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Trade Liberalization and Mortality: Evidence from US Counties†

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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. (*JEL* F13, F16, I12, R12)

Recent research by Case and Deaton (2015) suggests an alarming rise in "deaths of despair"—drug overdose, suicide, and diseases of the liver—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

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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 contemplating 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. Thus, a higher NTR gap indicates that an industry was 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 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 1990 to 2013. We then employ a generalized difference-in-differences (DID) identification strategy to examine whether counties that are more exposed to PNTR experience differential changes in mortality and labor market outcomes after the policy is implemented. We include 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 that 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 3 per 100,000, or 10 to 15 percent of the average mortality rate from these causes across counties in 2000, the year of the policy change. Within deaths of despair, we find that the link between PNTR and mortality is driven by drug overdoses. For this cause of death, an interquartile shift in exposure is also associated with a relative increase of 2 to 3 per 100,000, a sizable share of the 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 alcohol-related liver disease (ARLD).

[1] Pierce and Schott (2016) 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 Limão (2017) find that PNTR accounts for one-third of the growth in Chinese exports to the United States between 2000 and 2005. Earlier research by Autor, Dorn, and Hanson (2013) finds 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.

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's (2017) hypothesis that "cumulative disadvantage" in the labor market for less-educated workers may lie behind the increase in mortality. Indeed, while employment opportunities for lower-skilled workers have been declining for some time (Autor, Levy, and Murnane 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.

The link we find between PNTR and mortality also 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 deaths of despair, while Hollingsworth, Ruhm, and Simon (2017) find that macroeconomic shocks at the county and 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 (Lang, McManus, and Schaur 2019; McManus and Schaur 2016), provision of local public goods (Feler and Senses 2017), and innovation (Bloom, Draca, and Van Reenen 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—mortality—that has only recently gained attention in the trade literature. Using an alternate identification strategy, Autor, Dorn, and Hanson (2019) find that areas subject to larger increases in Chinese import competition exhibit increases in male, relative to female, mortality for young adults, which they put forward as one factor contributing to a decline in the supply of marriageable males.[2] Relative to that study, our use of proprietary

[2]Using the identification strategy of Autor, Dorn, and Hanson (2013), Adda and Fawaz (2017) find evidence of worsening of health and increased mortality among areas with larger increases in import competition. Hummels, Munch, and Xiang (2016) find that increased effort due to positive export demand shocks is associated with higher

data from the CDC allows us to examine mortality by cause of death among detailed demographic groups and geographic regions, to explore potential mechanisms linking trade liberalization to mortality, and to investigate the robustness of our results to a broader set of controls.

The paper proceeds as follows: Section I describes the data, Section II presents our main results, Section III provides robustness checks, and Section IV discusses mechanisms. Section V concludes. An online Appendix provides additional empirical results and dataset details.

I. Data

A. *Mortality Rates across Counties*

We calculate the number of deaths by county, demographic category, and cause using proprietary data from the CDC'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., 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. We use these population estimates to compute both "crude" and "age-adjusted" mortality rates, expressed per 100,000 population. The crude death rate for a county-year is simply the total number of deaths in the county-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, using the US population shares in those age categories in 2000 as weights.[3] Across counties, the population weighted average mortality rates (and standard deviations) for drug overdose, suicide, ARLD, and overall deaths of despair are 5 (4), 10 (5), 4 (3), and 20 (8).

B. *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 larger than the corresponding NTR rates. Imports from nonmarket

rates of illness and injury among Danish workers, while Bombardini and Li (2016) show that higher pollution associated with expanded exports is related to a substantial increase in Chinese infant mortality.

[3] These population shares are reported in online Appendix Table A.1. We 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 find similar results if we restrict the analysis to the working-age population, i.e., age bins between 20 and 64.

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 (1994, p. 3) 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."[4] 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.[5]

We follow Pierce and Schott (2016) 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,

$$NTRGap_j = NonNTRRate_j - NTRRate_j. \tag{1}$$

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, Romalis, and Schott (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 (2016), 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

[4] Storer H. Rowley, "China Woos Western Businesses, Snubs Clinton," *Chicago Tribune*, May 20, 1993, https://www.chicagotribune.com/news/ct-xpm-1993-05-20-9305200182-story.html. Further anecdotal evidence is provided in Pierce and Schott (2016).

[5] We 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.

