The Downward Nominal Wage Rigidty Gap For Lower-Income Workers

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Abstract

Recent labor market trends reveal that lower-skilled jobs have exhibited weaker growth during economic expansions and more severe job losses during recessions. This pattern raises important questions about the evolution of downward nominal wage rigidity (DNWR), particularly for lower-income workers. Understanding whether and why DNWR has increased for vulnerable sectors of the labor market is crucial for addressing the broader challenges facing working-class Americans. This paper develops a theoretical and empirical framework to explore these questions, investigating how key factors such as weakening labor bargaining power and increasing labor market substitutability affect DNWR. Specifically, utilizing 41 years of CPS data from 1983[1] to 2023, we examine the role of right-to-work laws, which erode union influence and thus labor bargaining power, and NAFTA, which increases competition and labor substitutability. Our findings indicate that lower-income workers have experienced a relative increase in DNWR with respect to higher-income workers overall, yet neither right-to-work laws or NAFTA – which should both impact vulnerable members of the labor force more – are associated with increasing DNWR. In fact, we find that right-to-work laws decrease DNWR, and there is not a clear effect of NAFTA on DNWR. Together, these results suggest that there may be more factors at play, underscoring the need for more research in this realm.

*Acknowledgements: I'm grateful for the guidance, feedback, and insights from Professor Douglas O. Staiger and my fellow classmates in ECON80.

[1]This was initially starting from 1979, but there were enough missing variables that we use for controls in each of these years where we might be safer just dropping the entire year.

Introduction

Economic expansions have long been expected to deliver broad-based benefits across the labor market, particularly in the form of increased employment opportunities. However, in recent decades, the gains from economic expansions have been unevenly distributed, with lower-skilled workers often seeing both smaller wage growth and fewer improvements in employment prospects compared to their higher-skilled counterparts. For example, Bernanke (2003)[1] and Jaimovich and Siu (2012)[2] highlight an increasingly prevalent trend over the past forty years: nearly all contractions in aggregate employment during recessions are driven by job losses in routine occupations, with these same disappearing jobs more frequently experiencing "jobless recoveries" – periods of economic expansion that fail to generate meaningful gains in job supply. Moreover, according to a 2023 report from the Bureau of Labor Statistics,[3] employment in occupations typically involving a bachelor's degree will increase by 6.4% over the next decade, while those only requiring a high school diploma are expected to only grow by 2.9%. This trend suggests that low-skilled occupations are experiencing weaker job growth during a period of economic expansion, highlighting a worsening disparity between high-skilled and low-skilled workers.

This growing disparity in job growth has far-reaching consequences, not only for the individuals directly affected but also for the broader economy. Lower-skilled workers, who often rely on routine jobs as a pathway to economic stability, face increasing difficulty maintaining their livelihoods in the face of diminishing opportunities. This erosion of stable employment options exacerbates income inequality, reduces upward mobility, and heightens economic insecurity for a significant portion of the workforce. Moreover, weakened job prospects for lower-skilled workers can lead to broader societal issues, including declining labor force participation, greater reliance on public assistance programs, and growing political and social tensions rooted in economic dissatisfaction. While many economists have pointed to a plethora of different explanations in an attempt to explain current labor market inequities[2], one crucial issue that has yet to be fully explored in this context is the role of downward nominal wage rigidity (DNWR).

DNWR is a key factor in explaining how these labor market challenges can be exacerbated, particularly for lower-skilled workers. As a cornerstone of both classical Keynesian and New Keynesian economic theory, DNWR highlights the labor market's strong resistance to nominal wage reductions, even in the face of high unemployment (Keynes, 1936).[6] This resistance stems from both institutional and behavioral factors, including the violation of social norms and the negative impact on employee morale that typically accompanies wage cuts. As a result, firms often prefer layoffs to wage reductions when seeking to cut costs, which can contribute to higher unemployment. Empirical evidence supports this, as wage changes tend to cluster around zero nominal

[2]Well-known examples include Krugman (2008)[4] and Autor et al. (2008)[5]

wage growth, and negative wage adjustments are exceedingly rare (Akerlof, 1986).[7] By examining DNWR in the context of today's labor market disparities, this study aims to shed light on how these rigidities can help explain the mounting employment challenges faced by lower-skilled workers.

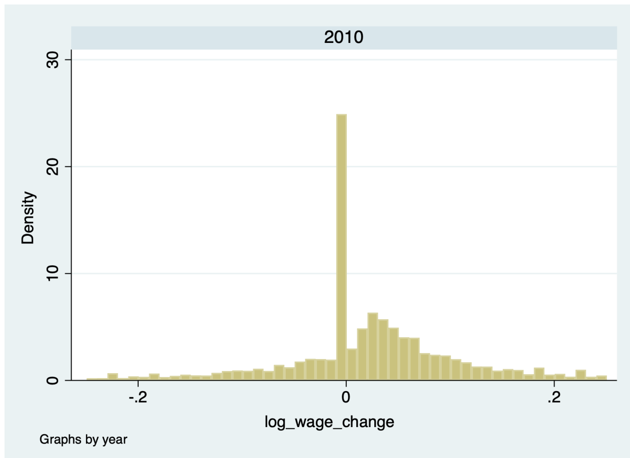

Figure 1: Nominal Wage Distribution in 2010

As shown in the nominal wage change histogram in Figure 1, the spike at zero nominal wage change stands out significantly, dominating the rest of the distribution. This spike represents individuals who, in an efficient market, would have experienced a nominal wage cut but did not due to DNWR. When DNWR is prevalent, employers may be unable to reduce wages and are instead forced to lay off workers to cut labor costs, which leads to increased unemployment. Therefore, understanding how to reduce the spike at zero nominal wage change is crucial for addressing inefficiencies caused by behavioral factors. This makes the issue of DNWR highly relevant to macroeconomists, labor economists, and behavioral economists alike.

Given the growing disparity in employment opportunities between lower- and higher-income workers, and the potential role of DNWR in explaining these disparities, this paper addresses two key questions:

1. Has DNWR increased for lower-wage workers relative to higher-wage workers?

2. If so, what labor market factors or changes have driven this shift?

To explore the second question, we examine two major structural changes since the 1970s: the decline in union power and the rise of labor market globalization. Specifically, we focus on the implementation of right-to-work laws and the North American Free Trade Agreement (NAFTA) to assess whether these changes can help explain the findings from the first question. Right-to-work laws reflect the erosion of union power ane labor bargaining leverage, while NAFTA symbolizes the increasing globalization of labor markets.

Our analysis reveals that, historically, lower-wage workers experienced less DNWR compared to higher-wage workers; however, in recent years, lower-wage workers have seen *more* DNWR than higher-wage workers. We also find that right-to-work laws have contributed to a reduction in DNWR overall, indicating that the decline in union strength and labor bargaining power is not responsible for the recent increase in DNWR. Additionally, our analysis shows that NAFTA and the growing integration of foreign labor markets did not have a clear impact on DNWR, suggesting that labor market globalization alone is not accountable for this trend.

Background

Related Literature

The existing literature on DNWR identifies variations in the extent of DNWR by examining the effects of different economic conditions and policies, often using macroeconomic data or cross-country comparisons. Schmitt-Grohé and Uribe (2016),[8] for example, explore how DNWR varies across developing countries with different exchange rate regimes and capital control policies. They find that DNWR tends to be higher in countries with fixed exchange rate regimes and without capital controls. In these settings, nominal wages are more rigid downwards because the inability to adjust the nominal exchange rate in response to external shocks forces firms to absorb the costs through wage rigidity rather than adjusting wages downward. Schmitt-Grohé and Uribe use macroeconomic data to compare periods before and after the adoption of fixed exchange rate regimes or capital controls, and by examining wage behavior during these periods, they infer when DNWR is more pronounced. Specifically, they identify higher DNWR during periods when countries face economic shocks and cannot adjust their exchange rates, which increases the resistance to downward wage adjustments.

Other studies, such as Fallick et al. (2016),[9] take a different approach by analyzing DNWR during economic crises, such as the Great Financial Crisis. Fallick et al. show that there were high levels of workers who experienced zero nominal wage growth, but the DNWR itself did not increase during the crisis. However, their study is primarily descriptive and focuses on documenting trends rather than exploring the underlying causes of these changes. By comparing aggregate wage data from the U.S. before, during, and after the crisis, they identify a clear spike in DNWR during the economic downturn, but do not delve into the mechanisms—such as institutional factors or policy changes—that may have contributed to this trend. Similarly, Kaur (2014)[10] investigates DNWR in Indian villages, using rainfall shocks as an exogenous factor that affects agricultural income. She finds that nominal wages in these villages remain sticky despite income shocks, but like Fallick et al., she does not investigate the underlying causes of this rigidity, such as labor market policies or structural factors like union strength. These studies reveal a gap in the literature regarding the role of specific labor market policies or structural transformations in shaping the extent of DNWR. Furthermore, many studies rely on aggregated data, leaving unexplored the potential intracountry variations in DNWR, which could provide more nuanced insights into how different labor markets experience wage rigidity, and most studies are more correlational than causal. These studies' empirical designs also exemplify a major issue: they simply infer DNWR when there are more people with zero nominal wage growth, but they do not definitively prove that the higher incidence of zero nominal wage growth is caused by an actual increase in DNWR or poor economic conditions that would encourage employers to cut wages. Our empirical design aims to address this particular issue.

The most important contribution to this study comes from Card & Hyslop (1997),[11] which establishes the inverse relationship between inflation and the magnitude of the zero nominal wage change spike. By constructing annual wage change distributions and calculating the total proportion of the distribution that lies under the spike, Card and Hyslop provide evidence that inflation can alleviate wage rigidity by eroding real wages, enabling firms to adjust labor costs without resort to explicit nominal wage reductions. This mechanism helps stabilize employment levels by reducing the need for layoffs, while preserving employee morale and minimizing turnover risks. Essentially, inflation shifts the entire nominal wage change distribution to the right, decreasing the proportion of wages stuck at zero nominal growth and mitigating the rigidity-induced spike at this point. As a result, inflation can ease the unemployment pressures associated with wage rigidity, facilitating more effective labor market adjustments during economic expansions. It is through this dynamic in which economic expansions take place: an increase in aggregate demand leads to higher inflation, which leads to decreased real wages, which provides firm employment flexibility. A similar dynamic is expected to occur with increases in labor productivity, as higher productivity allows firms to adjust wages more flexibly without triggering nominal constraints. This finding that inflation makes the zero nominal wage change spike smaller will help inform our empirical specification when trying to measure the level of DNWR. We will expand on the theory that justifies our empirical specification in the methods section.

As we can see below in Figure 2, we compare the wage change distribution for 1980[3] against the distribution for 2010. The year 1980 was marked with a historically high 13.5% annual inflation within the United States, while the year 2010 held a mere 1.5% annual inflation due to the impacts of the Great Financial Crisis. The

[3]Our analysis does not include 1980. We simply use it here because it really emphasizes the difference of the zero nominal wage change spike. Our analysis originally had data for 1980, but there were too many missing variables.

zero nominal wage change spike in 1980 was low enough where the histogram appears to be bimodal; meanwhile, the zero nominal wage change spike in 2010 towers over the rest of the distribution. Together, these two histograms demonstrate the impact inflation has on the zero nominal wage change spike. We will take advantage of the effect inflation has on the proportion of people who experience zero nominal wage growth in order to measure changes in DNWR.

