

Trade Liberalization and Mortality: Evidence from U.S. Counties*

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Abstract

We investigate the impact of a large and persistent economic shock on “deaths of despair.” We find that areas more exposed to a plausibly exogenous change in international trade policy exhibit relative increases in fatal drug overdoses, specifically among whites. We show that these results are not driven by pre-existing trends in mortality rates, that the estimated relationships are robust to controls for state-level legislation pertaining to opioid availability and health care, and that the impact of the policy change on mortality coincides with a deterioration in labor market conditions and uptake of disability insurance.

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1 Introduction

Recent research by Case and Deaton (2015) suggests an alarming rise in “deaths of despair” – drug overdose, suicide and diseases of the liver – among middle-aged whites in the United States. Identifying potential contributors to this increase is an important topic for researchers across a broad range of disciplines, with Case and Deaton (2017) arguing that the trend may be driven by a combination of negative social and economic outcomes that accumulate over time. Though large literatures in economics and public health examine the effect of economic shocks on health and mortality, finding exogenous sources of variation in economic conditions remains an important challenge. In this paper, we document a link between deaths of despair, particularly from drug overdose, and a large, plausibly exogenous shock to local labor markets driven by a change in US trade policy.

In October, 2000, the United States Congress passed a bill granting Permanent Normal Trade Relations (PNTR) to China, a trade liberalization that differentially exposed US regions to increased import competition via their industry structure. While US imports from China had already been subject to the low “normal trade relations (NTR)” tariff rates available to most US trading partners before PNTR, continued access to these rates was subject to annual renewal by the US President and Congress. Absent these renewals, US tariffs on most Chinese imports would have risen abruptly to the non-NTR rates set by the Smoot-Hawley Tariff Act of 1930. Before passage of PNTR, the possibility of these dramatic tariff increases created a disincentive for US firms considering sourcing goods from China, and Chinese firms considering expansions into the United States. PNTR eliminated the need for annual renewals, rendering production in China for export to the United States more attractive and thereby increasing import competition for US producers.¹

We refer to an industry’s exposure to PNTR as the “NTR gap” and define it as the difference between the higher, non-NTR rates to which tariffs could have risen prior to PNTR and the lower NTR rates that were locked in by the change in policy. As is clear from this definition, a higher NTR gap indicates that an industry had been facing larger potential tariff increases before PNTR, and therefore experienced a larger trade liberalization after its passage. Importantly for our identification strategy, we show that NTR gaps exhibit substantial variation across industries, and that they are unrelated to mortality and employment outcomes prior to the change in policy. Furthermore, nearly all of the variation in the NTR gap is accounted for by variation in non-NTR rates, which were set in 1930, implying that NTR gaps did not respond to changes in current economic conditions. We then calculate county-level exposure to the policy change using the labor-share-weighted-average NTR gaps of the industries active within their borders in 1990.

Using proprietary microdata from the US Centers for Disease Control (CDC), we compute mortality rates for various causes of death by gender, race, age group and county for

¹Pierce and Schott (2016a) find that US industries with greater exposure to PNTR exhibit relative reductions in manufacturing employment and relative increases in imports from China and firms engaged in US-China trade. Handley and Limao (2017) find that PNTR accounts for one-third of the growth in Chinese exports to the US between 2000 and 2005. Earlier research by Autor et al. (2013) find that regions more exposed to Chinese import competition experience relatively large declines in employment and greater uptake of social welfare programs. Autor et al. (2014) show that workers more exposed to Chinese imports exhibit a relative decline in earnings.

1990 to 2013. We then employ a generalized difference-in-differences (DID) identification strategy to examine whether counties that are more exposed to PNTR (first difference) experience differential changes in mortality and labor market outcomes after the policy is implemented (second difference). Our baseline specification includes controls for counties' initial demographic and economic attributes, including the initial share of employment in manufacturing, policy changes in China, and fixed effects that capture time-invariant characteristics of counties and aggregate shocks that affect all counties in a particular year.

We find that counties more exposed to the change in US trade policy exhibit relative increases in deaths of despair. We show that these increases occur at the time of the policy change, and consistent with the dramatic rise in mortality among middle-aged Americans reported in Case and Deaton (2015), we find these effects are present primarily among working-age whites. Coefficient estimates imply that an interquartile shift in counties' exposure to PNTR is associated with a relative increase in mortality from overall deaths of despair of 2 to 4 per 100,000, or 10 to 20 percent of the average mortality rate from these causes across counties in 2000, the year of the change in US trade policy. Within deaths of despair, we find that the link between PNTR and mortality is driven by drug overdoses. In this case, an interquartile shift in exposure is also associated with a relative increase of 2 to 4 per 100,000, a sizable share of the average 5 per 100,000 average death rate across counties in 2000. As these magnitudes imply, we find little relationship between PNTR and mortality from either suicide or ARLD, though we do find pre-existing links between those causes of death and both initial employment concentration in manufacturing and the initial share of veterans in the population.

We show that our findings are robust to the inclusion of controls for other potential contributors to changing mortality rates, and perform several exercises to place the results in a broader context. In addition, we find that results are similar when the analysis is conducted at a higher level of geographic aggregation, and that there is no relationship between PNTR and other causes of death, such as cancer, which are arguably less likely to respond quickly to a severe shock to the local labor market.

Our analysis contributes to several important literatures. First, it relates to an emerging body of research on the economics of deaths of despair, including Case and Deaton (2017)'s hypothesis that "cumulative disadvantage" in the labor market for less-educated workers may lie behind the increase in middle-aged whites' mortality. Indeed, while employment opportunities for lower-skilled workers have been declining for some time (Autor et al. (2003); Jaimovich and Siu (2012)), PNTR may have served as a catalyst for increasing mortality rates for at least two reasons. First, because PNTR was a change in policy, its effects were abrupt, potentially exacerbating labor market disruption by requiring the reallocation of a large number of workers in a short amount of time. Second, unlike the cyclical declines in employment studied elsewhere in the literature, the labor market effects of PNTR are long-lasting, with counties more exposed to the policy change exhibiting relatively elevated unemployment rates well into the 2000s.

Second, the link we find between PNTR and mortality relates to a series of papers studying the health and mortality consequences of unemployment. Two seminal contributions in this literature are Ruhm (2000), which reports a positive relationship between the unemployment rate and suicide in a panel of US states, and Sullivan and von Wachter (2009), which finds that high-tenure workers displaced as part of a mass layoff experience a sharp increase

in their probability of death. More recently, Browning and Heinesen (2012) and Classen and Dunn (2012) find that unemployment duration is a major force in the relationship between job loss and suicide, while Hollingsworth et al. (2017) find that macroeconomic shocks at the county or state-level are associated with increases in deaths and emergency room visits due to opioid overdoses. Our contribution to this literature is to exploit a plausibly exogenous change in policy for identification.

Finally, in the international trade literature, our analysis adds to a growing body of research finding links between import competition and an array of socioeconomic outcomes, including self-reported health assessments (McManus and Schaur (2015, 2016)), the provision of local public goods (Feler and Senses (2017)), marriage and fertility (Autor et al. (2017)) and innovation (Bloom et al. (2016); Autor et al. (2016)). Here, our results contribute to a broader understanding of the distributional implications of trade liberalization by focusing on an outcome – health – that has received relatively little attention in the trade literature.²

The paper proceeds as follows: Section 2 describes the data, Section 3 presents our main results, Section 4 provides robustness checks, and Section 5 discusses mechanisms. Section 6 concludes. An online appendix provides additional empirical results and dataset details.

2 Data

2.1 Mortality Rates across Counties

We calculate the number of deaths by county, demographic category and cause using the proprietary data from the US Centers for Disease Control’s National Center for Health Statistics. These data provide information from all death certificates filed in the United States from 1990 to 2013. Observable demographics include the deceased’s age, gender, race and county of residence. As discussed in greater detail in the online appendix, causes of death are classified according to one of several hundred “external” or “internal” categories according to whether they originate within (e.g., liver disease) or outside (e.g., suicide or drug overdose) the body.

We match year by county by age by gender by race death counts to corresponding population estimates compiled by the National Cancer Institute’s Surveillance, Epidemiology and End Results (SEER) Program.³ We use these population estimates to compute both “crude” and “age-adjusted” mortality rates, conventionally expressed per 100,000 population. The crude death rate for a county is simply the total number of deaths in that county in that year divided by its total population in that year. We follow the standard approach in the literature and calculate the age-adjusted death rate for a county as the weighted average of the crude death rates across age categories within a county, where the shares of the total US population

²Following the research presented in an earlier version of this paper (Pierce and Schott (2016b)), both Autor et al. (2017) and Adda and Fawaz (2017) report relationships between Chinese import competition and mortality. Hummels et al. (2016) find that increased effort associated with positive export demand shocks is associated with higher rates of illness and injury among Danish workers, while Bombardini and Li (2016) show that higher pollution associated with expanded export production is related to a substantial increase in Chinese infant mortality.

³SEER population estimates are available at <http://seer.cancer.gov/popdata/download.html>.

in each age category are used as weights.⁴ We use US population shares in the year 2000 (reported in online appendix Table A.1) for constructing age-adjusted mortality rates. Across counties, the population weighted average mortality rates (and standard deviations) for drug overdose, suicide, ARLD and overall deaths of despair are 5(4), 11(4), 4(3) and 20(7).

