The Impact of Trade Liberalization on Demand for Opioids*

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Abstract

Opioid overuse has grown substantially in the United States over the past two decades, contributing to a nationwide public health crisis. In this paper, I seek to understand whether a large international trade shock may have contributed to the opioid crisis. I leverage the increase in import competition resulting from the restoration of normal trade relations between the United States and China in 2000 as an instrument for local economic conditions to quantify the effects of economic conditions on demand for opioid pills. My analysis suggests that import exposure does not have a significant effect on opioid pill sales. In contrast with other economic variables, trade shocks appear to have little impact on opioid use.

Keywords: China shock, opioid use, opioids

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1 Introduction

The United States has experienced a surge in prescription opioid misuse and addiction over the past two decades. Opioid overdoses accounted for 47,000 deaths in the United States in 2017 (U.S. Centers for Disease Control and Prevention, 2020). Meanwhile, U.S. localities experienced an average 44.3 percent increase in prescription opioid pill sales between 2006 and 2014 (Figure 1, Table 1). In recent years, researchers have sought to understand the causes of the opioid overdose crisis by examining the extent to which adverse economic conditions may have contributed to opioid misuse (Carpenter et al., 2017; Hollingsworth et al., 2017; Charles et al., 2018). This research strongly suggests that fluctuations in macroeconomic conditions are related to opioid overdoses and demand for opioids. However, difficulty locating exogenous sources of variation in economic conditions has contributed to identification problems within this literature, rendering it difficult to distinguish whether it is economic conditions that encourage opioid misuse or opioid misuse that encourages undesirable economic outcomes, such as increases in unemployment.

This study adds to the literature by exploiting a large, plausibly exogenous employment shock to examine the impact of changes in economic conditions on opioid use. In 2000, the United States granted Permanent Normal Trade Relations (PNTR) to China, liberalizing trade relations between the two countries. Increases in import competition from China after PNTR spurred sharp declines in employment and wages within a number of U.S. sectors (Autor et al., 2013). Accordingly, several authors have leveraged the "China shock" as a source of exogenous variation in economic conditions to causally determine how import competition affects the health of U.S. workers (Autor et al., 2019; Ukil, 2019). My study adds to this literature by considering the impact of this trade shock on an additional public health outcome that has historically been correlated with economic conditions (Carpenter et al., 2017).

In this paper, I examine the impact of exposure to Chinese import competition on demand for prescription opioid pills in U.S. labor markets. I analyze the relationship between local exposure to Chinese imports and the local sales of over 19 billion opioid pills in the United States between 2006 and 2014.[1] In order to understand the character of the relationship between import exposure and adverse economic conditions during the period in which the opioid crisis has been particularly acute, I also examine the impact of import exposure on the manufacturing employment of young adults between 2000 and 2014.

My results suggest that the impact of Chinese import exposure on opioid use is limited.

[1] My data measure shipments of opioid pill sales to local pharmacies or other local points of distribution to consumers.

The impact of the trade shock on manufacturing employment is negative and significant, explaining roughly 13.6 percent of the mean decline in the manufacturing employment share of young adults in U.S. localities between 2000 and 2014. However, this import-induced decline in manufacturing employment did not occur alongside a similar import-induced change in opioid pill sales trends. The impact of import exposure on opioid pill sales is instead both insignificant and small in magnitude. My preferred specification indicates that localities at the 75^{th} percentile of import exposure would have had a roughly 1 percentage-point greater increase in demand for opioid pills over the 2006 to 2014 period than those at the 25^{th} percentile, but this result is not significant at the 10 percent level.

This paper speaks to two distinct strands of literature. First, it adds to a growing literature on the relationship between economic conditions and opioid use and mortality. This literature typically locates positive associations between opioid use and mortality and adverse economic conditions, such as unemployment and declines in manufacturing employment. However, much research is either correlational or otherwise does not use methods that address the possibility of reverse causality (Carpenter et al., 2017; Charles et al., 2018; Hollingsworth et al., 2017). Some recent studies on the impact of manufacturing plant closures on opioid use and mortality seek to avoid reverse causality issues with quasi-experimental designs (Dean and Kimmel, 2019; Venkataramani et al., 2019). However, in these studies, unobservable variables, such as social capital, may affect both decisions to close plants and opioid mortality (Venkataramani et al., 2019, p. 260). Recently, Currie et al. (2019) and Musse (2019) have sought to address these misspecification issues by analyzing relationships between opioid use and instruments designed to simulate local employment shocks. These studies yield conflicting results, but they present a promising path forward for the literature. My research builds on these studies by analyzing the effect of an alternative instrument for economic conditions on demand for opioid pills.