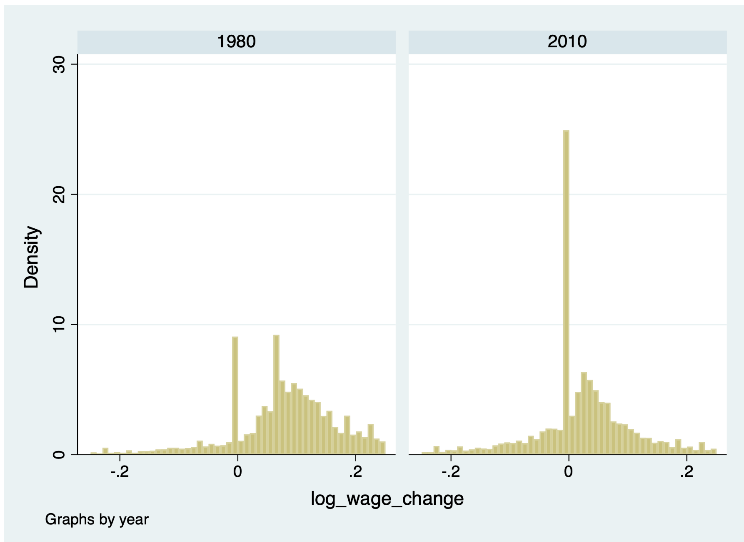

Figure 2: High Inflation vs. Low Inflation Nominal Wage Change Distributions

Background on Right-to-Work and NAFTA

Because this study aims to relate the impact of right-to-work laws and NAFTA on DNWR, it is essential to first provide background information on these policies and explore why they are particularly relevant to this study. We begin by examining right-to-work laws.

Right-to-Work Laws

Right-to-work laws are state-level statutes in the United States that prohibit union security agreements between employers and labor unions. These laws ensure that individuals are not required to join a union or pay union dues as a condition of employment. The key rationale behind right-to-work laws is to protect workers' freedom to choose whether or not to unionize, thereby promoting individual liberty and potentially attracting businesses to states with lower labor costs.

From an economic perspective, right-to-work laws reduce the bargaining power of unions and laborers. By reducing union membership, these laws may weaken unions' ability to negotiate higher wages and better working conditions. Consequently, right-to-work laws could influence wage dynamics, particularly by limiting the upward pressure on wages that unions often exert. However, there is also the potential for these laws to affect DNWR. One could argue that in a labor market where unions are weaker, employers may have more freedom and less pressure to adjust wages downward during economic downturns, thus increasing DNWR. But on the other hand, there is evidence suggesting that unions may prioritize job security over constant wages during economic downturns.[4] In such cases, unions might negotiate slight nominal wage cuts for all members to prevent layoffs, ensuring the firm remains viable while retaining its workforce. This approach was notably practiced in the automobile industry during the Great Financial Crisis, where unions such as the United Auto Workers agreed to temporary wage reductions and concessions on benefits to help companies like General Motors and Chrysler avoid bankruptcy and preserve jobs. These agreements demonstrate that while unions often resist wage cuts, they may also act as a stabilizing force during economic downturns by spreading financial sacrifices across the workforce to prevent layoffs, serving as a signaling agent to employers for the acceptability of nominal wage cuts.

In sum, right-to-work laws are a symbol for the broader trend of the reduction in union and labor bargaining power, and looking at right-to-work laws is a natural choice for analyzing this trend particularly as it relates to changes in DNWR. For the years we have data for, there were 7 different states that adopted right-to-work laws, and we leverage these state adoptions to measure the change in DNWR before and after such adoptions. The specific states with right-to-work laws, and the specific states which adopted right-to-work laws at some point contained within the span of our dataset are depicted below in Figure 3.

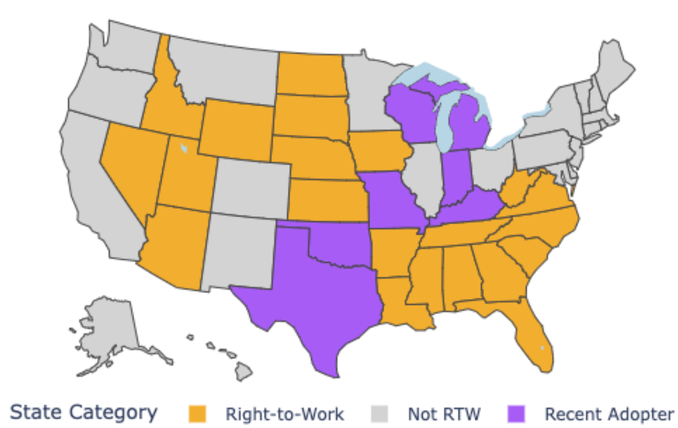

Figure 3: Right-to-Work States

[4]Example papers about this include Visser (2016)[12] which talks about union concessions during the GFC.

NAFTA

NAFTA, signed in 1994 by the United States, Canada, and Mexico, created a trilateral free trade block, the largest in the world at the time. The agreement slashed tariffs and expanded trade across borders, leading to increased cross-border trade in goods, services, and investments. NAFTA has had a mixed impact on the labor market, especially in the United States.[5] NAFTA has led to the relocation of lower-skilled jobs to Mexico, where labor costs were lower, contributing to job losses in these certain sectors in the United States. On the other hand, NAFTA has also created new opportunities in sectors such as technology and services. Therefore, NAFTA – and expanding international trade – has been linked to a growing inequity between the wage structures of skilled and unskilled labor, making it a suitable choice of a policy shock in order to analyze how the increasing substitutability of labor affects DNWR.

We focus on NAFTA specifically, rather than other trade agreements, because it represents the United States' first major step toward free and open trade, and at the time it created the largest free trade zone in the world. Its impact on eroding the manufacturing, agricultural, and raw materials industries of the United States is well documented, and we look to analyze whether it made an impact on DNWR for these particular jobs. What is super important in our decision to look at NAFTA as opposed to other trade shocks is that there were relatively few confounding trade shocks before, during, or even up to a decade after its implementation. There were only three free trade agreements signed by the United States between 1979 and the implementation of NAFTA in 1994: the US-Canada Free Trade Agreement in 1988, the Carribean Basin Initiative in 1983, and the US-Israel Free Trade Agreement in 1985. Neither of these three trade agreements made much of an impact on the US labor market because Canadian labor is not cheaper than American labor, and the Carribean countries and Israel are too small to make an impact. Looking past 1994, the next big trade shock happened in 2006, with the passage of the Dominican Republic-Central American Free Trade Agreement (CAFTA-DR). Because of this, we limit this particular analysis to only go up to the year 2005. Even after accounting for the major free trade deals, one may also be concerned about the rise in exports coming from Asian countries, particularly China, but we should note that the trade balance between the United States and Asia was relatively balanced until around 2005.[6]

As it pertains to its impact on DNWR, one could argue that NAFTA may influence DNWR in opposing ways. By providing firms with access to foreign labor markets, NAFTA gives employers additional options for cutting wages during a recession. For example, it may be more socially and culturally acceptable to reduce the nominal wages of workers in developing countries because there is less DNWR in other countries. In such cases, firms might preserve the nominal wages of domestic workers

[5] Many studies to choose from, but an example is Benguria (2023)[13] which focuses on inequities.

[6] This can be visualized on macrotrends.net, which has a section on the US-China trade balance.

while cutting those of foreign employees, thus reducing overall costs and maintaining morale without causing domestic unemployment. Conversely, NAFTA could decrease DNWR domestically by reducing concerns over turnover. Employers might become less sensitive to domestic worker dissatisfaction, knowing they can outsource jobs to foreign laborers willing to work for lower wages. Additionally, firms could leverage the lower foreign wage as justification for reducing domestic wages, further undermining wage rigidity. Given these competing mechanisms, our analysis seeks to shed light on how these dynamics actually manifest in practice.

Data

Our data construction process follows the procedure established in Card & Hyslop (1997).[11] We first collect the "merged outgoing rotation group files" from the 1983 to 2023 CPS, which is publicly available from the NBER[7]. Each month, the CPS gathers hourly and weekly earnings data from employed workers in one-quarter of the sample frame. Half of this group (approximately one-eighth of all wage and salary workers in the sample) will be reinterviewed twelve months later and asked the same earnings questions. The other half, interviewed twelve months prior, provide comparable earnings data from that earlier time. By matching individuals across consecutive CPS samples, we can construct a series of "rolling panels" that include two years of wage information.

Even though there are more than 350,000 observations associated with a single year of the data, much needs to be filtered and cleaned. Following the filtering procedure established by Card & Hyslop (1997),[11] we first drop anyone whom we cannot create a proper link to across two consecutive years of data. This can be caused by people leaving the survey or moving, but much of this is also driven by poorly coded identifiers and general inconsistencies with demographic data registration, which is particularly worse for the earlier years of CPS data. Furthermore, because we do not want to measure the wage change of someone who switches jobs, we drop anyone who is no longer in either the same recorded industry or occupation. Altogether, these consistency-driven changes bring us down to around 50,000 observations per year. This decline was mostly driven by an inability to establish a proper link across two consecutive years of data, but there are also a large number of people who we can link who seems to have changed jobs.

We also are not interested in people who are not paid by the hour, which typically accounts for an additional drop of 10,000 observations per year, and we are also not interested in people with missing or clearly incorrect hourly earnings data (for example, hourly earnings that are 9999), which removes an additional 30,000 observations per year. Finally, we removed anyone who has wage change that we deem as being

[7]The datasets can be downloaded here: https://data.nber.org/morg/annual/

too large to be either accurate or to be associated with the same job; in particular, we decide to drop anyone whose log wage changes is greater than 0.25 in either direction, which corresponds to around a 30% change in wages. On average this contributes to only around an additional 250 decrease in observations. Lastly, we note that many recent years has imputed wage data, which are not accurate, so we drop any observations that were imputed; this drops around 6,000 observations for the most recent years of data, starting at around 2020. Altogether, after these changes, we have on average 7500 observations per year. In total, we end up with n=298,043 observations across all 41 years from 1983 to 2023.

From the CPS data, there are a few variables that we use throughout our analysis. First, we take the log of the change in hourly earnings across two years for the same individual. If the absolute value of the log of this wage change is within 0.01 (we will go more in depth regarding this in the empirical design section), we can consider it a nominal wage freeze. Additionally, we are interested in the dataset's variables for education, age, gender, race, marital status, union members, industry, and occupation, which will be used to construct our individual level controls. Our education variable is continuous, ranging from 0 to 18, where each each one "point" increase in education corresponds to an additional year of grade school or attainment of a higher education degree. We do not directly use the age variable in the data, but rather we create an experience variable that is based off of age and education; our exact formula we used to create the experience variable is $experience = age - education - 6$, which should capture how much experience someone has in the workforce. Additionally, we take the gender and race data to create female and white dummies, respectively, where we define white to not include Hispanics. We create a married dummy which combines anyone who is legally married to a living spouse, even if their spouse lives somewhere else. Our union dummy covers people who are in unions, but also people who are covered by union contracts. Lastly, we have industry and occupation dummies which were constructed from the industry and occupation codes. The coding for these changed for many years, so we utilize the codebook to hardcode a new consistent industry and occupation coding across all years of the data. Although there initially are hundreds of specific industries and occupations, we reorganize them into 20 broad industry categories[8] and 23 broad occupation categories[9].

