“Unshocking” a shock: Using stock market reactions to measure labor market outcomes from variations in trade policy exposure*

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Abstract

This paper examines the effect of protectionist trade policy, namely the 2018 trade war, and its ability to “unshock” deteriorating labor market outcomes following the establishment of permanent normal trade relations (PNTR) with China in 2000. Using stock market returns to measure county-level trade exposure, I find that, while negatively exposed counties see a sharp rise in unemployment during PNTR, the trade war failed to produce any significant short-term results aimed at reversing these adverse employment trends. These results demonstrate the asymmetry of trade policy outcomes and suggest the potential permanence of trade-related labor displacement.

Keywords: Reverse legislation, trade liberalization, protectionism

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1 Introduction

In October 2000, the United States Congress passed a bill granting permanent normal trade relations (PTNR) with China. The share of Chinese imports has since quadrupled, low-income-country US imports have risen from 15 percent to 28 percent, and total US spending on Chinese goods has increased by 800 percent (Autor, Dorn, and Hanson 2013). Much of this growth is attributed to the rise in China's manufacturing sector, which the St. Louis Fed describes as a "second industrial revolution" that "featured mass production of the means of mass production" (Wen et al. 2016).

As Chinese manufacturing has surged, US manufacturing has significantly weakened. Between 2000 and 2010, the number of US manufacturing sector jobs decreased by one third, dropping from 17 million to below 12 million. From 1998 to 2018, while total GDP increased by 47 percent, manufacturing-related GDP increased by just 5 percent, most of which can be attributed to higher skilled work in computers and electronics (USNews 2019). This dramatic decline in manufacturing is further accompanied by deteriorating economic conditions for the remaining college degree-less working class. Between 1990 and 2013, wages for men without a high school diploma decreased by 20 percent, and median income for men with a high school diploma or some college fell by 13 percent (OECD 2017).

To counter this "threat" to American jobs and "unfair" practices by the Chinese government, former president Donald Trump started a trade war in 2018. Trump imposed a series of tariffs amounting to more than 400 billion dollars in Chinese goods, while additional sanctions were placed on trade with Mexico and the European Union (BBC 2020).

In this paper, I evaluate the effectiveness of the 2018 trade war in reversing labor market outcomes for manufacturing dependent and other trade exposed communities. To measure trade exposure, I utilize the method developed by Greenland et al. (2020) to calculate industry-level average abnormal returns (AAR), which I then aggregate into county-level scores. Using these scores, I perform a series of difference-in-differences regressions to compare employment outcomes between positively and negatively exposed counties, before and after PNTR and the trade war. Similar to Pierce and Schott (2020), I also use AAR to measure differential health outcomes related to Case and Deaton (2015) documented deaths of despair. Finally, I examine the effect of reverse legislation and its ability to "unshock" the deteriorating economic conditions created by a past shock.

By studying the outcome of the reverse shock, this paper seeks to answer questions related to the asymmetry of labor displacement and health outcomes that emerge from trade and general policy which structurally alters industry and geographic employment trends. While some may argue that the benefits from trade liberalization outweigh the costs of manufacturing displacement (e.g. Silicon Valley vs the Rust Belt), I show that the labor market consequences for the "losers" were not only significant but potentially permanent. Unlike cases where trial and error can lead to balanced, optimal policy outcomes, the magnitude of labor displacement arising from major trade policy such as PNTR may be irreversible.

The paper proceeds as follows. Section 2 provides a literature review describing previously documented relationships between trade, employment, and health. Section 3 introduces AAR as a measure of trade exposure. Section 4 explains the data and methods used to aggregate our county-level AAR scores and outcome variables. Section 5 provides exploratory analysis and validation for AAR. Section 6 details the regression methodology and results. Section 7 interprets these results and concludes.

2 Literature Review

Free trade has long been thought to increase economic welfare, but several recent studies have highlighted potential consequences. Autor, Dorn, and Hanson (2013) demonstrate that the Chinese "trade shock" adversely affected local labor markets that were particularly vulnerable to import competition. They estimate that Chinese import competition explains 21 to 44 percent of US manufacturing sector employment decline between 1990 to 2007, and 26 to 55 percent isolating from 2000 to 2007. At the local labor market level, which includes industries outside of manufacturing, they find that spillovers associated with the increase in import exposure reduce overall employment and wages and increase government transfer payments and unemployment benefits. Pierce and Schott (2016), using the "NTR Gap" measure for trade exposure, find that moving an industry from the 25th to 75th percentile of trade exposure increases unemployment by 1.2 percent. Additionally, they find that this increase in unemployment is accompanied by disproportionate increases in total US imports from China, the number of US firms importing from China, and the number of Chinese firms exporting to the US.

Greenland, Lopresti, and McHenry (2019) examine labor market adjustments following PNTR by analyzing internal migration patterns. They find that geographic regions most exposed to trade face declining population growth rates, which are especially pronounced among younger males and Hispanic individuals. These changes are experienced with a time lag of 7 to 10 years. However, Autor, Dorn, and Hanson (2016) determine trade adjustment and the reallocation of labor across alternative US industries is slow-moving at best, finding no evidence to conclude that workers displaced from import competition were able to find employment in other sectors.

Looking at earnings and educational outcomes, Lin (2019) finds that while increased trade pressure from China provides increased incentives for people to attend college, future economic prospects decline. He documents that college enrollment rates increase by roughly 4 percent, mostly concentrated in 2 year programs and public institutions, while future earnings decrease by 4 to 6 percent. These increases in degree attainment are however, not accompanied by a subsequent rise in skill acquisition, demonstrating how possible foregone earnings and student debt make college enrollment an undesirable outcome. Furthermore, Lin finds college-educated workers holding at least a bachelor's degree experience significantly fewer adverse effects relative to workers without a bachelor's degree.

Kim and Vogel (2020) address overall welfare by looking at adjustments in labor supply, frictional unemployment, and sector-related job application trends. They find that counties at the 90th percent of exposure to PNTR saw a 3.1 percent decrease in welfare compared to counties at the 10th percent of exposure and conclude that changes in unemployment explain roughly 65 percent of this decline.

Many studies have also documented a relationship between declining macroeconomic conditions and health. Evidence suggests that worsening labor market conditions lead to weight gain and reduced mental health, especially pronounced for less-educated males (Charles and DeCicca 2008). Researchers have also found job loss to increase the risk of overall mortality, mostly from circulatory disease, suicide attempts, traffic accidents, alcohol related disease, and mental illness (Browning and Heinesen 2012). Previous upward trends in life expectancy in the US from the end of the 20th century have also reverted. Between 1999 and 2013, Case and Deaton (2015) report rising midlife mortality rates, mostly among less educated white males, from alcohol poisoning, suicide, and liver disease–a phenomenon they describe as "deaths of despair". Concurrently, midlife morbidity from the same population has increased as evidenced through self-reported declines in mental health and increases in chronic pain that inhibit everyday activities.

Specific to opioids, there is strong, county-level evidence that an increase in unemployment predicts an ensuing rise in opioid-related mortality rates, and opioid-related emergency department visits are expected to rise during economic downturns. State level analyses illustrate these same results to a higher magnitude (Hollingsworth, Ruhm, and Simon 2017). Other research focuses on geographic variation, finding increased rates of opioid abuse in the Midwest, Appalachia, and New England as economic conditions worsened (Monnat 2016).