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,

$$NTRGap_c = \sum_j \frac{L_{jc}^{1990}}{L_c^{1990}} NTRGap_j, \tag{2}$$

where c indexes counties, j indexes industries, and L represents employment. We use employment shares from 1990, ten years before the change in policy.[6] NTR gaps are defined only for industries whose output is subject to import tariffs, primarily in the manufacturing and agricultural sectors. Industries whose output is not subject to tariffs, such as service industries, are assigned NTR gaps of zero. Across counties, the unweighted 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.

C. *Other Control Variables*

Our baseline specification controls for several changes in United States 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, to help identify counties more exposed to the introduction of labor-saving technical change; share of population that are veterans, as a control for a group susceptible to deaths of despair; share of population that is foreign born, to account for the possibility that nonnatives have different propensities for deaths of despair and that they locate nonrandomly across counties; and the share of employment in manufacturing, which accounts for the various ways in which industrial counties might differ from those whose activity is predominantly in other sectors. Each of these variables is discussed in detail in the online Appendix.

II. 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 ARLD. We focus on these causes of death for several reasons: they account for a substantial portion of the increase in mortality rates 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

[6] Data sources are described in online Appendix Section E.2.

schemes used by the CDC over time is straightforward; and they may be more easily observable than other forms of death.[7]

A. *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,

$$(3) \qquad DeathRate_{ct} = \sum_t \theta_t \mathbf{1}\{year = t\} \times NTRGap_c + \beta \mathbf{X}_{ct} + \sum_t \gamma_t \mathbf{1}\{year = t\} \times \mathbf{X}_c + \delta_c + \delta_t + \varepsilon_{ct}.$$

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 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 to determine when any such relationship is first observed. The term $\mathbf{X}_{ct}$ represents the time-varying controls for policy discussed in Section IC: 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 MFA ($MFAExposure_{ct}$). The term $\mathbf{X}_c$ 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 five initial (1990) county attributes discussed above. Including interactions of these attributes with the full set of year dummies allows their relationship with mortality rates to differ before and after passage of PNTR. Variables 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. Regressions are weighted by 1990 population. Standard errors are clustered at the state level, allowing for correlation of errors across counties within states, and therefore yield 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 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.

B. *Baseline Estimates*

Given the large number of coefficient estimates in equation (3), we summarize our results visually.[8] 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 twenty-fifth percentile to

[7] While listed causes of death are noisy (Schottenfeld et al. 1982), this problem is likely less severe for deaths of despair given the higher scrutiny they attract.

[8] Tables reporting all coefficient estimates and standard errors are available upon request.

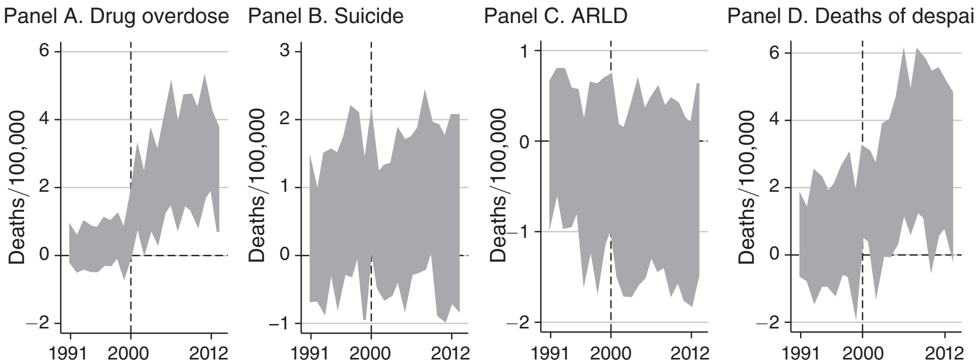

Figure 1. Implied Impact of PNTR on Deaths of Despair

Notes: 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 population-weighted estimation of equation (3) on death rates for fatal drug overdose, suicide, ARLD, and overall fatalities from these deaths of despair. The population-weighted average death rates across counties of these causes of death, in deaths per 100,000 population, are 5, 10, 4, and 20. Each regression has 74,924 observations across 3,122 counties and R^2 values ranging from 0.41 to 0.65. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Source: Authors' calculations based on CDC data.

the seventy-fifth 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.[9]

As indicated in panel A 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 panel A reveal that an interquartile shift in exposure to PNTR is associated with a relative increase in the mortality rate from drug overdoses of 2 to 3 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 panels B and C 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

[9] The results discussed below are robust to ending the sample in 2007, around the start of the Great Recession.