2.2 Measuring Exposure to PNTR

Our measure of exposure to PNTR is based on two sets of tariff rates in the US tariff schedule. The first, known as NTR tariffs, are generally low and apply to goods imported from members of the World Trade Organization (WTO). The second, known as non-NTR tariffs, were set by the Smoot-Hawley Tariff Act of 1930 and are often substantially larger than the corresponding NTR rates. Imports from non-market economies, such as China, are by default subject to the higher non-NTR rates, but US law allows the President to grant such countries temporary annual access to NTR rates subject to approval by Congress.

US Presidents began granting China this temporary access to NTR tariff rates in 1980. Initially uncontroversial, annual renewal became politically contentious and less certain of approval following the Chinese government’s crackdown on Tiananmen Square protests in 1989 and other flashpoints in US-China relations during the 1990s. Indeed, the US House of Representatives passed resolutions to end China’s NTR status in 1990, 1991 and 1992. Because the Senate failed to act on these votes, China’s temporary NTR status remained in place.

The possibility that China’s NTR status would be withdrawn – and that tariffs would increase – created a disincentive for US-China trade. According to a US General Accounting Office report, US firms doing business in China “cited uncertainty surrounding the annual renewal of China’s most-favored-nation trade status as the single most important issue affecting US trade relations to China,” while a 1993 letter signed by the CEOs of 340 firms including General Motors, Boeing, and Caterpillar noted that “the persistent threat of MFN withdrawal does little more than create an unstable and excessively risky environment for US companies considering trade and investment in China, and leaves China’s booming economy to our competitors (Rowley (1993)).”⁵ These disincentives disappeared in October, 2000 when Congress passed a bill granting permanent NTR (i.e., PNTR) status to China, eliminating the need for annual NTR renewals effective upon China’s entry into the WTO in December 2001.⁶

We follow Pierce and Schott (2016a) in measuring the impact of PNTR as the rise in US tariffs on Chinese goods that would have occurred in the event of a failed annual renewal of China’s NTR status prior to PNTR,

$$NTR\text{Gap}_j = NonNTRRate_j - NTRRate_j. \quad (1)$$

⁴We use the following age categories in our baseline results: less than 1 year old, 1 to 4 years, 5 to 14 years, 15 to 19 years, 20 to 24 years..., 80 to 84 years, and greater than 85 years. We note that we find similar results if we restrict the analysis to age bins that capture the working-age population, i.e., age bins between 20 and 64.

⁵Further anecdotal evidence is provided in Pierce and Schott (2016a).

⁶We control for other policy changes enacted by China and the United States as part of China’s accession to the WTO – such as changes in Chinese import tariffs and production subsidies – to isolate the effect due specifically to PNTR.

We refer to this difference as the NTR gap, and compute it for each SIC industry j using *ad valorem* equivalent tariff rates provided by Feenstra et al. (2002) for 1999, the year before passage of PNTR. Larger NTR gaps indicate that an industry’s output had been subject to larger tariff increases—and greater disincentives to locating production in China—prior to PNTR, and therefore to a larger trade liberalization after PNTR. NTR gaps vary widely across industries, with a mean and standard deviation of 30 and 18 percentage points, respectively. As noted in Pierce and Schott (2016a), 79 percent of the variation in the NTR gap across industries is due to variation in non-NTR rates, set 70 years prior to passage of PNTR, while less than 1 percent of variation is due to variation in NTR rates. This feature of non-NTR rates effectively rules out reverse causality that would arise if non-NTR rates were set to protect industries with declining employment or surging imports. Furthermore, to the extent that NTR rates were set to protect industries with declining employment prior to PNTR, these higher NTR rates would result in lower NTR gaps, biasing our results away from finding an effect of PNTR.

We compute county-level exposure to PNTR as the employment-share-weighted-average NTR gap across the four-digit SIC industries active in the county,

$$NTR\ Gap_c = \sum_j \frac{L_{jc}^{1990}}{L_c^{1990}} NTR\ Gap_j, \quad (2)$$

where c indexes counties, j indexes years and L represents employment. We use employment shares from 1990, ten years before the change in policy.⁷ NTR gaps are defined only for industries whose output is subject to US import tariffs, primarily in the manufacturing and agricultural sectors. Industries whose output is not subject to tariffs, such as service industries, have NTR gaps of zero by definition. Across counties, the NTR gap averages 7.2 percent and has a standard deviation of 6.5 percent, with an interquartile range from 2.2 to 10.5 percent.

2.3 Other Control Variables

Our baseline specification controls for several changes in US or Chinese policy: the average US import NTR tariff associated with the goods produced by each county; the average exposure of the county to the end of quantitative restrictions on textiles and clothing imports associated with the phasing out of the global Multi-Fiber Arrangement (MFA); and average changes in Chinese import tariffs and domestic production subsidies.

We also control for initial (1990) values of several county demographic attributes: median household income, as a proxy for access to healthcare; share of population without a college degree, as a proxy for counties more susceptible to the introduction of labor-saving technical change; the share of population that are veterans, as a proxy for economic opportunity and as a control for a group susceptible to deaths of despair; and the share of employment in manufacturing, which accounts for the various ways in which industrial counties might

⁷Employment by county-industry are available from the US County Business Patterns database at <http://www.census.gov/econ/cbp/download/>. We follow Autor et al. (2013) in imputing employment for cells where only a range of employment is reported.

vary from those whose activity is predominantly in other sectors. Each of these variables is discussed in detail in the online appendix.

3 PNTR and Mortality Rates

This section examines the link between PNTR and deaths of despair, which, for purposes of this paper, include suicide, drug overdose and alcohol-related liver disease (ARLD). We focus on these causes of death for several reasons: they account for a substantial portion of the increase in mortality rates for middle-aged whites highlighted in Case and Deaton (2015); there is an established link between these causes of death and job loss (Classen and Dunn (2012), Browning and Heinesen (2012)); their concordance across the cause-of-death coding schemes used by the CDC over time is straightforward; and they may be more easily observable than other forms of death, particularly in the case of suicide and drug overdose.⁸

3.1 Identification Strategy

Our baseline difference-in-differences (DID) specification examines whether counties with higher NTR gaps experience differential changes in mortality after the change in US trade policy versus before,

$$\begin{aligned}
 DeathRate_{ct} = & \sum_t \theta_t 1\{year = t\} \times NTRGap_c + \\
 & \beta \mathbf{X}_{ct} + \\
 & \sum_t \gamma_t 1\{year = t\} \times \mathbf{X}_c + \\
 & \delta_c + \delta_t + \varepsilon_{ct},
 \end{aligned} \tag{3}$$

The left-hand side variable represents the age-adjusted death rate for a particular cause of death for county c in year t . The first set of terms on the right-hand side are the DID terms of interest, interactions of a full set of year dummies (excluding 1990) with the (time-invariant) county-level NTR gap. This specification allows us to examine whether there is a relationship between mortality rates and the NTR gap, and also allows us to determine when any relationship is first observed. \mathbf{X}_{ct} represents the time-varying controls for policy discussed in Section 2.3: the overall US import tariff rate associated with the industries active in the county (NTR_{ct}) and the sensitivity of the county to the phasing out of the global Multi-Fiber Arrangement ($MFA Exposure_{ct}$). \mathbf{X}_c represents the two time-invariant Chinese policy variables – exposure to changes in Chinese tariffs and exposure to changes in Chinese domestic production subsidies – and the four initial (1990) county attributes discussed above: median household income, share of population without a college degree, share of population that are veterans and the share of employment in manufacturing. Including interactions of these attributes with the full set of year dummies allows their relationship with mortality rates to

⁸While listed causes of death are noisy (Schottenfeld et al. (1983)), this problem is likely less severe for deaths of despair given the higher scrutiny they attract.

differ before and after passage of PNTR. δ_c and δ_t represent county and year fixed effects, which net out characteristics of counties that are time-invariant—such as whether they are near the coast or inland—as well as aggregate shocks that affect all counties identically in a particular year. Standard errors are clustered at the state level, allowing for correlation of errors across counties within states, and therefore yielding conservative estimates of statistical significance. The sample period is 1990 to 2013.

An attractive feature of this DID identification strategy is its ability to isolate the role of the change in US trade policy. While counties with high and low NTR gaps are not identical, comparing outcomes within counties over time isolates the differential impact of China’s change in NTR status.

3.2 Baseline Estimates

Given the large number of coefficient estimates in Equation 3, we summarize our results visually.⁹ Toward that end, we use the estimates of the DID terms of interest (θ_t) to calculate the effect of shifting a county from the 25th percentile to the 75th percentile in terms of exposure to PNTR. We calculate this effect by multiplying the DID coefficients by 8.3 percentage points, the magnitude of an interquartile shift in county exposure to PNTR. The four panels of Figure 1 report the 95 percent confidence intervals of these estimates for each death of despair as well as for all three deaths of despair as a group.