Second, this paper adds to the literature on the impact of trade with China on the social and economic experiences of U.S. workers. In the study closest to my own, Pierce and Schott (2016) observe that exposure to the China trade shock is associated with higher rates of deaths resulting from accidental poisonings, a category which includes drug overdoses. Ruhm (2018) draws on this finding to examine the relationship between Chinese import exposure and deaths attributed specifically to the use of illicit opioids and opioid analgesics. Ruhm (2018) generally observes little effect of import exposure on opioid mortality, although he finds that related economic variables, such as poverty rates, do correlate with opioid death rates. My study assists in clarifying the relationship between import exposure and opioid use and mortality trends by introducing a new dependent variable, demand for opioid pills in local labor markets. Opioid pill sales are a strong correlate of emergency department visits for opioid overdoses, so there is reason to expect that they are related to opioid mortality (Modarai et al., 2013). This study is the first to leverage the China trade shock to causally understand the impact of import exposure on demand for opioid pills.

This paper is organized as follows. Section 2 provides an overview of my empirical strategy and data. Section 4 presents my main results, with information on the impacts of Chinese import exposure on both employment and demand for opioids. Section 4 concludes.

2 Empirical Strategy

The empirical strategy employed in this paper is adapted from Autor et al. (2019), whose strategy builds on that of Autor et al. (2013) and Acemoglu et al. (2016). These papers leverage the recent growth in U.S. imports from China to analyze the impact of international trade on employment and outcomes in the United States. Rising trade with China is responsible for nearly all of the increase in trade between low-income countries in the United States since the 1990s (Autor et al., 2013), and imports from low-income countries have been negatively associated with manufacturing plant survival in the United States (Bernard et al., 2006). For this reason, a number of authors have examined whether import competition from China has contributed to the decline of the U.S. manufacturing industry over the last three decades (Autor et al., 2013; Acemoglu et al., 2016).

In this paper, I seek to understand both the relationship between trade and opioid use in the United States and the relationship between trade and manufacturing employment during the period in which the opioid crisis has been particularly acute. I expect that import-induced job losses are the primary channel through which trade may impact opioid use. However, in this paper, I do not directly analyze the relationship between manufacturing employment and opioid pill sales, but instead analyze each outcome separately. First, I replicate the Autor et al. (2019) analysis of the impact of Chinese import competition on manufacturing employment for the 2000-2014 period. Then, I analyze the impact of Chinese import competition on opioid pill sales between 2006 and 2014, the longest period for which data is available. I integrate my findings from both analyses in my discussion.

My analysis approximates local labor markets using commuting zones (CZs), geographic units that typically include multiple counties and that are based primarily on economic geography (Tolbert and Sizer, 1996). My analysis includes 713 CZs on the U.S. mainland, indexed by industry. The measure of local labor market shocks employed in this paper is the average change in Chinese import penetration in a CZ's industries, weighted by each industry's share in the CZ's initial employment:

$$\Delta IP^{cu}_{i\tau} = \sum_j \frac{L_{ij90}}{L_{i90}} \Delta IP^{cu}_{j\tau} \tag{1}$$