Seltzer (2020) finds an inverse relationship between manufacturing sector employment and opioid-related drug mortality rates. Decreases in wages are also associated with increases in drug overdose. Venkataramani et al. (2020) show that counties experiencing the closure of an automobile manufacturing plant saw opioid mortality rates increase 85 percent relative to counties in which a plant closure did not occur. Concerning the relationship between free trade and opioid overdose, Dean and Kimmel (2019) find a positive association between job loss due to international trade and opioid overdose mortality at the county-level. Death rates were heavily concentrated in rural areas like Appalachia, and the presence of fentanyl in the local heroin supply increased the percentage of opioid-related overdose deaths nearly fivefold. Pierce and Schott (2020) find that a jump from counties in the 25th percentile of exposure to the 75th percentile increases mortality rate from drug overdoses by two to three deaths per 100,000 people annually. For reference, the average county in 2000 experienced 5 overdose deaths per 100,000 people.

Preliminary findings from the recent trade war suggest that the tariffs have actually damaged the US economy. A study conducted by Moody's Analytics in September 2019 documents roughly 300,000 jobs and an estimated 0.3 percent of real GDP had already been lost due to the trade war (Zandi, Rogers, and Cosma 2019). Amiti, Redding, and Weinstein (2020) find that US companies lost 1.7 trillion dollars worth of stock due to the US tariffs imposed on Chinese imports, and a majority of the tariffs were paid for by US companies.

3 Measuring Trade Exposure

Several previous studies measure trade exposure by mapping changes in tariff rates or import volumes to industries. For instance, Pierce and Schott (2016) exploit differences in Normal Trade Relations (NTR) tariff rates and non-NTR tariff rates to measure exposure to PNTR with China in 2000. Here, the authors construct an "NTR-Gap" for each SIC industry. Larger NTR gaps indicate a given industry is subject to larger tariff rate decreases, making that specific industry more susceptible to trade liberalization–the cost of outsourcing production to China is cheaper, so domestic production is expected to decline.

There are, however, several drawbacks to this standard type of approach. First, changes in import tariffs or quantities are not easily mapped to service firms, failing to capture the effect of trade shocks on non-goods producing industries. Service firms can be directly impacted by trade agreements through changing costs of intermediate goods from suppliers or through varying consumption behavior from affected consumers. These service firms also comprise a vast majority of employment in the United States. As of 2019, nearly 80 percent of the workforce was employed in a service sector (O'Neill 2021). Next, since tariff changes are not aggregated at the firm level, such measures may assume that all firms in a given industry respond to tariff changes similarly. This assumption fails to account for firm level differences that mostly give larger firms an advantage over smaller firms. Larger firms are better positioned to exploit the increased availability of lower-cost inputs by outsourcing production, creating a potentially offsetting channel of exposure (Antras, Fort, and Tintelnot 2017; Bernard et al. 2018). Finally, this measure cannot capture trade policy exposure for non-tariff related trade shocks, such as changes in intellectual property rights.

Following Greenland et al. (2020), I utilize event specific average abnormal returns (AARs), looking at industry-level stock market reactions to changes in trade policy to evaluate the expected effects of a shock on the economy. Abnormal returns are defined as

$$AR_{j,t} = ActualReturn_{j,t} - ExpectedReturn_{j,t} \tag{1}$$

for industry j at time t. A more detailed explanation of AAR can be found in section 4.1.

AAR addresses two of the aforementioned concerns. Using changes in stock data, the effects of a trade policy can be easily determined for all industries in a given economy. Furthermore, under the assumption that a specific trade event is the sole determinant of an abnormal return, changes in stock prices capture both tariff and non-tariff related trade shocks. Unfortunately, because I could not gain access to firm level stock data, firm level differences may still impact the results. With firm level stock data, we could aggregate firms into industries after accounting for firms that benefit from trade exposure.

To offer some additional interpretation, positive abnormal returns indicate a firm or industry benefits from a trade shock, while negative abnormal returns indicate the opposite. We expect that variation in stock market price properly captures an industry's future outlook.

4 Data Methods

4.1 AAR

County-level AAR scores are computed using a combination of daily industry stock returns, daily market portfolio returns, and county-level industry employment data. Daily industry stock return data is available on Kenneth French's personal website. This data set includes daily returns for 49 separate industries which are groupings of related SIC4 codes (French and Fama 2021). A list of these industries can be found in Table A.4. I calculate industry-level AAR following the method proposed by Greenland et al. (2020).

We start by assuming an industry's stock price at time t is a function of e_t, the policy announcement of interest at time t, and X_t, all other fim level information at time t. Recalling equation (1), we denote actual returns as $R_{j,t}$ and expected returns as $E(R_{j,t}|X_t)$, giving us

$$AR_{j,t} = R_{j,t} - E(R_{j,t}|X_t). \tag{2}$$

Actual returns are calculated by

$$R_{j,t} = \frac{P_{j,t} - P_{j,t-1}}{P_{j,t-1}}, \tag{3}$$

where price P for industry j can be easily accessed using our industry stock returns data for each day t. Expected returns, $E(R_{j,t}|X_t)$, are the returns we would have expected absent the policy announcement, and we estimate these values using the function

$$E(R_{j,t}|X_t) = \alpha_j + \beta_j F_t + \epsilon_{j,t}, \tag{4}$$

where the vector F_t consists of a single factor–the market portfolio returns for the year prior to a trade policy announcement. This is also known as the capital asset pricing model (CAPM) approach. By taking market portfolio returns from the year prior, estimates for α_j

and β_j are not affected by periods when relevant legislative information about a trade policy becomes known. Taking equations (2) and (3), we rewrite equation (1) as

$$AR^*_{j,t} = R_{j,t} - (\hat{\alpha}_j + \hat{\beta}_j F_t), \tag{5}$$

where $AR^*_{j,t} = AR_{j,t}$ only if expected returns are unbiased.

To understand how we can mitigate bias in expected returns, decompose F_t and ϵ_t into components X_t and e_t, such that

$$F_t = F^X_t + F^e_t \tag{6}$$

and

$$\epsilon_{j,t} = \epsilon^X_{j,t} + \epsilon^e_{j,t}. \tag{7}$$

Rewriting equation (5) to account for equations (6) and (7), we have

$$AR^*_{j,t} = R_{j,t} - (\hat{\alpha}_j + \hat{\beta}_j F^X_t + \hat{\beta}_j F^e_t) + \epsilon^X_{j,t} - \epsilon^X_{j,t}. \tag{8}$$

Rearranging terms,

$$AR^*_{j,t} = R_{j,t} - (\hat{\alpha}_j + \hat{\beta}_j F^X_t + \epsilon^X_{j,t}) + \hat{\beta}_j F^e_t + \epsilon^X_{j,t}, \tag{9}$$

and using equations (2) and (4), we arrive at

$$AR^*_{j,t} = AR_{j,t} + \hat{\beta}_j F^e_t + \epsilon^X_{j,t}, \tag{10}$$

such that $AR_{j,t} = AR^*_{j,t}$ only if $\hat{\beta}_j F^e_t = 0$ and $\epsilon^X_{j,t} = 0$. $\hat{\beta}_j F^e_t$ is interpreted as bias induced by the effect of the policy announcement on systematic factors. We avoid this by taking market portfolio returns from the year prior to the policy announcement as our factor vector to estimate expected returns. The term $\epsilon^X_{j,t}$ can be interpreted as confounding non-trade policy factors that occur at the same time as the policy announcement (e.g. dividend announcements or anticipatory trading). We reduce this bias by subsetting our data set to only include dates on which policy announcements are made and the two day windows surrounding each policy announcement, computing the average of abnormal returns for these date groupings. Following the event study literature, these 2 day windows should be sufficient to capture anticipatory or lagging trading. Thus, for policy announcement k that occurs at time $t = 0$, the average abnormal return associated with that policy announcement for industry j is