In addition to CPS data, we also leverage public macroeconomic data from the BLS in order to gauge macroeconomic conditions, particularly inflation, productivity growth, and state-level unemployment rates, which will be important for the purposes of our analysis.[10] Note that we de-mean inflation for the purposes of the analysis,

[8]There are technically 21 broad industry categories, but one of them, which is Military, does not have any observations that were paid by the hour. The new categories are listed out in detail in Appendix Table 1

[9]These are listed out in Appendix Table 2

[10]Technically, these data series were obtained from FRED, which got them from the BLS. Here is a link: https://fred.stlouisfed.org/release?rid=112.

and thus all recordings of inflation are with respect to their mean. In general, Table 1 below shows the general means of our key variables, grouped by pre- and post-2000 observations. As we can see, the general trends in how the means of the key variables shifted from the pre-2000 data to the post-2000 data make intuitive sense.

Table 1 - Descriptive Summary Statistics for Key Variables

This table shows the means for the key variables used within the analysis, including both the wage and demographic information provided from the CPS and the macroeconomic information provided from the BLS. We partition our analysis, looking at the means from before 2000 and after 2000 separately, which can allow us to see some key trends and shifts that have occurred over time. Standard deviations are given in parentheses.

	Pre-2000	Post-2000
Nominal Wage Freeze	0.210	0.235
	(0.408)	(0.424)
Inflation	0.008	-0.923
	(1.107)	(1.458)
Productivity Growth	1.990	1.881
	(1.295)	(1.418)
Unemployment	6.029	5.652
	(1.979)	(2.159)
Education	12.708	13.600
	(2.251)	(2.204)
Experience	22.299	24.112
	(13.412)	(13.950)
Female	0.544	0.557
	(0.498)	(0.497)
White	0.814	0.737
	(0.389)	(0.440)
Married	0.618	0.558
	(0.486)	(0.497)
Union	0.267	0.171
	(0.443)	(0.377)
Observations	111,187	163,607

Methods

The challenge of our empirical design is that we need to figure out a way to measure changes in DNWR, which is not directly observable at the individual level. In order to address this issue, we first establish a theoretical framework that can motivate the empirical design we ultimately use to capture changes in DNWR. As such, this section is split into two parts: the first part establishes the theory which motivates our empirical design, the second part dives into the specifics of the actual empirical design.

Theoretical Framework

A profit-maximizing firm aims to set nominal wages such that the marginal product of labor equals the ratio of nominal wages to the price level, as stated by (1). By taking the logarithm of this relationship and then differencing, we find that the percent change in the marginal product of labor equals the difference between the percent change in nominal wages and the inflation rate. Rearranging this equation yields the result (2) that the percent change in nominal wages should equal the sum of the inflation rate and the percent change in labor productivity.

$$MP_L = \frac{w}{p} \tag{1}$$

$$\Delta MP_L = \Delta w - \Delta p$$

$$\Delta w = \Delta p + \Delta MP_L$$

$$\Delta w = \pi + \Delta MP_L \tag{2}$$

This expression, while straightforward, does not align with the empirical trends observed over the past forty years. Between 1979 and 2012, net productivity increased by 72.2%, yet real wages for the median worker rose by only 8.7% during the same period.[11] To construct a more accurate theoretical model, it is crucial to incorporate factors that have exerted downward pressure on wages, particularly those that have intensified in recent decades: declining labor bargaining power and increasing labor substitutability. We denote these factors as *bargain* and *sub*, respectively.

Broadly, these two types of pressures encompass the primary mechanisms influencing wage suppression: either the value of labor is diminished, or workers face reduced wage bargaining power due to structural changes in the economy. Additionally, we introduce an error term ϵ, acknowledging that wages follow a distribution rather than a fixed value. Together, these components define the latent nominal wage change distribution Δw^*, representing the underlying wage adjustments that would occur in the absence of downward nominal wage rigidity. Formally, the expression for the latent nominal wage change is given by:

$$\Delta w^* = \pi + \Delta MP_L + bargain + sub + \epsilon \tag{3}$$

The difference between the actual, *observed* wage change distribution and the *latent* wage change distribution is that the observed wage change distribution assigns

[11] From an Economic Policy Institute report by Bivens & Mischel (2015)[14]

zero nominal wage change to many observations who would have obtained a nominal wage cut if DNWR did not exist. We can then relate the *observed* wage change distribution Δw to the *latent* wage change distribution Δw^* by incorporating DNWR, as such:

$$\Delta w = \Delta w^*, \text{ if } DNWR = 0$$
$$\Delta w = \max(0, \Delta w^*), \text{ if } DNWR = 1$$

where the probability of experiencing DNWR, denoted by $\Pr[DNWR = 1]$, is again directly unobservable. Even though DNWR itself is directly unobservable, what we can, however, observe is the magnitude of the zero nominal wage spike.

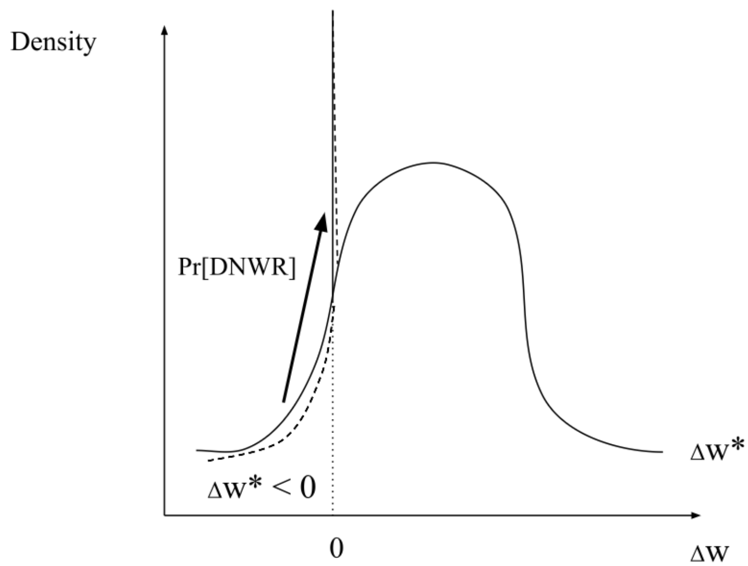

Figure 4

Looking at Figure 4, we can make the critical observation that for someone to be in the nominal wage spike is for that person to, first, have a negative latent nominal wage, and, second, for that person to experience DNWR. To rewrite this observation in terms of probabilities, we have

$$\Pr[\Delta w = 0] = \Pr[\Delta w^* < 0] \ \times \Pr[DNWR = 1] \tag{4}$$

Note that the probability of being on the spike $\Pr[\Delta w = 0]$ is completely observable. Although we do not know what $\Pr[\Delta w^* < 0]$ is exactly, we know how it can shift given (3). Now, suppose for simplicity that $\Pr[\Delta w^* < 0]$ can be expressed as a linear combination of π, ΔMP_L, *bargain*, and *sub*, and the expression for $\Pr[DNWR = 1]$

contains some linear combination of unobservable terms $X\beta$. We can then make substitutions on (4) as follows.

$$\Pr[\Delta w = 0] = (\alpha_1 \pi + \alpha_2 \Delta MP_L + \alpha_3 bargain + \alpha_4 sub) \times X\beta \qquad (5)$$

The multiplicative nature of (5) implies that all terms that make up X will be multiplied with all terms that determine the latent wage change distribution, which essentially behave as interactions. Because, from (3), inflation should push nominal wage growth upwards, it should decrease the probability of the latent nominal wage change to be less than zero; therefore, we know that α_1 must be negative. Then, if some policy were to increase the probability someone experiences DNWR – making the effect of the β coefficient positive – this would seem to amplify the impact inflation has on *preventing* observations of zero nominal wage change (because multiplying a negative and a positive makes a negative). Therefore, if a treatment *increased* DNWR, we would see a *negative* coefficient on the interaction term between the treatment and inflation. If a treatment *decrased* DNWR, we would see a *positive* coefficient on the interaction term between the treatment and inflation.

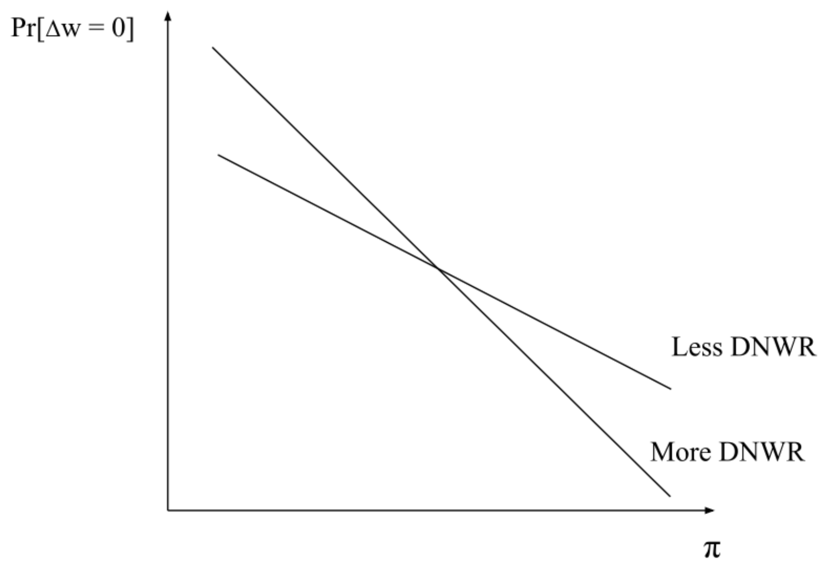

Figure 5: Less Tilt Means Less DNWR

We can think of this more intuitively as so: if there is more DNWR, there will be more people on the zero nominal wage change spike. Therefore, inflation will have a much larger (a more negative) impact because for the same increase in inflation, if there is a larger spike, there will be more potential observations to be rescued from sticky wages. Thus, the impact of inflation on the probability of having zero nominal wage change is even more negative, which represents a negative coefficient on the interaction term between the treatment and inflation. Inflation simply has the opportunity to "do more work" in a world with more DNWR.

With this in mind, the way we can use right-to-work laws and NAFTA to test how *bargain* and *sub* impact DNWR becomes clear. Right-to-work laws should decrease *bargain* since the weakening of labor unions should reduce the amount of bargaining power workers have against their employers, and NAFTA should increase *sub* since the access of new labor markets where firms can exploit cheaper labor increases the substitutability of workers. Thus, in order to know how they affect DNWR, all one has to do is to interact the application of these treatments with inflation, and then check the sign of the coefficient it has on the outcome of $\Pr[\Delta w = 0]$.

Empirical Design

Leveraging our theoretical framework, these findings then suggest the following empirical design:

$$\Pr[|\Delta w_{ist}| \leq k] = F(\beta_0 + \beta_1 T \times \pi_t + \beta_2 T + \beta_3 X_i + \beta_4 UR_{st} + \lambda_s + \delta_t + \epsilon_{ist}) \qquad (6)$$

where Δw_{ist} represents the nominal wage growth for person i in state s in year t, and k represents a threshold value that we use to determine whether someone should be considered to have received a nominal wage freeze. We set our threshold value $k = 0.01$ in order to account for rounding issues, as the surveyor or the respondent may choose to round differently the second time a survey is conducted for a specific individual. Thus, the entire left-hand side simply denotes the probability someone is on the zero nominal wage change spike.

