage increase. We therefore also include Virginia in the additional treatment group. All other states are excluded from the main analysis.