$$AAR_{k,j} = \frac{1}{5} \sum_{t=-2}^{2} AR^{*}_{k,j,t}. \tag{11}$$

For each overarching event, such as the introduction of PNTR or the 2018 trade war, there are n policy announcements corresponding to each event. These policy announcements are described in Tables A.1 and A.2 in the Appendix. As such, AAR_j^{event} represents the overall industry score for one of these events, and is calculated as

$$AAR_j^{event} = \frac{1}{n} \sum_{k=1}^{n} AAR_{k,j} \tag{12}$$

We now aggregate these industry-level AAR scores to county-level scores by taking the employment-share-weighted-average industry score for each county c. To avoid issues associated with anticipatory labor reallocation, I use employment counts from 1990 for PNTR and 2013 for the trade war. County-level industry employment counts are publicly available through the Eckert et al. (2020) imputed County Business Patterns (CBP) data. These imputed datasets are publicly available on Fabian Eckert's personal website. CBP data is aggregated at the county industry level and includes employment counts for NAICS 2, 3, 4, 5, and 6 industry classifications. I use NAICS 6 codes because they are the most compatible with SIC 4 codes necessary for linking my data sets to create AAR scores. Since the imputed CBP files are not available at the NAICS 6 level for the PNTR period, I use the 1990-2013 county-industry employment count data set available for download from Pierce and Schott (2020).

In order to link our industry-level AAR scores with county-industry employment counts, we need to map NAICS 6 codes to the Fama French 49 industry definitions. We start by joining NAICS 6 industry level employment counts with 2012 SIC 4 codes using a crosswalk created by Sasha Anderson from the website "data.world" (Anderson 2017). I then update 2012 SIC codes to 2017 codes using a similar crosswalk from the same website, and connect 2017 SIC 4 codes to Fama French 49 industry definitions using a crosswalk created by Volkova (2018).

Next, I calculate county-level AAR scores using the employment-share-weighted-average industry score given by

$$AAR_c^{event} = \sum_j \frac{L_{jc}}{L_c} AAR_j^{event}, \tag{13}$$

where c indexes counties and L represents a county's employment share for each industry j. To offer some additional interpretation for this measure, counties with positive AAR_c^{event} have a high concentration of firms in an industry that outperform expectations during the trade shock. Counties with negative AAR_c^{event} do not fare as well, and counties with an AAR_c^{event} near zero either have a large concentration of industries that are not affected or have employment concentrated in industries whose effects are offsetting.

4.2 Outcomes

I use Bureau of Labor Statistics unemployment and labor force participation data as my proxy for economic conditions (BLS 2022). This data contains county-month level unemployment and labor force participation counts, which I aggregate into years for PNTR and quarters for the trade war.

To measure deaths of despair, I use the restricted-use 'All Counties 1990-2019 Detailed Mortality' data set from the National Center for Health Statistics (NCHS). This individual-level data set lists geographic, demographic, and cause of death information, and requires an application to access (it is possible that individuals are identified in smaller counties).

To classify deaths of despair from 1990-1998, I use NCHS 282 cause recodes 33700-34400 for suicide, 31700 and 35300 for drug overdose, and 24200 for alcohol-related liver disease (ARLD). From 1999-2019, I use NCHS 358 cause recodes 424–431 for suicide, 420 and 438 for drug overdose, and 298 for ARLD.

I aggregate these death counts by county-year-age for PNTR and county-quarter-age for the 2018 trade war, where age is broken into five year age brackets. I also use the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) data to calculate county-year-age population estimates (SEER 2021). Then, using the 2000 US standard million population for 18 age groups given by Table A.3, I calculate age-adjusted death rates with

$$AgeAdjustedDeathRate_{ct} = \frac{1}{10}\sum_{i}^{18}\frac{USPopPerMM_i * Deaths_{ict}}{CountyPop_{ict}}, \tag{14}$$

where i indexes the 18 US age group brackets, c indexes counties, t indexes time, and the age-adjusted death rate represents deaths per 100,000. Age-adjusted rates are important since we are comparing counties with varying age distributions.

5 Exploratory Analysis

In this section, I perform a series of exploratory analyses to examine the validity of my AAR_c^{Event} measure. Looking at the industry-level AAR scores for PNTR, Figure 1 depicts the 10 industries with the highest scores and the 10 industries with the lowest scores. Notice that many of the lowest performing industries are manufacturing related, such as steel works and fabricated products, while many of the highest performing industries are not. Here, the stock market accurately predicts the subsequent manufacturing decline following PNTR. As such, these results are mostly consistent with our expectations of industry-level AAR behavior.

Figure 2 provides an overview of the AAR_c^{PNTR} distribution. The mean AAR_c^{PNTR} is -0.065 with a standard deviation of 0.12. The shape of the distribution indicates that

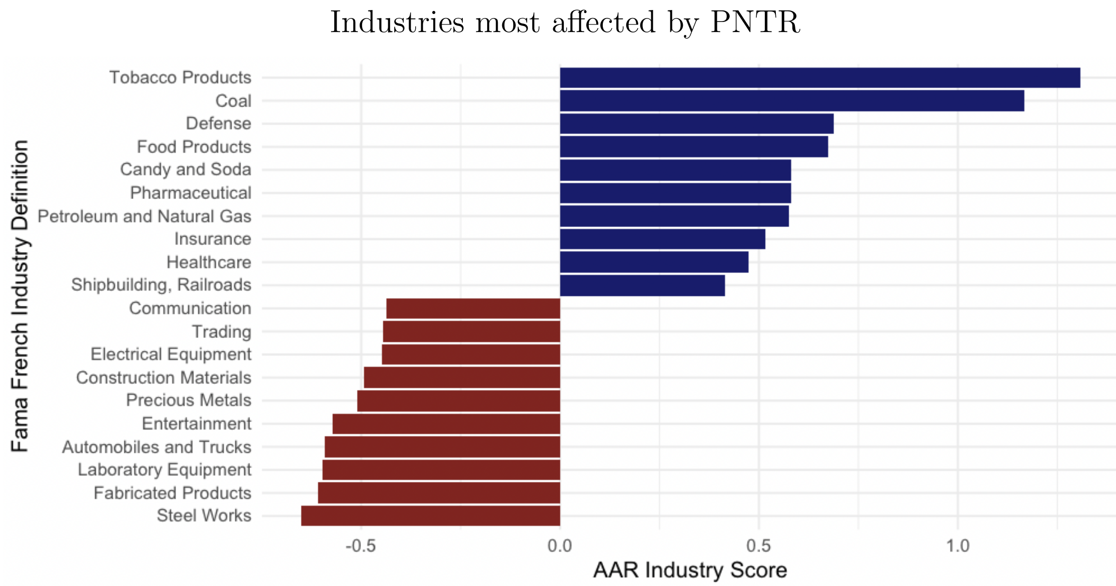

Figure 1: Top 10 best and worst performing industries based on AAR^{PNTR} scores

the scores are approximately normally distributed. Thus, counties are mostly negatively impacted.

Figure 3 depicts the geographic distribution of these county-level scores. This map is consistent with manufacturing employment distribution and previous manufacturing decline literature, as the most negatively impacted counties are largely concentrated in the Rustbelt, Midwest, and Appalachian regions. Several negatively impacted counties are also dispersed along the west coast and southern regions, where, presumably, local industries are more exposed to import competition post-PNTR due to their proximity to a coastline.