In this equation, the average change in Chinese import competition in each CZ i, $\Delta IP^{cu}_{i\tau}$, is computed as the sum of the product of the lagged employment shares of each industry j in CZ i's total employment (L_{ij90}/L_{i90}) and the growth of Chinese import penetration in each industry j over period τ, or the period 2000 to 2014. Lagged industry employment shares are computed based on County Business Patterns data from 1990. The growth of Chinese import penetration for industry j over period τ is equivalent to $\Delta IP^{cu}_{j\tau} = \Delta M^{cu}_{j\tau}/(Y_{j91}+M_{j91}-X_{j91})$. In this expression, the growth in U.S. imports from China, $\Delta M^{cu}_{j\tau}$, is divided by initial absorption (U.S. industry shipments plus net imports, $Y_{j91} + M_{j91} - X_{j91}$) in the year 1991, near the beginning of China's period of rapid manufacturing growth. Accordingly, variation in $\Delta IP^{cu}_{j\tau}$ stems from differences in the employment structure of a CZ's industries toward the start of China's export boom. These differences in employment structure stem from both a CZ's relative concentration of employment in manufacturing or non-manufacturing production and the extent of its specialization in import-intensive varieties of manufacturing. The "*cu*" superscript distinguishes this measure of the actual Chinese import exposure experienced by industries in the United States from the instrument for import exposure ("*co*") in equation (2) below.

The supply-driven component of Chinese import growth to the United States is identified by instrumenting for contemporaneous Chinese import growth to eight other high-income countries.[2] The instrument for exposure to Chinese imports among these comparison countries, $\Delta IP^{co}_{i\tau}$, is constructed using data on their respective industry structures:

$$\Delta IP^{co}_{i\tau} = \sum_j \frac{L_{ij80}}{L_{i80}} \Delta IP^{co}_{j\tau} \tag{2}$$

In contrast with equation (1), this expression replaces Chinese imports to the United States with Chinese imports to the eight countries listed above. It also replaces the start-of-period employment shares with 10-year lagged values and initial absorption with 3-year lagged values to mitigate potential simultaneity bias.

My instrument for Chinese import penetration, from Autor et al. (2019), uses data on bilateral product imports to the United States and other high-income countries from the U.N. Comtrade Database. These data are harmonized with U.S. industry data to analyze how Chinese imports differentially impacted various U.S. industries. To analyze the impact

[2] The eight comparison countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain and Switzerland.

of industry-based variation in import exposure on local employment, data on the industry composition of employment structures in U.S. commuting zones are taken from the U.S. Census County Business Patterns annual data series.

Data on oxycodone and hydrocodone pill sales are from the U.S. Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS). The portion of this dataset used in this paper was made public and cleaned by the Washington Post (The Washington Post, 2020). These data show the number of legal oxycodone and hydrocodone pills shipped annually to chain pharmacies, retail pharmacies and practitioners in U.S. counties from 2006-2014. Local pill sales are consolidated by commuting zone for analysis. Notably, the two commuting zones that contain the counties that include Charleston, South Carolina, and Leavenworth, Kansas are excluded from this analysis. These counties contain Veterans Affairs Department distribution pharmacies that distribute medication throughout wider regions, rather than merely to local patients.

3 Main Results

3.1 Employment

The causal effect of trade shocks on manufacturing employment is assessed using the following equation:

$$\Delta Y_{i\tau} = \alpha_t + \beta_1 \Delta IP^{cu}_{i\tau} + \mathbf{X}'_{it}\beta_2 + e_{i\tau} \tag{3}$$

In this expression, $\Delta Y_{i\tau}$ is the change in the share of the young adult population ages 18 to 39 employed in manufacturing in CZ i over the time interval 2000 to 2014. The variable $\Delta IP^{cu}_{i\tau}$ represents the change in import penetration from China in a CZ i. This variable is instrumented by $\Delta IP^{co}_{i\tau}$, the contemporaneous growth in import penetration experienced by eight other high-income countries. The vector $\mathbf{X}'_{it}$ contains a series of controls, including regional dummies for U.S. Census Divisions; the lagged share of commuting zone manufacturing employment; controls for employment in occupations susceptible to automation and offshoring (from Autor and Dorn, 2013 and Goos et al., 2014); and controls for commuting zone demographics, including race, education and the fraction of employed working-age women. Models are weighted by the start-of-period commuting zone share of national population, in year 2000, multiplied by one tenth of the period length (14 years).