As indicated in the first panel of the figure, we find that in the period prior to the policy change in 2000, the confidence interval for the impact of an interquartile shift in NTR gap on drug overdose mortality is moving sideways and is statistically indistinguishable from zero. This lack of a pre-existing trend in counties that are more exposed to PNTR offers support for our DID strategy. By contrast, after passage of PNTR, the confidence interval shifts up noticeably and becomes statistically different from zero, indicating that counties more exposed to the policy change experience increases in drug overdose deaths relative to those that are less exposed. Estimates in the first panel reveal that an interquartile shift in exposure to PNTR is associated with a relative increase in the mortality rate from drug overdoses of approximately 2 to 5 deaths per 100,000 of population in each year after the policy, a sizable share of the 5 deaths per 100,000 average mortality rate for drug overdose across counties in 2000.¹⁰

Results in the middle two panels of the first column of Figure 1 show that we find no evidence of a relationship between PNTR and mortality from either suicide or ARLD. Coefficient estimates for the DID terms in specifications for each of these causes of death are not statistically significant over the sample period. Notably, however, the relative increase in drug overdoses associated with exposure to PNTR is sufficiently large that it yields a statistically significant increase in the final panel, for overall deaths of despair, even with the lack of observed effects for suicide or ARLD.

Analogous estimates of interquartile shifts in counties’ initial demographic attributes (online appendix Figure A.1) reveal that counties with higher initial shares of manufacturing em-

⁹Tables reporting all coefficient estimates and standard errors are available upon request. R-squares and number of observations are noted in the figure notes.

¹⁰The results discussed below are robust to ending the sample period the year before the Great Recession, in 2007.

ployment and veterans have rising mortality from deaths of despair throughout the sample period. To the extent that counties with these attributes were experiencing long-run economic decline, they provide some support for the argument in Case and Deaton (2017) that rising deaths of despair may reflect a cumulation of negative social and economic outcomes that aggregate over time.

Given the findings reported in Figure 1, we focus on drug overdose mortality for the remainder of the paper.

3.3 Baseline Estimates by Gender and Race

Figure 2 uses our baseline specification to examine the link between PNTR and drug overdose across genders and racial categories. As shown in the figure, we find that the positive relationship between exposure to PNTR and fatal drug overdoses is only present for whites, with the step up in overdose mortality for white females being somewhat less sharp than for white males. By contrast, we do not find an association between PNTR and drug overdose deaths for males or females of other races.

3.4 Baseline Estimates by Age

Finally, we estimate the association between crude drug overdose death rates for whites and PNTR across nine five-year age categories, from 20 to 24 up to 60 to 64, that capture the working-age population. As indicated in Figure 3, an association between PNTR and drug overdose mortality is evident across most of the age bins from 20 to 54.¹¹ These results suggest a labor market mechanism, discussed further below.

4 Robustness

We examine the robustness of our baseline results to analysis of larger geographic areas and the inclusion of additional covariates and fixed effects that might control for state-level changes in health policy or opioid supply. We also consider the relationship between PNTR and other causes of death. These exercises reveal that the results reported above are robust to these alternative approaches.

Geographic areas: While analysis of counties is advantageous for capturing variation in exposure to PNTR and outcomes, the relative infrequency of deaths of despair may lead to noisy estimates of mortality among sparsely populated counties. To address this concern, we re-estimate our results on aggregations of counties that are based upon the US Census Bureau's Public Use Microdata Areas (PUMAs), which have a minimum population of 100,000 and are constructed by the US Census Bureau for each decennial Census to cover the entire United States. Because counties can span more than one PUMA, we combine PUMAs from the 2000 Census as needed so that all counties map into a unique PUMA or unique combina-

¹¹As noted earlier, our findings in Figures 1 to 2 are very similar, and somewhat large in magnitude, if the analysis is restricted to the working age population, i.e., to the five-year age bins that span 20 to 64 years old.

tion of PUMAs. We refer to these 950 geographic areas as “CUMAs.”¹² Figures A.6 to A.8 of the online appendix reproduce Figures 1 to 3 for CUMAs. Comparison of these figures reveals that the link between PNTR and deaths of despair is very similar across CUMAs, and slightly larger in magnitude.¹³

Medicaid expansion: Sommers et al. (2012) find that expansion of Medicaid in New York, Maine and Arizona in 2001, 2002 and 2006 is associated with a significant reduction in age-adjusted mortality among older adults, non-whites, and residents of poorer counties. To control for the potential influence of these expansions on our results, we construct three variables that interact indicators for these states with indicators picking out the years after the expansion. To this group, we add two additional variables to capture the introduction of “Romneycare” in Massachusetts in 2006 and the expansion of Medicaid in Oregon in 2008 (Baicker et al. (2013); Finkelstein et al. (2012)). As indicated by the comparison of the first and second panels of Figure 4, inclusion of these covariates has little impact on the estimated link between PNTR and fatal drug overdose.

Opioid supply: Surging opioid abuse has attracted substantial attention (e.g., Rudd et al. (2016)). Exogenous increases in the availability of opioids in areas exposed to PNTR – but that were unrelated to the change in policy – could lead to a spurious relationship with mortality.¹⁴ This concern seems plausible given that laws regarding the licensing and regulation of doctors as well as the tracking of opioid prescriptions varied substantially across states (Meara et al. (2016); Morden et al. (2014)). We assess the impact of this (potentially endogenous) variation in policy using data on state-level legislation pertaining to opioid regulation collected by Meara et al. (2016) For each state, we sum the number of categories of opioid legislation (e.g., pain-clinic regulation) enacted over the years covered in Meara et al. (2016), 2006 to 2012, and then interact these counts with the full set of year dummies used in our baseline specification. As indicated in the third panel of Figure 4, inclusion of these measures also has little impact on the estimated link between PNTR and white drug overdose.

State-year fixed effects: A very conservative approach to controlling for changes in medical and drug policies is to include state by year fixed effects, which capture any state-year-level factor that might exogenously affect mortality rates, including changes in health policies, economic shocks unrelated to exposure to PNTR, and changes in states’ underlying demographic characteristics. This approach is particularly stringent, as it absorbs substantial across-state variation in the NTR gap. Moreover, it sweeps out the effects of factors, such as increases in the demand or supply of opioids that might be related to PNTR (see further discussion below), that belong in estimates of its impact. Unsurprisingly, as illustrated in the final panel of Figure 4, inclusion of these fixed effects severely degrades the precision of the estimated impact of PNTR on drug overdose. Even so, an upward shift, albeit statistically insignificant, remains apparent.

Other causes of death: Labor market disruption could, in principle, affect mortality due to a range of causes, particularly if access to health insurance is tied to employment, as

¹²Case and Deaton (2017) analyze mortality across a similar geographic unit. Our county-CUMA mapping is available upon request, and we compare their population distributions in online appendix Figure A.2.

¹³We discuss the potential impact of migration on county-level mortality rates in the online appendix.

¹⁴Ruhm (2018), for example, argues that variation in mortality rates from drug overdoses is driven more by the availability and regulation of drugs than by economic or social conditions.

is the case for most areas of the United States during our sample period. In fact, we find no relationship between PNTR and the sixteen major categories of internal causes of death, e.g., cancer or diseases of the respiratory system. These results (displayed in online appendix Figure A.3) are consistent with the idea that these broad internal causes of death may be less likely than deaths of despair to respond to economic conditions.

5 Potential Mechanisms

We find that passage of PNTR is associated with relative increases in mortality due to deaths of despair among the working-age population. One straightforward mechanism for this relationship is a deterioration in employment opportunities (Browning and Heinesen (2012); Classen and Dunn (2012)). Here, we examine the association between PNTR and several labor market outcomes using Equation 3. As above, we report the estimated impacts of interquartile shifts in the NTR gap on these outcomes using figures analogous to those reported above.

The first two panels of Figure 5 reveal that greater exposure to PNTR is indeed associated with substantial – 1 to 2 percentage point – adverse relative changes in the unemployment and labor force participation rates (LFPR), respectively. In both cases, the estimated impact is centered around zero and not statistically significant prior to the change in trade policy, with the estimates for LFPR exhibiting larger standard errors. These outcomes are consistent with the finding in Autor et al. (2014) that workers with greater exposure to imports from China exhibit a stark decline in relative earnings.

The third and fourth panels of Figure 5 show that real disability payments and the number of disabled workers also exhibit relative increases after 2000, though estimation of the latter is hampered by data unavailability at the county level prior to 1999.¹⁵ While a link between deteriorating labor market conditions and increased disability take-up is well-known (Black et al. (2002); Autor et al. (2013)), here it may constitute an additional channel by which drug overdose deaths increased after the change in US trade policy. That is, if workers displaced by trade liberalization applied for disability during a period when opioid prescriptions were rising (Rudd et al. (2016)), rates of addiction and overdose might increase even further. Quinones (2015), for example, describes the possibility of opioid supplies responding to economic conditions in *Dreamland*:

“The pain treatment revolution had many faces and these mostly belonged to well-meaning doctors and dedicated nurses. But in the Rust Belt, another kind of pain had emerged. Waves of people sought disability as a way to survive as jobs departed. Legions of doctors arose who were not so well-meaning, or who simply found a livelihood helping people who were looking for a monthly government disability check as a solution to unemployment. By the time the pain revolution changed US medicine, the Ohio River valley had a class of these docs. They were an economic coping strategy for a lot of folks.”

¹⁵Disability payments by county-year are from the BEA Regional Economic Accounts website (linecode 2120), and are deflated using the personal consumption expenditures deflator. Disabled workers counts are from the Social Security Administration at https://www.ssa.gov/policy/docs/statcomps/oasdi_sc/2017/index.html.

This link may have been even more important if firms skirted safety regulations to remain competitive against Chinese competition, prompting an increase in injuries, an association documented in McManus and Schaur (2016). Further research into this mechanism, perhaps making use of pharmacy- or individual-level data on drug prescriptions or disability filings, to which we do not have access, would be both interesting and useful.