On the right-hand side, the $F(\cdot)$ represents the sigmoid function, indicating that we are using a logistic regression – all regressions in this paper should be assumed to have been done using a logistic regression model unless stated otherwise. T represents whether someone received a treatment (so, in the context of this paper, it represents either being in a state after its implementation of right-work or being in an effected industry after implementation of NAFTA), and π_t represents the national US inflation rate in the year t. X_i represents individual-level controls, which include common demographic information that are used for predicting wages – including education, experience, a quadratic term on experience, sex, race, marriage status, and industry and occupation dummies/fixed effects. UR_{st} represents the state-level unemployment rate in state s in year t, which may be helpful as it can account for any losses made to the zero nominal wage spike caused by those who would have been there, but the employer deciding to simply cause layoffs instead of keeping wages at zero nominal wage. λ_s and δ_t, respectively, represent state and year fixed effects.

We are interested in β_1 because, as we have established, the way we can see the impacts of a change in DNWR is through how the treatment interacts with inflation.

The coefficient β_2 on the baseline term – or any baseline term – captures how the variable causes more observations at zero nominal wage change, which can be caused by either a leftward shift in the latent wage distribution, or an actual increase in DNWR. The issue is that we cannot separate these two effects from the baseline coefficient itself, so β_2 represents the combined effect of a variable on negative nominal wage change pressure and its relationship with DNWR. Therefore, to reiterate, a *positive* β_1 represents that the treatment *decreased* DNWR, and vice versa.

In particular, the first major outcome we will investigate is to address whether lower income workers are now experiencing more DNWR relative to higher income workers. We first run the regression without the interaction term twice, once on the sample of observations before the year 2000 and the sample of observations after the year 2000. The change in the slope coefficient on inflation represents whether there has been, in general, more DNWR or not. We then add an interaction between inflation and a dummy for whether a person is in the bottom quartile of wage earners, using their first observation of wages in the CPS. We partition the dataset into pre- and post-2000, and run the regression on both partitions. If the coefficient on the interaction term becomes more negative or less positive, this demonstrates that lower-income workers have experienced a greater increase in DNWR relative to higher-income workers.

Next, we will utilize the regression specification we have developed in order to investigate whether right-to-work laws have increased or decreased DNWR. In addition to test the effect of this on workers in general, we partition the dataset to see whether union workers felt a stronger impact than non-union workers. If this is the case, then it adds evidence towards the relationship between right-to-work laws and DNWR because right-to-work laws should primarily lower the bargaining power of unionized workers rather than non-union workers. Furthermore, as one final check, we do an event study analysis on right-to-work laws and the probability of receiving a nominal wage decrease and a nominal wage freeze. Although a nominal wage decrease can simply be attributed to a treatment shifting leftwards the latent wage distribution, if coupled with no increase in nominal wage freezes, it can serve as a good check in order to corroborate other findings in this section.

We will then utilize the regression specification (6) in order to examine whether the implementation of NAFTA has increased or decreased DNWR on the workers that it most impacts – particularly, workers who work in manufacturing, agriculture, and raw materials. We choose these specific industries because they are the industries that produce goods at a large scale that are easily and commonly tradable across borders. We limit the analysis to include only years up to 2005 in order to avoid the confounding effects of other major trade deals and the rise in Chinese exports. Because the types of people who end up working in the treatment industries are very different from people who do not, we utilize propensity score matching in order to alleviate the concerns and rerun the regression, weighting the control group to look more similar to people who would take manufacturing, agriculture, or raw material jobs. In the same spirit as before, as one final check, we do an event study analysis on NAFTA and the probability of receiving a nominal wage decrease and a nominal wage freeze.

Results

This section presents the findings from our analysis of right-to-work laws and NAFTA on DNWR. But first, we begin by establishing the empirical fact that for the same level of inflation, there is now a greater probability that someone will be found at the zero nominal rigidity spike than there were before. We accomplish this by running the baseline regression without any interaction on observations before the year 2000 and on observations during and after the year 2000. If the magnitude of the coefficient on inflation decreases (less negative), the same amount of inflation will have less of an impact on stimulating the economy than before because there is less DNWR. This is, in fact, what we do see in Table 2, and the same observation can be seen with productivity growth, which should in theory under perfect conditions have the same impact as inflation on mitigating the effects of DNWR.

From the second column, we see that before the year 2000 a one percentage point increase in inflation yields a 2.37% decrease ($p=0.015$) in the logodds of having zero nominal wage change. From the rightmost column, we see that after the year 2000 a one percentage point increase in inflation yields a 2.64% decrease ($p<0.01$) in the logodds of having zero nominal wage change. Both slope coefficients are statistically significant, so from this we can infer that DNWR in general has not changed much to the average worker in general as the coefficients are relatively close to each other. The coefficient on inflation in the first column is notably insignificant, but if anything this underscores the importance of controlling for unemployment. Furthermore, what is particularly interesting is that the slope coefficients on productivity growth and the slope coefficients on inflation are very similar. From the second column, a 1% increase in labor productivity yields a 2.94% decrease ($p<0.01$) in the logodds of having zero nominal wage change, and from the fourth column, a 1% increase in labor productivity yields a 2.64% decrease ($p<0.01$) in the logodds of having zero nominal wage. All of these coefficients are significant, very close to one another, and the fact that they are all negative perfectly with the theory.

Table 2 - Pre vs. Post 2000 DNWR

This table compares the coefficient of inflation on the probability of getting zero nominal wage change before and after the year 2000. The change in the coefficient of inflation between these two periods represents the change in DNWR between these two periods for the labor force in general. For each period, two similar but separate regressions were run: the first regression does not utilize unemployment as a control, while the second regression does. Furthermore, we decide to show the coefficients on the control variables for this one table. As usual, these regressions are logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Pre-2000		Post-2000	
Inflation	-0.00912	-0.0237**	-0.0427***	-0.0264***
	(0.00996)	(0.00972)	(0.00390)	(0.00348)
Productivity Growth	-0.0164**	-0.0294***	-0.0115***	-0.0298***
	(0.00825)	(0.00832)	(0.00425)	(0.00438)
Unemployment		0.0452***		0.0596***
		(0.00713)		(0.00479)
Education	0.00509	0.00749	0.0227***	0.0230***
	(0.00566)	(0.00560)	(0.00334)	(0.00333)
Experience	0.00983***	0.0105***	0.00472***	0.00469***
	(0.00209)	(0.00207)	(0.00124)	(0.00124)
Experience2	4.73e-05	3.90e-05	0.000101***	0.000102***
	(4.13e-05)	(4.11e-05)	(2.36e-05)	(2.37e-05)
Female	-0.0493**	-0.0506**	0.0353**	0.0362**
	(0.0221)	(0.0222)	(0.0144)	(0.0142)
White	-0.0146	-0.0165	0.0682***	0.0692***
	(0.0177)	(0.0177)	(0.0178)	(0.0179)
Married	0.0202	0.0164	-0.0295***	-0.0326***
	(0.0164)	(0.0163)	(0.0112)	(0.0114)
Union	-0.181***	-0.187***	-0.290***	-0.289***
	(0.0194)	(0.0192)	(0.0249)	(0.0250)
Industry FE	✔	✔	✔	✔
Occupation FE	✔	✔	✔	✔
State FE	✔	✔	✔	✔
Year FE	✔	✔	✔	✔
Observations	111,186	111,186	163,607	163,607

Examining the controls in Table 2, we find that the slope coefficients are reasonable and align with theoretical expectations. Direct effects on the probability of zero nominal wage change can be attributed to either 1) changes in DNWR or 2) shifts in the latent wage change distribution; for instance, a positive effect on the probability of zero nominal wage change could mean an increase in DNWR that brings more negative latent wages change to zero or a decrease in latent nominal wage change. The statistically significant positive coefficient on unemployment indicates that higher unemployment rates are associated with more individuals being stuck at zero nominal wage change, which is consistent with the literature suggesting that employers are more likely to hold wages fixed during economic downturns. The coefficient on education has increased, how higher skilled jobs have less frequent but larger wage increases, and the coefficient on experience has decreased, which may reflect how worker experience has become more important in the modern world. The fact that the coefficients on experience are always significant and positive may also suggest that there may be more DNWR associated with experienced workers as employers may feel more hesistant towards cutting the wages of a workers who have already a lot of working experience. The quadratic term on experience becoming positive and significant implies that this aforementioned effect of experience only becomes stronger as one gains more experience. The coefficient on female is also particularly noteworthy: it used to be significantly negative, but now it is significantly positive – perhaps this might be explained by how the expansion of paid leave benefits may have provided a downward pressure on women wages. The coefficient on white is insignificant but negative before 2000 and becomes significant and positive after 2000, which may be driven by a desire to decrease any racial wage gap. The coefficient on married is insignificantly positive before 2000, but it becomes significantly negative after 2000, which might reflect how the normal age of getting married is now higher today so the married workers now are also people with more experience with larger latent nominal wage growth. Finally, the significant negative coefficient on union suggests that unions may either experience more DNWR or tend to have more rightward-shifting latent wage distributions.

Having established that the average worker experiences around the same DNWR today than in previous decades, we now turn our attention to the differential effects of DNWR on lower-income versus higher-income workers, and how these differences may have changed. Our goal is to better understand how the most vulnerable members of the labor market are impacted.

In Table 3, we again partition the dataset into pre- and post- 2000, but this time we look at the coefficient between the interaction of inflation and a dummy that represents being in the bottom quartile of income. The coefficient on this interaction term[12] represents how much more DNWR does being in the lowest quartile of income earners have relative to those in the other three quartiles of income earners. Looking at the third column, the direct impact of a 1 percentage point increase in inflation leads to a 3% reduction ($p<0.01$) in the logodds of having zero nominal wage change for before the year 2000, and all three columns for pre-2000 suggest that the direct impact of being low-income leads to around a 8% increase ($p<0.01$) in the logodds of having zero nominal wage change. The interaction term tells us that by being low-income, the percent reduction in the logodds caused by a 1 percentage point increase in inflation is smaller by around 2 percentage points ($p=0.212$). Because the coefficient on the interaction term is positive for pre-2000, lower-income workers

[12]One could also add an interaction term using productivity growth instead of inflation and should see similar trends. For the purposes of this paper, we focus on just inflation to narrow the focus.

faced less DNWR than higher-income workers before the year 2000. It should be noted that the interaction term for pre-2000 is not significant, though it is not completely insignificant.