Table 1 provides summary statistics for the variables used in expression (3). Table 2 examines the impact of an increase in import exposure on the manufacturing employment share of young adults. Estimates in columns 1 through 6 indicate that growth in import exposure has a significant, negative impact on the share of young adults who are employed in manufacturing. In each case, a one-unit trade shock, or the rough equivalent of the average change in trade exposure over the 2000-2014 period among CZs, led to a cumulative decline of between 0.62 to 1.58 percentage points in the share of young adults employed in manufacturing in the CZs included within this study. These estimates correspond to an average CZ-level decline of between 5.97 and 15.21 percent in the population of young adults employed in manufacturing between 2000 and 2014.

Examining the impacts of the covariates included in Table 2 on manufacturing employment shares assists in explaining the divergence between these two estimates. In column 1, I estimate an OLS regression with no controls. The coefficient estimate is negative and significant at the 10 percent level (t = -9.74), suggesting that a one-unit increase in import exposure depresses the share of young adults employed in manufacturing by 1.29 percentage points. In the 2SLS regression in column 2, the coefficient estimate increases in magnitude and becomes more precisely estimated (t = -9.88).

Column 3 introduces geographic dummies that correspond for the Census divisions for eight U.S. regions. The inclusion of these dummies in the model corresponds with a moderate decline in the magnitude of the trade shock coefficient estimate, suggesting that region-specific trends slightly attenuate the effect of the import shock on the share of young adults employed in manufacturing.

Column 4 augments the model with a control for the lagged total manufacturing share in each CZ. This control assists in addressing the concern that the Chinese import exposure variable is picking up an overall decline in the U.S. manufacturing sector, rather than the component of manufacturing decline that can be attributed to variation in local import penetration resulting from the diverse industry compositions of local manufacturing sectors (Autor et al., 2013, p. 2136). The inclusion of this control corresponds with a sharp decline in the magnitude of the trade shock coefficient estimate. The coefficient on the lagged manufacturing share covariate in the 2SLS regression is negative and significant ($-.123,\ t = -10.76$), suggesting that some of the negative effect of import exposure on manufacturing employment has been absorbed by this covariate. Indeed, the lagged manufacturing employment share control is positively correlated with the instrument. Similar, though less severe, declines the effect of the trade shock on employment with the inclusion of a lagged manufacturing employment share control are identified in Autor et al. (2018, p. 22) and Autor et al. (2013, p. 2137), both of which analyze the evolution of manufacturing employment over a longer time span than does this study. The result in column 4 suggests that the capacity of the trade shock instrument to track variation in import penetration resulting from local variation in industry composition, rather than from a general decline in manufacturing, may be less developed within this shorter time span and with fewer observations.

Column 5 introduces controls for characteristics of the composition of a CZ's employment structure that may render its jobs particularly susceptible to automation or offshoring. These include a control for the start-of-period employment share in routine occupations, or those occupations that involve either repetitive motion or repetitive information processing tasks, and a control for the start-of-period employment share in offshorable occupations, or those that do not require close proximity to a specific workplace or face-to-face contact with U.S. workers. These controls are negatively correlated with the change in manufacturing employment, and they modestly decrease the magnitude of the trade shock coefficient.

Finally, the fully-augmented specification in column 6 introduces controls for the start-of-period population composition of each CZ. These controls include variables representing the respective share in each CZ of the population that is Hispanic; the shares of the population by race; the share of the population that is foreign born; the share of the population that has a college education; and the fraction of women who are employed. The trade shock coefficient remains roughly the same as in column 5. Comparing CZs at the 25^{th} versus the 75^{th} percentiles of trade exposure, this specification implies that the latter would have had a 0.37 percentage-point (0.566×0.653) larger decline in its manufacturing employment share of young adults over a roughly decade-long period than the former. This corresponds to roughly 13.6 percent of the mean CZ decline in the manufacturing share of young adults between 2000 and 2014.