[10] While our DID specification is useful for comparing mortality rates across counties with different levels of exposure to PNTR, it does not reveal the share of any increase in death rates attributable to PNTR. As a result, we evaluate economic significance as a share of the 2000 mortality rate.

increase in drug overdoses associated with exposure to PNTR is sufficiently large that it yields a statistically significant increase in panel D, for overall deaths of despair, even with the lack of observed effects for suicide or ARLD. We discuss potential explanations for why the relationship between PNTR and mortality is particularly stark for drug overdoses in Section IV.

Analogous estimates of interquartile shifts in counties' initial demographic attributes (online Appendix Figure A.1) reveal that counties with higher initial shares of manufacturing employment 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.

C. *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.

Data on the composition of the manufacturing workforce provides some 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.[11] 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.[12] 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, Deaton, and Lleras-Muney 2006). Finally, the population of other racial groups is more geographically concentrated than that of whites, which might decrease the precision of estimated relationships between PNTR and mortality rates for those groups.

D. *Baseline Estimates by Age*

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

[11] These percentages are reported in online Appendix Table A.2.

[12] See online Appendix Table A.2. Ebenstein et al. (2014) finds that workers displaced from manufacturing on average experience wage declines in moving to another sector.

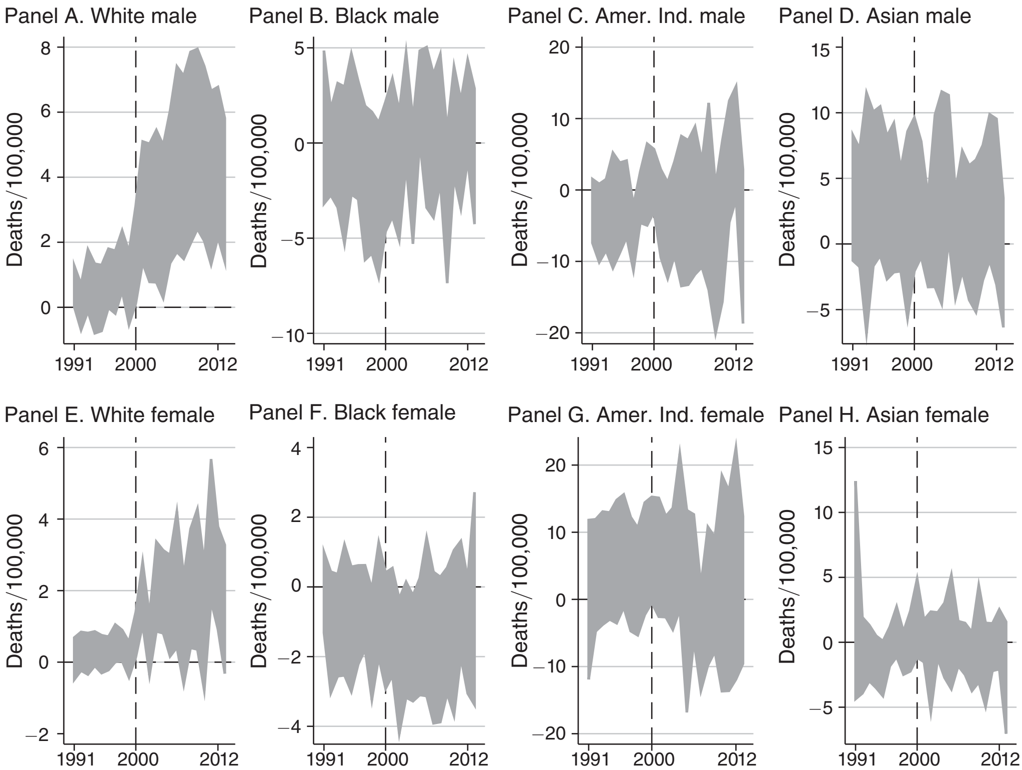

Figure 2. Implied Impact of PNTR on Drug Overdose Deaths, by Gender and Race

Notes: 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 panels) and females (bottom panels), by racial category. Each panel presents the results of a separate population-weighted estimation of equation (3). The population-weighted average death rates 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 3,122 counties. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Source: Authors' calculations based on CDC data.

PNTR and drug overdose mortality is evident across most of the age bins from 20 to 54.[13] These results suggest a labor market mechanism, discussed further below.