Table 3 - Pre vs. Post 2000 Income-Based Differences in DNWR

This table compares the DNWR “gap” between the bottom 25% of wage earners and the top 75% of wage earners before and after the year 2000. The interaction term alone between inflation and the low-income dummy should be interpreted as how much more or less DNWR there is for a bottom quartile wage-earner with respect to the top three quartiles of wage earners. A single interaction coefficient is meaningful by itself, but by running it over different periods, we can see how labor market policies and trends have reshaped any preexisting dynamics. For each period, two similar but separate regressions were run: the first regression does not utilize unemployment as a control, while the second regression does. As usual, these regressions are logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Pre 2000			Post 2000		
Inflation x Low Income	0.0241	0.0216	0.0225	-0.0313***	-0.0311***	-0.0288***
	(0.0184)	(0.0179)	(0.0180)	(0.00797)	(0.00793)	(0.00785)
Low Income	0.0794***	0.0833***	0.0842***	0.0536**	0.0536**	0.0482**
	(0.0213)	(0.0218)	(0.0217)	(0.0226)	(0.0227)	(0.0221)
Unemployment		0.0822***	0.0455***		0.0158	0.0594***
		(0.0107)	(0.00696)		(0.0107)	(0.00480)
Inflation			-0.0301***			-0.0184***
			(0.0109)			(0.00432)
Productivity Growth			-0.0299***			-0.0301***
			(0.00758)			(0.00441)
Controls	✔	✔	✔	✔	✔	✔
Industry FE	✔	✔	✔	✔	✔	✔
Occupation FE	✔	✔	✔	✔	✔	✔
State FE	✔	✔	✔	✔	✔	✔
Year FE	✔	✔		✔	✔	
Observations	115,066	115,066	115,066	163,607	163,607	163,607

However, things are completely different if we look at post-2000. Looking at the interaction terms (all three are similar), there is around a 3 percentage point further decrease ($p<0.01$) in the percent reduction in the logodds of having zero nominal wage change (caused by a 1 percentage point increase in inflation). The highly significant negative interaction term suggest that lower income workers face more DNWR with respect to higher income workers in the post-2000 era, demonstrating a complete reversal in the trend seen in the pre-2000. In addition to this finding, another interesting finding is this table is that the direct effect of being low-income is significantly positive across both periods, but it has become smaller in the post-2000 period. This suggests that while lower income workers are more likely to see zero nominal wage growth, it has become more balanced over time. The combination of seeing an increase in the relative DNWR but a decrease in the relative probability of seeing a nominal wage freeze suggests that the latent change distribution for lower income workers has shifted leftwards, indicating that lower income workers have had slowed wage growth with respect to that of higher income workers.

Overall, this finding of a newly existing DNWR gap that disfavors lower income workers raises the issue of equity in today's labor market: in addition to comparatively slower wage growth, they are now experiencing comparatively more DNWR than higher-income workers, likely due to certain shifts in the labor market that have made them increasingly more vulnerable. Before 2000, it was the opposite: higher income workers likely faced more DNWR than lower income workers. We dedicate the rest of this analysis towards utilizing the framework we have already established towards evaluating two potential hypotheses which may have driven this change.

Right-to-Work Laws and Decreasing Bargaining Power

One of the major trends in the labor market since the 1970s is the general decline in union power, which has been attributed to lower wage growth. We leverage the implementation of right-to-work laws in order to see how this decline in labor bargaining power has added to DNWR. However, before we proceed with our analysis, we first demonstrate that right-to-work laws actually made an impact on weakening unions.

Table 4 - Impact of Right-To-Work Laws on Union Membership and Contract Coverage

This table demonstrates that right-to-work laws actually made a meaningful impact on union strength, proxied by union membership and union contract coverage, in the states that implemented them. As usual, we use logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Union Membership	Union Contract Coverage
Right-to-Work	-0.314***	-0.290***
	(0.0958)	(0.0886)
Controls	✔	✔
Industry FE	✔	✔
Occupation FE	✔	✔
State FE	✔	✔
Year FE	✔	✔
Observations	274,794	274,794

The results from Table 4 demonstrate that right-to-work laws represent a 31.4% decrease (p<0.01) in the logodds of having union membership, and a 29.0% decrease (p<0.01) in the logodds of having a contract covered by a union. We would expect such an outcome for right-to-work laws, as they should decrease the strength and number of per ple who join unions. Now that we have analyzed the exact effect right-to-work laws had on union membership and contract coverage, we now return to our analysis of right-to-work laws and DNWR.

Because we will be running a difference-in-difference which looks at right-to-work states, we need to ensure that the observed differences are not caused by changes in demographic characteristics in the right-to-work states or target industries when the treatment is applied. Obviously, it is not a concern and entirely expected if a the union variable changes due to right-to-work states, but we have it in the table for the sake of completeness.

Table 5: Balance Tests For Right-To-Work States

This table shows a balance table for our analysis of right-to-work states. We need to ensure that the observed differences are not caused by changes in demographic characteristics in the right-to-work states when the treatment is applied. Evidently, it should not be a surprise to see a decline in union coverage post right-to-work. Unlike most other regressions in this paper, these are assumed to be linear regressions unless specified otherwise to be logistical. As usual, we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Unemployment	Education	Experience	Female	White	Married	Union
Right-to-Work	-0.626***	-0.0681	-0.1000	-0.0344	0.0898	-0.00232	-0.237***
	(0.155)	(0.0852)	(0.244)	(0.0280)	(0.170)	(0.0324)	(0.0560)
State FE	✔	✔	✔	✔	✔	✔	✔
Year FE	✔	✔	✔	✔	✔	✔	✔
Logit				✔	✔	✔	✔
Observations	274,794	274,794	274,794	274,794	274,794	274,794	274,794

Notably, we have a significant coefficient on the unemployment variable (p<0.01) and the union variable (p<0.01). The idea that right-to-work reduces unemployment – i.e. unions increase unemployment – aligns with mainstream theory; through the wage premium effect and the insider-outsider effect, one should not be too surprised that right-to-work states might have less unemployment. The fact that right-to-work states reduce the probability of one being in a union makes sense and provides evidence that the right-to-work laws actually did what they were supposed to do. We should not be concerned if union is unbalanced because its decrease still counts as a channel through which right-to-work laws might impact DNWR. All of the other characteristics that are tested are not significant.

Table 6 - Right-to-Work Laws and DNWR

This table shows whether people employed in right-to-work states face different levels of DNWR. By extension, this analysis carries implications for labor bargaining power and unions in general. A positive coefficient on the interaction term would suggest that right-to-work laws decrease DNWR, while a negative coefficient on the interaction term would suggest that right-to-work laws increase DNWR. As usual, we use logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Zero Nominal Wage Growth		
Inflation x Right-to-Work	0.0106	0.00961	0.00843
	(0.00972)	(0.00860)	(0.00854)
Right-to-Work	-0.0729	-0.0417	0.0201
	(0.0485)	(0.0461)	(0.0394)
Unemployment		0.0486***	0.0545***
		(0.00790)	(0.00438)
Inflation			-0.0357***
			(0.00484)
Productivity Growth			-0.0307***
			(0.00354)
Controls	✔	✔	✔
Industry FE	✔	✔	✔
Occupation FE	✔	✔	✔
State FE	✔	✔	✔
Year FE	✔	✔	
Observations	274,794	274,794	274,794

We now implement the our actual difference-in-difference specification for right-to-work laws. From the middle column in Table 6, the direct effect of being right-to-work is a 4.2% decrease in the logodds (p=0.37) of having zero nominal wage change, which is not very significant. The coefficient on right-to-work changes sign depending on whether year fixed effects are included, which may underscore the usage of year fixed effects to control for unobserved factors that vary over time, such as macroeconomic trends or other policy changes, or it may simply reflect how the net direct effect of right-to-work laws on the odds of a nominal wage freeze is zero. The direct effect of inflation from the right-most column that a 1 percentage point increase in inflation leads to a 3.6% decrease ($p<0.01$) in the logodds of seeing zero nominal wage growth. The interaction coefficients all suggest that being right-to-work causes around a 1 percentage point further percent decrease (p=0.26)[13] from inflation. Although these

[13] Using the middle column

interaction coefficients are not significant, they do provide some evidence that right-to-work laws do make an impact on DNWR. Because the sign on the interaction term is positive, this specifically provides evidence that right-to-work laws decrease DNWR.

Table 7 - Right-to-Work Laws and DNWR by Union Coverage

This table repeats the analysis done in Table 6, but instead partitions the dataset into people covered by a union contract and people who are not converved by a union contract. The motivation behind this table is to see whether union members are affected more by right-to-work laws with regards to DNWR. A more positive coefficient for the interaction terms when regressing only on those covered by a union contract would suggest that right-to-work laws gave union members a larger decrease in DNWR than nonunion members. As usual, we use logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Not Covered By Union Contract			Covered By Union Contract		
Inflation x Right-to-Work	0.0136	0.0126	0.0124	0.0268	0.0258	0.0233
	(0.0101)	(0.00915)	(0.00938)	(0.0179)	(0.0168)	(0.0175)
Right-to-Work	-0.0877*	-0.0550	0.0126	0.00238	0.0311	0.105
	(0.0529)	(0.0510)	(0.0447)	(0.0797)	(0.0785)	(0.0792)
Unemployment		0.0514***	0.0599***		0.0437***	0.0320***
		(0.00763)	(0.00416)		(0.0126)	(0.00713)
Inflation			-0.0429***			-0.0153*
			(0.00613)			(0.00878)
Productivity Growth			-0.0269***			-0.0480***
			(0.00384)			(0.00857)
Controls	✔	✔	✔	✔	✔	✔
Industry FE	✔	✔	✔	✔	✔	✔
Occupation FE	✔	✔	✔	✔	✔	✔
State FE	✔	✔	✔	✔	✔	✔
Year FE	✔	✔		✔	✔	
Observations	217,094	217,094	217,094	57,699	57,699	57,699

In order to identify whether there were disparities in the impact of right-to-work laws on those actually covered by a union and those not covered by a union, we segment this previous analysis by partitioning the dataset between those covered by a union contract and between those not covered by a union contract. Let us begin by looking at those who are not covered by a union contract, whose results are placed on the left half of Table 7. From the middle column, we see that the direct effect of being right-to-work is a 5.5% decrease (p=0.17) in the logodds of seeing a nominal wage freeze. Looking at the column which does not use year fixed effects, we see that the direct effect of a one percentage point increase in inflation is a 4.3% decrease ($p<0.01$) in the logodds of seeing a nominal wage freeze, and from the interaction term, this percent decrease in logodds is reduced by 1.2 percentage points for right-to-work states ($p=0.187$). Thus, right-to-work laws decrease DNWR even for people who are not covered by a union contract.

Now, looking at those who are covered by a union contract, whose results are on the right half of Table 7, from the middle column we can see that the direct impact of being right-to-work leads to a 3.1% decline ($p = 0.69$) in the logodds of having a nominal wage freeze, but this is not even remotely significant. In the rightmost column, the direct effect of a one percentage point increase in inflation is a 4.8% decline in the logodds of seeing zero nominal wage change; this percent decrease in logodds is reduced by around 2.3 percentage points for those in right-to-work states ($p=0.184$). Thus, the result that right-to-work laws decrease DNWR still holds for specifically those covered by a union contract.

The interaction terms for the regressions run on those covered by a union contract are more than double the interaction terms for the regressions run on those not covered by a union contract. Furthermore, though their statistical significance is about the same, there are nearly four times less observations of people covered by union contracts than people not covered by union contracts. One would expect that the statistical power would be much worse for those covered by a union contract, yet these coefficients are still far from completely insignificant. These results, therefore, provide strong evidence that individuals covered by union contracts experience a greater reduction in DNWR due to right-to-work laws than those not covered by such contracts. This finding supports the idea that right-to-work laws, by diminishing labor bargaining power, influence DNWR, as we observe a more significant change in DNWR among those whose bargaining power is more sensitive to shifts like right-to-work laws, namely people whose wage setting power depends on unions.