3.2 Demand for Opioids

The causal impact of trade shocks on opioid pill sales is assessed using the following equation:

$$\Delta Y_{i\tau} = \alpha_t + \beta_1 \Delta IP^{cu}_{i\tau} + \mathbf{X}'_{it}\beta_2 + e_{i\tau} \tag{4}$$

In this specification, $\Delta IP^{cu}_{i\tau}$ again represents the change in import penetration from China in a CZ i over the period 2000-2014. This variable is instrumented by $\Delta IP^{co}_{i\tau}$, the contemporaneous growth in import penetration experienced by eight other high-income countries. In contrast with equation (3), $\Delta Y_{i\tau}$ now represents the change in log opioid pill sales in CZ i over the time interval 2006 through 2014. Reliable data on opioid pill sales is largely unavailable before 2006. The vector $\mathbf{X}'_{it}$ contains the same controls as in specification (3), with the addition of a new control for the start-of-period (2006) log opioid pill sales volume in each CZ.

The results for the instrumental variables regression in model (4) are reported in Table 3. In general, the results suggest that there is no apparent effect of import exposure on opioid pill sales. The most complete specification, in column 7, indicates that a one-unit change in exposure to Chinese imports corresponds with an increase in opioid pill sales of roughly 2.31 percentage points during the 2006 to 2014 period. However, this observation is not significant at the ten percent level ($t = 0.83$). Even if this result had been significant, it suggests that trade would have had little impact on opioid pill sales. Comparing CZs at the 25^{th} versus the 75^{th} percentiles of trade exposure, this result implies that the latter would have had a 1.31 percentage-point (0.566×2.31) larger increase in opioid pill sales over the 2006 to 2014 period than the former.

The trade shock coefficient in column 7 provides the baseline result for this paper. However, the trade shock coefficient estimates reported in columns 1 through 6 also merit investigation. In column 1, I estimate an OLS regression with no controls. The coefficient estimate is positive and significant at the 5 percent level ($t = 2.04$), suggesting that a one-unit increase in import exposure corresponds with an increase in opioid pill sales of 3.06 percent. This relationship is visualized in Figures 2 and 3, both with and without instrumenting for import exposure, respectively. In the 2SLS regression in column 2, the coefficient estimate remains positive but declines in magnitude and becomes less precisely estimated ($t = 1.21$). The precision of IV estimates is generally lower than that of OLS estimates, even when instruments are near-perfect (Myers et al., 2011). However, in their analysis of the labor market impacts of Chinese import competition, Autor et al. (2013) observe that IV regressions consistently indicate more adverse impacts of import exposure on labor market outcomes than OLS regressions. If import shocks affect opioid pill sales through adverse labor market conditions, one would expect a greater impact of imports on opioid pill sales when transitioning from OLS in column 1 to 2SLS in column 2. Counterintuitively, the opposite result is obtained here.

Turning to column 3, the inclusion of Census division dummies for eight U.S. regions both substantially increases the trade shock coefficient estimate and renders it significant at the 10 percent level. In addition, the model now explains 19.3 percent of the variation in opioid pill sales, a substantial increase from the R^2 values of approximately 1.3 percent in both of the previous estimates in columns 2 and 3. Despite these increases, it appears unlikely that the trade shock explains a substantial portion of the variation in opioid pill sales. The regression coefficients for each of the Census division dummies are uniformly larger than the trade shock coefficient reported in column 3, and several eclipse the trade shock coefficient by orders of magnitude. These coefficients are typically highly significant. Regional variation appears to explain a significant portion of variation in opioid sales, while trade does comparatively little to explain variation in revealed demand for opioids.

In column 4, the model is augmented with a control for the lagged total manufacturing share in each CZ. The inclusion of this control corresponds with a sharp decline in the trade shock coefficient estimate, such that the impact of the trade shock on opioid pill sales becomes negative and insignificant. This decline can be attributed to correlation between the instrument and the lagged manufacturing employment share control, which is positively correlated with change in opioid pill sales.