Finally, data on the composition of the manufacturing workforce provides intuition for why the relationship between PNTR and mortality is strongest among white males. According to the US Bureau of Labor Statistics, males account for 68 percent of US manufacturing employment in 1999 versus 49 percent of the population. For whites, the analogous percentages are 84 versus 82 percent.¹⁶ Moreover, within manufacturing, over-representation of whites is highest among occupations likely to be earning the highest wages – such as managerial and professional occupations – that could lead to the largest declines in income following job separation.¹⁷ The negative impact on mortality of these earnings declines might be magnified by the psycho-social stress induced by an accompanying loss of status (Cutler et al. (2006)).

6 Conclusion

We document a relationship between a plausibly exogenous change in US trade policy and drug overdose fatalities among working-age whites, helping to explain the alarming rise in “deaths of despair” among this group since 2000. While our findings do not provide an assessment of the overall welfare impact of this liberalization, they do offer a broader understanding of the distributional implications of trade. Moreover, by providing new evidence regarding the effects of major labor market disruptions, our results offer insights into the potential effects of future technology shocks – such as those arising from automation or artificial intelligence – that might lead to similarly sudden and geographically concentrated declines in employment.

¹⁶These figures are reported in online appendix Table A.2.

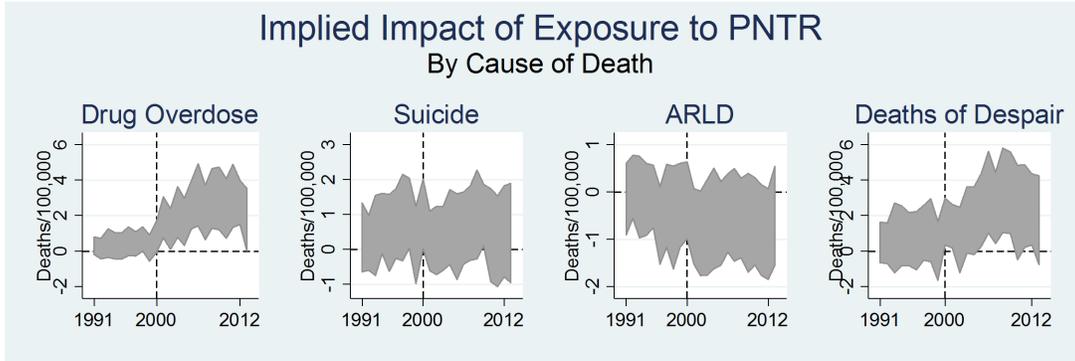
¹⁷Ebenstein et al. (2014) finds that workers displaced from manufacturing on average experience wage declines in moving to another sector. Whites accounted for 90 percent of managers and professionals, 86 percent of technical, sales, administrative and service positions, and 83 percent of precision production positions, versus 79 percent among operators, fabricators, laborers and other occupations (see online appendix Table A.2).

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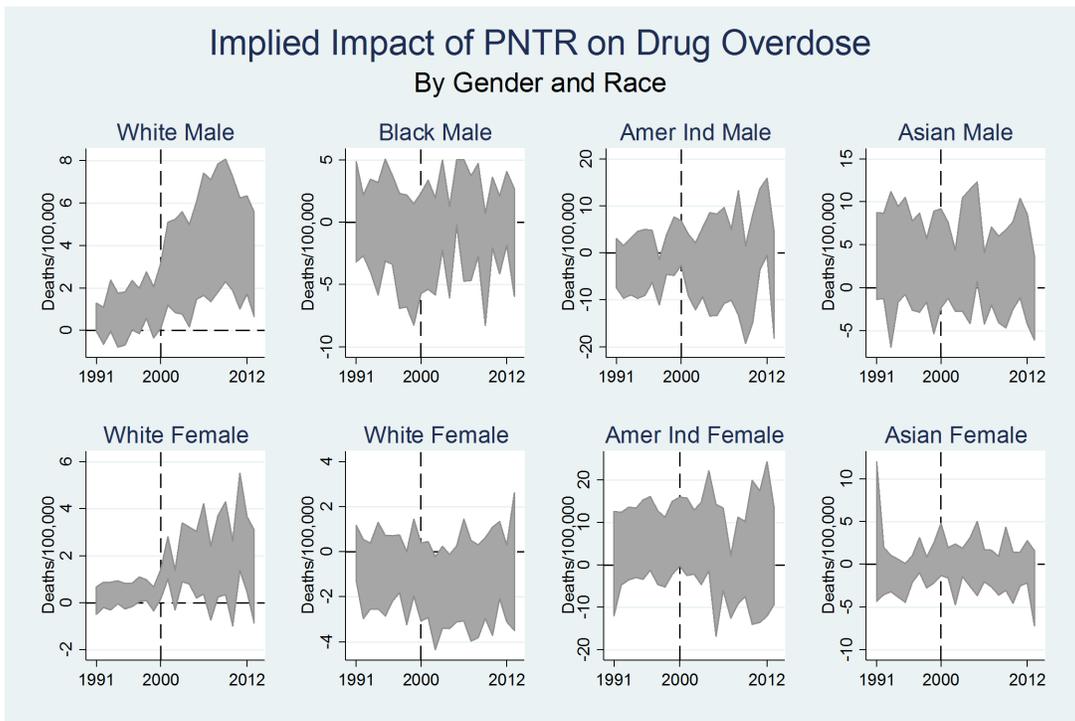
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Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence intervals of an interquartile shift in counties' exposure to PNTR on noted cause of death. Y-axis is in units of deaths per 100,000 population. Each panel presents the results of a separate estimation of equation 3 on death rates for fatal drug overdose, suicide, alcohol-related liver disease (ARLD) and overall fatalities from these deaths of despair. The population weighted average death rate across counties of these causes of death, in deaths per 100,000 population, are 5, 11, 4 and 20. Each regression has 74,924 observations across 3122 counties and R-squares ranging from 0.41 to 0.65. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

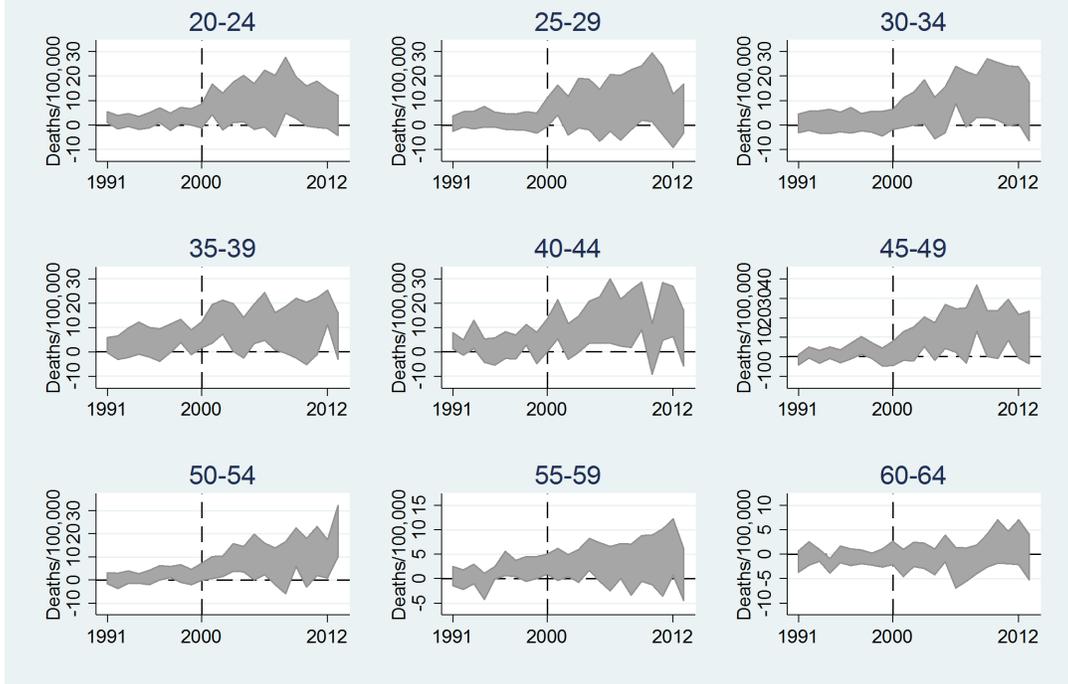
Figure 1: Implied Impact of PNTR on Deaths of Despair



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in counties' exposure to PNTR on drug overdose mortality for males (top panel) and females (bottom panel), by racial category. Each panel presents the results of a separate estimation of equation 3. The population weighted average death rate across counties for fatal drug overdoses among white males and females is 7 and 3 per 100,000 population. Each regression has 74,924 observations across 3122 counties. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

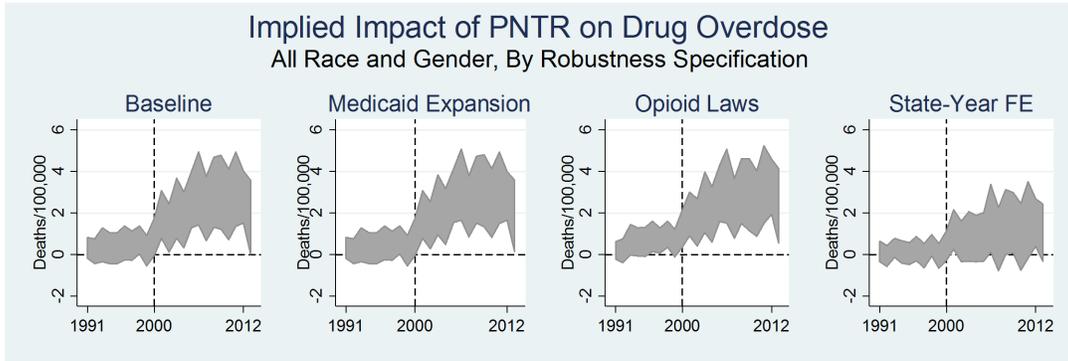
Figure 2: Implied Impact of PNTR on Drug Overdose Deaths, by Gender and Race