Now finally, as one more check, we analyze the change in the probability of someone actually receiving a nominal wage *decrease* and then we compare it to how the probability of someone receiving a nominal wage *freeze*. While looking at a single dimension might not help us differentiate between whether what we are seeing is caused by a change in DNWR or a change in the latent nominal wage change, a combination of trends on both the wage decrease and the wage freeze sides could help paint a more clearer picture as to what is going on. If there really is less DNWR from right-to-work laws, then we should expect an increase in the probability of someone receiving a nominal wage cut after the law's implementation. Note that we removed any states that had an existing right-to-work law before the earliest year used in our analysis 1983.

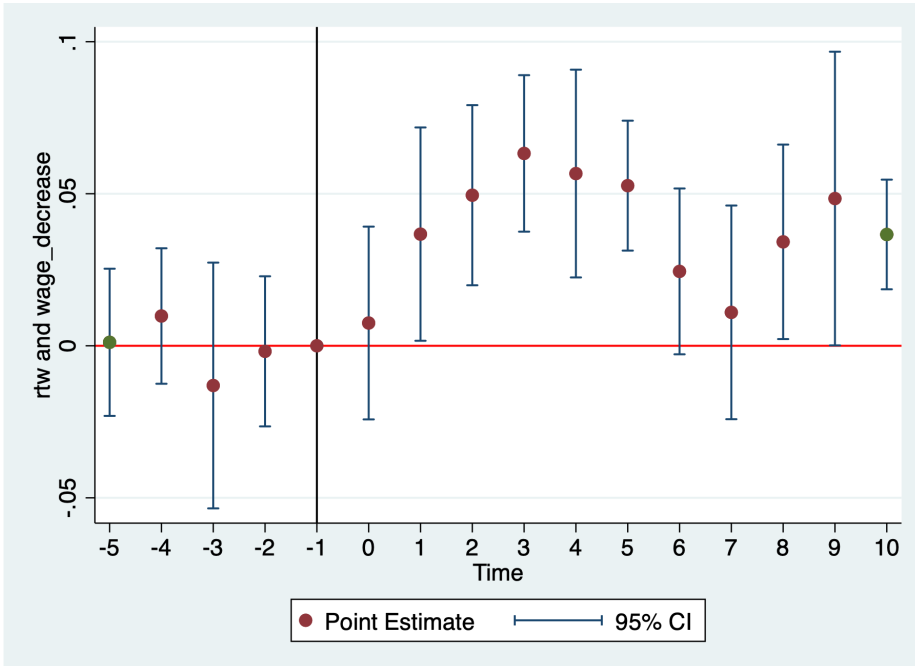

Figure 6: Event Study on Right-to-Work Laws and Nominal Wage Decreases

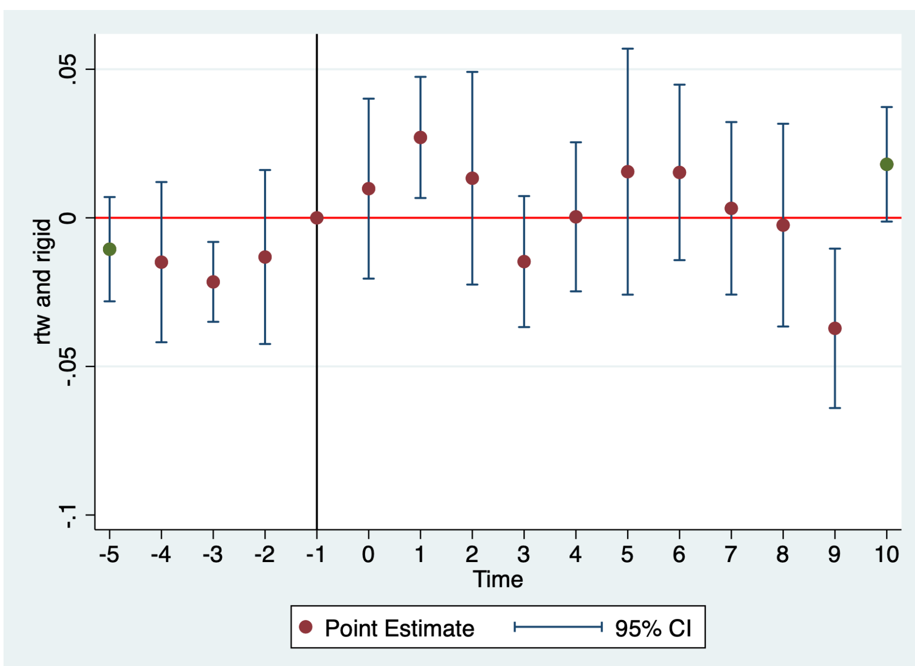

Figure 7: Event Study on Right-to-Work Laws and Nominal Wage Freezes

From our event study on the wage decrease outcome, we can see that from the onset of the right-to-work law's implementation, there is an increased likelihood of receiving a nominal wage cut with respect to the year before, which is very consistent with our findings. There is not much of a change on the year of implementation, which can be attributed to the fact that previous negotiated union contracts may still be in place for some workers during this time immediately after implementation. The years after the implementation year have seen higher frequencies of wage decreases, and most of the years are significant relative to the year before implementation. With regards to pretrends, there is not much going on as the frequencies of wage decreases are about the same for the five years leading up to the implementation of the right-to-work law.

Meanwhile, for the event study on the wage freeze outcome, we immediately notice that there are no distinct or significant trends on the impact of right-to-work laws on the probability of receiving a nominal wage freeze. There are some pretrends where it is less likely to receive a nominal wage freeze in the years before implementation, and the year after implementation there are more nominal wage freezes – but afterwards the nominal wage freezes remains relatively stable. There is one peculiar year for the ninth year after implementation, but this observation is most likely a spurious feature of the data.

Overall, our findings through our event study analysis corroborate the notion that right-to-work laws decrease DNWR. The fact that there were more frequent nominal wage cuts after the implementation of a right-to-workm laws suggests that either the right-to-work law increased DNWR or it decreased the latent wage growth. There was, however, no notable or sustained increase in the frequency of nominal wage freezes, which demonstrates that it is more likely that right-to-work laws decreased DNWR instead of people's latent wages going dow and then being stuck at zero nominal due to the rigidity once they cross the negative nominal wage change threshold.

NAFTA and Increasing Labor Substitutability

Following our analysis of unions, we explore whether labor substitution effects influence DNWR. There are two competing hypotheses regarding the impact of labor substitutability on DNWR. First, greater labor substitutability could increase DNWR, as firms have more options to maintain output while reducing costs. Instead of facing a binary decision between cutting wages or laying off employees, firms can preserve nominal wages for certain workers and find alternative cost-cutting methods, such as reducing the wages of foreign labor, which may be more socially acceptable as some developing countries have lower DNWR. Second, greater labor substitutability could decrease DNWR, as firms may feel less constrained by employee dissatisfaction with nominal wage cuts. If workers are easily replaceable, either by cheaper labor (e.g., foreign workers) or by automated capital, employers might be less concerned about the possibility of increasing churn from dissatisfied workers.

We test these hypotheses using the impact of NAFTA on workers who work in the manufacturing, agricultural, or raw materials industries. We set these industries as the treatment group because these industries are the most highly tradable sectors and are the most impacted by international trade agreements. Thus, we can identify a clear treatment group and pre and post periods – NAFTA was implemented in 1994. Additionally, as a reminder, we limit our analysis such that the latest year it extends to is 2005 for the purpose of shielding it from the effects of other trade shocks.

We first begin by running the following balance test in order to determine whether certain characteristics of the highly tradable industries changed post NAFTA. As we can see in Table 8, there is much more going on than when we ran the balance test for right-to-work laws.

Table 8 - Balance Test for NAFTA on Highly Tradable Industries

This table shows a balance table for our analysis of NAFTA on highly tradable industries (manufacturing, agriculture, raw materials). We need to ensure that the observed differences are not caused by changes in demographic characteristics in the highly tradable industries from NAFTA when the treatment is applied. Unlike most other regressions in this paper, these are assumed to be linear regressions unless specified otherwise to be logistical. As usual, we cluster on state, but we did consider using industry for our clusters. We did not use industry for our clusters due to concerns that there are not enough industries. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Unemployment	Education	Experience	Female	White	Married	Union
Post x Treat	-0.0340	-0.836***	1.638***	-1.159***	-0.232**	0.288***	0.308**
	(0.0434)	(0.106)	(0.187)	(0.0570)	(0.101)	(0.0334)	(0.122)
Industry FE	✔	✔	✔	✔	✔	✔	✔
Year FE	✔	✔	✔	✔	✔	✔	✔
Logit				✔	✔	✔	✔
Observations	141,446	141,446	141,446	141,446	141,446	141,446	141,446

These highly tradable industries have become less educated, more experienced, more male-dominated, more racially diverse, more unionized, and contain proportionately more people who are married. The less educated aspect of these trends can potentially be explained by the fact that there is a growing skilled economy, so people who are now more educated would much rather work in the skilled economy.[14] The increase in experience might simply reflect that younger people may have stopped entering these industries, due to the creation of more higher-skilled jobs and the declines in these industries overall. The decrease in women might reflect how women have more opportunities to take on skilled jobs. The decrease in the proportion of white people may reflect changes in hiring culture. The significant increase in the

[14]Shown in Autor (2014)[15]

proportion of married workers is particularly interesting, and this could be explained by the fact that perhaps younger people are no longer entering these industries. The increase in union may suggest that it might now be more of a necessity to be part of a labor union for those working in these industries.

Table 9 - NAFTA and DNWR

This table shows whether people employed in highly tradable industries saw a different change in DNWR due to NAFTA than people who do not work in these highly tradable industries. By extension, this analysis carries implications for labor substitutability in general. A positive coefficient on the triple interaction term would suggest that NAFTA decreased DNWR for people who worked in highly tradable industries, and a negative coefficient on the triple interaction term would suggest that NAFTA increased DNWR for people who worked in highly tradable industries. As usual, we use logistic regressions, and we cluster on state. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Zero Nominal Wage Growth		
Inflation x Post x Treat	-0.00311	-0.00399	-0.00690
	(0.0444)	(0.0444)	(0.0440)
Post x Treat	-0.000714	0.000413	-0.00524
	(0.0445)	(0.0435)	(0.0438)
Inflation x Post	-0.181**	-0.145*	-0.0229
	(0.0839)	(0.0836)	(0.0242)
Inflation x Treat	-0.0264	-0.0281	-0.0261
	(0.0218)	(0.0219)	(0.0218)
Unemployment		0.0734***	0.0515***
		(0.0109)	(0.00851)
Post			0.0465
			(0.0329)
Inflation			-0.000102
			(0.0111)
Productivity Growth			-0.0236***
			(0.00709)
Controls	✔	✔	✔
Industry FE	✔	✔	✔
Occupation FE	✔	✔	✔
State FE	✔	✔	✔
Year FE	✔	✔	
Observations	141,446	141,446	141,446

Paying attention to Table 9, all three triple interactions are negative but do not come close to being statistically significant. This may be a result of the need for more statistical power, especially when doing triple interactions, but this may also reflect the notion that there might not be much going on. Looking at the double interactions, we can see that for *Inflation x Post*, we do have a statistically significant negative coefficient in the leftmost column, a negative coefficient that is significant at the p=0.10 level in the middle column, and an insignificant negative coefficient on the right. This does speak to the idea that DNWR has increased over time, which supports our findings from Table 2. In a similar vein, the double interaction for *Inflation x Treat* is negative and is not completely insignificant (p=0.20)[15] which does lend some credence to the notion that perhaps people who work in these more vulnerable industries experience more DNWR. We cannot say for sure that it has to do with the fact that these industries are heavily exposed to international shocks, or if it happens to be through some shared common characteristic of the types of people who work in these industries.