In column 5, the addition of a control for the log opioid pill sales volume at the start of the time interval, in 2006, corresponds with an increase in the trade shock coefficient estimate, rendering it positive. The start-of-period log opioid pill sales control is negatively correlated with change in opioid pill sales, so the increase in the trade shock coefficient magnitude is expected as a result of correlation between the instrument and this control. In column 6, the inclusion of occupational composition controls decreases the magnitude of the trade shock coefficient. In both cases, the coefficient remains insignificant, and R^2 values do not greatly increase. Finally, the addition of population controls in column 7 increases the magnitude of the trade shock coefficient and substantially increases the R^2 value, suggesting that demand for opioid pills is strongly tied to local demographics. This complete specification suggests that the impact of trade exposure on opioid pill sales is both insignificant ($t = 0.83$) and of relatively small magnitude (a one-unit trade shock corresponds to an increase in opioid pill sales of only 2.31 percent). The covariates included within this specification together explain only 48.5 percent of the variation in opioid pill sales between 2006 and 2014. These results suggest that the impact of trade exposure on opioid pill sales is minimal. In general, collinearity between my instrument and control variables may have contributed to reduced precision and inflated standard errors in my model estimates.

4 Conclusion

In this paper, I explore the effect of a large import competition shock on the manufacturing employment share of young adults and the demand for opioid pill sales in over 700 U.S. labor markets. I observe that while this trade shock corresponded to a significant decline in the manufacturing employment share of young adults, this decline in employment did not occur alongside an increase in opioid pill sales. Indeed, the effect of import competition on opioid pill sales is both of minimal magnitude and insignificant.

My results correspond with findings by Ruhm (2018), who observes that exposure to Chinese import competition typically does not significantly affect mortality rates for deaths attributed to use of opioid analgesics or illicit opioids. Ruhm (2018) attributes his null finding to a weak instrument, observing that import competition does not correlate strongly with other economic proxies that he finds do affect opioid mortality (poverty, incomes, home prices and average unemployment rates). Import competition may affect the manufacturing employment shares of young adults, but it may not be related to other economic variables that do correlate with opioid mortality, such as incomes or employment outcomes among the broader population. In this sense, although this paper's findings suggest that import competition may not affect opioid pill sales, it remains possible that other economic shocks may impact demand for opioid pills. Further research can assist in clarifying the channels through and the extent to which economic shocks may affect opioid pill sales and opioid mortality.

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Tables

Table 1: Summary Statistics for U.S. Commuting Zones

	Mean	**Std. Dev.**	**Min.**	**Max.**
Share of Young Adults Employed in Manufacturing , 2000	10.39	(4.74)	1.95	35.86
Change in Share of Young Adults Employed in Manufacturing, 2000-14	-2.71	(1.53)	-11.62	2.51
Change in Import Exposure, 2000-14	1.15	(0.77)	-0.79	6.41
Opioid Pill Sales, 2006	7.41e+07	(8.26e+07)	14,100.00	3.35e+08
Log Opioid Pill Sales, 2006	17.40	(1.40)	9.55	19.63
Percentage Change in Opioid Pill Sales, 2006-14	44.33	(27.75)	-27.52	403.11

	25th pctl	**50th pctl**	**75th pctl**
Share of Young Adults Employed in Manufacturing, 2000	7.445	9.572	12.707
Change in Share of Young Adults Employed in Manufacturing, 2000-14	-3.432	-2.517	-1.698
Change in Import Exposure, 2000-14	0.733	1.019	1.299
Opioid Pill Sales, 2006	1.60e+07	4.87e+07	1.04e+08
Log Opioid Pill Sales, 2006	16.590	17.701	18.456
Percentage Change in Opioid Pill Sales, 2006-14	27.949	42.711	60.835
Observations	713		

Table 2: Impact of Trade Shock on Change in Manufacturing Employment, 2000-2014.

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Change in Import Exposure	-1.287***	-1.576***	-1.462***	-0.713***	-0.622***	-0.653***
	(0.132)	(0.160)	(0.128)	(0.107)	(0.111)	(0.117)
Constant	-1.225***	-0.891***	-1.179***	-0.248	1.665	0.101
	(0.140)	(0.163)	(0.427)	(0.267)	(1.200)	(1.522)
Observations	713	713	713	713	713	713
R-squared	0.415	0.394	0.476	0.712	0.725	0.738
Census Division Dummies			YES	YES	YES	YES
Manufacturing Emp Share				YES	YES	YES
Occupational Composition					YES	YES
Population Composition						YES

Note: N = 713 (713 commuting zones). The manufacturing employment share control in columns 3-6 controls for the lagged share of CZ employment in manufacturing. The occupational composition controls in columns 5-6 contain the start-of-period indices of employment in routine occupations and employment in offshorable occupations, from Autor et al. (2013). The population controls in column 6 contain the start-of-period shares in CZ population that are Hispanic, Black, Asian, other race, foreign born, and college educated, and the fraction of women who are employed. Standard errors are clustered by state. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Impact of Trade Shock on Change in Log Opioid Pill Sales, 2006-2014.