III. 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.

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

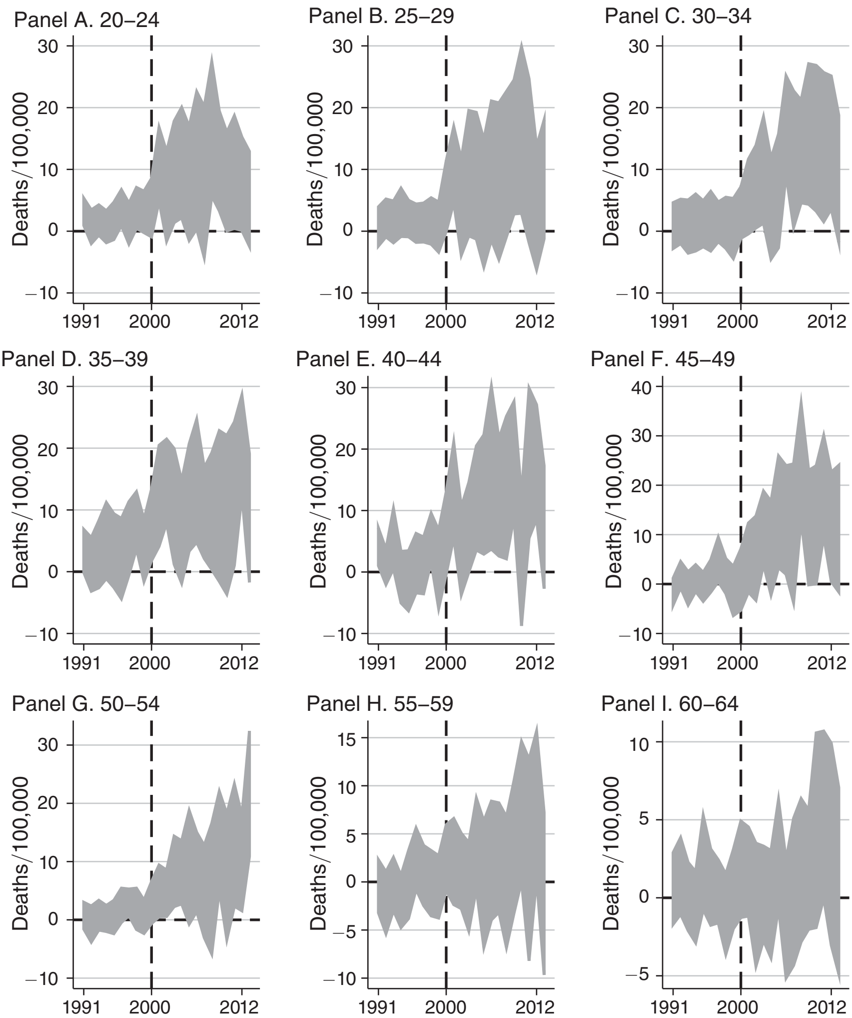

Figure 3. Implied Impact of PNTR on White Drug Overdose Deaths, by Age

Notes: 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 population-weighted estimation of equation (3). Each regression has 74,924 observations across 3,122 counties. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Source: Authors' calculations based on CDC data.

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 combination of

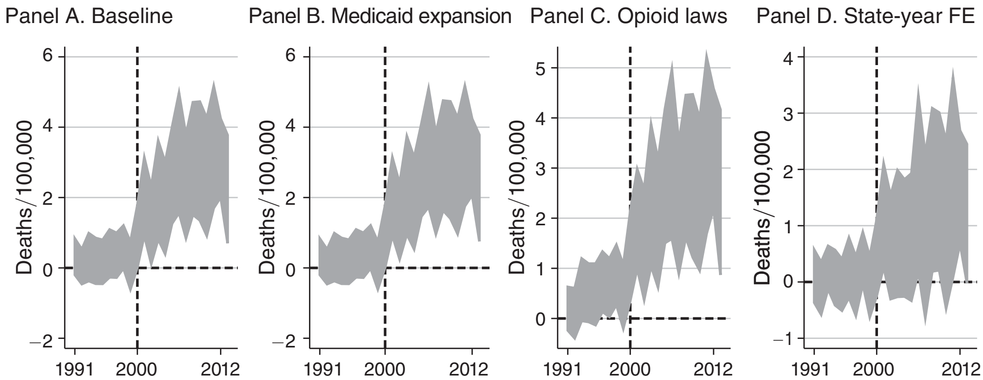

Figure 4. Implied Impact of PNTR on Drug Overdose Deaths, by Robustness Specification

Notes: 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 population-weighted estimation of equation (3) using a different set of control variables. Each regression has 74,924 observations across 3,122 counties. Panel A displays the baseline result from Figure 1. Panel B is for the baseline specification plus dummy variables for state-years in which Medicaid expansion occurs. Panel C is for the baseline specification plus controls for state opioid-law restrictiveness. Panel D 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.