Implied Impact of PNTR on Drug Overdose Whites by Age



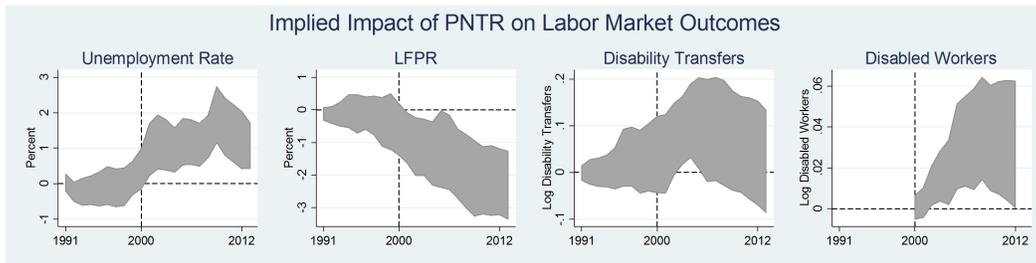
Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in counties' exposure to PNTR on drug overdose mortality for whites by noted five-year age category. Each panel presents the results of a separate estimation of equation 3. Each regression has 74,924 observations across 3122 counties. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure 3: Implied Impact of PNTR on White Drug Overdose Deaths, by Age



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in counties' exposure to PNTR on drug overdose mortality. Each panel presents the results of a separate estimation of equation 3 using a different set of control variables. Each regression has 74,924 observations across 3122 counties. First panel displays baseline result from Figure 1. Second panel is for the baseline specification plus dummy variables for state-years in which Medicaid expansion occurs. Third panel is for the baseline specification plus controls for state opioid-law restrictiveness. Fourth panel is for the baseline specification plus state-year fixed effects. The population weighted average death rate across counties for fatal drug overdoses is 5 per 100,000 population. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure 4: Robustness Specifications



Source: Authors' calculations based on data from the U.S. Bureau of Labor Statistics, the U.S. Bureau of Economic Analysis and the Social Security Administration. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in counties' exposure to PNTR on noted outcome. Each panel presents the results of a separate estimation of equation 3. Each regression has 74,924 observations across 3122 counties. Unemployment Rate and LFPR are the unemployment rate and the labor force participation rate, in percent. Disability Transfers and Disabled Workers are log current transfer payments for disability and log number of disabled workers. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure 5: PNTR and Labor Market Outcomes

Online Appendix

This online appendix contains additional empirical results and information on data creation referenced in the main text.

A Summary Statistics

Table A.3 reports counties' population-weighted-average death of despair mortality rates for the year 2000, as well as several initial county attributes discussed in the main text.

B Cause of Death Codes

We use proprietary “compressed all-county mortality files” available by petition from the US Centers for Disease Control’s National Center for Health Statistics (NCHS). Causes of death are classified by the NCHS based on codes listed in the International Classification of Diseases (ICD), where version 10 of the ICD codes (ICD-10) is used for years 1999 to 2013 and version 9 (ICD-9) of the ICD codes is used for years 1990 to 1998.¹⁸ NCHS recodes the ICD causes of death into classification systems of varying levels of aggregation. We use the NCHS 282 cause recodes for the years 1990 to 1998 and the NCHS 358 cause recodes for the years 1999 to 2013. The following codes are used to define the three categories of deaths of despair considered in this paper:

- Suicide:
 - NCHS 282 Cause Recodes (1990-1998): 33700-34400
 - NCHS 358 Cause Recodes (1999-2013): 424-431
- Drug Overdoses
 - NCHS 282 Cause Recodes (1990-1998): 31700, 35300
 - NCHS 358 Cause Recodes (1999-2013): 420, 443
- Alcohol-Related Liver Disease¹⁹
 - NCHS 282 Cause Recodes (1990-1998): 24200
 - NCHS 358 Cause Recodes (1999-2013): 298

¹⁸The “blue form” instructions for completing the cause of death section of a death certificate are available at http://www.cdc.gov/nchs/data/dvs/blue_form.pdf. Death certificates note both county of residence and county of work. We focus on the former, and note that 81 percent of deaths occur in the deceased’s county of residence.

¹⁹We do not consider other forms of liver disease which might be classified as deaths of despair given difficulties associated with how they are tracked by the CDC over time.

Case and Deaton (2015) highlights a substantial rise in deaths due to suicide, poisoning – which primarily consists of drug overdoses – and chronic liver disease among middle-aged whites starting in 1999. In Figure A.4, we use the above CDC death codes to replicate these trends and extend them backwards in time. As indicated in the figure, the weighted-average rates of suicide and ARLD across counties are more or less flat during the 1990s but begin increasing around the time of the change in US trade policy in the year 2000, particularly for suicide. Deaths due to drug overdose, by contrast, are increasing before 2000, but exhibit an inflection point around that time.

Table A.4 reports the overall US mortality rates for major external and internal causes of death in 2000. Internal causes account for more than 90 percent of the 2.4 million deaths in that year, with the three leading causes being cancer (neoplasm), circulatory disease and respiratory ailments. Suicide, drug overdose and ARLD account for 29,416, 14,160 and 12,126 deaths, or approximately 10, 5 and 4 per 100,000.

C NTR Gap

Figure A.5 reports the distribution of NTR gaps across four-digit SIC industries, counties and CUMAs. Relative to the distribution across industries, the distributions for counties and CUMAs are shifted towards the left, reflecting the fact that most workers in most areas are employed outside goods-producing sectors.²⁰

D Other Control Variables

NTR Rates: counties' labor-share-weighted US import tariff rates, NTR_{ct} , are computed as in Equation 2, except that the US NTR tariff rate for industry j (in percent) is used in place of the NTR gap for industry j . We find that the distribution of NTR_{ct} across our sample period declines during the late 1990s due to implementation of tariff reductions agreed upon during the Uruguay Round.²¹

MFA Exposure: As discussed in greater detail in Khandelwal et al. (2013), the MFA and its successor, the Agreement on Textile and Clothing (ATC), grew out of quotas imposed by the United States on textile and clothing imports from Japan during the 1950s. Over time, the MFA evolved into a broader institution that regulated the exports of clothing and textile products from developing countries to the United States, European Union, Canada and Turkey. Bargaining over these restrictions was kept separate from multilateral trade negotiations until the conclusion of the Uruguay Round in 1995, when an agreement was struck to eliminate the quotas over four phases. On January 1, 1995, 1998, 2002 and 2005, the United States was required to remove textile and clothing quotas representing 16, 17, 18 and the remaining 49 percent of their 1990 import volumes, respectively. Relaxation

²⁰The distribution for industries in Figure A.5 omits SIC industries that are not subject to import tariffs.

²¹NTR tariff rates from Feenstra et al. (2002) are unavailable after 2001 and so are assumed constant after that year. Analysis of analogously computed “revealed” tariff rates from public US trade data during this interval in Pierce and Schott (2016a) suggests this is a reasonable assumption that avoids having to make do with the smaller set of industries for which “revealed” rates are available.

of quotas on Chinese imports did not occur until it became a member of the World Trade Organization in 2001; as a result, its quotas on the goods in the first three phases were relaxed in early 2002 and its quotas on the goods in the fourth phase were relaxed as scheduled in 2005. The order in which goods were placed into a particular phase was chosen by the United States.

Computation of counties' exposure to elimination of the MFA proceeds in three steps. First, we follow Khandelwal et al. (2013) in measuring the extent to which MFA quotas in industry j and phase p were binding as the average fill rate of the industry's constituent import products in the year before they were phased out, $FillRate_{jp}$.²² Specifically, for each phase, we measure an industry's exposure to MFA expiration as its average quota fill rate in the year prior to the phase's expiration. Industries with higher pre-expiration average fill rates faced more binding quotas and are therefore more exposed to the end of the MFA. Second, we compute counties' labor-share-weighted-average fill rate across industries for each phase, $FillRate_{cp}$, using a version of Equation 2. Finally, the county-year variable of interest, $MFA\ Exposure_{ct}$, cumulates the calculated fill rates as each phase of expiration takes place. This measure of exposure to the MFA rises over time, as quotas for additional products are removed, by phase.

Changes in US Export Opportunities: As part of its accession to the WTO, China agreed to institute a number of policy changes that could have influenced US manufacturing employment and thereby mortality, including liberalization of its import tariff rates and reductions of production subsidies, which might increase export opportunities for US manufacturers. Following Pierce and Schott (2016a) we use product-level data on Chinese import tariffs from 1996 to 2005 from Brandt et al. (2017) to compute the average change across those years in Chinese import tariffs across products within each US industry. For production subsidies, we use data from the Annual Report of Industrial Enterprise Statistics compiled by China's National Bureau of Statistics (NBS), which reports the subsidies provided to responding firms.²³ For both changes in Chinese import tariff rates and production subsidies, we compute the labor-share-weighted average of this change across the industries active in each US county as in Equation 2, and then interact these variables with an indicator for post-PNTR years.