VARIABLES	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS	(7) 2SLS
Change in Import Exposure	0.0301**	0.0300	0.0496**	-0.00232	0.0255	0.00937	0.0228
	(0.0148)	(0.0247)	(0.0225)	(0.0250)	(0.0347)	(0.0348)	(0.0276)
Constant	0.313***	0.313***	0.116***	0.0511	0.795**	1.121**	1.499**
	(0.0354)	(0.0450)	(0.0443)	(0.0431)	(0.321)	(0.460)	(0.599)
Observations	713	713	713	713	713	713	713
R-squared	0.013	0.013	0.193	0.260	0.292	0.315	0.485
Census Division Dummies			YES	YES	YES	YES	YES
Manufacturing Emp Share				YES	YES	YES	YES
Log Opioid Sales					YES	YES	YES
Occupational Composition						YES	YES
Population Composition							YES

Note: N = 713 (713 commuting zones). The manufacturing employment share control in columns 3-7 controls for the lagged share of CZ employment in manufacturing. The occupational composition controls in columns 5-7 contain the start-of-period indices of employment in routine occupations and employment in offshorable occupations, from Autor et al. (2013). The opioid sales control in columns 5-7 controls for the number of opioid pills sold in a commuting zone in 2006. The population controls in column 7 contain the start-of-period shares in CZ population that are Hispanic, Black, Asian, other race, foreign born, and college educated, and the fraction of women who are employed. Standard errors are clustered by state. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Figures