To further validate my county-level AAR scores, I compare AAR_c to the proven county-level Pierce Schott NTR-Gap. For this exercise, I compare AAR_c^{PNTR} to $NTRGap_c$ using a simple OLS estimator,

$$AAR_c^{PNTR} = \beta NTRGap_c + \epsilon_c. \tag{15}$$

Because AAR_c^{PNTR} includes negative values while $NTRGap_c$ does not and given the measures' varying magnitudes, I standardize both. The OLS results are displayed in Table 1. A relative one point increase in NTR-Gap yields a -0.326 point decrease in AAR_c^{PNTR}. This is because of the inverse relationship between the change in tariff rates and economic con-

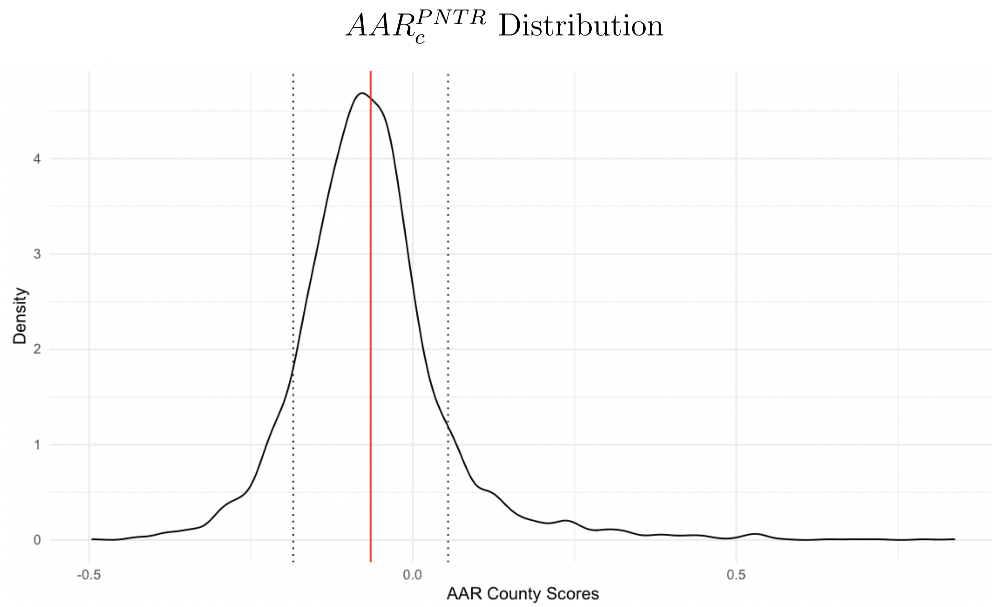

Figure 2: AAR_c^{PNTR} scores have mean -0.065 and standard deviation 0.12

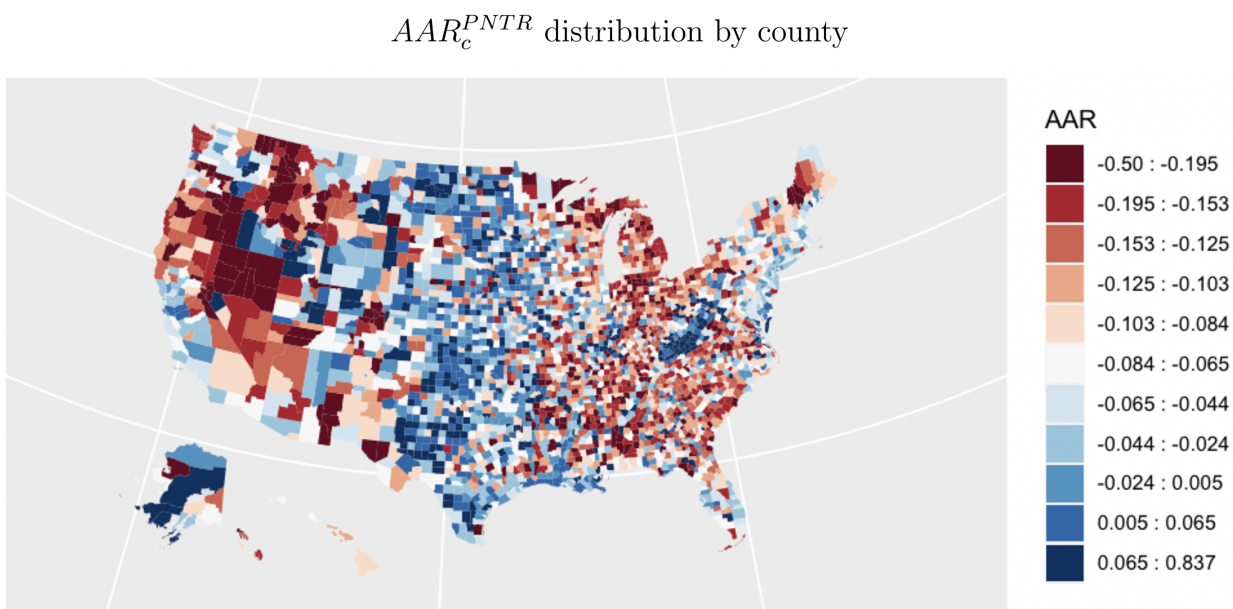

Figure 3: Rustbelt, Midwest, Appalachian, and coastal regions appear to be most negatively impacted by PNTR.

Table 1: OLS estimator to compare AAR_c^{PNTR} with $NTR-Gap_c$

	Standardized AAR_c^{PNTR}
Standardized $NTRGap_c$	-0.326^{***}
	(0.017)
Constant	-0.000
	(0.017)
Observations	3,182
Adjusted R^2	0.106
Residual Std. Error	0.945 (df = 3180)
F Statistic	379.221^{***} (df = 1; 3180)
Note:	*p<0.1; **p<0.05; ***p<0.01

ditions. A higher NTR-Gap signifies a greater difference in tariff rates. Greater difference in tariff rates indicates a county reliant on goods-producing industries is more exposed to trade. And a county that is more exposed to trade is expected to have a negative AAR.

6 Regression Methodology and Results

This section examines the relationship between county-level trade war exposure to PNTR and labor market outcomes. Additionally, I develop a method to explore whether the 2018 trade war was able to reverse any negative consequences associated with PNTR.

6.1 Methodology

To measure how counties that were differentially exposed to trade before and after a trade policy event fared with employment and health outcomes, I utilize the following fixed effects difference-in-differences regression model,

$$Y_{c,t} = \beta_0 + \delta_c + \delta_t + \beta_1 AAR_c^{Event} * post_t + \epsilon_{c,t}, \tag{16}$$

where $Y_{c,t}$ represents the county level outcome–employment or deaths of despair–in time period t. For PNTR, the sample period for t is in years from 1990 to 2012, and for the trade war, t indexes quarters from 2016 Q1 to 2019 Q4. δ_c and δ_t are county and time fixed effects. Our DID terms of interest follow, where AAR_c^{Event}, a discrete indicator, is labelled "1" if a county's AAR is less than one standard deviation below zero, and "0" for counties with an AAR greater than one standard deviation above zero. $post_t$ is a dummy indicator labelled "1" for the time period after the start of a policy event and "0" for the time period before. Additionally, because smaller counties tend to skew employment and health outcomes, I weight this regression by log county population.

Our DID model allows us to isolate the effect of a trade policy event by examining whether a relationship between employment outcomes and AAR exists and when any such relationship first occurs. By calculating the effect of shifting a county's AAR score from the negative portion of our data to a similarly positive score–a 2 standard deviation shift–we directly compare changes in employment and health outcomes for the trade policy "losers" relative to the "winners".

As I show in the next section, we validate our DID estimates by demonstrating parallel trends in the period prior to treatment. Furthermore, because our AAR scores are calculated using employment counts prior to any anticipatory labor reallocation, reverse causality is not a concern.