Demographics: Our baseline specifications control for interactions of a post-PNTR indicator variable with three initial-year (i.e., 1990) county attributes: the percent of the population without any college education, median household income and percent of population that are veterans.²⁴ These variables allow for the possibilities, respectively, that changes in technology unrelated to the trade liberalization might have replaced less-educated workers with technology disproportionately during the 2000s, that high-income households gained

²²As discussed in Brambilla et al. (2010), fill rates are defined as actual imports divided by allowable imports under the the quota. MFA products for which there were no restrictions on imports (i.e., there were no quotas), have fill rates of zero.

²³The NBS data encompass a census of state-owned enterprises (SOEs) and a survey of all non-SOEs with annual sales above 5 million Renminbi (~\$600,000). The version of the NBS dataset available to us from Khandelwal et al. (2013) spans the period 1998 to 2005. Following Girma et al. (2009) and Aghion et al. (2015) we use the variable "subsidy" in this dataset and compute the change in the subsidies to sales ratio for each SIC industry between 1998 and 2005 using concordances provided by Dean and Lovely (2010).

²⁴We use initial rather than contemporaneous levels of these variables as the latter may be affected by the change in policy.

better access to medical care after the 2000s, perhaps due to health insurance provided by their employers, and that an increase in suicide and opioid misuse might be the result of military experience associated with post-9/11 wars in Afghanistan and Iraq (Kemp and Bossarte (2012); Bauerlein and Campo-Flores (2016)). These attributes, summarized in Table A.3, are obtained from the US Census Bureau's 1990 Decennial Census.²⁵ As noted in the table, the unweighted means and standard deviations across counties are 54 and 11 percent (share of population with no college education), 40 and 11 thousand dollars (median household income), and 14 and 3 percent (percent of population that are veterans, respectively).²⁶

Manufacturing Share: Another means of controlling for counties' exposure to automation and competition from low-wage foreign workers is to include their initial share of employment that is in manufacturing. To the extent that areas with high NTR gaps had spuriously high manufacturing employment in the 1990s, this covariate also helps control for declines in manufacturing employment during the 2000s that are driven by mean reversion. On the other hand, because manufactured goods represent the vast majority of products present in the US tariff schedule, NTR gaps and manufacturing employment shares are highly correlated. As a result, inclusion of both exposure to PNTR and counties' manufacturing share may make it less likely to find an effect for either.

Table A.5 in the online appendix reports the results of OLS regressions of counties' NTR gaps on the control variables discussed in this section. As indicated in the table, counties with higher NTR gaps, have greater exposure to the MFA, have higher import tariffs across the goods they produce, are exposed to larger reductions in Chinese imports tariffs and subsidies, have lower household incomes in 1990, have lower share of population with a college education in 1990, and have a higher share of the population that are veterans in 1990.

E CUMA-Level Results

While our primary analysis uses counties as the level of observation, we also calculate mortality rates across groups of counties with larger populations, therefore potentially decreasing the amount of noise that can be present in mortality rates for sparsely populated counties. As noted in the main text, we refer to these areas as CUMAs.²⁷ In Figures A.6, A.7, and A.8 of this appendix, we report the analogues of Figures 1, 2 and 3 of the main text. As indicated by a comparison of the two groups of figures, results are very similar, if somewhat larger in magnitude for CUMAs.

F Migration

County-level mortality rates could, in principle, be influenced by two types of selective out-migration. The first would be migration based on age, e.g., if younger workers are more

²⁵These data can be downloaded from the Dexter Data Extractor from the University of Missouri, available at <http://mcdc.missouri.edu/>.

²⁶These unweighted averages across counties can differ from national averages as they are more affected by counties with small populations. Regression results are population weighted.

²⁷Our mapping of counties to CUMAs is available upon request.

likely to move in response to a labor market shock than older workers. Examining both population changes and migration, Greenland et al. (2016), for example, find that areas with greater exposure to PNTR experience relative reductions in population and relatively larger out-migration, especially among the young, though most of the out-migration occurs with a lag of 7 to 10 years. Such movement might bias our results downwards or upwards depending on whether younger workers are more or less likely to suffer deaths of despair. On the other hand, the SEER population data we use to calculate age-adjusted mortality rates track population changes by age, race, and gender, at an annual level, and therefore can reasonably be expected to reflect changes in the population of young people that might affect the mortality rates we compute. In addition, the Census population data upon which the SEER population data are based include explicit adjustments to account for migration.²⁸

A second type of selective out-migration that might influence our results involves differential movement of workers within age groups. If those least likely to suffer deaths of despair are more likely to migrate in response to the change in US trade policy, our results will be overstated, and *vice versa*. We do not have any data that allows us to address this issue directly. In either case, our finding that age-adjusted mortality rates increase within counties more exposed to PNTR is evidence of important distributional implications of changes in trade policy. Moreover, because overall deaths of despair increase substantially over the period we examine (Figure A.4; Case and Deaton (2015)), it is clear that the data do not simply reflect a reshuffling of population, and therefore mortality, across counties.

²⁸While Arthi et al. (2017) discuss potential errors in inter-censal population estimates, these issues are less of a concern once the estimates have been revised to reflect the information in subsequent censuses, which has occurred for the years 1990 to 2010, nearly our entire period of analysis. While data for 2011 to 2013 will not undergo this revision process until the 2020 Census is released, accuracy of inter-censal population estimates is less of a concern in the years immediately following a Census (Phipps et al. (2005)).

Appendix Tables and Figures

Age	Population	Share
Under 1 year	3,855,956	0.0137
1-4 years	15,322,337	0.0543
5-14 years	41,101,548	0.1457
15-19 years	20,294,955	0.0719
20-24 years	19,116,667	0.0678
25-29 years	19,280,263	0.0683
30-34 years	20,524,234	0.0727
35-39 years	22,650,852	0.0803
40-44 years	22,517,991	0.0798
45-49 years	20,219,527	0.0717
50-54 years	17,779,447	0.0630
55-59 years	13,565,937	0.0481
60-64 years	10,863,129	0.0385
65-69 years	9,523,909	0.0338
70-74 years	8,860,028	0.0314
75-79 years	7,438,619	0.0264
80-84 years	4,984,540	0.0177
85 and over	4,262,472	0.0151
Total	282,162,411	1.0000

Notes: Table reports the overall U.S. population weights associated with the age categories used in our baseline results. Data are for the year 2000.

Table A.1: Distribution of US Population Across Age Categories in 2000

Occupation	Male	White	Male	Total
Managerial, Professional	70.8	90.4	.	100
Technical, Sales, Admin, Service	49.6	86.3	.	100
Precision Production	83.0	85.5	.	100
Operators, Fabricators, Laborers, Other	67.0	78.9	.	100
Total	68.0	84.3	58.4	100
Total in Population	49.0	81.9	40.3	100

Notes: Table displays the share of manufacturing workers in 1999 that are male or white, by occupation within manufacturing. "." represents unavailable data.

Source: U.S. Bureau of Labor Statistics.

Table A.2: Share of Whites and Males Among Occupations in Manufacturing, 1999

Variable	N	Mean	SD
Age-Adjusted Death Rate (2000)			
Overall	3122	858	111
Deaths of Despair	3122	20	8
Drug Overdose	3122	5	4
Suicide	3122	10	5
ARLD	3122	4	3
NTR Gap (1999)	3122	6	4
Median Household Income (1990)	3122	40	11
Percent No College (1990)	3122	54	11
Percent Veteran (1990)	3122	14	3
Percent Manufacturing Employment (1990)	3122	20	11

Source: Authors' calculations based on data from the U.S. Centers for Disease Control (CDC), the National Cancer Institute's Surveillance, Epidemiology and End Results (SEER) Program, and the U.S. Census Bureau. Table summarizes distribution of noted attributes across counties, weighted by population, where the year in parentheses notes the year for which the attribute is measured. Death rates are expressed per 100,000 population. Median household income is in thousands of dollars.

Table A.3: Summary Statistics

	Total Deaths	Crude Rate
External causes of death		
Suicide	29,416	10
Drug Overdose	14,160	5
Other (e.g., motor vehicle accidents, falls, crime)	108,560	39
Total External	152,136	54
Internal causes of death		
Infectious or Parasitic Diseases (e.g., septicemia)	59,122	21
Neoplasms (i.e., cancer)	567,242	202
Diseases of the Blood (e.g., anemia)	9,337	3
Endocrine, Nutritional and Metabolic Diseases (e.g., diabetes)	94,456	34
Mental (e.g., dementia)	46,040	16
Diseases of the Nervous System (e.g., Alzheimers, Parkinsons)	91,182	32
Diseases of the Circulatory System (e.g., AML, hypertension)	943,068	336
Diseases of the Respiratory System (e.g., pneumonia, influenza)	231,253	82
Diseases of the Digestive System (e.g., liver failure)	84,136	30
Diseases of the Skin	3,756	1
Diseases of the Skeletal System (e.g., arthritis)	13,775	5
Diseases of the Genitourinary System (e.g., renal failure)	54,604	19
Pregnancy and Childbirth	404	0
Conditions Arising in the Perinatal Period	14,097	5
Congenital Malformations and Abnormalities	10,631	4
Not elsewhere classified	31,954	11
Total Internal	2,255,057	803
Total	2,407,193	857

Notes: Table displays overall number of deaths by noted cause for the year 2000. Crude rate is the number of deaths divided by total population in 2000 multiplied by 100,000.