Figure 1: Distribution of Opioid Pill Sales Growth, U.S. Commuting Zones

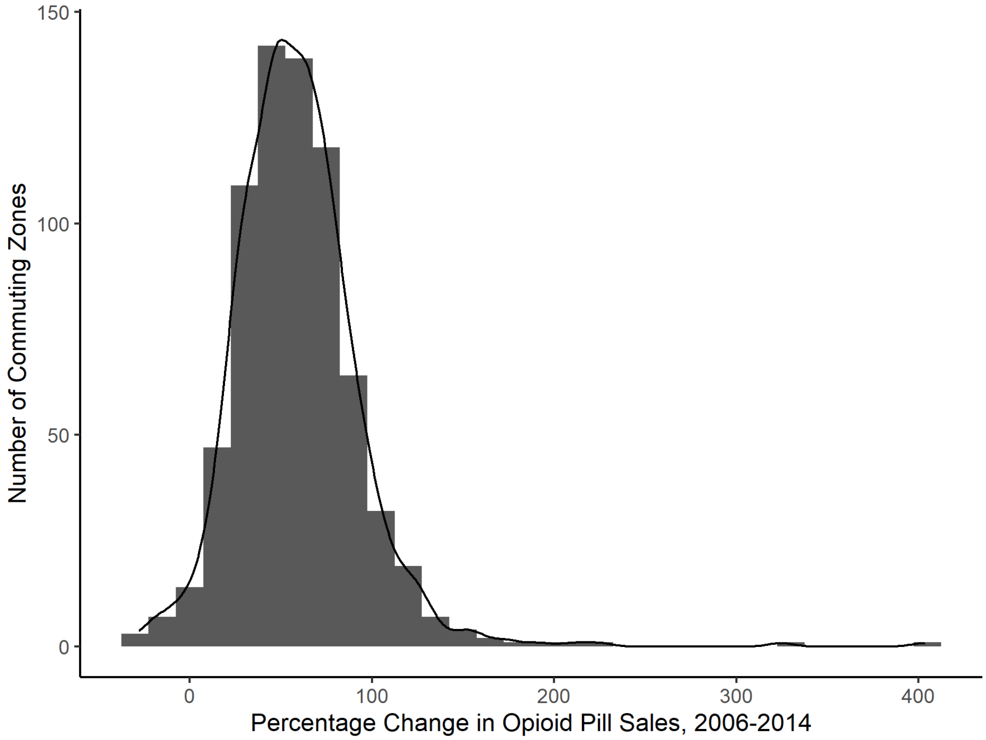
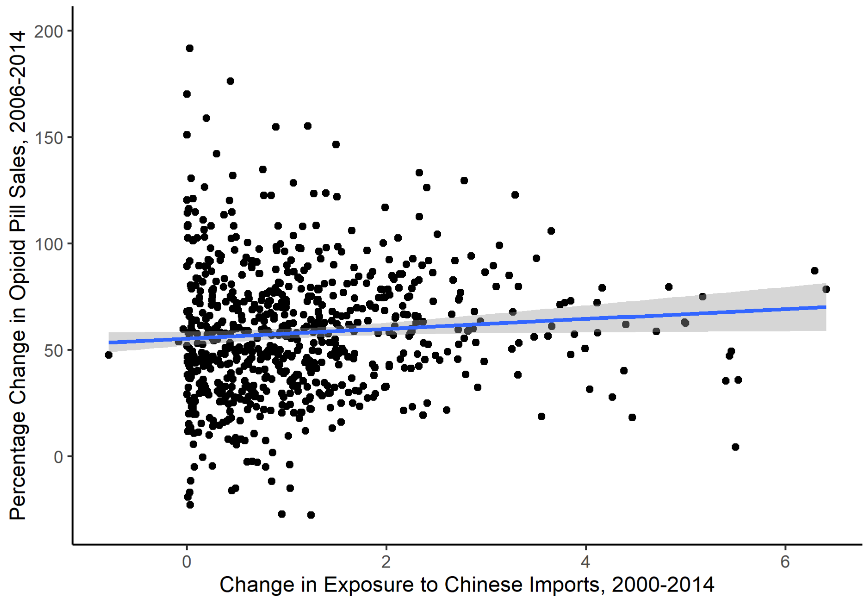
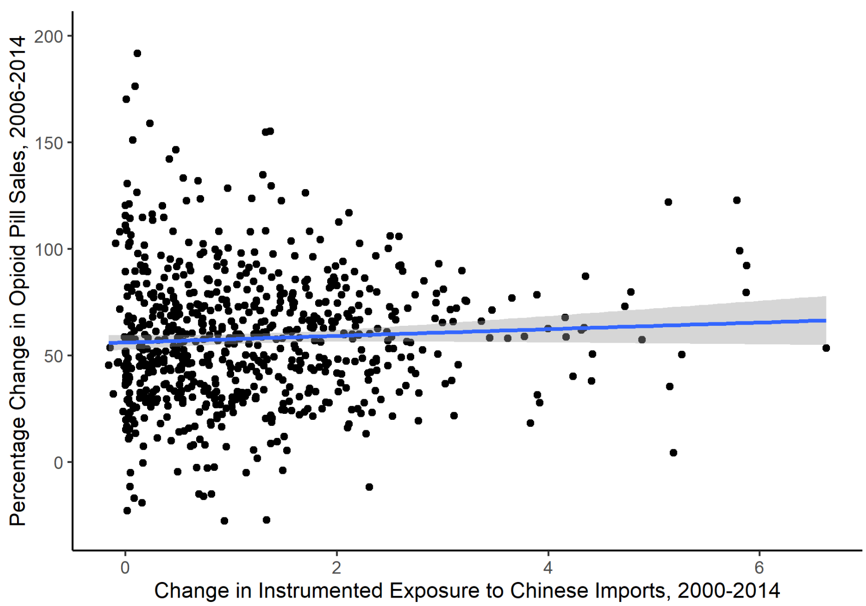


Figure 2: Relationship Between Import Exposure and Opioid Pill Sales, U.S. Commuting Zones



This plot removes three outliers, representing commuting zones with greater than a 200 percent change in opioid pill sales.

Figure 3: Relationship Between Instrument for Import Exposure and Opioid Pill Sales, U.S. Commuting Zones



This plot removes three outliers, representing commuting zones with greater than a 200 percent change in opioid pill sales.