Regardless, the previous results do not mean much, and we look to using propensity score matching to help us create better comparisons. We are concerned of an imbalance between those who would work in these highly tradable industry and those who would not, so we continue this analysis by utilizing propensity score matching to weight the control group to look more like the people who work in these highly tradable sectors. We match on the controls which we have been using, in addition to occupation. The following plot depicts the the control group's propensity distributions before and after matching.[16]

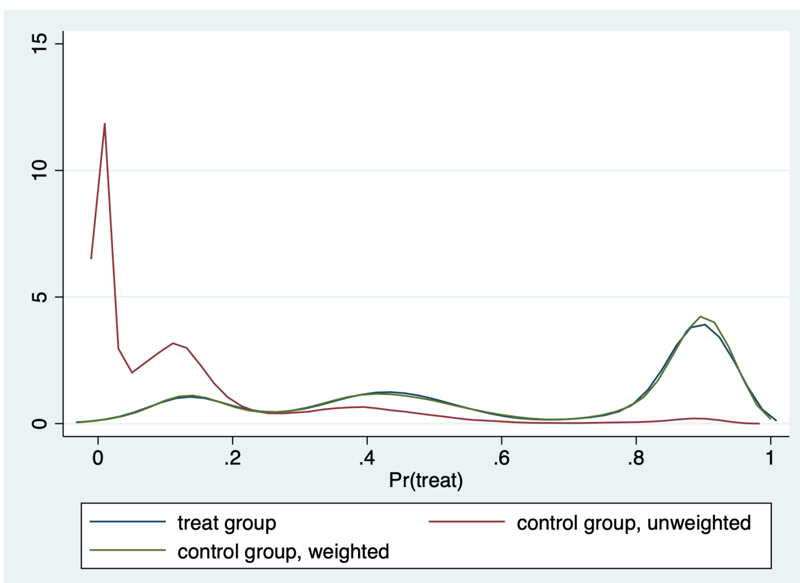

Figure 8: Propensity Score Distributions Before and After Weighting

[15]Using the middle column

[16]The Appendix Table 3 contains the updated balance table using the weighted control group, which shows some but not much improvement.

Table 10 - NAFTA and DNWR (PSM)

This table repeats the analysis done in Table 9, but implements propensity score matching in order to account for imbalances between the treatment and control groups (highly tradable vs non highly tradable industries). The four columns represent combinations of using/not using controls and weighting/not weighting the control group to look like people who would go into these highly tradable industries. In particular, we match on all of the observables we use as controls, which includes occupation. As usual, we use logistic regressions, and we cluster on state. Clustering on industry was heavily considered, but there is a concern which is that there are not enough industries to have valid clusters. Here, *** p<0.01, ** p<0.05, * p<0.1, and standard errors are in parentheses.

	Zero Nominal Wage Change			
Inflation x Post x Treat	0.00197	0.0612	-0.00399	0.0409
	(0.0451)	(0.0719)	(0.0444)	(0.0717)
Inflation x Post	-0.162**	0.0130	-0.145*	0.0258
	(0.0824)	(0.226)	(0.0836)	(0.227)
Inflation x Treat	-0.0308	-0.0855**	-0.0281	-0.0830**
	(0.0219)	(0.0362)	(0.0219)	(0.0341)
Post x Treat	0.00539	-0.0632	0.000413	-0.110
	(0.0474)	(0.0856)	(0.0435)	(0.0856)
Inflation	-0.341	-0.456		
	(0.260)	(0.336)		
Post	0.115**	0.494***		
	(0.0512)	(0.157)		
Treat	-0.0840***	-0.118**		
	(0.0241)	(0.0485)		
Controls			✔	✔
Industry FE			✔	✔
Occupation FE			✔	✔
State FE			✔	✔
Year FE			✔	✔
Weighting		✔		✔
Observations	141,446	140,626	141,446	140,626

Looking at Table 10,[17] we notice that after weighting, the coefficient on the triple interaction term becomes much more positive, suggesting that this may have been biased downwards. Although they are more significant than before, they are still far from being statistically significant, which may reflect that there really is not an effect or we need more statistical power for a triple interaction. Looking at the *Interac-*

[17]Appendix Table 4 has results for ATE on average and untreated

tion x Treat double interaction term, we can see that, with weights, the coefficients become significant (p=0.018); given that these coefficients are negative, they suggest that there is more DNWR being in the highly tradable industry than there is being in other industries. Moreover, the significant term on *Inflation x Post* goes away once we weight the control group, which suggests that these were previously biased downwards. Overall, although there is only extremely weak evidence suggesting that NAFTA decreased DNWR through the insignificant positive coefficient after weighting, the major fact this table reveals is that these vulnerable highly tradable industries see more DNWR, as seen from the significant negative coefficient on the double interaction term between inflation and treat.

Next, in the same spirit as before, we look at the event study on the probability of receiving a nominal wage cut and freeze. The event study will be helpful in seeing whether this was a sudden change or if there were gradual adjustments towards the free trade policy.

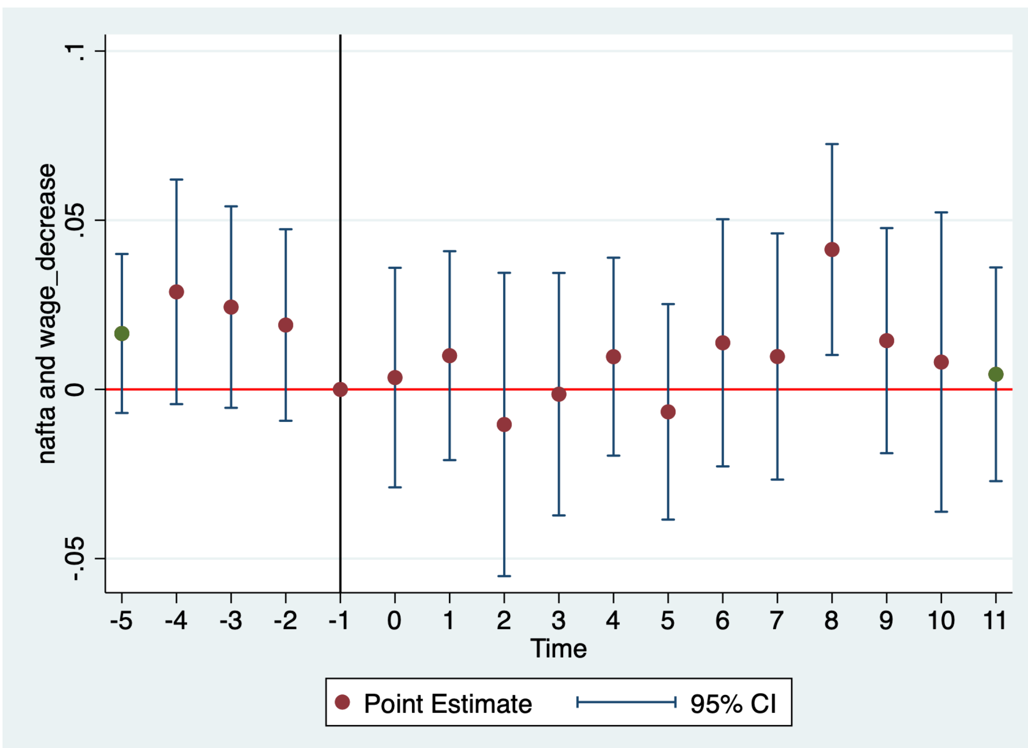

Figure 9: Event Study on NAFTA and Nominal Wage Decreases

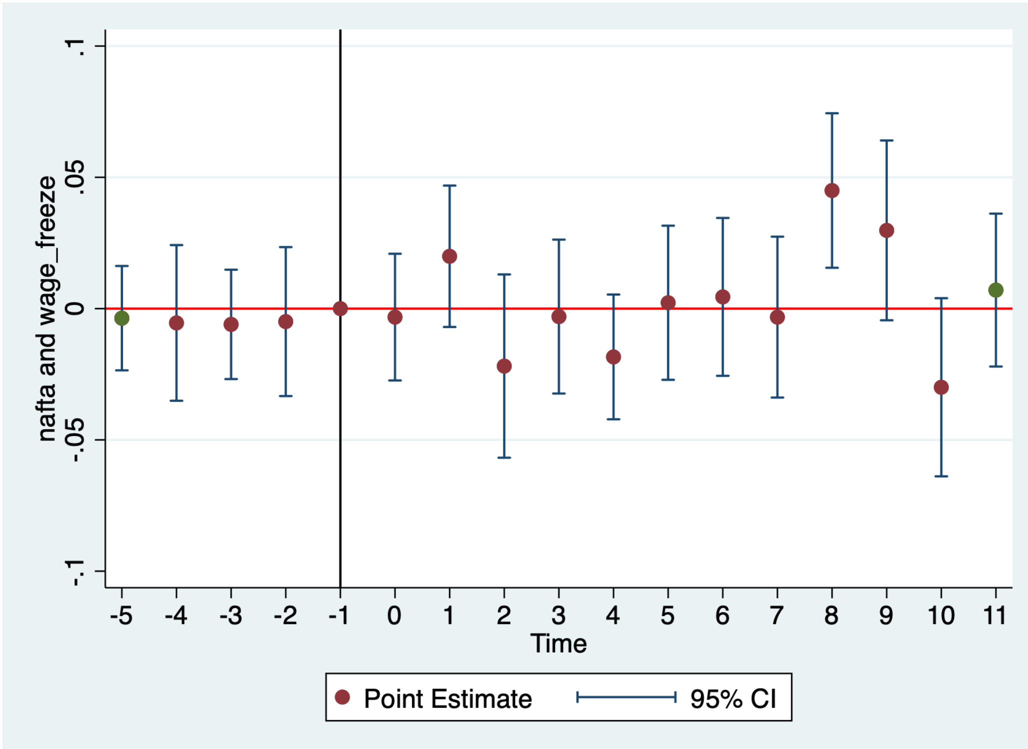

Figure 10: Event Study on NAFTA and Nominal Wage Freezes

The event study for nominal wage decreases suggests that NAFTA hardly had any impact on the frequency of nominal wage decreases for workers in these highly tradable sectors. However, the years leading up to NAFTA saw an increase in nominal wage decreases, which may suggest that firms shifted their behavior and standard pay levels for these particular roles ahead of time – it is true that firms had years of advance knowledge that a large free trade agreement between the United States and Mexico to occur. There is a notable point eight years after the implementation of NAFTA (2002) with a higher proportion of wage decreases. This may be attributed to some confounding effect, potentially due to the shock to world trade 9/11 represented.