Table A.4: Average Death Rates by Major Causes of Death

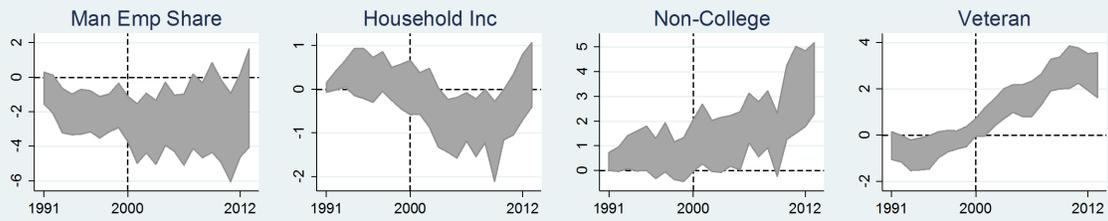
	NTR Gap _c						
MFA Exposure _c	18.086***						
	0.767						
2000 NTR _c		4.271***					
		0.066					
ΔChinese Tariffs _c			-1.572***				
			0.024				
ΔChinese Subsidies _c				-39.141***			
				4.565			
1990 Median HHI _c					-0.078***		
					0.013		
1990 Percent No College _c						0.252***	
						0.009	
1990 Percent Veteran _c							-0.586***
							0.041
1990 Percent Manufacturing Employment _c							0.346***
							0.003
Observations	3,122	3,122	3,122	3,122	3,122	3,122	3,122
R-squared	0.151	0.570	0.572	0.023	0.011	0.184	0.769

Notes: Table reports the results of county-level OLS regression of the 1999 NTR gap (in percent) on county attributes. First covariate is the labor share-weighted average fill rate of the MFA products produced in the county. Second covariate is the labor share-weighted average NTR tariff rate of the goods produced in the county. Third and fourth covariates are the labor share-weighted average 1996 to 2005 change in Chinese import tariffs and the 1998 to 2005 change in Chinese production subsidies across the industries active in the county. Fifth through seventh covariates are counties' median household income, percent of residents without college education and percent of residents who are veterans in 1990. Final covariate is the percent of employment in manufacturing. Results for the regression constant are suppressed. Standard errors are reported below coefficients. *, **, and *** signify statistical significance at the 10, 5 and 1 percent level.

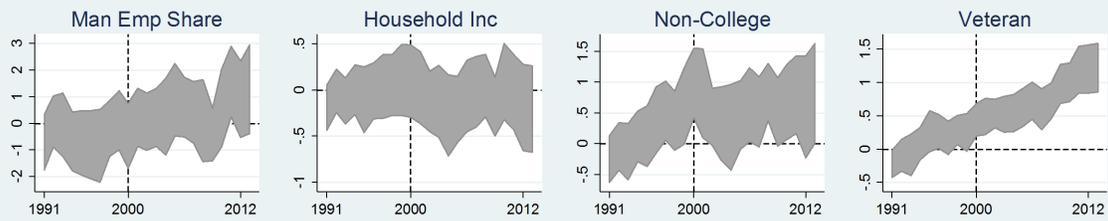
Table A.5: 1999 NTR Gap versus Other County Attributes

Implied Impact of an Interquartile Shift in Initial County Attribute

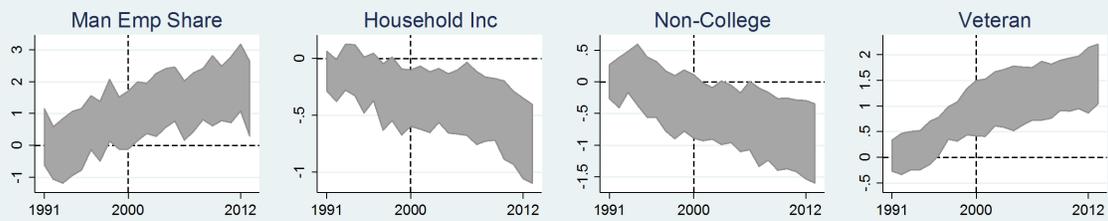
Drug Overdose



Suicide

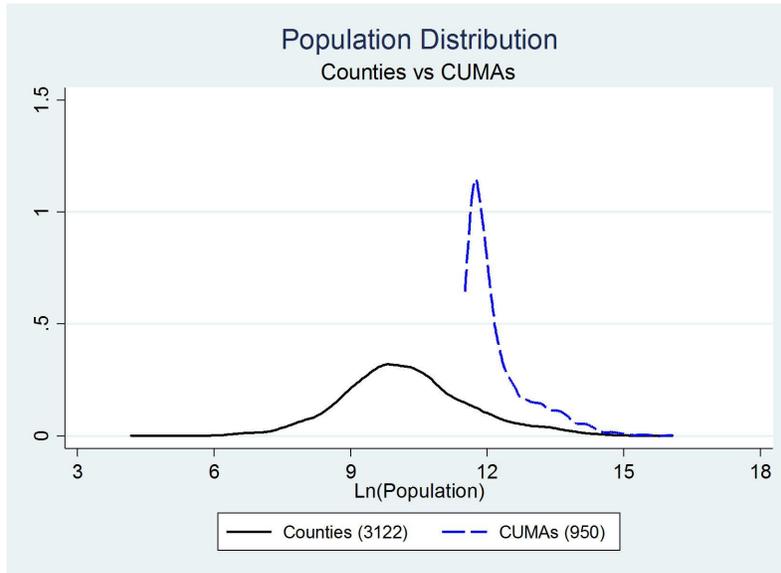


ARLD



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence intervals of the estimated impact of an interquartile shift in the noted initial county attribute with respect to the noted causes of death. Y-axis is in units of deaths per 100,000 population. Each row presents the results of a separate estimation of equation 3. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

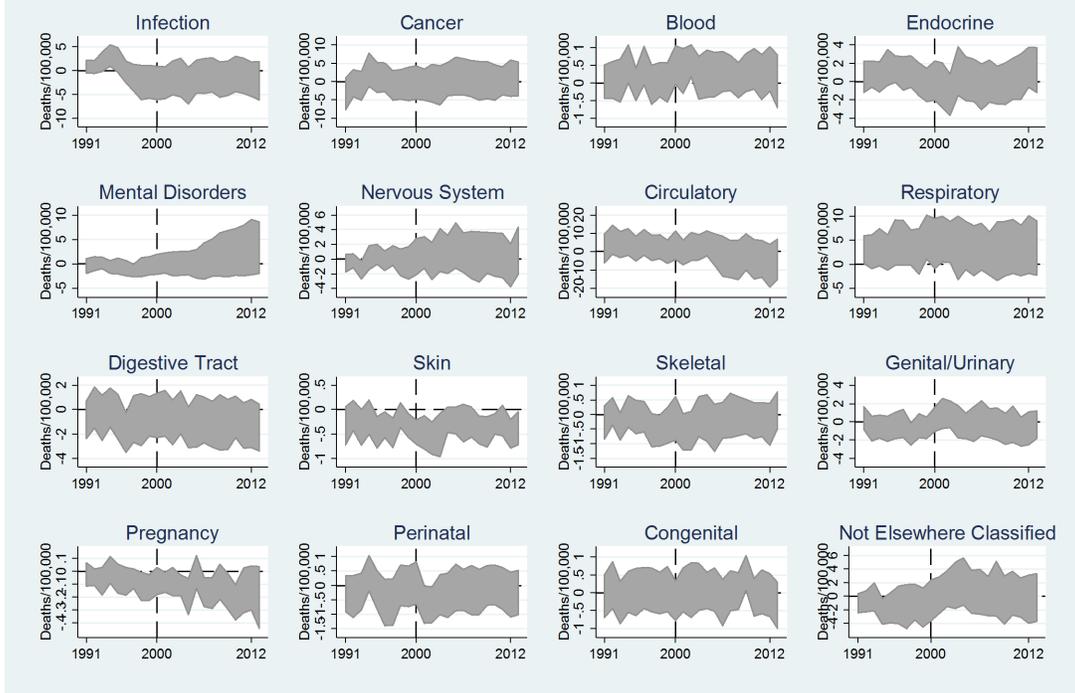
Figure A.1: Interquartile Shifts in Initial County Attributes



Source: Authors' calculations based on data from the U.S. Census Bureau and the National Cancer Institute's Surveillance, Epidemiology and End Results (SEER) Program. Figure displays distribution of employment across noted geographic units. As discussed in the main text, CUMAs are groups of counties based on Public Use Microdata Areas (PUMAs). Figures in parentheses refer to the number of units in each geography, e.g., 3122 counties.