6.2 PNTR and Employment

I take our regression model from equation (16) to provide baseline estimates for the relative difference in labor market and health outcomes between negatively and positively impacted counties before and after PNTR. Additionally, results that are similar to Pierce and Schott (2020) should further validate our measure for trade exposure and delineate the counties that we are interested in "unshocking". I create event study plots to better visualize parallel trends, the timing of any noticeable policy impact, and the lagging effects of PNTR.

AAR vs Unemployment Rate (PNTR)

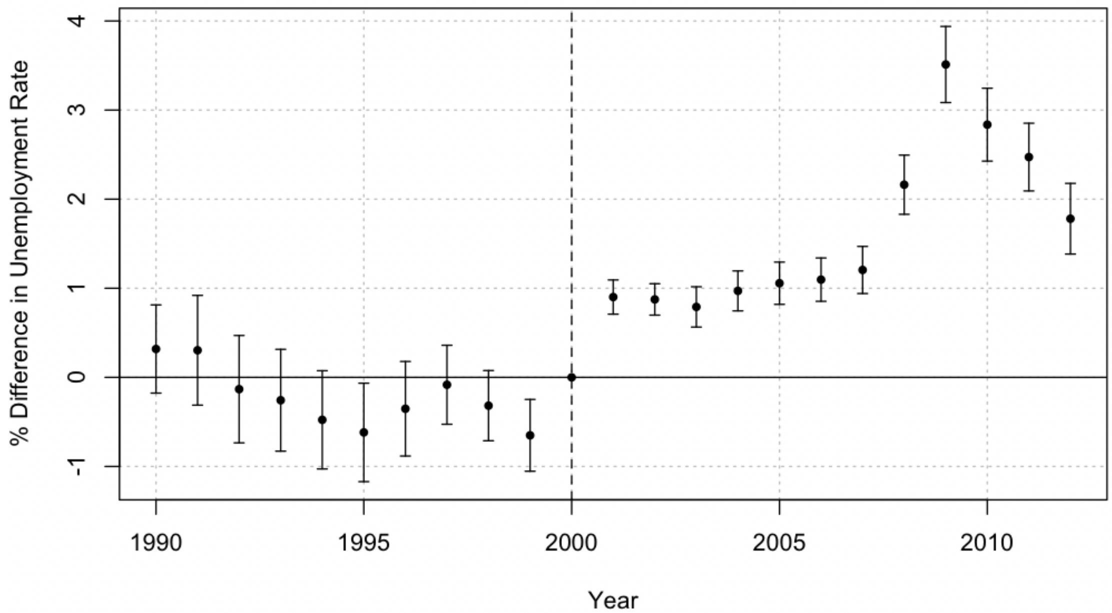

Figure 4: Relative to positively impacted counties, negatively impacted counties see a sharp rise in unemployment immediately following PNTR

AAR vs Labor Force Participation Rate (PNTR)

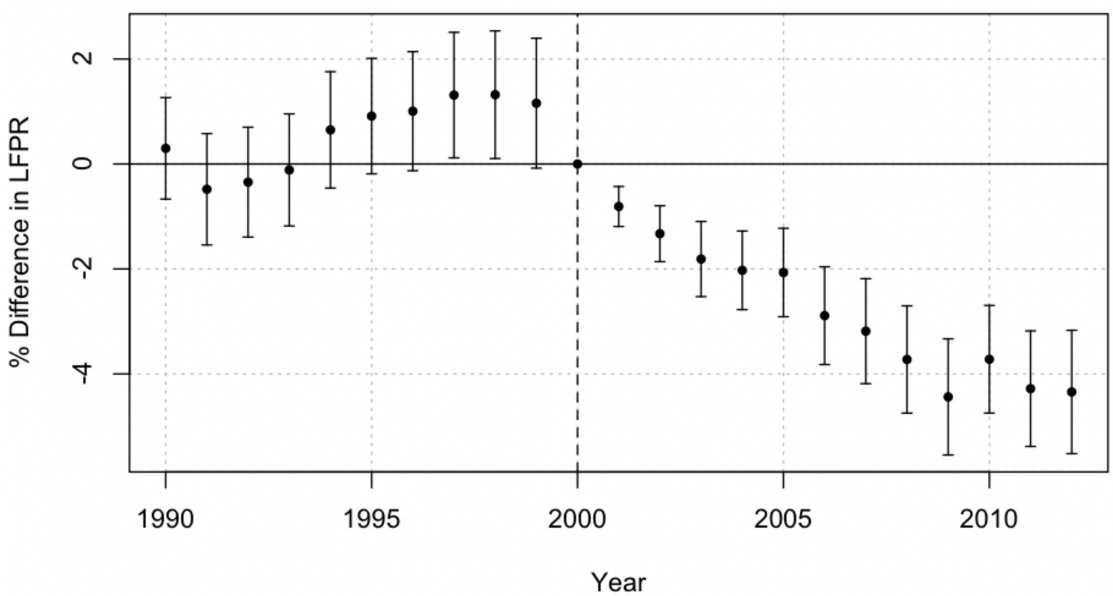

Figure 5: Relative to positively impacted counties, negatively impacted counties see a steady decline in labor force participation following PNTR

Figure 4 illustrates the relationship between AAR and unemployment rate over time, giving point estimates and standard errors for the difference in unemployment rates between our treatment groups. We see a slight downward trend in the pre-treatment period followed by a sharp increase in unemployment rate immediately after PNTR. The difference in unemployment rate remains relatively stable until 2007, and in 2008, presumably due to the Great Recession, our differences drastically increase before coming back to the pre-recession levels. Because confounding effects stemming the recession fall outside the scope of this paper, we remove the years 2008-2012 before calculating our baseline estimates. Furthermore, assuming the pre-treatment downward trend would have continued, we may be underestimating the true effect of PNTR on unemployment. This trend also suggests that labor conditions prior to PNTR were improving for negatively impacted counties relative to positively impacted counties.

Similar to unemployment, Figure 5 shows the relationship between AAR and labor force participation rate. The negative pre-treatment trend in unemployment is reflected in a slight positive increase in pre-treatment labor force participation. Additionally, the difference in labor force participation rate starts decreasing immediately prior to PNTR, suggesting that workers anticipating the legislation had already started exiting to workforce. Following PNTR, the event study plot highlights a steady decrease in labor force participation rate through 2012.

Table 2 provides our DID model estimates for labor market conditions during PNTR. These estimates are similar to Pierce and Schott (2020), and they demonstrate that PNTR had a significant adverse impact on labor market outcomes for counties with negative AAR.

6.3 PNTR and Deaths of Despair

I repeat the same procedure as above for deaths of despair. Suicide and alcohol-related liver disease results appear to be unaffected by PNTR as referenced by Figure 6. Drug overdoses, for which Pierce and Schott (2020) find a strong positive relationship between AAR and

Table 2: Labor market estimates for PNTR

	Dependent variable:	
	Unemployment	Labor Force Participation
	(1)	(2)
AAR x post	1.186^{***}	-3.448^{***}
	(0.137)	(0.381)
Observations	12,880	16,454
Mean Value	6.95	64.07
Adjusted R^2	0.087	0.031
Residual Std. Error	8.519 (df = 12874)	27.444 (df = 16448)
F Statistic	246.287^{***} (df = 5; 12874)	107.606^{***} (df = 5; 16448)
Note:		*p<0.1; **p<0.05; ***p<0.01

mortality, yield mixed results. Here, I split treatment groups into negatively affected vs unaffected counties and positively affected vs unaffected counties. This is necessary because, referencing Figures 7 and 8, unlike our previous dependent variables, both negative and positively affected counties see a drastic increase in drug overdose deaths relative to unaffected counties.