The event study for nominal wage freezes suggests that NAFTA, again, hardly had any impact on nominal wage freezes for workers in these highly tradable sectors. Unlike the previous event study on nominal wage decreases, there are no notable pretrends. Furthermore, the years after implementation were relatively consistent as well. Again, there is a notable point eight years after the implementation of NAFTA, but this might just be attributed to the 9/11 trade shock.

When putting these two plots together, they at most suggest that in the years leading up to NAFTA, it is possible for firms to have already began adjusting their wages, decreasing their domestic wages such that they reflect the cheapness of foreign labor. Given that NAFTA established one of the largest free trade zones ever, one would think that it should have a profound impact specifically on these highly tradable sectors, and there is much literature which has documented its effect. However, the event studies have shown that when it comes to wage decreases and wage freezes, it hardly has any particular effect, which may simply mean that labor substitutability is not that important in affecting DNWR, especially when compared to bargaining power. [18]

Discussion

To reiterate, we first found that 1) DNWR has increased in general, and 2) lower income workers face more DNWR than higher income workers, even though in the past it was likely the opposite. In light of this finding, we analyzed two major labor market shocks that have characterized the past forty years of changes to the labor market, especially for its most vulnerable members: decrease in labor bargaining power and increase in the substitutability of labor, particularly that of domestic labor for foreign labor.

As it pertains to right-to-work laws and unions, our findings suggest that right-to-work laws worked to decrease DNWR. It appears that unions help reinforce DNWR, which is likely through the channel that unions may provide more pressure to employers to not cut wages. In addition to mere pressure, unions can lock employers in multi-year labor agreements such that employers cannot adjust their employee's nominal wages downward in response to worsening economic conditions even if they actually wanted to. Moreover, our findings also demonstrated that the impact of right-to-work laws on DNWR was greater for those covered by a union contract over those who are not.

In our analysis of NAFTA and labor substitutability, our findings remained inconclusive about the nature between increased access to foreign labor and DNWR; if anything, our analysis suggests that there is not much of an effect. We defined our treatment group as anyone who worked in an occupation that was most impacted by NAFTA (and generally any free trade agreement): manufacturing, agriculture, and raw materials. These occupations are generally the most integrated in global value chains, and are simultaneously some of the most vulnerable labor market groups. What we did find through this portion of the analysis is that these vulnerable highly tradable industries see more DNWR than other industries.

While these findings alone are interesting, there remains just one piece of inconsistency. Our results have simultaneously demonstrated that:

[18] Before we move on, interestingly enough, the proportion of wage freezes between both groups were highly parallel before 1994, but no longer parallel post 1994 as seen in Appendix Plot 1.

1. In recent years, lower-income workers now see relatively higher DNWR than higher-income workers, while it was the other way around in the past.

2. Two of the arguably biggest changes to the fundamental nature of the labor market in the past forty years fail to provide any evidence that vulnerable workers should experience more DNWR than they did before. Weakening unions and labor bargaining power has been shown to decrease DNWR, while there was little to no effect of NAFTA on DNWR.

And thus, we put forth an empirical puzzle.

Further work regarding this topic can be aligned with solving this empirical puzzle. While it seems reasonable that changes in bargaining power and changes in labor substitutability covers pretty much all types of changes to the structure of wage changes, it might be possible that different types of changes in bargaining power and changes in labor substitutability can yield different results than our analysis of right-to-work laws and NAFTA. For instance, one could potentially analyze the impact of increasing labor market concentration on the bargaining power side of things, or one could also potentially analyze the impact of automation on the substitutability side of things.

It is important that we can determine why more vulnerable members of the labor market are experiencing more DNWR such that we can design effective policies to mitigate it. It is already a well-known fact that the working class has been experiencing decreasing labor market outcomes and increasing inequity. Historically, the working class usually benefits most from the fruits of economic expansion – most of the jobs added to the economy during an economic boom historically have been working class jobs.[?] It seems that this, too, is no longer the case, as reflected by our finding that DNWR is now higher for lower-income workers. Moreover, while lower-income jobs were always more likely to be removed during an economic recession due to the fact that the latent wage change distribution for lower-income workers is shifted more to the left, this increase in DNWR for lower-income workers means that they will suffer an even larger negative impact on their unemployment outcomes during a recession than before.

The urgency to remedy this becomes even more clearer when viewed through the lens of today's rising political populism. Economic frustration among the working class has become a defining force in modern political discourse. Politicians like Donald Trump have capitalized on these frustrations, rallying support from disaffected workers by criticizing globalization and labor market outsourcing's effects on domestic workers. Others, such as Andrew Yang, have drawn attention to automation and its disproportionate impact on routine, low-skill jobs. Even union advocacy, once considered a fading issue, has returned to prominence, with figures like Joe Biden touting their pro-union credentials to court the working-class vote. These narratives reveal how anxieties over such factors extend far beyond theoretical economic debates – they shape the political movements that resonate with working-class voters. Addressing the underlying factors of DNWR is therefore not just about economic fairness but also about understanding and responding to the political and social dynamics that define our era. By doing so, we can better serve the working-class American – the demographic most affected by these changes – and contribute to a more equitable and stable society.

Conclusion

Ultimately, this analysis has shed light on the evolving dynamics of DNWR in the U.S. labor market, particularly in relation to changes in labor bargaining power and labor substitutability over the past several decades. We first find that lower-income workers now face more DNWR than higher-income workers. Second, our findings suggests that, contrary to expectations, core major labor market shocks and trends that particularly affect lower-income workers like the decline in union strength and increased labor substitutability from the integration of foreign labor markets are not associated with increasing DNWR. In fact, we find that the decline in union strength decreases DNWR, and increased labor substitutability does not have a clear effect on DNWR.

The rise in DNWR among lower-income workers signals a troubling trend that warrants urgent attention. As these workers are already more vulnerable to economic downturns, increased wage rigidity will likely exacerbate their hardships during recessions, leading to higher unemployment and more prolonged economic insecurity. This trend is not just an economic concern but a political one as well, as rising political populism and the growing dissatisfaction of working-class voters highlight the broader social implications of economic inequality. To address this challenge, further research is needed to explore the complex forces driving DNWR and to identify targeted policy interventions. Policymakers must consider how to balance labor market flexibility with protections for the most vulnerable workers. In doing so, we can not only improve economic outcomes for those at the bottom of the income distribution but also strengthen the political and social fabric of the nation. Tackling DNWR is therefore not just a matter of economic theory but a crucial step toward ensuring a fairer, more resilient labor market in the future.

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Appendix

The data and code to replicate this study can be found here: `https://github.com/kevincow/ECON80-Paper`.

Appendix Table 1

Industry Code	Industry Name
1	Agriculture/Forestry/Fishing/Hunting
2	Mining/Oil
3	Construction
4	Manufacturing
5	Wholesale Trade
6	Retail Trade
7	Transportation/Warehousing
8	Utilities
9	Communications
10	Finance/Insurance
11	Real Estate
12	Professional/Scientific/Technical Services
13	Consulting
14	Business Administrative/Support Services
15	Education
16	Healthcare/Social Assistance
17	Arts/Entertainment/Recreation
18	Accommodation/Food Services
19	Other Services
20	Public Administration
21	Military

Table 1: Industry Labels

Appendix Table 2

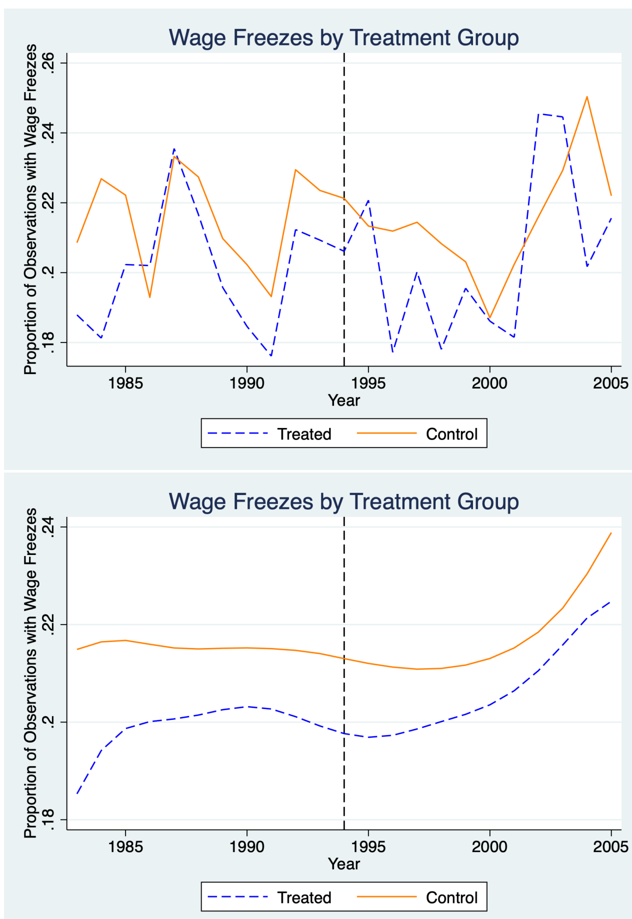
Occupation Code	Occupation Name
1	Management
2	Financial Operations
3	Computer/Math Operations
4	Architecture/Engineering
5	Life/Physical/Social Sciences
6	Community/Social Services
7	Legal
8	Education
9	Arts/Entertainment/Media
10	Healthcare Practitioner
11	Healthcare Support
12	Protective Services
13	Food Services
14	Cleaning
15	Personal Services
16	Sales
17	Administrative Support
18	Farming/Fishing/Forestry
19	Construction
20	Installation/Maintenance
21	Production
22	Transportation
23	Material Moving
24	Military

Table 2: Occupation Labels

Appendix Table 3

	(1) unemployme nt	(2) educ	(3) experienc e	(4) female	(5) white	(6) married	(7) union
post_treat	0.0253	0.0715	0.750***	-0.476***	0.0786	0.189***	0.0720**
	(0.0157)	(0.0600)	(0.206)	(0.0374)	(0.0483)	(0.0319)	(0.0360)
Industry FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Logit				X	X	X	X
Observatio ns	241,587	241,587	241,587	241,587	241,587	241,587	241,587

Appendix Plot 1



Appendix Table 4

	Weighted on Average		Weighted on Untreated	
inflation_post_treat	-0.319	-0.0546	-0.259	-0.0419
	(0.334)	(0.225)	(0.278)	(0.179)
post_treat	0.0470	0.0965	-0.00595	0.0284
	(0.335)	(0.309)	(0.270)	(0.247)
inflation_post	0.420	0.342	0.372	0.314
	(0.399)	(0.364)	(0.347)	(0.317)
inflation_treat	-0.122	-0.177	-0.108	-0.146*
	(0.123)	(0.108)	(0.0967)	(0.0852)
Controls		X		X
Industry FE		X		X
Occupation FE		X		X
State FE		X		X
Year FE		X		X
Average Weight	X	X		
Untreated Weight			X	X
Observations	141,446	141,446	140,626	140,626

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1