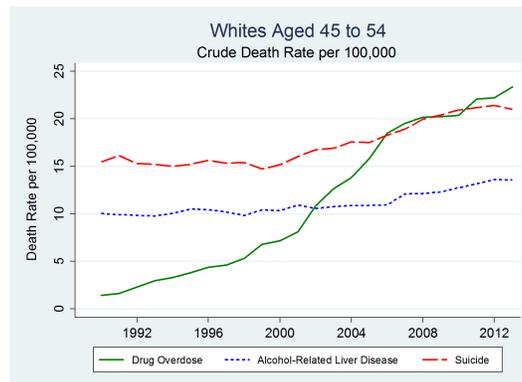
Figure A.2: Distribution of Population Across Various Geographic Units

Implied Impact of PNTR on Internal Causes of Death All Races, All Genders



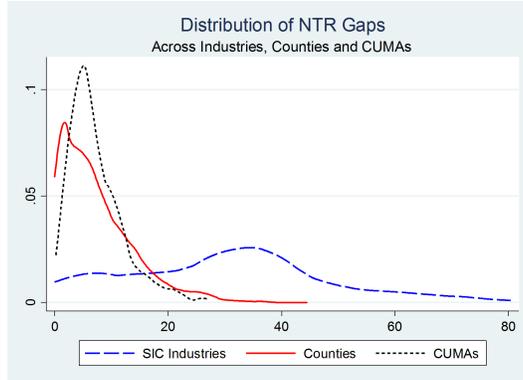
Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in counties' exposure to PNTR on major internal cause of death categories, e.g., cancer or diseases of the respiratory system. Each panel presents the results of a separate estimation of equation 3. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure A.3: PNTR and Internal Causes of Death



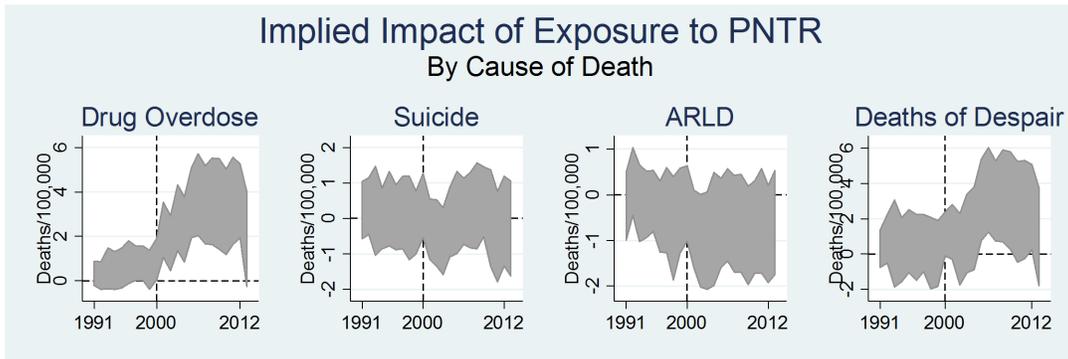
Source: U.S. Centers for Disease Control (CDC). Figure displays the crude death rate for three causes of death across all U.S. counties for whites aged 45 to 54.

Figure A.4: Death Rates for Whites Aged 45-54



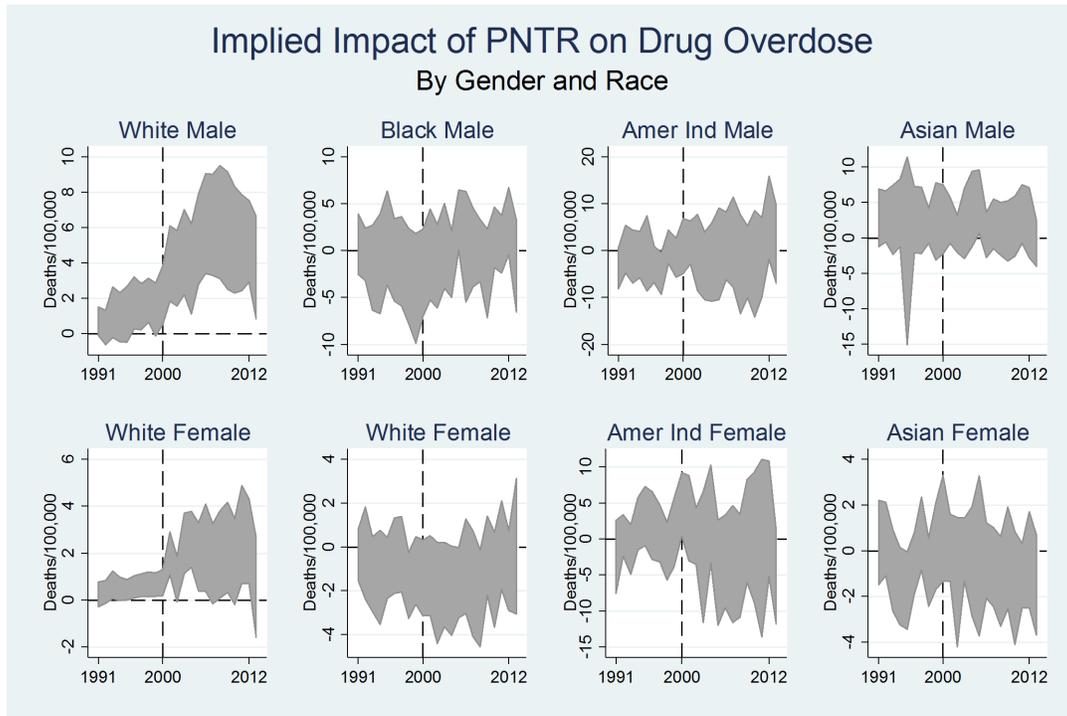
Source: Authors' estimates based on data from Feenstra, Romalis and Schott (2002) and the U.S. Census Bureau's 1990 County Business Patterns. Figure displays distributions of NTR gaps across SIC industries, counties and groups of counties based on Public Use Microdata Areas (PUMAs) referred to as CUMAs in the main text.

Figure A.5: Distribution of 1999 NTR Gaps Across Industries, Counties and CUMAs



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence intervals of an interquartile shift in CUMAs' exposure to PNTR on noted cause of death. Y-axis is in units of deaths per 100,000 population. Each panel presents the results of a separate estimation of equation 3 on death rates for fatal drug overdose, suicide, alcohol-related liver disease (ARLD) and overall fatalities from these deaths of despair. The population weighted average death rate across CUMAs of these causes of death, in deaths per 100,000 population, are 5, 11, 4 and 20. Each regression has 25,171 observations across 950 CUMAs and R-squares ranging from 0.68 to 0.76. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

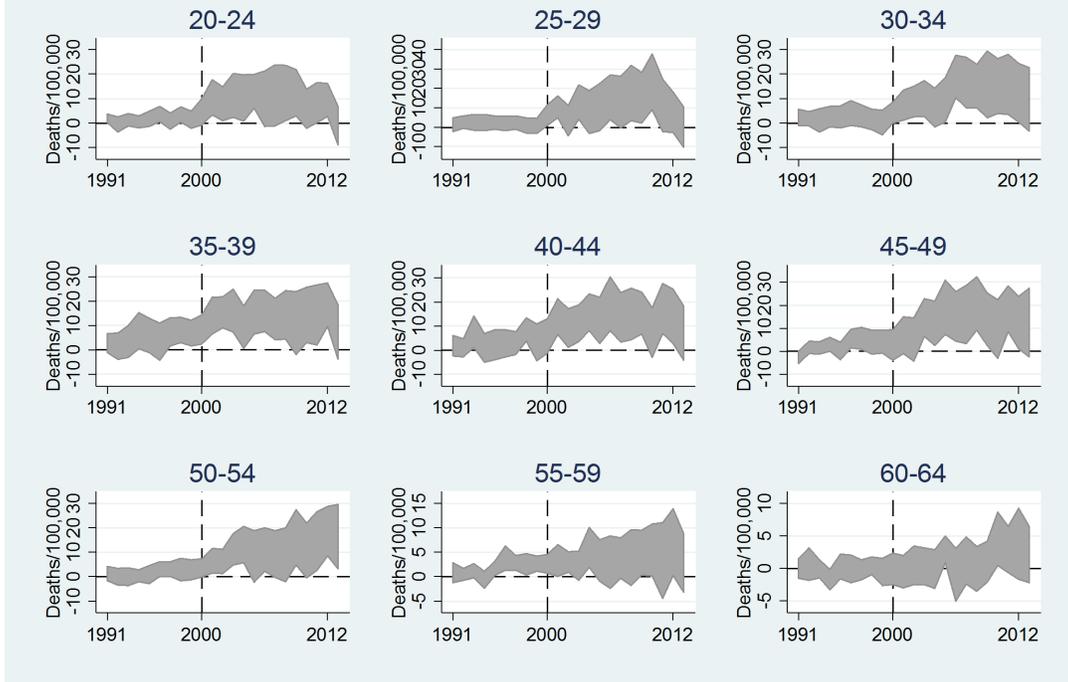
Figure A.6: Implied Impact of PNTR on Deaths of Despair (CUMAs)



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in CUMAs' exposure to PNTR on drug overdose mortality for males (top panel) and females (bottom panel), by racial category. Each panel presents the results of a separate estimation of equation 3. The population weighted average death rate across counties for fatal drug overdoses among white males and females is 7 and 3 per 100,000 population. Each regression has 22,800 observations across 950 CUMAs. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure A.7: Implied Impact of PNTR on Drug Overdose Deaths, by Gender and Race (CUMAs)

Implied Impact of PNTR on Drug Overdose Whites by Age



Source: Authors' calculations based on U.S. Centers for Disease Control (CDC) data. Figure displays the 95 percent confidence interval of the implied impact of an interquartile shift in CUMAs' exposure to PNTR on drug overdose mortality for whites by noted five-year age category. Each panel presents the results of a separate estimation of equation 3. Each regression has 22,800 observations across 950 CUMAs. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Figure A.8: Implied Impact of PNTR on White Drug Overdose Deaths, by Age (CUMAs)