Unlike the findings from Pierce and Schott (2020), these results do not demonstrate a causal relationship between AAR and drug overdoses. While we expect drug overdoses in negatively impacted counties to increase relative to unaffected or positively affected counties, we find that, relative to unaffected counties, positively affected counties actually see drug overdoses increase almost five times the magnitude of negatively affected counties.

This most likely indicates that the rise in drug overdoses during the PNTR period is more coincidental than causal. If anything, our findings reveal that counties which are in any way impacted by trade will see changes in drug overdoses. As such, I do not test to see if deaths of despair get “unshocked” by the trade war.

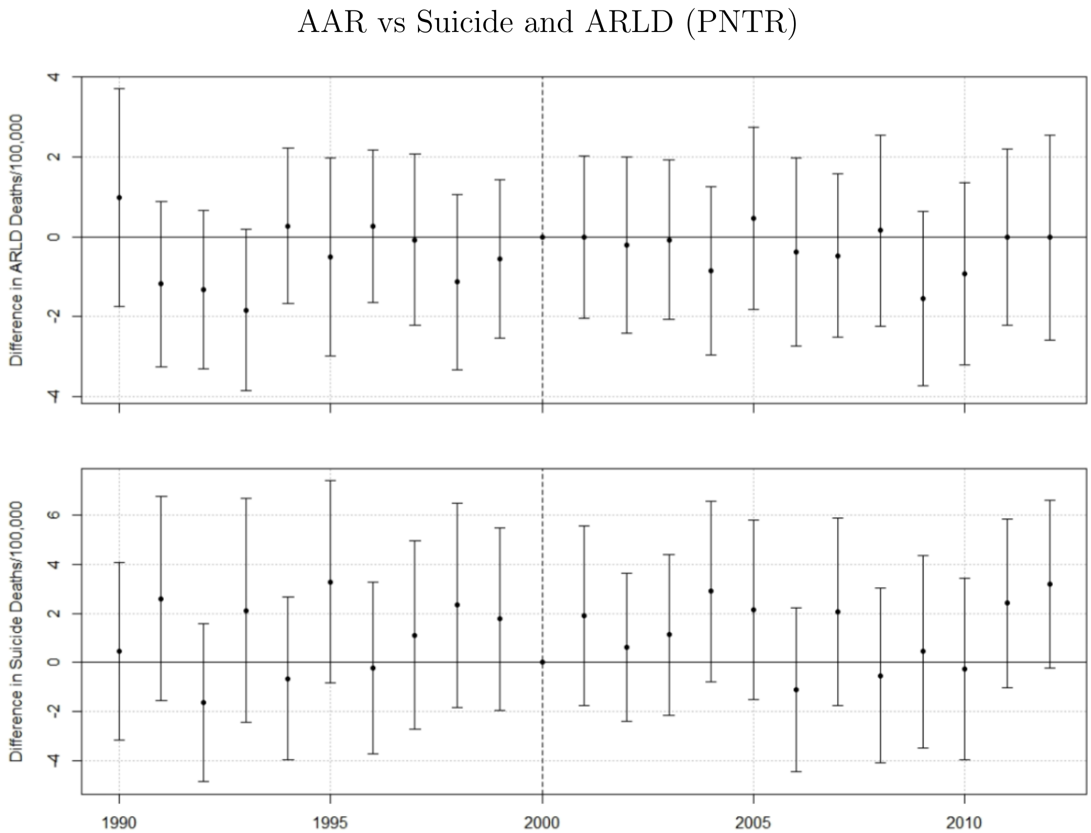

Figure 6: ARLD and Suicide rates are unaffected by PNTR

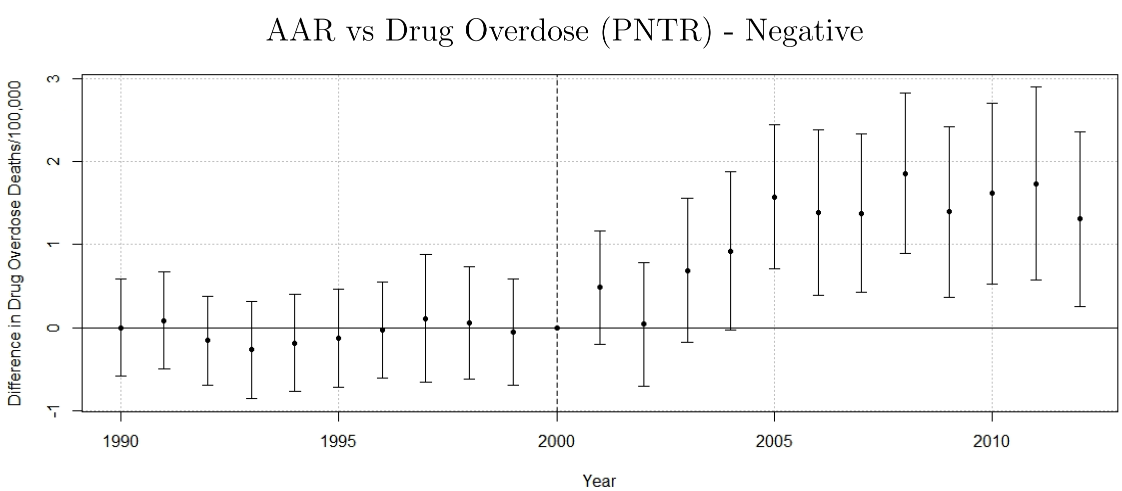

Figure 7: Compared to unaffected counties, negatively impacted counties see a significant increase in drug overdoses

AAR vs Drug Overdose (PNTR) - Positive

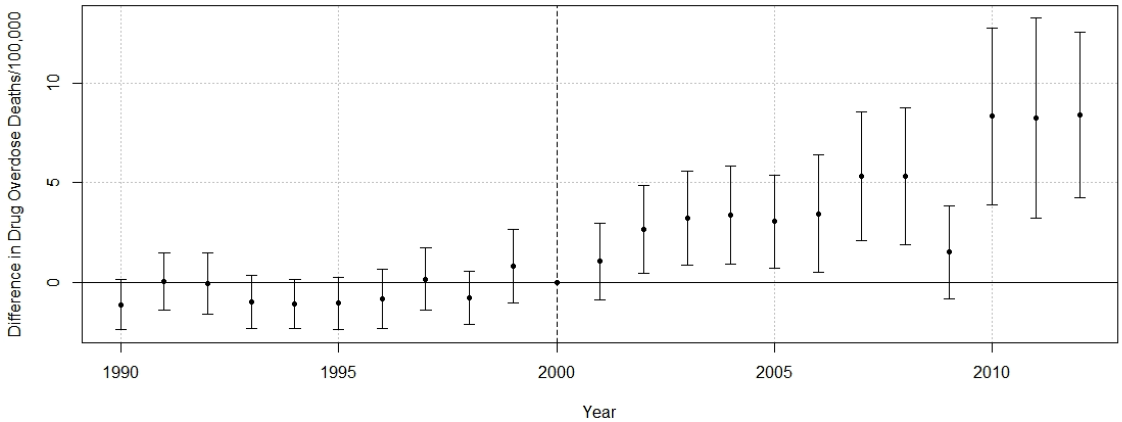

Figure 8: Positively impacted counties see a significant increase in drug overdoses relative to unaffected counties at a magnitude of around 5 times greater than negatively impacted counties

6.4 "Unshocking" Labor Market Conditions

Having demonstrated a strong relationship between AAR and unemployment during PNTR, this section examines if negatively affected counties are "unshocked" by Trump's trade war. Because policy aimed towards improving labor market conditions is industry focused and not county focused (i.e. motivation for the trade war most likely sought to improve employment outcomes for the manufacturing industry, not Davidson County), I use industry-level AAR scores to filter counties. Taking our industry-level AAR_j^{PNTR} measure, I subset for counties with at least 20 percent employment in an industry with negative score. Among these counties, I then split my data into two groups. The first group includes counties with at least 20 percent employment in an industry that is $AAR_j^{TradeWar}$ negative–which I label "1"–and the second group includes counties with at least 20 percent employment in an industry that is $AAR_j^{TradeWar}$ positive–which I label "0".