Source: Authors' calculations based on CDC data.

PUMAs. We refer to these 950 geographic areas as "CUMAs."[14] 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.[15]

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 reduction in age-adjusted mortality among older adults, nonwhites, 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 panels A and B 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

[14] Case and Deaton (2017) analyze mortality across a similar geographic unit. We compare county and CUMA population distributions in online Appendix Figure A.2.

[15] We discuss the potential impact of migration on county-level mortality rates in the online Appendix.

relationship with mortality.[16] 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 panel C of Figure 4, inclusion of these measures also has little impact on the estimated link between PNTR and drug overdose mortality.

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), and that belong in estimates of its impact. Unsurprisingly, as illustrated in panel D 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 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 is the case for most areas of the United States during our sample period. In fact, we find no relationship between PNTR and the 16 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.

IV. 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 potential mechanism through which PNTR might lead to increased mortality from these causes—highlighted in Browning and Heinesen (2012) and Hollingsworth et al. (2017)—is via a deterioration in employment opportunities. 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.

[16] Ruhm (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.

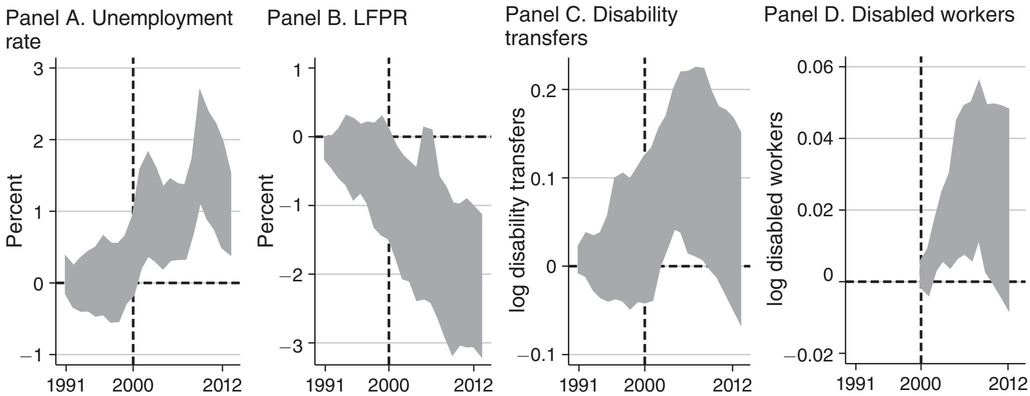

FIGURE 5. IMPLIED IMPACT OF PNTR ON LABOR MARKET OUTCOMES

Notes: 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 population-weighted estimation of equation (3). 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. Regressions for these outcomes have 74,886, 74,893, 72,227, and 43,462 observations across 3,121, 3,121, 3,031, and 3,112 counties, respectively. Confidence intervals are based on robust standard errors adjusted for clustering at the state level.

Sources: Authors' calculations based on data from the US Bureau of Labor Statistics, the US Bureau of Economic Analysis, and the Social Security Administration.

Panels A and B 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. Moreover, the effects of PNTR's labor market shock on drug use may have been exacerbated by the increasing availability of prescription opioid painkillers, such as Oxycontin, which was introduced in 1996 (Rudd et al. 2016). Counties experiencing relative worsening of labor market conditions associated with PNTR may have been more susceptible to drug use in the face of this increase in the supply of opioids.

Panels C and D of Figure 5 show that higher exposure to PNTR is also associated with relative increases in real disability payments and the number of disabled workers after 2000, though estimation of the latter is hampered by data unavailability at the county level prior to 1999.[17] 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 could increase after the change in US trade policy. That is, if workers displaced by trade liberalization applied for disability, they may have been introduced to prescription opioid painkillers as part of the process. Quinones

[17] Data sources are described in online Appendix Section E.1.

(2015, p. 154), 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.*

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.

V. 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.

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