To better conceptualize this method, we expect that, because both treatment groups are AAR_j^{PNTR} negative, equation (16) during PNTR will not yield significant results. Our event study plot should show a flat line. This is our "first stage" analysis which is essential for demonstrating that counties in separate treatment groups are not fundamentally different during PNTR.

For the "second stage" trade war, we expect to see a negative relationship between AAR and unemployment. Similar to AAR^{PNTR}, we assume $AAR^{TradeWar}$ captures the expected economic impact of the trade war shock. If the estimate is at least as negative as the increase in unemployment from PNTR, we say these industries are "unshocked".

"Unshocked" vs Unaffected (PNTR)

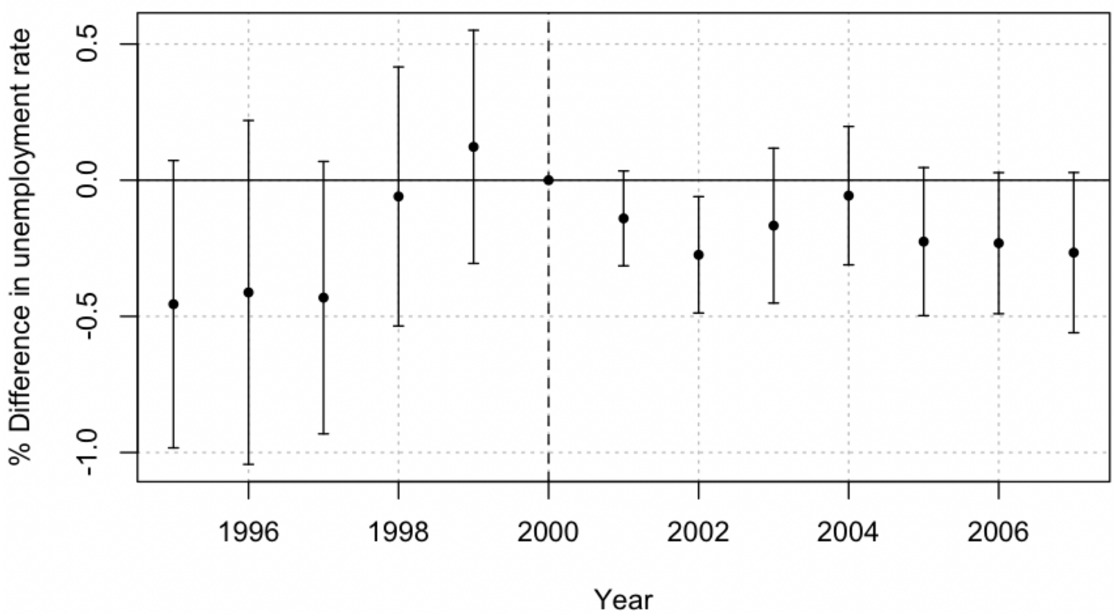

Figure 9: As expected, there is no noticeable difference in unemployment rates before and after PNTR

While Figure 9 illustrates that our "first stage" results follow as expected, Figure 10 shows that deteriorating labor market outcomes do not get unshocked. There is no significant decrease in unemployment rates for trade war negative counties relative to trade war positive counties. Counties with an employment share concentrated in industries that should have benefitted from the trade war did not see those benefits materialize. Therefore, we conclude that the 2018 trade war was unsuccessful in reversing outcomes for counties negatively affected by PNTR.

Table 3 gives estimates for both stages, neither of which yield significant results. To further confirm that the trade war failed to "unshock" counties negatively affected by PNTR, "Unshocked" vs Unaffected (Trade War)

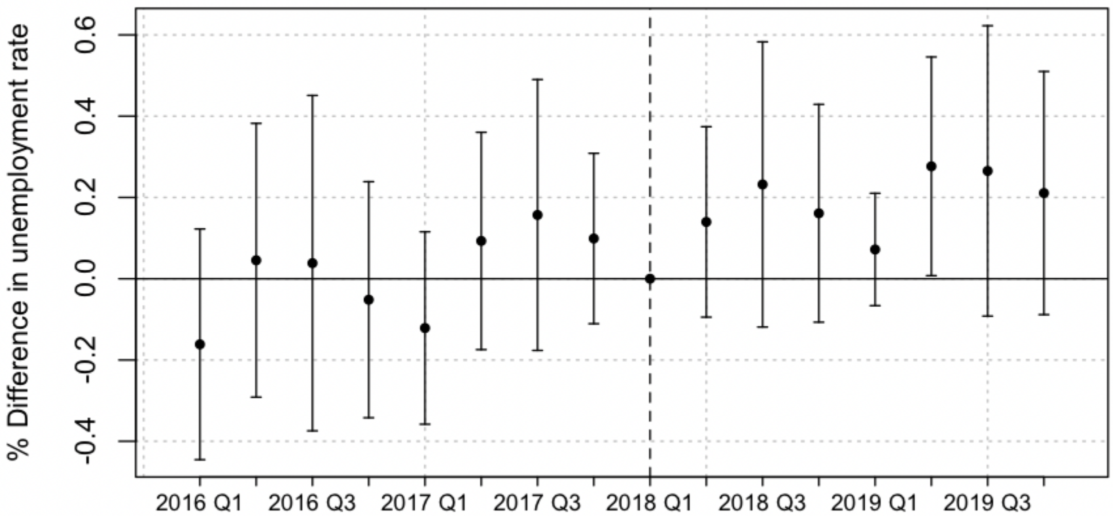

Figure 10: There does not appear to be a significant decrease in unemployment

Table 3: Unemployment estimates for "unshocking" stages

	Dependent variable:	
	Unemployment Rate	
	(PNTR)	(Trade War)
unshocked x post	0.033	0.154
	(0.180)	(0.145)
Observations	5,783	7,136
Mean	5.30	4.62
Adjusted R^2	0.036	0.095
Residual Std. Error	8.165 (df = 5777)	6.305 (df = 7130)
F Statistic	43.630*** (df = 5; 5777)	150.433*** (df = 5; 7130)
Note:		*p<0.1; **p<0.05; ***p<0.01

we compare unemployment estimates for PNTR and the trade war using 95% confidence intervals. From Table 1, the interval for our PNTR unemployment *increase* estimate is (0.92, 1.45), and using Table 3, the interval for our trade war unemployment *decrease* estimate is (-0.39, 0.32). These intervals do not overlap, and we can confidently reject that the trade war was able to "unshock" adverse labor market effects stemming from PNTR.

7 Conclusion

Our DID regression estimates indicate that the 2018 trade war failed to reverse the declining unemployment trends caused by PNTR, demonstrating the difficulty faced by protectionist legislation that aims to "unshock" previous trade liberalization and emphasizing that counties damaged by trade were, as of one year after the trade war, unable to recover.

These results suggest that the labor re-adjustments expected to occur never materialized. Counties that were negatively impacted by PNTR saw a subsequent outward migration as workers sought employment in counties comprised of industries unaffected by trade. Following the announcement of Trump's trade war, incentives for workers to return to their pre-PNTR jobs either were not attractive enough or the immediate creation of these pre-PNTR manufacturing jobs did not transpire as expected–manufacturing plants that closed down because of trade liberalization were not reopened at a rate that would have significantly affected unemployment trends.

Our results may also speak to the sentiment of larger US multinational corporations which control a significant portion of the stock market. Following PNTR, select publicly traded US firms were perhaps more profitable *because* they exploited cheaper Chinese labor. As such, trade restrictions that required US companies to reintroduce domestic labor were likely met with negative investor sentiment. Here, our assumption that the stock market accurately reflects measures of US economic welfare may be misguided, as the growing disjoint between stock prices and labor market outcomes could explain the null trade war DID estimates.

Figure A.1 provides support for this theory, as many of the negatively affected regions during PNTR in Figure 3 are again red during the trade war. For future research, I would need access to firm-level stock data, which would allow me to filter out such companies.

Similarly, because our measure for trade exposure only considers publicly traded firms, the industry scores for smaller, rural counties which are primarily comprised of small, privately owned business may not be accurately represented.

A final limitation for our study is the insufficient post-treatment time period. Because of the beginning of the COVID-19 pandemic in 2020 and our short time window in writing this paper, we do not know if changes in post-trade war employment trends require more time to materialize. While PNTR yielded immediate results for unemployment, it is difficult to fully assess employment outcomes for the trade war without a longer post-treatment study period. As such, we cannot yet conclude that trade labor displacement creates permanent consequences for negatively affected counties.

Continuation of this research could focus on other instances of reverse legislation. Specifically, given the two-party structure of the United States government and the incessant back and forth nature of legislation, more localized research could examine the relationship between changes in party control and county or state level outcomes. For policy measures that disproportionately harm select groups of people, future contributions to this type of literature could determine the conditions necessary for successful reverse legislation.

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Appendix

Date	Event
March 1, 2018	Trump announces a series of steel and aluminum tariffs
March 22, 2018	Trump announcement that the U.S. was proposing tariffs on a large fraction of Chinese imports
April 2, 2018	Chinese announces retaliation on 128 categories of U.S. exports
June 15, 2018	China announces retaliation against additional $50 billion of U.S. exports
July 5, 2018	Mexican announcement that they were going to retaliate in response to the steel and aluminum tariffs
July 25, 2018	European Union announces retaliatory tariffs on $20 billion of U.S. exports
September 17, 2018	US announcement of tariffs on $200 billion of Chinese imports

Table A.1: Events selected following Amiti, Redding, and Weinstein (2020)

Date	Event
May 15, 2000	Introduction of the PNTR bill in the US House of Representatives
May 24, 2000	Vote to approve PNTR in the House
July 27, 2000	Successful cloture motion to proceed with a vote on PNTR in the US Senate
September 19, 2000	Vote to approve PNTR by the Senate
October 10, 2000	Signature of PNTR into law by President Clinton

Table A.2: Events selected following Greenland et al. (2020)

Age	Population	Ratio
1-4 years	56092	0.0561
5-9 years	73549	0.0735
10-15 years	74055	0.0741
15-19 years	73180	0.0732
20-15 years	67410	0.0674
25-19 years	65433	0.0654
30-15 years	72039	0.0720
35-19 years	81894	0.0819
40-15 years	82998	0.0830
45-19 years	73129	0.0731
50-15 years	63595	0.0636
55-19 years	49133	0.0491
60-15 years	39337	0.0393
65-19 years	34744	0.0347
70-15 years	32218	0.0322
75-19 years	27377	0.0273
80-15 years	18091	0.0181
85 and over	15725	0.0157
Total	1000000	1.0000

Table A.3: 2000 US standard million population for 18 age groups

Code	Industry	Code	Industry
1	Agriculture	26	Defense
2	Food Products	27	Precious Metals
3	Candy & Soda	28	Non-Metallic and Industrial Metal Mining
4	Beer & Liquor	29	Coal
5	Tobacco Products	30	Petroleum and Natural Gas
6	Recreation	31	Utilities
7	Entertainment	32	Communication
8	Printing and Publishing	33	Personal Services
9	Consumer Goods	34	Business Services
10	Apparel	35	Computers
11	Healthcare	36	Computer Software
12	Medical Equipment	37	Electronic Equipment
13	Pharmaceutical Products	38	Measuring and Control Equipment
14	Chemicals	39	Business Supplies
15	Rubber and Plastic Products	40	Shipping Containers
16	Textiles	41	Transportation
17	Construction Materials	42	Wholesale
18	Construction	43	Retail
19	Steel Works Etc	44	Restaurants, Hotels, Motels
20	Fabricated Products	45	Banking
21	Machinery	46	Insurance
22	Electrical Equipment	47	Real Estate
23	Automobiles and Trucks	48	Trading
24	Aircraft	49	Other
25	Shipbuilding, Railroad Equipment		

Table A.4: Fama French 49 Industry Definitions

AAR distribution map for the trade war

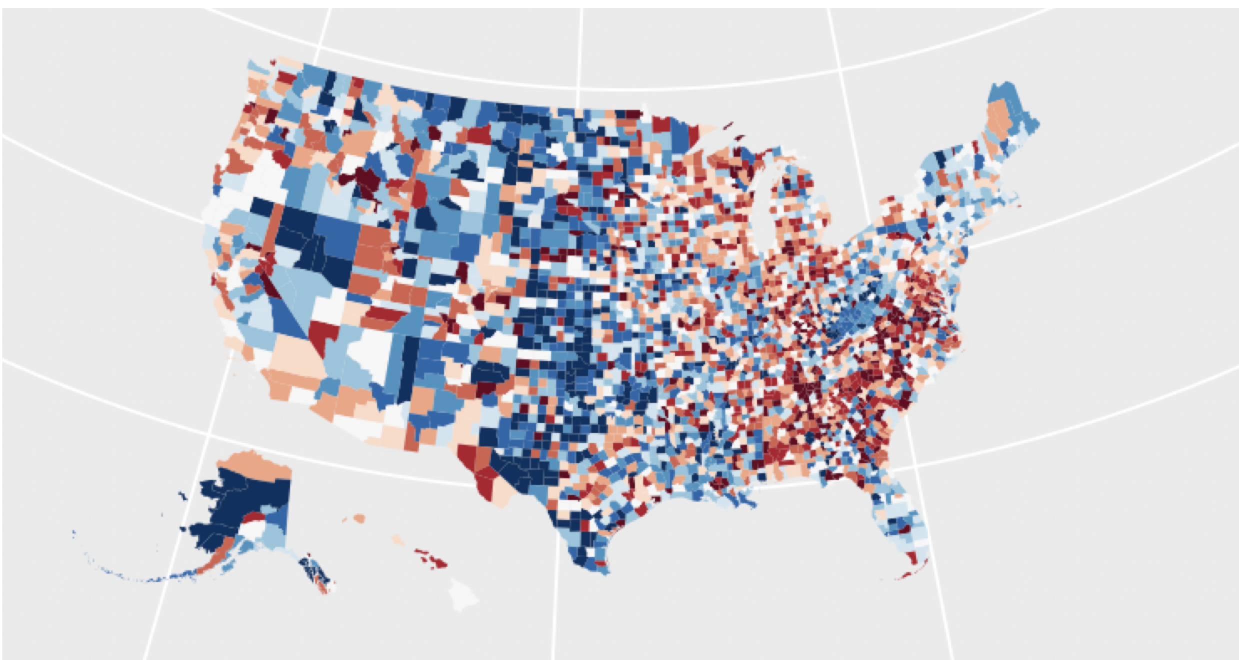

Figure A.1: Compare affected regions with Figure 3